To Invest or Insure?: How Authoritarian Time Horizons Impact Foreign Aid Effectiveness

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Comparative Political Studies 2008 41: 971 originally published online 20 December 2007
DOI: 10.1177/0010414007308538

Comparative Political Studies
Volume 41 Number 7
July 2008 971-1000

10.1177/0010414007308538
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To Invest or Insure?

How Authoritarian Time Horizons Impact Foreign Aid Effectiveness

Joseph Wright
Princeton University

In this article, the author argues that the time horizon a dictator faces affects his incentives over the use of aid in three ways. First, dictators have a greater incentive to invest in public goods when they have a long time horizon. Second, dictators with short time horizons often face the threat of challengers to the regime; this leads them to forgo investment and instead consume state resources in two forms that harm growth: repression and private pay-offs to political opponents. Third, dictators with short time horizons have a strong incentive to secure personal wealth as a form of insurance in case the regime falls. Using panel data on dictatorships in 71 developing countries from 1961 to 2001, the author finds that time horizons have a positive impact on aid effectiveness: Foreign aid is associated with positive growth when dictators face long time horizons and negative growth when time horizons are short.

Keywords: *foreign aid; dictatorships; growth; time horizons*

For many years, the literature on the effectiveness of foreign aid revolved around the question of how aid affected growth under different economic policy regimes (Burnside & Dollar, 2000; Easterly, Levine, & Roodman, 2004).[1] Economists believed that when recipient countries pursued "good" economic policies such as low inflation, low budget deficits, and high trade volume, aid would spur growth. This debate, though, generally neglected to consider the domestic political incentives aid recipient governments face over the use of aid. If certain types of aid recipient regimes face strong domestic political incentives to steal aid resources or use them to pay off or repress political opponents, there is little reason to believe that "good" policies in these regimes should make aid more effective.

The most recent round of the aid debate pits optimists, such as Sachs (2004), who believe that substantial increases in aid giving are necessary to lift the world's poor out of poverty, against realists, such as Easterly (2006), who argue that piecemeal aid projects with narrow, easily measurable goals

are the most effective. The optimists believe that governance does not matter, whereas the realists believe that governments are so bad that donors should skip them entirely and somehow simply give aid directly to the poor.[2] Neither perspective directly addresses the governance question. To understand how aid can promote growth, we need to think through the political processes that shape how aid is used in recipient countries. Political leaders (and even nongovernmental organizations) who receive aid are located in different types of settings that place different sets of constraints on their behavior. Formal institutional constraints, such as the existence of a legislature or the type of electoral system, may affect how aid is likely to be used, but so too will incentives provided by informal institutions,[3] such as a clientelism (Keefer, 2007), or as I argue in this article, time horizons. Understanding how aid is likely to be used in these different types of settings will offer insight into whether and how aid promotes growth.

From 1960 to 2002, two thirds of aid recipient countries were authoritarian regimes. Even in 2005, 53 of 120 aid recipient countries were authoritarian regimes. Given the historical propensity of aid flowing to dictators, it is instructive to look at how political constraints shape the use of aid in authoritarian regimes. The current consensus in the aid community is that aid should only be given to countries with good governance scores.[4] This focus on governance as the key mechanism for selecting aid recipient countries marks an important step toward rationalizing aid distribution according to measurable recipient country attributes (Kaufmann & Kraay, 2004; Kaufmann, Kraay, & Mastruzzi, 2003). But given that efforts to measure governance in a systematic fashion have only recently begun and data on governance date only from the late 1990s, we still do not have a reliable way to assess how political incentives condition aid effectiveness throughout most of the history of aid giving. It might be tempting to give aid only to democracies where political accountability is by definition higher than in dictatorships. The reality is much more complex than this, however, as some types of authoritarian regimes, such as Botswana and South Korea (1961-1987), have a very good track record of using aid, whereas others, such as the former Zaire and Kenya, do not. To understand how authoritarian regimes use aid, in this article I concentrate on how a dictator's time horizon shapes his incentives over the use of aid. To model the impact of authoritarian time horizons on the aid–growth relationship, I argue that the predicted probability of authoritarian regime failure serves as the best proxy for measuring time horizons.

Time horizons affect a dictator's incentives over the use of aid in three ways. First, politicians of all stripes, not just dictators, have a greater incentive

to invest in public goods, including property rights protection and social spending, when they have a long time horizon. Second, dictators with short time horizons often face the threat of challengers to the regime. This leads them to forgo investment and instead consume state resources in two forms that harm growth—repression and private pay-offs to political opponents. State resources spent on repression and private pay-offs not only hurt growth by substituting consumption for investment but also deter private investment (repression) and increase rent-seeking behavior (private pay-offs). Third, dictators with short time horizons have a strong incentive to secure personal wealth as a form of insurance in case the regime falls.

Theoretical Perspectives

An autocrat with short time horizons has a strong incentive to engage in predatory and distortionary economic policy. With little incentive to forgo immediate taxation (predation), the roving bandit (Olson, 1993) will confiscate much of the economic product under his rule in the present period. This predatory behavior destroys the incentive for citizens to engage in productive activity, and economic output declines. Alternatively, a stationary bandit who anticipates that he will rule for some time into the future will limit taxation (i.e., predatory behavior) and provide more public goods to increase national output and yield more net income for himself in the long run. The stationary bandit with long time horizons has an incentive to invest in the protection of property rights and enforce private contracts. This in turn should contribute to long-term increases in economic output. Clague, Keefer, Knack, and Olson (1996) sum up this logic by contrasting an autocrat's incentives for investment in property rights with a democrat's: "Whereas in an autocracy it is the leader's interest in his future tax returns (and thus in the future income of his domain) that is the source of any property and contract rights, in any lasting democracy it is the very mechanisms that ensure that a leader cannot unilaterally extend his hold on power that are the source of property and contract rights" (p. 245).

Applying this logic to the use of foreign aid in authoritarian regimes is quite straightforward. This assumes that aid is generally fungible in dictatorships.[5] Autocrats who face short time horizons would likely use foreign assistance for personal consumption, whereas those who face long time horizons should invest aid in public goods that grow the economy so the autocratic regime can take from a larger pie in the future. In the context of developing countries, long-term investment could include property rights

protection and infrastructure projects, but also investment in public health and education and protection of the environment. To take one notorious example, Reno (1997) points out that social service spending during Mobutu's regime in the former Zaire fell from 17.5% of government spending in 1972 to 2% in 1990, and agricultural spending (mostly subsidies) fell from over 40% of the budget to 11% in 1990. Meanwhile, during that time, the president's share of the budget increased from 30% to 95%. It was not until after 1990 that the real economic chaos began (see Table 1). So although Mobutu is generally viewed as having plundered the economy at every step, he invested substantially in public goods during some periods of his rule. Even if a dictator with a long time horizon seizes all the assets in a country (contrary to the logic of the stationary bandit), this long time horizon still provides him with an incentive to invest in public goods and infrastructure. Rafael Trujillo, who ruled the Dominican Republic for over 30 years, systematically seized the productive assets of the country's agricultural economy. His rule, particularly in the first 25 years, was not one of plunder and pillage though, as he invested in agriculture, infrastructure, and financial institutions (Wiarda, 1975).[6] These brief examples point toward the possibility that investment in growth-enhancing public goods (which may include private property protection, but not necessarily—as the case of Trujillo illustrates) is a function of a dictator's time horizon. The probability that aid would be directed toward investment should follow a similar logic.

There is a second line of reasoning that points to the same expectations regarding autocratic time horizons and the use of aid. Unstable autocrats who face short time horizons have an incentive to use aid money to pay for repression or buy off potential threats to the regime in a time of crisis (Wintrobe, 1998). The short time horizon these autocrats face forces them to raid any available revenue, including foreign aid, in an effort to repress or pay off challengers to the regime. Alternatively, a stable autocrat with long time horizons institutionalizes a mechanism, such as rotating elites[7] or forming a hegemonic party and holding elections, to co-opt potential rivals more cheaply (Magaloni, 2006). In an unstable regime, the autocrat must resort to lump sum payments to rivals who are threats to the regime, because they know that the regime is faltering and do not see promises about future payments as credible. Aid money spent on repressing political opponents not only substitutes consumption for potential investment; the repression itself can deter private investment (Feng, 2001) and reduce the return on public investment (Isham, Kaufmann, & Pritchett, 1997). And state resources, including aid, spent on private pay-offs to political opponents can induce renting-seeking behavior: As opportunities for rent seeking

increase, citizens replace investment in productive activities with investment in rent-seeking activities, which in turn slows economic growth (Krueger, 1974; Murphy, Schleifer, & Vishny, 1993; Tornell & Lane, 1999).

Consider the last years of Mobutu's rule in the former Zaire. As economic resources declined in the early 1990s, Mobutu conceded to multiparty elections that were meaningless in the sense that no challenger had any realistic probability of winning, but the elections did put key opponents in the legislature so that Mobutu could monitor his payment to rivals and opposition leaders. Schatzberg (1997) writes of political liberalization: "[s]eemingly overnight, hundreds of political parties appeared (some with regime financing). . . . The opposition, however, remained largely fragmented. Many oppositionists could not resist the regime's blandishments and rallied to it—for a price" (p. 74). Although Mobutu could not control the multiplication of opposition parties, he successfully used the Sovereign National Conference to locate and buy off the strongest opposition parties. As Wrong (2000) notes, "Mobutu bought them as enthusiastically as he once bought individuals" (p. 108). He bought off two important opposition parties during the Congress, the PRI-Fenadec and UFERI, by naming UFERI's leader Nguza Karl-i-bond as the new prime minister (Leslie, 1993). Subsequently, Nguza and UFERI were expelled from the opposition coalition (Union Sacrée), and Nguza quickly suspended the National Sovereign Conference altogether. Although many regime opponents thought the conference signaled a legitimate political opening, it enabled Mobutu to identify the powerful opponents he needed to co-opt to survive. Askin and Collins (1993) write of the chaotic period after 1990, "Mobutu shrewdly rode the wave of protest. . . . He offers minor reforms, followed by major disreforms, buys the support of some oppositionists; sends troops to kill or threaten others" (p. 80). During this period of instability, however, aid to the former Zaire did not decrease significantly below the averages received in the 1970s and early 1980s.[8] Thus, unstable autocrats who face short time horizons should have an incentive to use aid money for repression of and/or private pay-offs to threats to the regime.

A third reason why authoritarian leaders who face short time horizons should misuse aid is that as their tenure on power becomes more tenuous, they have a stronger incentive to secure personal wealth as a form of insurance once the regime falls. In the most unstable regimes, the autocrat makes a choice between (a) using all rents to quell threats in the hope of remaining in power until the next period or (b) sending all the rents abroad and fleeing the country, depending on whether he thinks he is in the last period or the penultimate period of his regime.

Some rough estimates are available of how much certain high-profile dictators have stolen during their time in power. Transparency International estimates that Mohamed Suharto (Indonesia) stole over $15 billion, Ferdinand Marcos (Philippines) and Mobutu Sese Seko (former Zaire) each stole over $5 billion, Sani Abacha (Nigeria) took between $2 and $5 billion, the Duvalier family (Haiti) stole between $300 and $800 million, and Alberto Fujimori (Peru) confiscated over $600 million. There is also some scattered evidence of when this theft occurred. During the trial of Imelda Marcos, for example, prosecutors focused on evidence that from 1982 to 1986—the last 4 years of the regime—between $83 and $236 million in cash from the Philippine treasury was deposited in secret accounts (Wolff, 1990). Investigators also recently found that Pinochet's secret accounts at Riggs Bank in Washington, D.C., were opened in 1985, just 4 years before Pinochet stepped down after the 1989 referendum (O'Hara, 2004). It was not until the early 1980s—after Suharto had been in power for over 15 years—that the Suharto clan acquired the lucrative monopolies that enabled them to collect the billions noted above ("The Family Firm," 1997). Finally, when Jean Duvalier finally left Haiti for France in 1986, he (and his entourage) took nearly half his estimated stolen booty with them in suitcases (Treaster, 1986). These anecdotes suggest that these dictators moved the money in the later stages of their regimes—often when the handwriting was on the wall.

When the dictator engages in these behaviors and thus has an incentive to obfuscate the real numbers, obtaining accurate time-series data on public goods spending, pay-offs to political rivals, and stolen state resources is nearly impossible.[9] We do, however, have economic growth data for most regimes, which we can use to infer these unobservable behaviors from economic outcomes. The first place to look is at kleptocratic personalist regimes where the regime is often a unitary actor (and perhaps his family). Researchers have developed complex theoretical models to explain how kleptocratic rulers can remain in power while systematically stealing large portions of the national economic product. According to Acemoglu and Robinson (2004), "[e]xamples of kleptocratic regimes include the Democratic Republic of the Congo (Zaire) under Mobutu Sese Seko, the Dominican Republic under Rafael Trujillo, Haiti under the Duvaliers, Nicaragua under the Somozas, Uganda under Idi Amin, Liberia under Charles Taylor, and the Philippines under Ferdinand Marcos. In all these cases, kleptocratic regimes appear to have been disastrous for economic performance and caused the impoverishment of the citizens" (p. 163). Although these rulers were certainly kleptocrats in stealing large portions of their state's

resources, it is simply not true that kleptocratic rule in these countries was "disastrous for economic performance"—at least not in the early periods of their regimes.

As Figure 1 indicates, all of the kleptocrats who make Acemoglu and Robinson's (2004) list, with the exception of Amin and Mobutu, ruled over periods of sustained economic growth early in their regimes. Even Mobutu managed to grow the economy, albeit modestly, in his first decade of rule. A tendency to focus on the final years of kleptocratic regimes has resulted in their being associated with disastrous economic performance. However, paradigmatic kleptocrats, early in their regimes (when they have long time horizons), are quite successful at growing the economy, but they systematically destroy the economy late in their regimes when they have short time horizons. Of course, causation works both ways: As they near the end of their tenure on power, kleptocrats have weaker incentives to grow the economy, and a slumping economy often weakens their grip on power. The data in Figure 1, however, are not consistent with a kleptocratic strategy to uniformly steal the national product and destroy the economy in all periods of rule. Rather, if the latter years of these dictatorships coincide with shorter authoritarian time horizons, then the data are consistent with a strategy strongly influenced by autocratic time horizons.

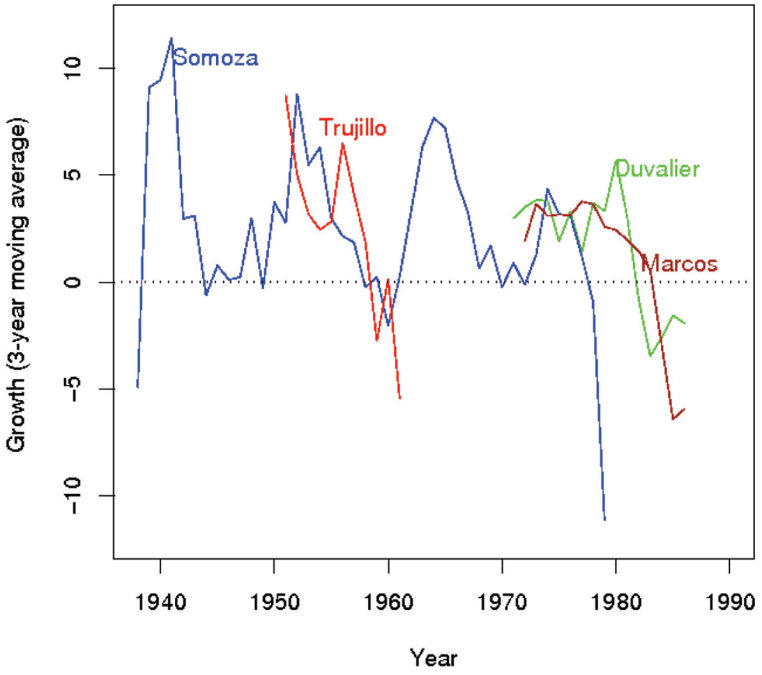
Leaders in single-party dictatorships provide a revealing contrast to these kleptocrats. Dominant single party regimes typically institutionalize turnover within the party structure, thereby removing uncertainty over succession (Geddes, 1999; Magaloni, 2006). Leaders in these regimes are more likely to leave power by regular means (as opposed to assassination, coup, or exile) than leaders in other types of regimes (Weeks, 2006), and their posttenure fate is typically not exile or death. Thus, leaders in these regimes should have a weaker incentive to steal as a form of insurance against being ousted by irregular means.

Hypothesis: Aid to dictators with long time horizons (low probability of failure) should be positively associated with economic growth, whereas aid to dictators with short time horizons (high probability of failure) should be associated with zero or negative growth.

Data and Method

To test this hypothesis, I turn to the literature on aid and growth and use a data set constructed by Easterly et al. (2004) to test the effect of aid on growth in different types of policy environments (sources for all data are

Figure 1
Growth in Kleptocracies



Note: Growth is a 3-year moving average ($t = 0$, $t = -1$, $t = -2$).
Source: GDP per capita data are in 1990 U.S. dollars from Maddison (2004), except for Liberia (World Bank, 2005). In the Dominican Republic, 1950 is the first year of available data. Latin American Centre at Oxford University (2007) estimates indicate that the Dominican Republic's GDP (not GDP per capita) grew at an average annual rate of over 10% from 1945 to 1950.

listed in Table 5). This is the most current version of the panel data used in much of the aid–growth literature (Burnside & Dollar, 2000; Easterly et al., 2004; Hansen & Tarp, 2000, 2001). These data contain 4-year panels from 1961 to 2001, where the dependent variable is the annual growth rate averaged over 4 years.[10] The control variables used in this analysis are the following: log GDP, ethnic fractionalization, assassinations, International Country Risk Guide (ICRGE) as a measure of the quality of government, money supply, sub-Saharan Africa dummy, East Asia dummy, and a macroeconomic policy index.[11] All variables that change over time are 4-year

panel averages. I divide this data into democracies and authoritarian regimes using a dichotomous measure of democracy based on Przeworski, Alvarez, Cheibub, and Limongi (2000) and Cheibub and Gandhi (2004): Sixty-four percent of the nonmissing observations from Easterly et al. (2004) are authoritarian regimes.

In addition to the control variables used in the literature on aid, policies, and growth, I report results that include domestic investment, human capital, and capital stock growth as control variables. All three of these variables are frequently used as controls in cross-national growth regressions (Barro, 1997; Levine & Renelt, 1992). Domestic investment is measured as a share of GDP; the average total years of schooling for persons over the age of 15 years is used as a proxy for human capital; and the percentage change in total physical capital stock is the measure of capital growth. All of these measures are averaged over the 4 years of each panel.

Measuring Autocratic Time Horizons

To construct a proxy for autocratic time horizons, previous researchers have used either (a) the number of changes in the chief executive and the number of coups in each authoritarian spell as proxies for regime stability (Folch, 2003; Gandhi & Przeworski, 2006) or (b) the age of the regime (Clague et al., 1996; Folch, 2003). Both of these measures are problematic.

First, the proxies for regime stability are taken from the Przeworski et al. (2000) data, which do not separate out authoritarian regime changes with a particular authoritarian spell. That is, the data do not account for successive authoritarian regimes, only authoritarian spells uninterrupted by democratic spells. According to this measure, countries that have many autocratic regime changes within an autocratic spell (i.e., many successive authoritarian regimes) are more unstable than countries that have no or few autocratic regime changes during a particular autocratic spell—which may be true. But this measure also assumes that every regime within a particular autocratic spell has the same amount of instability, which is less likely. For example, Benin had a series of very unstable authoritarian regimes before Kerekou consolidated power in 1972 and ruled for nearly 20 years, but in Przeworski et al.'s coding, these unstable regimes are grouped into a single authoritarian spell with Kerekou's regime. Because all these regimes (Kerekou's 20-year rule and the series of military and civilian regimes that preceded him) represent a single autocratic spell, they are all coded with the same number of changes in the chief executive and the same number of coups. Yet Kerekou's rule was more stable than the military regimes that preceded him.

Second, the age of an autocratic regime (or spell for that matter) is not a good indicator of the time horizon a dictator faces, because it assumes that all dictators in their first year of rule have the same time horizon. Again, it is unlikely that the military regimes that ruled Benin (each for less than 2 years) from 1963 to 1972 faced the same time horizon as popular independence leaders who had the backing of strong unions (e.g., Kenya and Tanzania) or strong parties (e.g., China) faced during their first years of rule.

Instead of using these measures, I use the predicted probability of regime failure as a proxy for autocratic time horizons. We might assume that autocrats themselves are attuned to the same predictors of leadership survival as researchers.[12] With this assumption, we can build an empirical model of regime survival and calculate the predicted value of survival for each autocrat in every year of rule. These predicted probabilities, based on the observable causes of regime failure, give us a measure of how likely an autocrat is to be replaced in any given year; the greater the perceived probability of failure, the shorter the time horizon. Using the predicted probability of regime failure as a measure of autocratic time horizons and regime stability is less problematic than the other measures, because (a) it captures variation between different authoritarian regimes within a particular authoritarian spell; and (b) it does not assume that all authoritarian leaders, no matter their type, face the same time horizon at the same age of the regime. The drawback to using the predicted probability of regime failure is that we are measuring something that is not directly observable.[13]

To generate the predicted probability of failure for each regime-year, I calculate the probability of a positive outcome from a logit model of regime failure.[14] The model of regime failure, shown in Table 1, includes the following explanatory variables: Log(*GDPpc*), $\text{Growth}_{t\text{-}1,\, t\text{-}2}$, share of the population that is Islamic, $\text{CivilWar}_{t\text{-}1}$, Foreign Occupied, regime-type variables (military, single party, monarchy, single party–military–personalist hybrid, military–personal hybrid, single party–military hybrid, and single party–personal hybrid), and Time Splines to control for duration dependence.[15] This model combines information on these explanatory variables with information on regime failure to produce the predicted probabilities of failure that are used as key variables in the empirical models testing the relationship between foreign aid and economic growth.[16]

To provide an intuitive understanding of the predicted failure probabilities, Figure 2 shows these values for selected countries. The predicted failure probabilities for the two single-party regimes, Botswana and Hungary, are quite low. Hungary has a slight increase in 1956, reflecting the Hungarian uprising suppressed by Soviet intervention and relatively substantial

increases throughout the 1980s as Communist party rule eroded. For each of the brief Argentine military regimes, the predicted failure probability increases the longer the regime is in power. The large spike in the last Argentine regime, in 1982, reflects the regime instability brought on by the Falklands' War. Finally, the predicted failure probabilities for the former Zaire (Democratic Republic of the Congo) are quite volatile. After an initial period of instability, the regime stabilized through the mid-1970s. There is another period of relative stability through most of the 1980s. Beginning in 1990, the data show the destabilizing impact of severe economic crisis on Mobutu's regime, followed by a large spike in 1997, which reflects the conflict associated with Kabila's march through the Democratic Republic of the Congo and eventual overthrow of Mobutu in May of that year. As this graph illustrates, in addition to recent growth, conflict and authoritarian regime type are important factors used to predict authoritarian regime failure.

Method

To estimate the effect of aid on growth, researchers typically use instrumental variables estimation to correct for possibly endogeniety. The instruments used in previous research on aid and growth (Dollar & Burnside, 2004; Easterly et al., 2004) include the following: Central America dummy, Egypt dummy, arms imports, log population, arms imports*policy, log population*policy, log population squared*policy, log GDP*policy, and log GDP squared*policy. The samples that I test below, however, are smaller than previous researchers' samples, because I have restricted my analysis to authoritarian regimes, and the usual list of instruments is not very useful because many of these instruments are not orthogonal to the errors.[17] In particular, the policy instrument and any instrument that interacts with policy is not orthogonal to the errors in the second-stage equation. This should not come as a surprise, because we know that policy (an index composed of inflation, budget balance, and trade openness) is an important determinant of growth. Therefore, we should not use them as instruments for aid; instead, I use orthogonal instruments such as log(GDPpc)-squared, log(population), and dummy variables for military regime, Jordan, and Egypt.[18] The dummy variables for countries that receive large amounts of foreign aid (e.g., Egypt and Jordan) make particularly good instruments for aid if these countries, on average, grow at the same pace as other countries.

Using a two-stage equation to estimate the impact of aid on growth addresses potential selection bias. Donors give foreign aid for strategic reasons. In short, bilateral donors are more likely to give aid to trade partners,

Table 1
Authoritarian Regime Failure

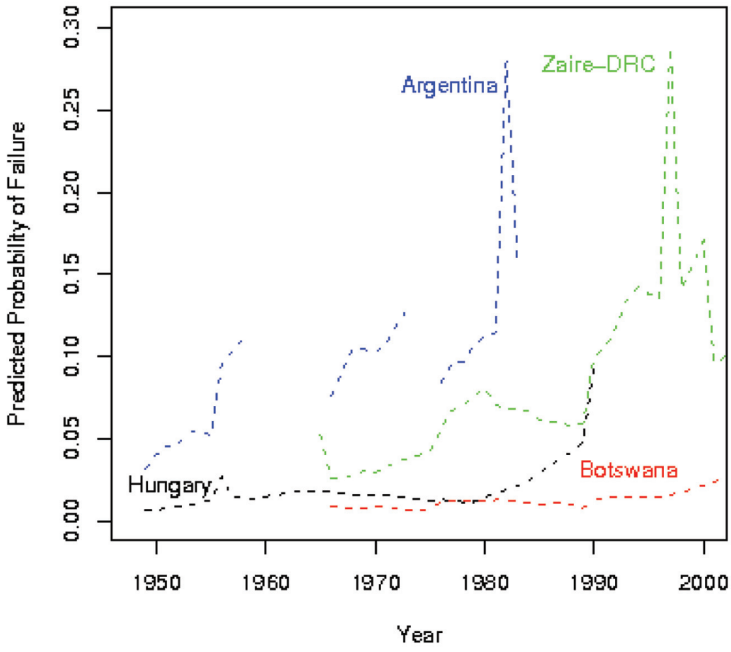
	Original		Updated	
	Beta	*SE*	Beta	*SE*
Log(GDP)	–0.54***	0.159	–0.393***	0.116
Growth	–0.068***	0.025	–0.056***	0.016
Islam	0.012***	0.004	0.003	–0.004
Foreign maintained	1.004	–0.746	–0.839	0.853
Civil War	0.464	0.287	0.437	0.250*
Military	1.31***	0.296	0.831**	0.345
Single party	–1.188***	0.356	–0.897***	0.340
Monarchy			–0.415	–0.651
Single party–personal–military	–1.911***	0.504	–2.1**	1.002
Military–personal hybrid	0.33	0.339	0.205	0.348
Single party–military hybrid	0.276	0.316	–0.34	0.428
Single party–personal hybrid			0.284	0.448
Central Europe	–0.542	0.584	0.084	0.511
Middle East	–0.704	0.691	–2.284*	1.314
North Africa	–1.682***	0.618	–1.156	0.779
Sub-Saharan Africa	–0.632*	0.363	–0.767**	0.386
South America	1.185**	0.491	0.714*	0.410
West Europe	–0.095	0.52	0.583	0.456
Asia	–0.298	0.395		
East Asia			–0.521	0.428
Central Asia			–1.338**	0.632
Constant	0.885	1.249	–0.53	0.908

Note: Standard errors are clustered on regime. Omitted regime type is personalist. Duration splines included but not reported. Original model (N = 2118; % correctly predicted = 85.5; Years covered: 1950-2000). Updated model (N = 2881; % correctly predicted = 90.6; Years covered: 1946-2002).
* p <0.05; ** p <0.01; *** p < 0.001.

former colonies, and strategic allies (Alesina & Dollar, 2000), whereas multilateral donors are more likely to give aid to countries with a good history of growth and political stability (Schraeder, Hook, & Taylor, 1998). Recently, scholars have shown that donors also favor politically salient recipients, particularly countries that sit as rotating members of the U.N. Security Council (Kuziemko & Werker, 2006).[19]

The fact that donors are strategic suggests that the naive models may suffer from selection bias if these factors also affect growth outcomes. In research similar to this study, Vreeland (2003) uses Heckman selection

Figure 2
Authoritarian Time Horizons



Note: Depicts the predicted probability of failure for selected countries, across time (from Table 1, column 3).

models to get unbiased estimates of the impact of IMF program participation on economic growth. In a selection framework, one supposes that the sample of countries receiving aid (or participating in IMF programs) is biased. That is, selection into the sample is correlated with the outcome of interest, which in the present analysis is economic growth. Without correcting for selection, one could get a biased estimate of β. To solve this problem, we estimate a selection parameter λ (really a rescaling of the data to account for nonrandom selection) and add λ to the second-stage equation.

To resolve the endogeneity problem in an instrumental variables/two-stage least squares framework, we estimate first-stage models of the endogenous regressors and calculate the predicted value of the endogenous regressors. If the instruments are exogenous, then we are left with a measure of the endogenous regressor that strips away any of the reverse causation, producing unbiased estimates. Both techniques, instrumental variables/two-stage least squares

and selection models, address potential bias in the β estimates. Thinking of the selection problem first actually helps in choosing instruments for two-stage least squares models, because sifting through potential identifying regressors in the literature on selection for the first stage of a selection model gives us a head start on good instruments for the two-stage models if they are exogenous to the outcome of interest (economic growth).

Time Horizons, Aid, and Growth

In Table 2, I report the results of models using a time horizon measure constructed from the predicted probabilities of regime failure taken from the model in Table 1. Models reported use two-stage least squares to correct for the fact that *Aid* is most likely endogenous to *Growth*.[20] In the first column, I include *Aid* and *P*(*Fail*) but not their interaction. The coefficient for *Aid*, while negative, is small and not statistically significant. The coefficient for *P*(*Fail*) is negative and significant, indicating that more unstable regimes are less likely to grow. In the next six columns, I include the interaction between *Aid* and *P*(*Fail*). The second model is the base model, which I discuss at length below.[21] The third and fourth columns exclude influential observations.[22] The fifth column reports a generalized method of moments estimate, which provided consistent two-stage least squares estimates when there is panel heteroskedasticity.[23] The sixth column substitutes time period dummies for decade dummies. Finally, the seventh column includes additional region dummies.[24]

In columns 2-7 in Table 2, the coefficients for the interaction between *Aid* and *P*(*Fail*) are all negative and significant, indicating that as the probability of regime failure increases, the relationship between aid and growth decreases. The coefficients for *Aid* in all of these models are also positive and highly significant, suggesting that when the probability of regime failure is zero (a very stable regime), the impact of aid on growth is positive. Substantively, a 1.5% of gross national income (GNI) increase in aid is associated with a 1.67% increase in average annual growth. The other control variables are generally in their expected direction: the negative coefficient on *Log*(*GDPpc*) suggests that richer countries grow more slowly; negative coefficients for sub-Saharan Africa and positive coefficients for East Asia; negative coefficients for inflation; and positive coefficients for institutional quality (International Country Risk Guide). The results for *Aid* and the interaction between *Aid* and *P*(*Fail*) are consistent across multiple specifications using the same control variables and roughly the same sample.

Table 2
Aid, Failure Probability, and Growth

Column	(1)[a]	(2)[a]	(3)[a]	(4)[a]	(5)	(6)[a]	(7)[a]
Estimation					Generalized Method of Moments		
Aid	–0.582	1.111***	1.109***	1.118***	1.172***	1.150***	1.144***
	(0.42)	(0.16)	(0.16)	(0.16)	(0.14)	(0.14)	(0.15)
P(Fail)	–24.392***	9.904	9.142	9.493	9.055	10.049	9.083
	(4.54)	(7.08)	(7.09)	(7.17)	(6.67)	(6.62)	(7.25)
Aid*P(Fail)		–40.346***	–39.424***	–39.414***	–42.114***	–41.902***	–38.693***
		(8.37)	(8.17)	(9.09)	(7.57)	(7.35)	(7.60)
Log(GDP per capita)	–1.555**	–1.698***	–1.688***	–1.704***	–1.660***	–1.625***	–1.447*
	(0.55)	(0.48)	(0.48)	(0.51)	(0.47)	(0.46)	(0.60)
Ethnic fractionalization	–0.988	–0.710	–0.600	–0.477	–0.656	–0.732	–1.164
	(0.82)	(0.81)	(0.81)	(0.82)	(0.80)	(0.82)	(0.87)
Assassinations	–0.153	–0.110	0.388	2.566	–0.044	–0.150	–0.221
	(0.59)	(0.54)	(0.79)	(2.23)	(0.52)	(0.53)	(0.52)
Ethnic*Assassination	–0.095	–0.515	–1.357	–4.660	–0.669	–0.532	–0.339
	(1.33)	(1.25)	(1.58)	(3.52)	(1.22)	(1.21)	(1.26)
Sub-Saharan Africa	–2.019*	–1.998*	–2.065*	–1.978*	–2.037**	–1.988*	–1.227
	(0.82)	(0.82)	(0.82)	(0.83)	(0.79)	(0.79)	(1.03)
East Asia	1.050	1.973**	2.015***	2.156***	1.935**	2.028***	2.824**
	(0.62)	(0.60)	(0.60)	(0.61)	(0.59)	(0.59)	(0.86)
Inflation	–2.025***	–2.448***	–2.547***	–2.628***	–2.352***	–2.525***	–2.580***
	(0.61)	(0.64)	(0.63)	(0.65)	(0.63)	(0.59)	(0.60)
Money supply	–0.000	–0.016	–0.017	–0.014	–0.018	–0.019	–0.020
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
International Country Risk Guide	0.547***	0.515***	0.503***	0.467***	0.523***	0.509***	0.439*
	(0.13)	(0.14)	(0.13)	(0.14)	(0.13)	(0.13)	(0.17)
Constant	14.023***	14.412***	14.352***	14.403***	14.177***	13.590***	(dropped)
	(4.21)	(3.62)	(3.65)	(3.86)	(3.55)	(3.55)	
Hansen's J	2.025	1.250	1.341	0.980	1.250	1.543	0.733
p value (J)	0.567	0.741	0.719	0.806	0.741	0.672	0.865
Shea's partial R^2 (Aid)	0.19	0.47	0.47	0.48	0.47	0.47	0.47
Shea's partial R^2 (Fail*Aid)		0.27	0.27	0.25	0.26	0.28	0.26
R^2	0.328	0.394	0.393	0.389	0.394	0.416	0.449
Observations	375	375	370	362	375	375	375
Countries	61	61	60	60	61	61	61
Exclude influential observations	no	no	yes	yes	no	no	no
Decade dummies	yes	yes	yes	yes	yes	no	no
Period dummies	no	no	no	no	no	yes	yes
Region dummies	no	no	no	no	no	no	yes

Note: Robust SE's in parentheses. Instruments in columns 2-7: Military Regime, Egypt, Jordan, Log (GDPpc)-squared, and Log(population). Period, decade, and region dummies not reported.
*p <0.05; ** p <0.01; *** p < 0.001.*
a. Indicates two-stage least squares.

Table 3
Aid, Probability of Failure, and Growth: Possible Omitted Variables

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Aid	1.154***	1.183***	1.055***	1.203***	1.220***	1.092***	1.183	1.080***	1.239***	0.949***
	(0.23)	(0.17)	(0.15)	(0.17)	(0.21)	(0.18)	(1.06)	(0.17)	(0.22)	(0.17)
Aid*p(Fail)	−50.498***	−42.254***	−39.682***	−38.733***	−47.443***	−47.961***	−48.083***	−36.241***	−55.351***	−35.184***
	(10.17)	(8.85)	(8.47)	(8.39)	(10.66)	(10.38)	(14.52)	(9.00)	(11.06)	(9.44)
p(Fail)	35.044*	23.262*	9.604	19.338	10.507	9.095	9.365	5.194	20.498*	12.661
	(14.42)	(10.42)	(6.64)	(10.15)	(8.20)	(7.80)	(9.98)	(7.71)	(9.93)	(7.30)
Log(GDP per capita)	−1.571***	−1.511***	−1.746***	−1.320**	−2.194*	−2.318**	−2.306**	−1.980**	−2.254***	−2.953***
	(0.40)	(0.46)	(0.47)	(0.46)	(0.89)	(0.85)	(0.84)	(0.72)	(0.48)	(0.59)
Ethnic fractionalization	−4.689***	−2.551**	−0.448	−2.368**	−0.712	−1.428	−1.511	−0.716	−1.655	0.463
	(1.42)	(0.83)	(0.80)	(0.81)	(0.93)	(0.86)	(0.89)	(0.76)	(0.95)	(0.74)
Assassinations	−0.294	0.118	−0.130	0.138	−0.113	−0.199	−0.204	−0.019	−0.102	0.003
	(0.62)	(0.62)	(0.53)	(0.61)	(0.54)	(0.50)	(0.50)	(0.58)	(0.62)	(0.68)
Ethnic*Assassination	−0.528	−1.044	−0.463	−1.040	−0.493	−0.215	−0.188	−0.589	−0.557	−0.455
	(1.38)	(1.38)	(1.23)	(1.37)	(1.23)	(1.16)	(1.14)	(1.40)	(1.26)	(1.32)
Sub-Saharan Africa	1.607	−0.117	−2.005**	−0.378	−2.591*	−1.926*	−1.855	−1.999*	−2.250*	−2.924***
	(1.10)	(0.65)	(0.74)	(0.67)	(1.06)	(0.96)	(1.00)	(0.89)	(0.93)	(0.81)
East Asia	3.917***	3.684***	1.564**	4.053***	1.764**	1.447*	1.389*	1.087	2.387***	−0.606
	(0.59)	(0.50)	(0.57)	(0.51)	(0.66)	(0.66)	(0.66)	(0.60)	(0.71)	(0.67)
Inflation		−2.989***	−2.339***	−2.986***	−2.637***			−2.449***	−2.485***	−0.742
		(0.81)	(0.68)	(0.75)	(0.79)			(0.66)	(0.73)	(0.64)
International Country Risk Guide			0.542***		0.583***	0.507**	0.494**	0.412*	0.645***	0.752***
			(0.13)		(0.17)	(0.17)	(0.18)	(0.17)	(0.16)	(0.16)
Money supply				−0.018	−0.023	−0.020	−0.020	−0.037*	−0.016	0.003
				(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Budget balance					7.992					
					(6.67)					
Sachs Warner Index					−0.243					
					(0.56)					
Policy						0.823***	0.932			
						(0.24)	(0.68)			

(continued)

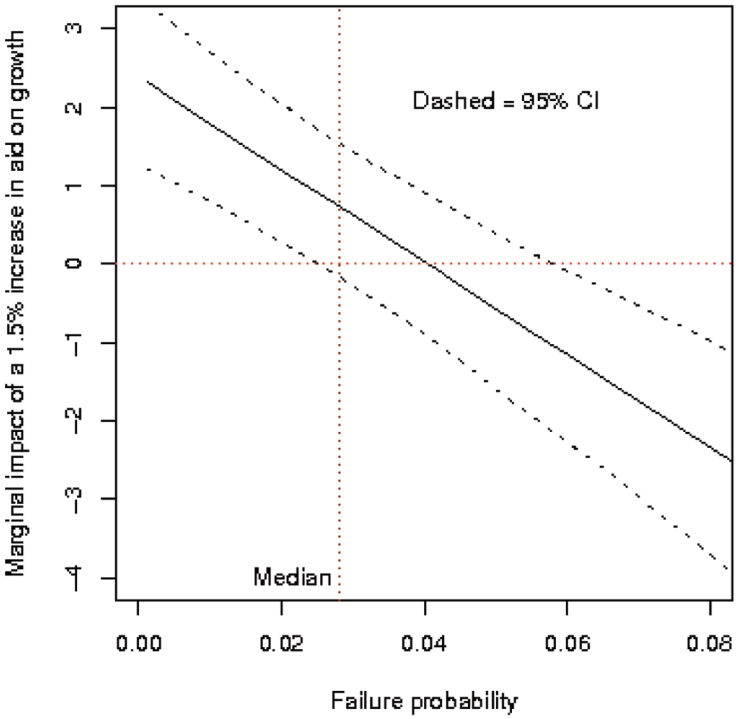
Table 3 (continued)

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Aid*Policy							−0.064			
							(0.42)			
Investment								0.131***		
								(0.04)		
Human capital									−0.367	
									(0.21)	
Capital stock per worker										0.348***
										(0.06)
Constant	15.220***	14.820***	13.937***	13.837***	18.925**	18.145**	17.932**	16.243**	20.481***	18.937***
	(3.30)	(3.74)	(3.62)	(3.64)	(6.49)	(6.09)	(6.14)	(5.36)	(3.53)	(4.25)
Hansen's J	0.879	2.679	1.327	1.048	2.836	1.847	2.693	2.716	2.491	2.210
p value (J)	0.831	0.444	0.723	0.790	0.418	0.605	0.610	0.257	0.477	0.530
Shea's partial R^2: Aid	0.37	0.37	0.47	0.38	0.45	0.47	0.10	0.44	0.43	0.60
Shea's partial R^2: Aid*p(Fail)	0.11	0.14	0.25	0.16	0.20	0.19	0.12	0.18	0.13	0.34
Shea's partial R^2: Aid*Policy							0.11			
R^2	0.566	0.124	0.223	0.200	0.273	0.231	0.338	0.213	0.331	0.447
Observations	506	441	391	423	289	289	289	315	304	234
Countries	75	70	62	69	53	53	53	56	52	49

Note: Robust SE's in parentheses. Instruments in columns 2-10: Military Regime Egypt, Jordan, Population Growth, and Log(Population). Decade dummies included in all models, but not reported.

** p <0.05; ** p <0.01; *** p < 0.001.*

Figure 3
Marginal Impact of Aid on Growth, by Probability of Regime Failure



Note: An increase in aid from 1% of gross national income (GNI) to 2.5% of GNI in a relatively stable regime—P(Fail) = 0.006—yields a 2% increase in growth. This same increase in aid (1.5% of GNI) yields a 2% decrease in growth in unstable regimes—*P*(Fail) = 0.075.

Figure 3 shows the expected impact of a 1.5% (of GNI) increase in aid on economic growth across a range of failure probabilities.[25] In stable regimes, foreign aid has a positive impact on growth. For example, an increase in aid from 1% of GNI to 2.5% of GNI in a relatively stable regime—*P*(*Fail*) = 0.006—is associated with a 2% *increase* in annual average growth. However, in unstable regimes (*P*(*Fail*) > 0.028), the impact of aid on growth is negative. This same increase in aid to a regime where *P*(*Fail*) = 0.075 is associated with a 2% *decrease* in growth. The vertical line in Figure 3 depicts the median failure probability in the sample. The dashed lines depicting the 95% confidence level are greater than zero for the half of the sample to the left of the median line. This suggests that in the most stable regimes, aid increases

growth.[26] In the half of the sample to the right of the median line—the most unstable regimes—the impact of aid on growth becomes more negative. In the most unstable regimes ($P(Fail)>0.06$), the upper bound of the 95% confidence interval is negative, indicating that in these regimes, the impact of aid on growth is negative.

These simulations show that foreign aid is associated with positive growth in authoritarian regimes with long time horizons. This finding is consistent with the hypothesis that stable dictatorships have (a) stronger incentives to invest foreign aid in public goods, (b) fewer incentives to use aid money to buy off or repress regime opponents, and (c) weaker incentives to steal aid as a form of insurance in the event the regime fails. The results also indicate that aid decreases growth in unstable regimes, consistent with the conjecture that aid increases rent seeking in unstable regimes, thereby reducing growth. Thus, the shadow of the future plays heavily into decisions about how authoritarian regimes use foreign aid.

In Table 3, I report the results from estimates that use different sets of control variables. Adding and subtracting various control variables also changes the sample size from as few as 234 to as many as 506. In column 1, I omit controls for inflation, money supply, and institutional quality. In column 2, I include inflation but omit money supply and institutional quality. Next, I include inflation and institutional quality but omit money supply. In column 4, I include inflation and money supply but not institutional quality. Next in column 5, I include two additional controls that are used in the policy index found in much of the literature on aid and growth (Burnside & Dollar, 2000; Dollar & Burnside, 2004; Hansen & Tarp, 2000, 2001): budget balance and the Sachs Warner Trade Index. In the next column, I construct the policy index using inflation, budget balance, and the Sachs Warner Index.[27] In column 7, I include the policy variable and interact this variable with aid and still include the interaction between aid and *p*(fail). The specifications in columns 6 and 7 most closely resemble the models used in the aid, policies, and growth literature (Burnside & Dollar, 2000; Dollar & Burnside, 2004; Hansen & Tarp, 2000, 2001). In columns 8 and 9, I add two additional controls often found in growth regressions: investment and human capital stock. Finally, in the last column, I include the growth of physical capital stock (per worker) as a control. In all these specifications, the instruments meet both the exclusion and exogeneity restrictions.[28] The coefficients for *Aid* and the interaction between *Aid* and $P(Fail)$ reveal the same pattern we saw in Table 2: The *Aid* coefficients are all greater than 1 and highly significant, whereas the coefficients for the interaction are all negative and highly significant.

Table 4
Aid, Probability of Failure, and Growth: Robustness Checks

Model	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Exclude Personalist	Exclude Personalist and Personalist Hybrid	No Legislature	Legislature	No Growth in *p*(*Fail*)	Endogenize *p*(*Fail*)
					No Growth	Endogenize
Aid	1.282***	0.980***	0.901***	1.677	1.269**	1.137***
	(0.21)	(0.13)	(0.16)	(0.90)	(0.47)	(0.18)
p(Fail)	9.415	3.659	−13.523	2.375	7.578	7.281
	(7.08)	(6.36)	(7.99)	(13.38)	(6.81)	(7.07)
Aid**p*(Fail)	−52.905**	−28.889***	−20.748*	−50.991**	−43.817***	−43.367***
	(16.34)	(8.17)	(8.21)	(19.78)	(12.66)	(10.57)
Log(GDP per capita)	−1.419**	−1.431**	−1.371	−1.660*	−1.776**	−1.876***
	(0.53)	(0.51)	(0.78)	(0.85)	(0.59)	(0.56)
Ethnic fractionalization	−2.098*	−1.858*	1.557	−1.091	−0.867	−0.704
	(0.92)	(0.87)	(1.72)	(1.04)	(1.23)	(0.80)
Assassinations	−0.605	−0.651	−0.204	−0.401	−0.223	−0.065
	(0.34)	(0.42)	(0.75)	(0.58)	(0.55)	(0.55)
Ethnic*Assassination	1.043	1.160	0.178	0.231	−0.270	−0.564
	(0.61)	(0.70)	(2.11)	(1.33)	(1.27)	(1.27)
Sub-Saharan Africa	−1.636	−1.543	−2.970*	−2.487	−2.072	−2.105*
	(0.92)	(0.91)	(1.20)	(1.51)	(1.08)	(0.82)
East Asia	2.846***	2.802***	−0.970	2.444**	1.879**	1.864**
	(0.67)	(0.62)	(0.91)	(0.82)	(0.71)	(0.59)
Inflation	−1.129	−1.792***	−0.213	−2.056***	−2.562**	−2.371***
	(0.63)	(0.48)	(1.81)	(0.56)	(0.83)	(0.65)
Money supply	−0.026	−0.020	−0.024	−0.025	−0.012	−0.019
	(0.02)	(0.01)	(0.04)	(0.02)	(0.02)	(0.01)
International	0.451**	0.438**	0.407	0.662**	0.492**	0.536***
Country Risk Guide	(0.16)	(0.15)	(0.22)	(0.22)	(0.17)	(0.14)
Constant	13.248**	12.765***	12.584*	13.659*	15.201***	16.022***
	(4.12)	(3.80)	(6.21)	(6.43)	(4.59)	(4.39)
Hansen's J	2.701	2.075	3.362	3.892	1.234	0.650
p value (J)	0.259	0.354	0.339	0.273	0.745	0.722
Shea's partial R^2: Aid	0.45	0.52	0.75	0.15	0.27	0.47
Shea's partial R^2: Aid*P(Fail)	0.30	0.36	0.37	0.16	0.22	0.19
Shea's partial R^2: P(Fail)					0.49	0.49
R^2	0.302	0.316	0.321	0.350	0.053	0.192
Observations	259	291	108	267	375	375
Countries	48	54	37	55	61	61

Note: TSLS estimation with robust standard errors in parentheses. Instruments: Military Regime, Egypt, Jordan, Population Growth, and Log(Population). Decade dummies included in all models, but not reported.
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

The specifications in columns 6 and 7 also reveal that although good policy is correlated with growth (column 6), there is little evidence that policy mediates the relationship between aid and growth in this sample of dictatorships. Including the interaction between aid and policy also does not alter how the probability of failure conditions the relationship between aid and growth, as the coefficient for the interaction between *Aid* and *P*(*Fail*) is still negative and significant in column 7.

Further Robustness Checks

In this section, I address two additional concerns about the robustness of the empirical findings. First, as suggested by the kleptocratic leaders in Figure 1, the results could be driven by dictators who have few domestic institutional constraints on their behavior. The presence of binding domestic institutions might alter the dictator's incentives in two ways: (a) by constraining confiscatory behavior in the present period and/or (b) by rewarding dictators in the future who refrain from plundering the economy in the present period.

Single-party regimes frequently have regularized mechanisms of leadership turnover that permit the current elite to select future leaders who in turn will be likely to preserve the interests of past leaders. For example, the Mexican president under PRI rule nominated his successor and in doing so preserved immunity from prosecution from any corruption while the PRI ruled (Magaloni, 2006).[29] The practice of compensating outgoing dictators may also be present in military regimes, as evidenced by the constitution bequeathed by Pinochet that preserved a conservative Senate in Chile and guaranteed a certain percentage of future copper profits for military spending (Carey & Baldez, 1999). In short, regimes with regularized, institutionalized turnover may compensate outgoing leaders so that they need not confiscate assets at the end of their term in power.

To test whether the main results are simply due to the behavior of dictators without domestic institutional constraints, I rerun the base model but exclude personalist dictators. Both single-party and military regimes are more likely to have domestic constraints, so I include them. I also include monarchies because, similar to single-party regimes, they have institutionalized mechanisms for leadership turnover—namely, nepotism. In the first column of Table 4, I rerun the base model but exclude personalist regimes.[30] In the second column, I exclude personalist regimes and personalist-hybrid regimes.[31] Finally, the presence of a legislature could constrain the regime (Gandhi & Przeworski, 2006; Henisz, 2000). In columns 3 and 4, therefore,

I test subsamples of the data for regimes with and without legislatures to see if this variable mediates how time horizons affect aid effectiveness.

Second, the models reported in Tables 2 and 3 use *P*(*Fail*), which is constructed using a logit model of regime failure (see Table 1). The explanatory variables in this model include lagged growth. It is therefore possible that this lagged growth used in the construction of *P*(*Fail*) is correlated with the dependent variable in the two-stage models, namely, current growth. Thus, *P*(*Fail*) may be endogenous by construction. In the models reported above, I endogenized the key explanatory mechanism by instrumenting for *Aid* and the interaction between *P*(*Fail*) and *Aid*, but I did not instrument for *P*(*Fail*) itself. To alleviate concerns over the endogeneity of *P*(*Fail*), I pursue two strategies. First, I rerun the logit model of regime failure used to calculate *P*(*Fail*) without economic growth as an explanatory variable.[32] This new calculation of *P*(*Fail*) therefore is not endogenous by construction. I then rerun the base model using this exogenous measure of *P*(*Fail*) and its interaction with *Aid*—reported in column 5 of Table 4. Second, I endogenize *P*(*Fail*) by instrumenting for all three variables [*Aid*, *P*(*Fail*), and *Aid***P*(*Fail*)] in first-stage equations. Again, I rerun the base model with three endogenous variables, reported in column 6 in Table 4.

All the models in Table 4 reveal the same pattern seen previously. The coefficients for *Aid* are all positive and significant, whereas the coefficients for the interactions between *Aid* and *P*(*Fail*) are negative and significant. The main result of this article is robust to exclusion of personalist dictators and in subsamples of regimes with and without legislatures. Finally, the results in the last two columns of Table 4 should alleviate concerns that the endogeneity of *P*(*Fail*) is driving the main results.

Discussion and Conclusion

Not all dictators abuse aid. In fact, the evidence suggests that in authoritarian regimes with long time horizons (a low probability of regime failure), there is a robust positive relationship between aid and growth. The real abusers of aid, it appears, are dictators with short time horizons. This suggests that there is considerable variation within authoritarian regimes over how they use aid, which means that simply giving aid to democracies may miss a valuable opportunity to spur growth in some types of authoritarian regimes.

For many years, the literature on foreign aid effectiveness focused on whether sound economic policies conditioned the use of foreign aid. Policy, as measured in this literature, refers strictly to economic policy outcomes (inflation, budget deficits, and trade openness). Although this research

agenda has largely run out of steam (Easterly et al., 2004; Rajan & Subramanian, 2005), my findings suggest that looking at the political incentives foreign aid recipients face offers insight into whether aid is likely to be correlated with growth. As is well demonstrated in the political economy literature, examining political incentives is central to understanding variation in economic outcomes (North, 1990), as political incentives (including both formal institutional constraints such as electoral systems; Persson & Tabellini, 2000) or legislatures (Gandhi & Przeworski, 2006; Henisz, 2000), and informal incentives such as clientelism (Keefer, 2007; van de Walle, 2003), can effect economic policy outcomes. Understanding these political processes requires that we model the behavior of individuals under different institutional environments (both formal and informal) and measure a variable that shapes individuals' incentives. In this article, I argue that a dictator's time horizon provides him with the strong incentives over the use of aid. Using the predicted probability of regime failure as a measure of authoritarian time horizons, I find evidence that time horizons affect the relationship between aid and growth.

That a dictator's time horizon influences how he is likely to use aid concurs with realists' understanding of how political actors behave: Political survival is paramount (Bueno de Mesquita, Siverson, & Morrow, 2003). Theories that explicitly connect donors' propensity to give aid and recipients' likelihood of accepting it also concentrate on how political incentives and leaders' survival instincts can explain why foreign aid does not simply flow from the richest countries to the poorest (Bueno de Mesquita & Smith, 2007). If foreign aid is to be a tool of cooperation in the international system, where donors trade aid for policy concessions in recipient countries, the findings in this article suggest that donors should think through how the strategies of political survival in recipient countries will affect how leaders use aid. Thus, before committing budget support or extending debt forgiveness (the most fungible forms of aid) to a dictator, donors might first consider how likely it is a dictator will remain in power and then think through how a dictator's time horizon influences the strategies he will use to stay in power. A failing dictator is more likely to plunder the public purse to pay off immanent political threats or to provide himself with insurance should he fall; and public goods spending may likely be the furthest from his mind. The findings in this article suggest that the ability of international actors to influence domestic political outcomes in the recipient country, by trading aid for policy concessions, may be weakest in dictatorships when the time horizon is short. As much as promises from a failing dictator to domestic audiences are not credible, promises from a dictator with a short time horizon to international donors will also not be credible.

Appendix
Variables and Sources

Variable	*N*	*M*	*SD*	Min.	Max.	Source
Growth	375	1.59	3.86	−12.74	16.49	ELR
Aid	375	1.13	1.65	−0.27	12.61	ELR
p(Fail)	375	0.04	0.04	0.00	0.21	Author
Aid**p*(Fail)	375	0.04	0.08	−0.02	0.48	Author
Log(GDPpc)	375	7.40	0.84	5.22	10.58	ELR
Ethnic fractionalization	375	0.50	0.30	0	0.93	ELR
Assassinations	375	0.26	1.00	0	11.5	ELR
Ethnic*Assassination	375	0.11	0.50	0	7.36	ELR
Sub-Saharan Africa	375	0.43	0.50	0	1	
East Asia	375	0.12	0.33	0	1	
Inflation	375	0.16	0.31	−0.04	3.22	ELR
Money supply	375	27.14	18.56	0.05	120.31	ELR
International Country Risk Guide	375	4.27	1.74	1.58	9.50	ELR
Budget balance	309	-0.03	0.07	−0.39	0.48	ELR
Sachs Warner Index	343	0.28	0.44	0	1	ELR
Policy	289	1.50	1.08	−5.35	3.78	ELR
Investment	315	13.52	8.63	1.73	50.56	PWT
Human capital	304	3.68	1.77	0.27	8.93	GDN
Capital stock per worker	234	6.47	4.07	−2.42	20.97	VD
p(Fail)	303	0.05	0.06	0.00	0.41	Author
Aid**p*(Fail)	303	0.04	0.07	−0.01	0.50	Author

Note: ELR = Easterly, Levine, and Roodman (2004); PWT = Penn World Tables; GDN = Global Development Network Growth Database; VD = Vikram & Dhareshwar (1993). Sample (*N* = 375, last year of 4-year panel listed): Angola 2001, Argentina 1969-1981, Burkina Faso 1965-2001, Bolivia 1965-1981, Brazil 1965-1985, Botswana 1977-2001, Chile 1977-1989, China 1989-2001, Ivory Coast 1965-1997, Cameroon 1969-2001, Congo Brazzaville 1989, Congo Brazzaville 2001, Dominican Rep 1965-1977, Algeria 1973-2001, Ecuador 1965-1977, Egypt 1965-2001, Ethiopia 1985-2001, Gabon 1965-2001, Ghana 1965-1993, Gambia 1997-2001, Guatemala 1965-1985, Honduras 1965-1981, Haiti 1965-1993, Hungary 1989, Indonesia 1973-1997, Iran 1981-2001, Jordan 1977-2001, Kenya 1969-2001, Korea South 1965-1985, Kuwait 1973-1989, Morocco 1965-2001, Madagascar 1969-1989, Mexico 1965-1997, Mali 1989, Mozambique 1989-2001, Malawi 1981-1993, Malaysia 1965-2001, Niger 1965-1997, Nigeria 1969-1977, Nigeria 1985-1997, Nicaragua 1973-1989, Pakistan 1965-2001, Pakistan 1969-1985, Panama 1969-1989, Peru 1969-1977, Peru 1993-1997, Philippines 1973-1985, Paraguay 1965-1989, Saudi Arabia 1965-2001, Sudan 1969-2001, Senegal 1969-1997, Singapore 1969-2001, Sierra Leone 1969-1993, El Salvador 1965-1981, Syria 1965-2001, Togo 1969-2001, Thailand 1965-1985, Tunisia 1965-2001, Turkey 1981, Tanzania 1993-2001, Uganda 1989-2001, Uruguay 1977-1981, Congo Kinshasa 1969-2001, Zambia 1973-1989, Zimbabwe 1981-2001.

Notes

1. For the latest round of inconclusive empirical tests, see Rajan and Sabramanian (2005).

2. See Easterly (2006, p. 368).

3. On both formal and informal institutions, see North (1990).

4. Also see http://www.whitehouse.gov/infocus/developingnations/millennium.html

5. See Feyzioglu, Swaroop, and Zhu (1998) and Pack and Pack (1993), for evidence of fungibility.

6. This is not to say that he was not a kleptocrat or that his regime was not extremely repressive. Both of these features of his rule are well documented (Wiarda, 1968, 1975).

7. Trujillo used the Dominican legislature in the 1930s and 1940s as a sort of purgatory to gauge the support of potential rivals. Any cabinet member he suspected of getting too powerful or too contrarian was sent to Congress to demonstrate his loyalty to Trujillo. Wiarda (1975) writes, "[t]he Congress also served as a dumping grounds for out-of-favor Trujillo cronies, as an agency where they could demonstrate their continued loyalty and perhaps be "rehabilitated" and restored to favor" (p. 1262). Over time, turnover in the legislature increased dramatically, as Trujillo's grip on power weakened. Wiarda (1968) notes that during Trujillo's first term, only 2 of 12 senators and 19 of 33 deputies resigned. In his second term, the Senate saw 12 resignations for 13 seats and 46 resignations for 35 lower house seats. In his third term, 32 senators (19 seats) and 122 deputies (42 seats) resigned.

8. The data used below are 4-year panels. Mobutu received the following aid totals, expressed as a share of GDP: 1966-1969 (0); 1970-1973 (1.4); 1974-1977 (1.2); 1978-1981 (1.5); 1982-1985 (1.2); 1986-1989 (2.0); 1990-1993 (1.6); and 1994-1997 (1.3).

9. Investigators had much difficulty tracking the Duvalier family fortune, because before 1979, there were no written annual government budgets in Haiti (Treaster, 1986).

10. One of the drawbacks of using 4-year panels is that these panels are unlikely to capture the long-term growth effects of foreign aid. As an alternative, some scholars use 10-year panels (Rajan & Subramanian, 2005) or disaggregate into short-term aid and long-term aid (Clemens, Radalet, & Bhavnani, 2004). Ten-year panels would not capture many authoritarian regimes that lasted fewer than 10 years; and using the aggregated aid facilitates comparison with the most recent research on foreign aid and growth (Easterly, Levine, & Roodman, 2004).

11. The Policy Index, following Roodman (2004), is constructed using the following equation: \$1.574 + BudgetBalance*(5.919) + Inflation*(-1.889) + SachsWarner*(1.459)\$1.574 + BudgetBalance*(5.919) + *Inflation**(-1.889) + *SachsWarner**(1.459). Using alternative derivations of the Policy Index (again from Roodman, 2004) does not alter any of the results. The policy variable is thus a combination of information on inflation, budget balance, and the Sachs Warner Index on trade openness. As an alternative to the Sachs Warner Index of trade openness, I also tested models that use the level of trade (imports and exports as a share of GDP) as a control. The results remain robust in this specification.

12. Strictly speaking, I use ex post information when I generate the predicted probability of regime failure. For example, the model used to calculate the probability of failure includes information from dictatorships in the 1990s; however, this is not information that dictators such as Duvalier (Haiti, 1971-1986) and Somoza (Nicaragua, 1936-1979) could have known.

13. A further objection to using this measure is that it may be collinear with some of the other explanatory variables (e.g., log(GDP) and growth), because some of the variables were in the survival model used to construct the predicted probability of failure. If present, this multicollinearity might bias the coefficient estimates toward 0. As I discuss below, instrumental variables estimation circumvents potential endogeneity.

14. The data used in this model are updates to Geddes's (1999) data on authoritarian regime survival for different types of authoritarian regimes (military, single party, personalist, and hybrid version of these three prototypes). I updated the original data to include monarchies, such as Saudi Arabia, Morocco, Kuwait, Iran under the Shah, Nepal, Swaziland, and Ethiopia under Haile Salasse. I also updated all regimes to 2002, added regimes (and regime-years) that lasted less than 3 years, and have updated the set of regimes to include new regimes from the old Soviet bloc, such as the Central Asian republics, and Belarus. The predicted probabilities of failure for observations in common to the original and updated data are correlated at 0.71. To check the robustness of this measure, I also used a model of regime failure from Gleditsch and Choung (2004) and added regime type dummies. Using the predicted probabilities of failure from this model instead of from the model in Table 1 yielded results very similar to those reported in Tables 2 through 4.

15. For a discussion of duration dependence in models of regime survival, see Beck and Katz (1998).

16. The dependent variable in the models in Table 1 is regime failure, which includes transitions to a subsequent democracy and transitions to a subsequent dictatorship. Because the dependent variable includes transitions between subsequent dictators, this measure varies considerably from that used in the democratization literature (Acemoglu & Robinson, 2006; Boix & Stokes, 2003; Epstein, Bates, Goldstone, Kristensen, & O'Halloran, 2006; Przeworski, Alvarez, Cheibub, & Limongi, 2000). These studies only model transitions to democracy. To obtain panel averages for the aid and growth data set, I calculate the 4-year average predicted probability of failure for each time period in each country.

17. See Rajan and Sabramanian (2005) for a discussion of the problems posed by using the usual list of instruments. I use Sargan's test for overidentification and Hansen's J-statistic to test whether the instruments as a group are orthogonal to the errors. If the test statistic (reported for each model) is different from 0 (p value < 0.10), we can reject the hypothesis that instruments are orthogonal to the errors. I discard nonorthogonal instruments based on the C or the "difference-in-Sargan" test. See Baum, Schaffer, & Stillman (2002) for further details.

18. Durbin-Wu-Hausman tests (Baum et al., 2002) indicate that the instrumented regressors are endogenous, suggesting that instrumental variables estimation is appropriate.

19. However, other scholars find that aid distribution follows a more altruistic or at least a more competent pattern in that aid to poorly governed countries mostly takes the form of disaster relief, whereas aid to better governed countries is predominantly development aid (Bermeo, 2006). Furthermore, there is evidence from the 1990s that bilateral donors decrease aid in response to antidemocratic behavior in recipient countries (Hyde & Boulding, 2006).

20. The p values of the Hansen J statistics are all larger than 0.10, which indicates that the instruments are exogenous—that is, the errors in the first-stage equations are not correlated with the errors in the second stage. The partial R^2 for *Aid* and the interaction between *Aid* and *P*(*Fail*) are all greater than 0.10, indicating that the instruments explain a substantial part of the variation in the endogenous variables.

21. The coefficient for *P*(*Fail*) in column 2 changes signs once we include the interaction term, suggesting collinearity between *P*(*Fail*) and *Aid***P*(*Fail*). The correlation between *Aid* and *Aid***P*(*Fail*) is 0.67 and between *P*(*Fail*) and *Aid***P*(*Fail*) is 0.39. In a split sample (high *p*(*Fail*) and low *p*(*Fail*)), the results remain robust: a significant, positive coefficient for *Aid* in the low *P*(*Fail*) sample and a significant, negative coefficient for *Aid* in the high *P*(*Fail*) sample.

22. I use the Hadi (1992) method for determining influential observations. The five excluded observations in column 3 are Argentina, 1977; Egypt, 1997; El Salvador, 1981; Mexico, 1997; and Turkey, 1981. In column 4, I exclude the following as well: Algeria, 1997;

Argentina, 1973; the Dominican Republic, 1969; Egypt, 1993; El Salvador, 1977; Haiti, 1993; Nicaragua, 1981; and Syria, 1981.

23. The Pagan-Hall general test for heteroskedasticity indicates that we cannot reject the null hypothesis that the disturbance is homoskedastic: χ^2 p value = .99.

24. Additional regions are Central America/Caribbean (omitted), Central Asia, Central/East Europe, Middle East, North Africa, and South America.

25. The predicted probabilities are from the base model in column 2, Table 2, where I hold other explanatory variables at their mean values, except region dummy variables, which are set to East Asia = 0 and Sub-Saharan Africa = 1. Sample standard deviation for aid is 1.64% of gross national income (GNI). Sample mean for probability of regime failure is 0.04. Sample mean plus one standard deviation is 0.079. Simulations conducted using CLARIFY software. See King, Tomz, and Wittenberg (2000).

26. When I split this sample in two (above and below the median failure probability) and rerun the base model without the interaction between *Aid* and *P*(*Fail*), the coefficient for *Aid* in the sample below the median (the most stable half) is 0.73 (0.063), suggesting that the average impact of a 1.5% of GNI increase in aid to the most stable regimes is an increase in the growth rate of about 1.1%. In the half of the sample above the median (the most unstable regimes), the coefficient for aid is -1.21 (0.58), suggesting that the average impact of a 1.5% of GNI increase in aid to the most unstable regimes is to decrease growth by nearly 2%.

27. See Endnote 10.

28. The p value of the Hansen's J statistic is greater than .10, indicating that the instruments are exogenous. Shea's partial R^2 is greater than .10 for all endogenous regressors, indicating that the instruments explain a substantial amount of variation in endogenous regressors. F tests from the first-stage equations indicate the same.

29. Prosecution of Mexican presidents did not occur until after the Industrial Revolutionary Party lost power in 2000.

30. The regimes that I exclude are the following: Burkina Fasso, 1965-2001; Bolivia, 1965-1969; Cameroon, 1985-2001; Congo Brazzaville, 2001; Congo Kinshasa, 1969-2001; Dominican Republic, 1969-1977; Ecuador, 1969; Ghana, 1985-1993; Haiti, 1965-1985; Iran, 1981-2001; Madagascar, 1977-1989; Malawi, 1981-1993; Mali, 1989; Nicaragua, 1973-1977; Pakistan, 1965-1977; Peru, 1993-1997; Philippines, 1973-1985; Sudan, 1969-1985; Togo, 1969-2001; Uganda, 1989-2001.

31. In addition to those listed in Endnote 30, I exclude the following: Algeria, 2001; Burkina Fasso, 1981; Bolivia, 1973-1977; Chile, 1977-1989; Ethiopia, 1985-1989; Gambia, 1997-2001; Niger, 1977-1997; Nigeria, 1997; Pakistan, 1985-1989, 2001; Panama, 1969-1989; Sierra Leone, 2001; Sudan, 1989-2001 .

32. The original *P*(*Fail*) is highly correlated with the corrected *P*(*Fail*): 0.95.

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Wrong, Michela. (2000). *In the footsteps of Mr. Kurtz: Living on the brink of disaster in Mobutu's Congo*. New York: Harper Perennial.

Joseph Wright is a postdoctoral scholar at Princeton University's Center for Globalization and Governance. His research focuses on comparative political economy in the developing world. His dissertation explains first how foreign aid affects economic growth in dictatorships and democracies and then examines whether foreign aid helps or hinders democratization. His subsequent research looks at how authoritarian legislatures affect economic growth, investment, and regime survival.