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More donors, more democracy

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A country's democracy improves when it receives democracy aid from a larger number of donor countries. This finding appears surprising from a development perspective, as the presence of a large number of donors, and more generally 'fragmented aid', have been shown to impact negatively on the recipient country. But fragmented aid can be beneficial: Diversity on the donor side provides choice to the local actors involved in the process of democratization. It thus creates a 'marketplace of ideas' which increases the viability of the resulting institutions. In contrast, a highly-concentrated donor community can lead to the imposition of an institutional blueprint, designed in advance and not adapted to the needs of the recipient society. An instrumental variable analysis with panel data for 133 countries from 1994 to 2013, explicit tests of the causal mechanism, and anecdotal evidence from Ghana provide strong support for the benefits of diverse democracy aid.

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Online appendices:

- A: Data (12 pages)
- B: The instrumental variable approach (24 pages)
- C: Robustness checks (52 pages)
- D: Tracing the causal mechanism in Ghana (10 pages)

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Foreign aid harms democracy – this claim builds on political economy arguments and has found widespread support in the empirical literature (Ahmed 2012; Bueno de Mesquita and Smith 2009). However, aid that focuses on government and civil society issues, commonly referred to as 'democracy aid', has been shown to correlate with improvements in democracy (Dietrich and Wright 2015; Scott and Steele 2011). Democracy aid made up 15 percent of all official aid flows in 2013, or 18 billion USD. Despite the large sums being spent, the literature is undetermined on why democracy aid seems to work against the odds. Democracy donors themselves are similarly adrift:

> *Although increasing numbers of governments and people around the world now endorse the norm of democracy promotion, even democratic states disagree about how to do it. […] Should external actors press first for elections or for the adoption of a constitution? Should they push for presidential or parliamentary systems, federal or unitary states, proportional representation in parliament or majoritarian electoral systems? Should outsiders work with the state or society to press for change? No blueprint is universally recognized as the most effective way to promote democracy, and in fact, many even reject the idea that there can be a blueprint* (McFaul 2004: 157).

I make a new suggestion on why democracy aid works: it is the very absence of a blueprint that makes it effective. By virtue of diversity, fragmented aid allows different interests and actors in the recipient country to find support. As there is no blueprint for democracy promotion, there is no blueprint for democracy. Supporting different interests from the outside increases the chances that viable democratic institutions are established, and that those cleavages are politicized that best represent the interests of the relevant social groups. The recommendation to provide fragmented aid contradicts the current development discourse. Most famously, *The Paris Declaration on Aid Effectiveness* called for an end to this practice and for better coordinating aid. But a coordinated approach to democracy promotion, I argue, would have two fundamental flaws. On the conceptual level, as well-planned as an intervention could be from a technical point of view, it would create a designed, not a grown democracy. On the political level, any coordinated approach would ultimately constitute a compromise between donors rather than an intervention primarily adapted to local requirements.

The most explicit support towards democratizing recipient countries occurs through democracy aid. Democracy aid is understood as aid explicitly aimed at improving political institutions and supporting different types of interest groups (Carothers 2009: 5). It deals with polity issues. I refer to all other aid collectively as 'economic aid' (following Dietrich and Wright 2015), and to the sum of both as 'general aid'. I study the effects of democracy aid proliferation – i.e., the presence of a large number of democracy donors – on democracy in recipient countries. In a sample of 133 countries over the period 1994 to 2013, I find strong support for my hypothesis with both fixed effects and instrumen-

tal variable specifications. I explicitly examine the causal mechanism with placebo tests and detailed data on democratic diversity in the recipient country. Anecdotal evidence from Ghana suggests that the proposed mechanism is plausible on closer inspection. My findings contribute to various strands of literature. They contribute to the growing literature that disaggregates the effects of aid on democratization (e.g., Bermeo 2016; Birchler, Limpach, and Michaelowa 2016). They constitute the first assessment of the effects of aid fragmentation on democracy, and add to recent findings on benefits of fragmented aid (e.g., Han and Koenig-Archibugi 2015). And finally, they relate to the debate on how much competition young democracies tolerate – and require (Bunce 2003; Kapstein and Converse 2008; Wright 2008).

To set the stage for my argument, I first discuss the state of the art on the effects of foreign aid on democracy. Then I suggest a pathway on how fragmented aid alters these effects: Democracy donor proliferation provides a 'market for ideas' and thus improves democracy. The literature on early stages of democratization provides a foundation for this mechanism by showing that the establishment and consolidation of democratic institutions is an iterative and uncertain process which benefits from diversity at early stages.

The ambiguous link between aid and democratization

One major argument linking general aid and democracy asserts that the former harms the latter because it is fungible. Fungibility refers to the ability of recipient governments to respond to aid inflows by reducing their own contribution in the respective sector, thus freeing up resources for discretionary use (Feyzioglu, Swaroop, and Zhu 1998). In most theories of political power, 'discretionary use' is equivalent to employing these additional funds for whatever helps extending the incumbent's stay in power (Bueno de Mesquita and Smith 2009). Cruz and Schneider (2017) show that even if autocratic incumbents cannot make use of aid flows by means of fungibility, they can claim credit for their beneficial effects. Thus, the inflow of foreign aid makes democratization of authoritarian states less likely and undermines existing democratic institutions. This rationale has found widespread support in the empirical literature (e.g., Ahmed 2012; Kono and Montinola 2009), although Bermeo (2016) recently presented evidence that these deteriorating effects do not apply anymore after the end of the cold war.

Another channel by which aid may weaken democracy is the disruption of the accountability chain between government and citizens (Winters 2010: 223). When a government receives aid, it is subject to expectations by the donor, be they explicit or implicit. Pledges towards the donors compromise the ability of a government to serve its people. When a donor's expectations are articulated and the future disbursement of aid

depends on the compliance of the recipient, the mechanism is referred to as 'conditionality'. Conditionality, however, was originally supposed to have the positive connotation of nudging recipients towards 'good governance'. In practice, conditionality has contributed to spreading the norm of democracy across the globe after the end of the cold war (Crawford 2001). But the norm has impacted the labeling of regimes, and not necessarily actual practice, making many nominally democratic regimes today disfunctional (Brown 2005).

Technical aid explicitly aimed at fostering democracy, or democracy aid, has also many critics. They argue that democratization is an overwhelmingly domestic process that can hardly be fostered intentionally from the outside (O'Donnell and Schmitter 1986: 18). And if international forces played a role, one might expect large economic factors such as trade to dwarf aid efforts. When particular attempts to promote democracy with aid failed despite a strong (including military) engagement, it was often blamed on trying to impose an institutional blueprint – modeled after the western example – onto a society with different expectations and structure (Hill 2011).

Despite these pessimistic expectations, empirical studies have linked democracy aid to positive trends in various aspects of democratic governance (e.g., Finkel, Pérez-Liñán, and Seligson 2007; Scott and Steele 2011). This may be explained by a reduced fungibility of democracy aid, and by the effectiveness of certain aspects of technical democracy aid. Reduced fungibility is mainly due to the fact that a large part of democracy aid does not go to the government, but to non-state actors (Youngs 2003). Some funds are channeled directly from the donor to non-governmental organizations (NGOs). Bypassing recipient governments in general aid delivery is often justified with bad governance reducing efficiency (Dietrich 2013). In the realm of democracy aid, bypassing fulfills the additional task of building opposition capacity. And official statistics underestimate the level to which this tool is employed: Many funds are officially recorded as going to government ministries, but they are actually spent by NGOs (Crawford 2001). Technical democracy aid may be effective because it immediately addresses how governments works. For example, external involvement has been shown to make elections more credible (Hyde 2007). External advisers can also help to implement certain rules or practices that have been shown to improve democratic stability, such as proportional electoral systems, term limits, or improvements to the rule of law (Magen and Morlino 2009). These measures can help break the 'trap of pessimistic expectations' that would otherwise make people decide that all politicians are corrupt (Svolik 2013: 687). The accountability chain is thus actually strengthened, not weakened, by democracy aid: 'assistance to new democracies that empowers the civil society and independent media may help voters to evaluate candidates' performance with lesser effort and greater precision' (Svolik 2013: 698). Gottlieb (2016), for example, shows with a field experiment

in Mali that educating the population about democracy increases the accountability of governments. It is unclear, however, if all or any particular traits of democracy aid are sufficient to overcome the abundance of obstacles in promoting democracy. I make the novel claim that one of the driving forces behind the effectiveness of democracy aid is fragmentation. But before expounding the core of my argument, I present some properties of democracy processes relevant to my explanation.

The uncertainty of democratization processes

Most countries in the world today claim to be democracies. But many democracies are deficient, or simply a facade to an autocratic regime. And whenever societies attempt to become more democratic, they face a plethora of challenges (Casper and Taylor 1996; O'Donnell and Schmitter 1986). Democratization is an inherently uncertain process. At any stage – be it the liberalization of autocratic rule, the establishment of democratic institutions or their consolidation – the danger of an autocratic backlash looms. Until democratization succeeds, nobody can reliably predict the institutional setup that turns out to be stable. The uncertainty is due to the iterative process of power struggles among the participating actors. Since the end of the cold war, many transitions occurred as 'negotiated revolutions', and not as quick, clean and bloody breaks (Lawson 2005). During the negotiations, many options are tested, many fail, and only the subset that works is finally selected. It is a process of 'trial and error', a method that has proven successful in many socio-economic fields (Harford 2011). Out of many details on the way towards establishing a democracy, few work in a particular instance, and finding these can require several attempts.[1]

What increases uncertainty is a lack of control by political leaders. Bunce (2003) argues that the ability of leaders to steer transitions has often been exaggerated. Leaders are frequently doomed to reacting to developments rather than being able to shape them (Bellin 2000: 204). In past attempts to support democratization from the outside, excessive trust in the ability of leaders to manage democratization led donors to

[1] One could ask why trial-and-error approaches – and thus diversity – do not improve the effects of economic aid as well. The reason is, I argue, that economic aid in developing countries focuses mostly on basic, universal and tested interventions, such as maternal health, immunization, primary education, roads and water. Economic policies can also be highly complex and unpredictable, such as designing the global financial architecture. But most issues in developing countries tend to have rather simple solutions that tend to fail due to implementation and the political economy, not due to design. Democracy, however, is a high-tech product that requires innovation. And trial and error is necessary where one needs to solve complex tasks with outcomes that are nearly impossible to simulate in advance. Furthermore, advances in governance are set to generate better policies and stability in the future, whereas advances in economic policies – even if created through trial-and-error processes – may quickly be lost when faced with political instability. Thus, democracy aid benefits more from market-like approaches than other aid sectors, where coherence is comparatively more important than innovation.

bond with the in-group of a newly formed government, out of concern to compromise stability with a more open process (e.g., Rakner 2012: 17). This concern was derived from the notion that transitions should be negotiated in small circles of incumbent and opposition elites (Rustow 1970). Pacting among elites and demobilizing the public were considered useful because they help to reduce the number items on the bargaining table and the threat of polarization (Linz 1978). Recent empirical research finds that muted competition does not lead to better outcomes: Demonstrations, a wider spectrum of political parties, and other forms of participation make young democracies more resilient (Brownlee 2009; Bunce 2003; Kapstein and Converse 2008; Wright 2008). Diversity is particularly crucial in poor, young democracies. During the first couple of elections, party systems consolidate, defining those social cleavages that are politicized (Zielinski 2002). Only interests that are supported financially have good chances of finding a place in the system. In the words of Dahl (1971: 49), scattered resources result in dispersing the 'nonviolent means of coercion, persuasion and inducement'. Where a diverse set of domestic philanthropic sponsors is missing, external sources such as diasporas may take on the crucial role of funding pluralistic oppositions (Arriola 2013: 249). Not only does more diverse funding create more valid institutions. It also decreases the desire of out-groups to challenge the order that is being created, making them feel less excluded (Wright 2008). For example, Conroy-Krutz and Moehler (2015) show with a field experiment that young democracies can benefit from pluralism: Partisan media in Ghana did not lead to increased polarization, but it moderated political competition, in line with advocates of deliberation such as John Stuart Mill and Jürgen Habermas.

But these arguments do not apply exclusively to young democracies. In older regimes, external funding and inspiration can help overlooked groups to win the 'war of attrition' necessary to invade a frozen party system (Zielinski 2002: 202). Increasing diversity in authoritarian regimes can also be beneficial. Competitive authoritarian regimes are more likely to be followed by democracy than other authoritarian regimes (Brownlee 2009). On the contrary, pressure to transition to multiparty systems can hinder sustainable democratization when opposition parties are too weak to compete with the old elites (Brown 2005: 190). Democracies 'born with deeply oligarchic and exclusivist traits' are more vulnerable to failure (Slater 2013: 761–2). Diversified financial and ideational sources could help identify and strengthen those actors and policies that can compete, bringing the system a step closer to implementing true multiparty democracy.

Donor proliferation creates a marketplace for ideas

In the absence of diversified domestic wealth, fragmented aid can be the crucial ingredient to making emerging democracies sustainable. To development scholars and practi-

tioners alike, this claim may come as a surprise. Most would endorse the statement that more fragmented aid is less effective, and potentially harmful (Acharya, Fuzzo de Lima, and Moore 2006: 14). In particular, fragmentation has been shown to impact negatively on governance in the recipient country. The presence of a large number of donors puts high transaction costs on the recipient government, forcing officials to cater divergent donor interests at the same time (Morss 1984). The resulting conundrum gives corrupt officials the possibility of directing their individual attention selectively towards projects. This increases their leverage for gathering personal rewards and reduces bureaucratic quality. Bureaucratic ineffectiveness then leads to reduced economic growth (Djankov, Montalvo, and Reynal-Querol 2009). Donors also compete with the government for the best-qualified personnel. By hiring away the most capable managers, they reduce the administrative capacity in the recipient country (Knack and Rahman 2007).

The positive effects of fragmented aid on democratization stem from a mechanisms that alters the aid-and-democratization link: Fragmented aid usually comes with a presence of many donors that provide different ideas on democracy. The introductory quote by McFaul mentions how difficult it is for donors to do the 'right thing' in democracy promotion. Not knowing what is best, offering an array of options is more likely to provide useful input for the recipient society. Only recently have authors begun to point out advantages of aid fragmentation. Where more donors provide aid, aid shocks are less likely, which reduces the probability of conflict onset (Nielsen et al. 2011). My argument does not primarily build on the continuity of financial flows, but on the diversity of ideas on how democracy should work. Such ideas are part of many aid projects, as donors aim at including 'participatory development' components in all suitable aid sectors. But democracy aid is the natural suspect to transmit the bulk of ideas: reforming the polity is its main objective.

All donors offer a slightly different aid program with different political orientations, as has been shown particularly with regard to the European Union and the United States (Magen, Risse, and McFaul 2009). Some donors focus on the institutions of the state, some on non-governmental organizations and some on free press. These differences can be explained by donor political economies that favor, e.g., market-based or state-based approaches (Dietrich 2016). Online appendix B provides a detailed overview of donor portfolios in the area of 'strengthening civil society', a core component of democracy aid. It shows that donors actually emphasize support to different sectors. For example, several Nordic donors focus on human rights, Japan focuses on community participation, and Spain focuses on women organizations. Where more donors are present, they are more likely to support diverse stakeholders – both within and beyond government – that are able to hold the executive to account (cp. Rakner 2012). Figure 1 shows that there are no crowding-out effects either: donors providing aid to

recipient countries where many democracy donors are active do not have less projects in these countries, but more. Only in such a diverse environment can the multitude of non-governmental actors involved in a democratization process find the best-fitting support. Even with the best of intentions, a single donor could not cater as many political hues as a group of donors can. And while all donors share a basic set of western values, they differ sufficiently in how they intend to preserve these values, making each donor a potentially crucial contribution. Crawford (2001: 35) notes:

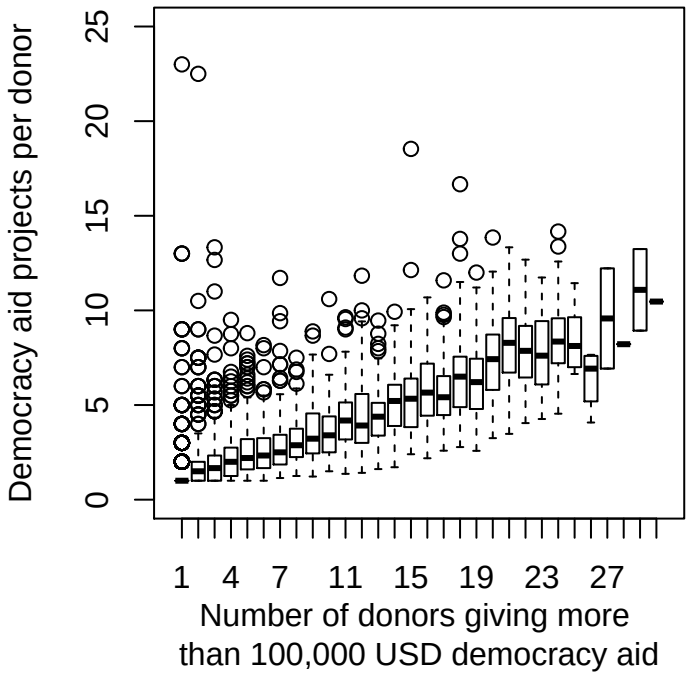

Figure 1: Number of democracy aid projects per donor in a recipient country by the number of democracy donors

> *The objection to the promotion of a western model […] does overlook the variety of institutional forms that exist within western nations themselves. Nevertheless, it remains valid to question the extent to which donors will encourage local participation in determining the most appropriate form.*

I argue that donor proliferation can encourage local participation much better than donor concentration could. The domestic 'marketplace of ideas' (Snyder and Ballentine 1996) is balanced by external intervention. One may certainly doubt that aid functions as a perfect market, efficiently aligning supply and demand. Easterly (2002) argues that foreign aid rather resembles a cartel. While this may be true on the global level, one does find variation on the project level: Competition between managers whose pro-

fessional careers depend on making their project visible creates market-like structures (Harford, Hadjimichael, and Klein 2004). But how do these small-scale effects aggregate to impact the overall quality of democracy in the recipient country? Decentralized approaches at problem-solving have been shown to be very effective in institution building (Ostrom 1990). As practicing participation fosters democratic attitudes (Quintelier and Van Deth 2014), foreign-funded diversity is likely to facilitate the establishment of democratic institutions on the national level, even if donors do not directly administer their establishment.

An additional benefit of a diverse aid landscape is a decreased danger of being left completely without aid when one donor pulls out unexpectedly, an event which has been quite frequent in the past (van de Walle 2012; Youngs 2003). If a particular donor pulls out, the remaining donors may not fully compensate this loss by supporting the very same actors; but at least some agents of change will continue to receive assistance. A similar logic applies to sudden changes in the political orientation of donors, which affect democracy assistance as well (Weissenbach 2015: 347). Only diversity can guarantee the adequate coverage of different ideological positions.

I contend that, beyond these positive effects of donor proliferation, the negative effects purported for economic aid are severely mitigated for democracy aid. The number of local partners – such as NGOs and political parties – exceeds the number of donors even in the most fragmented settings, inverting the unfavorable relationship of one host versus dozens of donors, as encountered in economic aid.[2] Supporting non-state actors is, of course, not a silver bullet. A country's NGO scene may be biased, for example by representing mostly urban interests, such as in Mali (van de Walle 2012: 11). Critics say that funding NGOs from the outside leads to clientelistic relationships, causing NGOs to search short-term benefits instead of pursuing their original goals (Henderson 2002). But advocates of democracy promotion insist that supporting NGO activity leads to positive political change (Diamond 1995). This positive effect may even be achieved by simply providing the necessary infrastructure, independent of the goals pursued by the agencies (Brown, Brown, and Desposato 2008). Jamal (2012) suggests that the positive effect only materializes when preceding political settlements were sufficiently inclusive. An exclusive NGO setting with little diversity exacerbates problems. This is where the supply of diversity from the outside can make a palpable contribution. The 'diversity hypothesis' that I test below thus reads:

The presence of a large number of democracy donors has a positive effect on democracy in the recipient country.

[2]In economic aid, the national government is of course not the only partner, but usually one main partner, or at least an involved gatekeeper.

Research design

To assess my hypotheses on fragmented aid and democracy, I apply fixed-effects and instrumental variable regression models to panel data for 133 countries over the period 1994 to 2013. If meaningful effects of fragmented aid exist, they should be observable on the macro level and in aggregate democracy indices (cp. Knack and Rahman 2007: 193). But I will also examine effects on sub-components of democracy that are closer to the suggested causal mechanism. The cross-country evidence is supplemented by anecdotal evidence from Ghana with the aim of verifying the plausibility of the causal mechanism.

The explained variable is democracy as measured by the V-Dem project in their *polyarchy* score (Coppedge, Gerring, Lindberg, Skaaning, Teorell, Altman, Bernhard, et al. 2017). The main explanatory variable is *democracy donor proliferation*, i.e., the number of donors reporting democracy aid in a given year to a given recipient. It is calculated from project-level data on aid commitments provided by the AidData Research Release 3.0 (Tierney et al. 2011). 'Commitments' are pledges made by the donors, whereas actual flows are known as 'disbursements'. AidData commitment data, however, is much more comprehensive than disbursement data provided by the Organization for Economic Co-operation and Development (OECD), in particular with regard to the sector coding. In line with previous research (e.g., Dietrich and Wright 2015), I consider the aid sector *government and civil society* to reflect democracy aid flows.[3] All aid variables employed in this study exclude debt relief, emergency aid and amounts spent in the donor country, as these contributions are less likely to transmit ideas or affect conditionality negotiations. The universe of cases analyzed is all countries with more than half a million inhabitants defined by the OECD to be eligible for receiving Official Development Aid (ODA). Democracy aid has been reported since 1973, but a substantial part of the strong time trend in the data is likely to be due to underreporting in early years. Records become more reliable in the mid-1990's. My sample ranges from 1994 to 2013 and makes use of the 20 most recent years of available data. The sample is placed well inside the post-cold-war period, creating a fairly constant geopolitical background for the analysis.

Most democracy assistance studies employ yearly data. This is problematic, as fluctuations are often due to technical or bureaucratic considerations, and not due to changes in actual implementation practice. While these issues exist for disbursement data, the temporal discrepancy between reporting and implementation grows even further for commitment data. Clemens et al. (2012: 594) discuss the appropriate length of tem-

[3]Online appendix C gives results for narrower measures of democracy aid (and a range of other robustness checks) and confirms my findings.

poral units of observation for the related field of aid-and-growth studies. Short time windows may not leave enough time for effects to materialize in common econometric settings with one-period lags, but they allow for using unit-fixed effects. Long time windows may blur short- and mid-term effects, but they also reduce endogeneity concerns even further and show whether effects persist (cp. Kersting and Kilby 2014: 133). I choose three-year averages as a compromise between analytic power and data reliability for my main specifications. A majority of three-year periods will also comprise national or sub-national elections, thus providing intervals between which one can plausibly expect to observe changes in democratic quality.

The 'naive' specifications I employ are ordinary least squares (OLS) models with period and country fixed effects and lagged explanatory variables. Clemens et al. (2012) argue that this approach takes care of most endogeneity issues, and many authors follow their recommendation (e.g., Jones and Tarp 2016). Country fixed effects remove alternative explanations that do not vary substantially over time. Year fixed effects capture global democratization trends. Causality could of course still run in the opposite direction: Donors might prefer countries that are improving their democracy scores. But as I lag my explanatory variables by one three-year period and use both country and year fixed effects, donors would have to be good in predicting trends in democratization for this effect to contaminate my estimates. I consider this unlikely, given the bad track-record of predicting events such as the Arab spring (cp. Bermeo 2016 online appendix: 2–3). A common cause for both increased donor interest in a recipient country and improvement in the latter's democracy would be another source of bias. Political stability might be conducive to both, at certain stages of liberalization, so I control for civil conflict (Gleditsch et al. 2002). In order to avoid a garbage can model, I restrict my model to two additional variables on the right-hand side: population and income per capita (Feenstra, Inklaar, and Timmer 2015). The country-fixed effects account for a large range of competing explanations of democracy with little temporal variation, such as ethnic diversity.

But given a plethora of alternative common causes, an excludable instrumental variable (IV) can bring more certainty with regard to the direction of causality. In my IV approach, I estimate the level of democracy Y in recipient country i at time t with the following equation:

$$Y_{it} = \beta_1 \hat{D}_{i,t-1} + \beta_2 \mathbf{X}_{i,t-1} + \alpha_i + \tau_t + \vartheta_{it},$$

where $\hat{D}$ is the estimated number of donors, $\mathbf{X}$ a vector of control variables, α country fixed effects, τ period fixed effects and ϑ the error term. This equation constitutes the second stage of a two-stage least-squares (2SLS) model. D is estimated in a first stage

using the instrument Z and the same set of control variables and country and period fixed effects, as well as an error term κ:

$$D_{i,t-1} = \delta_1 Z_{i,t-1} + \delta_2 \mathbf{X}_{i,t-1} + \alpha_i + \tau_t + \kappa_{ijt}.$$

Z is calculated on the dyadic level, with donor-recipient years as unit of observation, and aggregated by summing over all donors for each recipient year:

$$Z_{it} = \sum_{j=1}^{N} (Q_{jt} * K_{ij}),$$

where Q is the donor-level instrument varying by donor j and time t. K is the recipient-level instrument, varying by donor j and recipient i. With this 'interaction approach', I follow recent suggestions to interact exogenous variables on the donor side with endogenous recipient properties in order to increase cross-sectional variance (Ahmed and Werker 2015; Dietrich and Wright 2015; Dreher and Langlotz 2015; Galiani et al. 2017; Nunn and Qian 2014).[4]

As instrument Q for fragmented democracy aid, I employ the share of female legislators (or 'women in parliament') in the donor country, as suggested by Dietrich and Wright (2015: 223). They argue, building on Breuning (2001), that women are more likely to support social-equity issues, and they show empirically that the share of female legislators is a good predictor of democracy aid (Dietrich and Wright 2015: online appendix C). I use the instrument to predict whether a specific donor provides democracy aid at all. This introduces the additional assumption that female legislators are not only able to realize higher democracy aid budgets, but to initiate (or revive) democracy aid programmes. I consider this assumption defensible as it follows the same logic as the original rationale. In online appendix B, I show that the share of female legislators is indeed even better suited to predict this binary property than to predict democracy aid amounts.[5] The instrument is plausibly excludable: There is no reason to believe that the gender composition of a donor country's parliament should affect democracy in a recipient country directly. Dietrich and Wright (2015: 224) also show that alternative paths from women in parliament to democracy are unlikely, given low bivariate correlations with trade, immigration, membership in international organizations, alliances and NGO presence.

Since I want to compare the effects of democracy donor proliferation to donor prolif-

[4] Instead of simply aggregating the dyadic instrument onto the monadic level, Dreher and Langlotz (2015) employ a 'zero-stage' to explicitly model the expected amount of aid. I adapt their approach to predict the number of donors in online appendix B. The results of this zero-stage approach differ only marginally from the interaction approach.

[5] The appendix also provides plots that show the temporal variation in the instrument.

eration in other aid sectors, I also need instruments for general and economic aid. For these sectors, I employ legislative fractionalization – defined as 'the probability that two deputies picked at random from the legislature will be of different parties' (Beck et al. 2001: 174) – as instrument Q on the donor side. Ahmed (2016: 191–193) suggests this instrument and shows that the U.S. House of Representatives passes larger foreign aid budgets when it is fractionalized. Expanding the application of the instrument to 28 donor countries, Dreher and Langlotz (2015) show that legislative fractionalization leads to higher government expenditures, and thus presumably to higher aid budgets.

On the recipient side, K is the share of years in which a recipient receives aid in the respective sector. Nunn and Qian (2014) suggest this 'propensity' instrument, which is also used by Dreher and Langlotz (2015) and Ahmed (2016). It is very powerful in predicting whether a donor provides aid to a particular recipient and introduces variation at the recipient level that allows for using country fixed effects. I also test my hypotheses with an alternative recipient-level instrument suggested by Dietrich and Wright (2015: 224), who employ the inverted share of the distance between donor and recipient for K. I modify the approach by only inverting the distance, but dispensing from standardizing it to relative distances, as I expect the probability of a particular donor to cater a recipient country to be a function of absolute distance. This conforms to the specifications employed in most aid allocation studies.

Note that K_{ij} is time-invariant, whereas Q_{jt} is recipient-invariant. Combined with country and period fixed effects, there is no need to include the constitutive terms of the interaction in the first stage. More importantly, the fixed effects deal with the exogenous component of the interaction: Neither the propensity to receive aid not the distance to a donor country is independent from the level of democracy in the recipient country. But combining the interaction term with fixed effects reduces the demands on the identifying assumption: I must only assume that the level of democracy in countries with different propensities to receive democracy aid will be equally affected by changes in the share of women in the donor parliament if they have the same number of democracy donors, conditional on the other control variables (Ahmed 2016: 196; cp. Dreher and Langlotz 2015: 8–9).[6] The endogenous components cannot exert an independent effect on democracy.

To measure the share of female legislators, I use data from Paxton, Hughes, and Green (2006), provided and updated by the V-Dem project (Coppedge, Gerring, Lindberg, Skaaning, Teorell, Altman, Bernhard, et al. 2017). Data on legislative fractionalization comes from the Database of Political Institutions (Beck et al. 2001). Distance data is calculated from the cshapes package (Weidmann, Kuse, and Gleditsch 2010). Online

[6] See Nizalova and Murtazashvili (2016) for a formal proof for the case of combining exogenous treatments with endogenous factors.

appendix A provides summary statistics and more detailed information on all data employed. Online appendix B provides more detailed information on the instrumental variable approach.

Empirical analysis

Panel A in Table 1 provides OLS estimates of the correlations of the number of democracy donors with democracy levels. All specifications include country and year fixed effects, but they differ with regard to the aid sector of interest and the control variables that are included.[7] Standard errors are clustered by country and shown in terms of 95-percent confidence intervals. Model 1 demonstrates that democracy donor proliferation has a positive and significant correlation with democracy scores in the following three-year period. This relationship holds when aid amounts and other controls are introduced (model 2; model 3 is equivalent, it only differs in the IV specification). The coefficients for aid amounts are also reported in the table. They do not reach significance, albeit in a model that does not include the number of democracy donors, they do (see online appendix C). One could interpret this as tentative support for the claim that it is the number of democracy donors that makes democracy aid effective, not the amount of financial commitments. In substantive terms, an additional donor is associated with an increase in democracy of about 1 on the 0 to 100 scale, whereas democracy aid amounts must be doubled to achieve an effect similar in size (and of larger uncertainty). Democracy donor proliferation also maintains a positive and significant correlation with democracy when economic donor proliferation and economic aid amount are added to the model (model 4; the sector results are displayed side-by-side in two columns to economize on space). The latter's coefficient is not significant, which goes in line with the proposition that ideas on democracy are predominantly conveyed via democracy aid, and not via economic aid.[8] Model 5 shows that general donor proliferation is not significantly related to democracy levels either when employing control variables.

Panel C provides the estimates for the first stages of the 2SLS models.[9] Model 1, without control variables, shows a significant positive correlation of the interacted instrument with the actual number of democracy donors. A Kleibergen-Paap (KP) F-statistic of 32 suggests that there is little reason to worry about a weak instrument. Not using control

[7] Online appendix C provides full regression tables for all models.

[8] One might be concerned about introducing multicollinearity when employing donor proliferation and aid amounts simultaneously, even more so when two sectors are included at the same time. The correlations between these three variables range between 0.51 and 0.67. But the maximum VIF for a pooled version of model 4 is 4.9, thus multicollinearity should not be of concern.

[9] Table B2 in the online appendix also provides the reduced form estimates corresponding to the IV specifications. They behave as expected.

Table 1: Donor proliferation and democracy

Dependent variable (panels A and B):	democracy					
	(1)	(2)	(3)	(4*a*)	(4*b*)	(5)
Aid sector:	*democracy*	*democracy*	*democracy*	*democracy & economic*		*general*
Panel A: OLS estimates						
Sector donor prolif.	1.03*	0.96*	0.96*	0.84*	0.02	0.49
	[0.52; 1.54]	[0.47; 1.46]	[0.47; 1.46]	[0.32; 1.37]	[−0.51; 0.55]	[−0.08; 1.06]
Sector aid (log)		1.09	1.09	0.79	3.51*	5.21*
		[−0.15; 2.32]	[−0.15; 2.32]	[−0.37; 1.95]	[0.79; 6.24]	[1.89; 8.53]
R^2	0.93	0.93	0.93	0.93		0.93
Panel B: 2SLS estimates						
Sector donor prolif. (est.)	2.58*	2.72*	3.39*	1.98*	1.50	1.50
	[1.26; 3.91]	[1.42; 4.02]	[1.24; 5.55]	[0.31; 3.66]	[−0.43; 3.42]	[−0.51; 3.59]
Sector aid (log)		−0.60	−1.24	−0.40	−1.97	2.36
		[−2.13; 0.93]	[−3.53; 1.04]	[−2.01; 1.22]	[−5.98; 2.03]	[−3.44; 8.16]
R^2	0.91	0.91	0.89	0.91		0.92
Dependent variable (panel C):	sector donor proliferation					
Panel C: First-stage estimates (for panel B)						
IV democr. donor prolif.	0.36*	0.36*	0.21*†	0.32*	0.12*	
	[0.24; 0.49]	[0.24; 0.47]	[0.12; 0.31]	[0.21; 0.44]	[0.04; 0.20]	
IV econ. donor prolif.				−0.97	4.07*	
				[−2.84; 0.90]	[2.57; 5.57]	
IV general donor prolif.						3.68*
						[2.24; 5.11]
Democracy aid (log)		0.99*	0.80*	0.87*	0.17	
		[0.33; 1.64]	[0.18; 1.43]	[0.31; 1.44]	[−0.09; 0.42]	
Economic aid (log)				1.27*	1.52*	
				[0.02; 2.52]	[0.72; 2.33]	
General aid (log)						2.00*
						[0.94; 3.07]
KP F-statistic	32.39	34.68	18.02	15.08	21.64	25.16
Donor IV	*WiP*	*WiP*	*WiP*	*WiP*	*leg. fract.*	*leg. fract.*
Recipient IV	*propensity*	*propensity*	*distance*	*propensity*	*propensity*	*propensity*
Model properties (all panels):						
Covariates	*no*	*yes*	*yes*	*yes*		*yes*
Observations	715	715	715	715		715
Recipients	133	133	133	133		133

*) 95-percent confidence interval does not include zero [confidence intervals in brackets; clustered by recipient country].
†) coefficient and confidence interval scaled by factor 100 for presentation purposes.
Observation unit is a country three-year period. All explanatory variables are lagged by one period. All models include country and year fixed effects. Covariates include population (log), GDP per capita (log), and civil conflict (dummy).
Abbreviations and definitions: *IV:* instrumental variable; *(est.) donor prolif.:* (estimated) donor proliferation, i.e., number of donors providing more than 100,000 USD in a given year; *KP:* Kleibergen-Paap; *WiP:* share of women in donor parliament; *leg. fract.:* legislative fractionalization; *propensity:* propensity to receive aid in the respective sector within the overall time period; *distance:* distance between donor and recipient capitals in 1,000 kilometers, inverted.

variables would be justified with a 'good' instrument, as it would effectively randomize the treatment. However, in many applied situations, the quasi-randomization is conditional on other covariates (Angrist and Pischke 2009: 175).[10] In the present case, larger countries may be more likely to attract larger numbers of donors, as may poor countries, and countries at peace. I include these controls in model 2, as in the corresponding OLS model. Aid amounts play a somewhat ambiguous role, as they constitute a different type of information from the same channel. But the indicator *donor proliferation* does not simply contain less information than the indicator *aid amounts*. Figure 2 demonstrates that countries with many donors always get a lot of aid. But even with few donors, aid amounts may be high.

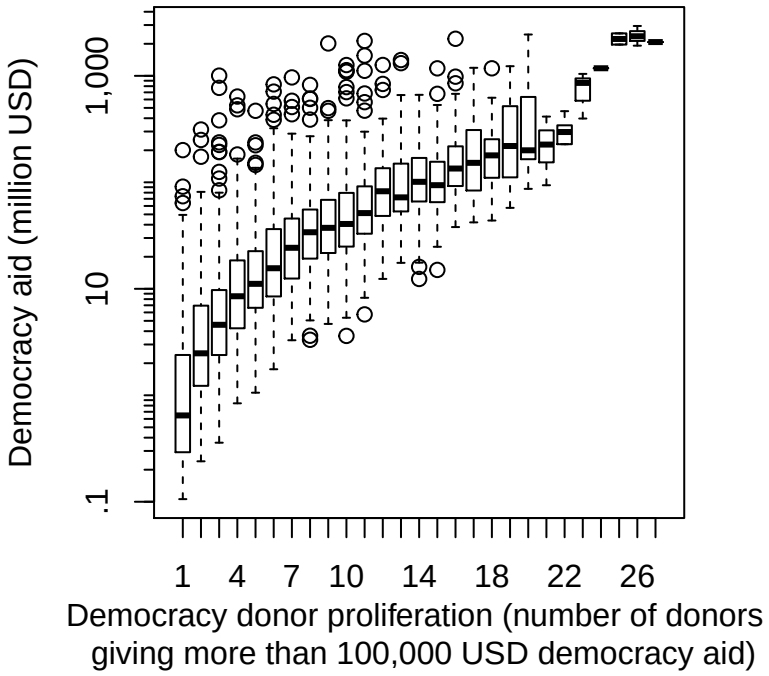

Figure 2: Distribution of democracy aid amounts by democracy donor proliferation over recipients by year

It shows in the aid coefficient of model 2 (panel C) that the amount helps predict the number of donors. Despite including this control, the instrument retains a significant coefficient of the same size (0.36) and a KP F-statistic well above the recommended threshold of 10 (Staiger and Stock 1997). In model 3, which employs the instrumental variable approach of Dietrich and Wright (2015), the role of aid amounts becomes some-

[10]Covariates may also help reduce the variability in the dependent variable and thus enable more precise estimation.

what more complicated.[11] Dietrich and Wright employ the interaction of the share of female legislators in the donor country with the distance between donor country and recipient country capital as an instrument for the *amount of aid per capita*. I employ the same approach to the number of donors. Online appendix B shows, however, that the instrument is strong for the number of donors, and much less so for aid amounts, in either absolute or per capita terms. I interpret this as support for the proposition that the instrument is well-suited to predict democracy donor proliferation in my sample. Model 4 instruments for both the number of democracy donors and the number of economic donors. Attempting to identify the causal effects of two endogenous regressors simultaneously is generally ill-advised. Nonetheless, my strategy provides me with instruments for both regressors that differ sufficiently in design, and if my findings hold in such a setting, I am inclined to be more confident that my hypothesis touches upon a causal mechanism. The coefficients for the instruments are positive and significant for the corresponding sectors, as expected. The KP F-statistics are above 10 for both instruments and thus indicate sufficient instrument strength. Model 5 replicates model 2 with general donor proliferation instead of democracy donor proliferation. The interacted instrument consisting of legislative fractionalization in the donor country and the propensity to receive aid seems to work fine, as coefficient sign, significance and KP F-statistic indicate.

Panel B presents the results of the second stage estimates of the 2SLS models. They fully confirm the OLS estimates, with the coefficients for democracy donor proliferation having positive signs and confidence intervals that exclude zero (models 1 to 4). Coefficient sizes for democracy donor proliferation are larger than the OLS estimates, by factors around three. Coefficients for economic donor proliferation – estimated in joint model 4 – and general donor proliferation (model 5) are larger than in the OLS specifications, but not significant.

Figure 3 gives an impression of the substantive effects estimated by IV model 2 (panel B). It provides estimates of the expected level of democracy, given the number of democracy donors. The other covariates are set to the average values that Ghana reached in the period 2012 to 2013, comprising a population of 26 million, a GDP per capita of 735 USD, no civil conflict and democracy aid commitments of 203 million USD per year. Reducing the number of democracy donors from the realized 22 to the sample median of 11 results in a decrease of expected democracy by 32 points (from 90 to 58) – a massive decrease on a scale ranging from 0 to 100. The size of the effect becomes more credible when one recalls that instrumental variables recover the *local* average treatment effect (LATE), which I discuss in detail in the next section. In a nutshell,

[11] As noted in the previous section, I employ a different transformation of the distance variable than Dietrich and Wright.

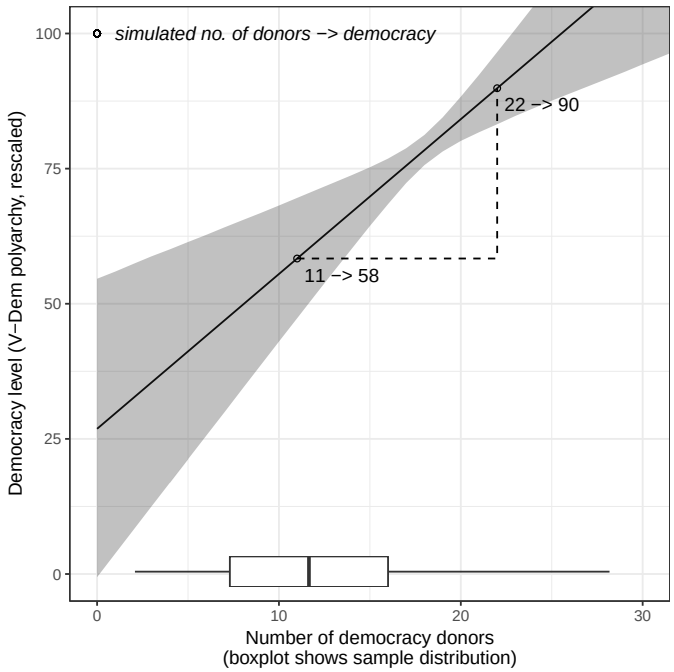

Figure 3: Expected levels of democracy conditional on the number of democracy donors

the OLS estimates average over all cases, whereas the IV estimates focus on the cases that are likely to be offered democracy aid and likely to accept it. The corresponding decrease predicted by OLS model 2 (panel A) would amount to only .96 x 11 = 11.

Discussing the IV assumptions

As a large share of my causal claims rests on the instrumental variables approach, a discussion of whether its assumptions are met is in order (cp. Sovey and Green 2011: 198). Exclusion restriction, independence assumption and instrument strength have been discussed above. But do the homogeneity of treatment effects assumption, the monotonicity assumption and the stable unit treatment value assumption (SUTVA) hold?

The homogeneity of treatment assumption states that the effect of the treatment on the outcome is independent from how the treatment was incentivized. Do democracy donors that cater a recipient country due to the influence of female legislators in the donor countries have a different effect than democracy donors present for other reasons? Female-legislator induced donor presence should, following the argument of Dietrich and Wright (2015), imply an honest intention to promote democracy. Democracy donors present for other reasons – such as geo-political interests – may not be that committed. Bouchet (2016) describes how the United States hardly pushed their Middle Eastern and North African allies towards democracy at all. While there were

some signs to correct this course during the Arab spring, recent disappointments have led the United States to fall back into their initial laissez-faire position. The effects estimated here are thus probably not generalizable to all democracy donors. This violation of the assumption of homogeneous treatment effects may explain the large difference between OLS and IV estimates. The LATE is restricted to honest attempts of democracy promotion. But there is no reason to expect that the female-legislators induced LATE differs substantially from the LATE of other honest attempts to promote democracy. The bottom line is almost trivial: a large number of honest democracy donors can promote democracy effectively.

The monotonicity assumption states that if any case behaves contrary to what the incentive provided by the instrument implies, one cannot estimate the causal effect of the variable of interest. This assumption is best discussed in potential outcomes terminology.[12] Cases that behave contrary to incentives are called 'defiers'. In this application, defiers would be recipient countries that have few democracy donors despite being frequent recipients of democracy aid from donors with large shares of female legislators. Recipient countries would also be defiers if they had many democracy donors despite being infrequent recipients of democracy aid or being catered by donors with few female legislators. The dyadic nature of my instrumental variable complicates justifying this argument. I disaggregate the potential outcomes further in online appendix B. My conclusion is that defiers are unlikely to exist. Female legislators should not stop donors from providing nor recipients from accepting democracy aid. Also, cutting ties in frequent democracy aid relationships (i.e., dyads with high propensity to record democracy aid) should be rather rare. The monotonicity assumption is thus met.

Finally, I must consider the SUTVA, which implies that a recipient's democracy is not affected by the treatment that another recipient receives, or by the outcome that another recipient experiences. One could very well imagine that increased participation induced by democracy donors spreads from one country to the other. Moreover, democracy is clearly spatially correlated (Gleditsch and Ward 2006). The Arab spring is a prime example for such a chain reaction (although it is unclear what role democracy aid played here). Considering the possibility of contamination across units, I test in online appendix C whether my results hold when using standard errors that allow for spatial clustering among neighbors. All results hold.

[12] See Morgan and Winship (2015: 305–324) for an introduction.

Robustness checks

While the models presented in table 1 constitute my best shot at modeling the relationship between democracy donor proliferation and democracy, a range of alternative, reasonable modeling decisions exists. Online appendix C verifies whether different choices would change the substantive results of my study. The general conclusion is that they would not.

The results are robust to using a range of alternative indicators: aid disbursements instead of aid commitments (for both the number of donors and aid amounts); aid controls in per capita terms and as a share of GDP instead of in absolute terms; alternative democracy indicators as dependent variables, including Polity IV, Freedom House, the Unified Democracy Scale. Using alternative indicators for aid fragmentation as suggested by Gehring et al. (2017) yields mixed results: the number of small democracy donors produces the same statistically significant relationship with democracy as the number of all democracy donors does. Specifications with an inverted Herfindahl-Hirschman-Index that measures the fragmentation of financial flows produce no results. This index, however, does not capture the concept of diversity as the number of donors does; higher amounts of aid do not necessarily transmit more ideas. A concentration ratio that captures the share of democracy aid that the three largest donors provide is not significant at the 95-percent level, but it comes close, with consistently correct signs. When looking at sub-indices of democracy to disaggregate the explained variable, I reject the null hypothesis of no effect for V-Dem's liberal and participatory principles, but not for the deliberative and the egalitarian principles. This finding makes sense since I expect diversity to foster primarily formal democratic practice and participation.

The results are also mostly robust to using alternative samples: samples based on one-year, two-year and four-year time periods as temporal unit, instead of three years; and geographic sub-samples (sub-Saharan African countries versus countries from all other regions). Temporal sub-samples of ten years each do not produce statistically significant results, but this may be due to a reduced number of observations. A 15-year period from 1999 to 2013 produces results similar to the main specifications. Also, regime sub-samples with countries above, below or around the global median in democracy do not produce consistently significant results. Again, this may be due to smaller sample sizes, since at least the IV models' confidence intervals only barely include zero.

The results are also largely robust to alternative modeling choices: lagged dependent variable models; models including a squared term for the number of democracy donors; models including global or regional linear time trends; models with contemporaneous instead of lagged explanatory variables; models with standard errors clustered by

Table 2: IV estimates with alternative and placebo sectors

	Democracy	Democracy, no PB	Civil society	Economic	Health	Energy
No. of donors (est.)	2.72*	4.25*	2.63*	−3.08	−1.06	−0.41
	[1.42; 4.02]	[0.97; 7.53]	[0.87; 4.39]	[−14.75; 8.59]	[−5.42; 3.30]	[−4.54; 3.72]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Additional controls	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715	715	715
# of countries	133	133	133	133	133	133
R^2	0.91	0.86	0.91	0.87	0.91	0.92
KP F-statistic	34.68	6.55	16.16	0.95	6.36	7.43

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
all explanatory variables lagged by one period.

both country and time period; and models with heteroscedasticity-and-auto-correlation-robust standard errors. Models including interactions of the number of democracy donors with the level of aid indicate that aid amounts actually condition the effect of donor proliferation somewhat: only above 6 USD per capita or above 10 million USD in absolute democracy aid commitments, the coefficient estimates for the number of democracy becomes significant. Most recipient years, however, record aid above these thresholds. Models including interactions of the number of democracy donors with the level of democracy suggest that donor proliferation is effective only for recipients with lower democracy scores. These models, however, are based on very conservative lagged dependent variable models and constitute a lower bound for my estimates. Reverse models show that the number of democracy donors in the preceding period does not predict levels of democracy well, providing further support to the hypothesized causal direction.

Finally, two additional empirical tests shall show whether one can trace the causal mechanism more closely with large-n data. First, differentiating aid sectors in more detail will show whether the effect is actually attributable to (broad and narrow definitions of) democracy aid, or whether our instrument would produce the same results for any sector. Table 2 presents results from the IV specification with control variables (model 2, panel B in table 1) with the number of donors in the aid sectors democracy (model 1), democracy excluding post-conflict aid (2), civil society (3), all sectors excluding democracy (4), health (5), and energy (6). The donor-level instrument for all models is female legislators, the recipient-level instrument the propensity of receiving aid in the respective sector over the period 1994 to 2013. As expected, donor proliferation in the democracy and civil society sectors shows a significant effect of the number of donors on democracy. Neither economic nor health nor energy donors seem to exert a positive or substantial influence on democracy, as expected. The empirical results are robust to these placebo tests.

Table 3: IV estimates with V-Dem indicators of democratic diversity

	Largest party share	Party platforms	Counterarguments	Engaged society	CSO environment
No. of dem. donors (est.)	−2.43	0.05*	0.07	0.04	0.08*
	[−5.19; 0.34]	[0.01; 0.09]	[−0.00; 0.14]	[−0.02; 0.10]	[0.03; 0.14]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*no*	*no*	*no*	*no*
# of observations	396	715	715	715	715
# of countries	123	133	133	133	133
R^2	0.71	0.96	0.87	0.90	0.90
KP F-statistic	28.81	32.39	32.39	32.39	32.39

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Second, I look at a differentiated outcome variable. We have seen above that democracy aid proliferation results in large number of projects, and that different donors have different democracy aid portfolios. But does this diversity translate into democratic diversity on the recipient side, below the level of aggregate diversity? Table 3 provides IV results that predict the level of indicators of democratic diversity provided by the V-Dem project (Coppedge, Gerring, Lindberg, Skaaning, Teorell, Altman, Andersson, et al. 2017). The first indicator considered here is the share of seats won by the largest party.[13] One might expect that political diversity increases the chances of parties other than the largest (and presumably ruling) party in elections. The coefficient is negative, as expected, but it does not reach significance (model 1). Note that the sample size decreases by almost 50 percent for this indicator, as data is only recorded for countries with partisan elections. The number of effective and distinctive party platforms, however, does increase significantly (model 2). Respect for counterarguments has the expected positive sign, but does not reach significance (model 3). The same holds for 'engaged society', an indicator on how 'wide and how independent' public deliberations are (model 4). The 'the involvement of people in civil society organizations' that measures both the diversity and the activity of civil society, however, is again significant (model 5). These results supports the notion that donor proliferation improves democracy by increasing the diversity of political organizations the recipient country. Electoral results and deliberative practice do not appear to be affected; such an effect is less credible given the low reach of democracy aid beyond actors that are already mobilized.

[13] See online appendix C for exact variable references and additional OLS specifications.

Tracing the causal mechanism in Ghana

The macro-quantitative evidence provides strong support for my hypothesis. But is the causal mechanism plausible when being confronted with accounts from actual practice in democracy aid? In order to conduct this sanity check, I review the effects of democracy aid in Ghana as reported in the scholarly literature and in donor publications. Ghana is a donor darling and has improved its democracy scores over the past two decades. If the mechanism of improving democracy via diversity in democracy donors is generalizable, Ghana is a likely case where this should be observable (cp. Goertz and Mahoney 2012: 177–191). Online appendix D contains the full case study and detailed statistics on democracy aid in Ghana. To summarize the results, one can state that two components of Ghanaian democracy have benefited substantially from the contributions of individual donors: Dutch aid contributed to the survival of small parties, and German aid contributed to professionalizing free mass media (Gyimah-Boadi and Yakah 2012; van Breukelen and Magolowondo 2010). Detailed tables on sectoral aid flows provided in the appendix indicate that both donors entered Ghana before the positive effects materialized. Dijkstra and Kumado (2004: 15) confirm that Dutch support provided a unique contribution: '[…] everyone interviewed by the evaluators stressed the uniqueness of the [N]IMD/IEA programme in relation to the interventions of other development partners in Ghana because of the bilateral programme and the cross party activities'. In a counterfactual world without these donors' engagement, it seems unlikely that other donors would have replaced their activities. Both small parties and free media are crucial in young democracies (cp. Conroy-Krutz and Moehler 2015; Lindberg 2010). Donor proliferation is thus likely to have had a positive causal effect on democracy in Ghana.

Conclusion

Diversity is both justification of and prerequisite for liberal democracy. This paper shows – with a range of quantitative models and anecdotal evidence from Ghana – that diversity provided by a multitude of donors helps improve a recipient country's democracy. The finding contradicts common wisdom in development studies that donor proliferation and other forms of aid fragmentation have detrimental effects. Insights on democratization processes suggests why this is not the case for democracy aid: diverse and participatory processes are more likely to produce sustainable institutions in young democracies (e.g., Kapstein and Converse 2008; Wright 2008). Different donors provide different ideas and thus improve trial-and-error processes of democratization.

These findings contribute to the democratization literature: They provide empirical

support to the surmise that external donors can – to some extent – replace a lack of endogenous funding for various political currents. The findings also contribute to the aid effectiveness literature by providing a first test on how aid fragmentation modifies the effects of democracy aid. My diversity hypothesis also resonates well with other research that disaggregates the effects of democracy promotion efforts. Birchler, Limpach, and Michaelowa (2016), for example, find that aid provided by the World Bank and the International Monetary Fund has positive effects on democratization, if made conditional on increased participation and accountability.

Immediate implications for development policy arise. My findings caution against uncritically expanding the division of labor in foreign aid. While economic aid may still be well-advised to follow the principle of harmonization, it is recommendable to maintain a sufficiently diverse donor environment in the area of democracy aid. Non-government organizations, political parties and independent media providers can be best catered by a group of donors with various backgrounds in a 'marketplace for ideas'. But is this a realistic scenario? A recent survey of policy makers in recipient countries concludes that it is: recipients frequently and purposefully leverage useful support from different donors (Parks, Rice, and Custer 2015).

Nobody expects democracy aid alone to create democratic regimes, and many countries will suffer setbacks in the future. But democracy aid can help make democracy a little less defective, and the more experience with participatory governance is accumulated, the higher the chances for real democracy in the future (cp. Quintelier and Van Deth 2014; Svolik 2013). This process may take long, but evidence presented here and in other recent publications shows that it may be possible to fast-forward it at least a little with effective modes of implementing democracy aid. And it is crucial that democracy aid is effective, as ineffective measures help autocrats build facades of democracy and consolidate their grip on power (Bader and Faust 2014).

Increasing the number of democracy donors, however, is not a feasible option for all recipients. The rate of never-takers that reject democracy aid is increasing: Carothers (2016: 361) observes that while the most authoritarian states have always kept out democracy support as much as possible, more recently even democratic and semi-authoritarian regimes such as Ethiopia, India, Russia and Venezuela have begun to follow suit. They consider democracy aid as 'foreign political meddling'. By using propensity to receive democracy aid as a component of my instrumental variable, I control for this selection bias. It shows that sufficient compliers remain for a positive net effect, despite authoritarian cocooning. In recipient countries that do allow democracy aid, the recommended measures are comparatively easy to implement: Donor and recipient governments have – provided a certain degree of international cooperation – direct control over the degree of donor proliferation, as opposed to structural variables

that influence democratization. McFaul (2004: 157) states that there is no blueprint for democracy promotion, and that 'many even reject the idea that there can be a blueprint'. I agree with this analysis, but the present study suggests a 'meta-blueprint' instead: keep democracy promotion diverse.

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More donors, more democracy

Online appendices

This version: November 2017

This file contains four online appendices to the paper *More donors, more democracy*:

- A: Data (12 pages)
- B: The instrumental variable approach (24 pages)
- C: Robustness checks (51 pages)
- D: Tracing the causal mechanism in Ghana (10 pages)

More donors, more democracy

Appendix A: Data

Contents

This file provides additional information on the data and software employed in the paper *More donors, more democracy*.

Universe of cases

The universe of cases analyzed is all countries defined by the OECD to be eligible for receiving Official Development Aid (ODA). Changes in developing country status over time have been derived from (incomplete) information available on the OECD website.[1] Only independent countries as defined by Weidmann, Kuse, and Gleditsch (2010) and with with more than half a million inhabitants are considered. On the donor side, I consider all bilateral donors that are OECD members and multilateral donors. This excludes authoritarian countries such as the United Arab Emirates that recently started to report to the Creditor Reporting System (CRS) of the OECD.

Description of indicators

Aid indicators

My primary measure of fragmentation is simply the number of donors providing foreign aid to the respective recipient country.[2] To determine whether a country receives aid from a particular donor, I draw on the AidData Research Release 3.0 (Tierney et al. 2011). AidData is mainly based on the CRS provided by the Donor Assistance Committee (DAC) of the OECD (2015). I choose AidData as primary source because it contains information on former aid recipients that have been removed from the CRS. AidData also contains some additional project coding. I am using the 'coalesced purpose codes' that combine the information of both CRS and additional AidData coding. The data is available on the project level. In order to remove irrelevant contributions, I do not count donors as 'present' if their reported commitments are less than 100,000 USD in a year over all projects in the respective sector. This threshold is somewhat arbitrary. I assume that 100,000 USD would roughly suffice for an international expert employed in that sector.[3] I exclude debt relief, humanitarian aid and aid spent in the donor countries from all aid variables, since these are unlikely to have direct effects on democracy, or are driven by external shocks such as natural disasters.

[1] http://www.oecd.org/dac/stats/historyofdaclistsofaidrecipientcountries.htm

[2] The number of actors as a measure of diversity is also used in related fields: Seymour, Bakke, and Cunningham (2016: 7–8), for example, employ the number of groups as an indicator of fragmentation in ethnopolitical movements.

[3] This would not cover the overhead, of course, but that could come from the general aid budget as well.

The sector *Government and Civil Society* (GCS) roughly captures activities that I define as democracy aid. It contains institutional support to the state, electoral assistance, and support to various forms of civil society, including human right groups and media outlets. Focusing only on aid that goes to civil society might seem to fit my argument of multiple recipients better, but this type of aid accounts for only 0.6 percent of total aid in my sample. Furthermore, much aid that is listed as institutional aid is channeled through government to civil society actors. The more detailed the information extracted from AidData, the more likely it is to obtain severely biased data, because reporting habits vary over time and across donors. Even for one donor it can be troublesome to maintain a standard, since the decision whether a particular project should be coded under a certain category is often ambiguous in practice. Despite these drawbacks, the AidData provides the most complete information on aid activities available. Employing a rough measure (number of donors) on a comparatively highly aggregated level (the GCS sector) helps minimize measurement error. Empirically, logged GCS and civil society aid per capita correlate at 0.63, and the results for the base model do not change substantially when using the latter specification (see online appendix C).

There is also an alternative identifier for democracy-related aid provided by the CRS, namely the 'Participatory Development and Good Governance' (PD/GG) marker. But this indicator has many missings and overlaps strongly with GCS for the non-missing data: It lists democracy activity only for two country years in my sample where GCS does not, but it misses many country years where GCS is reported. The number of activities recorded also correlates highly at .94 with the number of GCS activities. Another alternative is to distinguish types of aid by the channel of delivery: Do the flows go to government or non-government recipients (cp. Dietrich 2013)? But this distinction is only available from the year 2004 onward and thus does not lend itself for the present analysis.

The CRS-based AidData is the most encompassing source on donor activities, but it has other drawbacks that affect all aid data sources. There is underreporting by donors, both intentionally and unintentionally (Michaelowa and Michaelowa 2011). Since disbursement records are very incomplete, AidData has focused on collecting aid commitment data in the recent releases.[4] Commitments are not necessarily fulfilled, and if they are, the disbursement may occur several years later; but they provide a decent proxy for the engagement of a particular donor in the recipient country at a certain point in time.

Figure A1 provides information on how the number of donors in general aid, democracy aid and economic aid has changed over the time period under study. It shows

[4]The United States, for example, has been marking aid disbursements with GCS codes only since 2002.

that the data follows a strong time trend. The number of reported democracy donors per year in the mid-1990's was very low, which is one reason not to start the period of investigation earlier. One should also note that reports in the number of donors underestimate the number of agencies working on the ground, since some donors fund several different organizations that work independently, such as the German political foundations.

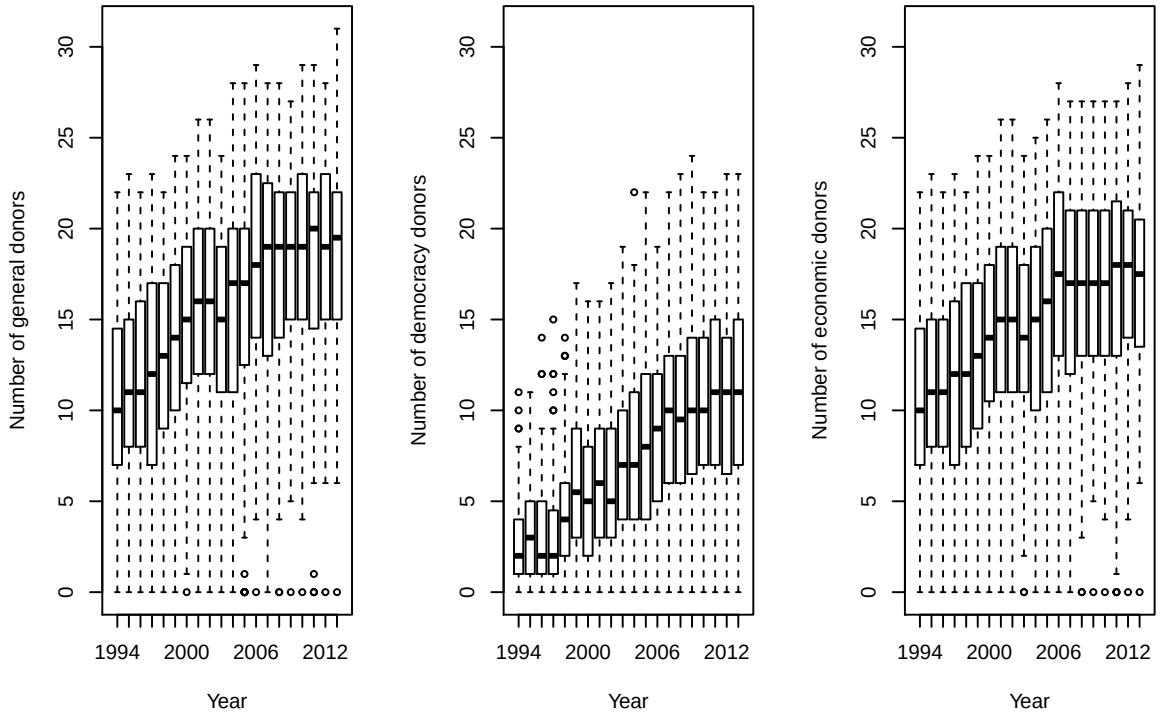

Figure A1: Donor proliferation by aid sector, 1994–2013

Democracy indicators

The aid variables are hypothesized to be correlated with changes in democracy. I measure democracy levels primarily with the *polyarchy* index provided by the *V-Dem* project (version 7.1; Coppedge et al. 2017). V-Dem is a new collection of institutional traits of all countries in the world since 1900 and provides a more detailed assessment of democracy than other data sources. It has a near normal distribution and is thus better capable of distinguishing countries at the extremes, as opposed to the prominent Polity IV data (Marshall, Gurr, and Jaggers 2016). An additional advantage of polyarchy index is the more fine-grained – in fact, continuous – nature, as opposed to the ordinal Polity IV scale. As a latent aggregate, the V-Dem polyarchy index is also much

smoother. This reduces the impact of small and spurious fluctuations in the individual indicators and helps to identify the general trends. Nonetheless, I use Polity IV, Freedom House and the Unified Democracy Scale to assess the robustness of my results. All democracy measures are normalized to a 0 to 100 scale to facilitate comparison across models with different indicators. Since my theory predicts improvements for various aspects of democracy, I employ an aggregate democracy index as the main dependent variable. But I also test how donor proliferation affects components of democracy as measured by V-Dem (see online appendix C for all robustness checks).

Control variable indicators

A set of controls is included in the regressions to account for the presence of competing factors that could also influence democratization. I include the amount of general and democracy aid that is spent in order to distinguish effects of aid quantity and aid fragmentation. Aid quantity is here measured as absolute aid, as the number of donors is also measured in absolute terms.[5] I include population size, per-capita income (both from Feenstra, Inklaar, and Timmer 2015) and civil war (Gleditsch et al. 2002).

Summary statistics

Table A1 provides summary statistics for the main dataset employed in the paper. Observations are recorded at the three-year-period recipient-country level. Table A2 provides the corresponding correlation matrix. (The row and column numbers in table A2 refer to the indicator names as listed in table A1.) Table A3 provides summary statistics for the country-year dataset from which higher levels of temporal aggregation were calculated. Table A4 provides summary statistics for the dyadic dataset with donor-recipient-year observations from which aid indicators and instrumental variables were calculated. Note that the rather low mean and median scores for the propensity to receive aid is partly due to the large number of donors I include in my sample (38, including multilateral donors): every donor that does not provide aid from the respective sector to a particular recipient generates a zero that drives down the average propensity score. Donors that provide most types of aid to most recipients are few.

Table A1: Summary statistics: three-year time windows, monadic level, 1994-2012

No.	Indicator	N	Mean	Median	Std. dev.	Min.	Max.
1	First year of the time window	895	2,002.64	2,003.00	5.93	1,994.00	2,012.00
2	Last year of the time window	895	2,004.64	2,005.00	5.93	1,996.00	2,014.00
3	Number of donors	895	18.59	20.00	6.08	0.00	33.00
4	No. of democracy donors	895	11.13	11.00	6.22	0.00	31.00
5	No. of economic donors	895	17.88	19.00	6.01	0.00	31.00
6	No. of small dem. donors	895	10.63	10.00	6.35	0.00	29.00
7	Dem. aid fragmentation index	895	0.60	0.68	0.25	0.00	0.91
8	Dem. aid concentration ratio	895	0.79	0.83	0.20	0.00	1.00
9	Aid commitments (million USD)	895	3,152.04	1,281.00	7,211.17	0.00	101,010.51
10	Democracy aid com. (m USD)	895	251.35	90.83	534.00	0.00	7,801.07
11	Economic aid com. (m USD)	895	2,900.69	1,132.85	6,988.40	0.00	98,883.30
12	Aid com. per capita	895	198.55	139.88	218.56	0.00	2,119.19
13	Democracy aid com. p.c.	895	23.22	9.49	57.48	0.00	1,128.91
14	Economic aid com. p.c.	895	175.33	122.61	189.54	0.00	2,038.19
15	Leg. fract. * aid propensity	895	4.82	4.91	2.03	0.00	9.34
16	WiP * democracy aid prop.	895	33.06	30.09	21.34	0.00	107.50
17	Leg. fract. * econ. aid prop.	895	3.95	3.84	1.74	0.00	8.53
18	Leg. fract. * distance (inverted)	895	278.83	276.99	56.94	102.41	422.85
19	WiP * distance (inverted)	895	9,077.71	8,958.62	2,361.06	2,563.69	16,156.21
20	V-Dem polyarchy, rescaled	878	48.60	46.89	25.38	0.03	99.81
21	Polity IV polity2, rescaled	880	60.70	72.92	30.91	0.00	100.00
22	Freedom House, rescaled	895	46.96	47.22	28.24	0.00	100.00
23	Population	895	41.52	9.46	152.19	0.50	1,354.04
24	GDP per capita	866	3,432.22	1,451.23	5,655.08	72.76	53,933.01
25	Civil war	895	0.20	0.00	0.37	0.00	1.00

Democracy aid portfolios

The argument presented in the main text assumes that all donors provide somewhat different approaches to democratization. I cite some literature that supports this notion (Dietrich 2016; Magen, Risse, and McFaul 2009), but the AidData project allows me to investigate the (reported) portfolios of individual donors more closely. Figure A2 shows in what areas of 'strengthening civil society' (CRS code 15150) donors spend their funds; table A5 reveals the abbreviations used for multilateral donors. I draw on AidData activity codes. Each project may have several activity codes from very different sectors. As I cannot assess the importance of the respective codes, I assign the full amount from a project to each activity code. The figure is a heatmap that compares the relative contributions of individual donors. Darker shades indicate that a higher share of a donor's commitments go to the respective activity. Donors and sectors are rearranged by similarity to facilitate interpretation. It shows, for example, that there is a group with a strong focus on human rights. This group includes Sweden, Finland and UNICEF. Poland even spends all aid in this sub-sector on human rights. Several multilaterals and Japan focus on community participation. Spain and Iceland focus their contributions

[5]Robustness checks with per capita aid to capture the relative impact that aid amounts have on a country are also conducted.

on women organizations. Other donors spread their funds more broadly, such as the European Union and the United Kingdom. I conclude that democracy aid portfolios differ substantially between donors.

Software

All calculations were performed with Microsoft R Open 3.3.2. All package versions as of a CRAN mirror snapshot taken on 1 November 2016 (see https://mran.microsoft.com for details).

Packages I employed include car (Fox and Weisberg 2011), clusterSEs (Esarey 2016), dplyr (Wickham and Francois 2016), ggplot2 (Wickham 2009), gplots (Warnes et al. 2016), interplot (Solt and Hu 2015), lfe (Gaure 2013), MASS (Venables and Ripley 2002), psych (Revelle 2015), texreg (Leifeld 2013), and xtable (Dahl 2014).

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Table A2: Correlation matrix for three-year time window dataset (column and row numbers refer to indicators in table A1)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
1																								
2	1.00*																							
3	0.37*	0.37*																						
4	0.51*	0.51*	0.88*																					
5	0.32*	0.32*	0.98*	0.84*																				
6	0.60*	0.60*	0.85*	0.95*	0.81*																			
7	0.27*	0.27*	0.58*	0.58*	0.57*	0.44*																		
8	-0.15*	-0.15*	-0.18*	-0.34*	-0.19*	-0.25*	-0.38*																	
9	-0.03	-0.03	0.27*	0.21*	0.29*	0.23*	-0.06	0.07*																
10	0.20*	0.20*	0.39*	0.48*	0.38*	0.49*	-0.03	0.04	0.45*															
11	-0.04	-0.04	0.25*	0.18*	0.27*	0.20*	-0.06	0.07*	1.00*	0.39*														
12	-0.02	-0.02	0.03	0.01	0.03	0.03	-0.12*	0.16*	0.20*	0.13*	0.20*													
13	0.18*	0.18*	0.10*	0.18*	0.07*	0.20*	-0.07*	0.07*	-0.02	0.25*	-0.04	0.60*												
14	-0.07*	-0.07*	0.01	-0.04	0.01	-0.03	-0.11*	0.16*	0.24*	0.07*	0.24*	0.97*	0.39*											
15	0.07*	0.07*	0.81*	0.70*	0.84*	0.62*	0.52*	-0.24*	0.27*	0.27*	0.25*	-0.07*	-0.03	-0.07*										
16	0.27*	0.27*	0.64*	0.73*	0.59*	0.66*	0.48*	-0.28*	0.06	0.34*	0.03	-0.02	0.15*	-0.06	0.60*									
17	0.05	0.05	0.76*	0.62*	0.81*	0.54*	0.46*	-0.21*	0.29*	0.22*	0.29*	-0.08*	-0.07*	-0.07	0.97*	0.41*								
18	0.16*	0.16*	0.07*	0.08*	0.03	0.11*	0.05	0.02	-0.05	0.01	-0.06	-0.02	-0.06	0.00	-0.10*	0.08*	-0.13*							
19	0.65*	0.65*	0.25*	0.34*	0.18*	0.40*	0.19*	-0.07*	-0.06	0.12*	-0.07*	-0.02	0.04	-0.04	-0.05	0.20*	-0.09*	0.84*						
20	0.04	0.04	0.11*	0.05	0.14*	0.07*	-0.03	0.13*	0.16*	0.11*	0.16*	0.31*	0.11*	0.32*	0.00	-0.18*	0.06	-0.11*	-0.06					
21	0.11*	0.11*	0.23*	0.19*	0.25*	0.19*	0.10*	0.05	0.13*	0.20*	0.12*	0.32*	0.18*	0.31*	0.10*	0.01	0.13*	-0.11*	-0.03	0.88*				
22	0.04	0.04	0.10*	0.03	0.12*	0.05	0.00	0.09*	0.10*	0.04	0.10*	0.34*	0.11*	0.36*	0.01	-0.19*	0.08*	-0.13*	-0.08*	0.92*	0.87*			
23	0.03	0.03	0.22*	0.20*	0.24*	0.18*	0.10*	-0.08*	0.39*	0.14*	0.39*	-0.15*	-0.08*	-0.15*	0.29*	-0.01	0.34*	-0.02	-0.01	-0.03	-0.02	-0.04		
24	-0.07*	-0.07*	-0.50*	-0.39*	-0.50*	-0.33*	-0.41*	0.11*	-0.02	-0.09*	-0.01	-0.01	-0.08*	0.01	-0.53*	-0.38*	-0.50*	-0.02	-0.04	0.18*	-0.03	0.12*	-0.07*	
25	-0.04	-0.04	0.19*	0.21*	0.18*	0.17*	0.09*	-0.05	0.15*	0.24*	0.14*	-0.18*	-0.07*	-0.18*	0.16*	0.28*	0.09*	0.05	0.02	-0.20*	-0.10*	-0.26*	0.16*	-0.14*

Table A3: Summary statistics: yearly data, monadic level, 1994-2013

Indicator	N	Mean	Median	Std. dev.	Min.	Max.
Year	2,527	2,003.12	2,003.00	5.69	1,994.00	2,013.00
Number of donors	2,527	15.19	16.00	6.20	0.00	31.00
No. of democracy donors	2,527	7.69	7.00	5.47	0.00	29.00
No. of economic donors	2,527	14.42	15.00	6.02	0.00	29.00
No. of small dem. donors	2,527	7.21	6.00	5.71	0.00	28.00
Dem. aid fragmentation index	2,527	0.52	0.59	0.27	0.00	0.92
Dem. aid concentration ratio	2,527	0.82	0.89	0.22	0.00	1.00
Aid commitments (million USD)	2,527	1,114.56	416.44	2,916.44	0.00	62,331.65
Democracy aid com. (m USD)	2,527	88.46	23.82	216.29	0.00	2,940.05
Economic aid com. (m USD)	2,527	1,026.10	359.25	2,848.87	0.00	62,289.99
Aid com. per capita	2,527	67.04	44.13	88.46	0.00	1,763.33
Democracy aid com. p.c.	2,527	7.17	2.35	14.88	0.00	257.19
Economic aid com. p.c.	2,527	59.87	39.00	83.61	0.00	1,761.00
Leg. fract. * aid propensity	2,527	4.49	4.61	2.20	0.00	9.21
WiP * democracy aid prop.	2,527	32.41	29.56	21.05	0.00	106.49
Leg. fract. * econ. aid prop.	2,527	3.67	3.61	1.87	0.00	8.55
Leg. fract. * distance (inverted)	2,527	254.08	261.26	76.05	0.00	411.71
WiP * distance (inverted)	2,527	8,563.03	8,449.61	2,193.16	2,267.62	15,573.27
V-Dem polyarchy, rescaled	2,477	48.24	46.64	25.64	0.00	100.00
Polity IV polity2, rescaled	2,476	60.17	75.00	31.39	0.00	100.00
Freedom House, rescaled	2,527	46.61	50.00	28.45	0.00	100.00
Population	2,527	41.73	9.50	152.29	0.50	1,357.38
GDP per capita	2,432	3,433.47	1,405.12	5,761.27	69.58	56,586.17
Civil war	2,527	0.21	0.00	0.41	0.00	1.00

Table A4: Summary statistics: yearly data (dyadic level), 1994-2003

Indicator	N	Mean	Median	Std. dev.	Min.	Max.
Year	21,092	1,998.53	2,000.00	3.34	1,994.00	2,003.00
Prop. of receiving aid (recipient)	21,092	0.21	0.00	0.28	0.00	1.00
Prop. of rec. democracy aid (rec.)	21,092	0.03	0.00	0.07	0.00	0.70
Prop. of rec. economic aid (rec.)	21,092	0.19	0.00	0.26	0.00	1.00
Donor-recip. distance (1000 km, inv.)	21,092	12.16	12.39	4.08	0.00	19.53
Women in parliament (donor)	20,450	19.85	17.87	10.62	2.20	45.20
Legislative fractionalization (don.)	20,294	0.68	0.68	0.10	0.44	0.90
Aid > 100,000 USD	21,092	0.44	0.00	0.50	0.00	1.00
Democracy aid > 100,000 USD	21,092	0.21	0.00	0.41	0.00	1.00
Economic aid > 100,000 USD	21,092	0.43	0.00	0.50	0.00	1.00

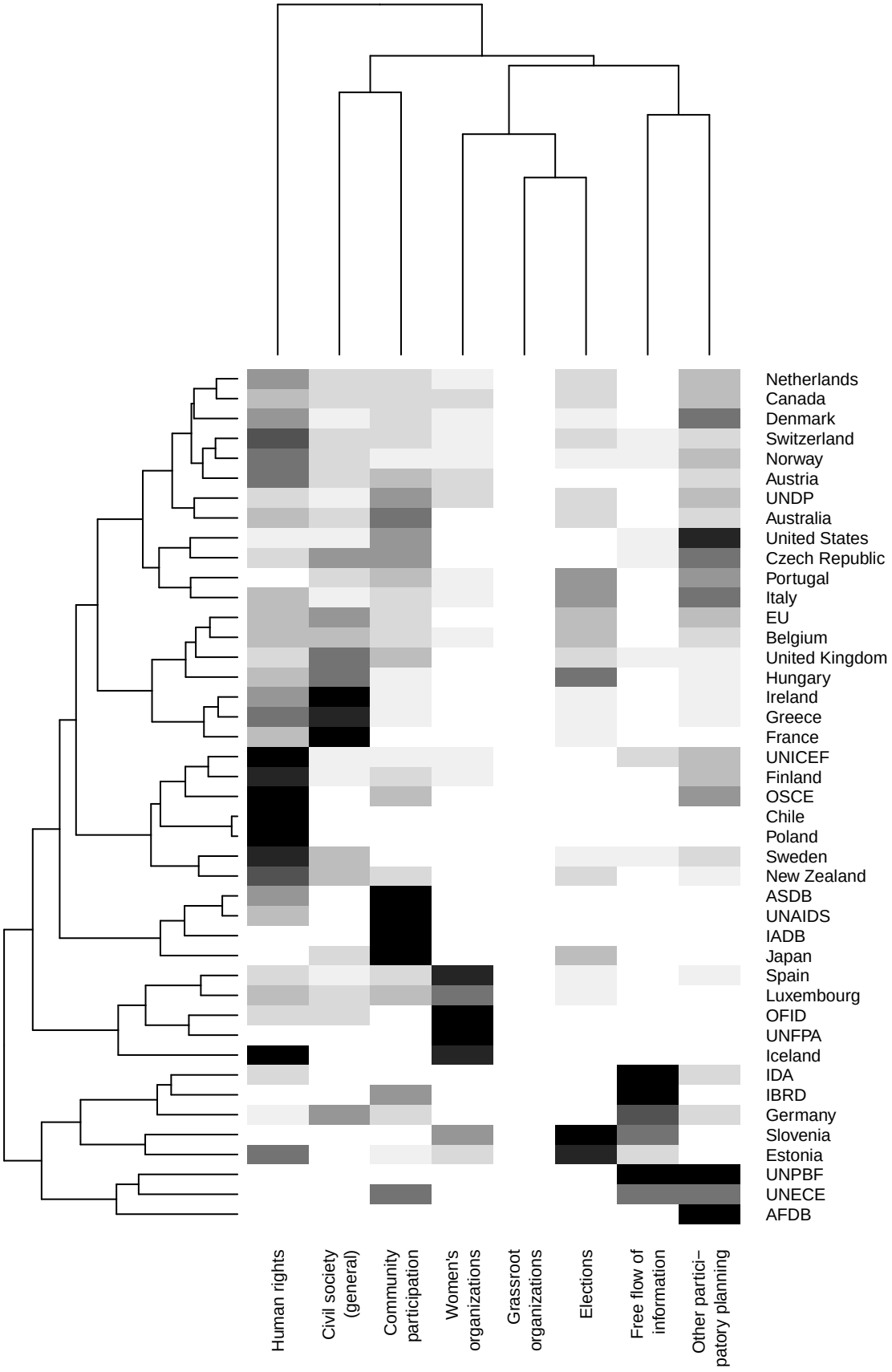

Figure A2: Civil soceity aid portfolios of donors, 1994–2013 (AidData activity codes); relative distributions by donor: darkest color indicates that more than 50 percent of aid is spent on that activity

Table A5: Abbreviations used in figure A2

abbrmulti	namemulti
AFDB	African Development Bank
ASDB	Asian Development Bank
EU	European Communities
IADB	Inter-American Development Bank
IBRD	World Bank - International Bank for Reconstruction and Development
IDA	World Bank - International Development Association
OFID	OPEC Fund for International Development
OSCE	Organization for Security and Co-operation in Europe
UNAIDS	Joint United Nations Programme on HIV/AIDS
UNDP	United Nations Development Programme
UNECE	United Nations Economic Commission for Europe
UNFPA	United Nations Population Fund
UNICEF	United Nations Children‘s Fund
UNPBF	United Nations Peacebuilding Fund

More donors, more democracy

Appendix B:

The instrumental variable approach

Contents

Identification strategy

This appendix provides additional information on the instrumental variable approach employed in the paper. I employ two strategies to construct the instrumental variable: *interaction* on the dyadic level, as shown in the paper, and the estimation of the number of donors on the dyadic level via a *zero stage*. The latter is described in the validation section of this appendix. Both strategies employ a recipient-side and a donor-side instrument, which are interacted on the dyadic level for both approaches. As instruments on the donor side, I employ two alternative indicators: the *propensity* to receive aid in the respective sector over the whole time period under study, and the *distance* between donor and recipient country. The following pages describe in detail how I modified my precursors' approaches to fit my research question.[1]

As argued in the paper, I follow recent suggestions to interact exogenous variables on the donor side with recipient properties in order to increase cross-sectional variance (e.g., Ahmed and Werker 2015; Dietrich and Wright 2015; Dreher and Langlotz 2015; Galiani et al. 2017; Nunn and Qian 2014: 1631). This is what I call the 'interaction approach'. I estimate the level of democracy Y in recipient country i at time t with the following equation:

$$Y_{it} = \beta_1 \hat{D}_{it-1} + \beta_2 \mathbf{X}_{it-1} + \alpha_i + \tau_t + \vartheta_{it},$$

where $\hat{D}$ is the estimated number of donors, $\mathbf{X}$ a vector of control variables, α country fixed effects, τ period fixed effects and ϑ the error term. This equation constitutes the second stage of a two-stage least-squares (2SLS) model. D is estimated in a first stage using the interacted instrument Z and the same set of control variables and country and period fixed effects, as well as an error term κ:

$$D_{it-1} = \delta_1 Z_{it-1} + \delta_2 \mathbf{X}_{it-1} + \alpha_i + \tau_t + \kappa_{ijt},$$

Z is calculated on the dyadic level, with donor-recipient years as unit of observation, and aggregated by summing over all donors for each recipient year:

$$Z_{it} = \sum_{j=1}^{N} (Q_{jt} * K_{ij}),$$

where Q is the donor-level instrument varying by donor j and time. K is the recipient-level instrument, varying by donor and recipient.^

[1] Some of the following paragraphs are duplicates from the main paper. I repeat this information here because it is a prerequisite to understand to following, more detailed arguments on the validity of my identification strategy.

Note that K_{ij} in time-invariant, whereas Q_{jt} is recipient-invariant. Combined with country and period fixed effects, there is no need to include the constitutive terms of the interaction in the first stage. More importantly, including fixed effects deals with the exogenous component of the interaction and reduces the demands on the identifying assumption: I must only assume that the level of democracy in countries with different propensities to receive democracy aid will be equally affected by changes in the share of women in the donor parliament if they have the same number of democracy donors, conditional on the other control variables (Ahmed 2016: 196; cp. Dreher and Langlotz 2015: 8–9).[2] The propensity indicator thus cannot exert an independent effect on democracy contaminated by endogeneity.

Instrumental variables

My main donor instrument Q applied to the democracy donor proliferation is the share of *female legislators*. Dietrich and Wright (2015, in their online appendix C) argue, building on Breuning (2001), that women are more likely to support social-equity issues and show empirically that the share of female legislators is a good predictor of democracy aid. The instrument is plausibly excludable: There is no reason to believe that the gender composition of a donor country's parliament should affect democracy in a recipient country directly. Dietrich and Wright (2015: 224) also show that alternative paths from female legislators to democracy are unlikely, given low bivariate correlations with trade, immigration, membership in international organizations, alliances and NGO presence.

For economic and general aid proliferation, donor instrument Q is *legislative fractionalization*, defined as 'the probability that two deputies picked at random from the legislature will be of different parties' (Beck et al. 2001: 174). Ahmed (2016: 191–193) suggests this indicator as an instrument for aid flows and shows that the U.S. House of Representatives passes larger foreign aid budgets when it is fractionalized. Dreher and Langlotz (2015) also employ government and legislative fractionalization as their donor instrument.

On the recipient side, K is the share of years in which a recipient receives aid in the respective sector. Nunn and Qian (2014) suggest this 'propensity' instrument, which is also used by Dreher and Langlotz (2015) and Ahmed (2016). More formally, propensity K^{prop} is defined as the share of years k in the examined time range in which a recipient receives aid from a particular donor that exceeds threshold h:

[2] See Nizalova and Murtazashvili (2016) for a formal proof for the case of combining exogenous treatments with endogenous factors.

$$K_{ij}^{prop} = \frac{\sum_k q_{ijk}}{max(k) - min(k)},$$

$$q_{ijk} = \begin{cases} 1 & if\ A_{ijk} > h \\ 0 & if\ A_{ijk} \leq h \end{cases}.$$

As in the donor proliferation variable (described in the main paper and online appendix A), I set threshold h to 100,000 USD, as lower amounts would certainly not suffice to provide for an actual presence of that donor.

I also test my hypotheses with an alternative recipient-level instrument suggested by Dietrich and Wright (2015: 224), who employ the inverted share of the distance between donor and recipient for K. I modify the approach by only inverting the distance, but dispensing from standardizing it to relative distances, as I expect the probability of a particular donor to cater a recipient country to be a function of absolute distance. This conforms to the specifications employed in most aid allocation studies.

To measure the share of female legislators, I use data from Paxton, Hughes, and Green (2006), provided and updated by the *V-Dem* project (Coppedge et al. 2017). Data on legislative fractionalization comes from the *Database of Political Institutions* (Beck et al. 2001). Distance data is calculated from the *cshapes* package (Weidmann, Kuse, and Gleditsch 2010). Online appendix A provides summary statistics and more detailed information on all data employed. Online appendix B provides more information on the instrumental variable approach.

In order to apply the donor-side instruments to multilateral donors, I follow Dietrich and Wright (2015 replication files) and use the average share of female legislators in the largest stakeholders of the respective organizations. For the distance instrument, I employ the distance to the capital of the host country of the multilateral donor. The multilateral donors considered comprise the European Bank for Reconstruction & Development (EBRD), the European Union (EU), the Inter-American Development Bank (IDB), the International Monetary Fund (IMF), as well as three World Bank units which have different stakeholder compositions: the International Bank for Reconstruction and Development (IBRD), the International Development Association (IDA), and the International Finance Corporation (IFC). Moreover, 14 United Nations (UN) organizations are considered, all with the same instrument on the donor side. Table B1 presents the stakeholders and locations for these multilateral donors. Note that all results hold when excluding multilateral donors, thus the assumptions induced here are not essential for my substantive conclusions.

Table B1: Stakeholders and approximate locations of multilateral donors

Multilateral donor	Largest stakeholders	Location (approx.)
EU	Germany, France, United Kingdom	Brussels
IDA	USA, Japan, United Kingdom	Washington, D.C.
IFC	USA, Japan, Germany	Washington, D.C.
IBRD	USA, Japan, China	Washington, D.C.
UN	USA, China, Russia, France, United Kingdom	Washington, D.C.
IDB	USA	Washington, D.C.
IMF	USA, Japan, Germany	Washington, D.C.
EBRD	USA, France, Germany, Italy, Japan, France	London

Validation

Scatterplots

To validate my approach, I first show that my main instrumental variables are related – positively and roughly linearly – to the explanatory variable of interest, the number of democracy donors. In figure B1, it becomes evident that the share of female legislators in a donor country is related to the number of recipient countries which this donor provides with democracy aid, when aggregated to the donor-period level. Note that both a linear regression (green line) and a smoothing spline (red curve) suggest a positive correlation (0.32, p-value < 0.000). This supports the idea of using female legislators as 'donor instrument' Q for the number of democracy donors. The propensity to receive democracy aid over the period 1994 to 2013 constitutes the 'recipient instrument' K. Figure B2 shows that K is related to the number of donors that provide democracy to a recipient in a particular time period, when aggregated to the recipient-period level (0.67, p-value < 0.000). Figure B3, again at the recipient-period level, shows that the interacted instrument correlates strongly with the number of democracy donors (0.74, p-value < 0.000).

Temporal variation of the instrument

The interacted instrument combines temporal and cross-sectional variation from two different variables. The temporal variation comes from the share of female legislators in donors countries. This number changes only with electoral cycles in the donor countries, which last four or five years in most cases. Electoral cycles overlap between donors, but one might nonetheless suspect that temporal variation is insufficient for my instrumentation strategy. Figure B4 shows how the share of female legislators varied in OECD countries between 1994 and 2013. A positive time trend is apparent in most countries, but the increase is not monotonous (see also figure B5 which plots the differences from year to year). Aggregated to three-year time windows that I employ as my

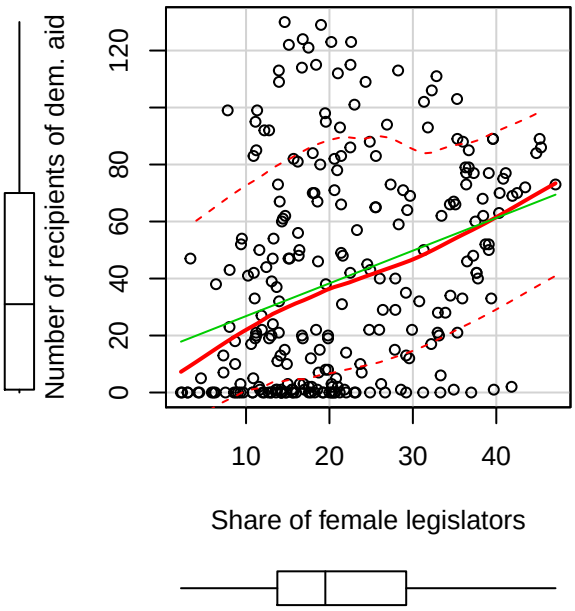

Figure B1: Share of female legislators and number of recipients provided with democracy aid (three-year donor periods, 1994–2013)

main specification, the patterns are obviously smoother, but they maintain substantial amounts of variation.

Reduced form estimates

Table B2 provides the results of the reduced form estimates of models 1 to 5 from the main results table. As expected, the interaction of women in the donor parliament with the propensity to receive democracy aid over the whole period under investigation is a good predictor of democracy (model 1). The relationship holds when adding controls (model 2), including economic aid proliferation and amounts (model 3). The interaction of legislative fractionalization in the donor country with the propensity to receive general aid is not a good predictor of democracy (model 4), which is in line with my argument that the causal channel goes through democracy aid.

Instrumenting proliferation or aid amounts?

The literature that inspires my identification strategy is interested in the effects of the *amount of aid*, and not, as I am, of the *number of donors*. I thus assess here whether my strategy actually predicts the number of democracy donors, or whether it predicts aid amounts instead. Table B3 shows what happens when the interaction approach with

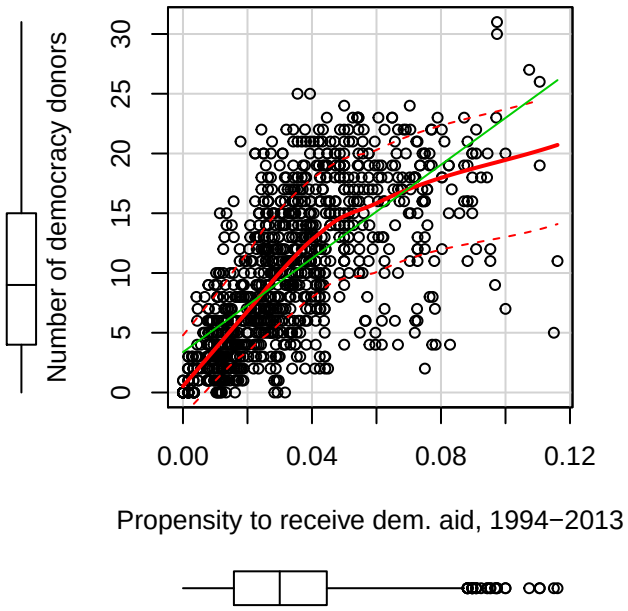

Figure B2: Propensity to receive democracy aid(three-year recipient periods, 1994–2013)

female legislators as the donor instrument and propensity to receive democracy aid as the recipient instrument is used to instrument the number of democracy donors (models 1 and 2; these are equivalent to model 2, panels C and B in table 1 in the paper), absolute democracy aid amounts (models 3 and 4) and democracy aid per capita (models 5 and 6). Looking at the Kleibergen-Paap (KP) F-statistic of 32 for model 2, it becomes clear that the interacted instrument is suitable for the number of donors. For absolute democracy aid amounts, the KP F-statistic drops to about 4 (model 4), and for democracy aid per capita, the model breaks down and produces a KP F-statistic near zero and very large estimates (re-scaled by a factor of 1/1000 for presentation purposes; see model 6).

Table B4 reproduces the same scenario with the inverted distance between donor and recipient capitals as recipient instrument K, as suggested by Dietrich and Wright (2015). A similar pattern emerges, with the exception of per-capita democracy aid (models 5 and 6). Here, the KP F-statistic of around 6 could be considered just sufficient. The instrumented variable also produces statistically significant effects on the second stage. This is in line with the application of Dietrich and Wright (2015). They further corroborate their results by using 'inside' instruments which relax the exogeneity assumption (Lewbel 2012). Since I employ a range of alternative specifications, and I have evidence that their modified instrument is sufficiently strong for donor proliferation, I refrain from this approach. The KP F-statistic of 16 for using this instrument on the number of

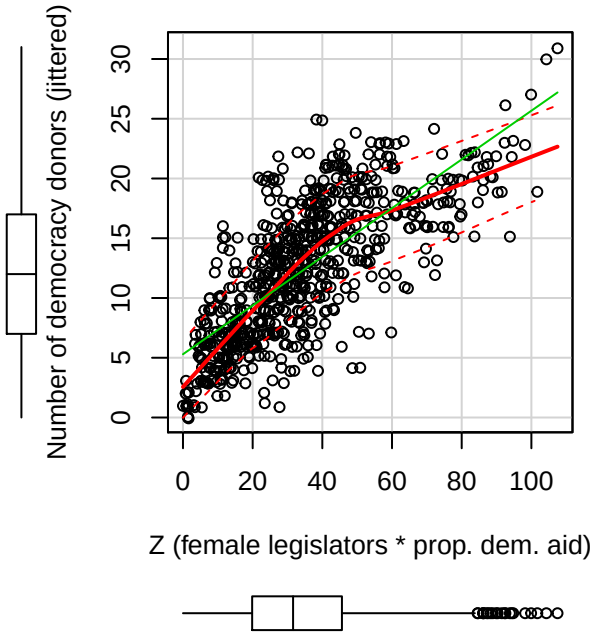

Figure B3: Interacted instrument and number of democracy donors (three-year averages, 1994–2013)

democracy donors generates more confidence than the KP F-statistic of 6 for democracy aid per capita.

Sector-specificity of the instruments

Table 2 in the paper (replicated below as table B5) uses the interaction approach to instrument various aid sectors and assesses whether they produce comparable effects on democracy. I find that only governance aid sectors generate statistically significant effects, and other sectors such as economic aid, health aid and energy aid do not. This increases my trust in the specificity of my instrument.

Adding a zero stage

Instead of simply aggregating the dyadic instrument onto the monadic level, Dreher and Langlotz (2015) employ a 'zero-stage' to explicitly model the expected amount of aid. They adapt the idea of a zero-stage from Frankel and Romer (1999) and estimate the amount of aid that a recipient receives from a particular donor in a particular year on the dyadic level from the interaction used above:

$$Aid_{ijt} = \gamma_1 Q_{jt} * K_{ij} + \varepsilon_{ijt},$$

Table B2: Donor proliferation and democracy, full reduced form estimates

	Model 1	Model 2	Model 3	Model 4
Z (WiP * Pr(dem. aid))	0.94*	0.97*	0.83*	
	[0.49; 1.38]	[0.54; 1.39]	[0.38; 1.27]	
Democracy aid (m USD; log)		2.08*	1.64*	
		[0.42; 3.75]	[0.24; 3.03]	
Number of economic donors			0.17	
			[−0.32; 0.66]	
Economic aid (m USD; log)			3.06	
			[−0.11; 6.23]	
Z (leg. frac. * Pr(aid))				5.66
				[−1.46; 12.78]
Aid (m USD; log)				5.45*
				[1.86; 9.03]
Pop. (log) x 10		−0.43	−0.85	0.35
		[−5.44; 4.59]	[−5.88; 4.18]	[−5.21; 5.90]
GDP p.c. (log) x 10		−1.05	−1.00	−0.21
		[−2.42; 0.31]	[−2.38; 0.38]	[−1.68; 1.26]
Civil conflict		−2.00	−2.13	−3.04
		[−7.33; 3.34]	[−7.18; 2.92]	[−8.15; 2.08]
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.93	0.93	0.93	0.93

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
all explanatory variables lagged by one period.

where *Aid* is the amount of aid given by donor j to recipient i at time t. The estimated amount is then summed to the recipient-period level and employed as the instrument on the first stage. In a balanced sample, both approaches are equivalent. As my sample is not balanced, and one might prefer a more intuitive modeling of the number of donors rather than the immediate application of the interaction term in the first stage, I adapt their approach to estimate the number of donors present in a recipient country in a particular time period. First, I use logistic regression on the donor-recipient-period level to estimate the probability that a donor is present:

$$Pr(Aid_{ijt} > 0 | Q_{jt}, K_{ij}) = \frac{1}{1 + exp(\gamma_1 Q_{jt} * K_{ij})}.$$

My instrument on the first stage is then the sum of these probabilities:

$$Z_{it}^{zero} = \sum_{j=1}^{n} Pr(Aid_{ijt} > 0 | Q_{jt}, K_{ij}).$$

The first and second stages are then equivalent to the interaction effects described above, with only slightly different estimates for D.

Table B6 replicates table 1 from the main article with a zero-stage added to the models that employ propensity as recipient instrument. The zero-stage estimates are provided

Table B3: Instrumenting the number of donors (models 1&2), aid amounts (3&4), or aid per capita (5&6) with propensity

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Z (WiP * Pr(dem. aid)) x 1/10	3.56*		−0.34*		0.01	
	[2.38; 4.75]		[−0.64; −0.05]		[−0.12; 0.14]	
No. of dem. donors (est.)		2.72*				
		[1.42; 4.02]				
Dem. aid (m USD; log) (est.)				−19.41		
				[−41.90; 3.08]		
Dem. aid (USD p.c.; log) (est.)						0.85
						[−12.96; 14.66]
No. of dem. donors			0.08*	2.40*	0.06*	−0.05
			[0.06; 0.11]	[0.58; 4.22]	[0.04; 0.08]	[−0.86; 0.76]
Dem. aid (m USD; log)	0.99*	−0.60				
	[0.33; 1.64]	[−2.13; 0.93]				
Pop. (log) x 10	0.33	−1.32	0.18	3.06	−0.09	0.07
	[−0.88; 1.54]	[−6.62; 3.99]	[−0.03; 0.39]	[−4.78; 10.91]	[−0.22; 0.05]	[−1.16; 1.31]
GDP p.c. (log) x 10	0.16	−1.50	0.03	−0.50	0.01	−0.01
	[−0.30; 0.63]	[−3.25; 0.24]	[−0.04; 0.10]	[−2.72; 1.72]	[−0.03; 0.05]	[−0.09; 0.08]
Civil conflict	0.26	−2.69	0.04	−1.26	−0.04	0.03
	[−1.18; 1.70]	[−7.07; 1.68]	[−0.23; 0.32]	[−9.03; 6.51]	[−0.19; 0.10]	[−0.54; 0.61]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Additional controls	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715	715	715
# of countries	133	133	133	133	133	133
R^2	0.90	0.91	0.76	0.73	0.80	-72.91
KP F-statistic		34.68		5.11		0.01

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
dependent variable in model 6 rescaled by 1/1000 for presentation purposes;
all explanatory variables lagged by one period.

Table B4: Instrumenting the number of donors (models 1&2), aid amounts (3&4), or aid per capita (5&6) with distance

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Z (WiP * inv. dist.) x 1/1000	2.14*		0.17		0.14*	
	[1.15; 3.13]		[−0.04; 0.38]		[0.03; 0.25]	
No. of dem. donors (est.)		3.39*				
		[1.24; 5.55]				
Dem. aid (m USD; log) (est.)				34.83		
				[−11.85; 81.50]		
Dem. aid (USD p.c.; log) (est.)						41.71*
						[3.47; 79.95]
No. of dem. donors			0.06*	−1.41	0.05*	−1.40
			[0.04; 0.09]	[−4.51; 1.69]	[0.04; 0.07]	[−3.59; 0.78]
Dem. aid (m USD; log)	0.80*	−1.24				
	[0.18; 1.43]	[−3.53; 1.04]				
Pop. (log) x 10	0.87	−2.03	0.11	−3.62	−0.09	4.26
	[−0.36; 2.11]	[−7.99; 3.94]	[−0.08; 0.31]	[−12.29; 5.05]	[−0.23; 0.04]	[−3.85; 12.37]
GDP p.c. (log) x 10	0.25	−1.81	−0.00	−1.05	−0.00	−0.94
	[−0.26; 0.77]	[−3.81; 0.20]	[−0.07; 0.06]	[−3.27; 1.17]	[−0.04; 0.04]	[−2.72; 0.84]
Civil conflict	−0.35	−2.54	0.08	−6.13	−0.05	−1.18
	[−1.70; 1.00]	[−7.17; 2.10]	[−0.21; 0.36]	[−16.06; 3.81]	[−0.20; 0.10]	[−8.13; 5.77]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Additional controls	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715	715	715
# of countries	133	133	133	133	133	133
R^2	0.90	0.89	0.76	0.38	0.80	0.79
KP F-statistic		18.02		2.38		6.01

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table B5: IV estimates with alternative and 'placebo' sectors

	Democracy	Democracy, no PB	Civil society	Economic	Health	Energy
No. of donors (est.)	2.72*	4.25*	2.63*	−3.08	−1.06	−0.41
	[1.42; 4.02]	[0.97; 7.53]	[0.87; 4.39]	[−14.75; 8.59]	[−5.42; 3.30]	[−4.54; 3.72]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Additional controls	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715	715	715
# of countries	133	133	133	133	133	133
R^2	0.91	0.86	0.91	0.87	0.91	0.92
KP F-statistic	34.68	6.55	16.16	0.95	6.36	7.43

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

in panel D. Models 1, 2 and 4a are equivalent and predict democracy donor proliferation with the interaction term of the share of women in donor legislatures and the propensity to receive democracy aid in the overall sample period 1994 to 2013. Model 4b predicts economic donor proliferation with the interaction term of legislative fractionalization in the donor country and the propensity to receive economic aid. Model 5 predicts general donor proliferation with the interaction term of legislative fractionalization in the donor country and the propensity to receive general aid. Model 3 does not include a zero stage, as it follows the Dietrich and Wright (2015) approach with a distance instrument. All zero-stage models suggest that the interaction terms of the respective donor-level and recipient-level instruments contribute significantly to predicting the presence of a donor in the respective sector. The sums of the estimated probabilities per recipient year are employed as instrumental variables in the first stages of models 1, 2, 4 and 5 (panel C). The estimates point estimates and confidence intervals for the coefficients are almost identical to those of table 1 in the main text.

Table B7 compares the results of the core IV models with and without zero stage side-by-side. The most salient difference is that the zero-stage model with controls has a somewhat smaller KP F-statistic and a somewhat larger point estimate. The general interpretation is almost identical. Figure B6 provides a scatter plot of both first-stage instruments. It shows that they covary strongly ($R^2 = 0.96$), with virtually no extreme outliers.

Discussing the assumptions

The main assumptions underlying the IV approach are discussed in the paper. Here, I will expand on the monotonicity assumption (Sovey and Green 2011: 199), which requires additional explanation due to the unusual nature of the interacted instrumental variable: the instruments are measured on the dyadic level, while the effect is estimated on the monadic level. Moreover, instruments, treatments and outcomes are continuous.

The assumption of monotonicity requires that the instrument does not produce any *defiers*. Defiers are, in potential-outcomes terminology, defined as subjects that do the opposite of what the incentive provided by the instrument suggests them to do.[3] *Compliers* are those cases that behave according to the incentive received through the instrument. *Always-takers* accept the treatment independent of the incentive; *never-takers* reject it independent of the incentive. Only if no defiers exist, and some compliers do exist, can the effect of the instrumented explanatory variable on the outcome be inter-

[3] See Morgan and Winship (2015: 305–324) for a discussion of the potential-outcomes terminology.

Table B6: Donor proliferation and democracy; estimated with a zero-stage

Dependent variable (panels A and B):	democracy					
	(1)	(2)	(3)	(4*a*)	(4*b*)	(5)
Aid sector:	*democracy*	*democracy*	*democracy*	*democracy & economic*		*general*
Panel A: OLS estimates						
Sector donor prolif.	1.03*	0.96*	0.96*	0.84*	0.02	0.49
	[0.52; 1.54]	[0.47; 1.46]	[0.47; 1.46]	[0.32; 1.37]	[−0.51; 0.55]	[−0.08; 1.06]
Sector aid (log)		1.09	1.09	0.79	3.51*	5.21*
		[−0.15; 2.32]	[−0.15; 2.32]	[−0.37; 1.95]	[0.79; 6.24]	[1.89; 8.53]
R^2	0.93	0.93	0.93	0.93		0.93
Panel B: 2SLS estimates						
Sector donor prolif. (est.)	2.73*	2.86*	3.39*	1.88*	1.03	1.20
	[1.45; 4.00]	[1.52; 4.21]	[1.24; 5.55]	[0.24; 3.52]	[−0.64; 2.70]	[−0.43; 2.83]
Sector aid (log)		−0.73	−1.24	−0.22	−0.70	3.28
		[−2.34; 0.87]	[−3.53; 1.04]	[−1.74; 1.30]	[−3.97; 2.58]	[−1.55; 8.11]
R^2	0.91	0.91	0.89	0.92		0.93
Dependent variable (panel C):	sector donor proliferation					
Panel C: First-stage estimates (for panel B)						
IV democr. donor prolif.	2.93*	2.76*	0.21*†	3.22*	1.33*	
	[1.96; 3.90]	[1.79; 3.72]	[0.12; 0.31]	[2.30; 4.13]	[0.60; 2.07]	
IV econ. donor prolif.				−0.77*	0.84*	
				[−1.23; −0.32]	[0.43; 1.26]	
IV general donor prolif.						1.11*
						[0.75; 1.48]
Democracy aid (log)		0.91*	0.80*	0.88*	0.18	
		[0.32; 1.49]	[0.18; 1.43]	[0.36; 1.40]	[−0.06; 0.43]	
Economic aid (log)				1.23*	1.29*	
				[0.04; 2.43]	[0.52; 2.06]	
General aid (log)						1.82*
						[0.76; 2.89]
KP F-statistic	34.99	31.05	18.02	24.32	31.45	35.51
IV type	*zero stage*	*zero stage*	*interaction*	*zero stage*	*zero stage*	*zero stage*
Donor IV	*WiP*	*WiP*	*WiP*	*WiP*	*Leg. fract.*	*Leg. fract.*
Recipient IV	*prop.*	*prop.*	*dist.*	*prop.*	*prop.*	*prop.*
Control variables (panels A, B and C):						
Covariates	*no*	*yes*	*yes*	*yes*		*yes*
Observations						
(panels A, B and C)	715	715	715	715		715
Recipients (all panels)	133	133	133	133		133
Dependent variable (panel D):	Aid_{sector} > 100,000 USD					
Panel D: zero-stage logistic regression estimates (for panel C)						
Donor * recipient IV	0.73*	0.73*	–	0.73*	27.25*	23.12*
	[0.67; 0.80]	[0.67; 0.80]	–	[0.67; 0.80]	[26.09; 28.40]	[22.14; 24.09]
Pseudo R^2	0.28	0.28	–	0.28	0.71	0.72
Dyads (panel D)	5,239	5,239	–	5,239		5,239
Obs. (panel D)	19,712	19,712		19,712		19,712

*) 95-percent confidence interval does not include zero [confidence intervals in brackets; clustered by recipient country].
†) coefficient and confidence interval scaled by factor 100 for presentation purposes.
Observation unit is a country three-year period. All explanatory variables in panels A, B and C are lagged by one period. All models include country and year fixed effects. Covariates include population (log), GDP per capita (log), and civil conflict (dummy).
Abbreviations and definitions: *IV:* instrumental variable; *(est.) donor prolif.:* (estimated) donor proliferation, i.e., number of donors providing more than 100,000 USD in a given year; *KP:* Kleibergen-Paap; *WiP:* share of women in donor parliament; *leg. fract.:* legislative fractionalization; *propensity:* propensity to receive aid in the respective sector within the overall time period; *distance:* distance between donor and recipient capitals in 1,000 kilometers, inverted.

Table B7: IV estimates with (models 2&4) and witout (1&3) zero stage

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors (est.)	2.58*	2.73*	2.72*	2.86*
	[1.26; 3.91]	[1.45; 4.00]	[1.42; 4.02]	[1.52; 4.21]
Dem. aid (m USD; log)			−0.60	−0.73
			[−2.13; 0.93]	[−2.34; 0.87]
Pop. (log) x 10			−1.32	−1.47
			[−6.62; 3.99]	[−7.18; 4.25]
GDP p.c. (log) x 10			−1.50	−1.57
			[−3.25; 0.24]	[−3.34; 0.21]
Civil conflict			−2.69	−2.66
			[−7.07; 1.68]	[−7.10; 1.77]
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*no*	*yes*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.91	0.91	0.91	0.91
KP F-statistic	32.39	34.99	34.68	31.05

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

preted as a causal effect. In practice, however, while one can observe which cases took the treatment after an incentive, one usually cannot tell whether they are compliers or always-takers. Accordingly, while one can observe which cases rejected the treatment after not being incentivized, one usually cannot tell whether they are compliers or never-takers. It is thus a theoretical task to argue that the monotonicity assumption is met ('there are no defiers'), and what cases the local average treatment effect (LATE) actually covers ('who are the compliers?').

I refer to instrument Q as the 'donor instrument', and instrument K as the 'recipient instrument', as this is where the indicators vary on the dyadic level. However, better understanding whether and how these instruments affect both parties of the donor-recipient dyad is prerequisite to assessing the validity of my claims that the interaction approach works for instrumenting democracy aid proliferation. Tables B8 to B9 dissect how both components of my instrument affect recipient and donor countries. Table B10 then shows how these effects aggregate to the dyadic level. The intention of these table is to show that compliers are likely to exist, while defiers are unlikely to exist.

Table B8 provides an overview of the potential outcomes that the instrument 'share of female legislators in the donor country' (Q_j) causes in recipient and donor countries. Note that for this illustration I must simplify the continuous instrument to one group of 'incentivized' and one of 'non-incentivized' cases. I do so by distinguishing a group of observations where the share of female legislators in the donor parliament has increased ($\Delta Q_j > 0$), and one where it has not ($\Delta Q_j \leq 0$). The second criterion for distinguishing potential outcomes is then whether a donor actually is present in the

respective recipient country at a specific point in time or not – the treatment I am interested in. As discussed in the paper, I consider a donor to be 'present' if it commits to at least 100,000 USD per year in democracy aid. Applying these two criteria, I obtain four quadrants representing the potential outcome types.

The donor never-taker just does not provide democracy aid, regardless of the share of legislators in the donor country. The recipient never-taker simply rejects admitting democracy aid, regardless of the share of legislators in the donor country. Both are likely scenarios. For always-takers, the situation is analogous: both donor and recipient always-takers are very likely to exist. Donor compliers are quite likely to exist, as argued in the paper: female legislators have been shown to have a preference for 'good' aid (Breuning 2001; Dietrich and Wright 2015). On the reverse side, this means that donors with less female legislators are on average less likely to extend democracy aid, *ceteris paribus*. A complier on the recipient level would be a country that accepts democracy aid only if this aid was sponsored by female legislators, and that rejects democracy aid only if it was not sponsored by female legislators. It would require a large amount of imagination to construct such situations. Recipient compliers are thus unlikely to exist. Do defiers exist? Donor defiers would be countries where female legislators are opposed to democracy aid, or where male legislators are disproportionately in favor of democracy aid. I cannot exclude this possibility for sure, but the aggregation process to monadic observations should level out such unlikely deviations. Recipients that would reject democracy aid only because it was sponsored by female legislators are hard to imagine. Those countries with the lowest esteem for female participation are autocracies that would anyway fall within the group of never-takers. The reverse idea of recipients only accepting aid when less women are present in the donor's parliament is similarly outrageous.

Table B9 provides an overview of the potential outcomes that the instrument 'propensity of democracy aid commitments' (K_{ij}) causes in recipient and donor countries. A non-incentive is here defined as 'almost no propensity' ($K_{ij} \approx 0$), an incentive through the instrument as 'high propensity' ($K_{ij} \gg 0$) – a necessary simplification that allows dealing with the continuous instrument. With the propensity instrument, both never-takers and always-takers are rather unlikely to occur: aid provision is very path-dependent, and if either donors or recipients have preferences or aversions (proxied by propensity), these are likely to persist. For that same reason, compliers exist with a high probability, and defiers are unlikely. Note that the 'strength' of the propensity instrument is

Table B8: Potential outcomes of the female legislators instrument on the dyadic level

		$\Delta Q_{jt} > 0$	
		$DemAid_{ijt} < 100,000$ USD	$DemAid_{ijt} > 100,000$ USD
$\Delta Q_{jt} \leq 0$	$DemAid_{ijt} < 100,000$ USD	***never-taker:*** *donor:* withholds democracy aid regardless of the share of female legislators (likely scenario) *recipient:* rejects democracy aid regardless of the share of female legislators in donor country (likely scenario)	***complier:*** *donor:* offers democracy aid only if share of female legislators in donor country is high, withholds democracy aid only if share of female legislators in donor country is low (likely scenario) *recipient:* accepts democracy aid only if share of female legislators in donor country is high, rejects democracy aid only if share of female legislators in donor country is low (unlikely scenario)
	$DemAid_{ijt} > 100,000$ USD	***defier:*** *donor:* withholds democracy aid only if share of female legislators in donor country is high, offers democracy aid only if share of female legislators in donor country is low (unlikely scenario) *recipient:* rejects democracy aid only if share of female legislators in donor country is high, accepts democracy aid only if share of female legislators in donor country is low (unlikely scenario)	***always-taker:*** *donor:* offers democracy aid regardless of the share of female legislators (likely scenario) *recipient:* accepts democracy aid regardless of the share of female legislators in donor country (likely scenario)

Q_{jt}: share of female legislators in donor j at time t

$DemAid_{ijt}$: amount of democracy aid that recipient i receives aid from donor j at time t

in part due to its endogeneity. But this endogeneity is removed from the final instrumental variable by the interaction with an exogenous instrument Q in combination with country fixed effects.

Table B10 brings both instruments together and summarizes their theorized effects on the monadic level, i.e., the recipient-period unit. But how are the effects of the two instruments best aggregated? One can imagine the process as a game with several rounds. In a first round, the donor experiences an increase in instrument Q, or not (column 2). In a second round, the donor can either offer aid, or not (column 3). This provides us with information on the potential outcome types on the donor side, as laid out in table B8 (column 4). In a third round, the recipient is confronted with recipient instrument K (column 5). As noted above, this stage is somewhat artificial and serves mainly the purpose of introducing variance across recipients and allowing the use of country fixed effects. The only likely reaction of the recipient on this stage is compliance (column 7). The interaction between donor offer and recipient acceptance produces the outcome on the monad level, i.e., the number of donors D (column 8). I aggregate the compliance of donors and recipients with these rules: I only consider compliance types that were judged to be likely in tables B8 and B9; the recipient's decision trumps the donor's decision if they are incompatible, as the former succeeds the latter; and if at least one of the parties only allows the conclusion that we have a complier and the outcome is according to the incentive, the monad is considered a complier. Note that while in tables B8 and B9 I discuss the effects of both instruments on both donor and recipient for capturing the whole picture, the synthesis focuses on the donor reaction to instrument Q and on the recipient reaction to instrument K, as this is the variation across donors and over time that I exploit in the first stage.

Looking at the potential configurations, cases 1 and 6 show that if the donor does not offer aid, the recipient cannot play. This reduces the total number of configurations to ten. In case 1, where the instrument suggests not giving democracy aid, we cannot tell whether the donor is a never-taker or a complier. This carries through to the

Table B9: Potential outcomes of the propensity instrument on the dyadic level

		$K_{ij} \gg 0$	
		$DemAid_{ijt} < 100,000$ USD	$DemAid_{ijt} > 100,000$ USD
$K_{ij} \approx 0$	$DemAid_{ijt} < 100,000$ USD	***never-taker:*** *donor:* withholds democracy aid regardless of the propensity to provide democracy aid to the recipient (unlikely scenario) *recipient:* rejects democracy aid regardless of the propensity to receive democracy aid from the offering donor (unlikely scenario)	***complier:*** *donor:* offers democracy aid only if the propensity to provide democracy aid to the recipient is high, withholds democracy aid only if the propensity to provide democracy aid to the recipient is low (likely scenario) *recipient:* accepts democracy aid only if the propensity to receive democracy aid from the donor is high, rejects democracy aid only if the propensity to receive democracy aid from the donor is low (likely scenario)
	$DemAid_{ijt} > 100,000$ USD	***defier:*** *donor:* withholds democracy aid only if the propensity to provide democracy aid to the recipient is high, offers democracy aid only if the propensity to provide democracy aid to the recipient is low (unlikely scenario) *recipient:* rejects democracy aid only if the propensity to receive democracy aid from the donor is high, accepts democracy aid only if the propensity to receive democracy aid from the donor is low (unlikely scenario)	***always-taker:*** *donor:* offers democracy aid regardless of the propensity to provide democracy aid to the recipient (unlikely scenario) *recipient:* accepts democracy aid regardless of the propensity to receive democracy aid from the offering donor (unlikely scenario)

K_{ij}: propensity of donor j providing aid to recipient i over the whole period under study
$DemAid_{ijt}$: amount of democracy aid that recipient i receives aid from donor j at time t

monad type in the last column. In case 6, where the instrument suggests giving aid, but none is given, the outcome is most likely due to a never-taker, as defiance to the instrument is considered unlikely. In cases 3 and 8, the donor offers aid, but the recipient instrument suggests that the recipient should not take it. As I assume that both always-takers and defiers are an unlikely scenario for the propensity instrument, I conclude that these configurations rarely occur in practice. Cases 4 and 9 mirror this setting with a favorable propensity and a rejection by the recipient, which is why I also consider them unlikely. Cases 2, 5, 7 and 10 combine donor always-takers with recipient compliers (since never-takers and always-takers are unlikely on the recipient side for the propensity instrument). I conclude that these cases can be considered compliers on the monadic level. In sum, this suggests that on the monadic level we only deal with never-takers and compliers. This would mean that the LATE I am estimating is rather broad, as almost all cases with many donors would be considered compliers. Such a conclusion would, however, overburden this exercise. One could, for example, imagine that some recipients that had not received democracy aid previously would happily accept some due to recent domestic changes. The probability of observing always-takers on the recipient side would also be much higher in the case of distance as the recipient instrument, which partly removes the issue of path dependency. But the main goal pursued here was to argue that the likelihood that compliers exists is very high, and that the likelihood that defiers exist is very low at the aggregate monadic level of my instrumental variable. I thus conclude that these are reasonable assumptions.

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Table B10: Potential outcomes translated to the monadic level

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Case	Donor instrument $\Delta Q_{jt} > 0$	Donor offers dem. aid	Donor types	Recipient instrument $\Delta K_{ij} > 0$	Recipient accepts dem. aid	Recipient types	Monad outcome $\Delta D_{it} > 0$	Likely monad types
1	no	no	n, c	*irrel.*	*irrel.*	-	no	n, c
2	no	yes	a, *d*	no	no	*n*, c	no	c
3	no	yes	a, *d*	no	yes	*a*, *d*	yes	-
4	no	yes	a, *d*	yes	no	*n*, *d*	no	-
5	no	yes	a, *d*	yes	yes	*a*, c	yes	c
6	yes	no	n, *d*	*irrel.*	*irrel.*	-	no	n
7	yes	yes	a, c	no	no	*n*, c	no	c
8	yes	yes	a, c	no	yes	*a*, *d*	yes	-
9	yes	yes	a, c	yes	no	*n*, *d*	no	-
10	yes	yes	a, c	yes	yes	*a*, c	yes	c

Q_{jt}: share of female legislators in donor j at time t

K_{ij}: propensity that i receives aid from donor j in the period under study

D_{it}: number of donors present in recipient i at time t

a: always-taker; *c:* complier; *d:* defier; *n:* never-taker

letters in italics indicate that the potential outcome is unlikely

hyphens (-) indicate that outcome is not expected to occur

irrel.: recipient instrument and reaction irrelevant, since no aid was offered

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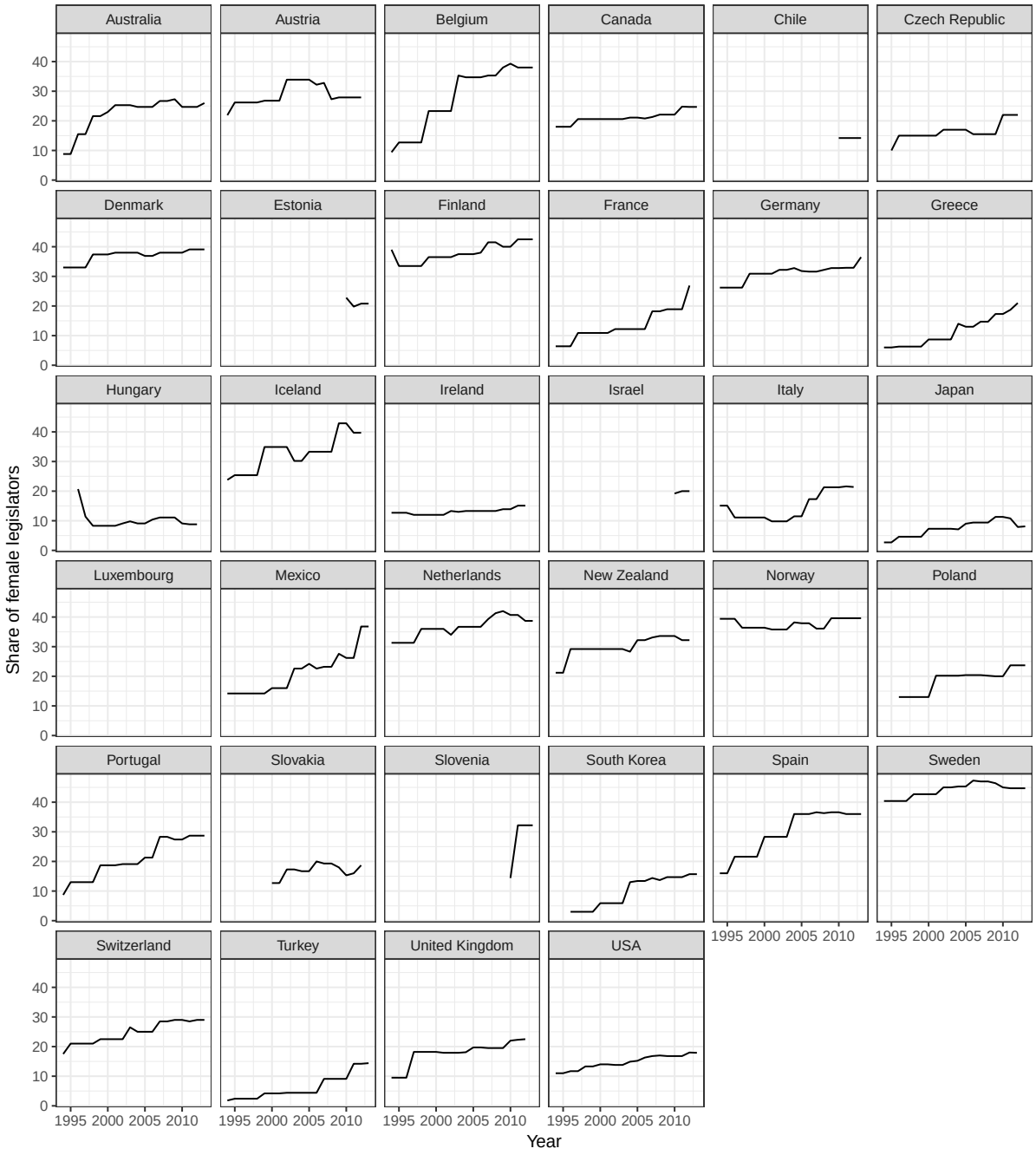

Figure B4: Variation in the share of female legislators in donor parliaments, 1994–2013 (levels)

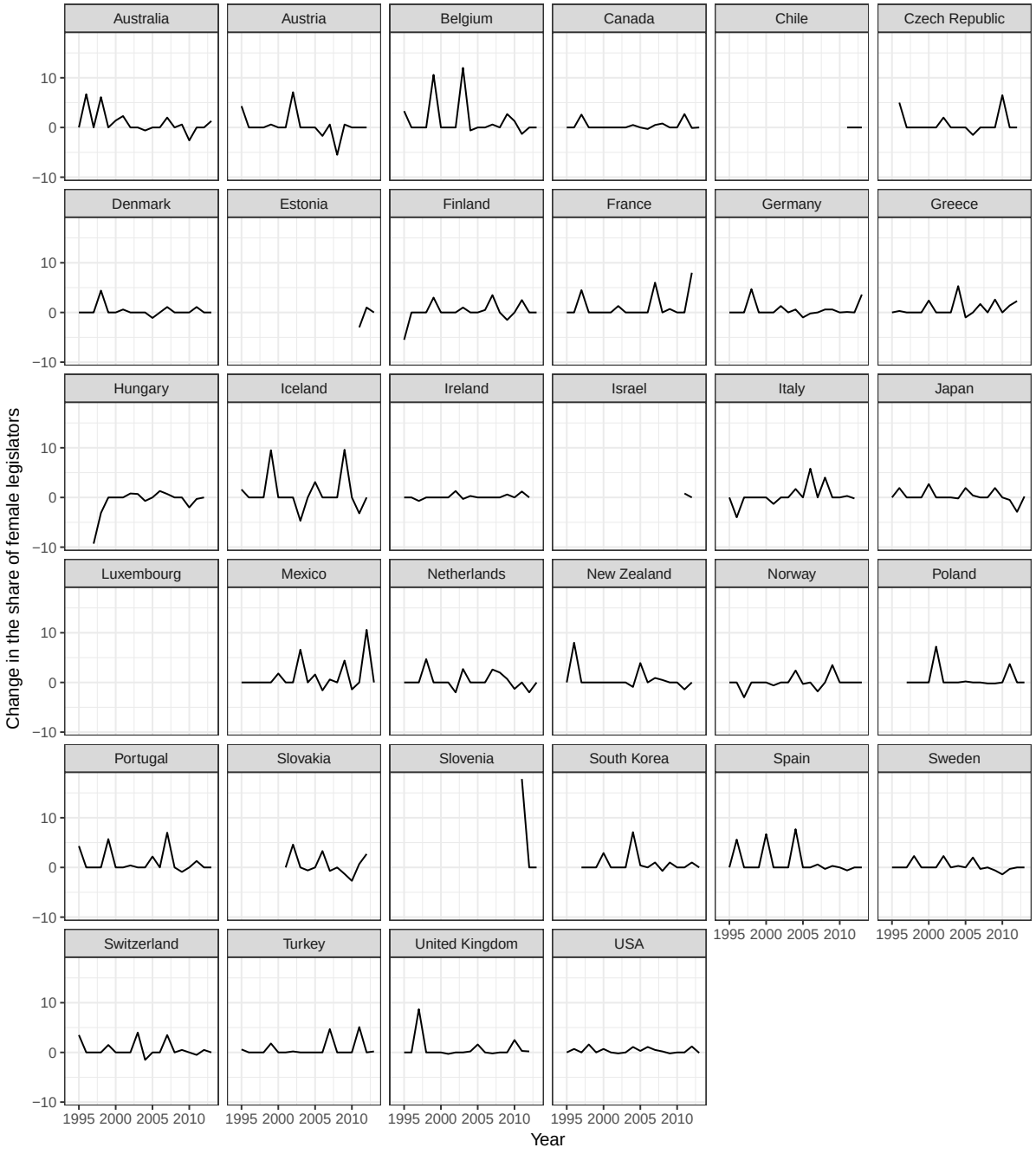

Figure B5: Variation in the share of female legislators in donor parliaments, 1994–2013 (changes)

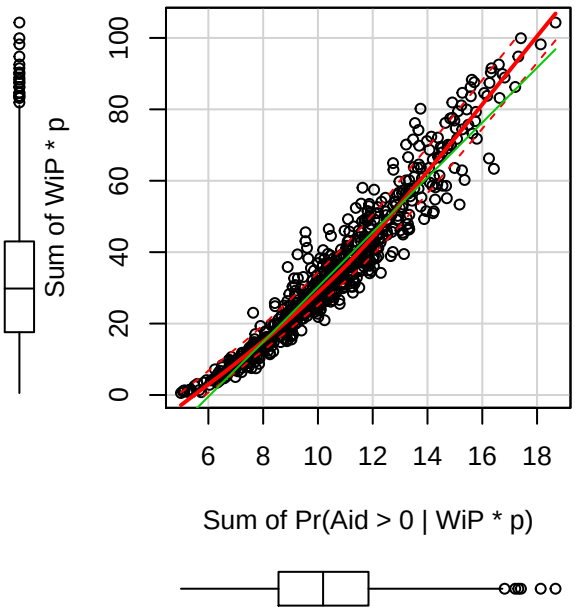

Figure B6: Scatterplot of zero-stage and interaction instruments (*WiP:* women in donor parliament; p: propensity to receive democracy aid)

More donors, more democracy

Appendix C: Robustness checks

Contents

Table C1: Base models

	Model 1	Model 2	Model 3	Model 4
Number of democracy donors	1.03*	0.84*		
	[0.52; 1.54]	[0.32; 1.37]		
Number of democracy donors (estimated)			2.58*	2.72*
			[1.26; 3.91]	[1.42; 4.02]
Number of economic donors		0.02		
		[−0.51; 0.55]		
Democracy aid (m USD; log)		0.79		−0.60
		[−0.37; 1.95]		[−2.13; 0.93]
Economic aid (m USD; log)		3.51*		
		[0.79; 6.24]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.93	0.93	0.91	0.91
KP F-statistic			32.39	34.68

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Base models

Table C1 provides results for the models that constitute the starting point for the robustness checks presented in this appendix. They comprise four models testing the main hypothesis that the number of democracy donors affects democracy in the recipient country. Model 1 is the OLS specification without controls, model 2 the OLS specification with controls (including the number of economic aid donors). These models are equivalent to models 1 and 4 in panel A in table 1 in the main article. Model 3 is the instrumental variable specification without controls, employing the interaction of women in the donor parliament with the propensity to receive democracy aid as instrument. Model 4 is the same specification with controls. These models are equivalent to models 1 and 2 in panel B in table 1 in the main article. Figures C1 and C2 present graphical impressions on the estimated effects of interest for OLS model 2 and IV model 4, corresponding to figure 3 in the paper.

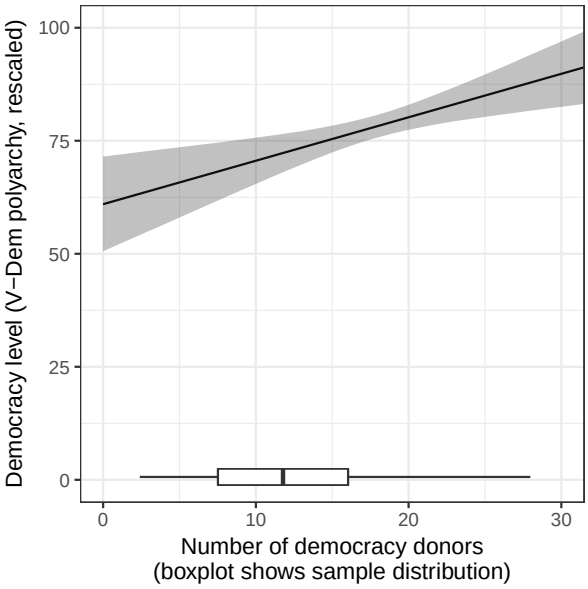

Figure C1: Expected levels of democracy conditional on the number of democracy donors; OLS model 2; covariates set to the levels of Ghana in the 2012 period

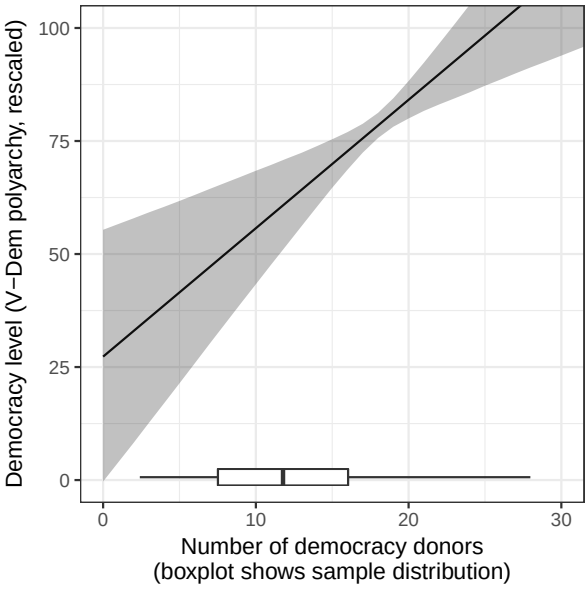

Figure C2: Expected levels of democracy conditional on the number of democracy donors; IV model 4; covariates set to the levels of Ghana in the 2012 period

Table C2: Donor proliferation and democracy, full OLS models

	Model 1	Model 2	Model 3	Model 4	Model 5
Number of democracy donors	1.03*	0.96*	0.96*	0.84*	
	[0.52; 1.54]	[0.47; 1.46]	[0.47; 1.46]	[0.32; 1.37]	
Democracy aid (m USD; log)		1.09	1.09	0.79	
		[−0.15; 2.32]	[−0.15; 2.32]	[−0.37; 1.95]	
Number of economic donors				0.02	
				[−0.51; 0.55]	
Economic aid (m USD; log)				3.51*	
				[0.79; 6.24]	
Number of donors					0.49
					[−0.08; 1.06]
Aid (m USD; log)					5.21*
					[1.89; 8.53]
Pop. (log) x 10		0.54	0.54	−0.11	0.64
		[−4.75; 5.82]	[−4.75; 5.82]	[−5.43; 5.21]	[−4.79; 6.07]
GDP p.c. (log) x 10		−0.71	−0.71	−0.71	−0.33
		[−2.14; 0.73]	[−2.14; 0.73]	[−2.15; 0.74]	[−1.78; 1.12]
Civil conflict		−3.10	−3.10	−2.98	−3.31
		[−7.95; 1.75]	[−7.95; 1.75]	[−7.69; 1.72]	[−8.37; 1.76]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715	715
# of countries	133	133	133	133	133
R^2	0.93	0.93	0.93	0.93	0.93

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
all explanatory variables lagged by one period.

Full regression tables for models from the main text

Tables C2 to C4 provide the full regression tables for all models that are presented in table 1 in the main text in a compressed form.

Table C3: Donor proliferation and democracy, IV first stages

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Z (WiP * Pr(dem. aid))	0.36*	0.36*		0.32*	0.12*	
	[0.24; 0.49]	[0.24; 0.47]		[0.21; 0.44]	[0.04; 0.20]	
Z (WiP * inv. dist.) x 1/100			0.21*			
			[0.12; 0.31]			
Z (leg. frac. * Pr(econ. aid))				−0.97	4.07*	
				[−2.84; 0.90]	[2.57; 5.57]	
Z (leg. frac. * Pr(aid))						3.68*
						[2.24; 5.11]
Democracy aid (m USD; log)		0.99*	0.80*	0.87*	0.17	
		[0.33; 1.64]	[0.18; 1.43]	[0.31; 1.44]	[−0.09; 0.42]	
Economic aid (m USD; log)				1.27*	1.52*	
				[0.02; 2.52]	[0.72; 2.33]	
Aid (m USD; log)						2.00*
						[0.94; 3.07]
Pop. (log) x 10		0.33	0.87	0.18	−0.59	−0.44
		[−0.88; 1.54]	[−0.36; 2.11]	[−1.01; 1.37]	[−1.58; 0.41]	[−1.46; 0.58]
GDP p.c. (log) x 10		0.16	0.25	0.16	0.04	0.11
		[−0.30; 0.63]	[−0.26; 0.77]	[−0.28; 0.60]	[−0.27; 0.35]	[−0.20; 0.43]
Civil conflict		0.26	−0.35	0.26	0.65	0.60
		[−1.18; 1.70]	[−1.70; 1.00]	[−0.98; 1.51]	[−0.15; 1.44]	[−0.36; 1.56]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715	715	715
# of countries	133	133	133	133	133	133
R^2	0.89	0.90	0.90	0.91	0.92	0.92

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
all explanatory variables lagged by one period.

Table C4: Donor proliferation and democracy, IV second stages

	Model 1	Model 2	Model 3	Model 4	Model 5
No. of dem. donors (est.)	2.58*	2.72*	3.39*	1.98*	
	[1.26; 3.91]	[1.42; 4.02]	[1.24; 5.55]	[0.31; 3.66]	
Democracy aid (m USD; log)		−0.60	−1.24	−0.40	
		[−2.13; 0.93]	[−3.53; 1.04]	[−2.01; 1.22]	
Economic aid (m USD; log)				−1.97	
				[−5.98; 2.03]	
No. of econ. donors (est.)				1.50	
				[−0.43; 3.42]	
Aid (m USD; log)					2.36
					[−3.44; 8.16]
No. of donors (est.)					1.54
					[−0.51; 3.59]
Pop. (log) x 10		−1.32	−2.03	−0.50	1.03
		[−6.62; 3.99]	[−7.99; 3.94]	[−5.75; 4.74]	[−4.13; 6.19]
GDP p.c. (log) x 10		−1.50	−1.81	−1.28	−0.38
		[−3.25; 0.24]	[−3.81; 0.20]	[−3.18; 0.63]	[−1.84; 1.08]
Civil conflict		−2.69	−2.54	−3.54	−3.96
		[−7.07; 1.68]	[−7.17; 2.10]	[−8.06; 0.97]	[−8.71; 0.79]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715	715
# of countries	133	133	133	133	133
R^2	0.91	0.91	0.89	0.91	0.92
KP F-statistic	32.39	34.68	18.02	15.08	25.16
...for the second instrument				21.64	

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;

dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];

all explanatory variables lagged by one period.

Table C5: Base models with OECD disbursement data instead of AidData commitment data

	Model 1	Model 2	Model 3	Model 4
Number of democracy donors	0.80*	0.81*		
	[0.28; 1.32]	[0.21; 1.42]		
Number of democracy donors (estimated)			1.37*	1.47*
			[0.40; 2.34]	[0.45; 2.50]
Number of economic donors		−0.34		
		[−0.96; 0.29]		
Democracy aid (m USD; log)		−0.20		0.01
		[−1.68; 1.29]		[−1.51; 1.53]
Economic aid (m USD; log)		4.26*		
		[0.19; 8.33]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	668	668	668	668
# of countries	121	121	121	121
R^2	0.92	0.92	0.91	0.91
KP F-statistic			59.20	70.81

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Alternative indicators

Aid flow types: commitments and disbursements

In the main paper, I employ commitment data from the AidData project due to superior coverage compared to the OECD's disbursement data available from the Creditor Reporting System (CRS). I also argue that commitments are a good proxy for donor's intentions and engagement over the three-year time periods used here. Nonetheless, one would expect the mechanism to be visible with disbursement data from the CRS as well. Table C5 presents the corresponding results. The disbursement variables are constructed exactly like the corresponding commitments variables, with the exception of the data source. The hypothesis is again confirmed by all models, albeit with much smaller coefficients in the IV specification. When including commitment and disbursement data simultaneously in the OLS models, the number of donors providing democracy aid commitments is significant, the number of those providing democracy aid disbursements is not (table C6, models 1 to 2). This latter finding is difficult to interpret substantially, but the robustness of the relationship of the *number of donors committing democracy aid* with democracy becomes evident.

My universe of donors includes the eight largest multilateral organizations. Restricting the analysis to only bilateral donors (30 countries) does not affect the results substantially (table C7).

Table C6: Base OLS models with both OECD disbursement data and AidData commitment data

	Model 1	Model 2
Number of democracy donors, com.	0.91*	0.60*
	[0.34; 1.48]	[0.08; 1.13]
Number of democracy donors, disb.	0.15	0.34
	[−0.41; 0.72]	[−0.32; 1.01]
Number of economic donors, com.		0.32
		[−0.29; 0.93]
Number of economic donors, disb.		−0.41
		[−1.09; 0.26]
Democracy aid (m USD; log), com.		1.95*
		[0.04; 3.87]
Democracy aid (m USD; log), disb.		−1.47
		[−3.18; 0.23]
Economic aid (m USD; log), com.		2.32
		[−0.83; 5.47]
Economic aid (m USD; log), disb.		2.14
		[−1.72; 6.00]
Country fixed effects	*yes*	*yes*
Period fixed effects	*yes*	*yes*
Additional controls	*no*	*yes*
# of observations	668	668
# of countries	121	121
R^2	0.92	0.93

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C7: Base models with bilateral donors only

	Model 1	Model 2	Model 3	Model 4
Number of democracy donors	1.07*	0.98*		
	[0.51; 1.63]	[0.41; 1.56]		
Number of democracy donors (estimated)			2.45*	2.63*
			[0.94; 3.96]	[1.16; 4.09]
Number of economic donors		−0.22		
		[−0.89; 0.44]		
Democracy aid (m USD; log)		0.17		−0.63
		[−0.76; 1.11]		[−2.05; 0.78]
Economic aid (m USD; log)		3.74*		
		[0.75; 6.74]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.93	0.93	0.92	0.91
KP F-statistic			26.73	28.11

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C8: OLS estimates with alternative and 'placebo' sectors

	Democracy	Democracy, no PB	Civil society	Economic	Health	Energy
No. of donors	0.96*	0.91*	0.83*	0.38	0.59*	−0.32
	[0.47; 1.46]	[0.43; 1.40]	[0.37; 1.30]	[−0.15; 0.92]	[0.09; 1.08]	[−0.81; 0.17]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
Additional controls	*yes*	*yes*	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715	715	715
# of countries	133	133	133	133	133	133
R^2	0.93	0.93	0.93	0.93	0.92	0.92

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Aid sectors: alternatives and 'placebos'

Table C8 repeats the analysis of table 2 from the paper for the OLS specification with control variables. Here, health aid unexpectedly reaches a positive and significant relationship, but all other substantive interpretations remain the same: governance aid proliferation is related to improved democracy in the recipient country; other aid sector proliferation is not.

Table C9: Aid controls per GDP and per capita

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	0.86*	0.68*		
	[0.30; 1.41]	[0.14; 1.21]		
No. of dem. donors (est.)			2.69*	2.86*
			[1.16; 4.22]	[1.28; 4.44]
No. of econ. donors	0.03	0.01		
	[−0.40; 0.46]	[−0.48; 0.50]		
Dem. aid (percent of GDP; log)	79.51		9.39	
	[−20.66; 179.68]		[−123.33; 142.12]	
Econ. aid (percent of GDP; log)	15.29			
	[−5.10; 35.68]			
Dem. aid (per capita; log)		4.15*		−2.12
		[1.67; 6.62]		[−7.11; 2.86]
Econ. aid (per capita; log)		3.26*		
		[0.65; 5.86]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.93	0.93	0.91	0.91
KP F-statistic			22.61	28.39

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
all explanatory variables lagged by one period.

Aid amount references: per GDP and per capita

The choice of including absolute aid amounts as a control for aid flows in my main specification could be questioned. Table C9 gives results when controlling for aid as a share of GDP and aid per capita instead (in base models 2 and 4, respectively; see table C1). The confidence intervals of all estimates of the effects of the number of democracy donors exclude zero. The estimated coefficients are of almost the same sizes as in the base specifications.

Table C10: Base models with fragmentation measured by the number of small donors

	Model 1	Model 2	Model 3	Model 4
Number of small dem. donors	0.78*	0.59*		
	[0.31; 1.25]	[0.18; 1.01]		
Number of small dem. donors (estimated)			2.80*	2.87*
			[1.29; 4.31]	[1.44; 4.30]
Number of small econ. donors		0.18		
		[−0.29; 0.64]		
Democracy aid (m USD; log)		0.87		−1.04
		[−0.34; 2.08]		[−3.09; 1.01]
Economic aid (m USD; log)		3.83*		
		[0.83; 6.82]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.93	0.93	0.89	0.89
KP F-statistic			28.75	30.91

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Aid fragmentation indicators

As I argue in the article, certain indicators of fragmentation are more suited to express the theoretical constructs that underlie my argument than others. My preferred indicator to measure the diversity of ideas and positions provided by donors is the number of democracy donors. Nonetheless, I here show results for specifications employing three alternative fragmentation measures as suggested by Gehring et al. (2017): the number of small donors only (i.e., donors that provide less than 10 percent of the overall aid flows to a recipient), the inverted Herfindahl-Hirschman concentration index (HHI) as a measure of financial fragmentation, and the share of aid covered by the three biggest donors. Table C10 presents the results of the base models with the number of small donors replacing my original fragmentation variables. Direction and significance of the coefficients remain essentially unchanged.

The inverted HHI is calculated as one minus the sum of squares of the aid shares s of all donors j that report aid commitments in the respective recipient country i and sector r in period t (Hirschman 1964):

$$fragmentation_{irt} = 1 - \sum_{j=1}^{N} s_{jirt}^2.$$

Table C11 replaces the number of democracy donors with the democracy aid fragmentation index. None of the models produces significant estimates for our variable of interest, the coefficients carry the wrong signs, and the IV models break down with KP

Table C11: Base models with fragmentation measured by the HHI fragmentation index

	Model 1	Model 2	Model 3	Model 4
Dem. donor fragm. index	−3.37	−2.98		
	[−9.15; 2.41]	[−8.11; 2.14]		
Dem. donor fragm. index (estimated) x 100			−3.63	−2.92
			[−13.19; 5.92]	[−9.39; 3.54]
Econ. donor fragm. index		0.36		
		[−0.15; 0.88]		
Democracy aid (m USD; log)		1.26		−7.24
		[−0.19; 2.71]		[−32.17; 17.70]
Economic aid (m USD; log)		4.17*		
		[1.32; 7.01]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.92	0.93	-3.58	-1.94
KP F-statistic			0.57	0.80

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

F-statistics very low. I do not consider this corrosive to my argument, as the number of donors has a much more straightforward link to the concept of diversity; my theory explicitly rejects the notion that higher amounts transmit more ideas, and the threshold of 100,000 USD per year for donors to be counted ensures that only donors with a notable presence are considered. Tables C37 and C36 further below investigate the *interaction* between the number of donors and the amount of aid more explicitly.

Table C12 gives the results for the concentration ratio. Donor proliferation coefficients in all models display negative coefficients, which is in line with my expectations, as larger concentration should result in less diversity and thus less positive effects on democracy. The confidence intervals exclude zero in all cases, except for the bivariate OLS regression (model 1). The KP F-statistics of the IV models, however, are very low. Nonetheless, these results increase my confidence in the main results. While I consider the number of donors to be the best representation of diversity on the donor side, two out of three alternative fragmentation indicators produce similar results.

Table C12: Base models with fragmentation measured by the concentration ratio

	Model 1	Model 2	Model 3	Model 4
Dem. donor conc. rate	−1.29	−8.55*		
	[−5.89; 3.31]	[−16.72; −0.39]		
Dem. donor conc. rate (estimated) x 100			−1.47*	−1.83*
			[−2.90; −0.03]	[−3.64; −0.01]
Econ. donor conc. rate		0.27		
		[−0.23; 0.76]		
Democracy aid (m USD; log)		2.45*		23.91*
		[0.57; 4.34]		[2.09; 45.72]
Economic aid (m USD; log)		3.93*		
		[0.98; 6.88]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.92	0.93	0.51	0.58
KP F-statistic			5.36	4.23

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C13: Base models with the Polity IV polity2 index as dependent variable

	Model 1	Model 2	Model 3	Model 4
Number of democracy donors	0.97*	0.92*		
	[0.33; 1.60]	[0.34; 1.50]		
Number of democracy donors (estimated)			2.76*	2.84*
			[0.80; 4.71]	[0.94; 4.74]
Number of economic donors		−0.22		
		[−0.85; 0.40]		
Democracy aid (m USD; log)		−0.01		−1.38
		[−2.65; 2.62]		[−4.52; 1.76]
Economic aid (m USD; log)		3.81		
		[−1.47; 9.09]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	701	701	701	701
# of countries	131	131	131	131
R^2	0.91	0.91	0.89	0.90
KP F-statistic			25.22	29.49

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: Polity IV democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Democracy indicators

My choice of V-Dem polyarchy as the main dependent variable is somewhat uncommon in empirical democracy research, since V-Dem was released only recently. Tables C13 to C16 present the base models with alternative dependent variables. Table C13 shows the results for the most common indicator, the *polity2* variable provided by the Polity IV project (Marshall, Gurr, and Jaggers 2016). Results remain almost exactly the same as in the base specifications, although I lose 14 observations compared to sample using the V-Dem indicator. Table C13 shows results for the interpolated Polity IV index as suggested by Plümper and Neumayer (2010). Again, results do not change substantially. Table C15 shows results for the aggregate Freedom House index (Freedom House 2017). Again, coefficients and confidence intervals remain almost identical to the base models. Only the Unified Democracy Scale (UDS; Pemstein, Meserve, and Melton 2010), a meta index consisting of ten democracy indices published by different sources, generates smaller estimates that are nonetheless statistically significant. One explanation for the smaller size of the estimates could be the smaller variance found in the UDS: on the normalized scale of 0 to 100, the standard deviation reaches only 17, compared to 25 to 31 in the other democracy indices. Note, however, that all alternative democracy indicators produce consistently significant results in line with the main specifications.

Table C14: Base models with the interpolated Polity IV index as dependent variable

	Model 1	Model 2	Model 3	Model 4
Number of democracy donors	0.84*	0.79*		
	[0.21; 1.47]	[0.23; 1.35]		
Number of democracy donors (estimated)			2.64*	2.68*
			[0.68; 4.60]	[0.75; 4.61]
Number of economic donors		−0.14		
		[−0.77; 0.49]		
Democracy aid (m USD; log)		−0.17		−1.52
		[−2.74; 2.40]		[−4.64; 1.60]
Economic aid (m USD; log)		3.35		
		[−2.07; 8.77]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	701	701	701	701
# of countries	131	131	131	131
R^2	0.91	0.91	0.90	0.90
KP F-statistic			25.22	29.49

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: Polity IV interpolated democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C15: Base models with the Freedom House index as dependent variable

	Model 1	Model 2	Model 3	Model 4
Number of democracy donors	0.83*	0.63*		
	[0.39; 1.26]	[0.14; 1.11]		
Number of democracy donors (estimated)			2.64*	2.61*
			[1.15; 4.13]	[1.06; 4.16]
Number of economic donors		−0.05		
		[−0.52; 0.41]		
Democracy aid (m USD; log)		1.07*		−0.47
		[0.24; 1.90]		[−2.20; 1.27]
Economic aid (m USD; log)		3.43*		
		[0.54; 6.32]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.93	0.93	0.91	0.91
KP F-statistic			32.39	34.68

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: Freedom House democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C16: Base models with the Unified Democracy Scale (UDS) as dependent variable

	Model 1	Model 2	Model 3	Model 4
Number of democracy donors	0.43*	0.34*		
	[0.13; 0.73]	[0.05; 0.64]		
Number of democracy donors (estimated)			1.18*	1.22*
			[0.38; 1.97]	[0.36; 2.09]
Number of economic donors		−0.10		
		[−0.37; 0.17]		
Democracy aid (m USD; log)		0.36		−0.22
		[−0.72; 1.45]		[−1.49; 1.04]
Economic aid (m USD; log)		2.71*		
		[0.46; 4.96]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.94	0.94	0.93	0.93
KP F-statistic			32.39	34.68

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;

dependent variable: UDS democracy index scaled to a range of [0; 100];

additional controls include population (log), GDP per capita (log) and a civil conflict dummy;

all explanatory variables lagged by one period.

Table C17: OLS estimates with V-Dem component indices as dependent variables

	Liberal	Participatory	Deliberative	Egalitarian
No. of dem. donors	0.74*	0.62*	0.88*	0.28*
	[0.30; 1.18]	[0.27; 0.97]	[0.34; 1.43]	[0.07; 0.49]
No. of econ. donors	−0.05	−0.11	−0.01	−0.03
	[−0.56; 0.46]	[−0.37; 0.16]	[−0.65; 0.63]	[−0.31; 0.25]
Dem. aid (m USD; log)	0.52	0.26	0.33	0.11
	[−0.40; 1.44]	[−0.44; 0.96]	[−1.32; 1.97]	[−0.32; 0.54]
Econ. aid (m USD; log)	2.36	2.95	4.23	0.36
	[−0.74; 5.45]	[−0.19; 6.10]	[−0.50; 8.96]	[−0.96; 1.68]
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.95	0.95	0.90	0.98

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Sub-components of democracy

A look at sub-components of democracy may shed additional light on whether the suggested causal mechanism is plausible (cp. Lührmann, McMann, and Van Ham 2017). Table C17 provides results of OLS model 2 for sub-components of democracy as measured by the V-Dem project (Coppedge, Gerring, Lindberg, Skaaning, and Teorell 2017). Table C18 conducts the same exercise for IV model 3 without control variables. (Including controls does not change the results substantively.) It shows that both the 'liberal principle' index, which measures individual rights as well as checks and balances, and the 'participatory principle' index, which measures the degree of participation and direct democracy, produce estimates with confidence intervals that exclude zero in both OLS and IV specifications. The 'deliberative principle' index, which measures the rationality and civility of political debate, and the 'egalitarian principle' index, which measures the ability of individuals to become politically active, only reach significant levels in the OLS specification. These findings resonate well with my main argument: diversity on the donor side is likely to improve democratic practice (liberal principle) and organized participation (participatory principle). It is less likely to change norms and practices in the short run (deliberative principle) or reduce political inequality of individuals (egalitarian principle).

Table C18: IV estimates with V-Dem component indices as dependent variables

	Liberal	Participatory	Deliberative	Egalitarian
No. of dem. donors (est.)	1.90*	1.81*	1.28	0.26
	[0.61; 3.20]	[0.85; 2.77]	[−0.15; 2.71]	[−0.40; 0.93]
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*no*	*no*	*no*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.94	0.94	0.90	0.98
KP F-statistic	32.39	32.39	32.39	32.39

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Indicators of democratic diversity

The causal mechanism suggests that donor diversity improves democracy in the recipient country by fostering local diversity. Tables C19 and C20 provide OLS and IV results for specifications that model this relationship more closely: the rather general dependent variable 'democracy' is here substituted by indicators of democratic diversity provided by the V-Dem project (Coppedge, Gerring, Lindberg, Skaaning, Teorell, et al. 2017). The first indicator considered here is the share of seats won by the largest party (*v2ellostlg* in the V-Dem database). One might expect that political diversity increases the chances of parties other than the largest (and presumably ruling) party in elections . In both the OLS and the IV specification, the coefficient for the share of the largest party is negative, as expected, but it does not reach significance (model 1). Note that the sample size decreases by almost 50 percent for this indicator, as data is only recorded for countries with partisan elections. The number of effective and distinctive party platforms (*v2psplats*), however, does increase significantly in both specifications (model 2). Respect for counterarguments (*v2dlcountr*) also increases when more donors are present, although not significantly when using the IV specification (model 3). The same holds for 'engaged society' (*v2dlengage*), an indicator on how 'wide and how independent' public deliberations are (model 4). The 'the involvement of people in civil society organizations' that measures both the diversity and the activity of civil society (*v2csprtcpt*), however, is again significant in both specifications (model 5). These results supports the notion that donor proliferation actually improves democracy by increasing the diversity of political organizations the recipient country, both in the realm of political parties and in the realm of civil society organizations. Electoral results and deliberative practice do not appear to be robustly affected.

Table C19: OLS estimates with V-Dem indicators of democratic diversity

	Largest party share	Party platforms	Counterarguments	Engaged society	CSO environment
No. of dem. donors	−0.02	0.03*	0.03*	0.03*	0.03*
	[−0.98; 0.94]	[0.01; 0.04]	[0.01; 0.06]	[0.01; 0.06]	[0.01; 0.06]
No. of econ. donors	−0.74	0.00	0.00	0.01	0.01
	[−2.09; 0.61]	[−0.01; 0.01]	[−0.03; 0.04]	[−0.02; 0.03]	[−0.01; 0.03]
Dem. aid (m USD; log)	−1.55	0.04	0.04	0.01	0.01
	[−5.10; 2.01]	[−0.01; 0.08]	[−0.03; 0.12]	[−0.06; 0.09]	[−0.05; 0.07]
Econ. aid (m USD; log)	−2.29	0.08	0.21	0.21	0.19
	[−9.73; 5.14]	[−0.02; 0.17]	[−0.05; 0.47]	[−0.06; 0.49]	[−0.02; 0.41]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Additional controls	*yes*	*yes*	*yes*	*yes*	*yes*
# of observations	396	715	715	715	715
# of countries	123	133	133	133	133
R^2	0.75	0.96	0.88	0.90	0.91

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C20: IV estimates with V-Dem indicators of democratic diversity

	Largest party share	Party platforms	Counterarguments	Engaged society	CSO environment
No. of dem. donors (est.)	−2.43	0.05*	0.07	0.04	0.08*
	[−5.19; 0.34]	[0.01; 0.09]	[−0.00; 0.14]	[−0.02; 0.10]	[0.03; 0.14]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*no*	*no*	*no*	*no*
# of observations	396	715	715	715	715
# of countries	123	133	133	133	133
R^2	0.71	0.96	0.87	0.90	0.90
KP F-statistic	28.81	32.39	32.39	32.39	32.39

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C21: Base models with additional control variables

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	0.82*	0.97*		
	[0.30; 1.33]	[0.37; 1.57]		
No. of dem. donors (est.)			2.50*	3.11*
			[1.14; 3.86]	[1.45; 4.76]
No. of econ. donors	−0.01	−0.16		
	[−0.47; 0.45]	[−0.69; 0.37]		
Dem. aid (m USD; log)	0.70	0.61	−0.53	−0.81
	[−0.37; 1.78]	[−0.18; 1.41]	[−2.09; 1.03]	[−2.68; 1.06]
Econ. aid (m USD; log)	3.19*	2.52		
	[0.35; 6.04]	[−0.33; 5.37]		
Pop. (log) x 10	−0.09	−0.25	−1.30	−0.89
	[−6.01; 5.83]	[−7.02; 6.53]	[−7.17; 4.58]	[−7.80; 6.02]
GDP p.c. (log) x 10	−0.85	0.27	−1.63	−0.62
	[−2.52; 0.83]	[−1.69; 2.22]	[−3.52; 0.25]	[−3.12; 1.89]
Civil conflict	−3.61	−2.88	−3.34	−3.54
	[−8.66; 1.44]	[−9.58; 3.82]	[−8.01; 1.34]	[−9.43; 2.35]
Trade (percent of GDP) x 1/10	0.43	0.49	0.25	0.33
	[−0.08; 0.94]	[−0.46; 1.43]	[−0.36; 0.86]	[−0.73; 1.40]
GDP growth (percent)	−0.22	−0.33	−0.23	−0.43*
	[−0.46; 0.03]	[−0.66; 0.00]	[−0.53; 0.07]	[−0.76; −0.09]
EU accession prospect	4.19	3.39	2.40	1.05
	[−2.67; 11.05]	[−3.98; 10.76]	[−5.87; 10.67]	[−8.15; 10.25]
Fuel exports (percent of exports)		−0.13*		−0.20*
		[−0.23; −0.03]		[−0.36; −0.03]
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*yes*	*yes*	*yes*	*yes*
# of observations	701	580	701	580
# of countries	132	121	132	121
R^2	0.93	0.93	0.92	0.91
KP F-statistic			29.56	16.79

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
all explanatory variables lagged by one period.

Additional controls

Table C21 shows that the main effects are immune to adding more control variables commonly found in studies of democratization. In models 1 and 3, I add trade as a share of GDP, GDP growth and prospects for EU accession to the OLS and the IV base models with controls, respectively. Note that including these control variables leads to a loss of 14 observations compared to the base models. In models 2 and 4, I add fuel exports as a share of merchandise exports, which reduces the sample by more than 100 observations. Fuel exports is the only additional variable that is consistently significantly related to the outcome democracy (with the expected negative sign). All data for the additional variables comes from The World Bank (2016), except EU accession status, which I obtained from the European Central Bank.

Table C22: Base models, one-year time windows

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	1.02*	0.83*		
	[0.63; 1.40]	[0.41; 1.24]		
No. of dem. donors (est.)			2.25*	2.51*
			[1.39; 3.12]	[1.45; 3.58]
No. of econ. donors		−0.06		
		[−0.33; 0.22]		
Dem. aid (m USD; log)		0.35		−0.34
		[−0.05; 0.74]		[−1.06; 0.39]
Econ. aid (m USD; log)		3.44*		
		[1.53; 5.34]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	2344	2255	2344	2255
# of countries	134	132	134	132
R^2	0.91	0.91	0.90	0.89
KP F-statistic			51.95	54.41

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Alternative samples

Alternative time windows

Tables C22 to C24 give results with one-year, two-year and four-year time windows. All estimates of the effects of the number of democracy donors on democracy are significant, except the four-year OLS specification with controls (table C24, model 2). One possible interpretation of this result is that effects created by donor proliferation dissipate over time. Carnegie and Marinov (2017) find similarly short-term results on human rights and democracy for aid given by the European Union. All in all, the overwhelming robustness over a large range of different temporal aggregation levels is comforting.

Table C23: Base models, two-year time windows

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	0.96*	0.80*		
	[0.54; 1.38]	[0.36; 1.23]		
No. of dem. donors (est.)			2.23*	2.38*
			[1.30; 3.15]	[1.30; 3.46]
No. of econ. donors		−0.06		
		[−0.45; 0.32]		
Dem. aid (m USD; log)		0.25		−0.59
		[−0.36; 0.86]		[−1.56; 0.37]
Econ. aid (m USD; log)		3.54*		
		[1.16; 5.92]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	1115	1076	1115	1076
# of countries	134	132	134	132
R^2	0.92	0.92	0.91	0.91
KP F-statistic			42.76	41.88

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C24: Base models, four-year time windows

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	0.55*	0.27		
	[0.01; 1.10]	[−0.28; 0.82]		
No. of dem. donors (est.)			2.24*	2.31*
			[1.06; 3.42]	[1.01; 3.62]
No. of econ. donors		0.21		
		[−0.42; 0.84]		
Dem. aid (m USD; log)		0.66		−0.09
		[−0.87; 2.19]		[−1.70; 1.52]
Econ. aid (m USD; log)		3.91*		
		[0.15; 7.66]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	498	482	498	482
# of countries	138	136	138	136
R^2	0.94	0.94	0.92	0.92
KP F-statistic			25.08	24.99

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C25: Sub-sample: time period 1994 to 2003

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	0.70*	0.66		
	[0.00; 1.40]	[−0.14; 1.46]		
No. of dem. donors (est.)			1.62	1.69
			[−0.15; 3.39]	[−0.00; 3.37]
No. of econ. donors		−0.01		
		[−0.50; 0.48]		
Dem. aid (m USD; log)		0.13		−0.14
		[−0.42; 0.69]		[−1.02; 0.74]
Econ. aid (m USD; log)		0.33		
		[−1.91; 2.57]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	396	381	396	381
# of countries	134	131	134	131
R^2	0.97	0.97	0.97	0.97
KP F-statistic			10.24	16.82

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Temporal sub-samples

Are the observed effects robust to alternative temporal samples? Considering only the period 1994 to 2003, the my main results are not statistically significant at the 95-percent level. Note, however, that the number of observations per country drops to only three. The same applies to results based on data from the period 2004 to 2013 (table C26). Extending the length of the period to 15 years (and thus five three-year periods), however, restores the statistical significance of the results (although the OLS specification with controls just barely includes zero; table C26). I blame the lack of findings for the 1994 to 2003 and the 2004 to 2013 periods to a lack of temporal variation and do not consider it detrimental to my overall conclusions.

Table C26: Sub-sample: time period 2004 to 2013

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	0.99	0.92		
	[−0.05; 2.04]	[−0.17; 2.02]		
No. of dem. donors (est.)			−2.88	−15.66
			[−13.70; 7.95]	[−146.16; 114.85]
No. of econ. donors		−0.32		
		[−0.81; 0.17]		
Dem. aid (m USD; log)		1.99		47.20
		[−1.96; 5.94]		[−316.58; 410.97]
Econ. aid (m USD; log)		2.24		
		[−1.52; 5.99]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	350	340	350	340
# of countries	120	117	120	117
R^2	0.96	0.96	0.90	0.01
KP F-statistic			0.56	0.07

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C27: Sub-sample: time period 1999 to 2013

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	0.71*	0.44		
	[0.22; 1.21]	[−0.04; 0.93]		
No. of dem. donors (est.)			3.19*	4.08*
			[0.63; 5.74]	[0.46; 7.69]
No. of econ. donors		0.27		
		[−0.12; 0.66]		
Dem. aid (m USD; log)		0.97		−5.79
		[−2.31; 4.26]		[−13.46; 1.88]
Econ. aid (m USD; log)		4.97*		
		[2.13; 7.82]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	489	476	489	476
# of countries	138	136	138	136
R^2	0.95	0.95	0.92	0.90
KP F-statistic			8.47	5.57

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C28: Sub-sample: only sub-Saharan African countries

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	0.89*	0.54		
	[0.18; 1.60]	[−0.25; 1.34]		
No. of dem. donors (est.)			2.34*	3.67*
			[0.64; 4.04]	[0.61; 6.74]
No. of econ. donors		0.78*		
		[0.00; 1.56]		
Dem. aid (m USD; log)		1.61		−7.36
		[−1.74; 4.96]		[−15.78; 1.05]
Econ. aid (m USD; log)		−0.15		
		[−3.54; 3.24]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	245	245	245	245
# of countries	41	41	41	41
R^2	0.93	0.94	0.91	0.87
KP F-statistic			8.86	7.38

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Geographic sub-samples

Table C28 gives the results for sub-Saharan Africa. The OLS model with controls does not produce significant effects for democracy donor proliferation at the 95-percent level. The remainings results are in line with the base specification (and substantially larger for IV model 4 with control variables). Table C29 gives the results for all recipient countries outside sub-Saharan Africa. All results are statistically significant and very close to the base models.

Table C29: Sub-sample: all countries outside sub-Saharan Africa

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	1.26*	1.24*		
	[0.56; 1.95]	[0.54; 1.93]		
No. of dem. donors (est.)			2.65*	2.73*
			[1.00; 4.31]	[1.17; 4.29]
No. of econ. donors		−0.40		
		[−0.93; 0.13]		
Dem. aid (m USD; log)		0.84		0.30
		[−0.28; 1.95]		[−1.06; 1.67]
Econ. aid (m USD; log)		3.67*		
		[0.28; 7.06]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	470	470	470	470
# of countries	92	92	92	92
R^2	0.93	0.93	0.92	0.92
KP F-statistic			41.61	39.06

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C30: Sub-sample: countries with a democracy score above the global median (democracies)

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	−0.05	−0.08		
	[−0.47; 0.37]	[−0.45; 0.29]		
No. of dem. donors (est.)			3.04	3.18
			[−1.10; 7.17]	[−0.75; 7.11]
No. of econ. donors		0.19		
		[−0.19; 0.56]		
Dem. aid (m USD; log)		0.29		−2.05
		[−0.64; 1.21]		[−6.01; 1.91]
Econ. aid (m USD; log)		2.57		
		[−0.30; 5.44]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	357	357	357	357
# of countries	88	88	88	88
R^2	0.93	0.93	0.80	0.80
KP F-statistic			4.64	6.33

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Regime type sub-samples

Table C30 shows results for the sample of countries with a democracy score above the global median (measured by the V-Dem polyarchy index). None of the main effects maintains significance. For countries below the global median in democracy scores, effects disappear as well (table C31). A sub-sample of countries between the 25th and 75th percentiles restores significance for both IV models (table C32). I interprete these results as an indication that my results are driven mostly by countries in the middle ranges of the democracy indicator. Here, sufficient non-state actors are present to benefit from diverse external support, and additional improvements are easier to obtain than in more consolidated democracies. The lack of statistical power due to reduced sample sizes makes it also more difficult to identify effects within these subsamples.

Table C31: Sub-sample: countries with a democracy score below the global median (autocracies)

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	0.24	0.09		
	[−0.13; 0.61]	[−0.23; 0.41]		
No. of dem. donors (est.)			1.07	0.99
			[−0.42; 2.55]	[−0.19; 2.17]
No. of econ. donors		0.23		
		[−0.12; 0.58]		
Dem. aid (m USD; log)		0.04		−0.24
		[−0.85; 0.92]		[−1.30; 0.82]
Econ. aid (m USD; log)		1.50		
		[−0.29; 3.28]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	357	357	357	357
# of countries	82	82	82	82
R^2	0.89	0.91	0.88	0.89
KP F-statistic			10.79	10.15

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C32: Sub-sample: countries with a democracy score within the middle quartiles (anocracies)

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	0.66*	0.54		
	[0.03; 1.29]	[−0.02; 1.10]		
No. of dem. donors (est.)			1.80*	2.31*
			[0.28; 3.32]	[0.85; 3.77]
No. of econ. donors		−0.14		
		[−0.68; 0.40]		
Dem. aid (m USD; log)		1.34		−0.04
		[−0.29; 2.97]		[−2.08; 1.99]
Econ. aid (m USD; log)		5.53*		
		[1.99; 9.07]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	378	374	378	374
# of countries	86	84	86	84
R^2	0.81	0.82	0.78	0.76
KP F-statistic			26.61	20.20

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C33: Lagged dependent variable (LDV) model

	Model 1	Model 2	Model 3	Model 4
Democracy	0.95*	0.93*	0.95*	0.94*
	[0.93; 0.97]	[0.90; 0.96]	[0.92; 0.97]	[0.91; 0.96]
No. of dem. donors	0.09	0.10		
	[−0.00; 0.18]	[−0.09; 0.29]		
No. of dem. donors (est.)			0.18*	0.30*
			[0.07; 0.28]	[0.12; 0.48]
No. of econ. donors		0.08		
		[−0.13; 0.29]		
Dem. aid (m USD; log)		−0.09		−0.43
		[−0.78; 0.61]		[−1.25; 0.39]
Econ. aid (m USD; log)		0.42		
		[−1.28; 2.12]		
(Intercept)	2.72*	−4.33	1.80*	0.84
	[1.34; 4.10]	[−18.33; 9.67]	[0.26; 3.33]	[−6.87; 8.56]
Country fixed effects	*no*	*no*	*no*	*no*
Period fixed effects	*no*	*no*	*no*	*no*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.92	0.92	0.92	0.92
KP F-statistic			337.29	243.82

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Alternative models

Lagged dependent variable (LDV) models

The fixed effects specification employed in the base models will overestimate the true effect if a lagged dependent variable (LDV) specification is closer to the true model (Angrist and Pischke 2009: 243–246). The LDV estimates in tables C33 and C34 can thus be interpreted as lower bounds for the effect sizes. With three-year time windows, IV estimates are significant, OLS estimates are not (table C33, models 1 and 2). Signficance of the latter is restored with yearly data, however (table C34, model 2). Since lagged democracy scores explain enormous amounts of variation (beyond 94 percent), the additional number of observations in the yearly dataset is welcome to support the estimation of the effects of the other variables. The effect size of all specifications is much smaller, amounting to only about ten percent of the fixed effects estimates. Note, however, that I cannot assess whether a fixed-effects model or a LDV model is more appropriate, and that the LDV may drown parts of the true effect. Given the large estimates of the fixed effects IV specification, as shown in figure C2 at the beginning of this appendix, even a fraction of those would still be substantial. I consider the LDV results as additional strong support for my hypothesis.

Table C34: Lagged dependent variable (LDV) model, yearly data

	Model 1	Model 2	Model 3	Model 4
Democracy	0.98*	0.97*	0.98*	0.97*
	[0.97; 0.99]	[0.96; 0.98]	[0.97; 0.99]	[0.96; 0.98]
No. of dem. donors	0.05*	0.10*		
	[0.01; 0.08]	[0.03; 0.17]		
No. of dem. donors (est.)			0.07*	0.11*
			[0.02; 0.11]	[0.04; 0.19]
No. of econ. donors		−0.06		
		[−0.13; 0.02]		
Dem. aid (m USD; log)		−0.03		−0.06
		[−0.19; 0.13]		[−0.26; 0.13]
Econ. aid (m USD; log)		0.61*		
		[0.15; 1.07]		
(Intercept)	1.07*	−3.93*	0.93*	−0.09
	[0.61; 1.52]	[−7.76; −0.10]	[0.42; 1.44]	[−2.16; 1.97]
Country fixed effects	*no*	*no*	*no*	*no*
Period fixed effects	*no*	*no*	*no*	*no*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	2343	2254	2343	2254
# of countries	134	132	134	132
R^2	0.97	0.96	0.97	0.96
KP F-statistic			466.28	412.92

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C35: Base OLS models with a squared term for the number of democracy donors

	Model 1	Model 2
No. of dem. donors	0.90	0.67
	[−0.22; 2.02]	[−0.43; 1.77]
No. of dem. donors (squared) x 1/100	0.50	0.67
	[−3.31; 4.31]	[−3.22; 4.56]
No. of econ. donors		0.02
		[−0.51; 0.56]
Dem. aid (m USD; log)		0.86
		[−0.23; 1.95]
Econ. aid (m USD; log)		3.45*
		[0.71; 6.20]
Country fixed effects	*yes*	*yes*
Period fixed effects	*yes*	*yes*
Additional controls	*no*	*yes*
# of observations	715	715
# of countries	133	133
R^2	0.93	0.93

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Nonlinear effects

One might suspect that there is a non-linear effect of democracy donor proliferation on democracy in the recipient country. Table C35 provides results for both OLS specifications with additional squared terms of the number of democracy donors (models 1 and 2). In both models, the linear term for the number of democracy donors ceases to be significant. The effect of an interaction, however, usually requires plotting for proper interpretation (see Brambor, Clark, and Golder 2006). Figure C3 provides information on the estimated coefficient of the number of democracy donor on democracy (solid line) over the range of values for the number of democracy donors (x-axis). The grey area represents the conservative, country-clustered 95-percent confidence interval, and the dotted line indicates whether the confidence interval excludes zero, i.e., whether we reject the null hypothesis of no association between the number of democracy donors and democracy levels in the recipient country. The visual inspection of the effect yields that the confidence intervals at the extremes increase substantially and include zero when less than 8 or more than 22 democracy donors are present. As the histogram in the plot indicates, a majority of observations has scores within this range (64 percent), indicating a predominantly significant relationship. Moreover, the coefficient of the squared term is so tiny that the estimated relationship appears linear even when allowing for non-linear effects. I conclude that the simpler models without interaction effects model the relationship sufficiently well. To be more cautious, one could say that at very low and very hight numbers of democracy donors, I am less certain that a diversity effect exists; I am more confident that is exists in the middle ranges.

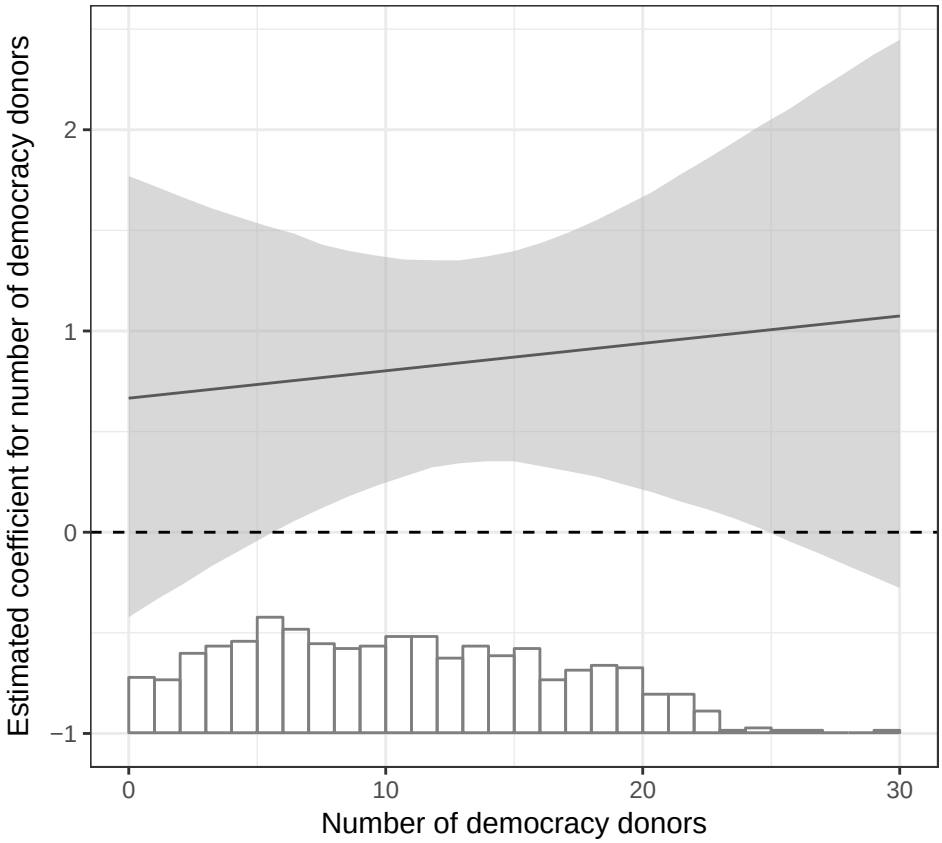

Figure C3: Estimated coefficient for the number of democracy donors on democracy for a model including a squared term

Table C36: Number of donors interacted with aid

	Model 1	Model 2	Model 3	Model 4	Model 5
No. of dem. donors	1.04*		0.96*		−0.41
	[0.52; 1.56]		[0.47; 1.46]		[−2.73; 1.91]
Dem. aid (m USD; log)		2.01*	1.09		0.79
		[0.16; 3.86]	[−0.15; 2.32]		[−0.40; 1.98]
No. of dem. donors * dem. aid				0.12*	0.15
				[0.06; 0.17]	[−0.09; 0.40]
Pop. (log) x 10	0.67	1.55	0.54	0.20	−0.01
	[−4.66; 6.01]	[−4.12; 7.22]	[−4.75; 5.82]	[−5.08; 5.47]	[−5.24; 5.23]
GDP p.c. (log) x 10	−0.70	−0.27	−0.71	−0.75	−0.76
	[−2.15; 0.76]	[−1.72; 1.18]	[−2.14; 0.73]	[−2.20; 0.70]	[−2.19; 0.68]
Civil conflict	−3.00	−3.32	−3.10	−2.95	−3.01
	[−7.88; 1.88]	[−9.03; 2.39]	[−7.95; 1.75]	[−7.75; 1.85]	[−7.80; 1.78]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Additional controls	*yes*	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715	715
# of countries	133	133	133	133	133
R^2	0.93	0.92	0.93	0.93	0.93

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Interactions with aid

One might expect that the effect of donor proliferation is conditional on the amount of aid that is being spent in democracy aid. Tables C36 and C37 give results for interactions of democracy donor proliferation with the per capita and absolute democracy aid, respectively. Figures C4 and C5 show that the confidence interval excludes zero only at levels above 4 USD per capita and 10 million USD in absolute aid commitments. Almost 63 and 90 percent of recipient time periods, however, record levels above these thresholds, respectively. One can thus conclude that there may be a minimum financial threshold at which democracy donor proliferation becomes effective, but this threshold is met in most cases.

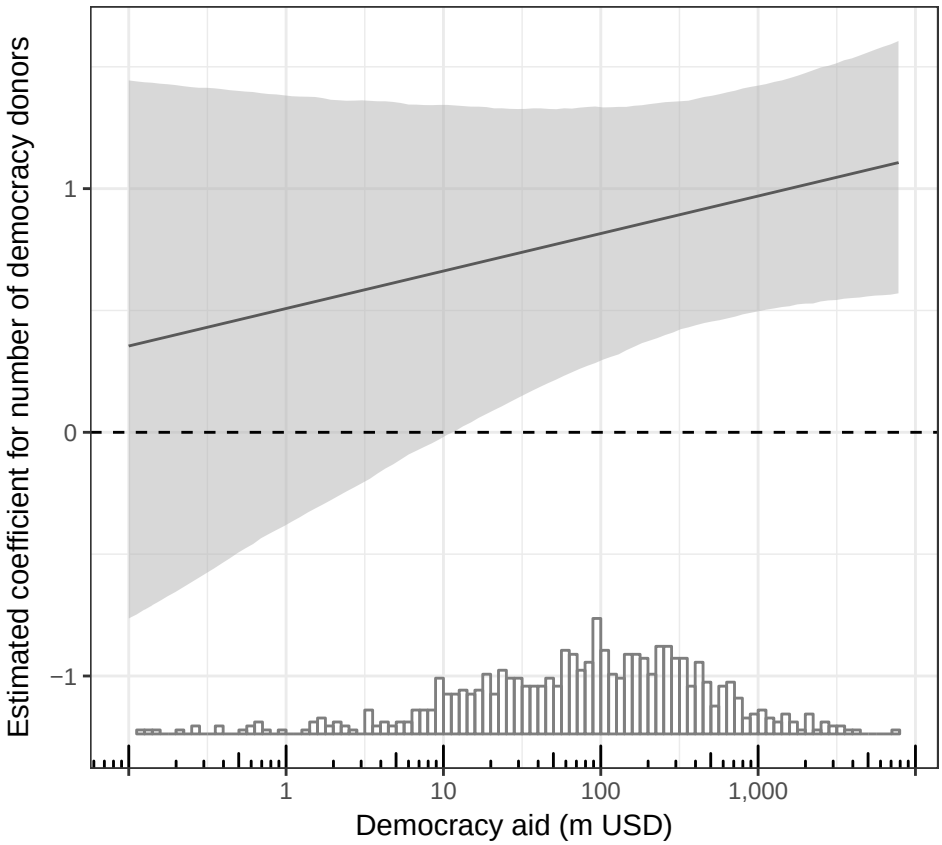

Figure C4: Estimated coefficient of the number of democracy donors on democracy conditional on the amount of democracy aid

Table C37: Number of donors interacted with aid per capita

	Model 1	Model 2	Model 3	Model 4	Model 5
No. of dem. donors	1.04*		0.76*		0.44
	[0.52; 1.56]		[0.27; 1.26]		[−0.30; 1.17]
Dem. aid (USD p.c.; log)		7.19*	4.70*		2.13
		[3.81; 10.57]	[1.98; 7.43]		[−1.92; 6.18]
No. of dem. donors x dem. aid p.c.				0.49*	0.26
				[0.30; 0.67]	[−0.09; 0.61]
Pop. (log) x 10	0.67	2.07	1.08	1.28	1.08
	[−4.66; 6.01]	[−3.25; 7.38]	[−4.06; 6.22]	[−3.76; 6.32]	[−3.99; 6.15]
GDP p.c. (log) x 10	−0.70	−0.43	−0.72	−0.68	−0.74
	[−2.15; 0.76]	[−1.82; 0.96]	[−2.13; 0.68]	[−2.11; 0.76]	[−2.17; 0.69]
Civil conflict	−3.00	−2.78	−2.80	−2.47	−2.61
	[−7.88; 1.88]	[−7.99; 2.42]	[−7.50; 1.91]	[−7.22; 2.29]	[−7.22; 2.01]
Country fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Additional controls	*yes*	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715	715
# of countries	133	133	133	133	133
R^2	0.93	0.93	0.93	0.93	0.93

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

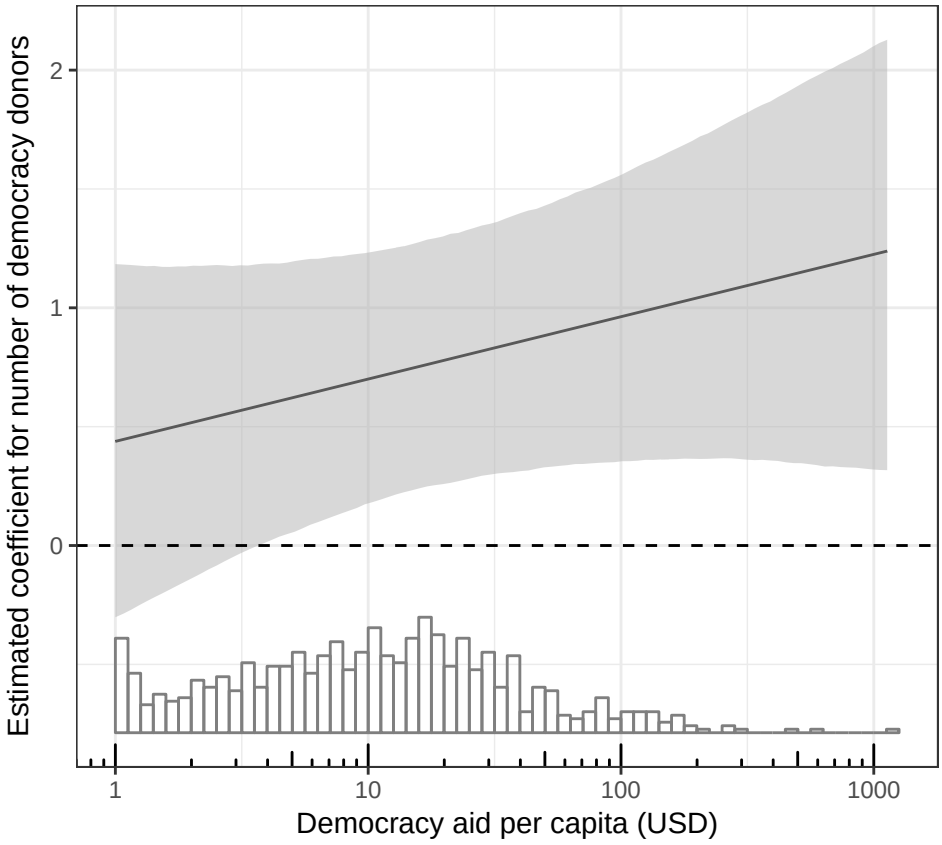

Figure C5: Estimated coefficient of the number of democracy donors on democracy conditional on the amount of democracy aid per capita

Interactions with democracy

As inspections of sub-samples of particular regime types above suggest, the effectiveness of donor proliferation on democracy may be conditioned by the previous level of democracy in the recipient country. In order to calculate the corresponding interaction, I must resort to the conservative lagged dependent variable (LDV) model, with lagged democracy scores soaking up almost all variance (see table C33 above). Table C38 shows results for the interaction. Again, these results require a visual inspection for proper interpretation. Figure C38 indicates that the 95-percent confidence interval for the coefficient of the number of democracy donors on democracy excludes zero only in countries that are in the lowest quartile of democracy scores. In other words, it suggests that democracy donor proliferation is only effective in autocracies. This is somewhat surprising, given my theoretical expectations and the sub-sample results provided above (tables C30 to C32). As the LDV model is the most conservative estimate available, however, it only provides a lower bound to my estimates. The fixed-effects and instrumental-variable specifications provide much higher coefficient estimates. Should the 'true' relationship thus be just slightly larger than the LDV estimates suggests, one would expect the effect to hold much more into the intermediate ranges of democracy. The significant range also increases well into intermediate scores of the democracy index when using yearly data (table C39 and and figure C7). I thus conclude that there is some evidence that my hypothesis applies to autocracies and anocracies rather than democracies. A potential explanation is that the latter might be able to provide sufficient diversity domestically and do not benefit as much from external nudging.

Table C38: Donor proliferation interacted with democracy, OLS models

	Model 1	Model 2	Model 3	Model 4	Model 5
No. of dem. donors	0.39		0.14*	0.35*	0.47*
	[−0.38; 1.16]		[0.03; 0.26]	[0.13; 0.57]	[0.19; 0.75]
Democracy		0.95*	0.95*	0.99*	0.97*
		[0.93; 0.97]	[0.93; 0.97]	[0.96; 1.02]	[0.94; 1.00]
No. of dem. donors x democracy x 1/100				−0.00*	−0.00*
				[−0.01; −0.00]	[−0.01; −0.00]
Pop. (log) x 10					0.04
					[−0.52; 0.61]
GDP p.c. (log) x 10					−0.12
					[−0.25; 0.01]
Civil conflict					0.18*
					[0.01; 0.35]
Dem. aid (m USD; log)					0.47
					[−1.02; 1.97]
Country fixed effects	*no*	*no*	*no*	*no*	*no*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*no*	*no*	*no*	*yes*
# of observations	715	715	715	715	715
# of countries	133	133	133	133	133
R^2	0.01	0.92	0.92	0.92	0.92

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C39: Donor proliferation interacted with democracy, OLS models, yearly data

	Model 1	Model 2	Model 3	Model 4	Model 5
No. of dem. donors	0.32		0.07*	0.16*	0.20*
	[−0.50; 1.15]		[0.03; 0.12]	[0.06; 0.25]	[0.07; 0.32]
Democracy		0.98*	0.98*	0.99*	0.98*
		[0.97; 0.99]	[0.97; 0.99]	[0.98; 1.00]	[0.97; 0.99]
No. of dem. donors * democracy				−0.00*	−0.00
				[−0.00; −0.00]	[−0.00; 0.00]
Pop. (log) x 10					0.04
					[−0.10; 0.19]
GDP p.c. (log) x 10					−0.03
					[−0.08; 0.02]
Civil conflict					0.07*
					[0.02; 0.13]
Dem. aid (m USD; log)					−0.46
					[−1.12; 0.21]
Country fixed effects	*no*	*no*	*no*	*no*	*no*
Period fixed effects	*yes*	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*no*	*no*	*no*	*yes*
# of observations	2344	2343	2343	2343	2254
# of countries	134	134	134	134	132
R^2	0.00	0.97	0.97	0.97	0.96

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

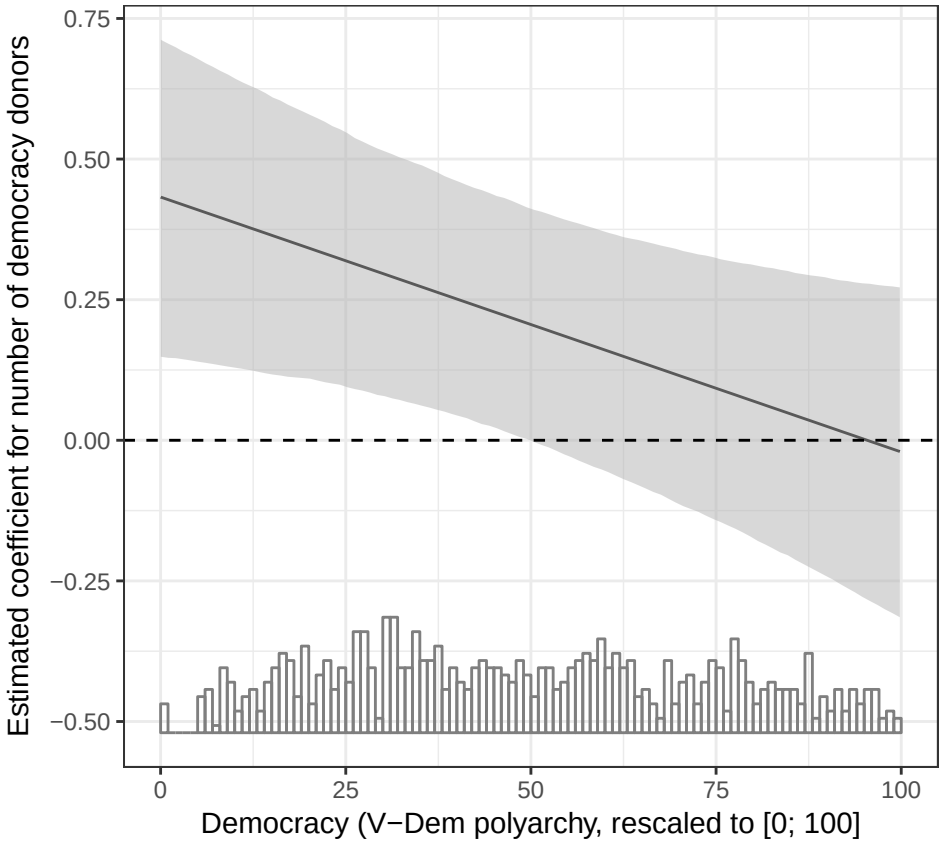

Figure C6: Estimated coefficient for the number of democracy donors on democracy for a model including an interaction with democracy

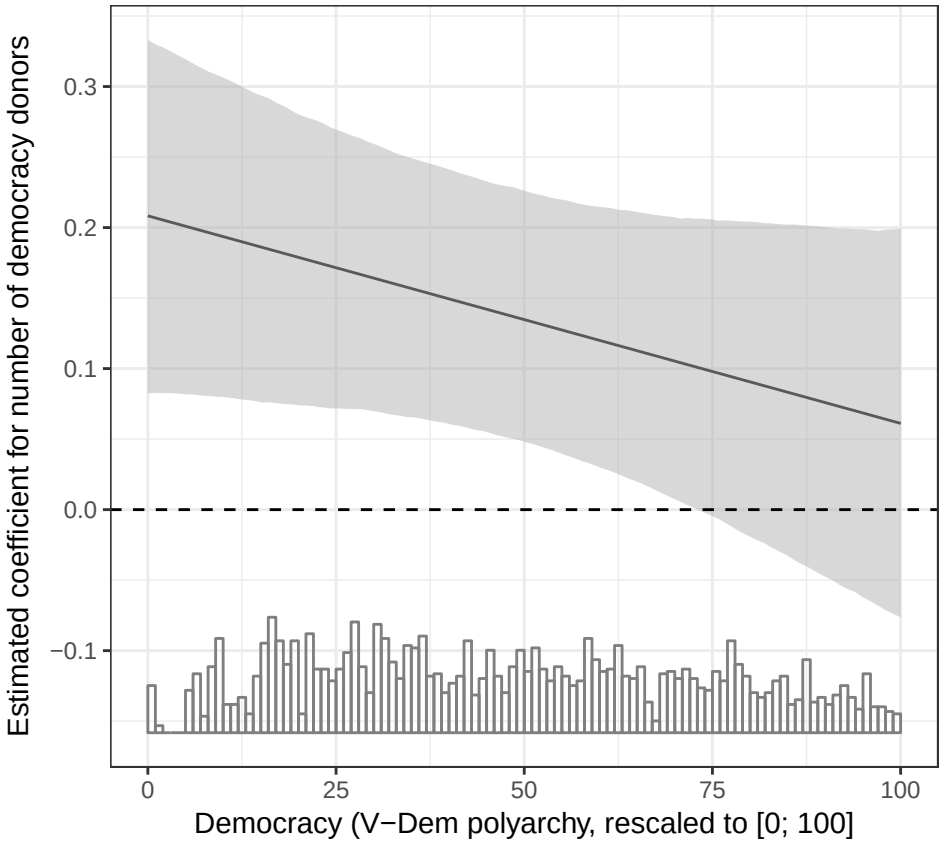

Figure C7: Estimated coefficient for the number of democracy donors on democracy for a model including an interaction with democracy; yearly data

Table C40: Base models with a global linear time trend

	Model 1	Model 2	Model 3	Model 4
Time trend	−0.75	−0.43	−3.21*	−2.73*
	[−1.78; 0.28]	[−2.26; 1.40]	[−5.57; −0.86]	[−5.35; −0.12]
No. of dem. donors	0.99*	0.82*		
	[0.50; 1.49]	[0.32; 1.33]		
No. of dem. donors (est.)			2.30*	2.38*
			[1.14; 3.46]	[1.24; 3.52]
No. of econ. donors		−0.03		
		[−0.53; 0.47]		
Dem. aid (m USD; log)		0.66		−0.60
		[−0.45; 1.77]		[−2.00; 0.81]
Econ. aid (m USD; log)		3.63*		
		[0.86; 6.40]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*no*	*no*	*no*	*no*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.93	0.93	0.92	0.91
KP F-statistic			40.98	45.72

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Time trends

An alternative to year dummies for specifying time dynamics are linear time trends. Table C40 shows that replacing year dummies with a global time trend does not affect the results substantially. Table C41 shows that even regional time trends in addition to year dummies do not affect the results either. The regional classification is taken from the World Bank. The baseline region is 'Europe and Central Asia'. It shows that in the instrumental variable models (3 and 4), sub-Saharan African countries performed better, on average, than European and Central Asian countries.

Table C41: Base models with linear region time trends

	Model 1	Model 2	Model 3	Model 4
Time trend, South Asia	1.48	1.96	1.66	3.22
	[−3.32; 6.28]	[−3.27; 7.19]	[−3.43; 6.74]	[−2.39; 8.83]
Time trend, Middle East & North Africa	1.66	2.15	1.06	2.87
	[−0.72; 4.05]	[−0.57; 4.88]	[−1.37; 3.49]	[−0.07; 5.80]
Time trend, Latin America & Caribbean	0.03	0.58	1.17	2.36
	[−2.03; 2.09]	[−1.45; 2.61]	[−1.16; 3.50]	[−0.32; 5.05]
Time trend, Sub-Saharan Africa	1.43	2.27*	2.59*	5.13*
	[−0.13; 2.99]	[0.13; 4.41]	[0.64; 4.53]	[1.80; 8.47]
Time trend, East Asia & Pacific	−1.13	−0.51	−0.50	0.89
	[−3.57; 1.31]	[−3.17; 2.14]	[−2.97; 1.97]	[−2.11; 3.89]
No. of dem. donors	1.02*	0.94*		
	[0.53; 1.52]	[0.41; 1.46]		
No. of dem. donors (est.)			2.66*	3.05*
			[1.38; 3.95]	[1.58; 4.52]
No. of econ. donors		0.05		
		[−0.46; 0.55]		
Dem. aid (m USD; log)		0.67		−0.68
		[−0.46; 1.81]		[−2.29; 0.92]
Econ. aid (m USD; log)		2.40		
		[−0.23; 5.04]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.93	0.93	0.91	0.91
KP F-statistic			31.53	29.79

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Table C42: Base models with contemporaneous effects

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	0.86*	0.59*		
	[0.39; 1.32]	[0.07; 1.10]		
No. of dem. donors (est.)			5.42*	5.97*
			[1.37; 9.46]	[1.21; 10.74]
No. of econ. donors		−0.17		
		[−0.70; 0.36]		
Dem. aid (m USD; log)		1.73*		−7.39
		[0.10; 3.37]		[−17.66; 2.88]
Econ. aid (m USD; log)		5.07*		
		[1.77; 8.38]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	715	713	715	713
# of countries	133	133	133	133
R^2	0.93	0.93	0.79	0.77
KP F-statistic			5.60	4.99

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Contemporaneous effects

My base model assumes that democracy is affected by donor proliferation in the preceding time period. It might also be possible that effects are contemporaneous. Table C42 provides results for models with a right-hand side that has not been lagged. I observe the same patters as in the base specifications. Only IV models 3 and 4 increase the estimates into incredible regions: They are more than twice as large as the already-large coefficients from the base specifications. In substantial terms, this would amount to an increase of six to seven percentage points on the democracy scale per additional donor. Given the much more conservative estimates of the LDV specifications, I do not interpret this as credible evidence for larger effect sizes.

Table C43: Base models, clustered by country and time period

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	1.03*	0.84*		
	[0.39; 1.67]	[0.18; 1.51]		
No. of dem. donors (est.)			2.58*	2.72*
			[1.35; 3.82]	[1.48; 3.96]
No. of econ. donors		0.02		
		[−0.46; 0.50]		
Dem. aid (m USD; log)		0.79		−0.60
		[−0.28; 1.86]		[−2.11; 0.92]
Econ. aid (m USD; log)		3.51*		
		[0.90; 6.13]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.93	0.93	0.91	0.91
KP F-statistic			23.46	20.97

*) 95% confidence interval clustered at the country and time period level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Two-way clustered standard errors

Table C43 gives estimates of confidence intervals based on two-way clustered standard errors. They are clustered by country and time period. The confidence intervals of my variables of interest do not increase much compared to the base specifications. Conclusions remain the same. Angrist and Pischke (2009: 319), however, note that clustered standard errors with few groups are unreliable. As there are only seven time periods in the standard dataset using three-year time windows, I refrain from clustering standard errors by time period in my main specifications.

Table C44: Base models with Conley standard errors

	Model 1	Model 2	Model 3	Model 4
No. of dem. donors	0.98*	0.77*		
	[0.67; 1.30]	[0.43; 1.10]		
No. of dem. donors (est.)			2.69*	2.91*
			[1.82; 3.56]	[2.01; 3.81]
No. of econ. donors		0.04		
		[−0.29; 0.36]		
Dem. aid (m USD; log)		0.83		−1.21
		[−0.23; 1.88]		[−2.73; 0.32]
Econ. aid (m USD; log)		4.05*		
		[2.00; 6.11]		
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*no*	*yes*	*no*	*yes*
# of observations	701	701	701	701
# of countries	0	0	0	0
R^2	0.93	0.94	0.91	0.91
KP F-statistic			30.55	31.82

* 95% confidence interval clustered over five periods and countries with capitals within 1,000 kilometers (in brackets) does not include zero; additional controls include population (log), GDP per capita (log) and a civil conflict dummy; all explanatory variables lagged by one period; dependent variable: V-Dem polyarchy democracy index [0; 100].

Heteroscedasticity-and-autocorrelation-robust standard errors

All models so far assume that the errors of our observations are independent over time and space. Conley (1999) suggests a method that allows units' errors to correlate over a certain number of periods and within a certain distance. Table C44 gives the results of the base model with standard errors that permit serial correlation over three periods (i.e., 12 years) and spatial correlation over 500 kilometers between countries' capitals. The resulting 95-percent confidence intervals are actually slightly smaller than the country-clustered standard errors from the base specification. The substantive interpretation of my results remains unchanged.

Table C45: OLS base models without fragmentation indicators

	Model 1	Model 2	Model 3	Model 4
Aid (m USD; log)	6.54*			
	[2.94; 10.14]			
Dem. aid (m USD; log)		2.01*		1.43
		[0.16; 3.86]		[−0.09; 2.96]
Econ. aid (m USD; log)			5.69*	5.02*
			[2.22; 9.15]	[1.98; 8.07]
Pop. (log) x 10	4.57	15.53	5.91	4.50
	[−50.38; 59.53]	[−41.17; 72.22]	[−49.84; 61.66]	[−50.46; 59.46]
GDP p.c. (log) x 10	−0.30	−0.27	−0.29	−0.34
	[−1.76; 1.15]	[−1.72; 1.18]	[−1.76; 1.17]	[−1.78; 1.09]
Civil conflict	−3.00	−3.32	−2.97	−3.11
	[−8.21; 2.21]	[−9.03; 2.39]	[−8.28; 2.34]	[−8.35; 2.14]
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.93	0.92	0.93	0.93

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Models without fragmentation indicators

Table C45 provides OLS estimates on the relationship between aid and democracy without including proliferation or fragmentation indicators. It serves as a point of reference to interpret the behavior of the aid variables in the absence of information on the number of donors. General aid (model 1), democracy aid (2) and economic aid (3) all display a positive and significant correlation with democracy. When adding democracy aid and economic aid simultaneously to the model, only the estimated confidence interval for the coefficient of economic aid excludes zero, although the lower bound for the coefficient estimates of democracy aid is very close to zero. Table C46 repeats the same exercise with aid per capita indicators. Here, democracy aid remains significant even when added to the model jointly with economic aid (model 4).

Table C46: OLS base models without fragmentation indicators

	Model 1	Model 2	Model 3	Model 4
Aid (USD p.c.; log)	6.93*			
	[3.20; 10.66]			
Dem. aid (USD p.c.; log)		7.19*		6.12*
		[3.81; 10.57]		[3.28; 8.95]
Econ. aid (USD p.c.; log)			5.98*	4.23*
			[2.37; 9.60]	[1.64; 6.83]
Pop. (log) x 10	11.68	20.67	12.21	15.18
	[−42.84; 66.19]	[−32.48; 73.82]	[−43.18; 67.59]	[−36.92; 67.28]
GDP p.c. (log) x 10	−0.31	−0.43	−0.30	−0.48
	[−1.77; 1.15]	[−1.82; 0.96]	[−1.77; 1.18]	[−1.87; 0.92]
Civil conflict	−2.92	−2.78	−2.89	−2.65
	[−8.13; 2.30]	[−7.99; 2.42]	[−8.22; 2.44]	[−7.58; 2.29]
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
Additional controls	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.93	0.93	0.93	0.93

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;
dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];
additional controls include population (log), GDP per capita (log) and a civil conflict dummy;
all explanatory variables lagged by one period.

Reverse models

Donors may prefer to give democracy aid to more democratic countries, as these are more inclined to further liberalize politically. I address this issue in the main paper using an instrumental variable approach and lagged three-year time windows. To complement this approach, I here investigate the possibility of reverse causation by estimating a reverse model that attempts to explain the number of democracy donors with the lagged level of democracy. Table C47 provides the results of these reversed specifications. There is no statistically significant relationship between democracy levels in the preceding period on the number of democracy donors (model 1). Note that the number of democracy donors in the preceding period did correlate significantly with the level of democracy (model 1 in table C1). Adding controls does not change the null finding (models 2 and 3). The amount of democracy aid in the preceding period is not a good predictor of the number of democracy donors, either. Population size and the number of economic donors are the only significant predictors in these specifications. The number of general donors is not predicted by democracy levels either (model 4). In sum, the reverse specifications imply that increases in the number of democracy donors precede increases in democracy levels, and not vice versa. Such an instance of 'Granger causality' is no definite proof of my causal mechanism. In combination with the evidence from the instrumental variable specifications, and the plausibility checks from the case study on Ghana, however, the reverse models provide additional support to my main hypothesis of a beneficial effect of democracy donor proliferation on democracy.

Table C47: Reverse models: democracy and donor proliferation

	Model 1	Model 2	Model 3	Model 4
Level of democracy	0.02	0.01	0.01	−0.01
	[−0.00; 0.05]	[−0.01; 0.04]	[−0.02; 0.03]	[−0.03; 0.02]
Democracy aid (m USD; log)		0.40	0.27	
		[−0.16; 0.97]	[−0.20; 0.75]	
Number of economic donors			0.20*	
			[0.07; 0.34]	
Economic aid (m USD; log)			0.36	
			[−0.47; 1.19]	
Aid (m USD; log)				1.05*
				[0.22; 1.87]
Pop. (log) x 10		1.40*	1.29*	1.08
		[0.05; 2.75]	[0.02; 2.56]	[−0.07; 2.23]
GDP p.c. (log) x 10		−0.06	−0.08	−0.07
		[−0.51; 0.38]	[−0.50; 0.33]	[−0.44; 0.29]
Civil conflict		0.39	0.29	0.43
		[−0.53; 1.31]	[−0.62; 1.21]	[−0.36; 1.22]
Country fixed effects	*yes*	*yes*	*yes*	*yes*
Period fixed effects	*yes*	*yes*	*yes*	*yes*
# of observations	715	715	715	715
# of countries	133	133	133	133
R^2	0.88	0.88	0.89	0.90

*) 95% confidence interval clustered at the country level (in brackets) does not include zero;

dependent variable: V-Dem polyarchy democracy index scaled to a range of [0; 100];

all explanatory variables lagged by one period.

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More donors, more democracy

Appendix D:

Tracing the causal mechanism in Ghana

Contents

Introduction

The case of Ghana will help illustrate how the mechanism of the diversity hypothesis works. It has several observable implications that can be traced: We can see how the democracy aid provided by donors varies by donor. We can see whether certain local actors benefited from particular donors And we can reasonably attempt to gauge whether this interaction improved democracy in Ghana. Note that this is not a test of causality, but rather an exploration of the plausibility of the causal mechanism.

Why choose Ghana for this purpose? One could argue that the case selection should be guided by the preceding quantitative analysis. This approach has found substantial interest in the political science community over the past decade. Lieberman (2005) proposes that a case situated on the regression line can be considered typical, thus being a good choice for model-testing. The danger of this approach is that an overly misspecified model – and all models are at least somewhat misspecified – could make an atypical case look typical, passing on the error from the statistical analysis to the case study while mistakenly increasing trust in the subsequent inference (Rohlfing 2008). I thus apply a more conservative approach for selecting my case, based on the discernible presence of both the dependent and the independent variable (Goertz and Mahoney 2012: 177–191). If fragmentation has an impact on democratization, it is best observed in a country that receives aid from many donors and has experienced an improvement of democracy scores during the period under scrutiny. To ensure the availability of sufficient information, it is furthermore beneficial to choose a case that has received a large amount of scholarly attention. Ghana fulfills these criteria.

Ghana gained independence from Great Britain in 1957. The country was governed by the leader of the independence movement, Kwame Nkrumah, and his Convention People's Party (CPP) until a coup in 1966. Ensuing attempts to re-establish a civil government resulted in instability and another pair of coups in 1979 and 1981, led by Jerry Rawlings. At the expense of civil liberties, Rawlings managed to prevent further revolts and to strengthen the economy. In 1992, Rawlings yielded to domestic and international pressure and introduced a multi-party democracy, not least due to his good chances of remaining in office (Haynes 2003; cp. Wright 2009). In the year 2000, Rawlings' National Democratic Congress (NDC) lost the elections to the New Patriotic Party (NPP) under John Kufuor, but won it back in a close race in 2008 under John Atta Mills, completing the two-turnover test for Ghana's democracy (Lynch and Crawford 2011). After his death in July 2012, Mills was followed in office by John Mahama, who also won the 2012 elections. The results of these last elections were initially challenged by the NPP, but subsequently confirmed by the Supreme Court. In the 2016 elections, the ruling party changed once more, with Nana Akufo-Addo and the NPP

beating incumbent Mahama.

Ghana is a presidential system with a majoritarian voting system. Even after the first turnover in the year 2000, Haynes (2003: 68) still argued that the 'rules of fair play acceptable to all significant political and social actors [were] not yet agreed upon'. Clientelism remains a strong force, due to considerable state control over economic resources (Ichino and Nathan 2013; Lindberg 2010). But it is by far not the only force driving election results: The provision of public services is indeed rewarded by the Ghanaian voter (Weghorst and Lindberg 2013). In contrast to many other African countries, ethnicity has not been politicized as a major cleavage, but regions play a strong role in informal politics (Koter 2013; Nugent 2001; Osei and Malang 2016; Whitfield 2009). Ghana had been close to the socialist block during the Cold War, but soon began to implement structural adjustment programmes in the 1990s (Handley 2008). Today, the country is one of the so-called "donor darlings", combining democratic governance with economic neediness.

Looking at general aid in Ghana, one may get the impression that some of the detrimental effects of aid fragmentation are actually at play. The number of donors reporting aid to Ghana increased from 15 in 1994 to 24 in 2013. The fragmentation index was already high in 1994 and increased only marginally until 2013, from 0.83 to 0.86. The presumed lack of coordination among donors is confirmed by observers: The rate of harmonization is judged to be very slow, and advanced coordination mechanisms such as EU joint programming have only been introduced recently (Wood et al. 2011). Hence, it is not easy for the Ghanaian government to accommodate the large number of donors wanting to work with them. There is evidence that "[…] the budget process was increasingly directed toward satisfying external donors rather than reflecting actual public spending preferences" (Moss, Pettersson, and Van de Walle 2008: 7). Also the direct funding of the government via budget support contributed to undermining parliament's ability to hold executive to account (Lawson et al. 2007). Donor funding to NGOs was often not reported to the government, violating accountability promises by the donors (Wood et al. 2011: 101). These observations suggest that the uncoordinated engagement of several donors may have damaged the domestic accountability chain.

The net effect of fragmented aid estimated by my models, however, is positive. And there is plenty of anecdotal evidence that democracy donor proliferation actually improved democracy in Ghana. I will look primarily at two types of local recipients, namely political parties and media outlets. The degree of fragmentation in democracy aid is high, as is the receptiveness of Ghana's non-governmental actors. The number of donors reporting democracy aid to Ghana rose from 5 in 1994 to 24 in 2013. An example of a larger governance programme is *Strengthening Transparency, Accountability*

and Responsiveness (STAR-Ghana), funded by the United Kingdom, Denmark and the EU. The programme aims at developing 'a vibrant, well-informed and assertive civil society able to contribute to transformational national development and inclusive access to high quality, accountable public services for all Ghanaian citizens.'[1] Experts differ in their assessments on whether democracy aid has had a positive impact on democratization in Ghana (Crawford 2005; Gyimah-Boadi and Yakah 2012). Civil society in Ghana is generally strong, but 'the opportunism of many so-called NGOs' has led the donors to tighten their funding criteria, which reduces local ownership (Mohan 2002: 148). NGOs also represent mostly urban interests (Haynes 2003: 69). Ghana's civil and political society is thus somewhat deficient, but it has the potential and the freedom to benefit from a differentiated array of external help.

Party aid

A specific sector of democracy aid that exemplifies this potential is party aid. In 2002, the *Netherlands Institute for Multiparty Democracy* (NIMD), a government funded organisation promoting democracy on behalf of several Dutch political parties, partnered with the *Institute of Economics Affairs* (IEA) in Ghana to help found the *Ghana Political Parties Programme* (GPPP) (Netherlands Institute for Multiparty Democracy 2002: 11). The GPPP constitutes an 'interparty dialogue platform' that aims at improving relations between political parties, including several extra-parliamentary parties (Dijkstra and Kumado 2004: 2; van Breukelen and Magolowondo 2010: 8). As no other forms of institutionalized exchange between parties existed in Ghana at the time of the establishment of the GPPP, it provided a crucial component in increasing communication (Dijkstra and Kumado 2004: 21). In 2007, the programme launched a 'Democracy Consolidation Strategy Paper', which led to various institutional reforms, and in 2012, it organized a public debate of candidates for the presidency (Mensa 2014). Other effects attributed to the GPPP are successful settlements around the electoral commission, generating additional trust in the institution, and general support to 'free and fair elections, a peaceful change of power and democratic reforms' (van Breukelen and Magolowondo 2010: 8, 12).

A tangible effect that deserves additional attention and that can be traced back to the Dutch funding is securing the survival of several small parties, including the CPP and the People's National Convention (PNC) (Gyimah-Boadi and Yakah 2012: 14). But what effect could support for small parties have in a de-facto two-party system like Ghana? First, despite their weakness, the survival of the small parties led to a slightly more competitive party system, thus improving chances of democratic consolidation (cp.

[1] http://www.star-ghana.org/about-star-ghana/about-star-ghana-2/

Wright 2008). Second, small parties have at times won a share of votes in the parliamentary elections that is bigger than the margin between NPP and NDC, giving them more leverage than their vote share suggests (Ichino and Nathan 2013: 432). They were sometimes necessary to secure a majority in parliament; one example was the coalition between the NPP and the People's Convention Party (PCP) after the 1996 elections (Arriola 2013: 264; Whitfield 2009: 622). Such formal involvements are an important but not the only channel by which small parties can influence the politicization of societal cleavages (cp. Zielinski 2002). Despite the sustained drive towards dominance of the two major parties that was strengthened in the 2016 elections, analysts still concede a crucial role to smaller parties. The main parties are often captured by elites, which is a problem in many young democracies, and also in Ghana (cp. Grzymala-Busse 2007; Osei 2013). The United States Agency for International Development (USAID) notes that '[…] the NDC and the NPP[,] are a substantial impediment to reducing the concentration of power in the executive branch' (Fox et al. 2011: 35). The survival of parties outside the establishment can reduce the threat of collusion among dominant parties by threatening exposure, as Gottlieb (2015) shows for Mali. Macerating the defenses of the political establishment requires organized alternatives as offered by the small parties.

In sum, the survival of small parties improved Ghanaian democracy, and the survival of these parties depended crucially on the support of a particular donor. The Netherlands are not the only donor supporting the IEA (Netherlands Institute for Multiparty Democracy 2012: 16), but the GPPP can be decidedly traced back to a Dutch initiative (Netherlands Institute for Multiparty Democracy 2002: 11). In a less fragmented donor environment, the probability of having a donor that bothered with saving the small parties would have been smaller, and thus, democracy would have benefited less from democracy aid. Dijkstra and Kumado (2004: 15) confirm that Dutch support provided a unique contribution: '[…] everyone interviewed by the evaluators stressed the uniqueness of the [N]IMD/IEA programme in relation to the interventions of other development partners in Ghana because of the bilateral programme and the cross party activities.'

Media aid

Media aid is a second example that illustrates how democracy donor proliferation may help improve democracy. The Friedrich-Ebert-Stiftung (FES), a German political foundation whose development activities are mostly funded through the official German aid budget, contributed substantially towards supporting independent media providers (Gyimah-Boadi and Yakah 2012). This included both immediate technical aid and pressure to liberalize the access to provide media. While Ghanaian media is often portrayed

as highly partisan (Temin and Smith 2002: 592), it is well documented that the media played a crucial role in the development of domestic democracy (Arthur 2010; Lindberg 2010). Conroy-Krutz and Moehler (2015) conducted a field experiment in Ghana that supports the notion that free and controversial media did not lead to additional polarization, but rather the opposite. They exposed participants in the experiment to different types of arguments over the radio during their public transport commutes, and partisan arguments increased the appreciation of opposing views.

As Dutch aid for party survival, German aid for free media constitutes a crucial contribution without which public debate would have had much less support. Figure D2 shows that Germany committed projects of at least 10,000 USD in five of the twenty examined years. Only the United States and UNICEF – an organizations whose main business is not press freedom – reported commitments with this activity code as frequently as Germany. All but one year record three or less donors committing new projects. One could ask whether free information could be supported via other measures. The most obvious alternative is infrastructure for radio, television and print media, a subsector of 'Economic Infrastructure and Services'. Here, a total of seven donors reports activities, and most intensely, France until 2004. Nonetheless, neither subsector seems to suffer from too much donor involvement. Aid data at low levels of disaggregation are most likely subject to large amounts of missing data, but the suspicion that donors make unique contributions to maintaining diversity in political approaches receives consistent support from the data that is available.

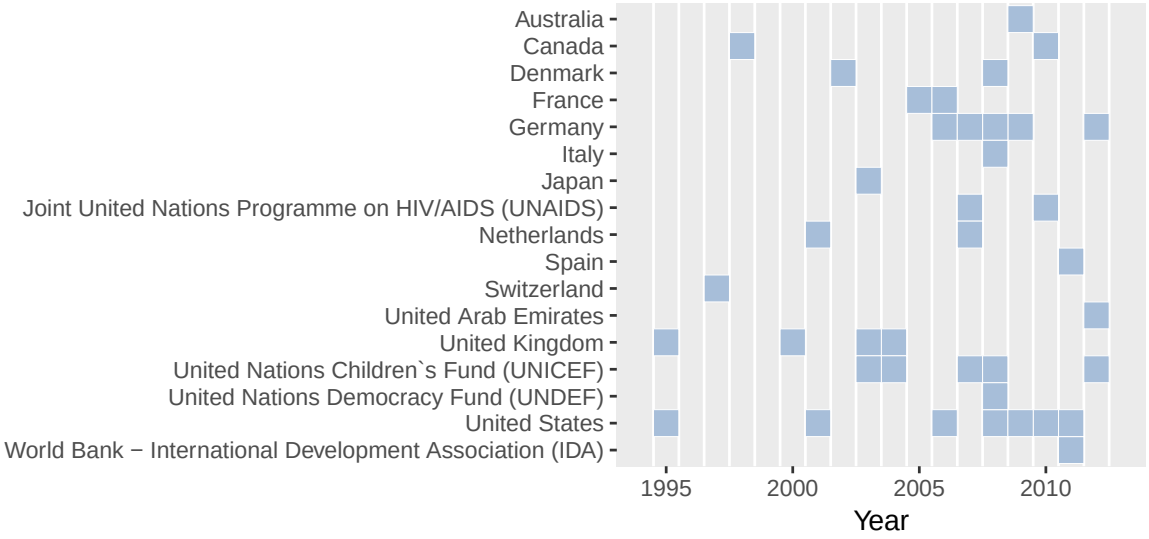

Figure D1: Commitments over 10,000 USD by donor and year coded with AidData activity code 'free flow of information' (code 15150.08)

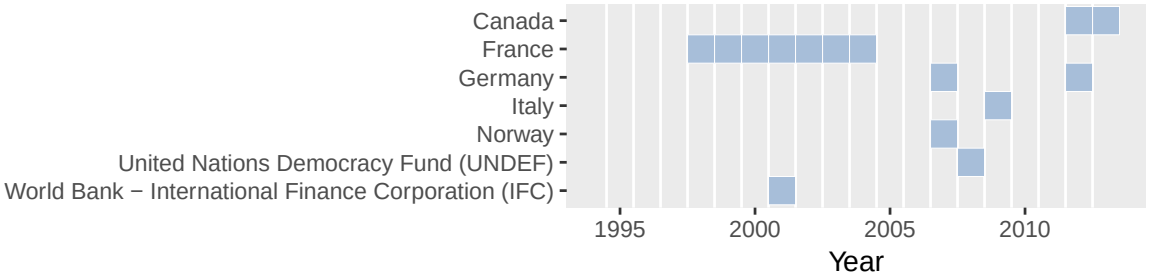

Figure D2: Commitments over 10,000 USD by donor and year coded with the AidData purpose code for mass media infrastructure (code 22030)

Conclusion

Given these anecdotes on democracy donor proliferation and democracy in Ghana, is it possible to construct a 'plausible counterfactual' (Lebow 2000) of a Ghana with less diversity in democracy aid? Both political parties and media have been shown to be advantageous in the democratization process. Since every donor has a particular way of approaching democracy-related issues, the loss of one donor means that some local actors lose their best-matching partner as soon as one donor leaves. Because donors pick certain areas and modes of engagement with their specific projects, a more fragmented donor environment provides more options. The corollary of the qualitative evidence is that even in a country with a fragmented aid setting such as Ghana, other donors would probably not have replaced the very same efforts of the NIMD and the FES, had one of them pulled out. The theorized causal direction also receives anecdotal support. While a democratizing country as Ghana may be more attractive to democracy promoters than authoritarian regimes, it becomes clear from these examples that particular improvements followed from a diverse donor presence. The NIMD did not come to Ghana because small parties survived, and the FES did not come to Ghana because there were gradual improvements in press freedom during the 2000's; both donors had arrived before these developments, and there is considerable evidence that both contributed crucially to the outcomes. Overall, recent history of Ghana supports the notion that a link from donor diversity to democratization is plausible.

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