

Aiding repression: The impact of US economic aid on political rights

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Abstract

For many developing countries, U.S. economic aid comprises a non-trivial share of state revenue and may affect government decision-making. I exploit plausibly exogenous variation in the legislative composition of the U.S. House of Representatives to construct a uniquely powerful instrumental variable for U.S. bilateral economic aid disbursements. For a sample of 150 countries from 1972-2008, U.S. aid raises political repression (measured along various dimensions) and extends the durability of authoritarian political institutions. U.S. aid engenders repression by empowering governments to exert less tax effort and be less accountable to their populations. Competing explanations related to rent-seeking are discounted. These findings counter the stated objectives of the U.S. government to promote democracy via bilateral economic assistance.

Key words: Foreign aid, Congress, political rights, taxes, political economy

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Since 2008 Ethiopia has been the recipient of the largest amount of U.S. aid in Africa, averaging around \$80 million per year. While intended to foster economic development, increasingly, practitioners are growing wary of the aid's political ramifications (Human Rights Watch 2010).¹ In the lead up to the country's national election in 2010, for example, foreign donors were charged of "subsidizing a regime that is rapidly becoming one of the most repressive and dictatorial on the continent" and that Western aid officials "seem reluctant to admit that there are two Prime Minister Meles Zenawis. One is a clubbable, charming African who gives moving speeches at Davos and other elite forums about fighting poverty and terrorism. The other is a dictator whose totalitarianism dates backs to cold war days" (Epstein 2010).

In contrast to the stated goal of promoting development in Ethiopia, U.S. aid to Egypt has sought to "invest" in regional stability and since the 1980s pumped in over \$60 billion dollars to the Mubarak regime (Sharp 2012). As is widely acknowledged in policy circles and academia, this aid enabled Mubarak to govern as an autocrat for almost 30 years. Foreign aid allowed Mubarak to win support from the military and to fund numerous instruments of political repression, such as the state's secret police.

Despite their divergent intentions (i.e., development in Ethiopia, political stability in Egypt), U.S. aid seems to have yielded the same political effects: repression. Facilitating repression in recipient countries, of course, is rarely the stated intentions of foreign aid donors (e.g., USAID 2012) and some scholarship has documented an empirical association between foreign aid and political liberalization (e.g., Wright 2009; Aronow et al 2012). Indeed,

¹In *Development with Freedom*, Human Rights Watch, for example, provides extensive documentation about how the Ethiopian government uses aid to repress. Based on interviews with 200 people in 53 villages and cities throughout the country, the report concludes that the Ethiopian government uses aid as a political weapon to discriminate against non-party members and punish dissenters, sending the population the draconian message that "survival depends on political loyalty to the state and the ruling party." For example, more than 50 farmers in three difference regions said that village leaders withheld government-provided seeds and fertilizers, and even micro-loans because they did not belong to the ruling party and some were asked to renounce their views and join the party to receive assistance.

untangling the effect of foreign aid on governance is plagued by endogeneity bias. On the one hand, U.S. aid may reward countries committed to political liberalization (e.g., aid to Eastern Europe after the Cold War); or aid may help stabilize autocratic allies (e.g., Egypt). To overcome this challenge, I leverage an innovative instrumental variables (IV) research design to provide robust, cross-national causal evidence that foreign aid from the world's largest bilateral donor - the United States - fosters repression and strengthens authoritarian politics by reducing a government's tax effort and relaxing the government's need to be accountable to its population.

In the United States, the funding and allocation of bilateral economic aid involves both the executive branch and Congress. Congress, in particular, is legally responsible for determining the aid budget. Building on this, the IV strategy exploits plausibly exogenous variation in the legislative fragmentation of the U.S. House of Representatives ($FRAG_t$) interacted with the probability a country receives U.S. aid (\bar{P}_i) as a uniquely powerful instrumental variable for U.S. bilateral economic aid to around 150 countries. The latter term, \bar{P}_i , captures how temporal changes ("shocks") in $FRAG_t$ are propagated to aid recipients. Countries with a higher value of \bar{P}_i are exposed to a greater aid shock.² The logic underlying the identification strategy builds on extant research that more fragmented legislatures tend to spend more (e.g., Roubini and Sachs 1989; Alesina and Tabellini 1990). And in the United States, empirically, when there is greater legislative fragmentation in Congress, average U.S. aid disbursements to recipients tend to be higher.

There are potentially several ways to measuring legislative fragmentation. In this paper, I employ a simple but surprisingly powerful one: the annual difference (absolute) between

²This identification strategy is similar to Nunn and Qian (2012) empirical analysis of the effect of U.S. food aid on civil war. Specifically, Nunn and Qian exploit variation in U.S. weather conditions interacted with the probability a country receives U.S. food aid as an instrumental variable for U.S. food aid measured in metric tonnes. The empirical strategy in this paper differs from Nunn and Qian in several ways. First, the outcome variable is political repression, not the onset (and duration) of civil war. Second, the key independent variable is U.S. bilateral economic aid (measured in US dollars) which is distinct (and does not include) U.S. food aid.

the number of Republican and Democrat legislators in the U.S. House of Representatives. This parsimonious measure ($FRAG_t$) avoids using partisan or ideological based differences across legislators or political parties (e.g., DW-NOMINATE scores) that are potentially endogenously with legislator’s preferences for foreign aid and foreign economic engagement more broadly (e.g., Therein and Noel 2000; Fleck and Kilby 2006; Milner and Tingley 2010, 2011). Indeed, since the composition of the House of Representatives changes in response to bi-annual elections, whose outcomes are primarily driven by district-level and U.S. national conditions (e.g. Fiorina 1978; Levitt and Synder 1997), $FRAG_t$ is a plausibly exogenous source of variation in U.S. aid disbursements that is uncorrelated with prevailing internal economic and political conditions in aid recipients.

Armed with this instrumental variable, the 2SLS results demonstrate that U.S. economic aid fosters repression on a variety of dimensions (and from different data sources), by diminishing the quality of political rights, civil liberties, and political participation. A one standard deviation increase in U.S. aid diminishes the quality of political rights by almost 1 index point (on a 7-point scale). U.S. aid also tends to increase actual (recorded) violations of human rights, such as those associated with religious freedom, personal integrity. The core finding linking U.S. aid to the diminution to political rights is robust to alternate samples (e.g., excluding “frequent” U.S. aid recipients), specifications (e.g., instrument with fragmentation from the US Senate), unobserved spatial and temporal heterogeneity (e.g., regional effects, Cold War) and concerns related to U.S. military aid and to crowding-out effects from non-U.S. aid donors.

Moreover, I show that U.S. aid fosters repression by lowering a government’s tax effort. A finding consistent with theories of government insularity in which lower tax effort is associated with diminished government accountability (e.g., North and Weingast 1989; Tilly 1992). Finally, to the extent that political repression is a rational and effective strategy for governments, especially in autocracies (Wintrobe 1998), I show that U.S. aid strengthens

authoritarian governance. U.S. aid expands the formal powers of government leaders, tends to lower the quality of democratic governance, and has a larger effect in stabilizing political institutions in autocracies than in democracies.

The findings from this paper contribute to several different literatures in political economy. The paper contributes to the literature on unearned income (e.g., rents from oil, minerals) and non-tax income to poor public policies and low economic development (e.g., Sachs and Warner 1995; Besley and Persson 2010, 2011). The also paper contributes to the expansive “aid effectiveness” literature, and in particular to scholarly and policy debates on whether good governance matters for aid to effectively raise economic growth (e.g., Burnside and Dollar 2000). This paper also ties in with the literature linking unearned income, and in particular foreign aid, to internal stability (e.g., Grossman 1992), rent-seeking (e.g., Svensson 2000), political violence (e.g., Besley and Persson 2011) and political reform (e.g., Casella and Eichengreen 1996).

The rest of the paper is structured as follows. The next section describes two channels and motivations existing research has identified for how (and why) governments use foreign aid to engage in repression. Section 2 describes the empirical strategy and data. The main results are reported in section 3. Section 4 examines mechanisms and section 5 concludes.

I. Conceptual framework

A. Taxation and accountability

The rich literature on the “natural resource curse” suggests a causal link between financial windfalls and political regime type (e.g., Madhavy 1970; Belbawi 1987; Ross 2001). The theoretical foundations of this literature extend from models of government insularity that view the evolution of the state as the result of bargaining between revenue-maximizing leaders and their citizens (North and Weingast 1989; Tilly 1992). As an application of

the government insularity model, the rentier state effect links the political consequences for most natural resource-rich countries (e.g., oil producing nations) that derive their income from the sale of a commodity as opposed to the tax base. The political bargains rentier states make with citizens hinge on the distribution of this income in exchange for staying in power, as opposed to relinquishing some influence over policy choice in exchange for taxes. As a consequence of these taxation and spending effects, rentier states (e.g., Saudi Arabia, Iran) are less likely to be accountable to their populations and increasingly repressive.

The logic of the rentier state argument has been applied to foreign aid (Moore 1998). Moore argues that as the share of government income from unearned income (e.g., foreign aid) increases, state/society relations are less likely to be “characterized by accountability, responsiveness, and democracy” (85). Thus, governments that do not collect taxes from their citizens because they are being financed by unearned income do not need to be as responsive to the needs of their populations (as would be the case with direct taxation) and may pursue policies that their populations would not necessarily choose. In particular, foreign aid can permit less tax effort and consequently less accountability. In autocracies, a logical extension is that surges in unearned income entrench the incumbent regime and deteriorate political participation and the overall quality of civil liberties and political rights.

B. Preventing rent-seeking

A repression effect associated with foreign aid is also likely to be exacerbated by group-fighting over unearned income. Models of group conflict generate predictions that a surge in unearned income raises rent-seeking behavior and heightens the probability of repression from an incumbent government. Basically, unearned government income increases the “size of the pie,” and if there are multiple groups dividing the pie, rent-seeking can contribute to increased fighting over it (Grossman 1992; Tornell and Lane 1999). These ideas have been formalized in various models. Svensson (2000), for instance, models a repeated game with

stochastic shocks, where the increase in rent-seeking behavior arises due to coordination failure across the interest groups. As applied to autocratic states where groups are less likely to attain their share of the pie through non-violent political channels, an exogenous financial windfall is likely to raise internal domestic discontent and incidences of political violence (Besley and Persson 2010, 2011). In response, the incumbent regime is likely to employ additional repressive tactics to quell this domestic unrest. For instance, foreign aid may be used to finance arm imports and expand overall military spending and personnel.

II. Empirical strategy

A. Importance of U.S. foreign aid

Distributing aid to other countries has been a key tool of American economic statecraft since World War II (Baldwin 1986). According to the U.S. government’s official bilateral aid agency, “U.S. foreign assistance has always had the twofold purpose of furthering America’s interests” by “expanding democracy and free markets while improving the lives of the citizens of the developing world” (USAID 2012). In pursuing this dual objective, the United States has allocated over \$700 billion in bilateral economic assistance since 1960. This figure excludes U.S. military aid, U.S. aid disbursements to multilateral organizations (e.g., World Bank, regional development banks), and food aid.

< **FIGURE 1 AROUND HERE** >

In aggregate dollars the United States is the world’s largest foreign bilateral aid donor. Figure 1 captures the temporal variation in total U.S. bilateral economic aid (left scale) and as a share of total DAC (right scale) aid since 1960. U.S. economic aid averaged nearly \$17 billion per annum in the 1960s, during a period of robust domestic economic growth coupled with relatively intense Cold War tensions. As super-power rivalry eased during the period of

detente and the U.S. experienced economic recession in the 1970s, U.S. economic assistance fell to around \$10 billion per annum (and maintained that annual average throughout the 1980s and 1990s). In the 2000s, primarily in response to the events of 9/11, U.S. bilateral economic aid increased substantially. Since 2001, U.S. bilateral economic aid has averaged over \$21 billion per year.

The U.S. share of total DAC aid has also varied over time, ranging from a high of 50 percent in 1963 to a low of 12 percent in 1997. Since 1960, aggregate U.S. aid has amounted to 28.5 percent of total DAC bilateral assistance, which exceeds the share of all other bilateral aid donors (see appendix table A1). Moreover, compared to the other 4 largest bilateral donors (France, Germany, Japan, and the United Kingdom), U.S. economic assistance also tends to more volatile (annually). From an econometric standpoint, this greater variability is advantageous as it will generate more precise estimates of the effect of U.S. bilateral economic aid on governance in recipient countries.

B. Legislative determinants of U.S. aid allocation

Legislative fragmentation and U.S. aid disbursements. The U.S. allocates varying amounts (and types) of bilateral economic aid to recipient countries over time. A large component of this allocative process is influenced by U.S. domestic politics. The majority of U.S. foreign assistance is contained in the international affairs budget requested and allocated through the State, Foreign Operations, and Related Agencies appropriations bill in the U.S. Congress.³ The legislative branch plays a critical role in U.S. foreign assistance, possessing the power both to authorize policy and appropriate funds. In response to the President's

³This is also referred to as Function 150 or the "150 account", and contains spending on global economic, diplomatic and humanitarian programs by the State Department (DOS), the United States Agency for International Development (USAID) and the Millennium Challenge Corporation (MCC) among others. The U.S. Global Leadership Coalition provides thorough updates on the status of 150 Account budget, including a summary of individual program, or "account," allocations.

budget submission (by February 2nd every year), the House and Senate Budget committees are the first to act, setting funding ceilings for various parts of the budget and guiding the work of both authorizing and appropriations committees.⁴ Each year, 11-12 appropriations bills, including the State, Foreign Operations, and Related Agencies bill, make their way through a long deliberative process in both the House and the Senate. The appropriations committees, in coordination with the authorizing committees, determine and allocate federal spending each year, including foreign aid. Frequently, the resulting appropriations bills and accompanying reports include numerous detailed directives on how funds should be spent by country and account (Lancaster 2000).

This legislative process frequently reflects the interests of those Congressmen involved (e.g., Therein and Noel 2000; Milner and Tingley 2010, 2011). Milner and Tingley (2010), for example, analyze votes related to U.S. foreign aid from members of the House of Representatives from 1979-2003 and find that members with a more right-leaning political ideology tend to oppose economic aid than do members from more left-leaning districts.⁵ Partisan affiliation often shapes the types of aid Congressmen support. For instance, analyzing U.S. bilateral aid for 119 countries from 1960-1997, Fleck and Kilby (2006) show that when Congress is more liberal (i.e., higher share of Democratic legislators) aid for economic development receives more weight than when Congress is more conservative. In contrast, when Congress is more conservative, aid for commercial purposes (e.g., aid that is tied to U.S. exports) has more weight.

The existence of these partisan differences over aid allocation suggests that the legislative composition of Congress (and the sub-committees that reflect this composition) influence aid

⁴Every year, by February 2nd the President submits a budget to the Congress outlining the Administration's spending priorities, including foreign aid outlays. Typically by April 15th, the budget committee sets spending caps for appropriations committees. By the end of May, the relevant sub-committees decide allocations to each function and by October 1st (typically), the full Congress votes on these allocations.

⁵In contrast, House members from more right-leaning districts favor *military aid* than do members from less right-leaning district.

disbursements. In particular, existing theories and empirical evidence suggests that a more fragmented legislature contributes to higher government spending (Roubini and Sachs 1989; Alesina and Tabellini 1990; Alesina and Perotti 1996), including foreign aid appropriations. The theoretical explanations stem from the well established proposition that higher levels of aggregate political conflict (e.g., stemming from greater ideological/partisan differences in legislatures) will result in equilibrium fiscal outcomes that favor greater spending since politicians will exhibit a greater proclivity in providing voters with program benefits (Alesina and Tabellini 1990; Roubini and Sachs 1989). Moreover, greater heterogeneity in partisan preferences over fiscal policy is likely to require legislative logrolling, thus contributing to higher overall spending to accommodate different spending initiatives and to better ensure the bill's passage in Congress. A number of studies have borne out this legislative fragmentation-spending relationship, both cross-nationally (Roubini and Sachs 1989; Alesina and Tabellini 1990) and, in particular for presidential systems, such as the United States (Alesina and Rosenthal 1995; Hankla 2012).

< **FIGURE 2 AROUND HERE** >

With respect to U.S. bilateral foreign aid disbursements, such a relationship is apparent in the legislative composition of the US House of Representatives. Figure 2 depicts a robust negative correlation between average U.S. aid disbursements to recipients and the absolute difference in the number of Republicans and Democrats in the US House of Representatives from 1972-2008. This relatively simple variable ($FRAG_t$) avoids using explicit measures of partisanship or ideology (e.g., DW-NOMINATE) which are potentially endogenous with actual preferences for foreign aid.⁶ In interpreting the measure of legislative fragmentation used in this paper, a *smaller difference implies greater legislative fragmentation*.

Such an interpretation is based on the legislative history of the U.S. House of Represen-

⁶DW-NOMINATE, for example, is based on roll-call votes, including those associated with foreign aid bills. To avoid introducing this bias, I use a much simpler measure.

tatives since World War II (figure 1 in appendix A graphs the legislative fragmentation of the House of Representatives over time). For much of the postwar period, the Democrats held an overwhelming majority in the House of Representatives, which corresponds to a large legislative gap in figure 2 and, based on the fragmentation-spending relationship, lower average U.S. aid disbursements to recipient countries. Following the Republican Revolution in 1994, Republicans gained control of the House and ushered in a period where the number of House Republicans converged to (and exceeded) the number of House Democrats. This contributed to a tightening of the legislative gap (i.e., greater fragmentation) and higher average aid disbursements to U.S. aid recipients since 1994.

Exogeneity. Exploiting the legislative fragmentation from the U.S. House of Representatives (rather than from the Senate) is advantageous for a number of reasons. First, all 435 members of House are subject to re-election every two years as opposed to only one-third of the 100 incumbent senators. Empirically, this means the House $FRAG_t$ exhibits greater temporal variation than the Senate $FRAG_t$. Second, as will be demonstrated shortly this greater variability in the House $FRAG_t$ generates a statistically stronger and more precise instrumental variable for U.S. bilateral aid disbursements than using the Senate $FRAG_t$. Third, and most importantly, $FRAG_t$ is a plausibly exogenous source of temporal variation in U.S. aid disbursements that is uncorrelated with political (and economic) conditions within U.S. aid recipients. Changes in the composition of U.S. House of Representatives occur bi-annually as a consequence of elections that are largely determined by local and national political and economic conditions, including (but not limited to) federal spending in Congressional districts (Levitt and Synder 1997), Presidential coattails (Campbell and Sumners 1990), midterm elections (Tuftes 1975), and retrospective economic voting (Fiorina 1978). To the best of my knowledge, political conditions in poor developing countries have not been identified as a determinant for electoral outcomes in the U.S. House of Represen-

tatives.

Aid frequency. The sensitivity of any particular country’s receipts of aid to $FRAG_t$ will be affected by probability that particular country actually receives U.S. aid in any given year. In fact, a striking feature of U.S. aid disbursements is that countries that receive U.S. aid more often tend to receive higher amounts of aid. Figure 3 plots a country’s average receipts of U.S. aid (over the period 1972-2008) against the country’s annual probability of receiving any U.S. aid, (\bar{P}_i) . For instance, Nigeria has a 68 probability of receiving U.S. aid in any given year, with aid disbursements averaging to \$31.3 million per annum. In contrast, Algeria receives U.S. a substantially amount of aid (\$41803 on average per annum) about once every three years. The cross-sectional relationship identified in figure 3 is analogous to Nunn and Qian’s (2012) observation that U.S. bilateral food aid is higher for countries that receive food aid more frequently from the United States.

< **FIGURE 3 AROUND HERE** >

C. Identification

I exploit these two sources of variation in U.S. aid disbursements to construct an innovative and powerful cross-national and time-varying instrumental variable. The instrument interacts the legislative fragmentation of the U.S. House of Representatives ($FRAG_t$) with the probability a country receives U.S. aid in any year (\bar{P}_i). An intuitive interpretation of this interaction term is that $FRAG_t$ measures the annual “shock” emanating from changes in the legislative composition of the U.S. House of Representatives *to* total U.S. bilateral aid and \bar{P}_i captures how the shock is propagated to *each* country. A country with a higher value of \bar{P} will experience a greater shock to U.S. aid. Such an interpretation is analogous to the instrumental variable employed by Nunn and Qian (2012) to evaluate the impact of U.S. food

aid on civil war. These scholars interact annual variation in U.S. weather conditions with the probability a country receives U.S. food aid in any particular year as an instrumental variable for U.S. food aid allocations (measured in metric tones).

Armed with this instrumental variable, the reduced form 2SLS setup is:

$$\text{First Stage: } AID_{it} = \alpha + \beta(FRAG_t \times \bar{P}_i) + \gamma X_{it} + \delta D_i + \kappa D_t + \epsilon_{it}$$

$$\text{Second Stage: } REPRESSION_{it} = a + b \times AID_{it} + c \times X_{it} + d \times D_i + f \times D_t + u_{it}$$

where i refers to the country, t indexes the year, X_{it} is a vector of controls, and D_i and D_t are dummies for country and year respectively. The inclusion of country fixed in both the first and, in particular, the second stage regressions implies the estimated coefficients will gauge each country's within-country variation in $REPRESSION_{it}$ due to the covariates (i.e., U.S. aid, economic growth, etc.). To account for potential serial correlation, the standard errors are clustered by country. This 2SLS strategy is similar in spirit to a difference-in-differences (DD) estimation strategy, where I compare U.S. bilateral economic aid receipts (and repression) between countries that frequently receive U.S. aid to countries that rarely receive U.S. economic aid during years in which the U.S. legislative fragmentation is small relative to years in which fragmentation is larger (and U.S. economic aid is lower). The main difference between the 2SLS approach and a DD strategy is that the treatment in this study is continuous, allowing me to exploit all of the variation in the treatment variable to gauge the causal effect of U.S. bilateral aid on repression.

Turning to each equation in the 2SLS setup. In the first stage, AID_{it} is each country's annual receipts of U.S. bilateral economic aid. Following Alesina and Dollar (2000) and Kuziemko and Werker (2006), aid is measured in logarithmic units. $FRAG_t$ the absolute difference in the number of House Republicans and House Democrats in each year, t . The

tendency for a country to receive any U.S. economic aid is given by $\bar{P}_i = \frac{1}{38} \sum_{t=1972}^{2009} P_i$, where P_i is equal to 1 if country i receives U.S. bilateral aid in year t .⁷ The instrument, which is constructed by interacting a plausibly exogenous term ($FRAG_t$) with one that is potentially endogenous (\bar{P}_i) can be interpreted as exogenous since the first stage regression controls for main effect of the endogenous variable (see Angrist and Krueger 1999 for a detailed explanation).⁸ In particular, since \bar{P}_i is specific to each country (i) and time-invariant, it is absorbed by the vector of country fixed effects. The inclusion of year fixed effects subsumes the main effect corresponding to $FRAG_t$.

In the second stage regression, $REPRESSION_{it}$, measures political repression on several dimensions, including the quality of political rights, political competition, and civil liberties. The paper’s core findings utilize the political rights (*POLITICAL RIGHTS*) index from Freedom House (although, as I will demonstrate, the findings are robust with alternate measures of repression). *POLITICAL RIGHTS* is 7 point index (1-7) where *higher values* correspond to a *lower* quality of political rights. This means that if U.S. aid fosters repression, the coefficient on AID_{it} (b) should be positive.

In both stages, I control for a parsimonious set of covariates (X_{it}) that both affect the allocation of aid in the first stage regression and $REPRESSION_{it}$ in the second stage regression. To properly account for recipient need, merit, and donor self-interest in the aid allocation equation (Hoeffler and Outram 2011), I control for log GDP per capita (“need”) and economic growth (“merit”).⁹ I also control for a country’s population size since smaller countries tend to receive more aid (Alesina and Dollar 2000) and the “cost” of political repression often varies by country size.

⁷Note that \bar{P}_i is specific to each country i and time-invariant.

⁸The identifying assumption is that the endogenous variable and the outcome of interest are jointly independent of the “exogenous” variable. For a more technical discussion, see section 2.3.4 of Angrist and Krueger (1999).

⁹For instance, foreign aid, in particular for economic development, is often channeled to poorer (i.e., “needier”) countries; and moreover, donors often reward those poor countries that are exhibiting signs of economic growth (i.e., “merit”).

Measuring donor self-interest, in contrast, is not as obvious as donor motives are numerous, often donor specific, and largely unobservable. For instance, geopolitical concerns (e.g., Cold War politics), especially for the United States, influence donor self-interest (Alesina and Dollar 2000). Since such concerns are often temporal, faced by all countries, and their effects are largely unobservable, including year fixed effects (D_t) is a means to account for these effects. The inclusion of year fixed effects also accounts for global trends that may affect the outcome variables, such as the “third wave” of democratization in the last quarter of the 20th century, the end of the Cold War, and global economic shocks (e.g., higher oil prices in the 1970s).

Several studies also find that donors strategically disburse greater amounts (and at a higher frequency) to recipients, for example, that are geographically closer or share a colonial (or other historical) relationship with the donor (Alesina and Dollar 2000). The inclusion of recipient country fixed effects (D_i) will account for these effects.¹⁰ Country fixed effects also account for observed and unobserved time-invariant country-specific factors that may affect a country’s quality of governance such as a country’s ethnic fractionalization, colonial history, legal system, and geographic location/proximity.

Geopolitical concerns, of course, are often time-varying and country specific. For instance, Kuziemko and Werker (2006) show that countries that rotate onto the United Nations Security Council (UNSC) are more likely to receive U.S. aid, in part, as an effort by the United States to “buy” support on the UNSC. Countries with formal military alliances with the United States are geo-strategically important and more likely to receive U.S. economic assistance (e.g., trade, foreign aid). To account for these effects, I control for whether a recipient is serving on the UNSC and is a U.S. military ally.

¹⁰For example, during the Cold War the United States and the Soviet Union “competed” for support from many African countries (by “buying” support with foreign aid). The inclusion of country fixed effects accounts for this geographic (time-invariant) bias in aid allocation.

Exclusion restriction. The validity of the paper’s identification strategy relies on whether the exclusion restriction is satisfied: the legislative fragmentation of the Congress affects repression abroad through U.S. economic aid only. A plausible alternate channel through which the composition of Congress might affect repression is via “aid for trade.” U.S. aid disbursements are often aimed at promoting U.S. exports (especially when Congress is comprised of more Republican legislators) to U.S. aid recipients (Fleck and Kilby 2006; Milner and Tingley 2010) and greater import penetration has been to better governance in countries (Ades and di Tella 1999). Thus, the existence of “aid for trade” implies that U.S. bilateral exports are expected to be negatively correlated with repression. A strategy to mitigate this potential violation of the exclusion restriction is to directly control for U.S. exports in the baseline 2SLS specifications.

D. Data

Measuring repression. I employ various measures of repression. The core measure of repression is the *POLITICAL RIGHTS* index created by Freedom House. Based on the opinions of experts, this index measures the ability for “people to participate freely in the political process, which is the system by which the polity chooses authoritative policy makers and attempts to make binding decisions affecting the national, regional, or local community” (e.g., the right to vote, the capacity of elected officials to have decisive votes on public policies). The index lies on a 7 point (1-7) scale, where *higher* values of *POLITICAL RIGHTS* (e.g., 6 or 7) correspond to *less* freedom. As an alternative measure of political freedom, I also use the “competitiveness of political participation” (*PARTICIPATION*) index from POLITY (Marshall and Jaggers 2010). This 5 point index (1-5) gauges the “extent to which alternative preference for policy and leadership can be pursued in the political arena.” *Lower* index values of *PARTICIPATION* correspond to lower competitiveness. For example, *PARTICIPATION* values of 1 and 2 correspond to “repressed” and “suppressed” levels of political

participation.¹¹

In addition to political participation, repression can be assessed on an individual's autonomy from the state. To capture such autonomy, I use the *CIVIL LIBERTIES* index created by Freedom House. Freedom House defines civil liberties to entail the “freedom to develop views, institutions, and personal autonomy from the state.” The country experts at Freedom House also consider the protection of minorities and the protection of freedom of expression in their annual assessments of each country's civil liberties. Like *POLITICAL RIGHTS*, *CIVIL LIBERTIES* is a 7 point index (1-7) where *higher* values correspond to *less* autonomy from the state.

Independent variables. The key independent variable, AID_{it} is the United States net disbursements of official development assistance (ODA) or official economic aid to over 150 countries.¹² There is wide cross-national and temporal variation in U.S. bilateral aid disbursements. Some countries (e.g., Algeria, Bhutan, Maldives) receive very little U.S. economic assistance aid (i.e., less than \$1 million), while some countries receive aid exceeding \$10 million per annum on average (e.g., Bangladesh, El Salvador, Ethiopia) and a few near (or over) \$1 billion annually (e.g., Egypt, Israel, Iraq after 2003). In the baseline estimating sample, the typical country receives U.S. economic aid equal to about \$60 million per annum.

The baseline economic and demographic controls are drawn from the World Development Indicators (World Bank 2010). UN Security Council membership is available from the United

¹¹“Repressed”, for instance, implies that the presence of “no significant oppositional activity.” “Suppressed” implies that the presence of “some” organized competition, but with “sharp limits in ways that exclude substantial groups (20% or more of the adult population) from participation.”

¹²Table A2 lists all U.S. aid recipients. The aid data is available from the OECD. Net disbursements are gross disbursements of grants and loans minus repayments of principal on earlier loans. ODA consists of loans made on concessional terms (with a grant element of at least 25 percent, calculated at a rate of discount of 10 percent) and grants made to promote economic development and welfare in countries and territories in the Development Action Committee (DAC) list of ODA recipients. Official aid refers to aid flows from official donors to countries and territories in part II of the DAC list of recipients: more advanced countries of Central and Eastern Europe, the countries of the former Soviet Union, and certain advanced developing countries and territories. Official aid is provided under terms and conditions similar to those for ODA.

Nations, alliances from the Correlates of War data set (Gibler and Sturkes 2004), and US exports from the International Monetary Fund (2012). Table 2 describes the variation in measures of repression, U.S. bilateral aid, and control variables for the baseline estimating sample of 151 countries over the period, 1972-2009 (table A2 lists the sample of U.S. aid recipients).

< **TABLE 1 AROUND HERE** >

E. Variation in repression

While the typical aid recipient has intermediate repression (i.e., *POLITICAL RIGHTS*=4.2), the quality of political rights exhibits wide cross-national and within-country variation. Table 2 shows that in any given year, nearly 1 out of 5 governments across all aid recipients may become more or less repressive from the previous year. A positive (negative) change in *POLITICAL RIGHTS* implies an increase (decrease) in repression from the previous year. Most of the time, repression increases or decreases by 1 index point from the previous year. Moreover, there is a greater probability of a decrease rather than an increase in *POLITICAL RIGHTS* from the previous year (10.4% versus 8.3%). This trend is consistent with the overall trend of political liberalization of developing countries since the 1970s (Huntington 1993).

< **TABLE 2 AROUND HERE** >

Splitting the sample of aid recipients in table 3 by whether a country is a “less” or “more” frequent recipient of U.S. bilateral economic aid reveals differences in the propensity of that country to experience a change in *POLITICAL RIGHT* from the previous year.¹³ Countries that receive U.S. aid more frequently (and, on average, with greater amounts of aid) are

¹³Countries that exhibit a tendency to receive U.S. aid above (below) the sample median ($\bar{P}=0.79$) are classified as a “more” (“less”) frequent aid recipient.

more likely to undergo changes in repression from the previous year. For example, in any given year, nearly 1 out of 4 governments in more frequent aid recipients are likely to become more or less repressive from the previous year, compared to only 15 percent of governments in less frequent aid recipients. This difference is statistically significant and suggests that there is a correlation between the frequency (and levels) of U.S. aid disbursements and the propensity for a country to experience changes in *POLITICAL RIGHTS*.¹⁴

III. Results

A. The determinants of U.S. bilateral economic aid

Table 4 reports the first stage regression describing the effects of the instrumental variable on the amount of U.S. foreign aid received by countries. In a specification without any fixed effects and recipient characteristics, the instrumental variable and its constituent parts (only) explains nearly 40 percent of the variation in U.S. aid disbursements (specification 1). The coefficient on the instrument is 0.09 and is highly statistically significant. For the typical country, which receives U.S. aid about 68 percent of the time in any given year, this estimated effect implies that a one standard deviation change in the legislative composition of the U.S. House of Representatives (=42) contributes to about a \$10 million increase in U.S. disbursements. In (1), the *F-statistic* on the instrumental variable is 40.1, which easily exceeds the threshold of weak instruments of 9.6 suggested by Stock et al (2002). In this specification, the constituent terms have the expected signs: countries that receive U.S. aid more frequently receive higher amounts (coefficient=7.42) while a larger difference between the number of House Republicans and Democrats lowers U.S. economic assistance (coefficient=-0.094).

The inclusion of country and year fixed effects, in specification 2 (which subsumes the

¹⁴Difference (=7 percent) is statistically significant with a p-value=0.000

constituent effects associated with the interaction term), also yields a strong and nearly identical effect of the instrumental variable on aid (coefficient=0.085). Moreover, in baseline specifications that also control for recipient characteristics (specification 3), the estimated effects on the instrumental variable are strikingly similar in magnitude and statistical significance to the effects from the parsimonious specifications. These additional baseline controls have the expected effects. For example, in (4), countries that exhibit economic growth are rewarded with more aid, while in poorer countries receive higher levels of US aid. Across all these specifications the instrumental variable is “strong”.

< **TABLE 3 AROUND HERE** >

Specifications (5) and (6) provide evidence that using the measure of Senate fragmentation can also serve as a valid instrument. However, this instrument is statistically less powerful than the one associated with the House, which exhibits greater temporal variation (i.e., each of the 435 House members face elections every 2 years, compared to only one-third of Senators) and yields greater precision. Thus, I use the measure of legislative fragmentation associated with the House of Representatives to gauge the effect of U.S. aid on political rights (although, the results are also robust to using the Senate measure).

B. U.S. bilateral economic aid fosters repression

Core 2SLS estimates. Table 4 reports estimated effect of U.S. bilateral economic aid on the core measure of repression, *POLITICAL RIGHTS* as well as on *CIVIL LIBERTIES* and *PARTICIPATION*. Panels A and B report estimates from separate OLS and 2SLS regressions respectively. Columns 1-3 report estimates from a parsimonious specification that includes country and year fixed effects only.¹⁵ Additional time-varying recipient country

¹⁵This parsimonious specification intentionally omits recipient characteristics that may be endogenous with U.S. aid receipts (e.g., log GDP per capita); and the inclusion of which may inadvertently introduce selection bias in the 2SLS specification. For instance, by increasing government revenues, foreign can raise

characteristics (i.e., log GDP per capita, GDP per capita growth, UNSC membership, etc.) are included as additional controls in the baseline specifications reported in columns 4-6.

To establish a baseline comparison, panel A reports the effect of aid on repression in OLS specifications. Across all six regressions, annual receipts of U.S. foreign aid tends to have no effect on repression.¹⁶ Panel B reports the core 2SLS estimates. In the parsimonious models (specifications 1 and 2), instrumented U.S. aid has a robust, positive and statistically significant effect on *POLITICAL RIGHTS* and *CIVIL LIBERTIES*. For these dependent variables, a positive coefficient implies a less freedom and civil liberties. In (3), instrumented U.S. aid has a negative and statistically significant effect on *PARTICIPATION* which implies a *reduction* in competitiveness of political participation. These estimated effects tend to be slightly larger in magnitude and stronger in statistical significance in the baseline specifications (columns 4-6).

< **TABLE 4 AROUND HERE** >

According to the estimates using the full set of baseline controls in column 4, for instance, a one standard deviation increase in log U.S. aid corresponds to 0.90 point increase in *POLITICAL RIGHTS*. Such an effect is equivalent to about a 0.44 standard deviation increase in *POLITICAL RIGHTS*. For the other measures of repression, a one standard deviation increase in log U.S. aid corresponds to a 0.30 and 0.50 standard deviation movement in *CIVIL LIBERTIES* and *PARTICIPATION* respectively. The 2SLS estimates are larger than the OLS estimates, suggesting that they correct for attenuation bias and thus adjust for the downward bias of U.S. aid that is often (and increasingly) directed to countries with higher

government consumption and thus, foster economic growth and higher GDP per capita. Controlling for GDP per capita (as well as per capita GDP growth) may therefore bias the estimated effect of foreign aid on political repression. Excluding potentially endogenous variables is a strategy to mitigate this form of selection bias (Nunn and Qian 2012)

¹⁶This effect likely reflects that U.S. aid is disbursed to both repressive and non-repressive countries for a variety of reasons (e.g., economic need, geopolitical significance). As consequence, a null effect of aid on repression in OLS specifications is unsurprising.

quality of democratic governance.¹⁷ Finally, panel C demonstrates that across all the 2SLS specifications, the instrumental variable is “strong” in the first-stage.

Heterogenous effects. The 2SLS estimates in panel B of table 4 report the average treatment effect of U.S. aid on repression. This effect, however, is likely to vary by recipient characteristics (see appendix for a detailed exposition). In particular, a country’s wealth and extant political institutions may influence a government’s decision to repress (e.g., Besley and Persson 2011). Empirically, this seems to be the case. For instance, U.S. aid received in poorer countries has a heightened effect in fostering repression, presumably, where the relative cost of repression is much lower (Smith 2008). Consistent with existing studies that repression is a viable, and often effective strategy in more authoritarian polities (Bueno de Mesquita et al 2003; Wintrobe 1998), U.S. aid strongly raises political repression in countries with predominantly authoritarian politics. In contrast, U.S. aid has no effect on repression in recipients with predominantly democratic politics. Moreover, the effect of U.S. aid on political rights is also likely to vary by the “intensity” of U.S. aid inflows in a recipient government’s overall expenditures, which includes money spent on resources for repression (e.g., security apparatus, weapons, etc.). In particular, the effect of aid on repression is larger in countries where U.S. aid receipts comprise a larger share of a government’s total expenditures.

Acts of repression. A potential drawback to gauging repression using *POLITICAL RIGHTS*, *CIVIL LIBERTIES* and *PARTICIPATION* is that these assessments are based on *perceptions* by third-party, country experts. To mitigate this concern, I leverage the CIRI Human Rights data set, which draws on annual reports from U.S. State Department and Amnesty International that describe actual violations of human rights (Cingranelli and Richards 2010).

¹⁷In table C1, specifications (1) and (3) show that more frequent aid recipients (i.e., higher value of \bar{P}_i) are associated with less repression.

This data contains standard-based quantitative information on government respect for 15 internationally recognized human rights for up to 195 countries, on an annual basis since 1981.¹⁸ I examine three different dimensions of government repression: religious freedom, empowerment rights, and political imprisonment.

Religious freedom indicates the extent to which the freedom of citizens to exercise and practice their religious beliefs is subject to actual government restrictions. Rather than providing raw counts of actual violations of religious freedoms, the data is grouped into a 3-point categorical variable: widespread violation (0), some violations (1), and no violations (2).¹⁹ A lower value, therefore, implies greater restrictions on religious freedom. Empowerment rights, in contrast, measures whether a government respects seven distinct human rights, such as freedom of speech, workers' rights, and freedom of movement.²⁰ Empowerment right ranges from 0 (no government respect for these seven rights) to 14 (full government respect for these seven rights). Finally, political imprisonment refers to the incarceration of people by government officials because of the beliefs.²¹ Like religious freedom, political imprisonment is coded on a 3 point scale, where a lower value implies a higher number of imprisoned individuals.²²

< **TABLE 5 AROUND HERE** >

¹⁸As Cingranelli and Richards note, the data set is designed for use by scholars and students who seek to test theories about the causes and consequences of human rights violations, as well as policy makers and analysts who seek to estimate the human rights effects of a wide variety of institutional changes and public policies including democratization, economic aid, military aid, structural adjustment, and humanitarian intervention.

¹⁹See Cingranelli and Richards (2010) for a justification/explanation why the counts are clustered into 3 categories.

²⁰Empowerment rights adds the Foreign Movement, Domestic Movement, Freedom of Speech, Freedom of Assembly and Association, Workers' Rights, Electoral Self-Determination, and Freedom of Religion indicators in CIRI (see CIRI for documentation).

²¹In particular, political imprisonment refers to the incarceration of people because of their speech; their non-violent opposition to government policies or leaders; their religious beliefs; their non-violent religious practices including proselytizing; or their membership in a group, including an ethnic or racial group.

²²Specifically, a score of 0 indicates that there were many people (more than 50) imprisoned because of their religious, political, or other beliefs in a given year; a score of 1 indicates that a few people were imprisoned (1-49); and a score of 2 indicates that no persons (0) were imprisoned for any of the above reasons in a given year.

Table 5 presents the effect of instrumented U.S. bilateral aid on these recorded acts of repression. Across all the specifications, U.S. bilateral aid exhibits a robust, negative effect; which implies an increase in human rights violations. The estimated effect in specification (1), for example, implies that a 2 standard deviation increase in U.S. aid contributes to 0.69 unit decrease in religious freedoms (which is equivalent to 0.80 standard deviation change in religious freedoms). Moreover, the estimated effects are substantively meaningful. For instance, the estimated effect in (3) implies that a 2 standard deviation increase in U.S. aid contributes to 0.42 unit decrease in political imprisonment. For the mean value of political imprisonment in U.S. aid recipients, this represents a movement from a moderate level of politically motivated incarcerations (0-49 citizens per year) to a higher level of incarcerations (more than 50 individuals per year).

Robustness. The baseline results reported in table 5 are robust to a variety of concerns (see appendix C for a more detailed exposition). The findings are not driven by potential “outlier” countries that receive high levels of U.S. aid nor countries U.S. aid more frequently. The detrimental effect of aid on political rights is also robust to the inclusion of potential confounding variables that are likely to mediate the effect of aid on repression, such as oil rents, measures of political institutions (e.g., POLITY score, executive constraints) and arms transfers.

The baseline results are robust to alternate specifications. For example, the results hold in specifications without country and year fixed effects (and variations thereof), with data averaged over 2 years (i.e., corresponding to the 2 year House electoral cycle), alternate measures of $FRAG_t$ (e.g., from the Senate), variations in the “propagation” mechanism (e.g., lags of whether a country receives U.S. rather the average probability of receiving aid), and two-way clustering (i.e, by country and year) that accounts for potential temporal and within-country correlation in the errors.

D. Potential threats to validity

Unobserved heterogeneity. The baseline findings are also robust to the inclusion of a variety of potentially unobservable spatial/geographic and temporal factors (table 6). For instance, political liberalization often occur within countries in the same geographic region (Huntington 1993). To account for these unobserved regional effects (many of which “deepen” or strengthen over time) I include a dummy for each region interacted with a year trend as additional controls. Including these differential trends (specification 1) does not change the core linking aid to repression.

In addition to unobserved cross-sectional heterogeneity, unobserved *temporal* effects may mediate the effect of U.S. foreign aid on repression. While the inclusion of year fixed effects accounts for common (temporal) shocks that affect all countries (e.g., oil price shocks, global financial crisis), it is plausible that unobserved “period” dynamics may also influence political repression. In particular, the geo-political rivalry between the United States and the USSR during the Cold War greatly influenced U.S. foreign policy interests. The Cold War rivalry often meant the U.S. supported highly repressive political regimes (e.g., dictatorships in Greece, Spain, Iran, Cuba, Haiti, Guatemala, Pakistan) and the United States often pursued different foreign policies in different geographic regions. A failure to account for the unobserved influences of the Cold War, therefore, may bias (upward) the effect of U.S. aid on repression.

To account for the differential effect of the Cold War across regions and countries, I estimate specifications with *Region x Cold War* and *Country x Cold War* trends (specifications 3-5). In these specifications, the substantive finding remains unchanged. A more directly strategy to explicitly purge concerns associated with the Cold War period is to estimate specifications for the post-Cold War period (1990-2009) only. In this restricted sample U.S. aid continues to exhibit a robust, positive effect on *POLITICAL RIGHTS* (specification 6). On balance, accounting for unobserved cross-sectional and temporal heterogeneity does not

alter the substantive finding that U.S. aid fosters repression.

< **TABLE 6 AROUND HERE** >

Crowding out. The interpretation of the main results is that U.S. foreign aid has a direct causal impact on political repression in recipient countries. However, a possible alternative explanation is that U.S. foreign aid affects repression by crowding out aid from other donors. For example, other donors may respond to an increase in U.S. foreign aid by reducing their own provisions. If these other forms of aid *reduce* repression, then this form of “crowding out” can explain why U.S. aid fosters repression. Conversely, U.S. aid could be negatively correlated with aid from other sources. And, the results could, instead, be explained by crowding out if U.S. aid reduces repression. In either instance, it is important to note that this does not undermine the validity of the estimated causal effects of U.S. aid on repression; but the mechanism of crowding-out offers a potentially different channel than those discussed in section 2.

To explore whether crowding out exists, I regress U.S. aid on total bilateral economic aid from *other* donors. In an OLS model, U.S. aid is positively associated with aid from other countries (table 7, specification 1) implying that U.S. aid does not crowd out from other donors. More interestingly, instrumented U.S. aid is uncorrelated with total aid from other donors (specification 2). Since the identification strategy exploits the legislative composition of the U.S. House Representatives to estimate variation in U.S. bilateral aid disbursements, the instrument should have *no* bearing on the aid allocation decisions in other countries (whose aid decisions are likely to be responsive to domestic politics in their own countries). Thus, the null finding associated with instrumented U.S. aid in (2) is reassuring and provides additionally validity to the instrumental variable research design.

< **TABLE 7 AROUND HERE** >

Military aid. A related concern is the potential bias introduced by excluding the effect of

U.S. military aid on repression. The expected direction of the omitted variable bias, however, is unclear ex-ante. To mitigate this concern, U.S. bilateral military aid disbursements is included as additional control variable in both the first and second stage. In the second stage of such a specification (table 7, specification 3), U.S. bilateral economic aid still exhibits a robust, positive effect on *POLITICAL RIGHTS*, while U.S. bilateral military aid has a negative effect. This latter finding is consistent with the fact that the United States tends to disburse greater amounts of military aid to military allies, many of whom tend to be democratic.

Any form of aid, of course, is often argued to be “fungible” in a government’s budget and thus should permit government repression. Thus, as an additional robustness check, I evaluate the effect of U.S. bilateral economic *and* military aid disbursements on repression. Doing so reveals that in the second stage, instrumented U.S. bilateral economic and military aid exhibits a positive and significant effect on *POLITICAL RIGHTS* (specification 4). The instrumental variable, however, is less powerful in the first stage than that associated with U.S. bilateral economic aid (see table 5). Indeed, the instrumental variable does not explain much of the variation in the U.S. military aid disbursements (specification 5). The estimated effect is essentially zero, statistically insignificant and the associated *F-statistic* (=0.68) is very low. The weak relationship between the instrumental variable and U.S. bilateral military aid disbursements is reassuring and reflects the fact that Congress has less influence over the allocation of U.S. military aid than it does over U.S. economic aid.

IV. Mechanisms

A. Rent seeking vs. taxation

As discussed in section 2, existing studies have identified two broad mechanisms through which foreign aid can lead to political repression: less tax effort and rent-seeking. The rent-

seeking explanation posits that foreign aid represents unearned government revenue that groups within society fight over. In response, governments engage in repression to quell this unrest. According to this explanation, therefore, U.S. aid should be positively associated with greater political discontent. Panel A in table 8 presents evidence that this is not the case. For instance, U.S. aid exhibits no effect on low-level intensity discontent, such as on the number of riots (specification 1) and anti-government demonstrations (specification 2). Nor is U.S. aid a robust determinant of higher forms of political discontent, such as attempts and/or successful killings of government officials (specification 3). Given these null effects, unsurprisingly, aid does not exhibit a robust (positive) effect on arms imports, military personnel, and total military spending. Nor does aid shift the allocation of a government's budget towards military spending (results discussed in appendix B).

There is more support for the second mechanism that foreign aid permits governments to exert less tax effort and consequently become less accountable and more repressive. According to the government insularity model, as a source of non-tax (unearned) government income, aid inflows should allow governments to reduce their tax effort. Empirically, therefore, the amount of taxes collected from individuals as a share of total government revenue should be negatively correlated with aid inflows. To test this mechanism, I regress taxes collected from income, profits, and capital gains (% government revenue) on U.S. bilateral aid disbursements plus the baseline controls.²³ A reduction in this dependent variable implies the exertion of less tax effort because a government is able to derive a larger share of its revenue from non-tax sources.

Empirically, *POLITICAL RIGHTS* is negatively correlated with tax effort, which affirms the underlying theoretical conjecture of a negative relationship between political repression and taxation (specification 4). Turning to the conjectured mechanism, U.S. aid seems to reduce tax effort. In an OLS model, U.S. aid is negatively correlated with tax effort (specifi-

²³Data on tax collection for a large set of developing countries is only available from 1990 onwards.

cation 5). Instrumented U.S. aid has a much larger and statistically significant effect on tax effort. A one standard deviation increase in U.S. aid, for instance, lowers tax effort by 13 percentage points (specification 6). Moreover, controlling for repression does not attenuate the negative effect of instrumented U.S. aid on tax effort (specification 7).

< **TABLE 8 AROUND HERE** >

B. U.S. aid and authoritarian stability

The ability and willingness for governments to engage in repression is feasible (and rational) if the aid inflows help make the country's underlying political regime, institutions, and rules more stable. Indeed, several extant studies demonstrate that foreign aid can help stabilize political regimes (Wright 2009; Ahmed and Werker 2013). To evaluate this claim for the case of U.S. bilateral economic aid, I use 3 variables from the POLITY data set to measure political institutional stability (Marshall and Jaggers 2010). The first is the executive constraints index (*XCONST*) which measures the extent of institutionalized constraints on the decision-making powers of chief executives, whether are individuals or collectives. *XCONST* lies on a 7 point (1-7) scale, where lower values correspond to a reduction in constraints (i.e., an aggrandizement of executive control) and usually implies a decline in a country's democratic governance. The second is the broader *POLITY* index (-10 to +10) where higher values correspond to higher quality democratic political institutions. The third, *DURABLE*, is a measure of regime durability. If a country experiences at a least a 3 index point change in its *POLITY* score from the previous year, *DURABLE* is reset to zero. If there is no such 3-point change, *DURABLE* increases by one point.

Panel B in table 8 shows that U.S. aid fosters more authoritarian-leaning political institutions. In specification (1), a one standard deviation increase in U.S. aid lowers *XCONST* by 0.75 index points, which is equal to about a one-third standard deviation change in

XCONST. Specification (2) substantiates this, as a one standard deviation increase in U.S. aid lowers *POLITY* by 3 index points (or roughly 0.43 standard deviation in *POLITY*). Instrumented U.S. also tends to make a country’s existing political regime (institutions and rules) less prone to rapid changes and overall more durable in both authoritarian and democratic regimes (specification 3). Yet the effects are not uniform across regime type. In countries that tended to have autocratic politics over the sample period, the effect of instrumented U.S. aid on *DURABLE* is more than twice as large as the effect in democratic polities (specifications 4 and 5).²⁴ This divergence is consistent with the notion that repression can be a prudent strategy for regime stability in autocracies (Wintrobe 1998; Smith 2008).

V. Conclusion

As an instrument of American economic statecraft, the United States maintains that it uses its bilateral economic aid to promote its national interest by expanding democracy and free markets, while improving the lives of citizens in developing countries (USAID 2012). Indeed, in pursuit of this objective, Congress has occasionally passed laws codifying the United States commitment to not funding gross violators of human rights (e.g., 1997 Leahy Amendment).²⁵ This paper provides robust, cross-national evidence casting doubt on these objectives.

Empirically, untangling the actual effects of aid on political rights can be difficult. Since donors allocate aid strategically and recipients choose to accept or decline aid, attempts at identifying variation in political rights using variation in observed aid flows faces endogeneity problems. To overcome this challenge, I leverage an instrumental variables research

²⁴Countries with an average *POLITY* score below zero over the sample period are classified as autocratic. Those with average *POLITY* scores equal to zero or greater than zero are classified as democratic.

²⁵In 1997, Congress passed the Leahy Amendment to the Foreign Operations Assistance Act which cuts aid to military or police forces engaged in gross human rights violations unless the government of the recipient country has taken steps to bring the violators to justice. Along these lines, Congress passed the International Religious Freedom Act (IRFA) I 1998 to channel US security and developmental assistance to governments other than those found to be engaged in gross violations of the right to freedom of religion.

design which relies on the institutionalized process of aid allocation in the United States, involving the participation of both the executive and legislative branch to pass a foreign aid budget. In particular, I exploit plausibly exogenous variation in the legislative composition of the U.S. House of Representatives to construct an innovative cross-national, time-varying instrumental variable for U.S. bilateral economic aid. For a sample of 150 countries, U.S. aid deteriorates the quality of political rights, civil liberties, and political participation and tends to strengthen authoritarian politics. U.S. aid does by reducing a government's tax effort and lowering its incentive to be accountable to its people.

These findings, of course, counter the stated aim of U.S. aid to not harm political rights (as discussed above) and refute some recent empirical findings that aid may improve political rights in countries that have exhibited a commitment to political liberalization (e.g., Aronow et al 2012; Wright 2009).²⁶ More broadly, the paper's findings speak to the large scholarly and policy interest in evaluating the "effectiveness of aid" and confirms existing studies that aid is often a political "curse" (Djankov et al 2008). Moreover, the finding that U.S. aid fosters repression by lowering tax effort supports the notion that aid may be viewed as a form non-tax (unearned) government income that lowers political accountability. Finally, by leveraging domestic political conditions in a donor government, this paper introduces a novel source of variation in aid disbursements that could be applied in settings outside the United States. For instance, exploiting the composition and/or fragmentation of legislative bodies in other bilateral donors or on governing boards in international organizations (e.g., World Bank, various regional banks) may allow researchers to gauge the effect of foreign aid from other donors on a variety of outcomes in developing countries.

²⁶Aronow et al, for example, use an innovative natural experiment to document that aid from the European Union improves political rights, but concede that their empirical strategy may be inherently biased in their favor since the EU tends to disburse to countries that have exhibited political liberalization (which the authors refer to as "positive conditionality"). In contrast, U.S. aid is allocated to countries for a variety of reasons (e.g., geopolitical significance, development needs) that may not hinge on whether a recipient is committed to enhancing or harming the quality of political rights.

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Figures

Figure 1: U.S. bilateral aid

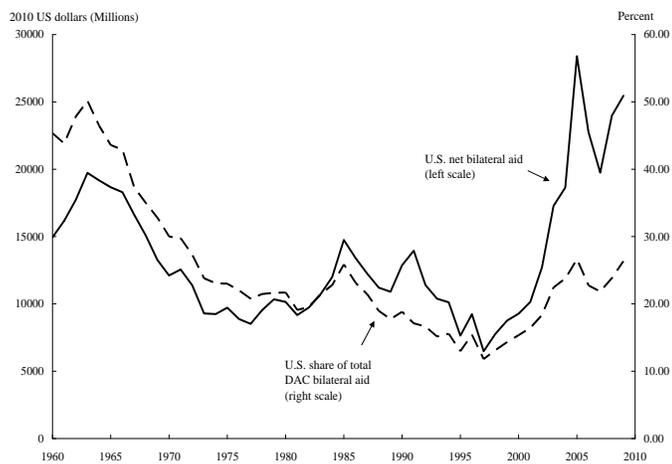


Figure 2: The legislative composition of the House of Representatives and annual average U.S. bilateral aid disbursements

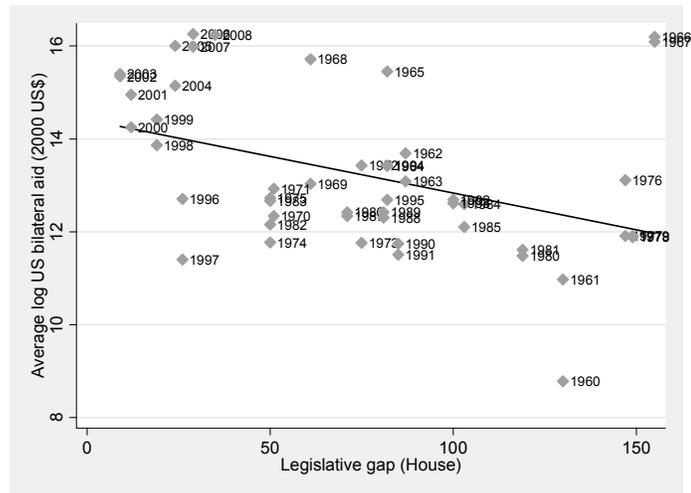


Figure 3: Probability of receiving U.S. bilateral aid and country average U.S. bilateral aid disbursements

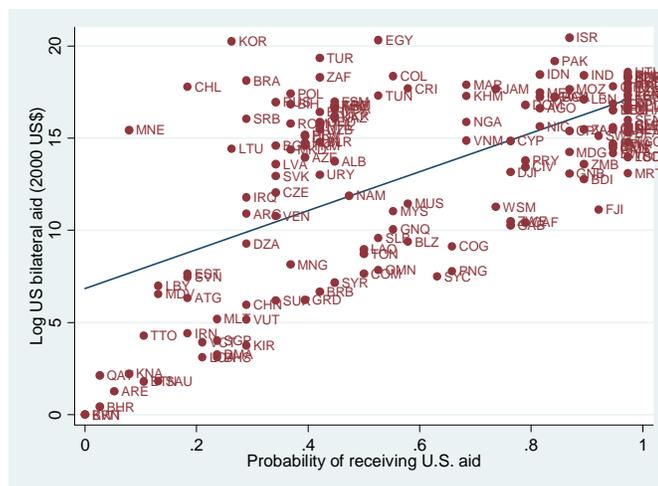


Table 1: Descriptive statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
	Measures of repression				
Political rights	3843	4.16	2.02	1	7
Civil liberties	3843	4.13	1.63	1	7
Political participation (PARCOMP)	3196	2.56	1.32	0	5
	Independent variables				
Legislative gap ($FRAG_t$)	3843	61.86	41.62	9	149
Prob. of receiving U.S. aid (\bar{P}_i)	3843	0.68	0.29	0	0.97
$FRAG_t \times \bar{P}_i$	3843	43.53	37.15	0	145.08
U.S. bilateral economic aid (2000 US\$)	3843	5.94E+07	2.47E+08	0	6.80E+09
Log U.S. bilateral economic aid (2000\$)	3843	13.68	6.35	0	22.64
Log GDP per capita (2000 US\$)	3843	6.94	1.27	4.13	10.77
Growth in GDP per capita (% annual)	3843	1.79	6.74	-50.00	90.50
Log population	3843	15.39	1.95	9.86	21.00
UN Security Council member	3843	0.05	0.22	0	1.00
US military ally	3843	0.19	0.39	0	1.00
Log US exports (2000 US\$)	3843	17.89	4.05	0	25.41

Notes: Sample of countries from baseline OLS model.

Table 2: Annual change in *POLITICAL RIGHTS* for U.S. aid recipients

Change from previous year	U.S. aid recipients		
	All	Less frequent	More frequent
		<u>Percent</u>	
-4	0.16	0.05	0.25
-3	0.62	0.37	0.87
-2	1.45	1.11	1.78
-1	8.15	6.68	9.57
No change	81.33	84.90	77.91
1	6.96	5.98	7.89
2	0.62	0.42	0.81
3	0.36	0.32	0.41
4	0.23	0.00	0.46
5	0.08	0.11	0.05
6	0.03	0.05	0.00

Notes: Sample of U.S. aid recipients, 1972-2009. Countries that exhibit a tendency to receive U.S. aid above (below) the sample median ($\bar{P}=0.79$) are classified as a “more” (“less”) frequent aid recipient.

Table 3: The determinants of U.S. bilateral economic aid

Dependent variable:	Log U.S. bilateral aid (2000 US\$)					
	(1)	(2)	(3)	(4)	(5)	(6)
		Parsimonious specification	Baseline specification	Senate specification	Senate legislative gap	
Fragmentation (House) x Probability of receiving U.S. aid	0.092 (0.014)***	0.085 (0.014)***	0.086 (0.014)***	0.087 (0.016)***		
Fragmentation (Senate) x Probability of receiving U.S. aid					0.294 (0.078)***	0.321 (0.084)***
Fragmentation (House)	-0.094 (0.013)***		-0.085 (0.012)***			
Fragmentation (Senate)					-0.331 (0.071)***	
Probability of receiving U.S. aid	7.423 (1.204)***		6.498 (1.310)***		8.558 (1.347)***	
Log GDP per capita (2000 US\$)			-0.001 (0.292)	-1.481 (0.841)*	-0.046 (0.307)	-1.330 (0.896)
GDP per capita growth (% annual)			0.034 (0.019)*	0.024 (0.012)*	0.047 (0.020)**	0.026 (0.013)**
Log population			0.651 (0.170)***	2.200 (1.928)	0.638 (0.175)***	1.007 (1.898)
UN Security Council member			-0.137 (0.417)	-0.035 (0.345)	-0.167 (0.411)	-0.003 (0.343)
US military ally			0.573 (0.586)	-0.164 (0.557)	0.186 (0.623)	-0.186 (0.617)
Log US exports (2000 US\$)			0.102 (0.063)	0.108 (0.066)	0.142 (0.069)**	0.137 (0.072)*
Constant	10.437 (1.019)***	9.145 (0.900)***	-1.216 (3.216)	-12.194 (32.382)	-3.089 (3.259)	6.902 (32.046)
Country fixed effects	N	Y	N	Y	Y	Y
Year fixed effects	N	Y	N	Y	Y	Y
Number of observations	3953	3953	3843	3843	3843	3843
Number of countries	153	153	151	151	151	151
R-squared	0.38	0.63	0.43	0.64	0.39	0.63
<i>F-statistic</i> on excluded instrument	40.09	35.39	38.37	30.58	14.02	14.65

Notes: Estimation via OLS. Robust standard errors, clustered by country reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%.

Table 4: The impact of U.S. foreign aid on repression

	Parsimonious specification			Baseline specification		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Estimation via OLS						
Dependent variable:	Political rights	Civ. Liberties	Participation	Political rights	Civ. Liberties	Participation
Log US bilateral aid (2000 US\$)	-0.009 (0.010)	-0.011 (0.008)	-0.002 (0.009)	-0.008 (0.010)	-0.011 (0.010)	-0.002 (0.009)
R-squared	0.74	0.77	0.73	0.74	0.77	0.75
B. Estimation via 2SLS						
Dependent variable:	Political rights	Civ. Liberties	Participation	Political rights	Civ. Liberties	Participation
Log US bilateral aid (2000 US\$)	0.130 (0.051)**	0.071 (0.037)*	-0.127 (0.058)**	0.141 (0.052)**	0.076 (0.037)**	-0.103 (0.057)*
R-squared	0.66	0.73	0.61	0.65	0.72	0.67
C. First stage estimates						
Dependent variable:	Log U.S. bilateral aid (2000 US\$)					
$FRAG_t \times \hat{P}_i$	0.085 (0.014)**	0.085 (0.014)**	0.079 (0.020)**	0.087 (0.016)**	0.087 (0.016)**	0.08 (0.022)**
R-squared	0.63	0.63	0.61	0.64	0.64	0.61
F -statistic on excl. instrument	35.39	35.39	15.68	30.58	30.58	13.03
Controls (all panels):						
Recipient characteristics	N	N	N	Y	Y	Y
Country fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
No. observations (all panels)	3953	3953	3269	3843	3843	3220
No. countries (all panels)	153	153	130	151	151	129

Notes: Estimation via OLS in panel A and 2SLS in panel B. Robust standard errors, clustered at the country level reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Political rights and Civil Liberties are both 7-point index (1-7) from Freedom House where higher values correspond to fewer political freedoms/civil liberties. Participation is a 5 point index of the "competitiveness of participation" from POLITY where lower values correspond to a less competitive participation. Specifications 4-6 include time-varying recipient characteristics. These recipient characteristics include: GDP per capita (2000 US\$), GDP per capita growth (% annual), log population, UN Security Council membership, US ally, and log US exports (2000 US\$). All specifications include country and year fixed effects. These coefficients and a constant are not reported.

Table 5: The impact of U.S. economic aid on *acts* of repression

Dependent variable:	Religious freedom	Empowerment rights	Political imprisonment
	(1)	(2)	(3)
Log U.S. bilateral aid (2000 US\$)	-0.049 (0.019)**	-0.218 (0.098)**	-0.033 (0.019)*
Number of observations	3172	2827	2835
Number of countries	153	150	150
R-squared	0.53	0.71	0.56

Notes: Estimation via 2SLS. Robust standard errors, clustered by country reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. All specifications control for log GDP per capita, GDP per capita growth (% annual), log population, UN Security Council membership, US ally, log US exports (2000 US\$), country and year fixed effects. These coefficients and a constant are not reported. As described in the text, downward movements in each dependent variable represents a reduction in the quality of rights.

Table 6: Accounting for unobserved heterogeneity

Dependent variable:	Political rights					
	(1)	(2)	(3)	(4)	(5)	(6)
Log U.S. bilateral aid (2000 US\$)	0.098 (0.035)***	0.086 (0.036)**	0.104 (0.038)***	0.059 (0.034)*	0.079 (0.037)**	0.078 (0.039)**
Leg. fragmentation		0.002 (0.001)**		0.009 (0.008)		
Cold War dummy		4.697 (0.533)***		3.538 (0.407)***		
Recipient characteristics	Y	Y	Y	Y	Y	Y
Country fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y		Y		Y	Y
Region fixed effects	Y	Y	Y	Y	Y	Y
Year trend	Y					
Region FE x Year	Y					
Region FE x Cold War						
Country FE x Cold War		Y	Y	Y	Y	Y
<i>Restricted sample</i>						
Post Cold War period				Y		Y
Number of observations	3843	3843	3843	3843	3843	2328
Number of countries	151	151	151	151	151	150
R-squared	0.7	0.71	0.7	0.82	0.81	0.83

Notes: Estimation via 2SLS. Robust standard errors, reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. All specifications control for the following recipient characteristics: log GDP per capita (% annual), GDP per capita, growth (% annual), log population, log US exports, UN Security Council membership, and US military ally. These coefficients, a constant, and those associated with various fixed effects and/or differential trends are not reported.

Table 7: The impact of alternate types of aid on political rights

Dependent variable:	Log total DAC bilateral aid excluding U.S. (2000 US\$)		Political rights		Log U.S. military aid (2000 US\$)
	(1) OLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	
Method of estimation:					
<i>Instrumented variables</i>					
Log U.S. bilateral aid (2000 US\$)	0.022 (0.010)**	-0.009 (0.038)	0.163 (0.067)**		
Log U.S. bilateral economic and military aid (2000 US\$)				0.734 (0.359)**	
<i>Non-instrumented variables</i>					
Fragmentation x Prob. of receiving U.S. aid					-0.006 (0.007)
Log U.S. bilateral military aid (2000 US\$)			-0.095 (0.40)**		
<i>F-statistic on instrument</i>					
Number of observations	3786	3786	2733	9.49	0.68
Number of countries	151	151	142	2829	3417
R-squared	0.74	0.74	0.69	142	162
			0.55	0.71	

Notes: Estimation via 2SLS. Robust standard errors, clustered by country reported in parentheses. Specifications (1) and (2) evaluate the presence of crowding out. Specifications (3), (4), and (5) evaluate the effects of U.S. bilateral military aid. All specifications control for the following recipient characteristics: log GDP per capita, (% annual), GDP per capita growth (% annual), log population, log US exports, UN Security Council membership, and US military ally. These coefficients, country and year fixed effects are not reported. All specifications control for the following recipient characteristics: log GDP per capita (% annual), GDP per capita growth (% annual), and log population. These coefficients, a constant, and those associated with various fixed effects and/or differential trends are not reported.

Table 8: Mechanisms

		Panel A. Taxation vs. Rent-seeking					
Dependent variable:		Riots		Anti-government demonstrations		Assassinations	
Method of estimation:		2SLS	2SLS	2SLS	OLS	OLS	2SLS
Log U.S. bilateral aid (2000 US\$)		0.004 (0.028)	0.002 (0.026)	0.033 (0.029)	(4)	(5)	(7)
Political rights					-0.126 (0.607)	-0.346 (0.208)*	-2.136 (0.988)**
Number of observations		3655	3655	3656	1379	925	925
Number of countries		145	145	145	136	113	113
R-squared		0.30	0.27	0.22	0.87	0.83	0.66

		Panel B. Authoritarian stability					
Dependent variable:		XCONST		POLITY		DURABLE	
Method of estimation:		2SLS	2SLS	All countries	Autocracies	Democracies	2SLS
Log U.S. bilateral aid (2000 US\$)		-0.117 (0.066)	-0.468 (0.228)**	1.707 (0.812)**	2.443 (1.306)*	1.131 (0.507)**	1.131 (0.507)**
Number of observations		3320	3336	3370	2048	1322	1322
Number of countries		129	129	130	74	56	56
R-squared		0.66	0.68	0.58	0.42	0.81	0.81

Notes: Robust standard errors, clustered by country reported in parentheses. † = significant at 11%; * = significant at 10%; ** = significant at 5%; *** = significant at 1%. In panels A and B, all regressions control for log GDP per capita (2000 US\$), GDP per capita growth (% annual), log population, UN Security Council membership, US military ally, log US exports (2000 US\$), and include country and year fixed effects. These coefficients and a constant are not reported. In panel B, autocracies are countries with an average POLITY score less than 0 over the sample period. Democracies are countries with an average POLITY score greater than or equal to 0 over the sample period.

ONLINE APPENDIX

Appendix A: Data

Table A1: Top bilateral economic aid donors

Country	Bilateral aid, net disbursements (2010 US\$, Millions)		Share of total DAC bilateral aid Annual average (%)
	Annual average	Std. Dev	
United States	13451.5	4972.1	28.5
France	6802.8	1133.8	14.7
Japan	6347.9	3181.8	12.5
Germany	4771.9	1474.8	9.8
United Kingdom	3306.2	1442.5	6.9

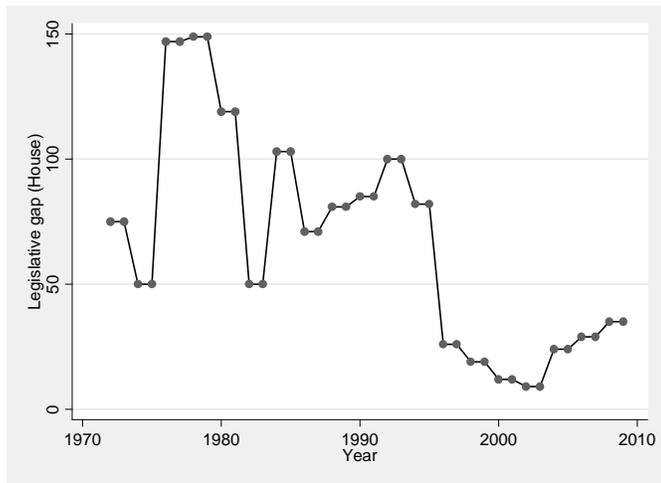
Notes: Data from OECD, 1960-2009. Based on authors' calculations.

Table A2: U.S. aid recipients

Country	Country	Country
Albania	Gambia, The	Oman
Algeria	Georgia	Pakistan
Angola	Ghana	Palau
Antigua and Barbuda	Grenada	Panama
Argentina	Guatemala	Papua New Guinea
Armenia	Guinea	Paraguay
Azerbaijan	Guinea-Bissau	Peru
Bahamas, The	Guyana	Philippines
Bahrain	Haiti	Poland
Bangladesh	Honduras	Qatar
Barbados	Hungary	Romania
Belarus	India	Russian Federation
Belize	Indonesia	Rwanda
Benin	Iran, Islamic Rep.	Samoa
Bhutan	Iraq	Saudi Arabia
Bolivia	Israel	Senegal
Bosnia and Herzegovina	Jamaica	Serbia
Botswana	Jordan	Seychelles
Brazil	Kazakhstan	Sierra Leone
Bulgaria	Kenya	Singapore
Burkina Faso	Kiribati	Slovak Republic
Burundi	Korea, Rep.	Slovenia
Cambodia	Kuwait	Solomon Islands
Cameroon	Kyrgyz Republic	South Africa
Cape Verde	Latvia	Sri Lanka
Central African Republic	Lebanon	St. Kitts and Nevis
Chad	Lesotho	St. Lucia
Chile	Liberia	St. Vincent and the Grenadines
China	Libya	Sudan
Colombia	Lithuania	Suriname
Comoros	Macedonia, FYR	Swaziland
Congo, Dem. Rep.	Madagascar	Syrian Arab Republic
Congo, Rep.	Malawi	Tajikistan
Costa Rica	Malaysia	Tanzania
Cote d'Ivoire	Maldives	Thailand
Croatia	Mali	Togo
Cyprus	Malta	Tonga
Czech Republic	Mauritania	Trinidad and Tobago
Djibouti	Mauritius	Tunisia
Dominica	Mexico	Turkey
Dominican Republic	Moldova	Turkmenistan
Ecuador	Mongolia	Uganda
Egypt, Arab Rep.	Montenegro	Ukraine
El Salvador	Morocco	United Arab Emirates
Equatorial Guinea	Mozambique	Uruguay
Eritrea	Namibia	Uzbekistan
Estonia	Nepal	Vanuatu
Ethiopia	Nicaragua	Venezuela, RB
Fiji	Niger	Vietnam
Gabon	Nigeria	Zambia
		Zimbabwe

Notes: Countries (151) in baseline estimating sample.

Figure 1: Fragmentation in the House of Representatives, 1972-2008



ONLINE APPENDIX

Appendix B: Additional results

Heterogenous effects

The core results in panel B of table 4 report the average treatment effect of U.S. aid on repression. This effect, however, is likely to vary by recipient characteristics. A transparent strategy to investigate the presence of these (potential) heterogeneous treatment effects is to estimate specifications where the sample is “split” (e.g., above and below the sample mean or median for the conditioning variable). The results are reported in table B1.

Income. U.S. aid engenders repression in poorer countries. In a sample of aid recipients with average income *below* than the sample median, instrumented U.S. aid has a robust positive (coefficient=0.147) and highly statistically significant (p-value<0.01) effect on *POLITICAL RIGHTS* (specification 2). In contrast, for a sample of countries with average per capita GDP *exceeding* the sample median, instrumented aid has a lower effect, both in magnitude and statistical significance, on *POLITICAL RIGHTS*.

Political institutions. Consistent with extant theories linking repression with authoritarian stability (e.g., Wintrobe 1998), U.S. aid fosters repression in authoritarian regimes. While U.S. has a small effect on political rights in democracies (i.e., countries with average POLITY scores exceeding or equal to zero over the sample period), the coefficient estimate of U.S. is statistically insignificant (specification 3). In countries with predominantly authoritarian politics (i.e., countries with average POLITY scores below zero over the sample period), in contrast, instrumented U.S. aid has a sizeable and statistically significant effect on *POLITICAL RIGHTS* (specification 4). The estimated effect of U.S. aid on repression in autocracies is 5 times larger (coefficient=0.296) than the effect in democracies.

Share of government expenditures. The effect of U.S. aid on political rights is also likely to vary by the “intensity” of U.S. aid inflows in a recipient government’s overall expenditures, which includes money spent on resources for repression (e.g., security apparatus, weapons, etc.). In split samples by countries that are below and above the median share of U.S. to total government consumption expenditures, U.S. aid engenders a larger repression effect in high “intensity” than in low intensity aid recipients (by a factor of 3). Results reported in specifications (5) and (6).

Table B1: Heterogenous effects, by recipient characteristics

Dependent variable:	Political rights					
	(1)	(2)	(3)	(4)	(5)	(6)
	By income		By regime type		By US aid share of government consumption	
	High	Low	Democracies	Autocracies	High	Low
Log US bilateral aid (2000 US\$)	0.077 (0.067)	0.147 (0.051)***	0.053 (0.054)	0.296 (0.146)**	0.152 (0.063)**	0.033 (0.086)
R-squared	0.74	0.56	0.57	0.61	0.58	0.85
Number of observations	1918	1925	1819	2024	2345	1498
Number of countries	85	66	78	73	81	70

Notes: Estimation via 2SLS. Robust standard errors, clustered at the country level reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Political rights is the 7-point index (1-7) from Freedom House where higher values correspond to fewer political freedoms. All regressions control for GDP per capita (2000 US\$), GDP per capita growth (% annual), and log population and include country and year fixed effects. These coefficients and a constant are not reported. “High” and “low” refer to the values above and below the sample median for average country GDP per capita (specifications 1 and 2) and average U.S. aid as a share of final government consumption expenditures (specifications 5 and 6). In specification (3), the sample is restricted to countries with an average POLITY score greater than or equal to zero (Democracies). In specification (4), the sample is restricted to countries with an average POLITY score less than zero (Autocracies).

Mechanism: Impact of U.S. aid on military spending

Table 8 shows that U.S. aid does not seem to foster rent-seeking. Consequently governments may not have to channel that aid to repress the population via militarization. Table B2 provides evidence that U.S. aid is not a robust determinant of higher military employment (specification 1) nor higher expenditures on arm imports (specification 2) and overall higher military spending (specifications 3 and 4). In these specifications, U.S. aid exhibits a positive association with militarization, but the effects are not statistically significant at conventional levels.

Table B2: Effect of U.S. aid on military spending

Dependent variable:	Log armed forces personnel	Log arms imports	Log military expenditure	Mil. Expenditure (% govt exp.)
	(1)	(2)	(3)	(4)
Log U.S. bilateral aid (2000 US\$)	0.022 (0.026)	0.069 (0.115)	0.101 (0.094)	0.874 (1.065)
Number of observations	2303	2131	2042	799
Number of countries	142	147	135	108
R-squared	0.91	0.43	0.88	0.56

Notes: Estimation via 2SLS. Robust standard errors, clustered by country reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. All regressions control for log GDP per capita (2000 US\$), GDP per capita growth (% annual), log population and include country and year fixed effects. These coefficients and a constant are not reported. In (2), log arm imports is measured in 1990 US dollars. In (3), log military expenditures are measured in 2000 US dollars.

ONLINE APPENDIX

Appendix C: Robustness

The baseline results reported in table 5 are robust to a variety of concerns. This appendix discusses the various robustness checks.

Outliers. The findings are not driven by potential “outlier” countries that receive high levels of U.S. aid nor countries U.S. aid more frequently. Specifications 1-4 in table C1 provide evidence that the findings are not driven by potential “outlier” countries. In (1), the estimating sample excludes country-year observations from the top decile of U.S. receipts (in aggregate dollars), while (2) is restricted to a sample of countries that receive U.S. aid more than 90 percent of the time (i.e., $\bar{P}_i > 0.90$). In these restricted samples, which essentially exclude countries that receive highest amounts of aid and/or aid most frequently, the estimated effect of instrumented of U.S. aid on political rights is quite similar in magnitude (and statistical significance) to the effect reported in table 3.

The confluence of country size and aid receipts introduces an additional potential source of “influential” observations. Specifically, the costs of repression are often lower in less populated countries and these countries tend to receive disproportionately higher amounts of aid (Alesina and Dollar 2000). Thus, the findings may be overly influenced by the presence of small countries in the estimating sample. To allay this worry, I estimate the effect of instrumented U.S. on political rights in samples that exclude countries with populations less than 2 and 5 million people respectively. Doing so significantly reduces the estimating sample. In (3), countries with populations less than 2 million people are excluded, resulting in a sample that is 30 percent smaller than the full sample in table 3, specifications 1 and 2. Nearly 50 percent of the full sample is excluded in (4) which excludes countries with populations less than 5 million people. Despite these smaller estimating samples, the substantive effect that U.S. aid worsens political rights remains unchanged. Moreover, the coefficient estimates are quite larger than the baseline findings, suggesting that the inclusion of smaller countries actually attenuates the effect of U.S. aid on political rights.

Confounders. The detrimental effect of aid on political rights is also robust to the inclusion of potential confounding variables that are likely to mediate the effect of aid on repression, such as various dimension of political institutions (e.g., executive constraints), regime durability, unearned income (e.g., log fuel exports), and arms transfers. Specifications 5-8 in table C1 control for these confounding effects. In (5), for example, countries with higher constraints on the executive (i.e., which tend to be more democratic) tend to be less repres-

sive (as expected). In (7), higher fuel exports are positively correlated with repression. On balance, while these additional controls are endogenous with aid receipts (e.g., Alesina and Dollar 2000), the existence of a robust effect of U.S. aid on political rights in their presence implies that the findings are not driven by their omission.

Specifications without fixed effects. The core findings also do not hinge on the inclusion of fixed effects (table C2, specifications 1-3). For instance, in specifications that exclude one or both country and year fixed effects (but do include the appropriate constituent parts of the instrumental variable, $FRAG_t$ and \bar{P}_i), the effect of U.S. aid on repression remains unchanged from the baseline 2SLS estimates. In particular, in a specification without either country or year fixed effects (column 3), the effect of instrumented aid on political rights (coefficient=0.14 ,SE=0.065) is similar to the estimates reported in table 5, panel B. Moreover, the coefficients on the constituent terms in the second stage regression are informative: $\bar{P}_i=-2.134$ (SE=1.005) and $FRAG_t=0.01$ (SE=0.002). The negative effect on \bar{P}_i , in particular, means that more frequent aid recipients tend to be *less* repressive and corroborates the earlier discussion that more frequent U.S. aid recipients are *not* inherently more likely to be repressive. Therefore, the results are not upward biased due to the interaction $FRAG_t$ with \bar{P}_i and as such the identified effect stemming from the interaction effect is not endogenous (that could be attributed to reverse causality). Indeed, in an OLS specification that regresses political rights on \bar{P}_i , $FRAG_t$ and the baseline controls, the effect of \bar{P}_i on *POLITICAL RIGHTS* is negative and not statistically significant (column 4).

Temporal correlation. While the baseline specifications account for potential within-country variation in the errors (by clustering at the country level), an additional concern is the possibility of temporal correlation in the errors. This may present since the instrumental variable leverages year “shocks” to the legislative composition of the House of Representatives to identify the effect of U.S. aid on repression. A solution to this worry is to cluster the standard errors at both the year and country level. As evident in specifications 5-7 of table C2, doing so does not diminish the statistical significance of U.S. aid *POLITICAL RIGHTS*, *CIVIL LIBERTIES*, and *PARTICIPATION*.

2 year average (by election). Elections in the House of Representatives are held every 2 years. In the absence of a non-electoral removal from office (e.g., resignation due to a scandal), this means legislative fragmentation varies typically at a 2 year frequency. Thus, the core 2SLS estimates (which uses annual level data) may be more statistically significant due to an inflated sample size. To allay this concern, I estimate the baseline specifications for a sample

restricted to election years (e.g., 1972, 1974, etc.) and with data averaged over the 2 year electoral period. Specifications 8-10 shows that doing so does not diminish the substantive findings of U.S. aid on repression. Average aid raises average *POLITICAL RIGHTS* and *CIVIL LIBERTIES* and lowers average *PARTICIPATION*.

Alternate measure of legislative “fragmentation”. As evident from the first stage regressions in table 3, the legislative gap associated with the House of Representatives is preferred as it is a statistically more powerful determinant of movements in U.S. bilateral aid disbursements than the legislative gap with the Senate (see table 3). Nevertheless, as evident from table C3, the substantive effects of U.S. aid on repression is unchanged in specifications that instrument for U.S. aid using the interaction of the legislative fragmentation of the U.S. Senate with \bar{P}_i and variations thereof.

Alternate measures of the “propagation” mechanism \bar{P}_i . Skeptics may still be concerned that use of \bar{P}_i in the identification strategy is problematic. One such concern is that the probability a country receives U.S. aid varies over time, which may for instance reflecting the changing relationship between the President and Congress and/or changing aid priorities of the US government (Fleck and Kilby 2006). To account for this possibility, I construct alternate instrumental variables for contemporaneous U.S. aid disbursements (i.e., in year t). Specifically, I interact $FRAG_t$ with dummy varies equal to 1 if a country received any U.S. aid in the past 1, 2, and 5 years (and zero otherwise). Panels A and B of table C4 report the second and first stage regressions respectively. In panel A, U.S. aid instrumented (with any of these alternate “propagation” mechanisms) has a positive and significant effect on political rights. This finding holds in specifications with country fixed effects only (specifications 1, 3, and 5) as well as in specifications with both country and year fixed effects (specifications 2, 4, and 6).

In the first stage these alternate instruments are strong determinants of U.S. aid disbursements. Across all 6 specifications in panel B, the F-statistic on the excluded instrument exceeds the threshold of weak instruments (=10). Moreover, as expected, countries that received U.S. in the recent past are more likely to get higher levels of contemporaneous aid. The estimated effects on P_{t-1} , P_{t-2} , and P_{t-5} decrease with as the lag increases.

Table C1: Alternate samples and additional confounders

Dependent variable:	Political rights							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log U.S. bilateral aid (2000 US\$)	0.128 (0.054)**	0.160 (0.060)***	0.266 (0.120)**	0.319 (0.145)**	0.093 (0.057)*	0.122 (0.066)*	0.138 (0.084)*	0.229 (0.102)**
Executive constraints index					-0.598 (0.045)***			
Regime durability						0.029 (0.010)***		
Log fuel exports (2000 US\$)							-0.040 (0.141)	
Arms imports (1990 US\$)								0.019 (0.015)
Country fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.67	0.68	0.46	0.42	0.8	0.65	0.67	0.56
Number of observations	3422	2541	2761	2009	3196	3346	2682	2086
Number of countries	135	114	112	86	128	129	145	144

Notes: Estimation via 2SLS specifications. Robust standard errors, clustered at the country level reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Log U.S. bilateral (2000 US\$) is instrumented. All specifications control for the following recipient characteristics log GDP per capita, GDP per capita growth (% annual), log population, UN Security Council member, US military ally, and log US exports (2000 US\$). These coefficients, country and year fixed effects, and a constant are not reported. In column 1, the top decile of U.S. bilateral aid recipients (by dollar amount) are excluded. In column 2, countries that receive U.S. bilateral aid more than 90 percent of the time are excluded. In columns 3 and 4, countries with populations less than 2 million and 5 million are excluded respectively.

Table C2: Alternate specifications

Dependent variable	(1)		(2)		(3)		(4)	(5)		(6)		(7)		(8)	(9)		(10)	
	2SLS	OLS	Fixed effects	2SLS	2SLS	2SLS	OLS	2SLS	Two-way clustered	Civ. Liberties	Participation	Pol. Rights	Civ. Liberties	Participation	Pol. Rights	Civ. Liberties	Participation	
Method of estimation:	2SLS	OLS	Fixed effects	2SLS	2SLS	OLS	OLS	2SLS	Two-way clustered	Civ. Liberties	Participation	Pol. Rights	Civ. Liberties	Participation	Pol. Rights	Civ. Liberties	Participation	
Log U.S. bilateral aid (2000 US\$)	0.131 (0.065)**	0.137 (0.063)**	0.119 (0.053)**	0.137 (0.063)**	0.137 (0.063)**	0.137 (0.063)**	0.137 (0.063)**	0.141 (0.052)**	0.076 (0.035)**	0.076 (0.035)**	-0.103 (0.054)*	0.139 (0.053)***	0.076 (0.037)**	-0.115 (0.066)*				
Log U.S. aid (2000 US\$), 2 yr avg.																		
Prob. of receiving U.S aid	-2.207 (0.989)**		0.003 (0.001)***	-2.113 (0.986)**	0.01 (0.002)***	-0.538 (0.515)	0.007 (0.001)***											
Legislative $FRAG_t$ (House)																		
Country fixed effects	N	N	Y	N	N	N	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.08	0.67	0.67	0.02	0.23	0.23	0.23	0.65	0.72	0.72	0.67	0.7	0.76	0.7	0.7	0.76	0.7	0.7
Number of observations	3843	3843	3843	3843	3843	3843	3843	3843	3843	3843	3220	1696	1696	1399	1696	1696	1399	1399
Number of countries	151	151	151	151	151	151	151	151	151	151	129	151	151	129	151	151	129	129

Notes: Estimation via OLS and 2SLS. Robust standard errors, clustered at the country level. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Log U.S. bilateral (2000 US\$) is instrumented. All specifications include the following recipient characteristics: log GDP per capita, GDP per capita growth (% annual), log population, membership on the UN Security Council and US ally. These coefficients, country and year fixed effects and a constant are not reported. In columns 5-7, the standard errors are clustered at the country and year level. In columns 8-10, all dependent variables, aid, and recipient characteristics are 2 year averages.

Table C3: Instrument with Senate fragmentation

Dependent variable:	Political rights			
	(1)	(2)	(3)	(4)
Log U.S. bilateral aid (2000 US\$)	0.215 (0.114)*	0.187 (0.111)*	0.158 (0.078)**	0.174 (0.077)**
Prob. of receiving U.S aid	-2.913 (1.447)**	-2.864 (1.439)**		
$FRAG_t$ (Senate)	0.066 (0.016)***		0.011 (0.006)*	
Country fixed effects	Y	N	Y	Y
Year fixed effects	Y	Y	N	Y
R-squared	.	.	0.62	0.61
Number of observations	3843	3843	3843	3843
Number of countries	151	151	151	151

Notes: Estimation via 2SLS. Robust standard errors, clustered at the country level. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Log U.S. bilateral (2000 US\$) is instrumented with the interaction of Prob. of receiving aid x Legislative fragmentation in the US Senate. All specifications include the following recipient characteristics: log GDP per capita, GDP per capita growth (% annual), log population, membership on the UN Security Council and US ally. These coefficients, country and year fixed effects (where applicable) and a constant are not reported.

Table C4: Instrument with different measures of “propagation” (\bar{P}_i)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second stage (2SLS)						
Dependent variable:	Political rights					
<i>Instrumented</i>						
Log US aid (2000 US\$)	0.253 (0.099)**	0.225 (0.079)***	0.207 (0.090)**	0.192 (0.075)**	0.126 (0.063)**	0.109 (0.052)**
<i>Non-instrumented</i>						
FRAG (House)	0.003 (0.001)**		0.002 (0.001)**		0.002 (0.001)**	
Receives aid in year t-1	-2.324 (0.928)**	-2.006 (0.741)***				
Receives aid in year t-2			-1.466 (0.676)**	-1.311 (0.556)**		
Receives aid in year t-5					-0.312 (0.211)	-0.217 (0.181)
R-squared	0.56	0.61	0.59	0.62	0.67	0.69
Panel B: First stage (OLS)						
Dependent variable:	Log US aid (2000 US\$)					
FRAG (House) x Rec. aid in t-1	0.025 (0.006)***	0.028 (0.006)***				
FRAG (House) x Rec. aid in t-2			0.03 (0.007)***	0.033 (0.007)***		
FRAG (House) x Rec. aid in t-5					0.04 (0.009)***	0.044 (0.009)***
FRAG (House)	-0.023 (0.006)***		-0.026 (0.006)***		-0.035 (0.009)***	
Rec. aid in t-1	7.252 (0.700)***	6.939 (0.694)***				
Rec. aid in t-2			4.801 (0.769)***	4.534 (0.769)***		
Rec. aid in t-5					-0.441 (0.665)	-0.904 (0.668)
R-squared	0.76	0.77	0.71	0.72	0.64	0.66
<i>F</i> -statistic on excluded instrument	18.04	22.21	16.68	20.62	20.58	24.81
Country fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	N	Y	N	Y	N	Y
Number of observations	3741	3741	3702	3702	3580	3580
Number of countries	151	151	151	151	150	150

Notes: Estimation via OLS in panel A and 2SLS in panel B. Robust standard errors, clustered at the country level reported in parentheses. * = significant at 10%; ** = significant at 5%; *** = significant at 1%. Rec. aid in t-1 is an indicator variable to 1 if a country received any U.S. aid in the previous year and zero otherwise. Rec. aid in t-2 is an indicator variable equal to 1 if a country received any U.S. aid two years ago and zero otherwise. Rec. aid in t-5 is an indicator variable equal to 1 if a country received any U.S. aid five years ago and zero otherwise. All specification control for log GDP per capita (2000 US\$), GDP per capita growth (% annual), log population, UN Security Council membership, US military ally, log US exports (2000 US\$). These coefficients, country fixed effects, year fixed effects (where applicable) and a constant are not reported.