

Triage for Democracy: Selection Effects in Governance Aid

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Abstract:

Foreign aid for improving governance may promote democracy because monitoring presents relatively low costs due to abundant third-party information and the aid largely bypasses governments in order to directly assist civil society groups. However, donors appear to give aid for reasons other than those stipulated in the project documents; instead, they may use aid to cement alliances, promote trade partnerships, bolster relations with former colonies, or buy votes in the United Nations Security Council. Moreover, aid is notoriously fungible and conditional aid may be largely incentive incompatible. So there are good reasons to be skeptical of aid for any purpose, including governance. We argue here that some governance aid may work as intended – and for the reasons stated above – but only in the most-likely cases. That is, donors perform a kind of development finance “triage” to select the recipients that seem most able to enact democratization reforms; donors then provide governance aid mainly in those most-likely cases. We test this argument using our new AidData information base, which adds more than fifty donors and nearly \$2 trillion to the well-known total of \$2.3 trillion in development finance since 1945. We employ a new pooled time-series technique of propensity matching to control for selection effects. Once the panel propensity matching has occurred, contra prominent research such as Finkel et al. (2007), we find limited evidence that governance aid promotes democracy in the main. Instead, the results suggest that governance aid leads to higher democracy scores only in the subset of observations most likely to receive the governance aid in the first place.

Triage for Democracy: Selection Effects in Governance Aid

Richard Nielson and Daniel Nielson

Introduction

In Uganda a lack of democratic accountability has long undermined government effectiveness, and one survey in 1996 showed that only thirteen percent of the budgeted education money actually made it to targeted schools. However, foreign aid donors' clever effort to promote greater citizen involvement and oversight by publicizing budgets on schoolhouse doors apparently boosted the money arriving at the schools to eighty percent by 2002 (see Reinicke and Svensson 2003). The implication: focused foreign aid can both buy democracy and promote better government.

Encouraged by experiences like Uganda's and ostensible successes in Eastern Europe and a few Soviet successor states, foreign aid donors have dramatically increased aid for governance and democracy, making democracy aid one of the fastest-growing categories among all types of foreign assistance. But does democracy aid work generally? If it does, governance aid would stand as one of the few sectors of foreign assistance boasting significant positive results. Indeed, most aid from other sectors does not seem to have the intended effects, and the center of gravity among econometric studies suggests that aid and economic growth are entirely unrelated (Easterly 2003; Easterly, Levine and Roodman 2004; Rajan and Subramanian 2008; see also Burnside and Dollar 2000).

But democracy aid may be different. Donors can monitor democracy levels through several independent sources, so they can more readily track aid effectiveness. And democracy aid – more than other sectors – bypasses corrupt governments and goes directly to non-governmental actors such as the opposition parties, labor unions, and civic associations that ought to demand government accountability and provide counterweights to previously empowered elites.

The early returns suggest reason for optimism. In a prominent and carefully developed study, Finkel et al. (2007) found that governance aid from the United States Agency for International Development (USAID) was associated with significant improvements in democracy scores in recipient countries. These results were robust to many alternative specifications, including two-stage models employing an instrumental variable for U.S. foreign policy priorities, which the authors measured by counting the number of times the U.S. Secretary of State referenced a given country in the *New York Times*. The reported results were very significant substantively as well as statistically: forty million dollars of democracy aid on average boosted Freedom House scores by one full point on the seven-point scale. Democracy aid apparently buys democracy at a relative discount.

Yet other analysts emphasize many reasons to be skeptical of aid in any form. In the first place, much aid was never intended to combat poverty or underdevelopment in the first place. Instead, aid goes to cement alliances, enhance trade partnerships, or influence former colonies (Alesina and Dollar 2000). Even more instrumentally, it appears that the United States in particular has used foreign aid to buy votes at the United Nations (Kuziemko and Werker 2006, Dreher et al. 2008). Even if some aid goes toward its stated purpose, many factors may intervene to reduce its effectiveness. Corruption, mismanagement, information asymmetries, conflicts over

the spoils, and the general difficulties of planning economies all combine to make aid relatively ineffective (Easterly 2006, Collier 2006). Indeed, because of these and other problems, one economist has even ventured that aid is the primary reason for economic backwardness in Africa (Moyo 2009). Such bleak analyses suggest that foreign aid donors are either stupid, venal, or both.

In this paper we suggest a third alternative: donors are strategic. That is, because aid resources are scarce and donors may seek to promote democracy for normative and/or instrumental reasons, donors should target democracy assistance where they expect the most positive results to occur. Like doctors confronting overwhelming numbers of ailing patients and thus treating patients first who are most likely to benefit from their care, donors may select the recipients where the aid ought to engender the greatest movement toward democracy. Thus, we argue that donors of democracy aid engage in triage: aid goes most where it will promote democracy best.

Testing this argument, however, requires a better method of sorting out the selection effects of democracy aid. That is, because donors may choose recipients of democracy aid based in large measure on prior democracy levels (they give more aid where countries show promise in democratizing), this may bias statistical results toward the conclusion that democracy aid works when in reality the estimates are simply picking up the fact that democratic countries are more likely to receive democracy aid in the first place. We thus employ the more conservative method – compared to conventional hierarchical models with instruments – of propensity score matching to control for these selection effects. We subsequently find evidence that democracy aid may indeed promote democracy, but only in the recipient countries most likely to receive the aid in the first place. In what follows we situate this argument in the literature on democracy aid, make

an argument in favor of “triage for democracy,” develop our measures, employ our propensity matching technique, and report our results.

Aid and Democracy

Since the Marshall Plan, Western foreign aid donors have attempted to use aid to shape the politics and political institutions of recipient countries. Of course, donors pursued these objectives alongside developmental and humanitarian goals. Throughout its modern history, foreign aid has almost always been highly politicized, flowing disproportionately to developing countries with greater strategic and political importance to donor states. In post-war Europe, the U.S. leaders concerned themselves with curbing the spread of communism and believed that allocating substantial resources to rebuild Europe would counter the communist threat they envisioned. Thus, even from its earliest moments, modern foreign aid was viewed as a way of potentially supporting and promoting democracy abroad.

As the Cold War deepened, the political competition between the United States and the Soviet Union continued to make aid strongly political although Western aid was certainly not primarily intended to promote democracy between the 1960s and the early 1990s. Instead, both superpowers used aid to buy allies and promote regime changes that would increase their respective spheres of influence (Bermeo 2008).

The fall of the Soviet Union ushered in a new decade of foreign aid, much of it aimed squarely at promoting democracy. Bolstered by the belief that democracies were more peaceful and better economic partners, Western donors viewed the end of the Cold War as an opportunity

to reshape the world toward like-minded institutions. The U.S. was particularly vigorous in its democracy promotion efforts, claiming democratic transitions in Ukraine, Georgia, and Serbia as clear successes for its foreign aid policies. European aid for democracy increased dramatically as well. Although several donors, notably the U.S., had maintained human rights as a condition for aid since the mid-1970s, donors began to allocate aid intended to improve human rights on a large scale only as the Cold War was visibly winding down. The Nordic countries in particular officially adopted human rights conditions in their aid policies in 1988, and other Western donors followed suit through the 1990s. The promotion of democracy became a centerpiece of the new thinking about foreign aid.

However, the effect of aggregate foreign aid on democracy seems to be either insignificant (Knack 2004) or negative (Kalyvitis and Vlachaki 2008; Djankov et al. 2006). However, this leaves questions open concerning the effects of different types of aid on democracy. The sectoral make-up of aid has changed from 1980-2005, the period of most previous studies, and if different sectors have different political effects in the recipient countries, then we have to qualify our understanding of aggregate aid to mean aggregate aid as it was in the period of our sample.

Most scholars attempting to identify the effects of aid have used highly aggregated measures of assistance, considering relationships between aid and growth (Burnside and Dollar 2000), aid and democracy (Knack 2004), and aid and corruption (Alesina 2002) as if all aid were likely to have similar effects on these outcomes (see Clemens et al. 2004, for an important exception). In this paper we attempt to correct this course by narrowing our focus and considering the specific effects of governance assistance on democracy levels. This disaggregation is vital because virtually all theories about the domestic political effects of

assistance rely on mechanisms implying that some types of aid will lead to different outcomes than other types. However, despite suggesting sector-specific mechanisms, previous studies have largely failed to test these hypotheses, instead attempting to establish a general correlation between aggregated assistance and some general outcome measure, including democracy levels.

On the other hand, some studies have disaggregated aid (Finkel et al. 2007; Bermeo 2007) especially in the context of democratization (Finkel et al. 2007; Bermeo 2007). In particular, Finkel et al. employed novel data set obtained directly from USAID, which enabled a much finer disaggregation of aid by intended purpose. Moreover, the totals for USAID in the official international repository, the OECD's Creditor Reporting System database, are likely incorrect for key years due to a systems problem at the agency that began in 1996. The Finkel et al. data correct for the misreported and missing projects.

The Finkel et al. study is thus impressive for its scope, accurate data, compelling argument, and sophisticated methods. The authors employ a hierarchical linear model (HLM) with standard control variables explaining democratization as a baseline, but then subject the base model to rigorous robustness tests. Their robustness checks include fixed effects for year and recipient and a two-stage model using American foreign policy priorities as an instrument. They measured U.S. priorities by counting the number of times in a given year that the U.S. Secretary of State is quoted in the *New York Times* as mentioning a given recipient country. This instrumental variable is both ingenious and plausible.

However, if we are correct in our argument that donors are strategic in selecting targets for democracy aid, the democracy levels of targeted countries will likely figure into the strategic priorities of the state department. Those considerations may, in turn, bias the degree that the Secretary of State identifies given countries in public pronouncements. If democracy levels are

indeed considered when identifying State Department priorities, the instrument cannot overcome the feared endogeneity problems and the results may well be biased. In this paper we employ different methods to account for the selection bias, yet we also employ the conventional hierarchical model as a baseline. The results from the two methods are strikingly different, and they reflect very differently on the argument that democracy aid causes higher democracy levels.

Thus it becomes especially important to ground the causal mechanism theoretically. Governance aid may improve democracy for at least two reasons: (1) the ease of monitoring outcomes and (2) the direct empowerment of a broader electorate. First, donor governments, several prominent non-governmental organizations, and many academics track governance outcomes – particularly the quality and extent of democracy. Indeed, leaders of developing governments often make choices that lower the costs of monitoring governance. For a variety of reasons, leaders display a propensity to invite observers to witness and comment on the freedom and fairness of elections. The poll watchers' subsequent conclusions about the quality of the elections provide fairly rich information about procedural democracy in targeted countries (see Hyde 2009).

Lowering direct monitoring costs even more, many other third-party actors provide systematic assessments of democracy levels. Organizations such as Freedom House, Transparency International, the Political Risk Services Group (which produces the *International Country Risk Guide*), and the Carter Center, among many others, are all for-profit or well-funded endeavors whose missions center on providing information about the quality of governance in the developing world. Moreover, several high-profile academic projects, such as the Polity effort and the work of New York University's Adam Przeworski and collaborators also provide independent assessments of countries' democracy levels. Most of these measures are updated

annually. Thus, where a given country stands on democracy vs. autocracy at a given time is not a mystery.

Moreover, donors themselves actively monitor outcomes and publish their conclusions, exemplified by the World Bank's Worldwide Governance Indicators database. In terms of outcomes measures, governance is a relatively rich sector. In many other sectors, the scarcity and low quality of information available makes monitoring outcomes much more challenging, and the number and independence of credible sources on the outcomes is much lower. So, for democracy at least, donors can obtain reasonably good estimates of whether or not they are getting what they pay for.

Second, to a much greater degree than is true for many other sectors, governance aid bypasses governments and instead goes directly to opposition parties, non-governmental watchdog organizations, social movements, and other civil-society groups. These newly empowered actors raise the costs to governments of engaging in anti-democratic practices and expand the number and breadth of relevant actors in the political process. To the degree that robust opposition and independent political movements enhance democracy, governance aid targeted directly at strengthening these groups should help (see Finkel et al. 2007). So, addressing the argument that aid to governments is largely or completely ineffective because of government mismanagement and corruption (see Easterly 2004, Moyo 2009), governance and democracy aid proves distinctive because it bypasses the governments prone to capturing the aid.

Once democratization has begun, donors then tend to target the strengthening of electoral commissions, judiciaries, legislatures, local governments, and other institutions believed central to democratic consolidation (Finkel et al. 2007, 410-411). To use Bueno de Mesquita et al.'s terms, democracy aid broadens both the "selectorate" and the "winning coalition" by

empowering actors that might have previously been excluded from accessing the levers of power (Bueno de Mesquita et al. 2004). This is the essence of democratization, and governance aid may have distinct advantages over other types of aid in promoting it.

Noting that a strong civil society seems to be an important element in establishing democracy, Carothers and others have argued that aid flowing through NGOs and civil society groups will have the effect of strengthening these groups in society, perhaps even if the aid is not intended to promote democracy directly. By providing revenue for these groups, aid donors strengthen their ability to provide voices of opposition in closed societies. In particular, USAID has spent significant funds assisting “civil society” by supporting labor unions, women’s organizations, and other local NGOs.

Perhaps all aid that goes through NGOs, regardless of sector, builds civil society and is thus effective (Carothers and Ottaway 2000, 13). One might also believe that democracy aid through NGOs will be particularly effective because NGOs have incentives to see that the government liberalizes, whereas the government may face countervailing incentives. Thus, democracy aid that goes through the government may not accomplish much, but democracy aid through NGOs may.

However, others are doubtful about the positive effect of aid to NGOs. In particular, Carapico (2002) argues that democracy aid channeled through NGOs to promote democracy in the Middle East actually undermined the legitimacy of the NGOs and led state authorities to crack down on them. Ottaway in Carothers and Ottaway (2000) discuss the case of Zambia, where the regime directly threatens NGOs, which have struggled to survive because incumbent leaders believe the NGOs seek chiefly to weaken regime power. So perhaps democracy aid

works best when given to NGOs based in regimes that are not repressive enough to dampen all NGO activity.

The contention that governance aid targets non-government actors can be partly explored empirically. While data scarcity prevents systematic hypothesis tests, we can use the “channel of delivery” field in the CRS data to assess the relative degree of independence from direct government control that each aid sector evinces. Roughly half of the foreign aid project observations are missing data in the channel of delivery field, but the “missingness” of the channel data is not correlated with democracy levels, so we may still be able to extract meaningful information. Preliminary analysis suggests that governance aid is less likely to flow through governments. For example, less than five percent of infrastructure aid goes through non-governmental channels, but more than thirty percent of human rights aid is handled by NGOs (also, see Bermeo 2008, Nielsen 2009).

We emphasize, however, that the above mechanisms require donors that carefully monitor democratization in recipient countries and tailor their aid accordingly. Indeed, for key parts of their causal mechanisms to operate, Finkel et al. (2007) must expect donors to be specifically aware of movements toward democratization. This strongly suggests strategic donors attempting to maximize their impact. Finkel et al. invoke Carother’s “transition paradigm,” where donors can be expected to “identify the constituent stages of the process of democratization and to intervene in support of critical actors at each stage” (2004, quoted in Finkel et al. 2007, 410). Thus, during autocratic periods donors target freedom of the press and promote civil society. Once democratization begins, donors shift to supporting voter registration, electoral commissions, fair vote counting, and international election observers (2007, 411-412). Finkel et al. hence argue that donors ought to be specifically conscious of

recipients' democracy levels and should be expected to factor the given amount of freedom into their aid allocations. This suggests strategic donors keenly aware of prospects for future democracy in recipient countries – assessments that they base in large measure on past observations of democracy. This also powerfully implies selection bias, as prior democracy levels specifically influence aid decisions. Finkel et al. acknowledge as much and thus place great weight on their methodological corrections for the endogeneity problems (2007, 413-14).

Thus, while impressive, the Finkel et al. analysis may not adequately deal with reverse causality – the possibility that the magnitude of democracy aid flows are affected by the democracy level of the recipient through aid donors' expectations about a recipients' propensity to democratize. Indeed, we argue that given scarce resources, donors will strategically target the recipients mostly likely to democratize and will base those judgments on their past observations of recipients' movement toward democracy.

More generally, it is unclear whether democracy aid from other OECD donors will have similar effects since the U.S. democracy aid program is unique in many respects. Indeed, Finkel et al. emphasize that their results hold only for United States aid. In this paper we extend our analysis to all democracy aid from all donors for which data are available.

Other studies have come to less promising conclusions about the role of democracy aid in democracy promotion. Ethier (2003), for example, argues that democracy aid has not proven to be very effective, but her evidence is mostly secondary and relies on qualitative cases that do not appear to have been selected systematically, perhaps revealing more nuanced details at the expense of generalization. On the other hand, corroborating Finkel et al., Kalyvitis and Vlachaki (2007) also find evidence that democracy aid programs are correlated with increased democracy.

Might Aid Weaken Democracy?

In contrast to the generally positive expectations of the effects of aid summarized above, other scholars argue that aid of various types retards democratization. Although this argument takes many forms, the basic idea is that aid acts like a natural resource, providing a source of income for a regime independent of its ability to raise taxes. This liberates the government from the need to pay attention to domestic public opinion and creates incentives for rentier behavior (Smith 2008).

In support of this argument, work by Djankov, Montalvo, and Reynol-Querol (2006) finds that aid significantly decreases the democracy level of recipient countries. The authors argue that this finding results from the ability of aid to strengthen non-democratic regimes, a finding similar to Bueno de Mesquita et al (2003), who present evidence that economic aid increases the tenure of leaders in authoritarian regimes. This was certainly the thesis of early work on foreign aid, which argued that assistance merely propped up authoritarian regimes (Danaher, Berryman, and Benjamin 1987; Chomsky and Herman 1979; Grossman 1992). These arguments rely on the fungibility of aid: it is either directly seized by recipient governments or it substitutes for expenses the recipient would have otherwise undertaken and thus frees up funds to be spent elsewhere at the recipient's discretion. Sogge's (2002) findings corroborate this claim, presenting evidence that democracy assistance has failed largely because donors have pursued their own interests via aid rather than allocate it with recipient needs in mind.

A related set of arguments holds that aid dependence induces poor governance, although the results of aid may cut both ways. Brautigam and Knack (2004) argue that in sub-Saharan Africa, large influxes of aid may undermine the ability of governments to govern, undercutting

the democracy that aid is often intended to promote. However, Goldsmith (2001) shows that for sub-Saharan Africa, aid leads to net gains in governance quality, albeit the gains are so small as to be easily overwhelmed by other factors.

Complementing the mechanism above where aid through NGOs is effective in causing democratization, it may be that aid which flows through the central government is bad for democracy, regardless of purpose, because it weakens accountability and encourages rentier behavior (Friedman 1958). Some of these mechanisms have already been partially tested. Knack has already found that there is no correlation between overall aid and democracy, although Djankov et al (2006) and Kalyvitis and Vlachaki (2008) find a negative correlation. However, as noted above, Finkel et al. (2007) and Kalyvitis and Vlachaki (2007) find that democracy aid increases democracy.

Endogeneity: Do Democracies Receive More Aid?

Identifying the effects of sector aid on democracy is an extremely difficult case for causal inference given the behavior of donor countries—aid for these sectors is almost certainly dependent upon a given recipients' current level of democracy and the donor's perceptions of that recipient's likelihood of future democratization. Svensson (1999) finds that "aid on average is not channeled to more democratic countries, even though there are large cross-country differences between major donors" (abstract), but Alesina and Dollar (2000) find contrary evidence that aggregate aid flows disproportionately to new democracies. In any case, there is plausibly reciprocal causation between the aid sectors examined and democracy. Unfortunately

we often have no clear theoretical prediction about ultimate direction of causation in the endogenous relationship.

Most obviously, it is unlikely that democracy aid is given irrespective of the democracy level of the recipient. If the donor's primary goals are to promote democracy where it is weakest and thus needed the most, then democracy aid will flow disproportionately to countries where democracy measures are weakest. If donors believe that democracy aid is most helpful in transitioning polities, then they will give their democracy aid disproportionately to countries with middling democracy scores (and recent changes in democracy levels). The same is true if donors are trying to display the effectiveness of their aid by "picking winners." This is our triage mechanism. If this happens then we would expect them to pick middling polities that are already showing signs of increased liberalization. Some preliminary evidence suggests that donors target democracy aid to countries that show promise of democratization (Nielsen 2007) but further research is needed to understand how donors allocate democracy aid. We perform part of that analysis here and turn to it now.

Data and Measurement

Dependent Variable: Democracy Levels

We measure the political impact of aid on levels of democracy. We use the Polity IV measure to gauge the democracy level of recipient countries (Marshall and Jaggers 2002). We note that this variable captures primarily institutional features of democracy, in contrast with measures such as Freedom House scores, which place a stronger emphasis on democratic rights. To measure levels of democracy, we simply use Polity IV scores. These have received some

criticism (Jackman and Treier 2008) and some claim that it is possible only to classify polities as democratic or undemocratic—in the future we hope to test the robustness of our results to alternative measures such as Pzeworski’s (2000) binary measure of regime type.

It is also not clear that an aggregate measure of democracy is completely adequate, especially because the causal mechanisms we test predict specific aspects of democracy which should be affected. Democracy is multi-faceted and it may be that aid liberalizes some aspects of a polity without affecting others. In future work we hope to measure the impact of aid on specific aspects of democracy—the competitiveness of elections, the probability of executive turn-over, and respect for various aspects of political and civil rights—but that is beyond the scope of this paper.

Independent Variables:

We expand on previous studies by including the effective universe of foreign aid; we include assistance from the 21 OECD bilateral donors, the World Bank, the all of the major regional development banks, UN organizations such as the UNDP, and non-regional development banks such as the Nordic Investment Bank and the Islamic Development Bank. These data are drawn from the Project Level Aid (PLAID) database, which combines lending from all of these sources between 1970 and 2005 and codes each multilateral project with a purpose code, comparable and “map-able” to those used by the OECD to determine project sector allocation. Because the OECD’s Creditor Report System codes, which comprise the vast majority of project records and bare majority of project dollars, are self-reported, some of it is imperfect, especially in comparison with the USAID data used by Finkel et al (2007). They note that their data is the more “extensive and finely grained” than that used by any previous study,

and they suggest that previous work has been hampered by “very coarse estimations of foreign assistance world-wide” (18). This may be the case, and we support their recommendation that “other international donors should undertake studies...isolating democracy-building assistance from other types of assistance in order to evaluate the real impact of the aid provided” (87). Indeed PLAID scholars are currently undertaking these efforts and we plan to use the resulting data in future iterations of this project.

Thus, in the meantime we take what we believe to be the second best option and use the data reported by the OECD as an estimate of the aid from each bilateral donor by sector. This solution is admittedly imperfect. Finkel et al note that their enhanced USAID data is only correlated with the OECD data for U.S. democracy aid at a .62 level (53-54).¹ However, we believe that even these flawed measures can tell us something about the relationship between aid and democracy and as better data become available, we expect to provide refined estimates. However, the multilateral lending we use has been consistently coded from the annual reports of the various development banks, and we can trust that these data are more consistently and accurately assigned to the correct purpose code than bilateral projects from the CRS source.

We separate democracy aid from other types of aid activities using the purpose codes either that donors themselves assign to indicate projects that target government administration in the case of bilateral projects or the inter-coder verified projects in the case of multilateral aid.

¹ We found many apparently miscoded democracy projects which had received purpose codes for other sectors. We identify these via word searches. Such mistakes may account for the low correlation between the two measures that Finkel et al compared.

Democracy aid includes aid intended to improve judicial and legislative capacity, election monitoring, and aid for civil society.

Control Variables: Other Causes of Democratization

We control for a variety of relevant factors noted in the literature on democratization. Noting the role of domestic economic and demographic factors, we include population, real GDP per capita, and GDP growth. In alternative specifications, we account for severe economic downturns, the land area of each recipient, and the distance from the normalized latitude of each recipient (La Porta et al. 1999). Education, religious tradition and legal heritage may be important for democratization so we include the adult literacy rate, the percentage of Protestant, Catholic, and Muslim adherents in each recipient (separately), and a dummy variable for recipients with a legal system patterned after British law. Noting the possibility that natural resources affect democratization, we include a oil exports as a proportion of GDP (Energy Information Administration 2008). We measure ethnic fractionalization using ethno-linguistic fractionalization (Fearon and Laitin 2003).

International factors may also affect democratization. We account for the openness of a country to international economic flows by measuring the total sum of trade (imports and exports) for each recipient in a given year, and consider terms of trade shocks in alternative specifications. We include a dummy variable equal to 1 after 1991 to account for period effects of the Cold War. To account for democracy diffusion, we include the average democracy score (measured using Polity IV) for the states contiguous to each recipient, with contiguity defined and coded by the Correlates of War project (Stinnett et al. 2002). We also include (but do not

report) regional fixed effects using the World Bank regional designations. Finally, we control for international non-governmental organizations using data from Landman (2005).

Matching Design and Results

We have argued that democracy aid is purposefully directed towards certain types of recipients and that, without correction, these selection effects in the aid allocation process will lead to biased estimates of the causal effect of democracy aid on subsequent democratization. We use propensity score matching methods, adapted for our time-series cross-sectional data structure, to account for these selection effects.

Propensity score matching is a nonparametric method for processing data in preparation for subsequent estimation of causal effects. In effect, matching tries to fix the “broken” experiment that gave rise to the data we observe. If aid had been assigned randomly, estimation of the causal effect of aid on subsequent democratization would be relatively trivial. Our objective with matching is to account for the systematic differences between countries that receive different levels of democracy aid by matching countries that have similar values of the factors that determine aid flows. We then assume that after accounting for the systematic parts of the aid allocation process, there is still some randomness in the process such that identical countries might still get somewhat different levels of aid. We exploit this randomness to estimate the effect of increased aid on democracy *for countries that were similar prior to receiving the aid*.

We perform our matching procedure and subsequent estimation in the following steps, each of which we discuss in detail below.

- We define the treatment, define the units of observation, and define the target causal effect.
- We estimate a model for how treatment is assigned using a statistical model of aid allocation.
- We use the allocation model to calculate propensity scores for each unit.
- Divide the sample into subclasses based on the generalized propensity scores.
- Estimate a hierarchical model within each subclass to estimate the treatment effect.
- Combine the subclass estimates into a single estimate -- the average linear effect across all units.

We define the unit of observation to the country-year; our dataset includes 1,517 country-years for roughly 120 developing countries between 1980 and 2004. To estimate the effect of democracy aid on subsequent democratization, we define treatment to be the average level of democracy aid received by a country in the past three years. We calculate this as *Avg. Dem. Aid*_{*i,t*} = 1/3(*Democracy Aid*_{*i,t-3*} + *Democracy Aid*_{*i,t-2*} + *Democracy Aid*_{*i,t-1*}). . The outcome of interest is subsequent democracy, which we measure using the Polity IV democracy scores for the year of observation. Thus, the causal question motivating our analysis is “what is the effect of the past three years of democracy aid on this year’s level of democracy?”

To account for the non-random allocation of democracy aid, we estimate an aid allocation model predicting the average level of democracy aid that a given country receives in a given year. In this model, we follow standard practices within the aid allocation literature by using a tobit model to account for the censoring of aid flows at zero with country-level random intercepts to account for some of the cross-country variation in aid allocation that is not explained by the explanatory variables. As predictors in this allocation model, we include *Democracy*_{*t-1*}, *Democracy*²_{*t-1*}, , a measure of human rights practices (CIRI from Cingranelli and

Richards 1999, updated through 2004), the natural log of GDP per capita, the natural log of population, the natural log of combined total trade (imports and exports) with OECD donors, a measure of alliances with OECD donor countries (ATOP – Alliance Treaty Obligations and Provisions from Leeds 2009), an indicator for former colonies of OECD donors, an indicator for whether a state is involved in an internal or external war, an indicator for the Cold War (equal to “0” prior to 1992, and “1” afterward), an indicator for states that were Socialist during the Cold War, and the interaction of *Socialist* \times *Cold War*.

In a possibly controversial move, we include the lag of treatment (the average of *Democracy*_{*t-4*}, *Democracy*_{*t-5*}, and *Democracy*_{*t-6*} in the aid allocation model. The reason for this is that to generate useful propensity scores, the model of treatment assignment must include factors that influence how aid is allocated and several recent studies have shown that bureaucratic inertia is important in aid allocation; more aid in the past leads to more aid in the future, with all else held constant (CITE Carey, others). Some may be uncomfortable with our inclusion of this variable in the selection equation because it means that we are not able to estimate the cumulative effects of democracy aid for any period longer than three years. While calculating such a cumulative effect would be desirable, it is also virtually impossible to do rigorously – we distrust the causal interpretation of any such estimates. We do try our procedure while omitting lagged democracy aid as a predictor in the propensity score model and find similar results to those presented here. We also include worldwide democracy aid flows which largely serves the same purpose as a time-trend or year dummies. This is essential because worldwide democracy aid flows have grown exponentially from almost nothing in the 1980s to being roughly 3 to 5 percent of overall aid by 2000.

The results of the propensity score model are shown in Figure 1. The coefficients on the predictors should probably not be interpreted causally (note that a propensity score model does not need to be causal), but the estimated effects are suggestive. We find that aid is clearly not randomly assigned – there are substantial and meaningful differences between country-years that receive different amounts of democracy aid.

Factors Affecting Democracy Aid Allocation

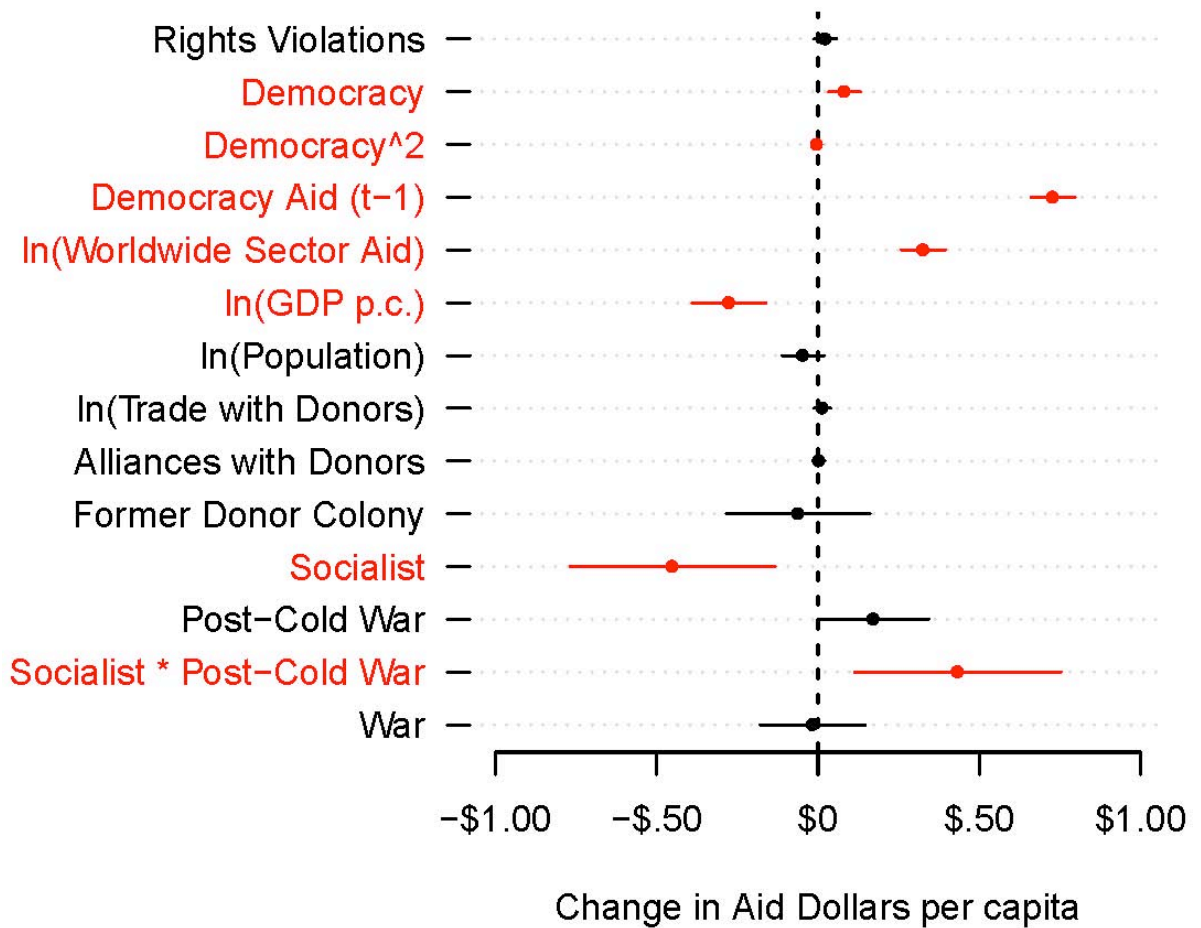


Figure 1: A model of democracy aid allocation that we use to generate propensity scores – the predicted amount of democracy aid for a given country-year. Variables in red are statistically significant at the 0.05 level.

First, we find that past democracy is an important predictor of democracy aid and that the relationship is in a direction that could have easily biased previous studies of democracy aid’s effects. Democracy aid flows disproportionately to *more* democratic countries. The effect is non-linear – country years with a polity score of roughly 12 receive the most democracy aid (we adjust the Polity IV scores to be between 0 and 20), and in general, states that are relatively

democratic receive more democracy aid than states that are more autocratic (see Figure 2). This correlation between past democracy and subsequent democracy aid may be the reason that previous studies of democracy aid often find large “effects” of aid on democracy.

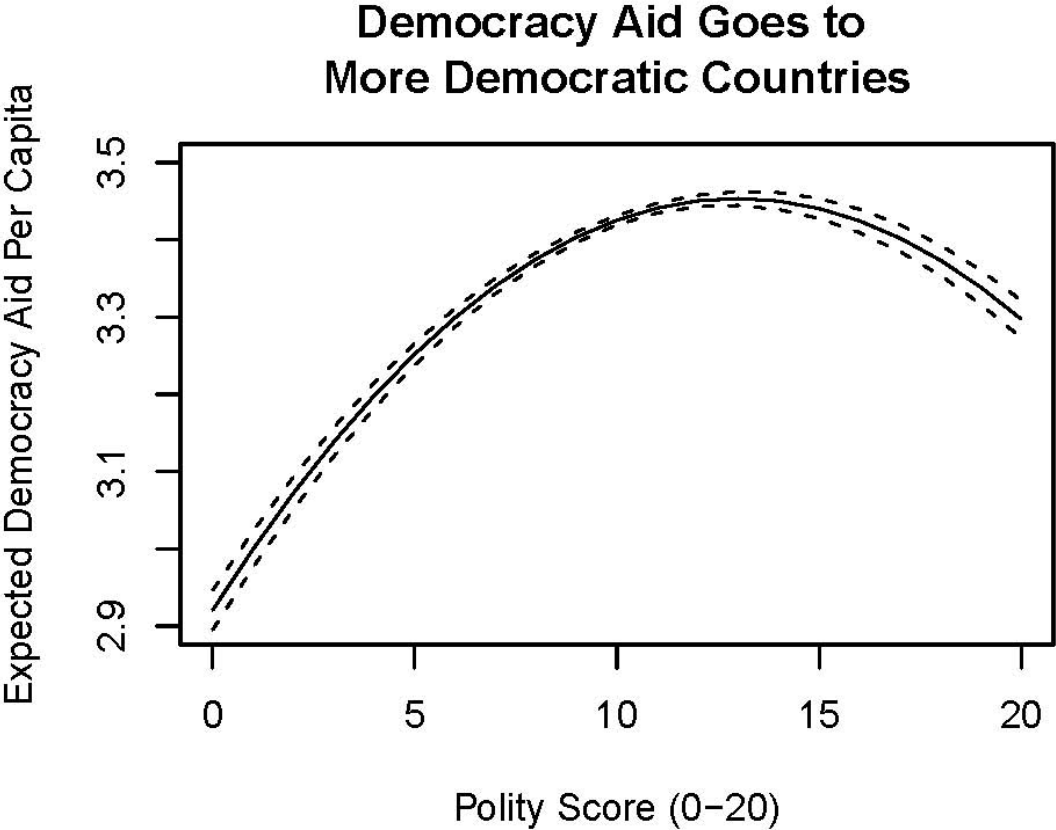


Figure 2: The marginal “effect” of increases in past democracy on the allocation of democracy aid, measured as $\ln(\text{Democracy aid per capita} + 1)$. Flows of democracy aid are highest to states that are moderately democratic – those with Polity IV scores between 10 and 16.

There are also other substantial differences between states that receive differing levels of democracy aid. In general, country-years that receive high levels of democracy aid have received much more democracy aid in the past. Democracy aid flows to the average country are

higher (not surprisingly) when global flows of democracy aid are higher. Countries that get more democracy aid tend to be poorer. During the Cold War, Socialist states received substantially less democracy aid (virtually none, in fact), but after the Cold War, these same states have been special targets of aid for democratization. Again, we note that each of these factors might also be related to future levels of democracy, meaning that without accounting for their influence on where democracy aid goes, past estimates of the effect of democracy aid on subsequent democracy may be seriously biased.

Having estimated a model of democracy aid allocation, we now use it to generate propensity scores for each country-year. In the case of a binary treatment (ex, “aid or not”), propensity scores are typically calculated by estimating a logistic regression predicting the treatment status of each unit. The predicted values from this logistic regression (ranging from zero to one) are then used as the propensity scores for each unit. In this setting with a simple binary treatment, the propensity score can be intuitively interpreted as the probability that a unit is assigned treatment. These propensity scores can then be used to match or subclassify the units in the sample; by comparing units that have similar propensity scores, analysts will be comparing units that had similar probabilities of receiving treatment, so actual assignment to treatment or control will be (hopefully) up to chance. Propensity scores are also a linear combination of the covariates in the propensity score model, so propensity scores provide a one-dimensional summary of the many-dimensional differences between units. Thus, within a matched sample or within matched subclasses, we are comparing units that have relatively similar values of the covariates predicting treatment. In short, rather than making extreme comparisons, the use of propensity scores allows us to compare “like to like.”

Our treatment is a continuous variable so we cannot use standard matching methods developed for binary treatments. Instead, we use a relatively new set of methods which use generalized propensity scores to create balance with multi-valued or continuous treatments (Dyk and Imai 2004). The basic intuition is the same as for propensity scores extracted from a logistic regression predicting binary treatment. However, we have used a tobit model to estimate the aid allocation model. As with the logit case, we simply take the predicted values for each unit as the generalized propensity score or “balancing” score. This no longer can be interpreted as the probability of treatment; instead it is the predicted amount of democracy aid for a given country year. Following the logic of propensity scores in the logit case, country-years that have similar balancing scores will have similar values of the background covariates. The major difference between propensity score methods with binary treatments and the generalized propensity score methods we use for our continuous treatment is that we require the assumption of linearity of the causal effect.

We collect the predicted values from the tobit regression, censoring the predicted values that are below zero to be exactly zero. Since the majority of country years do not receive much, if any, democracy aid, there are a substantial number of country-years that have generalized propensity scores of zero. We then subclassify the dataset into seven subclasses of observations. This is roughly in keeping with the rule of thumb developed by Cochran (1953) and extended by Rubin (1973) that using six or more subclasses removes roughly 90 percent of the bias for many applications. All of the observations with predicted aid values of zero are lumped together,

creating a rather large subclass with 818 country-years. We then divide the remaining data into six groups, roughly equally sized.²

The Causal Effects of Democracy Aid

Within each subclass, we estimate the subclass causal effect by estimating a linear regression in which the outcome is $Democracy_{i,t}$ and the treatment is our three-year lagged average of aid. We include a variety of other predictors that are common in models of democracy: Population, GDP per capita, GDP growth, Trade, literacy rates, a measure of ethnic fractionalization, oil production, percentages (in 1980) of Catholic, Muslim, and Protestant citizens, an indicator for whether a state has a British legal heritage, INGOs per capita, and a regional democracy measure, calculated as the average polity score of a country-year's neighboring states. These controls generally encompass the variables found to predict democracy scores in the quantitative literature on democratization, as noted above. We also include country-level random effects. Because we are estimating this model on subclasses of the data specified by propensity score ranges, the results within each subclass will be less model dependent than the same regression estimated using the entire sample (Ho et al., CITE). If we

² Inequalities in subgroup sizes are due to a missing data issue – some units that have complete data in the propensity score model are missing data (on different variables) in the subsequent causal model. We are working to fix this.

have successfully accounted for the factors that influence aid allocation,³ then the estimated coefficient on lagged democracy aid can be interpreted causally, as the effect of aid on democracy within the subgroup. We can then calculate the overall linear average effect by calculating the weighted average of the subclass estimates, with the weighting corresponding to the number of observations in each subclass.

Figure 3 shows the results of the seven models, one for each subclass, as well as the combined causal estimate (the average treatment effect) for all units, and the naïve estimate that we would have obtained if we used the hierarchical model *without* subclassification on propensity scores. The coefficients shown are only the coefficients on *Democracy Aid*. We do not report the coefficients of the control variables in these models because after matching, these other coefficients have no causal interpretation.

³ We cannot rule out that some unobserved and un-theorized variable is possibly driving our results, although we consider it unlikely.

Estimated Effect of Democracy Aid

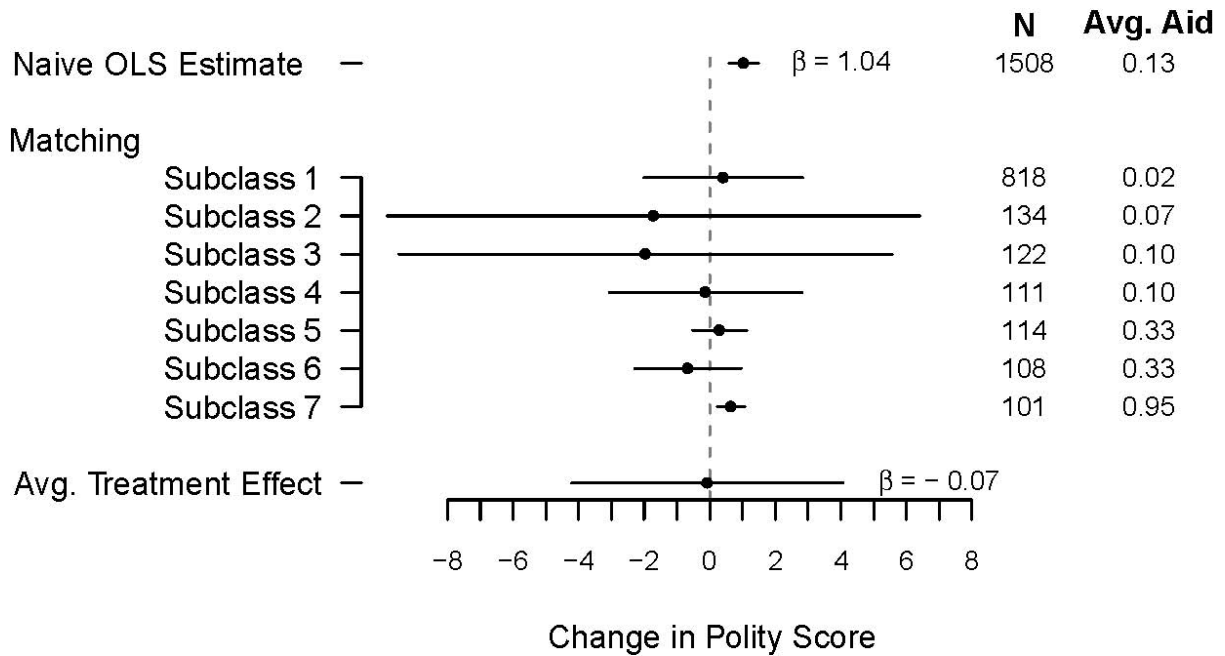


Figure 3: Estimates of the average effect of the past three years of democracy aid on the current year’s democracy levels. The “naïve OLS estimate” is obtained by estimating the hierarchical model described in the text using the entire sample without any matching or subclassification. The estimated causal effect within each subclass is shown and the average treatment effect (ATE) is the average effect for the seven subclasses, weighted by the number of observations in each subclass. “N” shows the number of country-years in each subclass, and “Avg. Aid” shows the average level of treatment – $\ln(\text{Democracy Aid } p.c. + 1)$ – in each subclass.

The results suggest that democracy aid is not generally effective at promoting democracy. In the largest subclass of observations – those with balancing scores of “0” – the estimated effect of aid is weakly positive but the standard errors are large meaning that this small increase is

statistically indistinguishable from zero. The estimates for subclasses 2 through 6 are similarly imprecise, with the 95 percent confidence intervals all containing zero. However, in subclass 7 – the country-years that were most likely to receive high levels of aid – the estimated effect of democracy aid is positive, statistically significant, and within the realm of plausibility. We find that for this subclass, the causal effect of a one standard deviation increase in democracy aid, a roughly 40 percent increase, is a 0.26 increase in Polity score that is statistically larger than zero. To some, this effect may seem too small. On a 21-point scale, an increase of a quarter point may seem insubstantial, but we note that a 10 percent increase in aid is completely feasible and that it would be rather optimistic to assume that an incremental increase can buy large jumps in democracy. If this were the case, the effectiveness of democracy aid would be fairly self-evident. Others may be skeptical that this effect is too large. We remind readers that we only find this substantial effect in a rather small subclass of country-years – the ones most likely to receive large amounts of aid. When we look at the combined average effect of an increase in aid for all country-years, the effect is virtually zero – a slightly negative coefficient buried in huge standard errors that give virtually no hint as to whether the effect should be negative or positive.

How would our results have differed if we had assumed that there were no selection effects operating on democracy aid allocation? With this naïve assumption, we would have been comfortable following the lead of other studies that simply estimate linear regression models linking aid to democratization, perhaps with a hierarchical structure to account for democratic “growth curves” (see Finkel et al. 2007). To see what results we would have obtained under this assumption, we estimated the same hierarchical model used in each of the subclasses, but this time we pool the entire sample of 1,508 country-years. In this model, we find that the coefficient on aid is extremely large and highly statistically significant (see Figure 3). This model seems to

be telling us that a 40 percent increase in democracy aid (for comparability to the 40 percent increase we discuss in the previous paragraph) would have resulted in almost a half-point increase in democracy scores. *This apparent “effect” is larger than the causal estimates we obtain in all of the subclasses.* If we had failed to account for the aid allocation process, we would have reached the radically different conclusion that democracy aid is wildly effective at increasing the democracy level of the average developing country!

How can it be that we would have obtained a large positive effect of aid when pooling the sample but that when we subclassify on propensity scores, the average effect is zero? This is a classic case of Simpson’s paradox, where conditioning on a variable can reverse the apparent direction of a relationship.⁴ In this case, if we failed to condition on the factors affecting aid allocation (by subclassifying on the propensity score), then we would have erroneously found that the apparent association between aid and democracy was causal. Instead, we have shown that most of this “causality” probably flows the other way; prior democracy is highly predictive of democracy aid.

We fear that this failure to model the allocation of democracy aid has led to optimistic estimates of the effectiveness of democracy aid. We find it likely that the estimates obtained by previous studies may be overly large. These studies assume, as is conventional for the sake of modeling, that aid allocation is close to random – or at least close enough that a regression model with some hierarchical structure can adequately account for the selection effects (Finkel et al. 2007). While these studies are careful and represent the state of the art at the time of publication,

⁴ Footnote giving an example of Simpson’s paradox here. The Berkeley admissions example.

our results suggest skepticism that selection bias has been adequately addressed in prior scholarship.

We are likewise skeptical of results obtained using various instrumental variables in two-stage least-squares regression and Arellano-Bond GMM-style estimators (Finkel et al, others). These instruments do not strike us as credibly satisfying the assumptions on which IV estimation hinges. And with bad instruments, IV regression may be severely biased, sometimes leading to estimates that are worse than those of simple linear regression. Finkel et al. (2007) argue that the similar effects they obtain with and without a battery of instrumental variables indicates that their results are robust to “reverse causation” resulting from selection effects in the aid allocation process.

However, we have shown that the selection effects are large and we recover dramatically different estimates when we ignore selection vs. when we account for it. It seems more likely that the instruments used by Finkel et al do not satisfy the necessary exclusion restriction and are generally not that influential in the aid allocation process, so their IV results are simply not working to solve the problem of endogeneity. Given that we have shown that strong selection effects exist, this is the more reasonable conclusion to draw from the similarity of their OLS and IV results.

We now turn to a more detailed exposition of what our findings may mean. From our matching results, we conclude that democracy aid is largely ineffective; donor countries cannot simply buy democracy by giving democracy aid to countries of any stripe. Still, the large standard errors suggest that there is high variance in the effectiveness of democracy aid. We do *not* find a precisely estimated “zero” effect for the majority of countries. Instead, the high

variance around our estimates may suggest that the effects of democracy aid are highly variable, helping in some countries and hurting in others.

The positive effect of aid that we find in the subclass of country-years most likely to receive large amounts of democracy aid indicates that donors may be able to identify states that are most likely to benefit from democracy aid and target these states with increased funds at critical times. At this end, we believe that donors effectively engage in triage – giving extremely large amounts of democracy aid where it will actually help and giving only small amounts (or nothing) to states that have democracy deficits but are unlikely to be shifted by aid flows.

What do these promising states look like? For one thing, they are already relatively democratic at the time they receive aid. The 101 country-years in the subclass where democracy aid is effective have an average prior democracy score of 14.5 (with 0 being pure autocracy and 20 being complete democracy). In contrast, the country-years in the other six subclasses where aid is ineffective all have lower average prior democracy scores and the subclass that is predicted to receive no democracy aid has relatively autocratic average democracy score of 6.3. It seems that in terms of democracy and aid, the biblical injunction holds true: “For he that has, to him shall be given.”

Perhaps not surprisingly, the states where democracy aid is effective have received substantially more democracy aid in the past. The country-years that had the highest propensity scores (and for which aid was effective) had an average of .89 logged aid dollars per capita in years $t-4$, $t-5$, and $t-6$, while those with the lowest propensity scores had an average of only .02 logged aid dollars per capita previous to treatment. Because we have conditioned on this past aid, we emphasize that this prior democracy aid is not somehow seeping into our causal estimate reported above. However, this previous aid is clearly important in donors’ decisions to give

more democracy aid. This may mean that democracy aid in the past primes countries such that subsequent democracy aid will be more effective; this interpretation is entirely consistent with our findings although we cannot test it directly with our statistical models. The “most likely” subclass was also slightly poorer than some of the other subclasses, although the differences were not particularly large. These recipients were also more likely to have been former socialist states.

Our causal framework does not allow us to do more than speculate that these factors may interact with democracy aid to make it more effective. This assertion would be equivalent to positing an interaction effect between, say, current democracy levels and democracy aid that would combine to result in varying levels of aid effectiveness at different levels of democracy. To test this via matching, we would need to reframe the causal question in terms of this interaction and then explicitly match on factors that influence the joint distribution of aid and prior democracy. Such an analysis is beyond the scope of this paper but suggests a promising avenue of future research.

Conclusion

The evidence supports our theoretical priors that democracy aid is sometimes effective and that donors are perhaps relatively skilled at allocating democracy aid where it will increase democracy most. If the argument is in fact correct, it would suggest that at least some of the systematic differences between countries that get low aid and those that get high aid are likely to be enabling factors which allow democracy aid to be effective. Our finding that democracy aid is most effective in states that are already partially democratic is most suggestive in this regard.

It conforms to the logic of several previous authors who argue that increasing democracy aid to autocratic societies will often harm democracy by forcing autocratic rulers to use repression to consolidate their rule in the face of increased opposition. It also conforms with statements by major donors such as USAID that claim to primarily “support to democratic opportunities and assist ‘democratic breakthrough’” with democracy assistance (CITE is in AJPS version of RHR). In the end, we find strongly suggestive evidence of donor triage in aid allocation from 1980-2004. But, as always, there remains much work to do to check for robustness and to eliminate alternative explanations of the patterns we observe.

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