Foreign Aid Allocation Tactics and Democratic Change in Africa

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Abstract

Over the past two decades, donors increasingly link foreign aid to democracy objectives in Africa. This study investigates whether and how foreign aid influences specific outcomes associated with democratic transition and consolidation. Using an instrumental variables approach for the period from 1989 to 2008, we show that economic aid increases the likelihood of transition to multiparty politics, while democracy aid furthers democratic consolidation by reducing the incidence of multiparty failure and electoral misconduct. However, we find little evidence that either economic or democracy aid influences opposition support in multiparty elections. These findings have implications for understanding how donors allocate aid and the political consequences of foreign assistance in Africa.

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Since the end of the Cold War, a consensus in the international donor community argues that democracy is an integral part of development efforts. Some foreign aid proponents suggest that without aid "the global democratic revolution cannot be sustained" Diamond (1992, 45). While a normative approach to democracy promotion emphasizes the importance of civil and political rights for human freedom (Sen, 1999), the instrumental perspective promotes democratization based on the premise that democratic institutions incentivize governments to increase spending on welfare outcomes (Stasavage, 2005; Huber, Mustillo and Stevens, 2008; Harding and Stasavage, 2014). In the 1990s and 2000s, donors contributed hundreds of billions of dollars of aid worldwide, with the largest proportion flowing to sub-Saharan Africa. Goldsmith (2001) and Dunning (2004) show that foreign aid positively influences democracy in recipient countries, yet neither study explores the causal mechanisms linking aid and democratic change.[1] Our contribution examines possible causal mechanisms by looking at the same region: Africa.

Sub-Saharan African cases are useful for testing arguments linking aid to democracy because in the past two decades most countries in the region adopted multiparty politics. This not only marked a shift in power in many countries but also provides much of the evidence for the global wave of democratic transitions since the end of the Cold War. From 1989 to 2008, roughly half of all democratic transitions in the world occurred in sub-Saharan Africa; and countries with a variety of autocratic regimes – including personalist rule, military dictatorships, and one-party states – experienced transitions.[2] However, the shift to multiparty politics did not lead to a wave of consolidated democracies (Posner and Young, 2007; Cheeseman, 2011). Indeed, in the twenty years to 2008 incumbent leaders left power only eight times after losing elections under established multiparty regimes.[3] Thus for many countries in the region, the introduction of multiparty politics

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[1]Other research suggests that aid has pernicious effects on democratic development (Bueno de Mesquita and Smith, 2009; Djankov and Reynal-Querol, 2008; Brautigam and Knack, 2004).

[2]Data from Cheibub, Gandhi and Vreeland (2010) show that 24 of 47 transitions to democracy took place in sub-Saharan Africa.

[3]These are: Benin 2001, Cape Verde 2001, Ghana 2000, Guinea-Bissau 2000, Kenya 2002, Madagascar 1996, Mali 2002, and Senegal 2000.

did not lead to strong democratic institutions. This variation in post-transition democracies spurred a vibrant debate about the determinants of democratic survival and consolidation. This paper examines how external assistance influences transition to and consolidation of democracy, and in doing so employs multiple measures to capture distinct aspects of consolidation.

We examine two mechanisms through which aid might influence democracy in recipient countries: (1) donors attaching political reform conditions to economic aid; and (2) donors directly investing in democracy promotion through activities aimed at strengthening governance institutions and civil society. These mechanisms focus on different categories of aid – economic aid and democracy assistance – and have implications for distinct political outcomes. For example, donor pressure to hold elections may be more likely to push a transition to multiparty regimes but may not influence electoral support for the opposition or electoral misconduct. Investing in civil society organizations, in contrast, is unlikely to operate through economic aid but has implications for whether democracy assistance improves electoral support for opposition parties.

To date, however, most research on foreign aid and political development tests how aid influences broad indices of democracy from sources such as the Polity index or Freedom House. While useful for many purposes, these measures are unable to distinguish particular aspects of democratic transition and consolidation. Further, only a handful of studies look at how the political effect of economic and democracy aid may differ. In this paper, we unpack the hypothesized mechanisms linking aid to democracy by examining multiple dimensions of democratic political change – including transitions to and from multipartyism as well as electoral misconduct and electoral support for the opposition – and test how different broad categories of aid influence these outcomes.

We show that donors pursue a strategy of incumbent-led democracy promotion in Africa when dealing with dictators *and* democrats. While donors use aid to propel top-down democratic reforms both prior to and after transitions to multiparty regimes, they do so by pursuing different tactics. When dealing with dictators, donors employ economic development assistance to buy political reform that is largely procedural in nature, such as legalizing opposition parties and holding multiparty elections. While these reforms are sufficient for "formal" transitions to multiparty regimes, they may not necessarily alter the balance of power between the incumbent and opposi-

tion groups. Nor do such reforms necessarily require elites to change their political practices. This makes transitions relatively cheap for many dictators.

Donors dealing with democrats in post-transition contexts influence democratic development through targeted investment in democracy-related activities rather than via conditionality attached to economic aid. Further, we show that donors are most successful at influencing democratic consolidation outcomes by investing in governance but find little evidence that democracy aid targeting civil society organizations influences election outcomes or incumbent electoral behavior.

Our findings have implications for democracy promotion and international actors who attempt to influence the process of democratic transition and consolidation. By shedding light on the causal pathways through which foreign aid changes political outcomes in recipient countries, we show both the areas where foreign actors can promote democratic outcomes and the limits of such strategies. We find that while economic aid can buy relatively cheap multiparty transitions, democracy aid can help consolidate multiparty regimes but rarely influences the political balance between incumbents and the opposition in these countries. Our findings illustrate the value of differentiating aid categories and precisely measuring the political outcomes that most closely match the causal story. An emerging literature links foreign aid to many important outcomes such as civil conflict, terrorism, and human rights (Nielsen et al., 2011; Savun and Tirone, 2012). Future research in these areas will benefit from examining different categories of foreign aid to pinpoint the mechanisms at work.

Foreign aid and democracy

Studies linking aid to democracy provide a mixed picture. Some argue that donors leverage their economic power by attaching political reform conditions to aid packages. Using conditionality to 'buy reform' requires that donors can credibly withdraw or redirect aid when recipient government do not comply (Burnell, 1997; Dunning, 2004). This mechanism is often associated with transitions to multiparty politics, particularly in the 1990s. For example, Resnick (2013) highlights the crucial role of donor leverage in Malawi's first multiparty election in 1993; and Handley (2008) argues that donor pressure motivated Ghana's President, Jerry Rawlings, to "consider liberalization of

the political regime and a return to constitutionalism." Citing this leverage mechanism, some find a link between aid and democratic transitions only during the post-Cold War period when donors' threat of aid withdrawal was most credible (Dunning, 2004; Wright, 2009; Bermeo, 2011).

Others posit that democracy assistance influences democracy through a direct investment channel, targeting either incumbent governments (by focusing on budgeting procedures, bureaucratic competence, and judicial and legislative independence) or democratizing agents in civil society (by focusing on particular organizations that typically stand outside the recipient government). Citing the investment mechanism, some studies find that democracy assistance increases the level of democracy, as measured by broad democracy indicators (Finkel, Pérez-Lián and Seligson, 2007; Scott and Steele, 2011). Bush (Forthcoming) explores the nature of civil society assistance and finds that, over time, democracy promotion has become more "tame" as international NGOs increasingly select more regime-compatible projects to guarantee future funding.

We contribute to this debate by presenting an argument that differentiates between mechanisms of democracy promotion (economic and democracy aid) and distinct delivery channels of democracy assistance (donor-to-government and donor-to-civil society). We specify when aid contributes to democratic outcomes and in doing so explain why donors successfully promote horizontal accountability but have fallen short in improving vertical accountability.

Donor tactics and democracy promotion

Transitions to multiparty politics and democratic consolidation represent different stages of democratic development, which trigger different donor tactics. Multiparty transitions require incumbents to focus on a targeted event – elections with opposition parties – while consolidation requires more complex political changes. In the 1990s, donor demands for elections and minimal procedural reforms converged around election day. For example, Brown (2011) provides evidence from interviews with donors working in countries as diverse as Rwanda, Kenya, and Malawi that aid recipients heard a consistent reform message emphasizing multiparty elections; while Crawford (2001) notes that donor coordination was crucial for leveraging multiparty elections across the continent.

After transitions, however, there is less donor consensus on the specific goals of democracy pro-

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motion. Not all donors agree on how to advance democratic consolidation, especially when donors face trade-offs among consolidation, development, and stability in recipient countries (Brown, 2011). While the pursuit of each of these policy goals may be desirable, they are not always compatible. Since the mid-1990s donor governments have emphasized improving service delivery and development, as is evident in the Millennium Development Goals. Donors fear that withholding foreign aid in response to lackluster progress in democratic consolidation may lead to far worse outcomes, such as instability and conflict (Resnick, 2013). What is more, key principles of the international aid architecture, such as country-ownership, create donor ambivalence about criticizing recipients' commitment to democratic consolidation.

In light of these tensions, donors should be less forceful in promoting democratic consolidation efforts than transitions to multipartyism. Instead of relying on threats to withdraw economic assistance, as was often the case prior to multiparty transitions, donors increasingly pursue more targeted tactics by earmarking aid for specific democracy promotion activities. The top panel of Figure 1 shows that while economic aid to sub-Saharan African countries dipped in the 1990s, democracy aid increased throughout the past two decades with largest bump after 1998.

Further, once countries transitioned to multiparty regimes, donors substantially increased democracy aid – both to the government and to civil society groups – with no corresponding economic aid dividend. The bottom, left panel of Figure 1b shows that democracy aid increased by nearly 20 percent in the three years after a multiparty transition relative to the pre-transition period. The bottom, right panel shows that the collapse of multiparty regimes also yields large increases in democracy assistance. Economic aid, on the other hand, remains at very similar levels pre- and post-transition, but drops considerably once a multiparty regime collapses.

When dealing with dictators prior to multiparty transitions, donors prioritize economic development assistance as a tool to extract political reform concessions. The political conditions attached to economic aid are often relatively minimal in scope and procedural in nature, consisting of items such as introducing executive term limits, legalizing opposition parties, and holding elections. While such reforms are often sufficient for "formal" transitions to democracy, they may not necessarily alter the political balance in the country in significant ways or require elites to change

their political practices. For instance, Tanzania's shift to a multipartyism in the 1990s represented a formal break from the past. Yet, the incumbent Chama Chama Mapinduzi (CCM) managed the transition and post-transition environments to its advantage, resulting in uninterrupted dominance over opposition parties (Tripp, 2012). During the transition to democracy in Ghana, the ruling National Democratic Congress (NDC) stacked transition bodies with pro-NDC members and kept the election timetable "closely guarded," while the opposition dismissed the introduction of multi-party politics as "transitions without change" (Gyimah-Boadi and Yakah, 2013, 1-2). In both cases, donors primarily focused their demands on holding of elections. Even though the advent of multi-partyism may not substantially alter politics in aid recipient countries, as these case illustrations suggest, there is nonetheless a clear empirical expectation that follows from the leverage argument: *economic aid should increase the likelihood of multiparty transitions in recipient countries.*

Once countries transition to multiparty regimes, democracy promotion initiatives aim to consolidate democratic rule by strengthening both horizontal and vertical accountability. Donor investments in judicial independence, legislative effectiveness, and anti-corruption commissions, for example, may check abuses by government officials, while donor aid targeting civil society groups and political party development may improve vertical accountability. In a post-transition context, donors are more apt to focus on influencing democracy through targeted investment in horizontal accountability. These investments emphasize strengthening state capacity, which is direclty compatible with economic development goals.

Further, whereas the leverage argument focuses on the relationship between donors and recipient governments, investments in democracy aid target two types of actors in recipient countries: the incumbent government and civil society groups, with the latter sometimes including opposition political parties. The strategies donors choose in attempts to influence recipient country politics have implications for how and to whom donors deliver aid. For example, Dietrich shows that donors increasingly bypass the government and give aid directly to non-state actors in poorly governed countries to prevent the capture of aid through corrupt officials or inefficient state institutions Dietrich (2013, 2014). Further, by strategically targeting sectors where recipient compliance with aid project goals is relatively easy, donors can provide recipient governments with a stronger incentive

to comply and thereby improve aid effectiveness Dietrich (2011).

Who receives democracy aid, in turn, has implications for its political consequences. Democracy aid projects aimed at increasing state capacity target recipient governments and may therefore increase incumbent strength. For example, an aid project such as Canada's $17 million investment in Mali's Justice Development Project in 2010 attempts to strengthen the credibility, effectiveness and accessibility of the national justice system to citizens. The recipient government – in this case the Ministry of Justice – was the direct recipient of democracy aid and implemented the program. Democracy aid that targets recipient governments may strengthen the incumbent regime by improving state capacity in predictable ways and thus increasing the regime's legitimacy vis-a-vis citizens. A case study of donor involvement in Mali by Van de Walle (2013) highlights this point and goes even further by suggesting that the focus of aid resources on the central governments provided a clear incumbency advantage vis-a-vis civil society and political parties. Evidence from Malawi (Resnick, 2013) and Zambia (Rakner, 2013) suggests a similar dynamic where donors contributed to improving electoral management and electoral fairness but executive dominance remained largely unchallenged. Recipient governments therefore have an incentive to implement democracy aid projects, but only when donor goals such as improving state capacity or service delivery do not threaten the ability of the incumbent to retain power.

In contrast, aid efforts that directly target civil society and opposition forces largely bypass the incumbent regime.[4] These projects should strengthen opposition groups and thus weaken incumbent leaders. For instance, Denmark's $4 million support for grass-roots mobilization efforts in Karamoja, Uganda, bypassed the incumbent government entirely and instead was implemented by a local non-governmental organization, Uganda Action for Social Change. By providing organizational and material resources to opposition groups, this bottom-up democracy aid can help level the playing field between incumbents and the opposition. Case study evidence from Benin (Gazibo, 2013) and Ghana (Gyimah-Boadi and Yakah, 2013) show that donor funding of civil society and opposition groups can significantly bolstered non-state actors' capacity to increase voter-turn out thus directly affecting the electoral playing field in a series of successive, competitive elections. In

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[4]In some contexts, however, incumbent governments may infiltrate NGOs.

these types of democracy promotion projects, donors select either the government or groups outside the government as primary recipients of aid. This selection, in turn, has implications for both the causal mechanism linking aid to democratic consolidation outcomes and the direction of the expected relationship: *democracy aid to the government strengthens the incumbent while democracy aid to civil society weakens incumbent power.*

Further, some outcomes associated with democratic consolidation, such as the duration of multiparty regimes, do not necessarily threaten the incumbent's hold on power, as the Tanzanian case illustrates. In contrast, other outcomes, such as stronger electoral support for the opposition, directly threaten government survival. Standard measures of democracy derived from the Polity or Freedom House, however, cannot distinguish between these. We therefore use measures of democratic consolidation that may entail both threats to incumbents and those that do not. This discussion suggests three expectations:

- **Transition hypothesis**: Economic aid increases the likelihood of multiparty transition

- **Consolidation hypothesis (a)**: Democracy aid to recipient governments furthers democratic consolidation that does *not* threaten incumbents

- **Consolidation hypothesis (b)**: Democracy aid to civil society furthers democratic consolidation that may threaten incumbents.

Research Design

Democratic transition and consolidation

We define *multipartyism* as the existence of an opposition party in an elected legislature. Minimally, this entails universal suffrage,[5] an elected legislature, legal opposition parties, and at least one party outside the regime front with seats in the legislature. This definition of multipartyism excludes regimes that allowed opposition parties but never held an election to place them in a legislature (e.g. the former Zaire from 1992-1997). It also says nothing about electoral fairness or civil liberties,

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[5]The suffrage rule allows the start of multiparty politics in South Africa (1994) and Zimbabwe (1980) after the end of restricted suffrage rule.

and thus groups together countries with relatively non-violent and free elections (e.g. Botswana 1999 and 2004) with countries which sometimes have unfair and violent multiparty elections (e.g. Kenya 1997 and 2007).

While multipartyism may be a minimal condition for democracy (Cheibub, Gandhi and Vreeland, 2010), two features of multiparty transitions are relevant for assessing the influence of foreign aid. First, donors can relatively cheaply and objectively identify whether multiple parties compete for and hold positions in an elected legislative body. Second, the introduction of multipartyism need not pose a direct threat to the incumbent regime. We examine how democracy assistance and economic aid influence the advent of 49 multiparty transitions between 1989 and 2008.

Our second measure is the breakdown of a multiparty system. After a country has transitioned to a multiparty regime, it then becomes at risk of reversal. We define *multipartyism failure* as any one of the following occurring: (a) government change via a coup or replacement of a leader/party during a civil conflict;[6] (b) institutional change that excludes the opposition, such that opposition parties are illegal or no party outside the regime front is seated in the legislature; or (c) opposition withdraw so that there is no party outside the regime front seated in the legislature.

The March 2003 coup in the Central African Republic, in which the former Army Chief of Staff – François Bozizé – ousted Félix Patassé, is an example of (a). After the coup, Bozizé suspended the constitution and abolished the legislature. Charles Taylor's government in Liberia banned all opposition parties in April 2002, marking the end of multipartyism (b). He had won the prior election (1997) by a large margin while the opposition UP (Johnson-Sirleaf's party) secured only seven seats, marking the start of multipartyism. The next scheduled election (2003) never took place. Finally, all the main opposition parties in Comoros, including the Movement for Democratic Progress (MDP-NGDC) and the former authoritarian party UDZIMA, boycotted the December 1996 legislative elections. The only other party to win legislative seats was allied with the ruling National Rally for Development. This election boycott ended multipartyism (c).[7]

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[6]Assassination of a leader does not end multipartyism unless this results in an opposition executive taking power or the closing of the legislature.

[7]Table A1 lists the multiparty sample; Table A2 lists the multiparty transition and failure events.

While transitions to and from multipartyism are central to understanding democratic consolidation, neither capture an essential feature of democratic politics: incumbent leaders or parties losing power in elections. Even though the introduction of multiparty politics in the 1990s entailed dislodging many longtime rulers, newly elected leaders only rarely lost at the ballot box, with only eights such instances in the two decades from 1989-2008. With so few data points, it is difficult to directly test how aid influences the prospects of incumbent turnover during this period.

To capture other aspects of democratic consolidation, we use measures of electoral misconduct and opposition vote-share. Electoral misconduct gauges observed incumbent behavior during election years, using an indicator constructed from variables in the NELDA data set: opposition harassment, preventing opposition parties from participating, and violence during elections periods (Hyde and Marinov, 2012). We treat unclear cases as 'no evidence' of misconduct. This codes 30 percent of election years with violence; incumbents prevented opposition participation in 12 percent; and incumbents harassed the opposition in 29 percent.[8] The dependent variable is coded 1 if any of these types of misconduct occur during an election year; and 0 otherwise. We examine 170 multiparty election years from 1989 to 2008 in 40 countries; 47 percent are coded as misconduct.

While we do not directly test how aid influences electoral turnover under multipartyism, this outcome requires that opposition parties become more competitive in elections vis-a-vis the incumbent. We therefore examine electoral support for opposition parties. If democracy assistance provides resources for democratizing agents to mobilize support for political parties and increase voter-turnout,[9] then we should find evidence linking this category of aid to electoral support for opposition candidates and parties. One criticism foreign leaders level at donors – particularly U.S. democracy assistance programs – points to the possibility that democracy aid hurts incumbents by mobilizing political opponents. In Russia and Venezuela, for example, incumbent leaders accused donors of directly funding opposition parties (Cole, 2007; Herszenhorn and Barry, 2012). Alternatively, aid critics often argue that economic aid helps rulers remain in power by providing non-tax revenues with which they can buy acquiescence if not outright support. This latter argument

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[8]Some election years were marred by more than one type of misconduct.

[9]NGOs may aid protesters when electoral institutions perform poorly (Boulding, 2010).

implies that aid should bolster electoral support for incumbents.

We measure opposition electoral support as the percentage vote for the largest opposition party (or candidate) as a share of the two-party vote: $\frac{O}{O+I}$, where O is the vote share for the largest opposition party and I is the vote share for the incumbent. This operationalization circumvents issues related to electoral rules and the fragmentation of party systems by focusing on a continuous measure of how close the largest opposition party is to defeating the incumbent at the polls. We concentrate on first round executive elections *after* a transition to multiparty politics because these contests pit the incumbent party against opposition candidates.[10]

Foreign Aid

We use foreign aid commitment data from AidData 2.0. We aggregate aid commitments at the recipient country year level and distinguish between economic aid and democracy aid sectors. Economic aid subsumes several sectors, including social, democracy, economic infrastructure and services, domestic production, environment, commodity aid, debt relief, budget support, and emergency relief. Democracy assistance has different purposes and distinct delivery modalities. For instance, democracy and governance aid (DGA) includes projects that directly target policy planning in areas such as fiscal and monetary policy, institutional capacity building, and structural reform. DGA also finances tax assessment procedures, legal and judicial development, and constitutional development. Donors use DGA to support government administration by helping finance civil service reform and government infrastructure. In addition to financing governance-related activities, DGA also flows to non-state development actors including civil society groups and political parties to

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[10]We examine 84 elections in 34 countries (listed in Appendix Table A4), a smaller number than in last section because the former included legislative elections and first multiparty elections. When there are no direct elections for President, we include parliamentary elections. We exclude: elections where the second round was never held (e.g. Angola 1992); where the results were annulled (e.g. Nigeria 1993); and where no incumbent contests (e.g. Liberia 2005). Appendix Table B4 shows that results are similar if we employ the *total* opposition vote share (instead of largest opposition vote as share of two-party vote) as the dependent variable.

support community participation, strengthen political accountability, and development.[11]

The top panel of Figure 1 shows that OECD economic aid to sub-Saharan Africa declined in the 1990s but increased in the 2000s, returning to levels that outpaced aid flows at the end of the Cold War. While donors give aid for a variety of reasons, the distribution of aid across different sectors reflects the relative priority donors attach to them. In the past two decades, donors have substantially increased aid earmarked for democracy promotion; and by 2008 we observe a five-fold increase in democracy assistance to Africa. The left panel of Figure 2 shows that the majority of DGA to Africa is direct investment in strengthening state institutions: while DGA rises from 3 percent of total aid in 1990 to over 10 percent in 2008, only a small fraction of democracy promotion activities targets civil society groups. Further, the right panel of Figure 2 shows that the U.S. share of total DGA – which is the focus of studies such as Finkel, Pérez-Lián and Seligson (2007) and Scott and Steele (2011) – remains only a small part of democracy assistance from all donors, particularly after 2000.

We leverage this disaggregated project-level information on aid delivery channels to buttress the micro-foundations of our argument: donors pursue different goals of democracy promotion by relying on government-led delivery mechanisms. We measure aid as the logged value of the lagged three-year moving average of aid per capita.[12] The time series for smaller categories of aid, such as democracy and governance aid, show large variation from year to year, so a moving average smooths these trends to better capture inflows over the prior period.

Empirical Approach

We test the influence of aid at two stages of democratic development: transition and consolidation. The first examines transitions to multiparty regimes; the sample contains 49 transitions in 44 sub-Saharan African countries. The second examines how aid influences the persistence of multiparty regimes in 44 countries, 16 of which fail. Subsequently we also examine the effect of aid on two

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[11]The data groups all non-state actors,including NGOs and political parties, in the same category. Table A5 lists the distinct purposes for democracy and governance aid as coded by AidData (2010).

[12]Aid is: $ln((A_{t-1} + A_{t-2} + A_{t-3})/3)$ where A is constant dollar aid commitments per capita.

additional measures of consolidation: electoral misconduct and opposition vote-share. For the analysis of transitions to and the persistence of multiparty regimes, we adopt a survival approach that accounts for time dependence in the data and include a cubic polynomial of regime duration (Carter and Signorino, 2010).[13] The control variables are: GDP per capita (log), population (log), and an indicator variable for civil war in the past two years.[14] While GDP per capita and population are standard controls in models of aid and political outcomes, civil war in recipient countries can cause donors to reduce aid, particularly democracy assistance, and aid often increases substantially after civil wars end. Further, conflict may directly cause political leaders to lose power because many multiparty failure events occur when rebels oust the incumbent.

To address the possibility that donors give more aid to recipient countries they expect to be more democratic, we first examine "placebo" tests to examine whether there is evidence of a reverse causal pathway, from political change to aid disbursement. The bottom left panel of Figure 1b shows that on average OECD donors reward multiparty transitions with more democracy aid but *not* with more economic aid. This suggests that a naive estimate of the influence of democracy aid on transitions may be biased upwards but this would not necessarily be the case for economic aid. The bottom right panel shows that donors both cut economic assistance and increase democracy aid after a multiparty failure event. These tests suggest the possibility that, even though we use a 3-year lagged moving average for aid, naive estimates may be upwardly biased estimates from reverse causation, particularly for democracy aid.

Addressing endogeneity through exogenous and model-based instruments Our identification strategy is three-fold. First, we employ instruments that exploit information from donor

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[13]The duration variables are correlated with the number of prior elections; thus we account for the possibility that repeated holding of elections promotes democratic norms (Lindberg, 2006).

[14]Economic and population data are from the Penn World Tables (version 7.0) and data for civil war is from Gleditsch 2002. In the Appendix, we examine the robustness of these results with additional control variables, such as economic growth, participation in an IMF adjustment program, oil rents, and ethnic fractionalization.

countries to capture the exogenous flows of aid to recipient countries. Second, we probe the exclusion restriction by (a) testing for alternative channels through which aid may affect democratic change in aid-receiving and (b) relaxing the exogeneity assumption to assess how doing so influences the estimates of interest. Third, we employ "internal" model-based instruments to identify the endogenous regressor through first-stage heteroskedasticity following Lewbel (2012).

We construct the exogenous instrument by leveraging information on two donor country characteristics – domestic inflation and the share of women in parliament – which correlate with aid flows but are unlikely to directly influence political outcomes in recipient countries. For example, inflation is associated with higher government spending, which also influences aid spending; and women in parliament is likely to be correlated with aid because as women increase their representation, aid policy often becomes more oriented toward social equality. Recent studies find that female legislators are more likely to support non-military aid as well as social equity in foreign economic policy (Breuning, 2001). These theoretical considerations do not offer clear-cut expectations about how these factors relate to different types of aid, so we select instruments based on how strongly they correlate with different types of aid to identify the most parsimonious model.

While it is impossible to establish statistical exogeneity, we begin by assuming that these factors shape budgetary decisions in donor countries, including the allocation of aid, but do not directly influence political change in recipient countries. Next, we probe the exclusion restriction by testing the association between the excluded instruments and alternative channels of influence; as well as by relaxing this exclusion restriction and treating the instruments as "plausibly exogenous."

Data on donor inflation (*Inflation*) is from the WDI (2010) and data on female legislators (or women in parliament, *WiP*) is from the Inter-Parliamentary Union (2012). To construct the excluded instruments, we weight the donor characteristic Z (either *Inflation* or *WiP*) by the distance between donor and recipient country.[15] We then sum across all donors for each recipient in each year. Because donors vary by recipient and donor characteristics change over time, the excluded instruments vary across both time and recipient country.

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[15]We thank an anonymous reviewer for this suggestion. Appendix C lists details about how the instruments were constructed.

Probing the exclusion restriction An additional concern arises because the factors we identify as statistically related to aid (Z) but causally *unrelated* to political change in recipient countries may still be correlated with alternative channels that influence the outcome variables. The existence of such variables mean the exclusion restriction may be violated. Our approach to probing the exclusion restriction has two components.

We first show that the excluded instruments are generally uncorrelated with alternative channels through which they might influence the outcome variables. Figure C-3 in the Appendix provides the partial correlation plots for each of the excluded outside instruments, *Inflation* and *WiP*, and an array of potential confounders that represent different areas of foreign policy: trade; membership in international government organizations; partnerships with international non-governmental organizations abroad; the stock of recipient-country migrants in donor countries; and security alliances between donor and recipient countries.[16] If the outside instruments are correlated with these factors, which might be causally related to the outcome variable, then the exclusion assumption is weaker. The partial correlation plots demonstrate that, with the exception of membership in international organizations (*WiP*) and migrant stock (*Inflation*), there is little empirical connection between the outside instruments and these alternative channels. Appendix B also shows that the reported results are robust to including these variables as controls.

We then relax the exogeneity assumption to assess how doing so influences the estimate of interest. In the spirit of treating the outside instruments as "plausibly exogenous", we show that even if these variables (Z) are weakly related to the outcome, inference from the reported tests is still valid. We employ a conservative approach, which Conley, Hansen and Rossi (2012) call the "unified confidence interval", that allows Z to be correlated (to some extent) with the errors in the outcome equation. As the assumed partial correlation between Z and the outcome varies, the estimate for *Economic aid* is still valid for the widest plausible range of this correlation. The estimate for *Democracy aid* is valid for a smaller range of this correlation, implying that the exogeneity assumption about the outside instrument (*WiP*) must be stronger for valid inference.[17]

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[16]Appendix C lists details about the data sources of the alternative channel measures.

[17]Appendix C includes a detailed description of our "plausibly exogenous" approach. Figure C3

Because it is difficult to find appropriate instruments that satisfy the exclusion restriction, we complement the "outside" instruments with information from "internal" instruments using an approach that exploits the fact that covariance between the "first-stage" errors and the exogenous variables (X) is not necessarily zero but rather heteroskedastic Lewbel (2012, 67).[18] Consider the following two-equation model:

$$Y_1 = \beta_1 X + \gamma_1 Aid + \epsilon_1$$

$$Aid = \beta_2 X + \gamma_2 Z + \epsilon_2$$

The main outcome variable of interest (Y_1) is a function of covariates (X) and Aid, which is endogenous (mismeasured). If Z is correlated with aid but unrelated to Y_1, then Z may be used as an "outside" excluded instrument to generate predicted values of Aid. If, however, the $cov(X, \epsilon_2^2) \neq 0$, the "first stage" residuals can be exploited as an "inside" excluded instrument. Because the heteroskedasticity arises from the "first stage", this does not threaten inference about Y_1. The construction of an "internal" excluded instrument is the following: (1) calculate the residual from a "first-stage" regression $(\epsilon_{i,t})$; (2) calculate the in-sample deviation from the mean for each independent variable in the first stage regression $(X_{i,t,} - \mu_X)$; and finally (3) multiply the two: $I = \epsilon_{i,t} \times (X_{i,t,} - \mu_X)$. We choose Z based on maximizing the "first-stage" explanatory power; and employ I for each of the variables in X (GDP pc, Population, and duration polynomials)

What is more, by employing "Lewbel" instruments, which are constructed to be orthogonal to the outcome error process, we can use standard diagnostic tests to assess the extent to which the excluded "outside" instruments (Z) are orthogonal to the outcome errors. For each specification, we report the p-value of the C-statistic, which under the null indicates whether the suspect instruments are valid. Thus p-values larger than conventional cut-points (e.g. 0.10) indicate the exclusion restriction is satisfied. We find no evidence that, conditional on the assumption that the Lewbel

reports the relevant coefficient estimates.

[18]See Appendix C for discussion of this method in our application; and Finkel, Pérez-Lián and Seligson (2007) for a similar application in the aid literature. Appendix Table C1 reports Breusch-Pagan tests for the presence of heteroskedasticity in the first-stage equations.

instruments are valid, the outside instruments are correlated with the outcome errors.

Results

Table 1 reports tests for transitions to multiparty rule. The first three columns examine economic aid. Column 1 reports the OLS estimate while column 2 reports the IV estimate; *Economic aid* is positive and significant in both. The third column reports an IV probit model to ensure that the result does not depend on the linear link function. In the first stage, *Inflation* is positively correlated with economic aid, as expected. The F-statistic for the excluded instruments exceeds the 5 percent threshold for the Stock & Yogo (2005) weak ID test, indicating an excluded instrument set that is strongly correlated with economic aid. Further, the C-statistic indicates that *Inflation* meets the exclusion restriction, conditional on the assumption that the other excluded instruments are valid.

The IV estimate for *Economic aid* is larger than the OLS estimate. This difference would be consistent with donors providing aid to countries that are *less* likely to transition to electoral multipartyism, especially in the late 1980s and early 1990s. For example, in June 1989 U.S. President Bush welcomed Zairian President Mobutu as the first visiting African head of State during his presidency, declaring him a close friend and staunch ally who deserved substantial financial support. By 1993, however, after the Clinton administration cut aid to Zaire, Mobutu declared "I am the latest victim of the cold war, no longer needed by the U.S." (Zagorin, 1993). One way to gain leverage on this conjecture is to exclude the first years of the sample to see if the coefficients converge. Indeed, when we exclude the first four years of the sample (with lagged aid measured from 1986-1991) the respective linear coefficients are 0.075 (OLS) and 0.089 (IV), while the probit coefficients are 0.302 (probit) and 0.310 (IV-probit).

The next three columns report corresponding models for democracy aid. Consistent with the placebo tests in the bottom left panel of Figure 1b, the estimate for *Democracy aid* is slightly smaller but not statistically significant in the two-stage models. The "Lewbel" instruments in this sample for this measure of aid do not contribute much explanatory power in the first stage (as shown by the relatively small F-statistic for I only) because the errors from the first stage equation

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do not have substantial heteroegeneity.[19]

To test the robustness of the *Economic aid* finding, we estimate specifications that: (1) include additional controls variables; (2) employ only the "Lewbel" instruments and not *Inflation*; (3) add two-way (time period and country) fixed effects; and (4) drop small island countries. Second, we estimated the IV probit model when excluding one country at a time (Figure B1). Third, we estimated "naive" fixed effects linear probability models with: (a) a quadratic time trend; and (b) country-specific time trends (B3). The result for *Economic aid* remains in each of these tests.

Table 2 presents results for multiparty failure, with a similar set of specifications. Because the dependent variable is multiparty *failure*, a negative coefficient estimate reflects a factor that increases the likelihood of multiparty survival. The estimates for *Economic aid* suggest a null finding; if anything the two-stage estimate indicates that this type of aid increases the chances of multiparty failure (but it is not statistically different from zero). And while the F-statistic is below the standard cut-points for weak ID tests, this results from the fact that the Lewbel instruments add little explanatory power in the first stage (the F-statistic for I only is quite low). We obtain a similar negative but insignificant estimate without the "Lewbel" instruments.

Turning to the estimates for *Democracy aid*, we find a consistent negative relationship with multiparty failure. Both with and without the "Lewbel" instruments, the F-statistic is greater than the thresholds indicating a weak instrument. Further, the C-statistic indicates that *WiP* meets the exclusion restriction, conditional on the "Lewbel" instruments being valid.

To test the robustness of the *Democracy aid* finding, we estimate specifications that: (1) include more control variables and no controls beyond duration; (2) employ only the "Lewbel" instruments based on duration time; (3) add two-way (time period and country) fixed effects; and (4) drop small island countries. Second, we estimate the IV probit model when excluding one country at a time (Figure B1). Third, we estimate "naive" fixed effects linear models with: a quadratic time

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[19]This causes the F-statistic for all excluded instruments (Z and I) to decrease. In Appendix C, we report tests of heterogeneity in the first-stage to explore the extent to which the "Lewbel" instruments boost efficiency. In Table C2, we report a similar null result for *Democracy aid* when using only *WiP* as an excluded instrument, with a large F-statistic.

trend; and country-specific time trends (B3). The result for *Democracy aid* remains.

The top panel of Figure 3 depicts a substantive interpretation of the findings for multipartyism. The left panel uses the estimates from Table 1, model 3 to simulate the risk of multiparty transition across values of economic aid. In this sample the outcome of interest occurs in 13 percent of observation years. Increasing economic aid from the 25th percentile to the 75th percentile is associated with an increase in the simulated transition risk from roughly 6 percent to 11 percent. The right panel shows the simulations for democracy aid and multiparty failure from column 6, Table 2. In this sample, multiparty failure occurs in just over 2 percent of observations. Increasing democracy aid from the 25th pcentile to the 75th percentile reduces the simulated risk of breakdown by over two-thirds, from 3.5 percent to 0.8 percent.[20]

Foreign aid and African elections

In this section we examine how foreign aid influences elections in sub-Saharan Africa. Thus far, we have shown that economic aid increases the chances of transition to multiparty politics but has little influence on the survival of multiparty politics. Democracy assistance, on the other hand, has little influence on the introduction of multiparty politics but helps multipartyism persist. However, this analysis, does not capture democratic consolidation outcomes that might threaten the incumbent. One outcome that represents a potential threat to the incumbent's ability to retain power is electoral misconduct. A second outcome, which more directly threatens incumbents in multiparty regimes, is stronger electoral support of opposition parties.

Electoral misconduct We look first at observed incumbent behavior during multiparty election years, using an indicator of electoral malfeasance by the incumbent constructed from variables in the NELDA data set: opposition harassment, preventing opposition parties from participating, and violence involving civilian deaths (Hyde and Marinov, 2012). These concepts capture aspects of

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[20]While the simulations indicate that the aid estimates reported in Tables 1 and 2 are substantively important, the simulation estimates should not be interpreted literally because the absolute size of the estimates reflects the observed incidence of the dependent variable in each of the samples.

electoral misconduct using information from election observer reports. The dependent variable is coded 1 if any of these types of misconduct occur during an election year; and 0 otherwise. Just under one-half of multiparty election years are coded as incumbent misconduct.

The explanatory variables include foreign aid, GDP per capita (log), population (log), IMF program participation, whether election monitors were present, and whether the election was the first multiparty election.[21] IMF program participation and the presence of election monitors may shape aid allocation; and as Hyde and O'Mahony (2010) show, these factors also influence whether the incumbent government attempts to win elections by focusing more on pre-electoral fiscal manipulation or direct manipulation such as vote stealing, intimidation, and harassing the opposition. Again we use a two-stage linear probability model, and check results with an IV-probit.

The first three columns of Table 3 examine economic aid. While the estimates for *Economic aid* are negative, they are not statistically different from zero; and the IV estimate is lower in absolute size than the OLS estimate. The F-statistic indicates an instrument set that is strongly correlated with economic aid; and the C-statistic suggests that *Inflation* is a valid instrument, conditional on the assumption that the "Lewbel" instruments are valid.

The estimates for *Democracy aid* show no result in the OLS model, but yield negative and significant estimates in the IV models. This suggests that the naive estimates may reflect donors giving more governance aid prior to multiparty contests in low state-capacity countries where electoral misconduct is likely, precisely because it is more difficult for the government to conduct an election in these countries.[22] This scenario would be consistent, for example, with donors

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[21]The NELDA data include measures of whether the election was the first multiparty contest and whether monitors are present. In robustness tests we show the results hold when adding further control variables: economic growth, civil conflict, ethnic fractionalization, trade, IGOs, NGOs, membership in IOs, migrant stock, and ally. Further controls from NELDA are indicators of whether the incumbent expected to win the election and whether there was an opposition boycott.

[22]Using the "Lewbel" instruments in this sample for democracy aid lowers the overall F-stat $(Z + I)$ to about 10 because the F-stat for these 'internal' instrument (I) is small: 1.8. Re-estimating the models with only Z and not I as an excluded instrument increases the F-stat such

financing roughly 90 percent of the costs associated with the 2006 election in the DRC, which was marred by violence (Kadima, Leonard and Schmidt, 2009, 24). The bottom panel of Figure 3 plots the simulated effect of democracy aid on the probability of observing electoral misconduct, from estimates in last column of Table 3. An increase in democracy aid from the 25th percentile to the 75th percentile lowers the simulated probability of misconduct from over 55 percent to 38 percent.

These findings are consistent with the results for multiparty breakdown: democracy assistance is associated with a more democratic outcome, in this case a lower likelihood of electoral misconduct. Economic aid, on the other hand, appears to have little consistent influence on electoral misconduct. The result for democracy aid is robust to additional control variables; and when dropping small island countries or civil war years from the sample.

Electoral support for the opposition Does democracy aid increase the strength of opposition parties? Our measure of opposition electoral support is the percentage vote for the largest opposition party (or candidate) as a share of the two-party vote: $\frac{O}{O+I}$, where O is the vote share for the largest opposition party and I is the vote share for the incumbent. This operationalization circumvents issues related to the fragmentation of party systems by focusing on a continuous measure of how close the largest opposition party is to defeating the incumbent at the polls. We concentrate on first round executive elections *after* a transition to multiparty politics because these contests pit the incumbent party against opposition candidates and capture support for the largest opposition party. We fit a linear model with a log-transformed dependent variable to account for the proportional nature of the vote share data.

Table 4 reports the results. Given the small number of observations, we use a minimum of control variables: GDP per capita, population, economic growth, and IMF program participation. The first three columns examine economic aid, the latter three democracy aid. For each, we report a naive model, an IV model, and one with the sample restricted to elections in which the incumbent wins less than 90% of the vote, as was the case in Equatorial Guinea in 1996 and 2002.[23]

that it easily passes weak instrument tests. Further, the C-statistic indicates that *WiP* is a valid instrument, conditional on the others being valid.

[23]Of the 84 elections in the sample, the largest opposition party takes at least 10% of the vote

The estimate for *Economic aid* is positive in the naive model but becomes negative in the IV model, though neither estimate is different from zero. We find a consistent negative estimate for economic aid in various alternative IV specifications reported in the Appendix, but none of the estimates are statistically different from zero. Despite the lack of statstical significane, this suggests that once we account for endogeneity, economic aid might *hurt* electoral competitiveness. For democracy aid, the naive and the IV estimate on the full sample yield positive estimates but these findings hinge entirely on including elections where the incumbent wins handily, as illustrated by the null finding in column 6. Democracy aid has little influence on opposition support in elections that are minimally competitive. The F- and C-statistics indicate valid instruments.

An aid curse?

Several studies posit that aid inhibits democracy by reducing political accountability for recipient governments (Remmer, 2004; Moss and van de Walle, 2008). Non-tax revenue such as aid relieves the state of taxing citizens, which may reduce citizens' incentive to demand accountability (Djankov and Reynal-Querol, 2008, 172). Recent studies, however, find that aid does not hurt tax effort (Carter, 2013; Morrissey, 2014) and has few deleterious effects on democracy post-1989 (Bermeo, 2011, 2013). While our findings suggest that economic aid helped purchase multiparty transitions in Africa – a finding consistent with aid curse dissenters (Dunning, 2004; Wright, 2009; Bermeo, 2011) – this form of aid is nonetheless associated with an increased risk of multiparty failure and lower opposition vote shares (though neither of these results are statistically significant). That said, the aid curse literature focuses on economic, not democracy, assistance. We find that though democracy assistance is associated with more stable multiparty systems and fair elections, there is little evidence that it helps the electoral opposition. Thus, once we examine different types of foreign aid and distinct aspects of democratic politics, the findings do not fit neatly with "aid curse" or "aid blessing" labels. Rather, the picture is more complicated because we account for donor strategies and the possibility that some forms of political change – such as multiparty transitions and fair

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in 70 cases, but fails to reach this threshold in 14 cases. These 14 cases receive roughly half the democracy aid (on average) than the other 70 cases: $2.3 per capita vs. $4.0.

elections – are not necessarily threatening to incumbents.

Conclusion

The findings suggest several insights into the relationship between foreign aid, democratic transition and consolidation in Africa. While economic aid is a catalyst for transitions to multiparty party regimes, democracy aid stabilizes multiparty regimes and decreases the incidence of electoral misconduct, which we interpret as increasing horizontal accountability. Importantly, neither of these outcomes may necessarily threaten incumbent governments in institutionalized multiparty regimes. However, we find little evidence that either economic or democracy aid increases the competitiveness of the opposition, which is a necessary condition for incumbent turnover. This sugggests that the primary channel through which democracy promotion occurs is government-led political reform. In short, we find evidence consistent with both the leverage mechanism linking economic aid to multiparty transitions as well as the investment mechanism linking democracy aid to consolidation outcomes – but only those that do not necessarily threaten incumbents.

Despite the rhetoric about strengthening civil society, we show that donors largely pursue democracy promotion through governance aid, with predictable effects on maintaining a stable multiparty electoral system. If the goal of democracy promotion is to increase opposition strength and, more broadly, consolidate democracy through channels that improve vertical accountability, we would expect donors to directly invest in opposition parties and civil society groups. This in turn, should promote vertical accountability by increasing the electoral strength of opposition parties. If, on the other hand, democracy aid largely targets governance programs and is implemented by incumbent parties, it is less likely that external democracy promotion yields reform outcomes that put incumbents at risk of losing power. Instead democracy aid may perpetuate a multiparty system in which incumbents retain power. Thus our finding for multiparty transitions and failure dovetails with Crawford Young's (1999, 35) claim that "semi-democracy is probably sufficient to deflect international system pressures for more complete political opening, particularly if macroeconomic management earns external approbation."

While our results are robust to excluding post-civil conflict periods from the tests of multiparty

stability, more can be done to account conflict's consequences on processes of democratic change. As Flores and Nooruddin (2009) and Flores and Nooruddin (2012) show, aid flows often spike during post-conflict periods. Future research should further investigate the political consequences of post-conflict-induced aid.

Finally, even though we find little evidence that democracy assistance strengthens opposition parties in elections, we cannot rule out the possibility that democracy promotion efforts that improve other consolidation outcomes – such as maintaining a stable multiparty regime and improving the conduct of elections – will bolster opposition parties at the polls in the future. A more level playing field may induce further domestic investment in opposition groups down the road.

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Table 1: Foreign aid and transitions to multipartyism

	OLS	IV OLS	IV probit	OLS	IV OLS	IV probit
	(1)	(2)	(3)	(4)	(5)	(6)
Economic aid	0.058**	0.108**	0.473*			
	(0.02)	(0.04)	(0.25)			
Democracy aid				0.073**	0.067	0.428
				(0.03)	(0.07)	(0.54)
Log GDP pc	0.002	0.004	0.006	0.001	0.001	0.005
	(0.02)	(0.02)	(0.11)	(0.03)	(0.03)	(0.12)
Log population	0.021	0.041**	0.169	0.010	0.009	0.060
	(0.01)	(0.02)	(0.11)	(0.01)	(0.01)	(0.10)
Civil war	-0.114**	-0.109**	-0.654**	-0.123**	-0.123**	-0.714**
	(0.04)	(0.04)	(0.29)	(0.04)	(0.04)	(0.28)
(Intercept)	-0.135	-0.452	-3.568**	0.085	0.098	-1.568
	(0.25)	(0.31)	(1.71)	(0.22)	(0.24)	(1.44)

	Economic aid	Democracy aid
Inflation	0.161**	
	(0.05)	
WiP		0.042**
		(0.01)
Log GDP pc	0.111	0.025
	(0.10)	(0.06)
Log population	-0.328**	-0.198**
	(0.05)	(0.04)
Civil war	-0.174	-0.022
	(0.13)	(0.14)
I_{GDP}	0.172	-0.072
	(0.15)	(0.16)
I_{pop}	0.182*	-0.096
	(0.10)	(0.10)
I_{civwar}	-0.273	0.206
	(0.33)	(0.33)
I_{d1}	-0.027	0.193**
	(0.09)	(0.09)
I_{d2}	-0.001	-0.009**
	(0.00)	(0.00)
I_{d3}	0.000	0.000**
	(0.00)	(0.00)
(Intercept)	4.475**	1.268**
	(0.76)	(0.58)

F-statistic (Z)	9.5	19.2
F-statistic (I)	23.4	3.9
F-statistic (Z & I)	22.6	7.2
5% maximal IV relative bias	19.9	19.8
10% maximal IV relative bias	11.3	11.3
p-value (C-statistic)	0.25	0.31

* $p<0.10$; ** $p<0.05$. Duration polynomials (3) not reported. F-statistic is the Kleibergen-Paap rk Wald F statistic. N×T = 370 observations in 44 countries from 1989-2008. First-stage F-statstic based on 43 degrees of freedom.

Table 2: Foreign aid and multiparty failure

	OLS	IV OLS	IV probit	OLS	IV OLS	IV probit
	(1)	(2)	(3)	(4)	(5)	(6)
Economic aid	-0.015	0.043	0.533			
	(0.01)	(0.04)	(0.37)			
Democracy aid				-0.030**	-0.043*	-0.637*
				(0.01)	(0.03)	(0.39)
Log GDP pc	-0.012	-0.012	-0.222*	-0.017**	-0.020**	-0.381**
	(0.01)	(0.01)	(0.12)	(0.01)	(0.01)	(0.18)
Log population	-0.011	0.008	0.096	-0.011*	-0.013*	-0.190*
	(0.01)	(0.01)	(0.15)	(0.01)	(0.01)	(0.10)
Civil war	0.059	0.070	0.588	0.057	0.054	0.471
	(0.06)	(0.06)	(0.40)	(0.06)	(0.06)	(0.41)
(Intercept)	-0.708**	-1.092**	-2.767	-0.692**	-0.642**	3.361*
	(0.14)	(0.26)	(2.75)	(0.11)	(0.15)	(2.03)

	Economic aid	Democracy aid
Inflation	0.276**	
	(0.05)	
WiP		5.556**
		(0.01)
Log GDP pc	0.020	-0.094
	(0.07)	(0.06)
Log population	-0.314**	-0.204**
	(0.04)	(0.04)
Civil war	-0.152	-0.191*
	(0.14)	(0.12)
I_{GDP}	0.137	-0.030
	(0.16)	(0.13)
I_{pop}	0.001	-0.205**
	(0.10)	(0.07)
I_{civwar}	-0.354	-0.248
	(0.62)	(0.56)
I_{d1}	-0.354	-0.114*
	(0.08)	(0.06)
I_{d2}	-0.002	0.007
	(0.01)	(0.00)
I_{d3}	0.000	-0.000
	(0.00)	(0.00)
(Intercept)	5.556**	2.257**
	(0.72)	(0.67)

F-statistic (Z)	28.7	47.7
F-statistic (I)	0.9	3.7
F-statistic (Z & I)	7.6	23.7
5% maximal IV relative bias	19.9	19.8
10% maximal IV relative bias	11.3	11.3
p-value (C-statistic)	0.48	0.73

* $p<0.10$; ** $p<0.05$. Duration polynomials (3) not reported. F-statistic is the Kleibergen-Paap Wald F statistic. N×T = 583 observations in 44 countries from 1989-2008.

Table 3: Foreign aid and electoral misconduct

	OLS	IV OLS	IV probit	OLS	IV OLS	IV probit
	(1)	(2)	(3)	(4)	(5)	(6)
Economic aid	-0.099	-0.068	-0.193			
	(0.08)	(0.08)	(0.29)			
Democracy aid				-0.035	-0.175*	-0.466*
				(0.06)	(0.10)	(0.27)
Log GDP pc	-0.019	-0.025	-0.066	-0.041	-0.046	-0.117
	(0.07)	(0.07)	(0.19)	(0.06)	(0.06)	(0.16)
Log population	0.023	0.031	0.075	0.042	0.023	0.052
	(0.06)	(0.06)	(0.17)	(0.05)	(0.05)	(0.12)
IMF program	0.011	-0.006	-0.004	-0.031	0.010	0.035
	(0.14)	(0.15)	(0.41)	(0.13)	(0.13)	(0.31)
First election	0.130	0.116	0.306	0.082	0.068	0.163
	(0.12)	(0.12)	(0.34)	(0.12)	(0.11)	(0.29)
Monitor	-0.060	-0.053	-0.140	-0.043	-0.055	-0.148
	(0.14)	(0.13)	(0.35)	(0.14)	(0.13)	(0.35)
(Intercept)	0.810	0.676	0.562	0.474	0.837	0.940
	(0.68)	(0.70)	(1.98)	(0.59)	(0.65)	(1.62)

	Economic aid	Democracy aid
Inflation	0.090**	
	(0.04)	
WiP		0.083**
		(0.01)
Log GDP pc	0.258**	-0.004
	(0.05)	(0.06)
Log population	-0.171**	-0.185**
	(0.05)	(0.05)
IMF program	0.481**	0.225
	(0.10)	(0.15)
First election	0.293**	0.315**
	(0.13)	(0.12)
Monitor	-0.089	-0.262**
	(0.11)	(0.11)
I_{GDP}	0.141	-0.231*
	(0.12)	(0.12)
I_{Pop}	0.122*	-0.238**
	(0.06)	(0.10)
I_{IMF}	-1.016**	-0.201
	(0.17)	(0.19)
I_{First}	-0.447	-0.008
	(0.42)	(0.21)
$I_{Monitor}$	0.090	-0.131
	(0.30)	(032)
(Intercept)	3.083**	1.245*
	(0.61)	(0.68
F-statistic (Z)	4.8	42.7
F-statistic (I)	27.1	1.8
F-statistic (Z & I)	27.4	10.5
5% maximal IV relative bias	19.3	19.3
10% maximal IV relative bias	11.1	11.1
p-value (C-statistic)	0.15	0.21

* $p<0.10$; ** $p<0.05$. F-statistic is the Kleibergen-Paap Wald F statistic. 170 election years in 40 countries from 1989-2008.

Table 4: Foreign aid and opposition vote share

	OLS	IV	IV	OLS	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Economic aid	0.262	-0.361	-0.273			
	(0.32)	(0.29)	(0.21)			
Democracy aid				0.578**	1.002**	0.146
				(0.26)	(0.44)	(0.25)
Log GDP pc	-0.193	-0.113	-0.210**	-0.088	-0.036	-0.213**
	(0.13)	(0.14)	(0.09)	(0.15)	(0.16)	(0.10)
Log population	0.011	-0.185	-0.227**	0.018	0.083	-0.108
	(0.19)	(0.18)	(0.08)	(0.13)	(0.15)	(0.08)
Growth	-0.021	-0.027	-0.004	-0.032	-0.038*	-0.007
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
IMF program	-0.096	0.136	0.250	-0.197	-0.343	0.080
	(0.33)	(0.31)	(0.22)	(0.32)	(0.37)	(0.25)
(Intercept)	-0.587	2.897	3.905**	-0.994	-2.367	1.720
	(3.06)	(2.98)	(1.35)	(2.14)	(2.45)	(1.49)
		Economic aid			Democracy aid	
Inflation		0.178**	0.164**			
		(0.05)	(0.05)			
WiP					0.073**	0.081**
					(0.02)	(0.03)
Log GDP pc		0.204**	0.200**		0.009	-0.027
		(0.07)	(0.06)		(0.09)	(0.10)
Log population		-0.289**	-0.322**		-0.200**	-0.211**
		(0.05)	(0.04)		(0.06)	(0.06)
Growth		-0.000	0.003		-0.006	-0.006
		(0.01)	(0.01)		(0.01)	(0.01)
IMF program		0.428**	0.502**		0.507**	0.487**
		(0.13)	(0.13)		(0.16)	(0.18)
I_{GDP}		-0.057	-0.055		-0.116	-0.098
		(0.16)	(0.14)		(0.15)	(0.17)
I_{Pop}		-0.021	0.013		-0.242**	-0.240**
		(0.12)	(0.10)		(0.06)	(0.09)
I_{Growth}		0.026**	0.040**		-0.042**	-0.035**
		(0.01)	(0.01)		(0.02)	(0.02)
I_{IMF}		-0.889**	-1.076**		-0.094	-0.267
		(0.23)	(0.20)		(0.30)	(0.35)
(Intercept)		4.383**	4.707**		1.155	1.384
		(0.87)	(0.77)		(0.97)	(1.13)
F-statistic (Z)		15.3	9.4		10.8	7.6
F-statistic (I)		13.7	13.4		12.8	7.3
F-statistic (Z & I)		22.5	15.5		19.1	12.8
5% maximal IV relative bias		18.4	18.4		18.4	18.4
10% maximal IV relative bias		10.8	10.8		10.8	10.8
p-value (C-statistic)		0.19	0.51		0.20	0.22

* $p<0.10$; ** $p<0.05$. F-statistic is the Kleibergen-Paap Wald F statistic. 84 elections in columns 1-2, 4-5; 70 elections in columns 3 and 6. 31 countries from 1989-2008. /Clustered standard errors reported.

(a) Foreign aid trends over time

Economic Aid
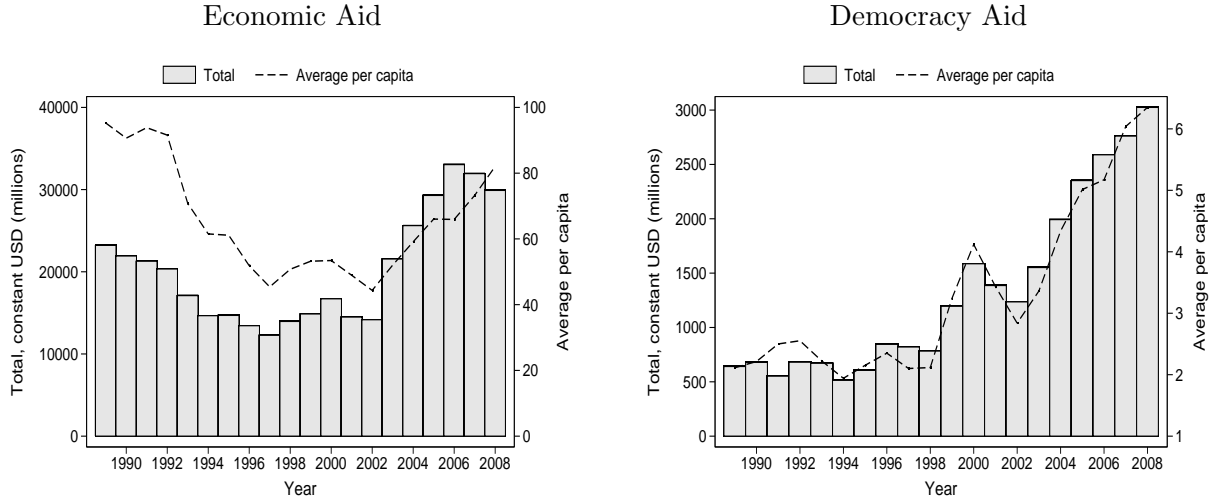
Democracy Aid

(b) Aid dividend from multiparty transition and collapse

Multiparty transition

Multiparty failure
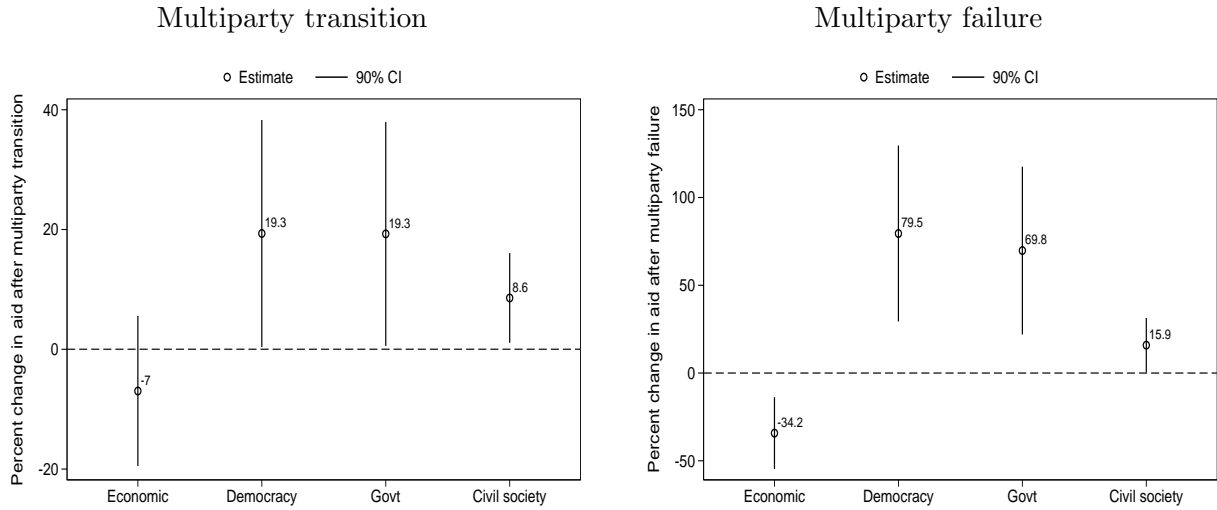
Figure 1: **Foreign aid in sub-Saharan Africa.** Top panel shows economic aid and democracy and governance aid (DGA). Total is constant USD in millions. Per capita is constant USD averaged across 48 countries. Two year moving average (t, t-1). Bottom panel shows the percent change in aid in the three years after multiparty transition (failure) relative to three years prior to transition (failure). Each panel shows the average change for economic aid, democracy aid, and two categories of democracy aid (civil society and government). Estimates in bottom panel obtained from a country fixed effects regression with controls for civil war, 1989-2008. Sources: AidData.org and authors' calculation.
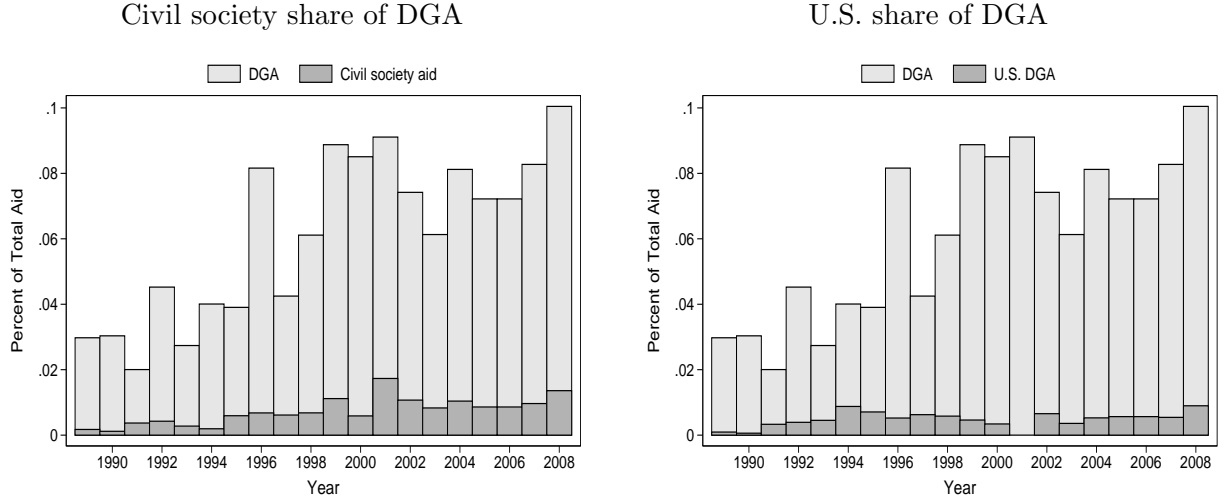
Figure 2: **Democracy and Governance Aid (DGA), share of total aid by year**. Left panel: large bars depict $\frac{DGA}{AllAid}$; small, darker bars depict $\frac{CivilSocietyAid}{TotalDGA}$. Right panel: large bars depict $\frac{DGA}{AllAid}$; small, darker bars depict $\frac{U.S.DGA}{TotalDGA}$. Two-year (t, t-1) moving average of yearly shares in constant dollars. Sources: AidData.org, WDI (2010), and authors' calculation.

Economic aid & transition

Democracy aid & failure
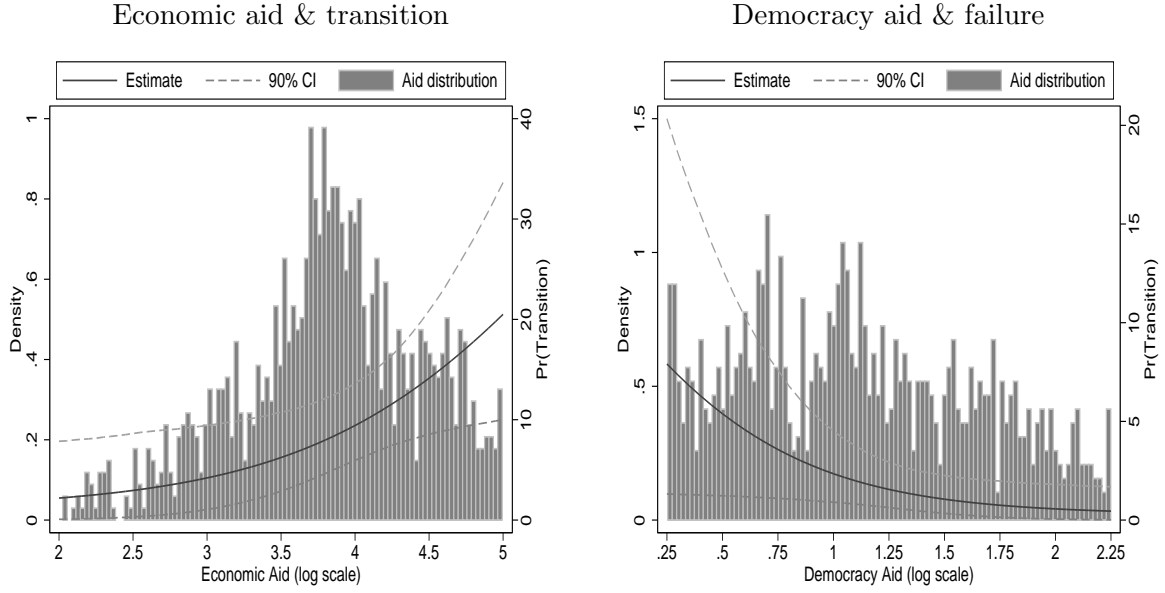
(b) Democracy aid & electoral misconduct
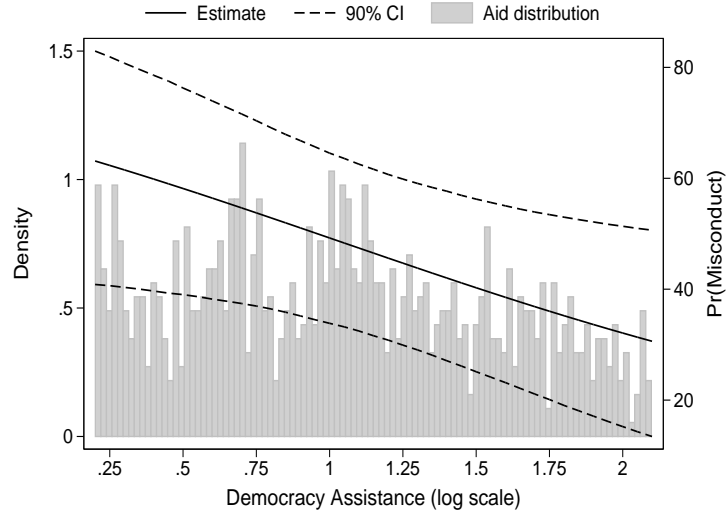
Figure 3: **Foreign aid and democratic consolidation**. Top panel shows the substantive effect of foreign aid on *multiparty transition and failure*. Horizontal axes measure aid and vertical axes represent the simulated risk of transition or failure. Shaded histograms are the distribution of aid in the sample. All control variables set at within-sample means or medians. Bottom panel shows substantive relationship between democracy aid and *electoral misconduct*. Years: 1989-2008.

Foreign Aid Allocation Tactics and Democratic Change in Africa[*]

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Abstract

Over the past two decades, donors increasingly link foreign aid to democracy objectives in Africa. This study investigates whether and how foreign aid influences specific outcomes associated with democratic transition and consolidation. Using an instrumental variables approach for the period from 1989 to 2008, we show that economic aid increases the likelihood of transition to multiparty politics, while democracy aid furthers democratic consolidation by reducing the incidence of multiparty failure and electoral misconduct. However, we find little evidence that either economic or democracy aid influences opposition support in multiparty elections. These findings have implications for how understanding how donors allocate aid and the political consequences of foreign assistance in Africa.

Appendix A: Data

Table A-1: Multipartyism

Country	Start	End	Country	Start	End
Angola	1993	censored	Liberia	1986	1990
Benin	1992	censored	Liberia	1998	2002
Botswana	1967†	censored	Liberia	2006	censored
Burkina Faso	1993	censored	Madagascar	1994	censored
Burundi	1994	1994	Malawi	1995	censored
Burundi	2006	censored	Mali	1993	censored
Cameroon	1993	censored	Mauritania	1993	2005
Cape Verde	1992	censored	Mauritania	2007	2008
Central African Rep.	1994	2003	Mauritius	1969†	censored
Central African Rep.	2006	censored	Mozambique	1995	censored
Chad	1998	censored	Namibia	1995	censored
Comoros	1993	1996	Niger	1994	1996
Comoros	2005	censored	Niger	1997	1999
Congo Br.	1993	1997	Niger	2000	censored
Congo Br.	2003	censored	Nigeria	1993	1993
Côte d'Ivoire	1991	1999	Nigeria	2000	censored
Côte d'Ivoire	2001	censored	Rwanda	2004	censored
Congo DR	2007	censored	São Tome	1992	censored
Equatorial Guinea	1994	censored	Senegal	1979†	censored
Ethiopia	1996	censored	Seychelles	1994	censored
Gabon	1991	censored	Sierra Leone	1997	1997
Gambia	1967†	1994	Sierra Leone	2003	censored
Gambia	1998	censored	South Africa	1994	censored
Ghana	1997	censored	Sudan	1987†	1989
Guinea	1996	censored	Tanzania	1996	censored
Guinea-Bissau	1995	2003	Togo	1995	censored
Guinea-Bissau	2005	censored	Uganda	2007	censored
Kenya	1993	censored	Zambia	1992	censored
Lesotho	1999	censored	Zimbabwe	1981†	censored

Start year coded for first year when multipartyism is observed on January 1; this is typically the year *after* the first multiparty election. † indicates left-censored at 1989; *censored* ≡ right-censored in 2008.

Table A-2: Multiparty failure events

Country	Date	Event description
Burundi	10 September 1994	Frodebu agrees to Government Convention to dampen threat of violence. The institutional change created a governing body, the Convention, that superceded the 1992 constitution and ended Frodebu's defacto 1993 legislative victory. Reyntjens (2009, 37) writes, "the constitution was shelved and the outcome of both the presidential and parliamentary elections was swept aside as the president and parliament were placed under the trusteeship of an unconstitutional body."
Central African Rep.	15 March 2003	Bozizé coup (Debos 2008, 229). His troops took Bangui, suspended the constitution, dissolved the National Assembly, and the elected leader (Patassé) fled.
Comoros	1 December 1996	The 1996 legislative elections were boycotted by opposition parties. Only 3 of 43 contested seats won by an alternative party which supported the ruling RND (Inter-parliamentary Union, 1996).
Congo-Brazzaville	14 October 1997	Sassou-Nguesso's militia, backed by Angolan troops, ousted the Lissouba government from the presidential palace (Clark 1998, 35). Sassou-Nguesso elected a transition advisory legislature in 1998 and announced a transition to democracy but no new legislature was elected.
Côte d'Ivoire	24 Demember 1999	Gen. Gueï ousts the Bédié government. Gueï amends the constitution in 2000 to exclude Outtara from the presidential contest (Daddieh 2001, 18). Gueï declares himself winner of the October 2000 elections, despite losing the vote.
Gambia	22 July 1994	The Armed Forces Provisional Ruling Council (AFPRC) seized power in a military coup, deposing the government of Sir Dawda Jawara. Lieutenant Jammeh, chairman of the AFPRC, suspended the Constitution, banned opposition parties, and held flawed elections in 1996 and 1997 (Saine 2002, 168).
Guinea-Bissau	14 September 2003	Coup ousts Yala. President Yala dissolves Parliament on 15 November 2012, promises and then postpones new elections (Election Guide; Malan 2005, 13). Transitional government under a military junta holds new elections in 2005.
Liberia	9 September 1990	Doe's government toppled when Prince Johnson's rebel forces kill Doe and capture Monrovia. An international interim government put in place, but Taylor controlled most of the territory (Reno 1999, 93).
Liberia	2 April 2002	Elections that were scheduled for 2003 never took place. A ban on political parties took effect on 2 April 2002 under February-September State of Emergency (Outram 2003, 604).
Mauritania	3 August 2005	President Taya was deposed in a bloodless coup (N'Diaye 2006). The ruling military council (CMJD) led by Colonel Vall dissolved Parliament (Pazzanita 2008, 132). New Constituent Assembly elected in November/December 2006.
Mauritania	6 August 2008	President Abdallahi was overthrown in a bloodless coup. The country was officially run by a 12-member High State Council (HSC) composed entirely of military officers (N'Diaye 2009).
Niger	27 January 1996	Baré Maïnassara coup replaces President Mahamane Ousmane. The ruling military council (Conseil de Salut National) suspended the Constitution and political parties, and dissolved the National Assembly (Englebert 2003, 794).
Niger	9 April 1999	Maïnassara assassinated by members of his predisential guard. Prime Minister Mayaki dissolved the assembly and political parties and two days later the military junta (CRN) led by Gen. Wanke "suspended the constitution and formally dissolved the government" (Political Handbook of the World, 2012).
Nigeria	16 June 1993	Babangida via the National Election Council (NEC) annuls 1993 presidential election. Babangida promises new elections (twice) but Abacha forces him to resign in November 1993 (Political Handbook of the World, 2012).
Sierra Leone	25 May 1997	The Armed Forces Revolutionary Council (AFRC), led by Maj. Johnny Paul Koroma, overthrow President Kabbah (Reno 1999, 138).
Sudan	30 June 1989	Coup led by Col al-Bashir and an Islamist faction of the military ousted the elected government (Burr and Collins 2003, 1).

Table A-3: Electoral misconduct during election years

Country	Year	Misconduct	Country	Year	Misconduct	Country	Year	Misconduct
Angola	1992	1	Gabon	1990	1	Mauritania	2006	0
Angola	2008	1	Gabon	1993	1	Mauritania	2007	1
Benin	1991	1	Gabon	1996	0	Mauritius	1991	0
Benin	1995	0	Gabon	1997	0	Mauritius	1995	0
Benin	1996	0	Gabon	1998	0	Mauritius	2000	0
Benin	1999	0	Gabon	2001	0	Mauritius	2005	0
Benin	2001	0	Gabon	2005	0	Mozambique	1994	0
Benin	2003	1	Gabon	2006	0	Mozambique	1999	0
Benin	2006	0	Gambia	1992	0	Mozambique	2004	0
Benin	2007	0	Gambia	1997	0	Namibia	1994	0
Botswana	1989	0	Gambia	2001	1	Namibia	1999	0
Botswana	1994	0	Gambia	2002	0	Namibia	2004	0
Botswana	1999	0	Gambia	2006	0	Niger	1993	1
Botswana	2004	0	Gambia	2007	0	Niger	1995	0
Burkina Faso	1992	0	Ghana	1996	1	Niger	2004	0
Burkina Faso	1997	0	Ghana	2000	1	Nigeria	1992	1
Burkina Faso	1998	0	Ghana	2004	1	Nigeria	1999	1
Burkina Faso	2002	0	Ghana	2008	0	Nigeria	2003	1
Burkina Faso	2005	0	Guinea	1995	1	Nigeria	2007	1
Burkina Faso	2007	0	Guinea	1998	1	Rwanda	2003	1
Burundi	1993	0	Guinea	2002	0	Rwanda	2008	0
Burundi	2005	0	Guinea	2003	1	Senegal	1993	1
Cameroon	1992	1	Guinea-Bissau	1994	0	Senegal	1998	1
Cameroon	1997	1	Guinea-Bissau	1999	1	Senegal	2000	0
Cameroon	2002	0	Guinea-Bissau	2000	1	Senegal	2001	0
Cameroon	2004	0	Guinea-Bissau	2004	0	Senegal	2007	0
Cameroon	2007	0	Guinea-Bissau	2005	0	Sierra Leone	1996	1
Central African Republic	1993	0	Guinea-Bissau	2008	1	Sierra Leone	2002	0
Central African Republic	1998	0	Kenya	1992	1	Sierra Leone	2007	1
Central African Republic	1999	1	Kenya	1994	1	South Africa	1994	1
Central African Republic	2005	1	Kenya	1997	1	South Africa	1999	0
Chad	1997	0	Kenya	2002	1	South Africa	2004	0
Chad	2001	0	Kenya	2007	1	Tanzania	1995	0
Chad	2002	0	Lesotho	1998	1	Tanzania	2000	0
Chad	2006	0	Lesotho	2002	0	Tanzania	2005	0
Comoros	1992	1	Lesotho	2007	1	Togo	1994	1
Comoros	1993	1	Liberia	1997	0	Togo	1998	1
Comoros	2004	0	Liberia	2005	0	Togo	1999	0
Comoros	2006	0	Madagascar	1993	0	Togo	2002	0
Congo	1992	0	Madagascar	1996	0	Togo	2003	1
Congo	1993	1	Madagascar	1998	0	Togo	2005	1
Congo	2002	1	Madagascar	2001	1	Togo	2007	0
Congo	2007	0	Madagascar	2002	0	Uganda	2006	1
Côte d'Ivoire	1990	1	Madagascar	2006	1	Zambia	1991	0
Côte d'Ivoire	1995	1	Madagascar	2007	1	Zambia	1995	0
Côte d'Ivoire	2000	1	Malawi	1994	1	Zambia	1996	1
Côte d'Ivoire	2001	1	Malawi	1999	1	Zambia	2001	1
Democratic Republic of Congo	2006	1	Malawi	2004	0	Zambia	2006	0
Equatorial Guinea	1993	1	Mali	1992	0	Zambia	2008	0
Equatorial Guinea	1996	1	Mali	1997	1	Zimbabwe	1990	1
Equatorial Guinea	1999	1	Mali	2002	0	Zimbabwe	1995	1
Equatorial Guinea	2002	1	Mali	2007	0	Zimbabwe	1996	0
Equatorial Guinea	2004	1	Mauritania	1992	1	Zimbabwe	2000	1
Equatorial Guinea	2008	1	Mauritania	1996	1	Zimbabwe	2002	1
Ethiopia	1995	1	Mauritania	1997	0	Zimbabwe	2005	1
Ethiopia	2000	1	Mauritania	2001	1	Zimbabwe	2008	1
Ethiopia	2005	1	Mauritania	2003	1			

Multiparty elections years: 1989-2008. Electoral misconduct source: NELDA13, NELDA 15, NELDA 31 and NELDA 33 from NELDA version 3.0, Hyde and Marinov (2012).

Table A-5: Democracy Aid, purposes defined by AidData (2010)

Economic and development policy and planning
Macro-economic, fiscal and monetary policy and planning
Institutional capacity building, Government
Support to other ministries and government departments when sector cannot be specified
Development planning and preparation of structural reforms

Public sector financial management
Improving financial management systems
Tax assessment procedures
Measures against waste, fraud and corruption

Legal and judicial development
Constitutional development, legal drafting
Institutional strengthening of legal and judicial systems
Legal training and education
Legal advice and services
Crime prevention

Government administration
General government services not elsewhere specified
Systems of government
Civil service reform
Government infrastructure

Strengthening civil society
Strengthening civil society, activity unspecified or does not fit elsewhere in group
Community participation and development
Cooperatives
Grassroots organizations
Other participatory planning and decision making procedures and institutions
Elections
Human rights
Free flow of information
Women's equality organizations and institutions

Conflict prevention and resolution, peace and security
Security system management and reform
Other security assistance

Civilian peace-building, conflict prevention and resolution
Support for civilian peace-building activities
Post-conflict peace-building (UN)
UN post-conflict peace-building activities
Reintegration and small arms, light weapons (SALW) control
Reintegration of demobilised military personnel into the economy
Conversion of production facilities from military to civilian outputs
Assistance to control, prevent and/or reduce the proliferation of SALW
Land mine clearance
Explosive mine removal
Child soldiers (Prevention and demobilisation)
Support to prevent the recruitment of child soldiers4
Support to demobilize child soldiers

Table A-4: Incumbent electoral support

Country	Year	Candidate	Party	Incumbent share	Country	Year	Candidate	Party	Incumbent share
Benin	1996	Nicéphore Soglo	PRB	0.513	Mali	1997	Alpha Oumar Konaré	ADEMA	0.959
Benin	2001	Mathieu Kérékou	FARD-Alafia	0.626	Mali	2002	Soumala Cissé	ADEMA	0.426
Benin	2006	Daniel Tawma	FARD-Alafia	0.016	Mali	2007	Amadou Toumani Touré	ADP	0.788
Botswana	1989		BDP	0.707	Mauritania	1997	Ould Taya	PRDS	0.928
Botswana	1994		BDP	0.585	Mauritania	2003	Ould Taya	PRDS	0.782
Botswana	1999		BDP	0.688	Mauritania	2007	Zeine Ould Zeidane	military-backed	0.381
Botswana	2004		BDP	0.665	Mauritius	1991		MSM-MMM	0.952
Burkina Faso	1998	Blaise Compaoré	CDP	0.930	Mauritius	1995		MLP-MMM	0.968
Burkina Faso	2005	Blaise Compaoré	CDP	0.943	Mauritius	2000		MMM-MSM	0.900
Cameroon	1997	Paul Biya	RDPC	0.974	Mauritius	2005		MMM-MSM	0.367
Cameroon	2004	Paul Biya	RDPC	0.803	Mozambique	1999	Joaquim Chissano	FRELIMO	0.523
Cape Verde	2001	Carlos Veiga	MpD	0.496	Mozambique	2004	Armando Guebuza	FRELIMO	0.668
Cape Verde	2006	Pedro Pires	PAICV	0.510	Namibia	1999	Sam Nujoma	SWAPO	0.880
Central African Rep.	1999	Ange Félix Patassé	MLPC	0.727	Namibia	2004	Hifikepunye Pohamba	SWAPO	0.913
Chad	2001	Idriss Déby	MPS	0.794	Niger	2004	Mamadou Tandja	MNSD-Nassara	0.623
Chad	2006	Idriss Déby	MPS	0.810	Nigeria	2003	Olusegun Obasanjo	PDP	0.658
Côte d'Ivoire	1995	Henri Konan Bédié	PDCI-RDA	0.962	Nigeria	2007	Umaru Musa Yar'dua	PDP	0.789
Equatorial Guinea	1996	Obiang Nguema	PDGE	0.993	STP	1996	Miguel Trovoada	ADI	0.511
Equatorial Guinea	2002	Obiang Nguema	PDGE	0.978	STP	2001	Fradique de Menezes	ADI	0.580
Ethiopia	2000		EPRDF	0.929	STP	2006	Patrice Trovoada	ADI	0.391
Ethiopia	2005		EPRDF	0.750	Senegal	1993	Abdou Diouf	PS	0.646
Gabon	1993	Omar Bongo	PDG	0.659	Senegal	2000	Abdou Diouf	PS	0.571
Gabon	1998	Omar Bongo	PDG	0.802	Senegal	2007	Abdoulaye Wade	PDS	0.789
Gabon	2005	Omar Bongo	PDG	0.853	Seychelles	1998	France-Albert René	SPPF	0.773
Gambia	1992	Dawda Jawara	PP	0.799	Seychelles	2001	France-Albert René	SPPF	0.547
Gambia	2001	Yahya Jammeh	APRC	0.619	Seychelles	2006	James Michel	SPPF	0.540
Gambia	2006	Yahya Jammeh	APRC	0.716	Sierra Leone	2007	Solomon Berewa	SLPP	0.463
Ghana	2000	John Atta Mills	NDC	0.481	South Africa	1999		ANC	0.874
Ghana	2004	John Kufuor	NPP	0.460	South Africa	2004		ANC	0.849
Ghana	2008	Nana Akufo-Addo	NPP	0.506	Tanzania	2000	Benjamin Mkapa	CCM	0.815
Guinea	1998	Lansana Conté	PUP	0.695	Tanzania	2005	Jakaya Kikwete	CCM	0.873
Guinea	2003	Lansana Conté	PUP	0.953	Togo	1998	Gnassingbé Eyadéma	RPT	0.604
Guinea-Bissau	1999	Malam Bacai Sanhé	PAIGC	0.376	Togo	2003	Gnassingbé Eyadéma	RPT	0.632
Guinea-Bissau	2005	Kumba Ialé	PRS	0.414	Togo	2005	Faure Gnassingbé	RPT	0.611
Kenya	1997	Daniel arap Moi	KANU	0.563	Zambia	1996	Frederick Chiluba	MMD	0.851
Kenya	2002	Uhuru Kenyatta	KANU	0.335	Zambia	2001	Levy Mwanawasa	MMD	0.517
Kenya	2007	Mwai Kibaki	DP [PNU]	0.513	Zambia	2006	Levy Mwanawasa	MMD	0.594
Madagascar	1996	Albert Zafy	UNDD	0.390	Zambia	2008	Rupiah Banda	MMD	0.513
Madagascar	2001	Didier Ratsiraka	AREMA	0.411	Zimbabwe	1990	Robert Mugabe	ZANU-PF	0.831
Madagascar	2006	Marc Ravalomanana	TIM	0.825	Zimbabwe	1996	Robert Mugabe	ZANU-PF	0.951
Malawi	1999	Bakili Muluzi	UDF	0.537	Zimbabwe	2002	Robert Mugabe	ZANU-PF	0.572
Malawi	2004	Bingu wa Mutharika	UDF	0.560	Zimbabwe	2008	Robert Mugabe	ZANU-PF	0.475

First-round multiparty executive elections with an incumbent candidate or party: 1989-2008. Elections where the second round was never held (Angola 1992) or where the results were anulled (Nigeria 1993) are excluded. Data source: African Elections Database. Vote listed is the incumbent share of the combined vote percentages of the incumbent candidate/party and the leading opposition party/candidate. In some cases (e.g. Senegal 2000) first-round winner loses to second round.

Table A-6: Summary statistics, Table 1

Variable	Mean	Std. Dev.	Min.	Max.	N
Multiparty	0.12	0.34	0	1	370
Economic aid	3.7	1	0.2	6.4	370
Democracy aid	0.9	0.7	0	2.9	370
Log GDP pc	6.9	0.8	4.8	9.6	370
Log population	8.6	1.5	4.2	11.7	370
Civil war	0.2	0.4	0	1	370
Duration	25.2	13.3	1	50	370
Duration^2	809.7	620.8	1	2500	370
Duration^3	28301	27742.7	1	125000	370
Inflation	2.9	1.1	1	7.8	370
WiP	16.2	5.1	4.4	31.8	370

Table A-7: Summary statistics, Table 2

Variable	Mean	Std. Dev.	Min.	Max.	N
Multiparty	0.97	0.16	0	1	583
Economic aid	3.9	0.8	0.5	6.2	583
Democracy aid	1.3	0.7	0	3.2	583
Log GDP pc	7.2	1	5.1	10.2	583
Log population	8.5	1.6	4.3	11.9	583
Civil war	0.1	0.2	0	1	583
Duration	9.2	7.5	0	39	583
Duration^2	141.5	229	0	1521	583
Duration^3	2919.2	7187.2	0	59319	583
$Inflation$	2.2	0.7	1.1	5.1	583
WiP	20.1	3.8	6.1	26.1	583

Table A-8: Summary statistics, Table 3

Variable	Mean	Std. Dev.	Min.	Max.
Electoral misconduct	0.47	0.50	0	1
Economic aid	3.9	0.7	0.3	5.7
Democracy aid	1.2	0.6	0	3
Log GDP pc	7.1	0.9	5.1	9.8
Log population	8.7	1.3	6	11.9
IMF program	0.5	0.4	0	1
First multiparty election	0.1	0.3	0	1
Election monitor	0.9	0.3	0	1
Inflation	2.3	0.8	1.3	5.1
WiP	19.5	4	7	25.7

N	170

Table A-9: Summary statistics, Table 4

Variable	Mean	Std. Dev.	Min.	Max.
Opp vote share	-1	1.3	-4.9	4.1
Economic aid	4	0.8	2.3	5.9
Democracy aid	1.3	0.7	0.1	3
Log GDP pc	7.3	1.1	5.1	10
Log population	8.5	1.7	4.3	11.9
Growth	3.5	7.4	-14.6	38.2
IMF program	0.6	0.5	0	1
Inflation	2.1	0.6	1.1	4.7
WiP	20.4	3.5	10	25.7

N	84

Appendix B: Additional results

Table B-1: Additional tests for multiparty transition

	(1)[a]	(2)[b]	(3)[c]	(4)[d]	(5)[e]	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Econ aid	0.083**	0.117**	0.127*	0.107**	0.083**	0.094**	0.101**	0.103**	0.110**	0.106**	0.104**	0.106**	0.106**	0.107**	0.109**
	(0.04)	(0.03)	(0.07)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Log GDP pc	0.003	0.004	0.045	0.005		0.001	-0.018	0.007	0.004	-0.033**	0.002	-0.001	0.004	-0.019	0.011
	(0.02)	(0.02)	(0.09)	(0.02)		(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)
Log pop	0.031*	0.044**	0.518*	0.043**		0.045**	0.026	0.039**	0.042**	0.004	0.041**	0.044**	0.042**	0.000	0.046**
	(0.02)	(0.02)	(0.30)	(0.02)		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Civil war	-0.112**	-0.108**	-0.046	-0.110**		-0.124**	-0.100**	-0.110**	-0.110**	-0.094**	-0.109**	-0.109**	-0.110**	-0.120**	-0.118**
	(0.04)	(0.04)	(0.06)	(0.04)		(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Ethnic frac.						-0.047									
						(0.08)									
Oil rents pc							0.022**								
							(0.01)								
IMF program								0.027							
								(0.05)							
Growth									-0.000						
									(0.00)						
Trade										0.045**					
										(0.01)					
Intl NGO											0.020				
											(0.09)				
Dom. NGO												0.042			
												(0.09)			
Migrant stock													-0.004		
													(0.01)		
IO member														0.008**	
														(0.00)	
Alliance															0.128**
															(0.03)
(Intercept)	-0.297	-0.513*		-0.469	-0.041	-0.379	-0.183	-0.452	-0.467	-0.181	-0.439	-0.454	-0.451	-0.398	-0.583**
	(0.32)	(0.28)		(0.32)	(0.11)	(0.33)	(0.33)	(0.30)	(0.35)	(0.25)	(0.30)	(0.30)	(0.30)	(0.32)	(0.29)
F-statistic	19.8	15.1	10.0	20.4	14.8	16.6	20.8	26.3	25.6	22.5	18.9	19.5	21.8	23.6	23.7
C (p-value)		0.46		0.36	0.37	0.72	0.26	0.30	0.23	0.35	0.23	0.29	0.21	0.26	0.30
Countries	44	44	44	42	44	39	42	44	44	44	44	44	44	44	44
Observations	370	370	370	362	370	332	362	370	369	370	370	370	370	370	370

* $p<0.10$; ** $p<0.05$. IV models with clustered standard errors in parentheses. Duration polynomials included in all models but not reported. $a \equiv$ only Lewbel instruments (no *Inflation*); $b \equiv$ *Inflation* + Lewbel instruments based on duration polynomials; $c \equiv$ two-way fixed effects (time period and country); $d \equiv$ drop island countries; $e \equiv$ no control variables.

Table B-2: Additional tests for multiparty failure

	(1)[a]	(2)[b]	(3)[c]	(4)[d]	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Democracy aid	0.055†	0.047††	0.059*	0.081*	0.052**	0.053*	0.043+	0.043*	0.044*	0.044*	0.046*	0.043*	0.047*	0.045*
	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Log GDP pc	0.022**	-0.026	0.018*		0.021**	0.019*	0.018*	0.019**	0.034**	0.016*	0.014+	0.019**	0.022**	0.022**
	(0.01)	(0.03)	(0.01)		(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log population	0.015**	0.083	0.020**		0.013*	0.018**	0.013*	0.013*	0.024**	0.021**	0.023**	0.013*	0.017**	0.013*
	(0.01)	(0.18)	(0.01)		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Civil war	-0.052	-0.096	-0.056		-0.053	-0.056	-0.055	-0.055	-0.057	-0.059	-0.058	-0.054	-0.061	-0.052
	(0.06)	(0.07)	(0.06)		(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)
Ethnic frac.					-0.016									
					(0.03)									
Oil rents pc						0.000								
						(0.00)								
IMF program							-0.007							
							(0.02)							
Growth								0.000						
								(0.00)						
Trade									-0.014					
									(0.01)					
Intl. NGO										0.059*				
										(0.04)				
Domestic NGO											0.065*			
											(0.04)			
Migrant stock												0.001		
												(0.01)		
IO member													-0.001	
													(0.00)	
Alliance														-0.034
														(0.03)
(Intercept)	0.600**		0.571**	0.830**	0.628**	0.587**	0.654**	0.645**	0.536**	0.591**	0.585**	0.645**	0.623**	0.627**
	(0.15)		(0.15)	(0.07)	(0.13)	(0.14)	(0.15)	(0.14)	(0.16)	(0.15)	(0.15)	(0.14)	(0.15)	(0.14)
F-stat	21.8	5.9	16.4	12.0	11.8	15.6	24.8	22.4	21.7	25.2	24.8	21.4	19.7	23.7
C-stat (p-value)	0.772		0.897	0.727	0.951	0.596	0.827	0.947	0.843	0.980	0.983	0.917	0.834	0.951
Countries	44	43	41	44	40	42	44	44	44	44	44	44	44	44
Observations	583	582	531	583	526	551	583	583	583	583	583	583	583	583

†† p<0.18; † p<0.11; * p<0.10; ** p<0.05. IV models with clustered standard errors in parentheses. Duration polynomials included in all models but not reported.
$a \equiv$ Inflation + Lewbel instruments based on duration polynomials; $b \equiv$ two-way fixed effects (time period and country); $c \equiv$ drop island countries; $d \equiv$ no control variables.

Table B-3: Linear probability models with unit FE

	(1)	(2)	(3)	(4)	(5)	(6)
Economic aid	0.065	0.123**	0.068*			
	(0.04)	(0.04)	(0.03)			
Democracy aid				0.050**	0.051**	0.039*
				(0.02)	(0.02)	(0.02)
(Intercept)	-0.031	-0.589**	-1.067**	0.800**	0.889**	0.926**
	(0.17)	(0.20)	(0.13)	(0.14)	(0.04)	(0.09)
Country FE	Y	Y	Y	Y	Y	Y
Year FE	Y	N	N	Y	N	N
Time trend	N	Y	N	N	Y	N
Country specific-time trend	N	N	Y	N	N	Y

* $p<0.10$; ** $p<0.05$. OLS with clustered standard errors in parentheses. Civil war control variable not reported.

An alternative approach to constructing causal estimates that does not rely on external instruments to identify the "exogenous" variation in foreign aid is to estimate fixed-effects models. This approach assumes that the unit fixed-effect "controls" for unobserved confounders. In our application, we model unit fixed effects in combination with three methods of accounting for a calendar time trend in the data: year fixed effects; a common time trend; and a country-specific time trend. Because we include subtantial cross-section variation in the unit-fixed effects, we drop GDP per capita and population as control variables, but retain the time-varying measure of civil war. To avoid separation issues that arise in a limited dependent variable model, we employ a linear probability model. In all specification reported in Table B-3, the main reported results from Tables 1 (multiparty transition) and 2 (multiparty failure) remain. We cannot employ this approach for the models of electoral misconduct and opposition electoral strength because there are too few observations per cross-section unit to reliably estimate a fixed effects model.

Table B-4: Additional tests for electoral misconduct

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)[a]	(13)[b]
Democracy aid	-0.174*	-0.154*	-0.170*	-0.179*	-0.215**	-0.149+	-0.200**	-0.167*	-0.172*	-0.138+	-0.186*	-0.218**	-0.146+
	(0.10)	(0.09)	(0.10)	(0.10)	(0.11)	(0.09)	(0.09)	(0.09)	(0.10)	(0.09)	(0.10)	(0.10)	(0.10)
Log GDP pc	-0.055	-0.049	-0.046	-0.055	-0.054	-0.183	-0.111**	0.003	-0.028	-0.062	-0.028	-0.029	-0.052
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.11)	(0.05)	(0.07)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)
Log population	0.019	0.033	0.024	0.023	0.034	-0.100	0.199**	-0.064	0.039	-0.054	0.014	0.011	0.017
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.09)	(0.07)	(0.10)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
IMF program	0.004	-0.022	0.008	0.004	-0.046	0.090	0.019	-0.027	0.044	0.031	-0.005	0.004	-0.003
	(0.12)	(0.12)	(0.13)	(0.13)	(0.12)	(0.10)	(0.12)	(0.12)	(0.13)	(0.10)	(0.12)	(0.13)	(0.13)
First election	0.092	0.073	0.069	0.071	0.034	0.080	0.091	0.035	0.027	0.075	0.065	0.053	0.024
	(0.11)	(0.11)	(0.12)	(0.12)	(0.12)	(0.11)	(0.11)	(0.12)	(0.11)	(0.11)	(0.12)	(0.12)	(0.12)
Monitor	-0.034	-0.036	-0.055	-0.058	-0.019	-0.006	-0.055	-0.081	-0.060	-0.014	-0.048	-0.100	-0.027
	(0.14)	(0.13)	(0.13)	(0.13)	(0.15)	(0.14)	(0.14)	(0.13)	(0.14)	(0.14)	(0.13)	(0.14)	(0.14)
Expect win	0.137												
	(0.09)												
Boycott		0.161											
		(0.10)											
Civil war			-0.007										
			(0.15)										
Growth				0.003									
				(0.01)									
Ethinc frac.					0.141								
					(0.25)								
Trade						0.157*							
						(0.08)							
Intl NGO							2.838**						
							(1.08)						
Domestic NGO								-1.130					
								(0.90)					
IO member									-0.004				
									(0.00)				
Migrant stock										0.138**			
										(0.04)			
Alliance											-0.181		
											(0.13)		
(Intercept)	0.844	0.707	0.824	0.901	0.763	1.707*	-0.486	1.485	0.800	1.186*	0.815	0.931	0.881
	(0.65)	(0.61)	(0.65)	(0.65)	(0.68)	(1.03)	(0.87)	(0.92)	(0.62)	(0.64)	(0.61)	(0.69)	(0.65)
F-statistic	10.4	9.8	10.6	10.3	9.5	10.3	11.5	11.0	11.7	10.7	10.4	9.8	10.5
C (p-value)	0.226	0.286	0.204	0.203	0.285	0.168	0.122	0.285	0.217	0.127	0.210	0.153	0.258
Countries	40	40	40	40	38	40	40	40	40	40	40	39	39
Observations	170	170	170.	170	159	170	170	170	170	170	170	166	158

+ p<0.15 * p<0.10; ** p<0.05. IV models with clustered standard errors in parentheses. $a \equiv$ drop island countries; $b \equiv$ drop civil war years.

Table B-5: Opposition vote share of total vote

	(1)	(2)	(3)[a]	(4)[a]
Economic aid	-0.122		-0.271	
	(0.27)		(0.20)	
Democracy aid		0.684*		0.054
		(0.38)		(0.27)
Log GDP pc	0.001	0.070	-0.098	-0.116
	(0.12)	(0.15)	(0.11)	(0.13)
Log population	-0.080	0.064	-0.179**	-0.076
	(0.16)	(0.12)	(0.08)	(0.08)
Growth	-0.034	-0.043*	-0.012	-0.014
	(0.02)	(0.02)	(0.02)	(0.02)
IMF program	0.037	-0.243	0.341	0.206
	(0.24)	(0.30)	(0.27)	(0.26)
(Intercept)	0.865	-2.030	3.164**	1.334
	(2.69)	(2.27)	(1.30)	(1.67)
F-statistic	22.5	19.0	15.4	12.8
C (p-value)	0.337	0.093	0.207	0.121
Countries	34	34	31	31
Election years	84	84	70	70

* $p < 0.10$; ** $p < 0.05$. IV models with clustered standard errors in parentheses. $a \equiv$ Incumbent vote share $< 90\%$.

Table B-5 reports result from models that use the largest opposition vote total as a *share of the total vote*. Initially, we find that *Economic aid* is associated with lower opposition vote shares, even when dropping the most lopsided elections (column 3). However, this result is much weaker when we include the full set of Lewbel instruments to the increase efficiency of the estimate (the F-statistic increases from 5.7 in column 3 to 15.5 in column 5). The estimate for *Economic aid* in column 5 can be interpreted to mean that a one-standard deviation increase in economic aid is associated with a roughly 5 percent decrease in opposition vote share, which is statistically significant at the 0.17 level in a sample with only 70 observations. Using the full set of Lewbel instruments to increase the efficiency of the estimate for *Democracy aid* (column 6) reduces the estimate for this type of aid to almost zero.

Table B-6: Lagged DV models for opposition vote

Opposition vote	Share of 2-party vote			Share of total vote		
	(1)	(2)	$(3)^a$	(4)	(5)	$(6)^a$
Lag DV	0.458**	0.394**	0.209**	0.356*	0.343**	0.155
	(0.11)	(0.11)	(0.09)	(0.19)	(0.17)	(0.19)
Economic aid	-0.012			0.222		
	(0.27)			(0.41)		
Democracy aid		0.563	0.194		0.476	0.206
		(0.42)	(0.27)		(0.36)	(0.31)
Log GDP pc	-0.109	-0.002	-0.186*	-0.101	0.051	-0.116
	(0.10)	(0.13)	(0.11)	(0.13)	(0.14)	(0.14)
Log population	-0.112	-0.017	-0.113	0.003	0.022	-0.051
	(0.10)	(0.11)	(0.08)	(0.14)	(0.09)	(0.09)
Growth	-0.034**	-0.042**	-0.021	-0.036**	-0.045**	-0.018
	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
IMF program	-0.045	-0.206	-0.115	-0.159	-0.118	-0.025
	(0.41)	(0.36)	(0.29)	(0.50)	(0.35)	(0.33)
(Intercept)	1.708	-0.609	1.954	0.061	-0.939	1.206
	(1.50)	(1.97)	(1.28)	(2.20)	(1.75)	(1.62)
F-statistic	15.0	28.2	22.1	8.4	32.3	25.3
Countries	30	30	26	30	30	26
Elections	50	50	44	50	50	44

* $p<0.10$; ** $p<0.05$. IV models with clustered standard errors in parentheses.
$a \equiv$ Incumbent vote share $<90\%$.

Table B-6 reported results from lagged dependent variable models to account for the history of voting in prior elections. The first three columns use the dependent variable reported in the manuscript: the opposition share of the 2-party vote. The latter three columns use the same depedent variable as in Table B-5: all opposition parties' share of the total vote. We find little evidence that aid influences either of these outcomes in the lagged DV models.
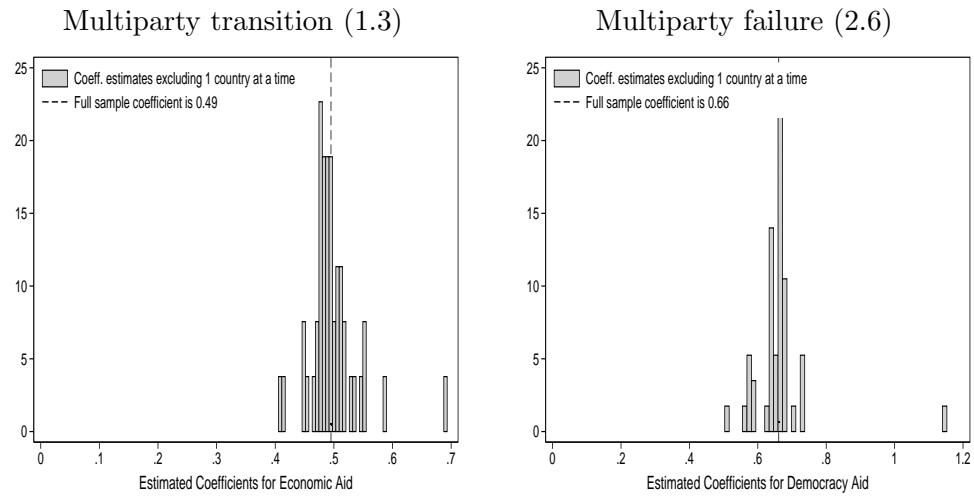
Figure B-1: Distribution of estimates from IV probit models for *Economic aid* (Table 1, column 3) and *Democracy aid* (Table 2, column 6) when excluding one country at a time from the sample.

Appendix C: Identification strategy

Constructing the "outside" instruments

To construct the "outside" excluded instruments, we use a donor characteristic $V_{d,t}^a$, which is one of two donor variables (*Inflation* or *WiP*, denoted by the superscript a), and which varies by donor d and year t. For each recipient i and donor d in each year t, we multiply $V_{d,t}^a$ by the inverted geographic distance in kilometers (denoted by $k_{d,i}$) between donor d and recipient i: $V_{d,t} \times k_{d,i}$. Then we sum these to create the instrument, for each donor characteristic $a \in (Inflation, WiP)$

$$Z_{i,t}^a = \sum_d (V_{d,t}^a \times k_{d,i})$$

For the binary dependent variable model (dropping the superscript a on Z), the resulting specification in an "exactly" identified equation would be:

$$Pr(Y_t = 1|Y_{t-1} = 0) = \widehat{Aid_{i,t}} + X_{i,t} + \zeta_{i,t} + \epsilon_{i,t}^1$$
$$Aid_{i,t} = Z_{i,t} + X_{i,t} + \zeta_{i,t} + \epsilon_{i,t}^2$$

where $Aid_{i,t}$ and $Z_{i,t}$ are three-year lagged moving averages of aid and the instrument, $\widehat{Aid_{i,t}}$ is the predicted value of aid from the first stage, $X_{i,t}$ are control variables, and $\zeta_{i,t}$ are duration polynomials.

Figure C-1 shows the partial correlation between different categories of aid and the two outside excluded instruments (*Inflation* and WiP), in the full sample. From these partial correlation plots, we can see that donor inflation is strongly and positively correlated with economic aid but weakly and negatively correlated with democracy aid. We find just the opposite pattern for women in parliament: *WiP* is strongly and positively correlated with democracy aid but negatively correlated with economic aid. One possible explanation for these patterns could be that during times of inflation, donors are likely to focus the aid portfolio on critical development sectors – at the expense of democracy promotion – while more women in parliament might shift aid portfolios towards more democracy-oriented sectors. Based on these partial correlations and expectations that each outside instrument should be *positively* correlated with foreign aid, we employ *Inflation* as an excluded instrument for economic aid and *WiP* as an excluded instrument for democracy aid.

Finally, we note that there are some potential outliers in the first stage partial correlation between *Inflation* and *Economic aid*, as observed in the lower right corner of the upper left panel of Figure C-1. These three observations come from South Africa prior to the multiparty transition in 1994 (South Africa received less Western foreign aid from fewer OECD donor countries). This helps explains the low partial correlation between *Inflation* and *Economic aid* in the transition sample used in Table 1 of the main text. Dropping South Africa and using *Inflation* in a just identified equation (i.e. without the Lewbel instruments) yields a strong first-stage (partial) correlation between *Inflation* and *Economic aid* (F-statistic= 17.8), with similar second stage results.

Identification with the Lewbel approach

The approach proposed by Lewbel (2012) identifies a two-equation model by using the presence of heteroskedasticity (or a correlation between residuals and some exogenous covariates) in the "first-
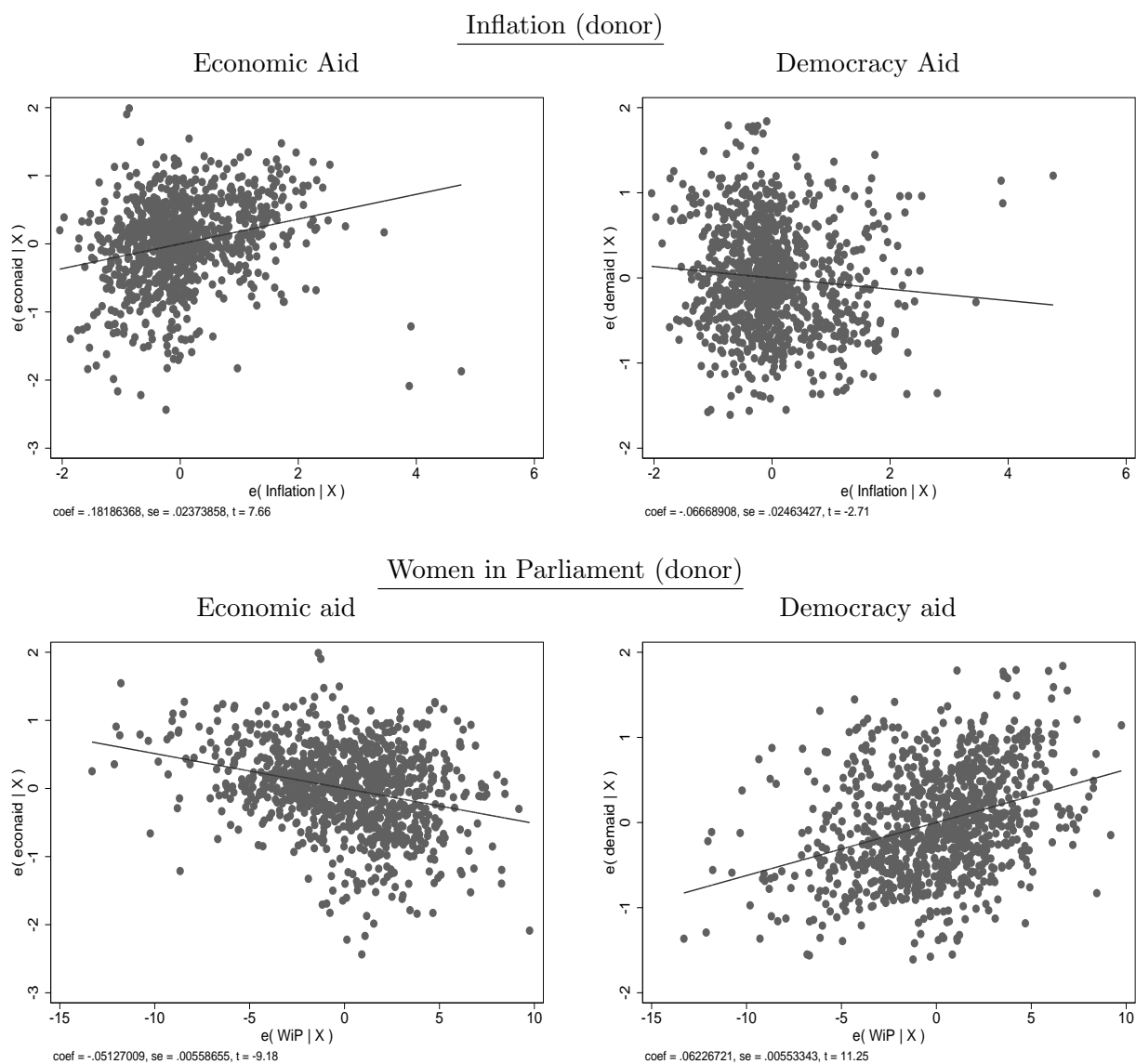
Figure C-1: **Partial correlations for "outside" instruments and foreign aid.**

stage" linear regression. Identification exploits the fact that covariance between the "first-stage" errors and the exogenous variables (X) is not necessarily zero but rather heteroskedastic. Consider the following two-equation model for our application:

$$Y_1 = \beta_1 X + \gamma_1 Aid + \epsilon^1$$
$$Aid = \beta_2 X + \gamma_2 Z + \epsilon^2$$

The main outcome variable of interest (Y_1) is a function of covariates (X) and (Aid), which is endogenous (mismeasured). If Z is correlated with aid but unrelated to Y_1, then Z may used as an "outside" excluded instrument to generate predicted values of Aid. If, however, the $cov(X, \epsilon_2^2) \neq 0$, the "first stage" residuals can be exploited as an "inside" excluded instrument. Because the heteroskedasticity arises from the "first stage", this does not threaten inferences from the estimate of γ_1. In practice, the construction of an "internal," model-based excluded instrument is the following: (1) calculated the residual from a "first-stage" regression ($\epsilon_{i,t}$); (2) calculate the in-sample deviation from the mean for each independent variable in the first stage regression ($X_{i,t,} - \mu_X$); and finally (3) multiply the two: $I = \epsilon_{i,t} \times (X_{i,t,} - \mu_X)$. Note that we can construct one "inside" instrument from each of the independent variables in the first stage equation. Throughout the manuscript, we refer to these "internal" excluded instruments as "Lewbel" instruments.

Since this approach requires the presence of heteroskedastic errors in the first-stage equation, we examine the main model (for each type of aid) from each of the reported tests in the manuscript. As Lewbel (2012) notes, it is easy to examine the scale-related heteroskedasticity with a Breusch-Pagan type test in an OLS context, such as the first-stage equations in the present application. Table C-1 reports these. The null in these tests is that the variance is constant (i.e. there is no heteroskedasticity). Thus a large test statistic (χ^2) indicates a rejection of the null, or the presence of heteroskedasticity; and this means the Lewbel approach is likely to enhance the efficiency of the IV estimator. When the test statistic is small, indicating little heteroskedasticity, including "Lewbel" instruments is unlikely to improve model efficiency.

Table C-1: Heteroskedasticity in the first-stage regressions

Model	Aid type	χ^2	p-value
Table 1, column 2	Economic	86.1	0.000
Table 1, column 5	Democracy	11.2	0.001
Table 2, column 2	Economic	0.4	0.528
Table 2, column 5	Democracy	8.8	0.003
Table 3, column 2	Economic	12.2	0.001
Table 3, column 5	Democracy	1.2	0.281
Table 4, column 2	Economic	0.06	0.804
Table 4, column 5	Democracy	0.56	0.455

The "inside" instruments are only likely to increase the efficiency of the IV estimator in four of the eight main models where the test statistic is large. This same result can be seen by looking at the F-statistic for all excluded instruments for each model and comparing it with the F-statistic for Z (the outside instrument) only. For example, the complete instrument set in column 2 of Table

1 has an F-statistic of 22.6, but the F-statistic for *Inflation* is only 9.5.[1] This indicates that the "inside" instruments substantially help the "explain" economic aid in that sample. This is another way of showing the same information contained in the large χ^2 statistic in the first row of Table C-1. In contrast, the "inside" instruments do little to explain economic aid in the samples used in Tables 2 and 4. Indeed the F-statistics for all excluded instruments in model 2 is not appreciably larger than the F-statistic for Z only. In fact, for the model in Table 2 column 2, the F-statistic for all excluded instruments is lower than the F-statistic for Z only, meaning that adding "inside" instruments hurts efficiency in the IV estimator. Again, this information is also reflected in the very low χ^2 statistic in the third row of Table C-1.

To provide a consistent estimator across all models and specifications in the main text and robustness checks, we include the "inside" (or "Lewbel") instruments in all reported tests. However, Table C-2 reports results from each of the main models where there is very little heteroskedasticity present in the first stage errors (implying that the "Lewbel" instruments are not very useful). These results parallel those reported in the main text, but the IV estimator is more efficient in the two models, reflected in a larger F-statistic than when using the "Lewbel" instruments as well.

Table C-2: Results without Lewbel instruments

Model	Table 2 column 2	Table 3 column 5	Table 4 column 2	Table 4 column 3	Table 4 column 5	Table 5 column 6
Dependent variable	Mulitparty failure	Electoral misconduct		Opposition vote share		
Economic aid	-0.037		-1.657**	-0.848		
	(0.04)		(0.80)	(0.79)		
Democracy aid		-0.303**			2.373**	0.963
		(0.14)			(1.02)	(0.66)
Log GDP pc	0.012	-0.051	0.052	-0.153	0.133	-0.078
	(0.01)	(0.06)	(0.25)	(0.15)	(0.31)	(0.18)
Log population	-0.007	0.005	-0.594*	-0.426*	0.294	0.036
	(0.02)	(0.05)	(0.31)	(0.25)	(0.20)	(0.14)
Civil war	-0.069					
	(0.06)					
IMF program		0.047	0.617	0.492	-0.814	-0.230
		(0.14)	(0.50)	(0.31)	(0.61)	(0.31)
First multiparty election		0.055				
		(0.12)				
Monitor		-0.067				
		(0.14)				
Growth			-0.040	-0.002	-0.058**	-0.021
			(0.04)	(0.02)	(0.03)	(0.02)
(Intercept)	1.054**	1.171	10.140*	7.362	-6.808	-1.362
	(0.29)	(0.74)	(5.51)	(4.52)	(4.27)	(2.98)
Observations	583	170	84	70	84	70
F-stat	56.4	36.2	8.4	5.7	7.4	3.6

* $p<0.10$; ** $p<0.05$. IV linear models with clustered standard errors in parentheses. Duration polynomials not reported in column 1.

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[1]This latter F-statistic reflects the partial correlation between *Inflation* and economic aid, conditional on the covariates *and* the "inside" instruments.

Exploring the exclusion restriction of the "outside" instruments

Overidentification tests

A method for exploring the strength of exclusion assumption is a test of overidentification (Baum, Schaffer and Stillman, 2003). This approach assumes that the "inside" instruments are valid – in the sense of meeting the exclusion restriction – and tests whether the "outside" instruments are orthogonal to the error process in the outcome equation. For each model in the main text, we report the C-statistic, which tests whether "outside" instrument is orthogonal to the errors in the outcome equation, conditional on the "inside" instruments being valid. In all models, the p-value of the C-statistic is greater than the conventional cut-point of 0.10, suggesting that *Inflation* and *WiP* are valid excluded instruments.

Alternative channel of influence

While the C-statistic provides some information on the exclusion restriction for the "outside" instruments, it still assumes some valid excluded instruments to conduct the overidentification tests. Theoretically, there may be alternative factors that are correlated with (even if not causally related to) donor inflation and women in parliament. And these additional, unmodeled (perhaps unobserved) factors may be causally related to the outcomes of interest. To explore this possibility, we examine the partial correlation (i.e. conditional on covariates) between the outside instruments and plausible alternative channels: trade; international and domestic non-governmental organizations; the stock of recipient-country migrants in donor countries; alliances with donors; and the number of international organizations to which the recipient government belongs.

We select these variables because they represent key areas of a country's foreign policy which could potentially be influenced by or correlated with female legislative representation or inflation. These areas include trade, immigration, membership in international organizations, nongovernmental organizations, and security. We do not have theoretical priors about the direction through which an increase in the number of women in parliament might influence these foreign policy areas. We thus focus our attention on the variables' statistical association with our instruments.

Measures and data sources

- *Trade*: The data for trade come from the IMF's DOT database. We focus on trade, measured as a logged sum of exports and imports from the aid-receiving country and donor countries in the sample. It could be that female legislators are systematically more/less likely to push for trade increase with democratizing countries than their male counterpart. This leads to the conjecture that an increase in women in parliament affects democratic change in the recipient country through the trade channel.

- *Immigration*: The data for immigration are from the OECD's International Migration Databse. We utilize a migrant stock measure, which captures the annual stock of people whose country of origin is the aid-receiving African country and who currently reside in an OECD donor country. It could be that female legislators are systematically more/less likely than their male counterparts to lobby for increased immigration flows from countries where democratic change occurs. This could imply that women in parliament affects democratic change in the aid receiving country through the channel of immigration.

- *Membership in international organizations*: We use IO membership data to capture "soft" diplomacy. The data for IO membership come from Pevehouse 2006. We create a count of international organizations to whom aid receipient countries belong as members. If women are more/less likely to promote integration through international institutions, then democratic change in the recipient country could be affected through the channel of diplomacy.

- *Alliances*: We use membership in security alliances to capture the security channel. The data for alliance membership come from the Correlates of War Project. We create a count of alliances that are shared between aid recipient countries and OECD countries in the donor sample. If women are more/less likely than men to promote security relations through military alliances then democratic change in the recipient country could be affected through the security channel

- *IGO and NGO Presence*: We use the logged number of international and local non-governmental organizations in aid-receiving countries as proxy for a donor governments' level of partnership with international and local NGOs. The data are drawn from the Yearbook of International Organizations.

Table 3: Bivariate correlations

Alternative Channels	Inflation	Women in Parliament
Trade	-0.012	0.234
International NGOs	-0.004	-0.225
Domestic NGOs	0.002	-0.203
Migrants	0.068	0.172
Alliance	-0.010	0.004
IO membership	-0.302	0.327

If the outside instruments are correlated with these factors, which might be causally related to the outcome variable, then the exclusion assumption is weaker. The partial correlation plots in Figure C-2 show that three of these variables are (partially) correlated with *Inflation*: trade; migrant stock; and IO membership. For trade and IO membership the correlation with *Inflation* is driven in large part by a handful of outliers: recipient countries with low trade and membership relatively few IOs. None of the proxies for alternative channels, however, are (partially) correlated with women in parliament. Thus there is little empirical connection between the *WiP* and these alternative channels. In Appendix B, we report robustness tests in which the specification adds each of these variables as a control. In each of these tests, the main reported result holds.

Treating the "outside" instruments as "plausibly" exogenous

In this section, we relax the exogeneity assumption to assess how doing so influences the estimate of interest. In the spirit of treating the outside instruments as "plausibly exogenous", we examine whether inference from the reported tests wiil still be valid even if these variables (Z) are weakly related to the outcome. Conley, Hansen and Rossi (2012) introduce an approach that allows researchers to assess how the estimate of interest in an outcome equation changes as an excluded
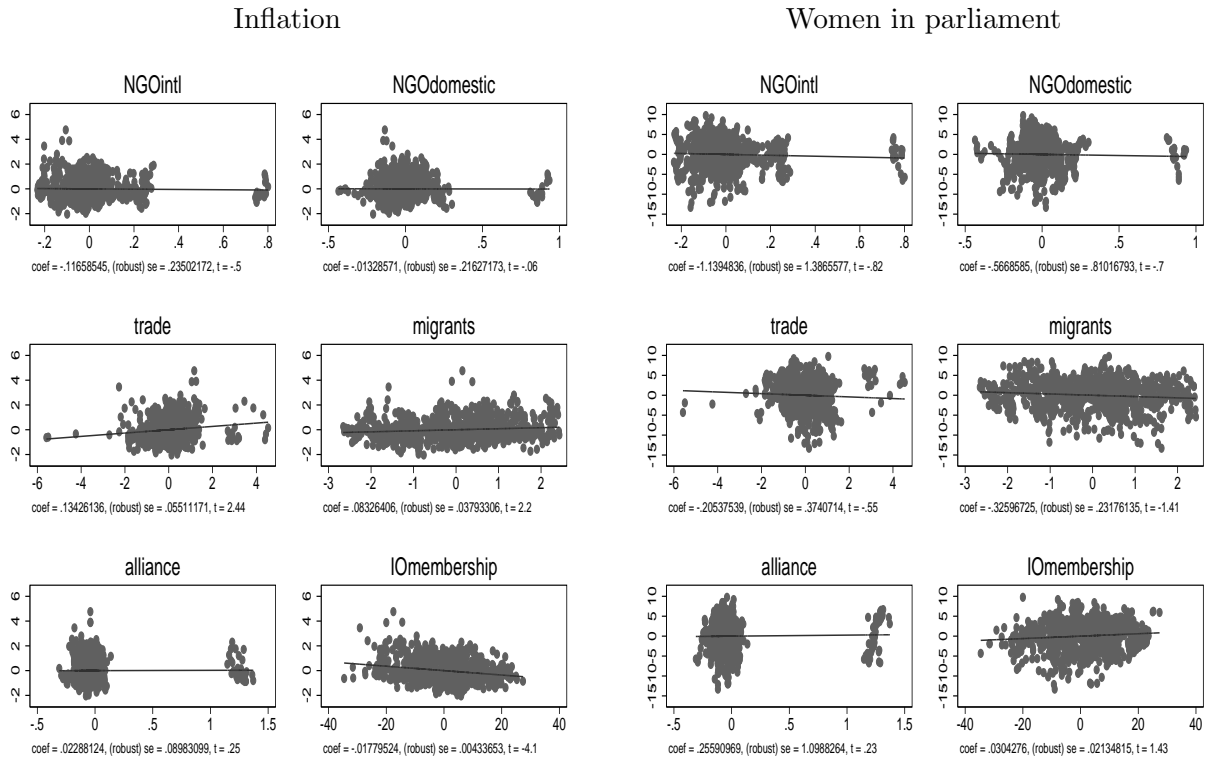
Figure C-2: **Partial correlation between outside instruments and alternative channels.**

instrument becomes more highly correlated with the errors in the outcome equation. Consider the following equation, where Y is the outcome variable, X are (potentially endogenous) covariates and Z is a (potentially) excluded instrument.

$$Y = \beta X + \gamma Z + \epsilon$$

The exogeneity restriction amounts to assuming that $\gamma = 0$. Conley, Hansen and Rossi (2012) examine several approaches to relaxing this assumption by allowing $\gamma \neq 0$ and assessing how do so influences estimates of β.

In our application, we use the unified confidence interval approach, which makes no prior assumption about the distribution of γ but only specifies the possible values for γ. As Conley, Hansen and Rossi (2012) show, this is the most conservative approach insofar as making assumptions about the distribution reduces the coverage area, thus narrowing the errors bands for the estimate of interest.[2] We adopt this approach because we do not have a theoretically informed prior belief about the possible causal relationship between donor inflation (and female parliamentary representation) and political change in African aid recipient countries.[3] We do, however, have an estimate of γ from the reduced-form equation, which we use as a "plausible" upper bound for γ. That is, if the outside instruments are not completely exogenous, then the worst case scenario is that γ is approximately as large as the reduced-form estimate of γ.

Figure C-3 shows how the estimates for *Economic aid* and *Democracy aid* change (for the models reported in Table 1, column 2 and Table 2, column 5, respectively) as we vary γ from zero ("exogenous") up to the reduced form estimate of γ. The differing horizontal scales in each panel of the figure reflect the fact that the estimates of the reduced-form coefficients differ for each model. In the multiparty transition model, the reduced-form estimate of $\gamma_{Inflation}$ is 0.038, while the reduced-form estimate of γ_{WiP} in the multiparty failure model is 0.0037. We might think of these as plausible upper bounds for the extent to which the outside instrument is not exogenous but simply "plausibly exogenous."

When $\gamma_{Inflation}$ equals zero, the left panel depicts the estimate of *Economic aid*, assuming a perfectly exogenous outside instrument. As $\gamma_{Inflation}$ increases, this assumption is less plausible. The left panel shows that even if $\gamma_{Inflation}$ exceeds the reduced-form estimate, then the coverage area for the estimate of *Economic aid* does not contain zero. The right panel of C-3 shows that the exogeneity assumption must be stronger for *WiP* for the estimate of *Democracy aid* to remain valid: the error band contains zero once γ_{WiP} reaches about 0.0006 – or about one-sixth of the size of the reduced-form estimate.

We reiterate that these tests are not tests of instrument exogeneity, but rather simulations of the estimate of interest *if* we assume that the exogeneity assumption is only "plausible" and not "perfect." These tests do not tell us whether we have a "weak" instrument but rather conservatively assess how the causal estimates for foreign aid would change if the "outside" excluded instruments

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[2]Conley, Hansen and Rossi (2012, 260) note that "[i]nterval estimates for β, the treatment parameter of interest, can be obtained conditional on any potential value of γ. Taking the union of these interval estimates across different γ values provides a conservative (in terms of coverage) interval estimate for β. A virtue of this method is that it requires only specification of a range of plausible values for β without requiring complete specification of a prior distribution. Its chief drawback is that the resulting interval estimates may be wide."

[3]We set the priors on the "Lewbel" instruments to zero, thus assuming they are exogenous.
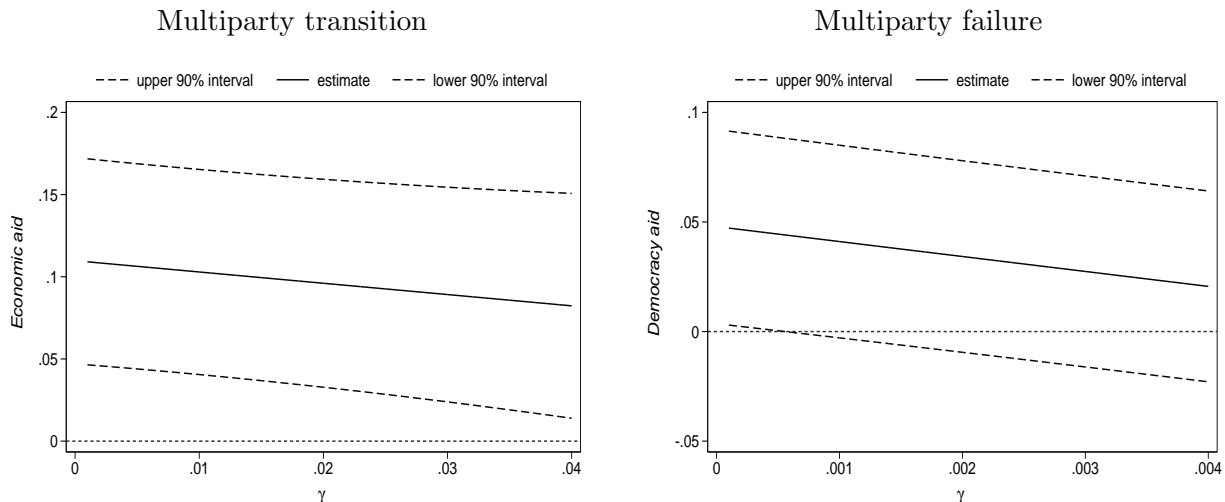
Figure C-3: **Coefficient estimates and 90 percent Unified Confidence Intervals.** Estimates for models reported in Table 1, column 2 (multiparty transition) and Table 2, column 5 (multiparty failure).

are correlated with the errors in the outcome equation.

Finally, we find these tests informative when we consider them alongside the information in Figure C-2, which shows that some plausible alternative channels are correlated with *Inflation* but not with *WiP*. While the union of confidence intervals are not as robust for *WiP*, we believe that this external instrument is more likely to be plausibly exogenous in the first place, as evidenced by the partial correlation plots in the right panel of Figure C-2.

Exploring possible heterogeneous treatment effects

For the IV estimate to recover the average treatment effect of the treated, treatment assignment cannot be heterogeneous. In other words, if the instrument set is only correlated with foreign aid in part of the sample and not another, then the treatment – in this case, the excluded instrument set – is not homogenous and the reported estimates in the outcome equation will not reflect the average treatment effect.

One way to explore for hetereogenous treatment effects is to (randomly) divide the sample in half and estimate the first-stage equation in each sub-sample to see if the partial correlation between the instrument set and the endogenous (foreign aid) variable is present in each. If there is a strong correlation in both sub-samples, then this provides some evidence consistent with the homogenous treatment effect assumption.

For the samples in Tables 1 and 2 of the main text, we randomly split each into two (half) sub-samples. Then we estimate the first stage equation and report the F-statistic for the joint signficance of the excluded instrument set. The top panel of Table C-4 shows the F-stastics from this exercise. For each model, the (random) split-samples each yield F-statistics greater than 10.0. Next, we divide each sample by the median year; and again the F-statistics are large for each sub-sample in each model. The only F-statistic that is less than 10 is the one for the earlier years in the

multiparty transition sample. (Note that the earlier sub-sample contains lagged aid information from the late 1980s.) Finally, when we split each sample geographically (roughly along northern versus southern parts of sub-Saharan Africa), the sub-samples again yield large F-statistics. This exercise suggests that, at least when we divide each of these samples in half, there is evidence of strong treatment assignment in each.

Table C-4: F-statistics from split samples

Model	Aid type	F-stat 1	F-stat 2
		Random sub-samples	
Table 1, column 2	Economic	11.7	15.6
Table 2, column 5	Democracy	15.8	36.1
		Divide by year	
Table 1, column 2	Economic	30.1	9.1
Table 2, column 5	Democracy	12.9	148.9
		Divide by country	
Table 1, column 2	Economic	27.1	17.6
Table 2, column 5	Democracy	35.2	14.6

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