

Bypass Aid and Unrest in Autocracies*

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Abstract

Scholars and policymakers have argued that bypassing recipient governments by channeling aid through intergovernmental and nongovernmental organizations can help donors pursue development goals without bolstering autocratic regimes. I argue that bypass aid can subsidize government transfers to citizens and thus reduce popular resistance to autocrats. By providing goods and services that benefit individuals under an existing authoritarian regime, bypass aid may affect citizens' willingness to challenge an incumbent regime. In particular, aid may lower the concessions that an autocrat needs to make to deter unrest or increase the opportunity costs of political resistance. As such, aid may subsidize autocratic rule by depressing the incentive to challenge the regime whether recipient governments have managerial control over aid or not. Statistical tests show that bypass aid is associated with less frequent domestic unrest in autocracies. To address potential endogeneity, I use an original data set that records instances where governments are suspected of or caught misusing aid to proxy for the distribution of aid across channels.

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Research on the political economy of aid in authoritarian regimes focuses primarily on government-to-government aid, or “budget support.” However, donors frequently bypass recipient governments by channeling aid through intergovernmental or nongovernmental organizations (IGOs and NGOs) and providing “in-kind” aid like food and medicine directly to citizens in recipient countries. For example, roughly 60% of the \$700 million Haiti received in development assistance in 2008 went to NGOs, multilateral organizations, or public-private partnerships (Dietrich 2013, 698). Nearly 80% of \$2.5 billion in foreign aid to Sudan in 2009 bypassed government channels (OECD 2014). By helping to ensure that aid reaches its intended beneficiaries, bypassing might achieve development goals more effectively than budget support in situations where donors worry that recipient governments will not use aid to promote citizen welfare (Dietrich 2013, 699; Ear 2013; Acht, Mahmoud and Thiele 2015). Are there domestic *political* implications of bypass aid? Those who warn that aid can stabilize autocratic regimes limit the scope of their arguments to budget support (e.g., Bueno de Mesquita and Smith 2009, 315; Morrison 2009; Kono, Montinola and Verbon 2015; Ahmed and Werker 2015). That aid often bypasses corrupt autocrats brings into question the empirical relevance of this connection (Dietrich 2013, 705). Some contend that the mechanisms that link aid to political outcomes like autocratic survival, democratization, and unrest do not apply to non-fungible aid (e.g., Altincekic and Bearce 2014, 21; Bermeo 2016, 7). But bypass aid may not be politically neutral given that other types of non-government foreign income like remittances can help autocrats by increasing complacency among citizens (Ahmed 2012, 148).

This article develops and tests hypotheses on the relationship between bypass aid and one dimension of political life in authoritarian countries: the occurrence of political unrest. While explaining and predicting episodes of political unrest is important on its own, unrest also has implications for autocratic survival and democratization (e.g., O’Donnell and Schmitter 1986, 53-54; Kono, Montinola and Verbon 2015, 410). I argue that insofar as bypass aid improves short-term welfare, it (1) lowers the concessions that an incumbent regime would have to make to deter unrest and (2) raises the opportunity costs to citizens of participating in acts of resistance. Whereas existing work seems to suggest that autocrats should be wary of bypass aid for its potential to sustain rebel movements (Nunn and Qian 2014, 1632-1633; Polman 2010, 111), this article argues that bypass aid can benefit authoritarian regimes indirectly by reducing domestic unrest. Assuming aid can even minimally advance development or humanitarian goals in the short-term, it may simultaneously act as a political subsidy to autocrats.

Using data from the Organization for Economic Cooperation and Development Creditor Reporting Services data base (OECD 2014) throughout the period of 2005-2010, I show

that aid delivered through bypass channels – whether measured as a population-weighted sum of bypass dollars or as a ratio of bypass aid to total aid – correlates with less frequent domestic unrest in autocracies. This finding persists across various model specifications that account for confounding factors, country- and year-fixed effects, and alternative measures of bypass aid. However, corrupt governments where individuals avoid protesting due to the government’s latent repressive capacity may be more likely to receive aid through bypass channels. To address endogeneity, I collected data on international aid scandals – instances where media sources reveal a recipient government’s misuse of aid – to identify exogenous variation in bypass aid. When donors observe aid scandals, they should be more inclined to deliver aid through bypass channels. Thus, scandals should act as exogenous shocks to the overall ratio of bypass aid to non-bypass aid. I use aid scandals that occurred outside a given country in an instrumental variables analysis and find further support for the argument.

The article proceeds as follows. The second section briefly reviews literature on the political effects of aid in recipient countries. In the third section I explain why aid that bypasses recipient governments should be politically relevant in terms of reducing domestic unrest. The fourth section describes the research design I use to test two hypotheses on the relationship between bypass aid and domestic unrest. The fifth section presents and interprets the baseline statistical results. The sixth section describes the aid scandals data and the justification for using the aid scandals data and presents the results of the instrumental variables models. The seventh section concludes.

Political Effects of Aid in Recipient Countries

A central agenda in the aid literature focuses on the question whether or not aid undermines democracy by weakening political accountability and popular pressure for reform. Many argue that it does (e.g., van de Walle 2001; Bräutigam and Knack 2004, 255-56; Djankov, Montalvo and Reynal-Querol 2008, 170; Kono and Montinola 2009; Ahmed 2012, 150-51; Ear 2013; Kono, Montinola and Verbon 2015, 412). Following arguments that link the historical development of democracy to elites’ needs to tax citizens (e.g., Tilly 1990), aid skeptics contend that, “non-tax revenue enables leaders to forgo taxing the citizenry, resulting in a decreased demand for representative democracy and good governance” (Dietrich and Wright 2013, 60; Djankov, Montalvo and Reynal-Querol 2008; Morrison 2009, 113; Collier and Dollar 2004, F263). Thus, when aid flows substitute for tax revenue, governments may relax demands on citizens, weakening the incentive to mobilize for democratic accountability. Alternatively, aid might allow autocrats to increase repressive military capacity and deter citizens from demanding accountability (Kono, Montinola and Verbon 2015). Yet many

are less pessimistic (e.g., Alesina and Dollar 2000; Wright 2009; Kersting and Kilby 2014). Alesina and Dollar (2000, 56) argue that donors can incentivize democratization with the promise of future aid, though Wright (2009, 554) suggests this only applies to autocratic leaders likely to retain office following a democratic transition. Some studies find no effect of aid on democratization (e.g., Knack 2004; Altincekic and Bearce 2014), and others argue that cash-transfers to a recipient government may actually encourage challenges to authoritarian regimes (e.g., Grossman 1992). A primary source of disagreement concerns the different research design strategies scholars use to test these competing theories (Wright 2009, 553).

This debate focuses mainly on government-to-government aid, though aid increasingly flows through NGOs and UN agencies that operate relatively autonomously within recipient countries (e.g., Riddell 2007, 259; Büthe, Major and Souza 2012, 572; Dietrich 2013, 698). Recent literature on aid allocation helps explain variation in donors' decisions about delivery channels (e.g., Milner 2006; Dietrich 2013; Acht, Mahmoud and Thiele 2015), but few have considered the political consequences of bypassing recipient governments.¹ And, while scholars usually emphasize budget support in theorizing about possible connections between aid and democratization, they rarely distinguish between budget support and other forms of aid in statistical tests.²

This matters for two reasons. First, corruption – a defining feature of autocracy – strongly predicts bypassing (Dietrich 2013, 705; Acht, Mahmoud and Thiele 2015, 26; Ear 2013, 10). Second, theoretical mechanisms linking aid to autocratic stability treat aid as a cash transfer between governments (Bueno de Mesquita and Smith 2007, 2009).³ As Altincekic and Bearce (2014, 21) point out, existing aid-democratization theories do not obviously apply to less fungible aid.

We know little about the political effects of bypass aid in autocratic regimes. Some speculate that bypass aid could stymie political reform (Dietrich 2013, 708; Bueno de Mesquita and Smith 2009), and case studies suggest that bypass aid sometimes undermines democracy (e.g., Ear 2013; de Waal 1997). Others see bypassing recipient governments as a way to increase aid effectiveness, or at least as a reason to doubt the existence of a political aid curse

¹Other literature considers many different conceptual dimensions of aid, including the choice of bilateral versus multilateral channels and the purpose for which donors give aid (Alesina and Dollar 2000; Neumayer 2003; Dollar and Levin 2006; Milner 2006; Dreher, Sturm and Vreeland 2009; Fink and Redaelli 2011; Milner and Tingley 2012; Dreher, Fuchs and Nunnenkamp 2013; Heinrich 2013; Fuchs, Dreher and Nunnenkamp 2014; Kevlihan, DeRouen and Biglaiser 2014).

²Other sub-literatures do distinguish between different types of aid. For example, in the aid-growth literature, some disaggregate aid based on whether they expect it to have short- or long-term effects on growth (e.g., Clemens, Redelet and Bhavnani 2012).

³Literature on democracy aid represents one exception. This literature finds some evidence that democracy aid promotes democratic governance in recipient countries (Goldsmith 2001; Dunning 2004; Finkel, Pérez-Liñán and Seligson 2007; Scott and Steele 2011; Dietrich and Wright 2013).

(Altincekic and Bearce 2014; Bermeo 2016). Altincekic and Bearce (2014, 19) consider the role of aid in the context of Acemoglu and Robinson’s (2001) theory of democratization and assume that aid presents no strategic advantage to a recipient government absent government control. Similarly, Bermeo (2016, 9) argues that while “non-fungible” aid may provide utility to a recipient government, it does not harm the prospects for democratization. The next section reconsiders the political relevance of aid that bypasses recipient governments.

Why Bypass Aid Should Not Be Politically Neutral

I consider the political effects of bypass aid in the context of economic theories of revolution (e.g., Acemoglu and Robinson 2001).⁴ Acemoglu and Robinson (2001) analyze a repeated game between a government and an opposition group where the government decides what proportion of resources to allot to the opposition in a given round, and the opposition decides whether or not to oust the government from power. Though the model emphasizes revolution and democratization as outcomes, it serves as a useful heuristic for analyzing actors’ incentives in any bargaining situation with costly outside options. I consider the effects of bypass aid on political unrest.⁵ Popular resistance, both violent and non-violent, can sometimes exert pressures on elites to democratize or can constrain the direction of institutional change (Bermeo 1997, 314; Stephan and Chenoweth 2008; Teorell 2010). O’Donnell and Schmitter (1986, 18) point out that “liberalization and eventual democratization of authoritarian regimes may have its conspiratorial side, but it also involves, ... a crucial component of mobilization and organization of large numbers of individuals.” They note that, “[i]n Greece, Peru, and Argentina circa 1970, the ‘decision to open’ was heavily influenced by the presence of strong opposition forces in the civilian population” (O’Donnell and Schmitter 1986, 19-20). In the context of institutional or leadership change, “[m]ass protests, riots, strikes, uprisings, and assorted terrorist actions are often prominent features of crises, helping to shape their severity and paths” (Dogan and Higley 1998, 7). Unrest can also be a direct mechanism through which citizens exert policy influence in otherwise non-democratic governments (Kono, Montinola and Verbon 2015, 410).

In connecting bypass aid to political unrest, I make two key assumptions. First, I assume that exerting political pressure is costly. It requires overcoming collective action problems (Olson 1965), it may have rippling economic effects of deterring foreign investment, or it may lead to wealth-destroying conflict. The outcome of an organized effort to pressure a regime is uncertain – the movement may succeed, or may be quashed by the government. The

⁴This approach follows Altincekic and Bearce (2014).

⁵See Kono, Montinola and Verbon (2015) for a similar approach.

crackdown on Egyptian protestors in late 2011 during the Tahrir Square protests represents one recent example of the latter outcome (Thomas 2016).

Second, I assume that aid achieves at least *some* of its intended goal in terms of improving human welfare in the short term. International actors routinely provide services that directly benefit vulnerable populations in autocratic countries. For example, the World Food Programme provides “social safety nets” by supplying “the most vulnerable with predictable support in the form of food, goods, cash or vouchers” (World Food Programme 2017). It provides these and other services in a number of non-democratic countries, including Central African Republic, Zimbabwe, Cameroon, Iran, North Korea, Tanzania, and Togo, among many others.⁶ Similarly, USAID supplies services like education and nutrition to citizens in Tanzania (USAID 2017) and access to water and sanitation in Angola (USAID 2016). Though a comprehensive analysis of bypass aid effectiveness is beyond the scope of this article, it seems reasonable to assume that bypass aid provides some immediate benefit to citizens in receiving countries. Existing evidence suggests that bypass aid can have welfare-improving effects. One study by the International Monetary Fund found that NGO aid reduced infant mortality more effectively than bilateral aid (Yontcheva and Masud 2005). Experimental evidence from Uganda shows that citizens sometimes prefer public goods delivered through international aid groups to government-funded programs (Milner, Nielson and Findley 2016).

These assumptions imply that bypass aid should matter in the context of the economic theories of political transition. A citizen’s willingness to incur the risks of political action under autocracy depends on the benefits she receives in the status quo and her expected future benefits. As noted, government-level mechanisms through which aid might affect these considerations include (1) funding government repression, (2) subsidizing public goods provision that appeases citizens or, relatedly, (3) providing the government with additional resources that reduce the need for taxing citizens. If the government can decrease the likelihood of a successful revolution, or can implement programs or policies that make revolution unnecessary, the frequency with which people become dissatisfied enough to incur the costs of political action should decrease. While aid may influence the incentives and behavior of a recipient government, the opposition’s incentives also determine equilibrium outcomes. Focusing on state capacity provides only a partial view of the relevant incentives (e.g., Collier and Hoeffler 2004). Factors that improve citizen welfare should discourage demands for reform by increasing the opportunity costs of demonstrating.⁷

⁶See <http://www1.wfp.org/countries/> for information on the specific activities of the WFP.

⁷Focusing specifically on civil war in Sierra Leone, Humphreys and Weinstein (2008, 452) argue that poorer individuals were more likely to participate in hostilities due to “greater vulnerability to political manipulation by political and military elites, a greater frustration with more peaceful forms of protest, or

Where governments fail to provide goods, people may accept the costs of unrest if it means a potential change from the status quo. For instance, “[i]n the second half of the nineteenth century, by its reluctance to take on responsibility for famine relief, the British Government discredited itself and so nurtured the nationalist movement [in India], which in turn sought to use famine as an issue for mass mobilization” (de Waal 1997, 12). Mwenda’s (2006:2) analysis of the effects of foreign aid in Uganda points out that services provided with the help of international donors like “primary education, free basic health care, and infrastructure rehabilitation and maintenance,” have historically allowed the Ugandan government to avoid the tough decision of pursuing democratic reforms or facing collapse. Similarly, Ear (2013, 12) argues that aid has had undermined democratization in Cambodia by “diluting political will.” Okruhlik (1999, 300) makes an analogous argument in pointing out that excluding groups from the distribution of rents in society can fuel resentment that leads to unrest. She traces riots in the Eastern province of Saudi Arabia in 1979-80 partly to the fact, that during this time, “schools and hospitals were few, and [...] infrastructure was inadequate” (Okruhlik 1999, 3000). Some critics of aid in Ethiopia point out that “in-kind” aid like seeds, fertilizers, and food aid – including services often provided by international NGOs (USAID 2017b) – may increase the opportunity costs of voicing opposition to the Ethiopian government, leading to reluctant or tacit support for the regime (Human Rights Watch 2017). Though the Ethiopian government plays some managerial role in delivering foreign assistance and can restrict access to aid, this highlights that the benefits of aid and the prospects of forfeiting some or all of those benefits by challenging the government do factor into citizens’ political behaviors. These examples suggest that aid does not need to fund military repression or feed directly into the government’s budget to deter political resistance; it can do so at the same time that it achieves its immediate goals of improving lives. Channeling aid directly to people increases the value of the status quo and should decrease the likelihood that a non-democratic government faces resistance.

Insights from related literatures reinforce this point. For instance, Ahmed (2012, 150-51) argues that remittances, a source of income over which governments have little control, can help autocrats by improving citizens’ wellbeing under the incumbent regime.⁸ Uribe and Buss (2007, 398) draw a direct comparison between aid and remittances in arguing that, “humanitarian aid and remittances complement one another by letting governments off the hook,” weakening demands for accountability in countries like Haiti. Recent research on violence and terrorism targeted at aid groups also assumes that aid can have basic welfare-

most simply, a lack of other options.”

⁸As Altincekic and Bearce (2014) point out, Ahmed’s (2012) statistical tests combine government-to-government aid with remittances into one measure of “unearned foreign income.”

enhancing effects. Narang and Stanton’s (Forthcoming) study of attacks on aid groups in Afghanistan highlights how services that non-state aid organizations provide may undermine the incentive for civilians to join in challenging the government. Crost, Felter and Johnston (2014) argue similarly that insurgent groups in the Philippines have tried to sabotage aid projects to prevent complacency among the population. Murdie and Stapley (2014, 81) argue that terrorist groups direct violence against NGOs when those NGOs “influence the behaviors of potential terrorist group supporters in ways not liked by terrorist organizations.” In a recent working paper, Weezel (2015, 21) analyzes conflict areas and finds that “non-fungible” aid correlates with decreased civil conflict, a result of aid increasing the opportunity costs of engaging in conflict. These articles study attacks against international aid groups and/or civil conflict, but the underlying logic motivating their hypotheses comports with the logic here: people are better off when they receive international aid, and that affects their willingness to engage in political action. This provides some additional albeit indirect evidence of the plausibility and implications of the argument that bypass aid may have pacifying effects.

Of course, aid might have heterogeneous effects. While bypass aid may remove a source of grievance, it may simultaneously empower or keep afloat groups with the capability to challenge the government. Sometimes this may even be a strategic objective of donors. For example, following the Russian Civil War, the Hoover administration in the United States tried to use humanitarian aid to undermine the Bolsheviks (Belgrad 1997, 4).⁹ Polman (2010, 111) details a number of international aid efforts that inadvertently financed rebel groups (“refugee warriors”), thus allowing them to oppose governments.¹⁰ If aid dramatically affects the standing of the opposition relative to the government, it might lead to commitment problems and power shifts that can lead actors to make greater bargaining demands, resulting in conflict (e.g., Fearon 1995; Powell 1999). Yet unlike civil conflict, domestic unrest entails the participation of ordinary citizens. When it comes to popular unrest, the pacifying effects of bypass aid likely outweigh, or at least offset, any empowering effects, on average. First, aid that provides basic services and skills may simultaneously provide a less-tangible source of utility for citizens in a recipient country, raising the opportunity costs of demonstrating. While rebel groups may remain committed to violence, ordinary citizens should be more easily deterred from joining in acts of resistance. Second, and more importantly, autocratic

⁹ “[B]y creating a food and medicine distribution system outside the control of the Soviet government, Hoover hoped to establish an alternative power center through which the loyalty of the recipient populations, weakened as they were at the end of the civil war by massive starvation and disease, might be turned against the Bolsheviks” (Belgrad 1997, 4).

¹⁰ Additionally, governments may host humanitarian aid operations that pose threats to their neighbors. Polman (2010, 111) provides a number of examples, including Angola/Namibia, Libya/Chad, and Kenya/Somalia.

governments may screen and reject aid that contributes meaningfully to an opposition movement’s ability to challenge the government. Governments may not perfectly anticipate aid will have this effect, but they have the ability to expel international or restrict the activities of aid groups when they desire (e.g., Russia’s expulsion of USAID in 2012). Additionally, donors often require that NGOs “be program-oriented and apolitical” to abide by domestic laws in host countries (Jalali 2013, 58). For these reasons, the welfare-increasing effects of bypass aid should generally outweigh any positive effects on unrest.

H1: As the amount of bypass aid in a country increases, the frequency of domestic unrest events decreases.

By improving popular welfare under the status quo, bypass aid should weaken the incentive for unrest. Yet some argue that government aid serves not only as a tool for repression, but might simultaneously attract challenges to a regime. Citizens may take into account what they could get by taking control of the government or pursuing concessions that give them greater discretion over government decisions. Knack (2004, 253), citing Grossman (1992), points out that, “[a]id may also encourage coup attempts and political instability, by making control of the government and aid receipts a more valuable prize.” Deaton (2013, 299) echoes this point, and Moyo (2009, 59) claims that “[t]he prospect of seizing power and gaining access to unlimited aid wealth,” largely explains the intractability of civil conflict in Africa throughout the 1990s. Similarly, Qian (2015, 282), citing Garfinkel (1990) and Besley and Persson (2011) notes that, “[s]tandard contest models of conflict predict that increased revenues can increase conflict in equilibrium because they increase the returns to controlling the government,” but that this effect should only obtain in the “presence of weak institutions or a nonrepresentative government.”

In the context of Acemoglu and Robinson’s (2001) model, the opposition only potentially enjoys benefits of government aid when it presents a credible challenge to the government. In contrast, the government always enjoys the benefits of aid (minus whatever it must transfer to make the opposition indifferent between challenging and acquiescing). As the quality of life under the incumbent regime improves, the need for political action to access the benefits of government aid declines. This implies that, all else equal, the distribution of aid should affect citizens’ incentives to seek political concessions. Taking the overall aid portfolio in a recipient country as given, as a greater proportion of aid flows directly to the population of a recipient country, aid to the government contributes less to the relative value of controlling the government. This implies an additional hypothesis.¹¹

¹¹In Section A of the Supplementary Files, I formalize the argument in the context of Acemoglu and Robinson’s (2001) model to show that the ratio of bypass aid to total aid should affect the incidence of unrest as described above.

H2: As the ratio of bypass aid to total aid in a country increases, the frequency of domestic unrest events decreases.

In sum, by raising citizens' opportunity costs of resisting an incumbent autocratic regime, bypass aid can act as a political subsidy to autocrats, deterring mass unrest. All else equal, this should be the case due to the welfare-enhancing effects of aid. Additionally, as more aid flows outside government channels, the incentive to pressure the government for policy reform or to attempt to gain control of the government should be less tempting. These arguments imply together that bypass aid should be associated with less frequent acts of unrest in autocracies. The next section develops a research design to evaluate these hypotheses.

Research Design

Testing the theory requires data on autocratic regimes, domestic unrest, and aid. Following recent work on the effects of aid on government survival and democratization (e.g., Wright 2009), I use country-year-level data on OECD-aid-receiving authoritarian regimes from Geddes, Wright and Frantz (2014). The data set includes information on 61 autocratic countries from 2005 to 2010.¹²

I use Kono, Montinola and Verbon's (2015) indicator of domestic unrest as my dependent variable. This variable counts strikes involving more than 1,000 people directed at the government or national policies, anti-government demonstrations involving more than 100 people, and riots involving more than 100 people from Banks (2011).¹³ Because the Banks (2011) data relies on media coverage, some justifiably worry that reporting practices might differ across countries due to variation in press freedom (e.g., Bueno de Mesquita and Smith 2010, 941). Since my sample includes only autocracies, this specific concern should be less problematic. Still, I also report results for a dichotomous indicator of unrest, which should be less susceptible to extreme variation. Both measures include riots, though the description of the riots count variable in Banks (2011) does not specify that the riots need to have anything to do with government policies. Table 6 in Section D of the Supplementary Files re-estimates my key models while excluding riots from the count variable and shows that the results are qualitatively similar to the measures that include riots. Still, including riots seems reasonable in that it may capture the willingness of citizens to take action in the face

¹²The names of the countries appear in Table 5 in Section C of the Supplementary Files.

¹³The overlap in temporal bounds for the aid and autocratic regime data sets leave only six years – 2005 through 2010 – for testing implications of the argument for regime failure (after lagging the independent variables by one year). Using an indicator of unrest rather than regime failure allows me to capture more variation in a measure relevant for autocratic survival.

of the failure of government policies (for example, food riots), and riots may trigger political change, as noted above (e.g., Dogan and Higley 1998, 7).

To measure bypass aid, I use the OECD’s Creditor Reporting System (CRS) data base. Beginning in 2004, the OECD CRS database reports amount of aid that OECD donors delivered through public channels, NGOs and civil society, multilateral organizations, public-private partnerships, and “other” channels (OECD 2014). Following Dietrich (2013), I include aid channeled through NGOs and civil society, multilateral organizations, and public-private partnerships in the category of bypass aid. I use gross disbursements of Official Development Assistance for all types of aid to all sectors from all donors in constant 2012 US dollars.¹⁴ I use this measure to create (1) a variable that divides total bypass aid by population, and (2) a measure of the ratio of bypass aid to total aid. I refer to these measures as *bypass aid per capita* and the *bypass ratio*, respectively. These measures allow me to capture the distinction between *H1* and *H2*. The population data for the *bypass per capita* measure come from the Penn World Tables (Heston, Summers and Aten 2012). I take the natural log (plus one) of the *bypass per capita* measure to reduce the influence of outlying observations. I multiply the *bypass ratio* by one hundred so that the coefficient estimates are interpretable as change in the conditional expectation of unrest events for a one-percentage point increase in the bypass aid ratio (e.g., from 0% bypass aid to 1% bypass aid).¹⁵

I control for potential confounders that may affect both unrest and the distribution of aid across channels in a recipient country.¹⁶ Because more corrupt countries may be more likely to receive bypass aid and less likely to face mass unrest, I follow Dietrich (2013) in using the World Governance Indicators data set (World Bank 2013) to create a measure of governance quality ranging between 0 and 5, with greater values indicating better governance. Involvement in civil conflict may lead donors to choose bypass to avoid directly aiding openly belligerent governments, and conflict may affect unrest. Alternatively, aid

¹⁴I accessed the OECD CRS database on April 7, 2015. The data base allows users to select either “Commitments” or “Gross Disbursements,” but not “net” disbursements.

¹⁵The OECD database does not necessarily provide enough specific information to determine where aid actually ended up – i.e., whether it went to the government or went directly to people – in a given case. The OECD coding scheme notes that “[t]he channel of delivery is the first implementing partner. It is the entity that has implementing responsibility over the funds and is normally linked to the extending agency... by a contract or other binding agreement, and is directly accountable to it” (OECD 2010, 2). Some aid might be labeled as bypass aid even if the loans ultimately end up going to the government. This same critique might apply to NGOs that work closely with recipient governments. However, in many other cases multilateral or NGO aid does bypass governments. For example, the United Nations Refugee Agency conducts much of its work independently. Detailed information on the eventual destination of aid given through these channels would be ideal. Absent that, this measure represents a reasonable approximation of the theory and follows previous research (e.g., Dietrich 2013).

¹⁶I lag all independent variables by one year to alleviate concerns about reverse causality. Since I am interested in evaluating the relationship between bypass aid and domestic unrest rather than developing an explanatory model of unrest, the primary model specification focuses on potential confounders.

through government channels may increase in times of civil conflict if third parties attempt to bolster friendly regimes. I control for a count of civil conflicts in the previous year using the UCDP-PRIO Armed Conflict Dataset v.4-2014 (Gleditsch et al. 2002). Finally, disasters might lead to more aid being delivered through specialized NGOs (or to governments weakened by a disaster) and disasters may affect unrest in autocratic systems (Quiroz Flores and Smith 2013). I control for a count of natural disasters from the International Disaster Database (EM-DAT 2009). I account for unobserved heterogeneity within years and for the possibility that countries have different baseline propensities of protest or norms about protest by including country- and year-fixed effects. I present models with and without these fixed effects.¹⁷ Table 1 reports summary statistics for the main variables in the analysis.

Table 1: Summary Statistics for Key Variables

Statistic	N	Mean	St. Dev.	Min	Max
Unrest Events	344	0.7	1.9	0	18
Bypass Aid/Capita (log)	332	0.01	0.02	0.0	0.2
Government Aid/Capita (log)	332	0.02	0.02	0.0	0.2
Bypass Ratio	344	15.2	17.1	0.0	79.6
Civil Conflicts	350	0.2	0.6	0	4
Natural Disasters	350	2.3	4.1	0	37
Governance Index	350	2.0	0.7	0.8	4.5

Note: Data sources described in text.

Results

Figure 1 plots (1) bypass aid per capita and (2) the proportion of aid delivered through bypass channels in a given country against the count of unrest events in the subsequent year. A casual look at these scatter plots suggests that there may be a negative relationship between bypass aid and unrest.¹⁸ To be more rigorous, Table 2 reports the results of seven regression models.¹⁹ The key independent variables are bypass aid per capita and the proportion of aid

¹⁷Ahmed (2012, 156) takes a similar approach to deal with the potential problem of inconsistent estimates when there are many group intercepts in a small sample as Chamberlain (1980) identifies.

¹⁸The figures also highlight the possibility that outlying observations could be influential in the estimations below. The results are robust to excluding extreme values on both the key independent variables and the dependent variable.

¹⁹I follow Kono, Montinola and Verbon (2015) in using negative binomial regression models rather than a Poisson model throughout the article, though the results are robust to using a Poisson model instead. Results for logit models using a dummy indicator of unrest and the results of zero-inflated negative binomial regression models appear in Sections E and F of the Supplementary Files.

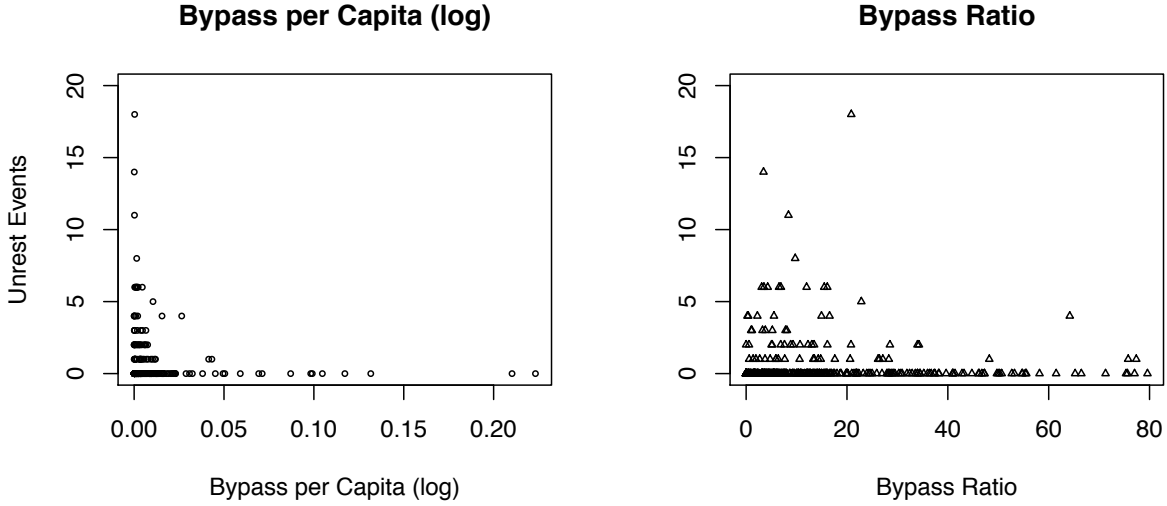


Figure 1: Scatterplots of Bypass Aid and Domestic Unrest, 2005-2010

delivered through bypass channels.²⁰ The models in Table 2 consistently show a negative association between the bypass aid and the occurrence of domestic unrest. In Models 1 and 2, the relationship between bypass aid and domestic unrest is negative and statistically significant. Models 3 and 4 focus on the bypass per capita measure and add country- and year-fixed effects, and then an additional measure for the total amount of government aid per capita (i.e., non-bypass aid). Again the coefficient on bypass per capita is negative and statistically significant. Interestingly, in Model 4 the effect of total government aid on unrest is positive and statistically significant, highlighting possible heterogeneous effects of aid on unrest. This finding comports with the expectations of those studies that see potential for government aid to attract challenges to a regime (e.g., Garfinkel 1990; Grossman 1992), and seems inconsistent with arguments that autocrats use aid to invest in repression to deter unrest (e.g., Kono, Montinola and Verbon 2015). However, many studies on the political effects of aid use different temporal domains that are not limited by the availability of the OECD CRS data, so it would be premature to interpret this finding as somehow invalidating previous research. Still, it suggests that accounting for the distribution of aid across government and bypass channels matters. Certainly future research should explore the effects of government aid on unrest further.

The negative coefficient on bypass aid holds when adding country- and year-fixed effects to the model that focuses on the bypass ratio (Model 5). In Models 6 and 7, I replace the dependent variable with a dichotomous indicator of whether or not at least one unrest event

²⁰Varying coverage in the population data, which weights the measures of bypass aid in Models 1, 3, 4, and 6, explains the difference in observations across the models.

Table 2: Bypass Aid and Domestic Unrest, 2005-2010

	Dependent Variable: Unrest Events						
	<i>Model 1</i>	<i>Model 2</i>	<i>Model 3</i>	<i>Model 4</i>	<i>Model 5</i>	<i>Model 6</i>	<i>Model 7</i>
Bypass Aid / Capita	-58.55*** (22.46)		-40.43** (16.34)	-40.56** (16.16)		-1.34* (0.80)	
Bypass Ratio		-0.03*** (0.01)			-0.03** (0.01)		-0.004*** (0.002)
Governance Index	-1.18*** (0.24)	-1.29*** (0.22)	-1.86 (1.23)	-2.94** (1.30)	-2.16* (1.20)	-0.51** (0.24)	-0.47* (0.24)
Civil Conflicts	0.40 (0.31)	0.29 (0.25)	0.25 (0.42)	0.33 (0.41)	0.41 (0.35)	0.06 (0.10)	0.08 (0.08)
Natural Disasters	0.12*** (0.02)	0.13*** (0.02)	0.02 (0.08)	0.004 (0.07)	0.01 (0.07)	0.02 (0.02)	0.02 (0.01)
Government Aid / Capita				26.55** (11.37)		2.08 (1.50)	
Country-fixed Effects	N	N	Y	Y	Y	Y	Y
Year-fixed Effects	N	N	Y	Y	Y	Y	Y
Observations	326	338	326	326	338	326	338
R ²							
Adjusted R ²							
Log Likelihood	-274.09	-283.56	-211.17	-208.96	-216.15		
θ	0.23*** (0.05)	0.21*** (0.04)	0.95*** (0.26)	0.98*** (0.26)	0.91*** (0.24)		
Akaike Inf. Crit.	558.19	577.11	558.34	555.93	572.29		
Residual Std. Error						0.34 (df = 257)	0.34 (df = 268)
F Statistic						4.79*** (df = 69; 257)	4.82*** (df = 70; 268)

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. (White's (1980) HC1 standard errors)

Negative binomial regression (Models 1-5) and OLS (Models 6 and 7). Outcome in Models 6 and 7 is dichotomous indicator of at least one unrest event.

occurred during a given year. In addition to including country-fixed effects, this dichotomous measure should address concerns about systematic variation in reporting on unrest events across countries that also might not be captured by the governance index. I estimate Models 6 and 7 as linear probability models which helps ensure the results are not sensitive to using negative binomial regression models.²¹ In Model 6, the coefficient for the bypass per capita variable remains negative, though it drops in statistical significance to the 90% confidence level. In Model 7, however, the coefficient on the bypass ratio variable remains negative and statistically significant. While the coefficient on government aid remains positive in the fixed-effects models, it loses statistical significance in Model 6.²²

The results for the bypass measures are consistent across the model specifications in Table 2. The same holds for the control variables, at least in terms of the sign of the estimated coefficients. As expected, better governance correlates with less unrest with at least 90% confidence in all models except Model 3. The measure of previous civil conflicts stays positive across all models but never attains statistical significance. Natural disasters correlate positively with unrest, but reach statistical significance only when omitting fixed effects (Models 1 and 2).

To substantively interpret the key results from the count models (Models 1-5), I calculate the average change in the expected count of unrest events for the observed sample values of the covariates for a zero to the sample mean shift in the bypass aid measures (the bypass per capita measure and the bypass ratio). To get a measure of uncertainty around this estimate, I generate 1000 sets of simulated coefficients from the estimated sampling distribution of the model coefficients and use each to repeat this procedure. I then extract the 2.5% to 97.5% quantiles of this distribution of mean changes to construct 95% confidence intervals.

Table 3: Substantive Effects from Table 2

	Mean % Change	95% lower	95% upper
Model 1	-41.03	-59.97	-18.10
Model 2	-38.80	-57.06	-14.03
Model 3	-28.89	-54.34	4.58
Model 4	-29.51	-55.36	5.72
Model 5	-33.11	-59.83	-0.31

Note: Changes from zero to sample means.
95% CIs from 1000 sets of simulated coefficients.

²¹As reported in Section E of the Supplementary Files, the results are robust to using a logit model instead.

²²Further, as shown in Section D of the Supplementary Files, the effect of government aid on unrest weakens with changes in model specification such as excluding riots from the dependent variable.

Table 3 reports the mean percentage change in the count of unrest events and its 95% confidence interval for Models 1-5 in Table 2. In all models, the average change in the sample is negative, and in Models 1, 2, and 5 the change is statistically significant at the 95% confidence level. Across the five models, the percent reduction in the count of unrest events tends to be around 30%. The results are slightly weaker for Models 3 and 4, which include country- and year-fixed effects, though the 90% confidence intervals (not shown) do not include zero for both of those models. Given the mean of 0.7 unrest events in a given year in the sample, in practical terms the results suggest that autocratic leaders might expect one unrest event approximately every 1.4 years. The results in Table 2 suggest that increasing bypass aid from zero to its mean (whether the per capita measure or the ratio) decreases the occurrence of unrest events by about 35%, or to a level where autocrats expect about one event every 2.1 years. This reduction seems modest, but unrest may directly threaten the survival of authoritarian regimes or help solve collective action problems that prevent the emergence of more sustained and widespread movements. Further, I am not arguing that autocrats should have a strong preference for bypass aid over government aid. Given that donors may face political pressure to deliver aid through bypass channels or may want to do so to improve development outcomes, autocrats still stand to benefit politically from bypass aid.

Exogenous Variation in Bypass Aid

The analysis above provides support for the theory: bypass aid is associated with less domestic unrest in autocratic countries. This finding holds when using alternate operationalizations of bypass aid (see also Section K of Appendix) and controlling for a number of potential confounders. Yet a government's latent capacity for repression may affect both the propensity of donors to give through bypass channels and the likelihood of unrest. The analysis above attempts to account for this using Dietrich's (2013) measure of governance quality, as well as through the inclusion of country- and year-fixed effects.²³ There would be a problem of reverse causality if domestic unrest affects the distribution of aid across channels in a country. If donors channel more resources to imperiled governments in times of unrest, the data might capture a relative decrease in bypass aid during times of heightened unrest. Alternatively, in periods of low unrest, conditions conducive to the involvement of international donors and NGOs might increase the presence of these actors, causing an uptick in the amount (or proportion) of bypass aid a country receives. These are reasonable concerns, though they

²³The main results in Table 2 are robust to including a measure of military expenditures as a percentage of government spending from the World Bank. Missingness in that measure reduces the number of observations in the data set by more than half.

should be addressed by lagging the bypass variables by one year.²⁴

Nonetheless, concerns about endogeneity may remain. I attempt to identify an exogenous source of variation in bypass aid using an instrumental variables approach. I seek to construct a measure that is (1) plausibly causally related to and strongly correlated with bypass aid in country i at time t , (2) not caused by unrest or factors that affect bypass aid or unrest in country i at time t , and (3) does not affect unrest through alternate channels.

Instances where recipient governments are caught or suspecting of misusing aid, which I refer to as “aid scandals,” would plausibly affect how donors choose to give aid in ways that would affect the ratio of bypass aid to total aid within countries. The above analysis uses both a logged measure of bypass aid per capita and the ratio of bypass aid to total aid as alternate key independent variables since both should be relevant for unrest. However, aid scandals should primarily be relevant for the bypass ratio measure. That measure will be the focus of the remaining analysis.²⁵

Aid scandals should be a good instrument for the bypass ratio for a few reasons. First, when concerns about aid misuse are prominent, international donors should be more inclined to bypass recipient governments. Anecdotal evidence suggests that donors respond to concerns about corruption and mismanagement by channeling aid to NGOs and civil society actors (French 1996; Akam 2012). Donors might intrinsically value improving welfare in foreign countries and want recipient governments to use aid effectively in pursuit of that goal. Alternatively, donors may see aid as an important diplomatic tool that helps them attain foreign policy goals, and domestic audiences may withdraw support for aid when they observe misuse by recipient governments. As Lancaster (2007, 7) argues, “publics in aid-giving countries will turn against aid for development and other purposes if they regard it as having been wasted or used corruptly.” Scandals attract media attention and increase the saliency of aid policy. This implies that they should be associated with policy changes. Reorienting one’s aid portfolio so that aid more often bypasses recipient governments may redeem the aid enterprise and retain an important foreign policy tool. Second, scandals could affect the bypass ratio by leading donors to cut budget support. Either of these mechanisms are plausible and imply that the ratio of bypass aid to total aid should increase in the wake of international aid scandals. This strategy parallels the approach to estimating the aid-growth relationship in Rajan and Subramanian (2008, 2011) in that it emphasizes the “supply” side of aid through donors’ motivations.

²⁴Including a lagged dependent variable changes the interpretation of the model coefficients in count models (see Brandt et al. 2000), but the results remain unchanged substantively.

²⁵In Section M of the Supplementary Files I conduct the baseline instrumental variables analysis using the bypass per capita measure. Though aid scandals should not necessarily always affect bypass per capita, the results are qualitatively similar to the results here, though statistically weaker.

To implement this strategy, I searched media reports for international aid scandals (See Section G of the Supplementary Files), distinguishing between “major” and “minor” scandals. I define a major aid scandal as an instance where an aid donor expressed concern or alleged that a recipient government had used aid for purposes other than those intended by the donor(s) and at least one major media outlet reported on those allegations. I define a minor scandal as an instance where an international news source published an article that discusses actual or potential aid misuse by a specific recipient government.

I focus specifically on allegations of corruption related to aid use, not general corruption. I exclude cases involving military aid, and I count only scandals reported in news stories about specific recipient countries, not general articles about foreign aid and corruption (most of which are editorials). Articles must be about corruption in using aid at that time or new revelations of recent corruption in aid practices. That is, I do not count articles that allude to aid previously-documented misuse by a former regime that no longer holds power at the time of writing. The data set also excludes cases where agents of donor countries were complicit in corruption (for example, a 1993 scandal in the use of Italian aid in Bangladesh).²⁶ With these guidelines in place, I collected data on international aid scandals from 1990 to 2010.²⁷ The primary analysis below includes a total of 27 aid scandals in the years 2003 through 2008 – 15 major scandals and 12 minor scandals.²⁸

The first measure I use counts all major aid scandals in year $t - 2$ (for the purpose of predicting the bypass share in year $t - 1$) *outside* of country i .²⁹ Excluding scandals that occurred in country i helps ensure that the effects of aid scandals work through overall changes in donors’ policies rather than any direct response to aid scandals in a particular country, which would likely be correlated with corruption. Yet outside scandals might affect unrest in autocracies through other channels. Scandals in country A could lead to unrest in country A, which could spillover to country B. Alternatively, autocrats may preemptively increase repression to deter unrest when neighboring autocracies face threats to survival. Further, if citizens in country B use country A as a benchmark and see the regime in charge of country A misusing aid, they may favorably update their beliefs about country B’s leaders. This would lead to less unrest. While these mechanisms are plausible, they are less relevant in autocracies where governments control and restrict information.

²⁶See Section G of the Supplementary Files for a description of the data collection process.

²⁷I limited the initial collection effort to the post-Cold War period to avoid problems associated with political aid giving during the Cold War.

²⁸The complete data set extends up to 2014, though since the Geddes, Wright and Frantz (2014) data ends in 2010, I only use the aid scandal data up to 2008 given the two-year lag. Section G in the Supplementary Files reports a slightly simplified version of the data that also lists cases used in a robustness check below.

²⁹The results below are unaffected by including a control variable that counts any scandals that occurred in country i .

Still, to reduce the chances that aid scandals would affect unrest through these channels, I create a second measure of “extra-continental aid scandals” that counts major scandals that occurred outside country i ’s continent. If aid scandals do lead to unrest in a country, it should be less likely that that unrest spills over to a country outside the region of the country experiencing the scandal and unrest. Further, scandals that occur outside a country’s continent should be less salient to the population in country i , and it should be less likely that citizens in country i use a country outside their region as a benchmark against which to evaluate their own leaders. As such, aid scandals outside country i ’s continent should influence the distribution of aid in that country only by affecting donors’ overall aid strategies. Though *all* scandals are relevant theoretically, excluding some scandals may help satisfy the exclusion restriction.

The theory linking aid scandals to the bypass ratio relies on an effect common to multiple countries. Because scandals do not vary within years, including year-fixed effects precludes detecting this type of relationship. I address this in two ways. First, I control for possible year-level confounders. Economic instability might lead to more aid misuse, leading to an uptick in scandals while setting in motion political conflict that leads to unrest. I take the average GDP growth and average inflation for consumer prices for all countries in the year $t - 2$ (the year in which any aid scandals occurred) using data from the World Bank World Development Indicators data set (World Bank 2013). Similarly, disasters may put strain on governments, increasing the temptation to redirect aid toward patronage uses and affecting global unrest. I include a count of the total number of natural disasters in year $t - 2$ (EM-DAT 2009). Controlling for these factors should reduce the chances that some year-level factor confounds the key relationship of interest.³⁰ Second, to permit the use of year-fixed effects while retaining a theoretically-relevant measure of aid scandals, I weight scandals by the foreign policy similarity of recipient country i to countries where an aid scandal occurred. I calculate the average foreign policy similarity for each dyad-year for the ten years prior to and including the scandal using a measure of voting similarity in the United Nations General Assembly (Strezhnev and Voeten 2013) rescaled to vary between 0 and 1. I record any countries outside country i ’s continent that experienced a major aid scandal or a minor scandal in *The New York Times*,³¹ and sum the rescaled affinity scores of country i with those countries. For country i during year t , this measure takes a value of $x_{it} = \sum_{j=1}^J \tau_{ij}$,

³⁰Including country-fixed effects creates problems with model convergence when carrying out the bootstrapping procedure to adjust standard errors below. However, the second-stage coefficient on the predicted bypass share remains statistically significant at the 90% confidence level when including recipient country-fixed effects in both stages of the unadjusted (i.e., non-bootstrapped) instrumental variables models implemented below.

³¹Because not all years have major aid scandals, the inclusion of minor scandals from the *New York Times* prevents the loss of a large number of observations (due to the inclusion of year-fixed effects).

where τ_{ij} captures the average foreign policy similarity of country i and country j throughout the ten years up to and including the year of the scandal and J represents the total number of countries outside country i 's continent that experienced a major or minor aid scandal in year t . Donors might adjust aid policies more dramatically toward countries with preferences that align with countries that misuse aid. This measure allows for variation across countries within years, making possible the use of year-fixed effects.

I use these three measures – major aid scandals, extra-continental aid scandals, and affinity-weighted aid scandals – to predict the bypass ratio in year $t - 1$ in separate models. I follow Dietrich (2013) (who follows Aitchison (1986)) in transforming the bypass aid proportion variable by taking the natural log of the ratio of bypass aid to non-bypass aid to make the measure continuous.³²

The first and second stage equations for the instrumental variables regression are

$$\text{Count}[\text{Unrest Events}_{i,t}] = \alpha B_{i,t-1} + \beta \mathbf{X}_{i,t-1} + \eta_{it} \quad (\text{Negative binomial, Stage II}) \quad (1)$$

$$\text{E}[\text{Logged Bypass Ratio}_{i,t-1}] = \gamma S_{i,t-2} + \beta \mathbf{X}_{i,t-1} + \epsilon_{it} \quad (\text{OLS, Stage I}), \quad (2)$$

where $B_{i,t-1}$ is the predicted proportion of bypass aid in country i in year $t - 1$, $\mathbf{X}_{i,t-1}$ is a vector of country-specific control variables in year $t - 1$, and $S_{i,t-2}$ is the measure of aid scandals outside country i in year $t - 2$. Parameters α , β , and γ denote estimated coefficients (or vectors of coefficients where applicable) and η and ϵ denote idiosyncratic errors for a given country in a given year in each stage of the analysis. The dependent variable in the first stage is the (log-transformed) bypass ratio and the dependent variable in the second stage is a count of unrest events. The analysis focuses on estimating the partial relationship between the predicted bypass ratio and the count of unrest events in the subsequent year. To implement the two-stage procedure I estimate coefficients for the first-stage model and generate predicted values of the (log-transformed) bypass ratio measure using those coefficients. I then transform these predicted values back into the $[0, 100]$ interval and include these predicted values as a covariate in the second-stage model that predicts unrest events (excluding the measure of aid scandals).

Table 4 reports the results from the first stage models.³³ The first-stage coefficient on all three measures of aid scandals are statistically significant, positive, and have F-statistics that exceed the rule-of-thumb threshold of at least 10 recommended by Staiger and Stock

³²To avoid taking the natural log of zero, I add .0001 to the numerator of each case. Estimating the first stage models on the raw bypass percentage variable also indicates that the three instruments exceed the $F > 10$ threshold, but predictions from those models may fall outside the $[0, 100]$ interval. The transformation process keeps the variable values within a sensible range and follows Dietrich (2013).

³³The full table output with control variables appears in Section N of the Supplementary Files.

Table 4: Stage I - Aid Scandals and Aid Channel Distribution, 2005-2010

	Dependent Variable: Bypass Ratio		
	<i>Model 8</i>	<i>Model 9</i>	<i>Model 10</i>
Major Aid Scandals	0.24*** (0.04)		
Extra-Continental Scandals		0.29*** (0.04)	
Affinity-weighted Scandals			0.29*** (0.08)
Controls	Y	Y	Y
Global year-level controls	Y	Y	N
Year-fixed Effects	N	N	Y
F-Statistic on Instrument	46.52	57.99	14.83
Observations	344	344	344
Adjusted R ²	0.46	0.48	0.48

*p<0.1; **p<0.05; ***p<0.01. Two-tailed tests.

Dependent variable log-transformed as described in text.

OLS estimates. Estimated standard errors in parentheses.

Full output reported in Section N of Supplementary Files.

(1997).³⁴ Because the predicted bypass ratio in the second stage is an estimate with associated uncertainty for which I do not account, this procedure underestimates standard errors. I adjust for this with bootstrapping. I estimate the two-stage model 1000 times, extracting the second-stage coefficient on the bypass ratio to obtain a distribution. Figure 2 reports the mean of the coefficient estimates on the predicted bypass share from the two second stage models along with 90% confidence intervals. For all three models, the 90% confidence interval on the predicted bypass share does not include zero, consistent with the baseline models.

On balance the results provide consistent evidence that bypass aid can reduce domestic unrest in autocratic countries. In the baseline models I find a negative association between the proportion of aid received through bypass channels and the occurrence of domestic unrest in autocracies. The instrumental variables analysis reinforces this finding. In the Supplementary Files I conduct a number of robustness checks. The results hold across various estimations including dropping riots from the count of unrest events in the dependent variable, using a logit model with a dichotomous indicator for unrest events, using a zero-inflated negative binomial regression model, and using alternative measures for bypass aid.

³⁴In Section H of the Supplementary Files I carry out the “plausibly exogenous” test for relaxing the exclusion restriction outlined by Conley, Hansen and Rossi (2012). The instrumental variables results weaken in that test, but are qualitatively in the same direction as the results in the main text. That method requires using two-stage least squares and does not allow for transforming the bypass ratio variable, making a direct comparison of the results impossible.

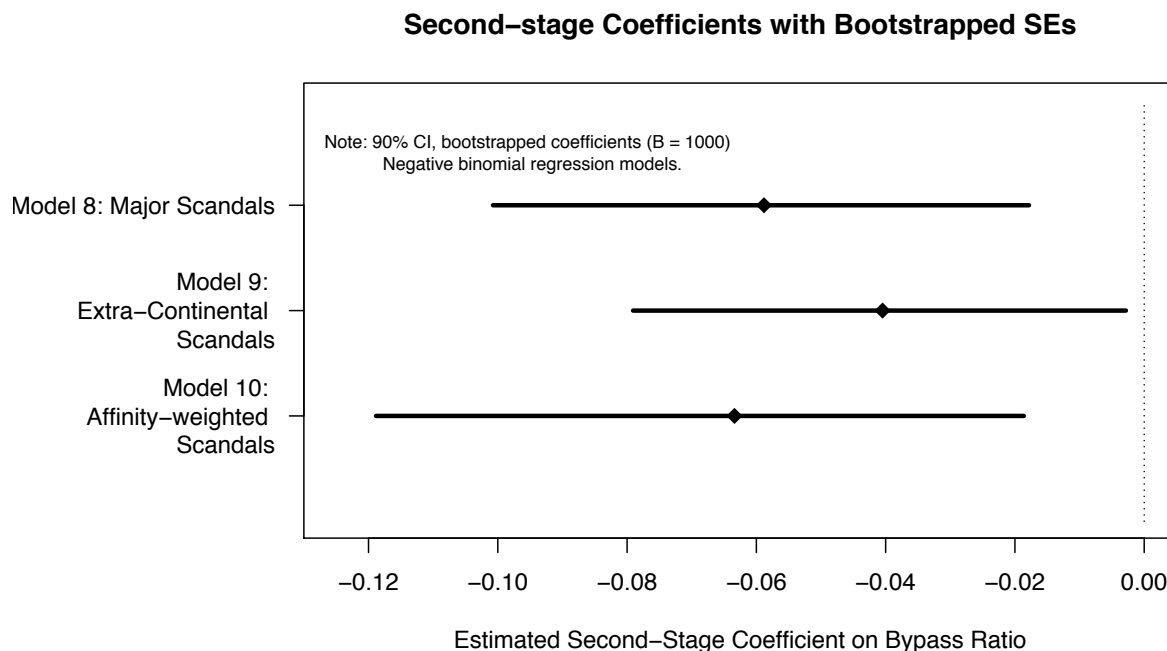


Figure 2: Second-stage Coefficients with Bootstrapped Standard Errors

Conclusion

This article argues and demonstrates that bypassing recipient governments has political implications. While bypass aid may improve short term welfare, it can reduce the incentive for citizens to challenge autocratic governments. This article focuses on unrest, though the findings have important implications for autocratic survival and accountability. Some are ambivalent about the role of bypass aid in the aid-democratization relationship (Bueno de Mesquita and Smith 2009, 337), others assume that bypass aid is politically neutral (Altincekic and Bearce 2014; Bermeo 2016). Insofar as popular mobilization can pressure regimes to broaden political participation and access to government services, bypass aid might indirectly delay such changes by improving the status quo under an autocratic regime.³⁵ Policymakers and donors concerned with promoting democracy should be aware of this potential tradeoff. The results suggest that bypass aid should not be seen as an alternative to conditional aid. Note that this article does not argue that aid does not “work.” In fact, the theory derives

³⁵Of course, this assumes that popular resistance is an important stage of democratization, which may not always be true. Future research might consider the conditions under which autocratic regimes may be most vulnerable to domestic unrest to test for a conditional effect of bypass aid on regime failure. However, the limited availability of data on bypass aid and rarity of regime failure make it difficult to get leverage on this question.

from the assumption, supported by some empirical evidence, that aid can modestly improve conditions for citizens in aid-receiving countries.³⁶ This article helps moves the debate forward by taking the assumption that aid can sometimes work as a starting point for thinking about the potential indirect consequences of “effective” aid.

This article has implications for a number of other literatures. First, the results are relevant for studying the effectiveness of sanctions by highlighting indirect mechanisms through which strategies designed to avoid or punish corruption may fail to achieve foreign policy objectives. Second, although I do not account for bargaining over aid-for-policy deals (e.g., Bueno de Mesquita and Smith 2009), the argument implies that bypass aid could facilitate such deals even if it does not directly contribute to the recipient government’s utility function. As such, bypassing could be reconciled with strategic motivations for aid giving. Third, the results point to an additional mechanism through which external assistance might depress participation in (nonviolent) social movements (Stephan and Chenoweth 2008; Jalali 2013). Finally, the findings highlight that different types of aid may matter differently for unrest. While the main hypothesis of this article argues that bypass aid may reduce unrest in a way similar to what others have argued about government aid, the results here actually suggest that only bypass aid is associated with reduced unrest. However, given the relatively limited overlap in samples between this study and previous studies, future research should explore potential differences in the effects of government aid on unrest. For now, this article shows that bypass aid can have political effects in terms of reducing the occurrence of domestic unrest in autocracies.

Supplemental Information

Supplemental information for this article includes an online appendix containing the robustness checks described in the article, as well as the data and scripts used to generate the statistical estimates and figures in the article and online appendix. This information can be found on the author’s website (mdilorenzo.com) and at the International Studies Quarterly data archive.

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³⁶For a review, see Glennie and Sumner (2016).

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Supplementary Files for “Bypass Aid and Unrest in Autocracies”

This Supplementary Files section contains materials that will be made available online upon publication of the article. It is not intended to appear in the main text of the published version of the manuscript.

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A Formal Model

Though the central hypotheses in the main text are relatively straightforward and intuitive, formalizing the argument is a helpful additional exercise for a few reasons. First, Altınçekic and Bearce (2014) draw on Acemoglu and Robinson’s (2001) formal model in arguing that non-fungible aid should not benefit autocratic regimes. Since I am using the same theoretical framework to predict the opposite outcome, the model helps show that I am not introducing any new or unstated assumptions to the argument. Second, using the formal model allows me to derive a clear comparative static on one of the variables of interest: the proportion of aid delivered through bypass channels. This is important since including measures of total aid along with a proportion that is calculated using total aid may raise concerns about bias in the statistical tests. Lastly, incorporating aid into this framework provides an important baseline for future extensions. In particular, this model should prove useful in endogenizing donor choices over aid channels with an explicit model of domestic politics in the recipient country.

The Model

Consider a dynamic game played between a group of elites and a population.² At the start of the game, the elites control the government. In any round, the elites can choose to allot a portion x of the available resources, R , to the population. I assume that prior to receiving any international aid, the stock of resources is 1. In every round the country receives an amount of aid denoted by $a > 0$. For simplicity I assume that a is exogenous and constant across rounds. Let $\beta \in [0, 1]$ (also exogenous) be the proportion of aid that goes directly to the population. In every round, the population derives utility from any foreign aid it receives directly (βa), plus whatever the government allocates to it (x). (The total amount of bypass aid is therefore equal to βa .) The government derives utility from the portion of aid that it receives $((1 - \beta)a)$ as well any remaining state resources $(1 - x)$.

The state of the world in time period t is defined by the pair (G^t, c^t) where $G^t \in \{A, R\}$ and $c^t \in \{c_L, c_H\}$. If $G^t = A$, the country is in a state of autocracy, while if $G^t = R$ the country is in the state of a revolutionary regime. The parameter c indicates the cost to the population of removing the incumbent regime from power in a given state, which corresponds to the extent to which national wealth would decrease in the aftermath of a revolution. Thus, the government can be either in one of two autocratic states – (A, c_L) or (A, c_H) – or one of two states of a revolutionary (i.e., post-autocratic) regime – (R, c_L) or (R, c_H) . I assume that $c_L \sim U[0, 1]$ is drawn at the beginning of the game by Nature and applies in all future rounds. As a simplifying assumption, I follow Gehlbach (2013) in assuming that c_H is high enough such that the population never chooses to initiate a revolution in the state (A, c_H) even when the regime transfers no resources to the population.³ In the state (A, c_L) , however,

²The game closely follows a simplified version of Acemoglu and Robinson’s (2001) model presented in Gehlbach (2013). The solution concept is Markov perfect equilibrium. Similar to others (e.g., Meirowitz and Tucker 2013), I abstract away from problems of collective action in treating the elites and the population as unified actors, acknowledging that these problems are not trivial.

³This amounts to assuming that $c_H \geq 1 + (1 - \beta)a$. Alternatively, one could assume that the opposition does not get to move if $c = c_H$ and derive the same result.

the population may want to revolt. In either state of autocracy, the population can decide whether or not to oust the elites from office and take over the government. If the population initiates a revolution, it takes over the government and inherits the government's resources, minus the costs of revolution.

In any given round, the probability that the next state is (A, c_L) (if there is no revolution) is $q \in (0, 1)$ and the probability that the state is (A, c_H) is $1 - q$. Let $\delta \in (0, 1)$ be a common discount factor. Denote the value to the population of state (A, c_H) as $V(A, c_H) = \beta a + \delta[qV(A, c_L) + (1 - q)V(A, c_H)]$. Here x is zero because an autocrat has no incentive to make concessions when revolution is not credible. The above expression can be rewritten in terms of $V(A, c_H)$ as

$$V(A, c_H) = \frac{a\beta + \delta qV(A, c_L)}{\delta(q - 1) + 1}. \quad (3)$$

When costs are low enough so that revolution is politically possible, the elites may want to transfer some resources to the population to prevent a revolution. I assume that the elites receive nothing under a revolutionary government. The value to the population of living in state (A, c_L) is now a function of the resources transferred to them by the elites, the amount of foreign aid they receive, and their expectations about the likelihood of being in the other of the autocratic states in the future, which can be expressed as

$$V(A, c_L) = x + \beta a + \delta[qV(A, c_L) + (1 - q)V(A, c_H)]. \quad (4)$$

Substituting (3) into (4) and simplifying yields $V(A, c_L) = \frac{a\beta + \delta(q-1)x + x}{1-\delta}$. The population will be content under autocracy if the amount of resources that the elites transfer to the population makes them at least as well off as they would be under a revolutionary regime.⁴ Since a revolution succeeds with certainty, the value of a revolutionary regime is $V(R, c_L) = \frac{1 + (1-\beta)a - c_L}{1-\delta}$. To avoid revolution, the elites' offer must satisfy $V(A, c_L) \geq V(R, c_L)$, or

$$\frac{a\beta + \delta(q-1)x + x}{1-\delta} \geq \frac{1 + (1-\beta)a - c_L}{1-\delta}. \quad (5)$$

The government's utility is decreasing in x , so it sets x such that Equation 5 holds with equality. Solving for this optimal offer and taking its first derivative with respect to β shows that the government's equilibrium transfer is decreasing in the proportion of bypass aid ($\frac{\partial x^*}{\partial \beta} < 0$). As the proportion of aid delivered through bypass channels increases, smaller offers from the government deter unrest. So long as aid provides some direct benefit to the population, governments can more easily stave off revolution. This is independent of any effect that government aid would have on allowing the government to make bigger offers. The intuition behind the result for the proportion itself is that as more of the total "pie"

⁴Under the assumption that the value of a revolutionary regime also includes the full amount of future bypass and government aid ($V(R, c_L) = \frac{1 - c_L + a}{1 - \delta}$), the offer that buys off the population is still decreasing in β ($\frac{\partial x^*}{\partial \beta} = -\frac{a}{\delta(q-1)+1} \leq 0$).

of aid goes directly to the population now, there is less to be gained from taking over the government (and, by extension, the aid it receives). In sum, in periods that are unfavorable for revolution, bypass aid acts as a humanitarian boon – without it the population would get nothing. In periods where revolution is possible, bypass aid acts a political subsidy to the government by making it easier to buy off the opposition.⁵

The probability of revolution in the model is the probability that the optimal transfer x^* is less than the resources available to the government, which can be written as

$$\Pr(x^* < 1 + (1 - \beta)a), \quad (6)$$

or

$$\Pr(c_L < \delta(q - 1)(a(\beta - 1) - 1) - a\beta). \quad (7)$$

Since $c_L \sim U[0, 1]$, this is equivalent to $\delta(q - 1)(a(\beta - 1) - 1) - a\beta$, which is decreasing in β .

B Extensions to Model

What If Bypass Aid Makes Revolution Easier?

As noted in the main text, an important stylized fact to account for is that bypass aid may actually help sustain rebel groups. The baseline model assumes that an attempted revolution succeeds with certainty. I now relax this assumption to account for the possibility of a failed revolution, which also allows me to account for the fact foreign aid may affect the probability of a revolution succeeding. Let $\rho(\beta) \in (0, 1)$ denote the probability that a revolution is successful, and let $1 - \rho(\beta)$ denote the probability of failure. Let $\rho(\beta)$ be an increasing, concave function of the proportion of bypass aid.⁶ For now I assume that in the case of success the resources of the state are available to be evenly divided across the population forever, minus the costs of revolution. If revolution fails, the population gets nothing forever. I also assume that the government can decide how much aid to allow into the country up to some exogenously determined threshold, \hat{a} . Think of this as the maximum amount of aid that the international community would be willing to contribute.

Now the population chooses not to revolt if

⁵It might also be the case that bypass aid could stabilize a regime if the regime intercepts aid or coopts bypass actors to invest in repressive capacity. In this case, the prediction would essentially be the same as models that assume government control of aid, but through a different mechanism. While the results here are similar to those in Smith (2008), the model here shows that government control of aid is not a necessary condition for aid to deter unrest. In Section I of the Supplementary Files I find no evidence for a relationship between bypass aid and changes in military expenditures in the sample.

⁶Note that the model implicitly accounts for the possibility that government aid increases the odds that the government wins in a revolutionary contest, since as the proportion of bypass aid decreases, the probability that the government wins ($1 - \rho(\beta)$) increases.

$$\frac{a\beta + \delta(q-1)x + x}{1-\delta} = \frac{\rho(\beta)(a(1-\beta) - c_L + 1)}{1-\delta}. \quad (8)$$

The value of x that will buy the population off is now

$$\hat{x} = \frac{\rho(\beta)(a(-\beta) + a - c_L + 1) - a\beta}{\delta(q-1) + 1}. \quad (9)$$

The derivative of this optimal \hat{x} with respect to β is

$$\frac{\partial \hat{x}}{\partial \beta} = -\frac{a\rho(\beta) + \rho'(\beta)(a(\beta-1) + c_L - 1) + a}{\delta(q-1) + 1}, \quad (10)$$

where $\rho'(\beta)$ denotes the first derivative of the probability that the revolution succeeds with respect to β . Now the optimal \hat{x} is decreasing in β only if

$$\rho'(\beta) \leq \rho'(\beta)^* \equiv -\frac{a(\rho(\beta) + 1)}{a(\beta-1) + c_L - 1}, \quad (11)$$

which is always positive. This threshold is less than 1 (i.e., it is possible that the relationship between bypass aid and the optimal offer may be negative or positive) if the condition $a(\beta + \rho(\beta)) + c_L - 1 > 0$. This highlights that governments may allow aid to bypass them even when it potentially increases the likelihood of a successful revolution because of the effects that it has on undermining the incentive for revolution.

C Countries Included in Analysis

Table 5 reports the country names (sometimes abbreviated) used in the analysis as they appear in the Geddes, Wright and Frantz (2014) data.

Table 5: Temporal and Spatial Domain

	Country	Year(s) in Data		Country	Year(s) in Data
1	Afghanistan	2010	32	Madagascar	2010
2	Algeria	2005 - 2010	33	Malaysia	2005 - 2010
3	Angola	2005 - 2010	34	Mauritania	2005 - 2010
4	Armenia	2005 - 2010	35	Morocco	2005 - 2010
5	Azerbaijan	2005 - 2010	36	Mozambique	2005 - 2010
6	Bangladesh	2008	37	Myanmar	2005 - 2010
7	Belarus	2005 - 2010	38	Namibia	2005 - 2010
8	Botswana	2005 - 2010	39	Nepal	2005 - 2006
9	Burkina Faso	2005 - 2010	40	Oman	2005 - 2010
10	Cambodia	2005 - 2010	41	Pakistan	2005 - 2008
11	Cameroon	2005 - 2010	42	Rwanda	2005 - 2010
12	Cen African Rep	2005 - 2010	43	Saudi Arabia	2005 - 2010
13	Chad	2005 - 2010	44	Singapore	2005 - 2010
14	China	2005 - 2010	45	Sudan	2005 - 2010
15	Congo-Brz	2005 - 2010	46	Swaziland	2005 - 2010
16	Congo/Zaire	2005 - 2010	47	Syria	2005 - 2010
17	Cuba	2005 - 2010	48	Tajikistan	2005 - 2010
18	Egypt	2005 - 2010	49	Tanzania	2005 - 2010
19	Eritrea	2005 - 2010	50	Thailand	2007
20	Ethiopia	2005 - 2010	51	Togo	2005 - 2010
21	Gabon	2005 - 2010	52	Tunisia	2005 - 2010
22	Gambia	2005 - 2010	53	Turkmenistan	2005 - 2010
23	Guinea	2005 - 2010	54	Uganda	2005 - 2010
24	Iran	2005 - 2010	55	United Arab Emirates	2005 - 2010
25	Ivory Coast	2005 - 2010	56	Uzbekistan	2005 - 2010
26	Jordan	2005 - 2010	57	Venezuela	2006 - 2010
27	Kazakhstan	2005 - 2010	58	Vietnam	2005 - 2010
28	Kuwait	2005 - 2010	59	Yemen	2005 - 2010
29	Kyrgyzstan	2005 - 2010	60	Zambia	2005 - 2010
30	Laos	2005 - 2010	61	Zimbabwe	2005 - 2010
31	Libya	2005 - 2010			

D Measure of Unrest Excluding Riots

Following Kono, Montinola and Verbon (2015), the dependent variable used in the main text is a count of strikes involving more than 1,000 people directed at the government or national policies, riots involving more than 100 people, and anti-government demonstrations involving more than 100 people from Banks (2011). Since the theory emphasizes actions that would challenge a government specifically and the count of riots does not specify that the riots need to have anything to do with government policies, I want to ensure the main result does not depend unduly on the inclusion of riots in the dependent variable. (Additionally, from a normative perspective, it might be desirable for bypass aid to reduce unrest if the unrest events it affects are riots.) Table 6 reports the results of rerunning the models from Table 2 in the main text while excluding riots from the count variable. Though the results are weaker in Models 5, 6, and 7, they are in the same direction as those reported in the main text (In Model 6, the coefficient on bypass aid is negative and statistically significant at the 90% confidence level in a one-tailed test). Still, including riots seems reasonable in that it may capture the willingness of citizens to take action in the face of the failure of government policies (for example, food riots).

Table 6: Bypass Aid and Domestic Unrest (Excluding Riots), 2005-2010

	Unrest Events						
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bypass Aid / Capita	-59.85** (28.19)		-34.41* (18.88)	-34.82* (18.59)		-0.99 (0.66)	
Bypass Ratio		-0.03** (0.01)			-0.02 (0.01)		-0.002 (0.002)
Governance Index	-1.11*** (0.25)	-1.15*** (0.24)	-1.53 (1.45)	-2.21 (1.49)	-1.61 (1.41)	-0.34 (0.22)	-0.34 (0.22)
Civil Conflicts	0.52 (0.35)	0.42 (0.27)	0.10 (0.40)	0.16 (0.40)	0.29 (0.35)	0.06 (0.07)	0.06 (0.07)
Natural Disasters	0.12*** (0.02)	0.13*** (0.02)	0.02 (0.08)	0.02 (0.07)	0.02 (0.07)	0.03* (0.02)	0.03* (0.02)
Government Aid / Capita				15.91 (11.46)		1.57 (1.28)	1.16 (1.08)
Country-fixed Effects	N	N	Y	Y	Y	Y	Y
Year-fixed Effects	N	N	Y	Y	Y	Y	Y
Observations	326	338	326	326	338	326	326
R ²							0.52
Adjusted R ²						0.39	0.39
Log Likelihood	-210.23	-219.22	-158.21	-157.54	-163.45		
θ	0.26*** (0.06)	0.23*** (0.05)	1.19*** (0.42)	1.21*** (0.43)	1.10*** (0.37)		
Akaike Inf. Crit.	430.45	448.44	452.42	453.08	466.90		
Residual Std. Error (df = 257)						0.32	0.32
F Statistic (df = 69; 257)						4.04***	4.05***

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. (White's (1980) HC1 standard errors)

Negative binomial regression (Models 1-5) and OLS (Models 6 and 7). Outcome in Models 6 and 7 is dichotomous indicator of at least one unrest event.

E Logit Models for Dichotomous Unrest Variable

To ensure that the results of the count models are not driven by outlying cases with many unrest events, Table 7 reports the results of the key models from Table 2 in the main text where the dependent variable is a dummy variable that takes on a value of “1” when there is at least one unrest event in the data. In both models, the relationship between bypass aid and unrest is negative and statistically significant.

Table 7: Bypass Aid and Domestic Unrest (Logit Models), 2005-2010

	Unrest Events				
	<i>Model 1</i>	<i>Model 2</i>	<i>Model 3</i>	<i>Model 4</i>	<i>Model 5</i>
	(1)	(2)	(3)	(4)	(5)
Bypass Aid / Capita	-47.84** (20.01)		-68.89* (39.01)	-66.76* (38.47)	
Bypass Ratio		-0.03*** (0.01)			-0.06*** (0.02)
Governance Index	-1.09*** (0.30)	-1.12*** (0.30)	-4.84** (2.07)	-6.45*** (2.21)	-5.42** (2.14)
Civil Conflicts	0.03 (0.32)	0.02 (0.26)	0.51 (0.75)	0.62 (0.77)	0.77 (0.63)
Natural Disasters	0.18*** (0.05)	0.18*** (0.05)	0.30** (0.15)	0.26* (0.15)	0.28** (0.14)
Government Aid / Capita				52.54** (20.76)	
Country-fixed Effects	N	N	Y	Y	Y
Year-fixed Effects	N	N	Y	Y	Y
Observations	326	338	326	326	338
Log Likelihood	-141.13	-147.89	-83.71	-80.02	-85.64
Akaike Inf. Crit.	292.26	305.77	303.42	298.05	311.28

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. Estimated standard errors in parentheses.

Logit regression (Models 1-5). Outcome is dichotomous indicator of at least one unrest event.

F Zero-inflated Negative Binomial Regression

To account for the possibility of excessive zeroes in the dependent variable, Table 8 reports the results of two negative zero-inflated binomial regression models where the inflation variable is a factored version of the country code variable. (I omit the coefficients on the model of the probability of observing a zero count.)

Table 8: Zero-inflated Negative Binomial Regression Models

	Unrest Events	
	<i>Model 1</i>	<i>Model 2</i>
	(1)	(2)
Bypass Aid / Capita	−0.54*** (0.20)	
Bypass Ratio		−0.03** (0.01)
Governance Index	−0.54 (0.37)	−0.76* (0.42)
Civil Conflicts	0.44* (0.25)	0.52** (0.26)
Natural Disasters	0.04** (0.02)	0.04** (0.02)
Year-fixed Effects	Y	Y
Observations	326	326
Log Likelihood	−214.17	−215.43

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. Estimated standard errors in parentheses.
Zero-inflated negative binomial regression.

G Aid Scandal Cases

Table 9 reports the basic case information for the aid scandals used in the construction of the instrumental variable. A complete version of the data with full citation information will be available on the author’s website. I conducted the data collection in three rounds as follows: On the first round, I used *Lexis Nexis Academic* to search for “corruption AND foreign aid” in the category of newspapers from 1990 to 2014, sorted the results by relevance, and then manually sifted through the results to identify and record possible scandals. In the second round, I repeated this process but with the search phrase “aid AND misus* AND government.” In the third round of data collection, I again searched for “aid AND misus* AND government,” but did a separate search for each individual year from 1990 to 2014. Note that the observations are scandals (or minor scandals), not news stories about scandals, even while they are based on news stories.

Table 9: Cases from Aid Scandal Data, 2003-2008

Number	Year	Month	Day	Country	Major?
1	2003	10	18	Philippines	0
2	2003	10	19	Zimbabwe	0
3	2004	2	24	Iraq	1
4	2004	7	14	Haiti	0
5	2005	1	13	Indonesia	0
6	2005	2	13	Sri Lanka	1
7	2005	7	6	Nigeria	0
8	2005	12	7	Malawi	0
9	2006	1	22	Cambodia	0
10	2006	5	7	Vietnam	0
11	2006	6	1	Uganda	1
12	2006	10	4	Macedonia	1
13	2007	2	20	Colombia	1
14	2007	7	31	Iraq	0
15	2007	8	20	Zimbabwe	1
16	2007	12	27	Pakistan	0
17	2008	2	6	Afghanistan	1
18	2008	2	11	Indonesia	1
19	2008	3	26	Afghanistan	0
20	2008	5	8	Myanmar	1
21	2008	6	30	Pakistan	1
22	2008	7	4	Pakistan	1
23	2008	7	19	Bulgaria	1
24	2008	7	27	Costa Rica	1
25	2008	9	23	Iraq	1
26	2008	10	15	Tajikistan	0
27	2008	11	3	Zimbabwe	1

H Evaluating Exclusion Restriction

While the requirement that the instrumental variable be strongly correlated with the endogenous variable of interest is straightforward to evaluate, it is more difficult to ensure that the aid scandals do not affect unrest through alternate channels. As discussed above, there are a few potential mechanisms through which outside aid scandals could potentially affect domestic unrest independently of their effects on bypass aid. In the main text I argued that these threats should not be serious theoretically given that one of the key instrumental variables is a count of aid scandals that occurred outside country i 's continent in year $t - 2$. Here I perform some statistical tests to evaluate those potential violations of the exclusion restriction.

Table 10 presents the results of a series of bivariate models. One concern is that aid scandals might affect overall aid amounts, which could affect a government's repressive capacity. However, there are equally plausible arguments for both a positive and negative effect here. On one hand, aid scandals might lead donors to reduce overall aid flows in light of evidence of misuse. On the other hand, donors may only reduce aid to the country experiencing an aid scandal, reallocating fixed aid budgets to other countries and regions. In any case, while aid scandals are strongly correlated with the channel distribution of aid in a given country, Models 1 and 2 in Table 10 shows that they do not appear to affect total aid amounts.

Models 3 and 4 shows that there appears to be no direct effect of aid scandals on unrest, and Model 5 shows (perhaps surprisingly) that there is no effect of aid scandals in country i on unrest in country i . Model 6 shows that outside unrest is weakly correlated with less unrest in country i , and Model 7 suggests that unrest outside a given country's continent is not associated with domestic unrest. A possible explanation for the result in Model 6 is that autocrats increase repression when outside countries experience unrest to deter unrest, but since there is no evidence that scandals in country i affect unrest in that country (Model 5), this concern should not apply to aid scandals.

Implementing Conley, Hansen and Rossi's (2012) "Plausibly Exogenous" Test

The tests in the previous section seek to identify potential theoretical mechanisms through which aid scandals might affect unrest outside of their effects on the distribution of bypass aid. On balance, it seems reasonable that aid scandals would serve as an exogenous source of variation in aid channel distribution.

Others have outlined procedures for formally relaxing the exclusion restriction (Conley, Hansen and Rossi 2012) in the context of two-stage least squares estimation (2SLS). Implementing those procedures here is messy for a number of reasons. First, the models in the main article use different estimators in each stage of the analysis to accommodate the count structure of the outcome variable (unrest events). I use OLS to estimate the relationship between the various measures of aid scandals and the logged bypass ratio, and then negative binomial regression to estimate the relationship between bypass aid and a count of unrest events. To adjust for the incorrect standard errors in the second stage of this procedure, I rely on bootstrapped coefficients from the second stage to draw inferences. Second, the

Table 10: Evaluating Exclusion Restriction

	<i>Dependent variable:</i>						
	DV: Total Aid		DV: Unrest Events				
	<i>OLS</i>		<i>Negative binomial</i>				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Major Aid Scandals	16.195 (19.961)		-0.024 (0.054)				
Major Aid Scandals Outside Continent		31.847 (26.259)		-0.008 (0.070)			
Scandals in Country					-1.864 (1.295)		
Outside Unrest						-0.031* (0.018)	
Unrest Outside Continent							-0.005 (0.019)
Constant	734.114*** (77.673)	721.415*** (73.893)	-0.353* (0.206)	-0.398** (0.197)	-0.386** (0.159)	0.678 (0.660)	-0.310 (0.443)
Observations	344	344	344	344	344	344	344
R ²	0.002	0.004					
Adjusted R ²	-0.001	0.001					
Log Likelihood							
θ			-323.800 0.140*** (0.024)	-323.892 0.140*** (0.024)	-322.810 0.143*** (0.024)	-322.211 0.146*** (0.025)	-323.865 0.140*** (0.024)
Akaike Inf. Crit.			651.599	651.784	649.620	648.421	651.729
Residual Std. Error (df = 342)	1,104.885	1,103.578					
F Statistic (df = 1; 342)	0.658	1.471					

Note:

*p<0.1; **p<0.05; ***p<0.01

analysis in the main article transforms the predictions for the first stage dependent variable (logged bypass ratio) before including it as a covariate in the second stage. The transformation of the dependent variable is intended to make the dependent variable continuous following Dietrich (2013). When transforming the predictions from these models back into the [0,100] interval, the models generate predictions that lie in that interval. This is not the case when using the raw percentage variable, which in the prediction stage can result in negative values or values above 100. For these reasons, the main analysis does not implement 2SLS.

Nonetheless, I implemented the procedures in Conley, Hansen and Rossi (2012) using modified versions of two of the models in the main text (Models 8 and 9). I use the percentage of bypass aid as the endogenous regressor in the first stage in all three models. The second stage outcome variable is a dummy variable indicating whether or not at least one unrest event occurred. I estimated robust standard errors using Stata's `vce(robust)` command. The procedures outlined in Conley, Hansen and Rossi (2012) allow for researchers to relax the exclusion restriction in an instrumental variables model by introducing a parameter γ that represents any direct effect that the excluded instrument may have on the second-stage outcome variable (in this case, any direct effect that aid scandals could have on the likelihood of unrest.) The user specifies the minimum and maximum bounds for γ , call these γ_L and γ_H . I set $\gamma_L=0$ for all three models, and set γ_H equal to the estimated coefficient from a regression of the dichotomous unrest variable on each of the aid scandals measures. This value is -.007 for both Model 8 and Model 9.

Figure 3 reports the results of this procedure including the instruments used in Models 8 and 9 from the main text along with the control variables included in those models. In

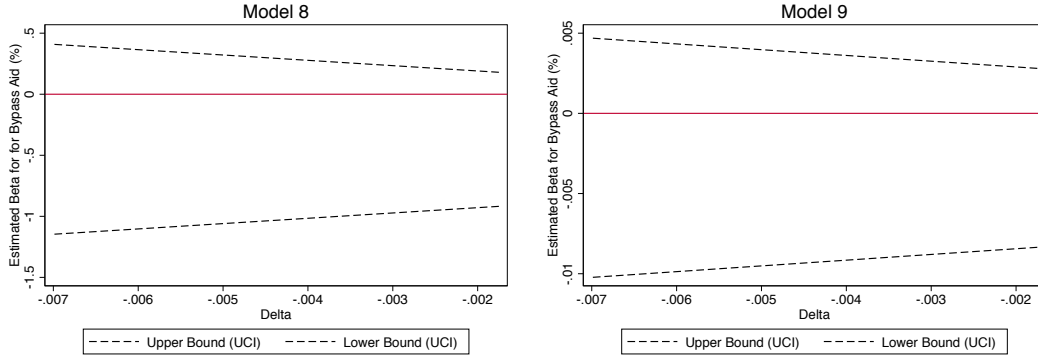


Figure 3: Union of Confidence Intervals Approach to Relaxing Exclusion Restriction.

all three cases, the mean of the estimated coefficient on the logged bypass ratio is negative, though the 80% union of confidence intervals includes zero in all three cases. As the assumption of no direct effect on the outcome is relaxed, the confidence intervals widen slightly. Again, a direct comparison with the results in the article is not possible given the different methods employed in each case. That is, the models used in the “plausibly exogenous” test are not the same as those reported in the main article, and are less appropriate for evaluating the key hypothesis given the nature of the data. Although the results are weaker when using this alternative framework, it is at least encouraging that these different design choices produce results that point substantively to the conclusions in the main analysis.

Ultimately the exogeneity of the excluded instrument needs to be justified on theoretical grounds, and the previous section identifies some potential threats to inference and shows that they are likely unwarranted. Further, the results in the main text show that the association between bypass aid and unrest is robust to a variety of alternate research design choices in ways that are consistent with the theory. Indeed, the main analysis explicitly seeks to control for confounders, and, as noted in the main text, the main results are also robust to controlling for military expenditures.

I Bypass Aid and Investment in Repressive Capacity

The theory suggests that government control of aid is not a necessary condition for aid to depress the incentive to revolt. Still, it is possible that bypass aid would influence unrest not through benefitting citizens, but rather because recipient regimes intercept or tax bypass aid and use these resources to invest in greater repressive capacity. In Table 11 I regress (logged) changes in military expenditures as a percent of government spending from the World Bank from year t to $t - 1$ on bypass aid. Model 1 uses the proportion of bypass aid as the key independent variable. Model 2 uses the logged total of bypass aid in millions of US dollars, controlling for logged government aid. Since interstate crises and civil conflicts might affect both bypass aid and military expenditures, I control for a count of militarized interstate disputes and civil conflicts in the previous year. The count of the number of militarized interstate disputes (MIDs) that a country was involved in during the previous year is based on data from the Correlates of War project (Palmer et al. 2015). (The results do not depend on the inclusion of these variables.) To account for unobserved heterogeneity, I include year-fixed effects. In both models, the relationship between bypass aid and changes in military expenditures is negative, though not statistically significant. This is consistent with the theory. If bypass aid provides a service that benefits people, then autocratic regimes need not invest in greater repressive capacity.

Table 11: Bypass Aid and Change in Military Expenditures (% Gov. Spending)

	Dependent variable: Change in Military Expenditures	
	<i>Model 1</i>	<i>Model 2</i>
	(1)	(2)
Bypass Aid / Capita	-12.40 (8.81)	
Bypass Ratio		-0.02 (0.02)
International Disputes	0.19 (0.25)	0.21 (0.25)
Civil Conflicts	-0.67 (0.63)	-0.53 (0.64)
Year-fixed Effects	Y	Y

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. Estimated standard errors in parentheses.

OLS estimates.

J Alternative *AidData* Measure

One potential concern in using the OECD data on aid channels is that the data could be subject to measurement bias due to underreporting as donors were adjusting to new reporting requirements that the OECD implemented in 2004.⁷ Unfortunately, no other source provides comprehensive data on aid channels. However, the *AidData* project (Tierney et al. 2011), which records aid data at the project level, does contain information about the stated purpose for which donors commit aid. To create an alternative indicator of bypass aid, I search for patterns in the “purpose” variable that might indicate that aid was given through bypass channels. The string of patterns I search for in *R* (R Core Team 2014) is “ngos|NGOs|NGO’s|civil society|food|Food|Material relief.” I create a measure of the proportion of aid that was committed to projects that included one or more of these terms for each recipient year.⁸ This measure is moderately correlated with the measure constructed from OECD data ($\rho = 0.54$).

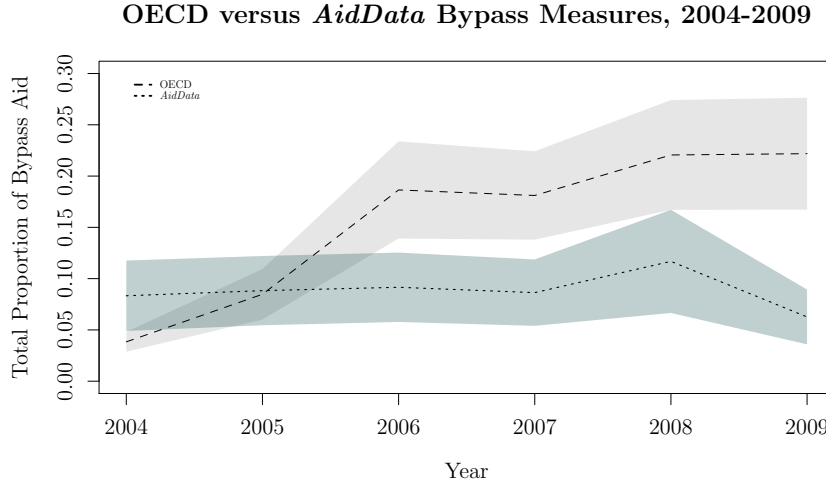


Figure 4: *Comparing Aid Measures*. The dashed line is the mean level of OECD bypass aid for all countries in the sample (i.e., all autocracies), and the dotted line is the mean level of bypass aid using my measure constructed from *AidData*. The shaded areas mark 95% confidence intervals.

Figure 4 plots the means of each measure ± 1.96 standard deviations for every year from 2004 to 2009. The mean level of bypass aid in the OECD measure is closely clustered around about four percent in 2004, while the *AidData* measure is more varied, centered around eight percent. The *AidData* measure is relatively consistent across time and may be undercounting bypass aid. Whether this is the case is unclear, though the measure is arguably less susceptible to concerns about underreporting during the early years of OECD reporting requirements.

⁷Dietrich (2013, 702) also acknowledges this issue in footnote 13 in her article.

⁸Though disbursements would be more appropriate, this data is not available from *AidData*. See Section J in the Supplementary Files for a description of a validity check for this coding procedure.

Table 12 reports the results of the basic model specification of Model 2 from Table 2 substituting in the alternative measure of bypass aid. Again, there is clear support for the theory in that the measure of bypass aid is negatively associated with unrest events, though this result is only statistically significant at the 90% confidence level in a one-tailed test in Model 1. Because the results are similar across measures and there is no clear benchmark to compare *AidData* measure in the pre-2004 period (and Figure 4 suggests that undercounting may not actually be much of a concern), I use the OECD measure in the main analysis. Additionally, the *AidData* measure captures only commitments, not disbursements. Because donors often withhold or delay the disbursement of aid commitments, using the OECD data on actual disbursements likely does a better job capturing the political effects of bypass aid in recipient countries. Still, the congruity of the results from using this alternative measure with the OECD measure is encouraging.

Table 12: Alternative AidData Measure

	Dependent variable: Unrest Events	
	<i>Model 1</i>	<i>Model 2</i>
	(1)	(2)
Bypass Aid / Capita	−0.07 (0.04)	
Bypass Ratio		−2.54** (1.05)
Governance Index	−0.82*** (0.21)	−0.90*** (0.29)
Civil Conflicts	0.50** (0.21)	0.41** (0.18)
Natural Disasters	0.14*** (0.01)	0.14*** (0.01)
Observations	633	655
Log Likelihood	−546.94	−559.60
θ	0.18*** (0.02)	0.18*** (0.02)
Akaike Inf. Crit.	1,103.89	1,129.19

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. (White's (1980) HC1 standard errors)

Negative binomial regression models.

Validity Check for *AidData* Measure

A preliminary validity check suggests that the coding procedure used to construct the alternate measure of bypass aid from the *AidData* project succeeds in identifying cases where bypass is likely. In a random sample of 50 cases where the bypass indicator takes a value of “1,” all of the coded projects could plausibly bypass the government. The unique purpose codes

in the sample are: Democratic participation and civil society, Material relief assistance and services, Strengthening civil society, Support to international NGOs, Emergency food aid, Food crop production, Support to national ngos, Food aid/Food security programmes, Food security programmes/food aid, Support to local and regional ngos. Of course, there is still potential for measurement error. For example, it could be the case that a donor gives aid for the purpose of “Support to national ngos,” but does so through the recipient government, and the aid never makes it to NGOs (or never makes it to citizens). As discussed in the main text, this is the same problem scholars face in using the OECD data. Still, relative to aid labeled as “budget support,” we should be confident that a great deal of what this measure picks up is in fact bypassing the government.

K Alternative Operationalizations of Bypass Aid with OECD Data

Table 13 reports the results of models that use alternative operationalizations of bypass aid. Model 1 calculates the bypass ratio variable excluding aid delivered through public-private partnerships. Model 2 uses a measure of total bypass aid (including public-private partnerships, as in the main measure) as a percentage of real GDP output from Gleditsch (2002). As in the main text, bypass aid is statistically significant and negatively associated with unrest events.

Table 13: Alternative OECD Measures of Bypass Aid

	Unrest Events	
	<i>Model 1</i>	<i>Model 2</i>
	(1)	(2)
Bypass Ratio (Excluding Public-Private Partnerships)	−0.03** (0.01)	
Bypass / GDP		−1.08** (0.52)
Governance Index	−2.17* (1.20)	−1.65 (1.26)
Civil Conflicts	0.41 (0.35)	0.41 (0.44)
Natural Disasters	0.01 (0.07)	0.04 (0.08)
Country-fixed Effects	Y	Y
Year-fixed Effects	Y	Y
Observations	338	295
Log Likelihood	−216.09	−206.07
θ	0.91*** (0.24)	0.97*** (0.26)
Akaike Inf. Crit.	572.18	536.15

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. Estimated standard errors in parentheses.

Negative binomial regression.

L Controlling for Strategic Importance of Recipient Country

To account for the possibility that the strategic importance of recipient countries explains both bypass aid and unrest in a given country, Table 14 reports the results of models controlling for variable measures of strategic importance: (1) years since independence (with the expectation that countries that achieved independence more recently may have been more valuable to colonizing states), (2) a dummy variable indicating whether the country had an alliance with the US during a given year, (3) a count of the number major-power allies that a country had during a given year (both of these measures come from the Correlates of War Formal Alliances data set version 4.1), and (4) a dummy variable that takes a value of one for countries that are members of the Organization of the Petroleum Exporting Countries (OPEC). In both models, the bypass variables remain negative and statistically significant.

Table 14: Controlling for Strategic Importance of Recipient Country

	Unrest Events	
	<i>Model 1</i>	<i>Model 2</i>
	(1)	(2)
Bypass / Population	−42.65** (17.58)	
Bypass Ratio		−0.02** (0.01)
Governance Index	−1.25*** (0.33)	−1.30*** (0.30)
Civil Conflicts	0.25 (0.26)	0.06 (0.23)
Natural Disasters	0.02 (0.03)	0.02 (0.03)
Years Since Independence	0.005*** (0.001)	0.01*** (0.002)
US Ally	1.04* (0.55)	1.29** (0.56)
No. Major Power Allies	0.24 (0.22)	0.19 (0.22)
OPEC Member	−1.78*** (0.55)	−1.59*** (0.60)
Observations	326	338
Log Likelihood	−267.82	−276.98
θ	0.27*** (0.06)	0.24*** (0.05)
Akaike Inf. Crit.	553.63	571.96

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. (White's (1980) HC1 standard errors)

Negative binomial regression.

M Instrumental Variables Analysis with Bypass Per Capita Measure

As I argue in the main text, the aid scandals measures are most appropriate to use as instruments for the bypass ratio – rather than the bypass per capita measure – from a theoretical standpoint. Nonetheless, here I replicate the analysis replacing the bypass measure with the bypass per capita measure. Although the second stage results are no longer statistically significant, the second stage coefficients on the predicted bypass per capita variable remain negative in two of the three models (though the standard errors are very large). Given that there is a stronger theoretical link between aid scandals and the bypass ratio rather the bypass per capita measure (i.e., aid scandals could lead to a shift in the distribution of aid across channels without leading to an increase in bypass per capita), using the bypass ratio is preferable from a research design perspective.

Table 15: Stage I - Aid Scandals and Aid Channel Distribution, 2005-2010

	Logged Bypass Aid Per Capita		
	<i>Model 8, Stage I</i>	<i>Model 9, Stage I</i>	<i>Model 10, Stage I</i>
	(1)	(2)	(3)
Major Aid Scandals	0.001*** (0.001)		
Extra-Continental Scandals		0.002*** (0.001)	
Affinity-weighted Scandals			0.001 (0.001)
Governance Index	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)
Civil Conflicts	0.003 (0.003)	0.003 (0.003)	0.003 (0.003)
Natural Disasters	0.0001 (0.0003)	0.0001 (0.0003)	0.0000 (0.0003)
Average Global GDP Growth	-0.001 (0.002)	-0.001 (0.002)	
Average Global Inflation	0.0000 (0.0000)	0.0000 (0.0000)	
Total Global Disasters	0.0002*** (0.0000)	0.0002*** (0.0000)	
Year-fixed Effects	N	N	Y
F-Statistic on Instrument	7.19	7.74	1.26
Observations	332	332	332
R ²	0.06	0.06	0.09
Adjusted R ²	0.04	0.04	0.07
Residual Std. Error	0.02 (df = 324)	0.02 (df = 324)	0.02 (df = 322)
F Statistic	3.10*** (df = 7; 324)	3.19*** (df = 7; 324)	3.69*** (df = 9; 322)

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. Estimated standard errors in parentheses.

OLS estimates.

Table 16: Stage II - Predicted Bypass Per Capita and Unrest, 2005-2010

	Unrest Events		
	<i>Model 8, Stage II</i>	<i>Model 9, Stage II</i>	<i>Model 10, Stage II</i>
	(1)	(2)	(3)
Predicted Bypass Per Capita	−18.01 (43.68)	−2.88 (42.37)	45.03 (117.50)
Governance Index	−1.14*** (0.30)	−1.11*** (0.30)	−1.06*** (0.37)
Civil Conflicts	0.46 (0.35)	0.43 (0.35)	0.28 (0.52)
Natural Disasters	0.14*** (0.03)	0.14*** (0.03)	0.13*** (0.03)
Average Global GDP Growth	−0.28 (0.23)	−0.25 (0.23)	
Average Global Inflation	0.003 (0.003)	0.002 (0.003)	
Total Global Disasters	−0.002 (0.01)	−0.004 (0.01)	
Year-fixed Effects	N	N	Y
Observations	326	326	326
Log Likelihood	−278.88	−278.96	−278.35
θ	0.20*** (0.04)	0.20*** (0.04)	0.20*** (0.04)
Akaike Inf. Crit.	573.76	573.91	576.70

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. Estimated standard errors in parentheses.

Negative binomial regression models.

N Full Output for Instrumental Variables Analysis

Table 17: Stage I - Aid Scandals and Aid Channel Distribution, 2005-2010

	Log-Transformed Bypass Ratio		
	(8)	(9)	(10)
Major Aid Scandals	0.24*** (0.04)		
Extra-Continental Scandals		0.29*** (0.04)	
Affinity-weighted Scandals			0.29*** (0.08)
Governance Index	-1.86*** (0.13)	-1.81*** (0.13)	-1.83*** (0.13)
Civil Conflicts	0.09 (0.17)	0.10 (0.16)	0.09 (0.16)
Natural Disasters	0.02 (0.02)	0.03 (0.02)	0.03 (0.02)
Average Global GDP Growth	0.35*** (0.12)	0.31*** (0.12)	
Average Global Inflation	0.003 (0.002)	0.003 (0.002)	
Total Global Disasters	0.01*** (0.003)	0.01*** (0.003)	
Year-fixed Effects	N	N	Y
F-Statistic on Instrument	46.52	57.99	14.83
Observations	344	344	344
R ²	0.47	0.49	0.49
Adjusted R ²	0.46	0.48	0.48
Residual Std. Error	1.62 (df = 336)	1.59 (df = 336)	1.58 (df = 334)
F Statistic	42.83*** (df = 7; 336)	45.56*** (df = 7; 336)	36.30*** (df = 9; 334)

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. Estimated standard errors in parentheses.

OLS estimates.

Table 18: Stage II - Predicted Bypass Ratio and Unrest, 2005-2010

	Unrest Events		
	(8)	(9)	(10)
Predicted Bypass Ratio	-0.06** (0.02)	-0.04* (0.02)	-0.06** (0.03)
Governance Index	-1.73*** (0.43)	-1.49*** (0.38)	-1.88*** (0.48)
Civil Conflicts	0.25 (0.27)	0.21 (0.27)	0.26 (0.27)
Natural Disasters	0.15*** (0.03)	0.14*** (0.03)	0.14*** (0.03)
Average Global GDP Growth	-0.25 (0.20)	-0.26 (0.20)	
Average Global Inflation	0.01 (0.004)	0.005 (0.004)	
Total Global Disasters	-0.001 (0.01)	-0.002 (0.01)	
Year-fixed Effects	N	N	Y
Observations	338	338	338
Log Likelihood	-284.16	-285.02	-283.93
θ	0.20*** (0.04)	0.20*** (0.04)	0.20*** (0.04)
Akaike Inf. Crit.	584.32	586.05	587.87

*p<0.1; **p<0.05; ***p<0.01

Two-tailed tests. Estimated standard errors in parentheses.

Negative binomial regression models.

O Appendix References

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