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Does Foreign Aid Harm Political Rights? Evidence from U.S. Aid

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ABSTRACT

The United States is the world's largest bilateral foreign aid donor. For many developing countries, this aid constitutes a nontrivial share of state revenue with the capacity to shape a recipient's governance. Whether such assistance has a causal effect on political liberalization, however, is plagued by concerns with endogeneity bias. To mitigate this concern, I exploit plausibly exogenous variation in the legislative fragmentation of the U.S. House of Representatives to construct a powerful instrumental variable for U.S. bilateral aid disbursements. For a sample of 150 countries from 1972 to 2008, U.S. aid harms political rights, fosters other forms of state repression (measured along multiple dimensions), and strengthens authoritarian governance. U.S. aid does so by weakening government accountability via the taxation channel. These findings counter the publicly stated objectives of the U.S. government to foster political liberalization abroad via bilateral economic assistance.

Since 1960, the United States has disbursed over $700 billion in bilateral economic aid; an amount exceeding that of any other nation. Such assistance has been an instrument of U.S. foreign policy (Baldwin, 1986; Morganthau, 1962). In official statements, U.S. foreign aid is declared to promote political liberalization. For instance, on its official website, the U.S. Department of

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State proclaims: "The protection of fundamental human right was a foundation stone in the United States over 200 years ago. Since then, a central goal of U.S. foreign aid has been the promotion for human rights, as embodied in the Universal Declaration of Human Rights."[1] In line with these promulgations, recent scholarship has documented an association between foreign aid (primarily from Western donors) and political liberalization, especially in the post-Cold War period (e.g., Dunning, 2004; Scott and Steele, 2011; Wright, 2009).

The significance of the United States as the world's largest bilateral aid donor has prompted several prominent studies to examine the sole effect of U.S. aid on political liberalization, in general finding a positive association (e.g., Finkel *et al.*, 2007; Scott and Steele, 2011). Aid, however, has the capacity to engender repression (e.g., Smith, 2008). For instance, aid can help finance a government's repressive apparatus (e.g., strengthen the regime's security forces, etc.) and "buy off" opposition. Aid is also a form of unearned income, which can diminish a government's accountability to its population (by reducing the its tax effort).

Of course untangling the *causal* impact of U.S. foreign aid on political liberalization is problematic since U.S. aid disbursements are often correlated with a recipient's political conditions. On the one hand, U.S. aid may reward countries committed to political liberalization, such as U.S. aid to shore up nascent Eastern Europe democracies after the end of the Cold War. On the other hand, aid may help stabilize autocratic allies (e.g., Egypt) and thus undermine political liberalization. Indeed, most extant studies that attempt to gauge the effect of aid, in particular of U.S. bilateral assistance on political liberalization, tend to sidestep these concerns with endogeneity. Consequently, these studies do not causally identify the effect of U.S. foreign aid on political rights.

To overcome this challenge, I leverage an instrumental variables (IV) strategy to provide robust, cross-national causal evidence that U.S. foreign aid harms political rights. The research design builds on the institutional foundations of U.S. aid decisions, in which 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 (P_i) as a powerful instrumental variable for U.S. bilateral aid to around 150 countries. The latter term, P_i, captures how temporal changes in FRAG_t are propagated to aid recipients. The logic underlying the identification strategy builds on

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[1] See http://www.state.gov/j/drl/hr/index.htm.

existing research that more fragmented legislatures tend to spend more (Alesina and Tabellini, 1990; Roubini and Sachs, 1989). And in the United States, empirically, when there is greater legislative fragmentation in Congress, average U.S. aid disbursements tend to be higher.

Armed with this instrumental variable and controlling for prevailing explanations for political liberalization (such as economic development, growth; time-invariant country characteristics, e.g., colonial legacy; and temporal effects, e.g., Cold War, War on Terror), the 2SLS results demonstrate that U.S. economic aid harms political rights.[2] U.S. aid also harms measures of civil liberties, political participation, and religious freedoms and tends to make authoritarian institutions more durable. And U.S. aid tends to engender these effects by reducing government tax effort; thus weakening the tradeoff between government accountability and taxation (e.g., Tilly, 1992).

The core finding that U.S. aid harms political rights is robust to an exhaustive list of potential concerns, including: alternate samples (e.g., samples restricted to the post-Cold War and pre-9/11 2001 periods only) and specifications (e.g., lags and leads of aid, additional controls), different formulations of the instrumental variable, unobserved spatial and temporal heterogeneity (e.g., differential regional and Cold War trends, "diffusion"), worries regarding the "crowding out" of non-U.S. aid, as well as potential violations of the exclusion restriction.

This article contributes to several different bodies of literature. It relates to ongoing scholarly and policy debates about the "effectiveness" of aid in economic development (e.g., Burnside and Dollar, 2000) and the potential pernicious effect of U.S. involvement in developing countries (e.g., Choi and James, 2013; Morganthau, 1962). The paper's core findings are also consistent with related scholarship demonstrating a pernicious effect of aid on political development through international organizations, such as the UN Security Council (Bueno de Mesquita and Smith, 2010) and the UN Human Rights Commission (Casper, 2015). More broadly, the article speaks to scholarship linking unearned (non-tax) government income (e.g., oil rents, foreign aid) to poor socio-economic outcomes (e.g., Poe, 1992; Regan, 1995; Smith, 2008).

The rest of the article is structured as follows. The next section describes the existing empirical findings and channels through which aid can affect political rights. Section 2 describes the empirical strategy and data. The results are reported in Sections 3–5. Section 6 concludes.

[2]The 2SLS specifications (e.g., using aid disbursements on an annual basis) follow the standard approach employed in existing studies, such as Nunn and Qian (2014), Scott and Steele (2011), and Wright (2009).

1 Existing Research

1.1 Channels: How Foreign Aid Can Affect Political Rights

1.1.1 Aiding political liberalization

Among both policymakers and academics there is a lively debate evaluating whether foreign aid can engender political liberalization. Those in the affirmative posit various channels. Based on existing theories of democratization, Finkel *et al.* (2007) argue aid can foster political liberalization in two ways: "indirectly, by transforming some of the structural conditions that serve as prerequisites for regime transition or survival, and directly by empowering agents (individuals, political institutions, and social organizations) that struggle for regime change in the domestic arena" (p. 410). Aid can also accelerate political liberalization for reform-minded governments. For instance, Wright (2009) shows that dictators with large distributional coalitions and who have a good chance of winning fair elections tend to respond to aid by democratizing.

"Context" may also matter, such as geopolitics. Dunning (2004), for example, examines the Cold War and post-Cold War periods separately and argues that donor intent changed with the end of bipolar power struggle. No longer concerned with the potential defection to the Soviet Union, donors are less willing to prop up authoritarian governments with aid. Building on this argument, Bermeo (2011) provides evidence that aid from democratic donors (e.g., the United States, the United Kingdom, etc.) in the post-Cold War era has helped improve democratic governance in recipient countries.

1.1.2 Aiding repression

Various scholars, of course, argue otherwise and maintain that aid diminishes political rights (e.g., Bueno de Mesquita and Smith, 2009). This perspective identifies at least two broad channels: the relationship between taxation and government accountability; and by preventing rent seeking.

The first channel stems from models of government insularity, which view the evolution of the state as the result of bargaining between revenue-maximizing leaders and their citizens (Tilly, 1992). In these models, the political bargains governments make 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. These models build on a long lineage of scholarship in political science — both in democratic theory (e.g., Locke, 1690) and political economy (e.g., Przeworski *et al.*, 1999) — arguing that a lack of accountability facilitates less representation from the masses and an expansion of executive power at expense of political rights. Whereas democratic institutions (e.g., elections) can serve to constrain government power, in autocracies (where the selectorate is smaller), the relationship between lack of accountability and repression is heightened as surges in unearned income can

further entrench the incumbent regime and harm political rights (Conrad and DeMeritt, 2012).[3] While these accounts do not explicitly delineate how quickly a reduction in accountability translates to a reduction in political rights, as the empirics will later show (see Table 5, panel C) the effect can be rather quick (e.g., at an annual frequency).

Foreign aid has been situated within such models. For example, Moore (1998) 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" (p. 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 repress their populations.

More than a mere academic argument, the logic that that nontax revenue can induce a lack of accountability and concomitantly diminish political rights has motivated social action. Historically, for example, such reasoning underlay the "no taxation without representation" motives that fueled the American Revolution. More recently, this logic has been applied to explain contemporary human rights abuses. For example, since foreign aid constitutes a nontax form of revenue akin to natural resource rents, "Human Rights Watch believes that a country's substantial reliance on natural resources can have a negative impact on human rights" (HRW, 2004, p. 57). In particular, "when a ruler of a governing elite are undemocratic or otherwise unaccountable to their citizens, poor management, poor economic decision-making, corruption and human rights abuses thrive" (HRW, 2004, p. 57).

A second, and not necessarily mutually exclusive, channel posits that aid can hurt political rights by exacerbating group conflict over unearned income. 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. As applied to autocracies where groups are less likely to attain their share of the pie through nonviolent means, an exogenous financial windfall is likely to raise internal domestic discontent and incidences of political violence. In response, an incumbent regime is likely to employ additional repressive tactics to quell this domestic unrest (Besley and Persson, 2011).

1.2 Existing Findings and Approaches

Given these divergent predictions, scholars have turned to the data to reconcile the effect of aid on political rights (e.g., Bermeo, 2011; Choi and James, 2013; Finkel *et al.*, 2007; Knack, 2004; Liang-Fenton, 2004; Poe, 1992; Regan, 1995;

[3]Examining data from 1981 to 2011, Conrad and DeMeritt (2012, p. 107) conclude that "although decreased reliance for revenue may lead to more widespread and severe abuse, this relationship is constrained by the extent to which the state is democratic."

Scott and Steele, 2011; Wright, 2009). To date, the evidence remains mixed. For example, in perhaps the most cited study on this topic, Knack (2004) finds no evidence that aid promotes democracy. With respect to a smaller subset of U.S. economic aid, Finkel *et al.* (2007) find democracy aid at "best" has modest effects on improving human rights. Their finding corroborates Liang-Fenton's 2004 detailed qualitative analysis from 14 country cases that U.S. economic aid has yielded mixed effects on political liberalization, perhaps due to changing geopolitical priorities. For instance, in the post-Cold War period donor increasingly channel aid towards political liberalization in recipient countries (e.g., Bermeo, 2011; Dunning, 2004; Finkel *et al.*, 2007). These studies, however, do not establish a causal relationship.

It is worth noting, however, that geostrategic considerations may undermine the purported democracy enhancing intent of U.S. aid. For instance, aid can advance geopolitical objectives by potentially "buying support" in the UN General Assembly (Lai and Morey, 2006), as well serving as "signal" to attract foreign direct investment into developing countries (Garriga and Phillips, 2014). Relatedly, Boschini and Olofsgard (2007) find that the end of Cold War contributed to a significant reduction in U.S. aid disbursements, while in post-conflict environments, aid is effective in *less* geostrategic settings (Girod, 2011).

1.2.1 Endogeneity bias

The mixed and at best weakly positive, *associations* between U.S. aid and political rights in the existing literature are plagued with endogeneity bias. On the one hand, U.S. aid may reward countries committed to political liberalization, such as aid disbursed to many Eastern European countries after the end of the Cold War. Yet on the other hand, U.S. aid may help stabilize its autocratic allies (e.g., Egypt) and undermine political liberalization. Thus, the effect of U.S. aid on political rights in recipient countries will be muddled. Of course, identifying empirical strategies to overcome this "endogeneity problem" is arduous.

Several prominent studies attempt to solve the endogeneity problem with the use of instrumental variables for foreign aid. However, the choices of instruments are *rarely truly exogenous* from political rights. For example, Knack's (2004) use of a recipient country's initial population, initial infant mortality, and colonial heritage as an instrument for aid is not truly exogenous to political rights. For instance, smaller populations may be easier to repress, while countries with certain colonial legacies (and resulting institutions) may be more prone to repressive politics (Smith, 2008). Moreover, these instruments are time-invariant and consequently incapable of explaining *temporal* variation in a country's aid receipts.

While narrower empirical studies that examine the sole effect of U.S. democracy aid (which constitute at most 10% of U.S. economic aid) on

democratization concede that endogeneity is a serious concern, they provide insufficient empirical strategies to estimate a causal effect.[4] To their credit, Finkel *et al.* (2007) strive to present some evidence using an instrumental variable for U.S. democracy aid. They use "foreign policy priority" which counts the number of the New York Times reports that the Secretary of State or Assistant Secretary of State mentioned a particular country in any given year. This instrument, however, is not exogenous from state repression. For instance, countries that are typically identified as being repressive (e.g., by Freedom House, Amnesty International) such as Cuba, Iran, and North Korea also tend to be "enemies" of the United States that are discussed frequently by State Department officials (which are then picked up the New York Times). Consequently, "foreign policy priority" is not truly exogenous to repression abroad.

The choice of instruments used in other important studies (primarily as a robustness check) is not truly exogenous either.[5] For instance, Wright's (2009, p. 567), instruments for foreign aid — a recipient country's life expectancy, log population, and a dummy variable for Guinea Bissau — are plausibly directly correlated with state repression. For example, since countries with more repressive politics tend to also spend less on public health, life expectancy is directly correlated with state repression (Bueno de Mesquita *et al.*, 2003). So is population since countries with smaller populations can be easier to repress (Smith, 2008).

On balance, much of the extant literature rarely effectively mitigates concerns with endogeneity bias. In part, this is due to the difficulty in identifying statistically powerful instrumental variables for U.S. foreign aid that are plausibly exogenous from economic and political conditions in recipient countries. This article tackles this endogeneity problem head on.

2 Empirical Strategy

2.1 Importance of U.S. Aid

In aggregate dollars the United States is the world's largest foreign bilateral aid donor. Since 1960, the United States has allocated over $700 billion in

[4]For example, Scott and Steele (2011, p. 52) "argue that the impact of democracy aid can not be considered apart from the effect of democratization on the aid allocation decisions themselves." However, their choice of variables to satisfy the exclusion condition in their simultaneous equation approach is not exogenous (see p. 56). For example, while alliance portfolio, military aid, and human rights may help predict U.S. democracy aid, they are not plausibly exogenous to democratization.

[5]A notable exception is Aronow *et al.* (2012). These scholars exploit a natural experiment to gauge the limited effect of European Union aid on governance and *only* identify an effect for countries that share a colonial relationship with the rotating President of the EU council (in year t).

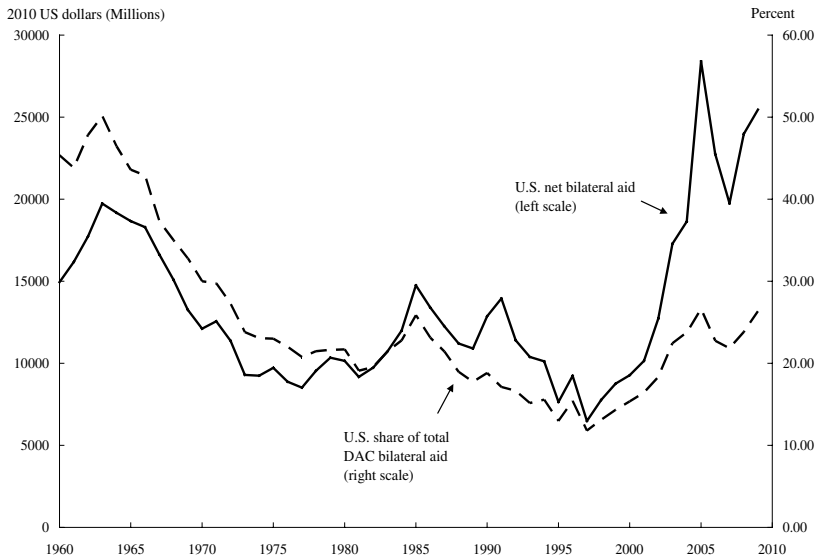

Figure 1: U.S. bilateral economic aid, 1960–2009.

bilateral economic assistance. This figure excludes U.S. military aid, U.S. aid disbursements to multilateral organization (e.g., World Bank, regional development banks) and food aid. 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 détente 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% in 1963 to a low of 12% in 1997. Since 1960, aggregate U.S. aid has amounted to 28.5% of total DAC bilateral assistance, which exceeds the share of all other bilateral aid donors. Moreover, compared to the other four largest bilateral donors (France, Germany, Japan, and the United Kingdom), U.S. aid also tends to be 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. aid on political rights.

2.2 Legislative Determinants of U.S. Aid Allocation

2.2.1 Legislative fragmentation and U.S. aid disbursements

The United States allocates varying amounts of bilateral economic aid to recipient countries over time. A large component of this allocation 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. 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 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. 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 Congress (e.g., Milner and Tingley, 2010; Therein and Noel, 2000). Milner and Tingley (2010), for example, analyze votes related to U.S. foreign aid from members of the House of Representatives from 1979 to 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.[6] Partisan affiliation often shapes the types of aid Congressmen support. For instance, analyzing U.S. bilateral aid for 119 countries from 1960 to 1997, Fleck and Kilby (2006) show that a more liberal Congress (i.e., higher share of Democratic legislators to Republican legislators) gives greater weight to aid for economic development. In contrast, a more conservative Congress gives more weight to aid for commercial purposes (e.g., aid that is tied to U.S. exports).

The existence of these partisan differences over aid allocation suggests that the legislative composition of Congress influences aid disbursements. In particular, existing theories and empirical evidence suggests that a more fragmented legislature contributes to higher government spending, including foreign aid appropriations (Alesina and Perotti, 1996; Alesina and Tabellini, 1990; Roubini and Sachs, 1989). The theoretical explanations stem from the well established proposition that higher levels of aggregate political conflict

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

(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 confirm this legislative fragmentation-spending relationship, both cross-nationally (Alesina and Tabellini, 1990; Roubini and Sachs, 1989) and, in particular for presidential systems, such as the United States (Alesina and Rosenthal, 1995; Hankla, 2013).

With respect to U.S. bilateral foreign aid disbursements, such a relationship is apparent in the legislative fragmentation of the US House of Representatives. Figure 2 depicts a robust positive correlation between average U.S. bilateral aid disbursements and a measure of legislative fragmentation based on the difference in number of Democrats and Republicans in the U.S. House of Representatives. Specifically, fragmentation (FRAG_t) in year t is defined as $(1 - \frac{|\text{DEMOCRAT}_t - \text{REPUBLICAN}_t|}{435}) \times 100$, where a higher value corresponds to greater fragmentation. Using the *absolute* difference in the number of

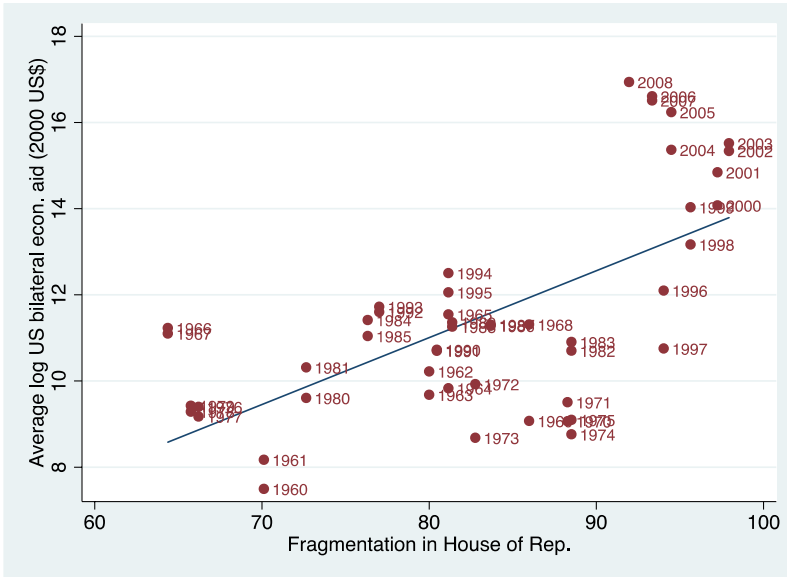

Figure 2: Fragmentation in the U.S. House of Representatives and average U.S. bilateral aid disbursements.

House Democrats and Republicans avoids explicitly incorporating measures of partisanship or ideology (e.g., DW-NOMINATE), which are potentially endogenous with actual preferences for foreign aid (e.g., Fleck and Kilby, 2006; Milner and Tingley, 2010).

2.2.2 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 senators. Empirically, this means the House FRAG_t exhibits greater temporal variation than the Senate FRAG_t and generates a statistically stronger and more precise instrumental variable for U.S. aid.[7] Second, 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* 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 (Tufte, 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 Representatives.

2.2.3 Aid frequency

The sensitivity of any particular country's receipts of aid to FRAG_t will be affected by that country's probability of actually receiving U.S. aid in any given year. As Figure 3 demonstrates, the U.S. government tends to dole out higher amounts of aid to more frequent recipients. This figure 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, P_i.[8] For instance, Nigeria has a 68 probability of receiving U.S. aid in any given year, with aid disbursements averaging $31.3 million per annum. In contrast, Algeria receives a substantially lower amount of aid ($41,803 on average per annum) about once every three years. The cross-sectional relationship identified in Figure 3 is analogous to Nunn and Qian's (2014) observation that U.S. bilateral food aid is higher for countries that receive food aid more frequently from the United States.

[7] Aid instrumented using Senate fragmentation also hurts political rights. These results are reported in Appendix Table B3.

[8] P_i is based on the proportion of years between 1972 and 2008 a country receives any U.S. aid.

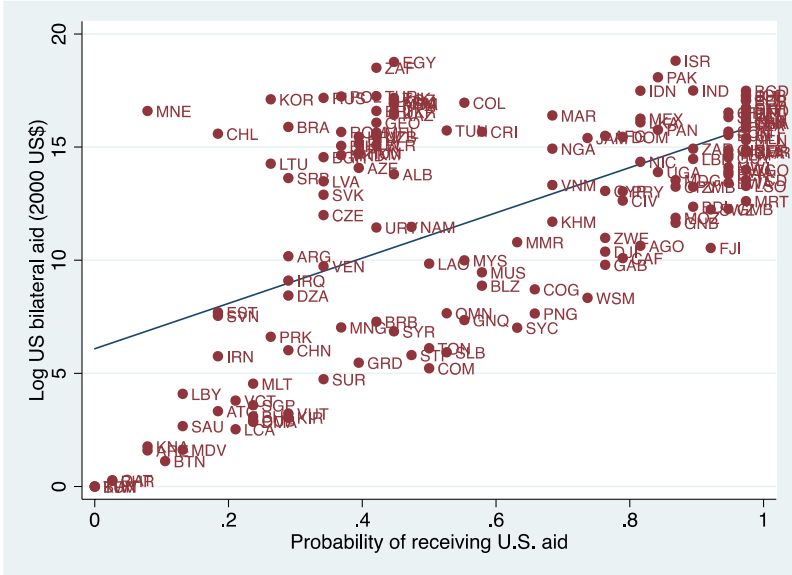

Figure 3: Annual probability of receiving U.S. aid (P_i) and average U.S. bilateral economic aid (2000 US$).

2.2.4 Variation in aid levels

Interestingly, countries that tend to receive U.S. aid *more* frequently are *less* likely to experience *changes* in their annual aid receipts. Or stated alternatively, more frequent aid recipient tend to have more stable (less variation) aid receipts from year to year. Figure 4 demonstrates this by plotting each country's standard deviation in its annual *level* of aid receipts against how frequently these countries receive U.S. aid (P_i). This negative association implies that *less (more) frequent aid* recipients exhibit *greater (less) variation* in annual aid disbursements. Together the relationships in Figures 3 and 4 imply that *more (less)* frequent aid recipients are *less (more)* sensitive to "shocks" in the *total* aid budget stemming from changes in the composition of Congress. Empirically, this means the interaction of FRAG_t and P_i will be negatively associated with U.S. aid receipts; an effect that the first stage regression demonstrates.

2.3 Identification

2.3.1 Instrumental variable

I exploit these sources of variation in U.S. aid disbursements to construct a powerful cross-national and time-varying instrumental variable (IV). The

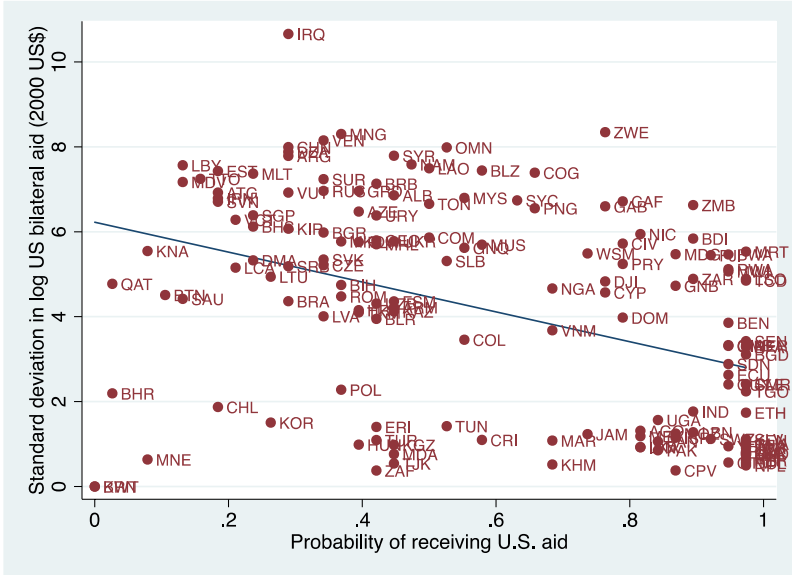

Figure 4: Annual probability of receiving U.S. aid (P_i) and annual variation (standard deviation) in U.S. bilateral economic aid (by country).

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 (P_i). This instrument ($\text{FRAG}_t \times P_i$), which is constructed by interacting a plausibly exogenous term (FRAG_t) with one that is potentially endogenous (P_i) can be interpreted as exogenous since the first stage and second stage regressions control for main effect of the endogenous variable (see the following equations). Specifically, the identifying assumption is that the "endogenous" variable and the outcome of interest are jointly independent of the "exogenous" variable (Angrist and Krueger, 1999).[9]

Constructing an instrumental variable for aid in this fashion underlies the identification strategy of several prominent articles in the foreign aid literature. For example, in their study of the impact of U.S. food aid on civil conflict, Nunn and Qian (2014) interact plausibly exogenous variation in annual U.S. weather conditions with the probability a country receives U.S. food in any particular year as an instrumental variable for U.S. food aid allocations. The latter term in their instrumental variable (probability of receiving U.S. food aid) is country-specific, time-invariant and is *identical* to the formulation of P_i employed in this article.

[9]See Section 2.3.4 of Angrist and Krueger (1999) for a more technical discussion.

A similar construction for an instrumental variable underlies Ahmed and Werker's (2015) analysis of the effect of aid on civil war. These scholars interact plausibly exogenous variation in world oil prices with a dummy variable for whether a country is Muslim as an instrumental variable for aid received in Muslim-majority countries. In their formulation, the "Muslim dummy" in the instrument is potentially endogenous with poor economic performance and governance (Chaney, 2012), but this potentially endogenous component is controlled for in their specifications with country fixed effects (since the Muslim dummy is country-specific and time invariant). Indeed, similar to the baseline specifications employed in this article, Nunn and Qian (2014) and Ahmed and Werker (2015) "control" for the potentially endogenous component of their instrumental variables with country fixed effects (since the endogenous component is country-specific and time-invariant).

Among the exhaustive robustness checks, the results in this article also hold in specifications that do not include fixed effects, but do include the relevant constituent terms of the instrumental variable (e.g., Table 2, column 2). The core results also hold in specifications with time-varying measures of P_i and alternate derivations of P_i based on the aid allocation decisions of the British and Canadian governments. These results are discussed in Sections 3 and 4.

2.3.2 Specification

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

$$\text{First stage:} \quad \text{AID}_{it} = \alpha + \beta(\text{FRAG}_t \times P_i) + \gamma X_{it} + Y_t + C_i + \varepsilon_{it}$$

$$\text{Second stage:} \quad \text{RIGHTS}_{it} = a + b^*\text{AID}_{it} + c^* X_{it} + Y_t + C_i + u_{it}$$

where i refers to the country, t indexes the year, X_{it} is a vector of controls, and C_i and Y_t are dummies for countries and years respectively. The inclusion of country fixed effects implies the estimated coefficients will gauge each aid recipient's within-country variation in RIGHTS_{it}. To account for serial correlation, the standard errors are conservatively clustered by country.

In the first stage regression, AID_{it} is each country's annual receipts of U.S. bilateral economic aid. As Scott and Steele (2011) argue, since aid appropriations and obligations can go unspent, I use disbursements to evaluate the impact of "realized aid."[10] Following several prominent studies (e.g., Alesina and Dollar, 2000; Aronow *et al.*, 2012; Boschini and Olofsgard, 2007; Kuziemko and Werker, 2006), aid is measured in logarithmic units (i.e., $log(1+\text{AID}_{it})$).[11] In the second stage regression, RIGHTS_{it}, is an increasing measure of political

[10] Other related studies also use disbursements, such as Steele and Scott (2011), Ahmed and Werker (2015), Nunn and Qian (2014), and Wright (2009).

[11] There are both theoretical and empirical reasons for measuring total aid (in log units). Several formal models link a country's *level of total receipts* to repression (e.g., Besley

rights (where higher values imply *greater* political repression). *This means that if U.S. harms political rights, the coefficient on* AID_{it} *(b) should be positive and statistically significant.*

Turning to the instrument, FRAG_t is equal to $(1-\frac{|\text{DEMOCRAT}_t-\text{REPUBLICAN}_t|}{435})\times 100$; where a higher value (i.e., closer to 100) implies greater legislative fragmentation in the House of Representatives. The tendency for a country (i) to receive any aid is given by $P_i = \frac{1}{38}\sum_{t=1972}^{2009} P_{i,t}$ where $P_{i,t}$ is equal to 1 if that country receives any aid in year t and 0 otherwise. Observe, the vector of country fixed effects absorbs this probability since P_i is specific to each country (i) and time-invariant. The inclusion of year fixed effects subsumes the main effect corresponding to legislative fragmentation since FRAG_t changes from year to year, but remains the same across all aid recipients. Year fixed effects also account for any constant time trend in the independent and dependent variables.[12]

It is worth emphasizing that the article's results do not hinge on the inclusion of fixed effects or on the construction of the instrument in this particular way. The findings are robust across a variety of different regression specifications and formulations of the instrument. For example, in first stage regression specifications that include the constituent terms of the instrumental variable (i.e., FRAG_t, P_i) as control variables (Table 1, column 1), the coefficients on FRAG_t and P_i are both positive and significant (inline with the positive trends in Figures 2 and 3), while their interaction ($\text{FRAG}_t \times P_i$) exhibits a negative relationship with aid (inline with the negative trend in Figure 4).

In both stages, I control for a parsimonious set of covariates (X_{it}) that affects both the allocation of aid in the first stage regression and RIGHTS_{it} in the second stage regression: log GDP per capita ("need") and economic growth ("merit").[13] I also control for a country's population size since smaller countries tend to receive disproportionately higher amounts of aid and the "cost" of political repression often varies by country size (Alesina and Dollar,

and Persson, 2011, Svensson, 2000), rather than per capita aid or aid as a share of GDP. Empirically, many scholars note that log total aid reduces skewness in the underlying measure of aid flows and accounts for potentially diminishing returns to aid (on the relevant outcome variable). Finally, numerous prominent studies in the empirical literature use log total aid as their independent variable (e.g., Aronow *et al.*, 2012; Kuziemko and Werker, 2006), as we as their dependent variable of interest (e.g., Boschini and Olofsgard, 2007).

[12]For instance, Figures 1 and 2 suggest that the overall U.S. aid budget (Figure 1) and average disbursements to countries (Figure 2) have tended to increase over time. The inclusion of year fixed accounts for this upward trend. The main results in this article also hold in specifications with a time-trend.

[13]According to Hoeffler and Outram (2011), for 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").

Table 1: Determinants of U.S. bilateral aid.

	Type of U.S. bilateral aid (in log units, 2000 US$)			
Dependent variable	Economic		Econ. and military	Military
	(1)	(2)	(3)	(4)
FRAG × P	−0.343	−0.342	−0.314	−0.153
	(0.058)	(0.068)	(0.062)	(0.062)
Recipient characteristics				
Fragmentation (FRAG)	0.37			
	(0.052)			
Prob. of rec. aid (P)	40.73			
	(5.384)			
Log GDP per capita (2000 US$)	−0.099	−1.412	−0.946	1.177
	(0.285)	(0.801)	(0.709)	(1.541)
GDP per capita growth (% annual)	0.032	0.023	0.023	0.048
	(0.019)	(0.012)	(0.013)	(0.020)
Log population	0.571	1.638	0.39	−2.684
	(0.171)	(1.907)	(1.913)	(3.013)
UNSC member	−0.275	−0.065	0.054	−0.201
	(0.406)	(0.329)	(0.309)	(0.476)
US ally	0.111	−0.256	1.279	0.872
	(0.538)	(0.534)	(0.419)	(0.651)
Log U.S. exports (2000 US$)	0.153	0.1	0.165	0.118
	(0.065)	(0.064)	(0.065)	(0.086)
Constant	−36.978	20.62	34.132	53.779
	(5.229)	(30.008)	(30.118)	(54.008)
Country fixed effects	N	Y	Y	Y
Year fixed effects	N	Y	Y	Y
R-squared	0.44	0.66	0.65	0.57
Number of observations	3853	3853	3853	3853
Number of countries	151	151	151	151
F-statistic on instrument	35.14	25.32	25.39	6.13

Note: Estimation via OLS. robust standard errors, clustered by country reported in parentheses.

2000). These control variables also serve to account for the main channels through which "modernization theories" can foster political liberalization (e.g., Finkel *et al.*, 2007).

Measuring donor self-interest, in contrast, is not as obvious as their motives are numerous, often donor specific, and largely unobservable. For instance, geopolitical concerns (e.g., Cold War politics), especially for the United States, can influence donor self-interest (Alesina and Dollar, 2000). Since such concerns are frequently temporal, faced by all countries, and their effects are largely unobservable, the inclusion of fixed effects (Y_t) accounts for these effects. The inclusion of year fixed effects also accounts for global trends that may affect political liberalization, such as the "third wave" of democratization, the end of the Cold War, the rise of global terrorism, global economic and commodity price shocks, and apparent secular trends in the underlying data (e.g., total U.S. aid in Figure 1).

Donors also tend to strategically disburse greater amounts of aid (and at a higher frequency) to recipients, for example, that are geographically closer or share a colonial relationship with the donor (Alesina and Dollar, 2000). The inclusion of recipient country fixed effects (C_i) will account for these effects. Country fixed effects also account for other observed and unobserved time-invariant country-specific factors that may affect a country's political rights such as its ethnic and religious fractionalization, colonial history, legal system, and geography.

Geopolitical concerns, of course, are also often time-varying and country specific. For instance, Kuziemko and Werker (2006) show that countries that rotate onto the United Nations Security Council (UNSC) receive higher amounts of U.S. aid, some of which can lead to pernicious political and economic outcomes (Bueno de Mesquita and Smith, 2010). Relatedly, countries with formal military alliances with the United States tend to receive U.S. economic assistance (e.g., trade, foreign aid) at both higher amounts and at a higher frequency. Finally, U.S. aid disbursements are often aimed at promoting U.S. exports (Fleck and Kilby, 2006) to aid recipients and greater import penetration has been linked to better governance in countries (Ades and Di Tella, 1999). To account for these effects, I control for a recipient's annual consumption of U.S. exports, its membership on the UNSC, and its alliance status with the United States.

2.4 Data

2.4.1 Dependent variable

I use Freedom House's *POLITICAL RIGHTS* index as the main dependent variable. This index has been used in similar studies and has the largest country (∼150) and temporal coverage (1973 onwards) compared to related

measures, such as those from POLITY and CIRI Human Rights Project (e.g., Finkel *et al.*, 2007; Kersting and Kilby, 2014).[14]

Based on the opinions of experts, *POLITICAL RIGHTS* gauges 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.

Skeptics may worry that the coding of *POLITICAL RIGHTS* is *potentially* biased, especially during the Cold War years. Qian and Yanagizawa (2009), for instance, show that U.S. allies were more likely to receive favorable human rights ratings (from Amnesty International and the US State Department) during the Cold War than non-allies. This supposed bias, however, works *against* this article's empirical findings. Specifically, if the United States gives more aid to its allies (especially during the Cold War) and these allies are likely to get favorable assessments from Freedom House then one would expect that U.S. aid (especially during the Cold War) to *not* make political rights worse. However, this article shows otherwise (in Section 3).

Nevertheless, I directly address this worry in a number of ways. First, unlike alternate measures based on State Department reports (e.g., CIRI, Political Terror Scale), Freedom House's measure is derived from its own *independent* assessments of political conditions in countries. Second, all the specifications control for whether an aid recipient is a U.S. ally and also include year fixed effects (which eliminates any systematic Cold War effect). Third, the core finding holds in samples restricted to the post-Cold War period and in specifications with various Cold War differential trends. Finally, panel B in Table 3 shows, U.S. aid fosters repression using alternate dependent variables from different sources (e.g., POLITY IV).

2.4.2 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. There is wide cross-national and temporal variation in U.S. economic aid disbursements. Some countries (e.g., 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) and several surpassing $500 million annually (e.g., Egypt, Israel,

[14] As robustness, I also show that U.S. aid hurts political rights using measures from CIRI. See Table 3, Panel B.

Iraq after 2003). In the baseline sample, the typical country receives U.S. economic aid equal to about $50 million per annum.

The economic and demographic controls are drawn from the World Development Indicators (World Bank, 2010). UN Security Council membership is available from the United Nations, alliances from the Correlates of War (Gibler and Sarkes, 2004), and U.S. exports from the International Monetary Fund (2012).

3 Aiding Repression

3.1 Determinants of U.S. Bilateral Aid

Table 1 shows that the instrumental variable ($\text{FRAG}_t \times P_i$) is a robust determinant of annual bilateral disbursements of U.S. aid to about 150 recipient countries. Column 1 presents a specification without any fixed effects. Consistent with Figure 2, greater legislative fragmentation raises U.S. aid disbursements (coefficient = 0.37). And consistent with Figure 3, more frequent aid recipients receive higher amounts of aid (coefficient = 40.73). The coefficient on the instrumental variable is −0.34 and is highly statistically significant (p-value = 0.00). This negative coefficient is consistent with Figure 4 in which *more* frequent aid recipients tend to experience *less variation* change in annual U.S. aid receipts. Indeed, Figure 5 graphs this interaction effect and shows *greater variation* in annual aid budgets for countries that receive U.S. aid *less frequently*. The corresponding F-statistic (=35.14) means the instrument is "strong" (since it exceeds the threshold of 9.6 suggested by Stock *et al.* (2002)) and implies the second stage estimates can be interpreted as causal.

Column 2 shows that instrument is a strong predictor of bilateral economic aid in a specification with country and year fixed effects.[15] Column 3 shows that the instrumental variable is also a strong predictor of bilateral disbursements of U.S. economic and military aid. Yet the instrument is a poor predictor of military aid by itself (Column 4). In this regression, the coefficient estimate is much smaller in magnitude and the corresponding F-statistic (=6.13) is smaller than that associated with economic aid only. This drop in coefficient size and statistical precision is expected since Congress has less influence over the allocation of U.S. military aid and therefore, movements in FRAG_t *should not* affect these disbursements.

Given the strong predictive power of the instrument for economic aid from these first stage regressions, I therefore gauge the causal impact of U.S.

[15] This specification excludes FRAG_t and P_i since they are subsumed by the relevant set of fixed effects. Specifically, since FRAG_t is time-varying but the same across all countries it is subsumed by the year fixed effects, while country fixed effects account for P_i which is time-invariant but vary across countries.

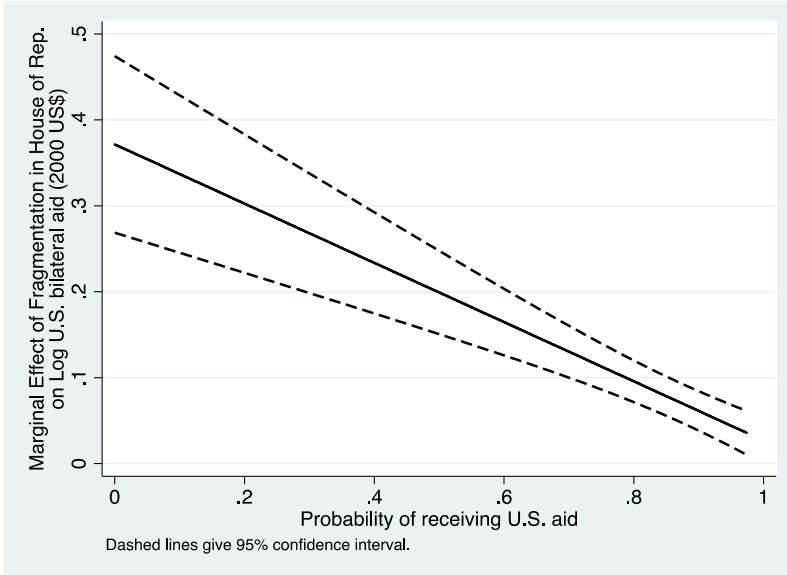

Figure 5: Marginal effect of legislative fragmentation on U.S. aid disbursements (from first stage regression).

bilateral economic aid on repression in the second stage (although, as Table 2 shows instrumented U.S. economic and military aid and instrumented military aid also harm political rights). Finally, in the first stage regressions, the control variables have their expected effects. In general, richer countries tend to receive lower amounts of aid, while those experiencing economic growth are "rewarded" with more aid. The time-varying geostrategic measures tend to have very little effect on U.S. aid.

3.2 The Impact of U.S. Aid on Political Rights

Table 2 evaluates the effect of U.S. aid on political rights. Column 1 shows that in a "naïve" OLS specification, U.S. economic aid has no impact on political rights. This effect is unsurprising and wholly consistent with the weak (or null) effects found in existing studies of aid on political rights. In contrast, column 2 shows that instrumented U.S. aid *causes* a deterioration of political rights: a unit increase in log U.S. economic aid raises POLITICAL RIGHTS by 0.15 index point. This effect is highly statistically significant (p-value < 0.01) and substantively meaningful: moving from the 10 percentile of aid receipts to merely the 50 percentile corresponds to around a 2.5 index point rise in POLITICAL RIGHTS. Such a jump corresponds to 1.25 standard deviation

Table 2: U.S. aid harms political rights.

Dependent variable	Political rights									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Method of estimation	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Economic aid	−0.01	0.149	0.157			0.188	0.172			
	(0.010)	(0.069)	(0.059)			(0.064)	(0.065)			
Econ. and military aid				0.171						
				(0.064)						
Military aid					0.351					
					(0.191)					
Econ. aid in $t-1$								0.135		
								(0.056)		
Econ. aid in $t-2$									0.124	
									(0.057)	
Econ. aid in $t-5$										0.137
										(0.076)
Controls										
Fragmentation		−0.048								
		(0.010)								
Prob. of rec. aid		−2.13								
		(1.012)								

(*Continued*)

Table 2: (*Continued*)

Dependent variable	Political rights									
Military aid						−0.069				
						(0.015)				
Recip. characteristics	Y	Y	Y	Y	Y	Y	N	Y	Y	Y
Country fixed effects	Y	N	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	N	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.73	0.003	0.64	0.64	0.07	0.62	0.61	0.67	0.68	0.68
Number of obs.	3853	3853	3853	3853	3853	3853	4554	3905	3836	3590
Number of countries	151	151	151	151	151	151	160	151	151	150
F-stat on instrument		35.14	25.32	25.39	6.13	19.88	25.08	29.12	29.1	18.92

Note: Robust standard errors, clustered by country reported in parentheses. Economic aid refers to log U.S. bilateral economic aid (2000 US$). Military aid refers to log U.S. bilateral military aid (2000 US$). Recipient characteristics include: log GDP per capita, GDP per capita growth, log population, UNSC member, U.S. ally, and U.S. exports. These coefficients, country fixed and year fixed effects, and a constant are not reported. In Column 6, military aid is included as an additional control variable. The specification (7) does not control for any recipient characteristics.

increase in the POLITCAL RIGHTS index and is equivalent to moving from a less repressive country like Peru to a more repressive country like Sudan or Vietnam.[16] This 2SLS estimate as well as those with fixed effects (reported in columns 3–10) 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 quality of democratic governance. The control variables (not reported, but available upon request) are consistent with existing findings. For example, richer countries (i.e., higher GDP per capita) are less repressive.

Instrumented U.S. economic and military aid also harms political rights (column 4), as does military aid separately (Column 5). Column 6 shows that instrumented U.S. economic aid harms political rights when controlling for military aid separately, while columns 8–10 show that aid lagged by 1, 2, and 5 years is also associated with a reduction in the quality of political rights. As expected, the effect is smaller than contemporaneous aid. The results also hold with data averaged over 2- and 5-year periods (results not reported).

Finally, to allay concerns that controlling for time-varying recipient characteristics (e.g., level of economic development), may introduce potential "post-treatment bias", column 7 shows that instrumented U.S. economic aid continues to damage political rights in a specification without these controls.

3.3 Robustness

The core finding in Table 2 is robust to a large number of potential concerns. For example, the results do not hinge on the inclusion of fixed effects in the baseline specification. The core results hold in specifications that vary the set of fixed effects as well as in specification with a time-trend (Table 3, Column 1a), which directly "controls" for any common time trend between the POLITICAL RIGHTS and AID.[17] The results are also robust to alternate ways to modeling temporal effects, such as specifications that control for YEAR, YEAR squared, and LOG YEAR (results not reported). Moreover, the core findings are robust to potential outliers. For instance, skeptics may worry that "temporal outliers" such as those associated with Cold War politics or the surge in U.S. aid after September 11, 2001 (see Figure 1) may unduly bias the findings. To directly address these concerns, in Table 3, I estimate models in samples restricted to the post-Cold War period (Column 2a) and pre-2001 period (Column 3a). The estimated effect in Column 2a, for instance, directly purges concerns that Cold War politics may be unduly driving the results. Moreover, the results

[16] Based on average POLITICAL RIGHTS for each of these countries over the sample period.

[17] In this specification (Table 3, Column 1a), while "year" exhibits a statistically significant negative effect on POLITICAL RIGHTS, there is still sufficient variation that is explained by aid. The coefficient on U.S. aid is 0.16 and highly statistically significant.

Table 3: U.S. aid harms political rights — additional results.

	Panel A: Alternate samples and specifications					
Dep. variable	Political rights					
	Time trend	Sample period		Excluded observations		
		Post-Cold War	Pre-2001	Aid - top decile	$P_i > 0.90$	$P_i < 0.10$
	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)
Economic aid	0.157	0.081	0.214	0.148	0.188	0.136
	(0.059)	(0.041)	(0.126)	(0.060)	(0.071)	(0.057)
Year trend	−0.066					
	(0.023)					
No. obs.	3853	2325	2879	3463	2551	3780
R-squared	0.64	0.83	0.54	0.65	0.64	0.66
F-statistic	25.32	24.21	7.03	24.06	14.42	25.4

Panel B: Alternate dependent variables					
	Alternate measures of political repression				
Dep. variable	Civil Liberties	Political Participation	Religious Freedom	Empowerment Index	Political imprisonment
	(1b)	(2b)	(3b)	(4b)	(5b)
Economic aid	0.084	0.111	0.056	0.246	0.033
	(0.041)	(0.063)	(0.020)	(0.109)	(0.020)
No. obs.	3853	3225	3177	2828	2836
R-squared	0.71	0.66	0.52	0.7	0.56
F-statistic	25.32	11.55	30.69	17.05	16.48

Note: Estimation via 2SLS. Robust standard errors, clustered by country reported in parentheses. Economic aid is instrumented using $P_i \times \text{FRAG}_t$. Economic aid is log U.S. economic aid (2000 US$). Each specification (i.e., 1a, 1b, 2a, etc.) controls for log GDP per capita, GDP per capita growth, log population, UNSC member, U.S. ally, and U.S. exports. These coefficients, country and year fixed effects, and a constant are not reported. The F-statistic on the instrument is reported for each specification.

hold in specifications that control for Cold War and post-Cold War "epoch" dummies, as well as unobservable region and country specific Cold War trends (i.e., $\text{REGION}_i \times \text{COLD WAR}_t$, $\text{COUNTRY}_i \times \text{COLD WAR}_t$).

The core results in Table 2 are robust to an exhaustive list of other potential concerns, such as: outliers (e.g., exclusion of frequent aid recipients); spatial "diffusion"; the inclusion of additional controls (e.g., political institutions, oil exports, arms imports, overall trade "openness"; conflict; percentage of born population); alternate instruments (e.g., using fragmentation in the Senate); alternate specifications (e.g., controlling for lags and leads of aid, time-varying measures of P_i); alternate clustering of the errors (e.g., by region, year, two-way clustering, etc.); the "crowding-out" of aid from other donors; regional trends (e.g., differential effects for Africa); and region and country-specific trends that vary across the Cold War and post-Cold War period.

3.4 Additional Dependent Variables

Panel B in Table 3 shows that U.S. aid harms other forms of human rights (and from different data sources): civil liberties (Freedom House, 2011), political participation (Marshall and Jaggers, 2010); and freedoms associated with religious, empowerment rights, and imprisonment (Cingranelli and Richards, 2008). In each of these specifications, a higher value of the dependent variable implies a diminution of human rights.

Columns 1b and 2b show that U.S. aid damages a country's quality of civil liberties and competiveness of political participation. Columns 3b–5b show the impact of U.S. aid on the number (counts) of various human right violations from the CIRI data set. Across all three categories, U.S. aid causes an increase in the number of human rights violations.

Finally, the ability and willingness for governments to harm political rights is feasible (and rational) if the aid inflows help make the country's underlying political regime and institutions more stable. This seems to be the case, as U.S. aid also tends to help expand the powers of autocrats, lower the overall level of democracy (e.g., gauged using the POLITY index), and increase the "durability" of regimes in autocracies more so than in democracies.

4 Potential Concerns with Identification

4.1 Evaluating the Exclusion Restriction

The validity of the article's identification strategy relies on whether the exclusion restriction is satisfied: Congressional fragmentation interacted with the average probability a country receives U.S. aid affects political rights abroad through U.S. economic aid only. This assumes that neither FRAG_t nor P_i affects political rights through other channels. Potential alternate

channels may include time-varying factors, such as the partisanship of the sitting United States President (and his relationship with Congress, e.g., unified versus divided governments); global commodity prices; the relationship of an aid recipient to the United States (e.g., UNSC member, party affiliation of the US President). For instance, Democratic Presidents may prefer giving aid to certain countries or regions (for example, for development in Africa rather than security in the Middle East). To account for this unobservable effect, I interact the party identification of each sitting President (e.g., Republican Presidents are coded 1, while Democratic Presidents are coded as 0) with P_i as an additional control. The alternate channels may also be time-invariant characteristics of aid recipients, such as each country's distance from the United States, its colonial legacy, and year of independence.

Thus to rigorously account for these potential alternate channels, I include a battery of "interactive" controls. These controls interact the (relevant) constituent term of the instrumental variable with the channels described earlier. The time-varying factors (e.g., oil prices, party identification of sitting President, etc.) are interacted with P_i, while the time-invariant factors (e.g., colonial legacy) are interacted with FRAG_t. In Table 4, the former is measured with $P_i \times Z_t$, while the latter is measured wit $\text{FRAG}_t \times Z_i$. In these models, the core result from Table 2 holds.

Table 4 reports the 2SLS (in Panel A) and corresponding first-stage estimates (in Panel B) from these specifications. Column 1 reports the effect of aid on POLITICAL RIGHTS in a specification that controls for the interaction of FRAG_t with a vector of country characteristics (Z_i: year of independence, oil exporter dummy, ethnic-linguistic fractionalization, former colony, US ally). While the effect of aid on repression is slightly smaller in magnitude than the baseline results in Table 2, the substantive interpretation remains unchanged. The interactive controls themselves are not jointly significant (F-statistic $= 4.08$). In the corresponding first-stage regression, the effect of the instrument on U.S. aid ($= -0.31$) is similar in magnitude the baseline first stage estimate ($= -0.34$ from Table 1, Column 2) and remains "strong."

In contrast, Column 2 reports the effect of aid on political rights while controlling for the "propagation" of various time-varying factors (Z_t) to aid recipients. Specifically, I include the following vector of additional controls: the interaction of P_i with the party identification of the U.S. President, oil price, and whether an aid recipient is member of UN Security Council (in year t). In this specification, the effect of aid on political rights (coefficient $= 0.162$) is slightly larger in magnitude than the baseline estimate (coefficient $= 0.157$). The interactive controls are not jointly significant (F-statistic $= 1.26$) and the instrument remains strong in the corresponding first-stage regression (F-statistic $= 26.6$). Finally, Column 3 shows that the main results hold (both in the first and second stages) in a specification that includes *both* sets of interactive controls.

Table 4: Evaluating the exclusion restriction.

	Panel A: Estimation via 2SLS				
	Political rights				
	(1a)	(2a)	(3a)	(4a)	(5a)
Dependent variable	Interactive controls			Alternate Pi	
Log US aid (2000 US$)	0.144	0.162	0.149	0.184	0.17
	(0.058)	(0.058)	(0.057)	(0.086)	(0.117)
R-squared	0.66	0.63	0.65	0.6	0.62
	Panel B: First stage regression				
Dependent variable	Log U.S. bilateral aid (2000 US$)				
	(1b)	(2b)	(3b)	(4b)	(5b)
$\text{FRAG}_t \times P_i$	−0.313	−0.366	−0.338		
	(0.068)	(0.071)	(0.071)		
$\text{FRAG}_t \times P_{\text{can},i}$				−0.219	
				(0.065)	
$\text{FRAG}_t \times P_{\text{UK},i}$					−0.189
					(0.067)
R-squared	0.66	0.66	0.66	0.65	0.65
F-statistic	21.19	26.6	22.65	11.4	8.08
	Additional controls (in both panels)				
Recipient characteristics	Y	Y	Y	Y	Y
Country fixed effects	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
$\text{FRAG}_t \times Z_i$	Y		Y		
$P_i \times Z_t$		Y	Y		
Number of observations (all panels)	3853	3853	3853	3853	3853
Number of countries (all panels)	151	151	151	151	151

Note: Robust standard errors, clustered by country reported in parentheses. All specifications control for recipient characteristics (i.e., log GDP per capita, GDP per capita growth, log population, UNSC member, US ally, and U.S. exports), country and year fixed effects. These coefficients and a constant are not reported. Z_i is a vector of country specific, time-invariant characteristics: year of independence, oil exporter dummy, former colony, US ally. Z_t is a vector of time-varying characteristics: party identification of the U.S. President, oil price, member of UN Security Council.

4.2 Alternate Measures of Aid "Propagation"

Skeptics may still worry that the instrument is problematic as the propensity for a country to receive U.S. aid (P_i) may inherently be correlated with U.S. foreign policy interests in propping up repressive regimes.[18] While the baseline specification includes country fixed effects to account for this inherent tendency,

[18]For instance, this propensity to prop up repressive regimes could be a goal of U.S. foreign policy in certain regions and/or under different (and changing) geopolitical periods such as the Cold War.

to allay this concern I construct additional (alternate) instrument variables with a *different* measure of P_i based on the allocation decisions of *different* countries with *different* legislative bodies. Specifically, I instrument for U.S. aid using the interactions of FRAG_t interacted with the probability a country receives aid from Canada ($P_{\text{CAN},i}$) and the United Kingdom ($P_{\text{UK},i}$).

While Canada and the United Kingdom are United States allies, their foreign policy interests do not align perfectly with the United States. For instance, the United Kingdom tends to favor aid disbursements to its former colonies, while the United States does not exhibit this preference. Empirically, the correlation between $P_{\text{CAN},i}$ and P_i is 0.70, while the correlation between $P_{\text{UK},i}$ and P_i is 0.67.

Columns 4 and 5 in Table 4 report the results using these alternate instruments. Column 4 in Panel A shows that when U.S. aid is instrumented using $P_{\text{CAN},i}$ it is a robust determinant of political rights. The estimated 2SLS effect is slightly larger in magnitude than the baseline estimates in Table 3. In the first stage (Panel B, Column 4), the instrument remains "strong" but explains less of the variation in annual U.S. bilateral aid disbursements. This is unsurprising since $P_i \neq P_{\text{CAN},i}$. The estimated effect ($= -0.22$) is about two-thirds that of first-stage estimate from the baseline specifications. In Column 5, U.S. aid that is instrumented using $P_{\text{UK},i}$ also worsens political rights and barely misses statistical significance (p-value $= 0.13$). This slightly less robust effect may arise since the instrument is somewhat weaker.

5 Evaluating Mechanisms

How does U.S. aid harm political rights? As discussed in Section 1, existing studies have identified two broad mechanisms through which foreign aid can lead to political repression: rent-seeking and incentivizing a government to exert less tax effort. 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. Panels A and B in Table 5 evaluate this mechanism.

Using data from Banks (2010), Column 1a shows that aid has no impact on the incidence of anti-government demonstrations, while at a higher threshold of political discontent, aid exhibits a small positive (but insignificant) effect on the number of assassination attempts (Column 2a). These null effects, however, may reflect the differential effects of aid on discontent across different political regimes as well as how the aid is potentially used to thwart rent-seeking

Table 5: Evaluating mechanisms.

	Panel A: Rent-seeking					
	Anti-gov Demons.	Assassinations			Transfers Military (% of gov. exp.)	
			Autocracies	Democracies		
Dependent variable	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)
Method of estimation	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Log U.S. aid	−0.004	0.038	−0.071	0.125	−0.661	0.867
	(0.029)	(0.034)	(0.045)	(0.069)	(1.080)	(1.039)
No. obs	3660	3661	1924	1738	795	800
No. countries	145	145	73	72	110	108
R-squared	0.27	0.21	0.11	0.16	0.89	0.54
	Panel B: Military spending, by regime type					
	Log military expenditures (2000 US$)			Log military personnel		
Dependent variable	(1b)	Autocracies (2b)	Democracies (3b)	(4b)	Autocracies (5b)	Democracies (6b)
Method of estimation	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Log U.S. aid	0.107	0.141	0.05	0.025	0.052	0.006
	(0.099)	(0.179)	(0.074)	(0.027)	(0.067)	(0.026)
No. obs	2043	1164	879	2301	1329	972
No. countries	135	74	61	142	75	67
R-squared	0.87	0.89	0.86	0.91	0.87	0.95
	Panel C: Taxation					
Dependent variable	Income tax (share of government revenue)					
	(1c)	(2c)	(3c)	(4c)		
Method of estimation	OLS	OLS	2SLS	2SLS		
Log U.S. aid		−0.324	−1.864	−1.938		
		(0.200)	(1.045)	(1.023)		
Political rights	−0.200			−0.050		
	(0.238)			(0.556)		
No. obs	1380	925	925	915		
No. countries	136	113	113	111		
R-squared	0.88	0.83	0.70	0.69		

Note: Robust standard errors, clustered by country reported in parentheses. All specifications control for recipient characteristics (i.e., log GDP per capita, GDP per capita growth, log population, UNSC member, US ally, and U.S. exports), country and year fixed effects. These coefficients and a constant are not reported.

(Cotet and Tsui 2013).[19] For example, on the former, U.S. aid is negatively correlated with assassinations in autocracies (Column 3a), but has a positive (and statistically significant) effect in democracies (Column 4a). This suggests that U.S. aid sparks discontent in less repressive states (i.e., more democratic) but is muted in repressive countries.

This finding may reflect how aid shifts the composition of government expenditures. Columns 5a and 6a provide suggestive evidence that U.S. aid is associated with a shift in the composition of government expenditures away from transfers (Column 5a) to the military (Column 6a). Panel B presents additional evidence to *suggest* that aid exhibits a greater effect on military expenditures and its personnel in autocracies (Columns 2b and 5b) than in democracies (Columns 3b and 6b). Thus, the observation that aid does not spark rent-seeking may be due to the expansion of the state's security apparatus in *autocracies* which mutes the emergence of rent-seeking behavior.

Panel C provides more compelling evidence for the second mechanism linking foreign aid to repression via a taxation channel. According to the government insularity model, as a source of nontax (unearned) government income, aid inflows should allow governments to reduce their tax effort and consequently permit it to become less accountable to its population (i.e., more repressive). 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.[20] 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 (Column 1c). Turing to the conjectured mechanism, U.S. aid seems to reduce tax effort. In an OLS model, U.S. aid is negatively associated with tax effort (Column 2c). In Column 3c, 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. Moreover, controlling for repression does not attenuate the negative effect of instrumented U.S. aid on tax effort (Column 4c). Instrumented aid also exhibits a negative effect on the level (in dollars and log units) of taxes collected (not reported). Together the results

[19]Cotet and Tsui (2013) present evidence that oil discoveries and rents (as a form of unearned government income) helps finance military expenditures in autocracies. Since aid constitutes an alternate form of unearned government income, its effects may be similar to those of unearned income (rents) derived from oil production.

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

in columns 1c–4c suggest that U.S. aid hurts political rights by reducing a government's tax effort.

6 Conclusion

As an instrument of American economic statecraft, the United States claims to use its bilateral economic aid to promote its national interest by expanding democracy and free markets, while improving the lives of citizens in developing countries. The causal evidence in this article casts doubt on this assertion. For a sample of 150 countries, U.S. aid harms political rights, civil liberties, and political participation and tends to strengthen authoritarian politics.

These findings refute some recent empirical findings that aid may improve political rights in countries that have exhibited a commitment — both instrumental and normative — to political liberalization. Consequently, to the extent that political liberalization is an important conduit for growth, the article's findings suggest that U.S. aid may also reduce economic development. Future research could investigate this, potentially through the lenses of aid's impact on domestic politics in recipient countries.

Such a research program could be evaluated for U.S. aid, as well as aid from other donors. In pursuit of such research questions, this article's identification strategy could be expanded beyond the U.S. context. 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 development banks) may allow researchers to gauge the causal effect of foreign aid from other donors on a variety of outcomes in developing countries.

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