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Aid and growth: New evidence using an excludable instrument

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Abstract. We use an excludable instrument to test the effect of bilateral foreign aid on economic growth in a sample of 97 recipient countries over the 1974–2013 period. Our instrument interacts donor government fractionalization with a recipient country's probability of receiving aid. The results show that fractionalization increases donors' aid budgets, representing the variation over time of our instrument, while the probability of receiving aid introduces variation across recipient countries. Controlling for country- and period-specific fixed effects that capture the levels of the interacted variables, the interaction provides a powerful and excludable instrument. Making use of the instrument, our results show a positive but insignificant effect of aid on growth. We also investigate the effect of aid on consumption, savings, investments and net exports and investigate heterogeneity according to the quality of economic policy, democracy and the Cold War period. We find that aid increases investment and consumption, while it decreases net exports. In no regression do we find that aid affects growth. However, the coefficients from the instrumental variables regressions are also not statistically different from the positive and significant OLS estimates.

Résumé. *Aide et croissance : nouveaux éléments de preuve grâce à un instrument exclusif.* À l'aide d'un instrument exclusif, nous évaluons l'impact de l'aide étrangère bilatérale sur la croissance économique d'un échantillon de 97 pays bénéficiaires entre 1974 et 2013. Notre instrument met en interaction le fractionnement de l'aide apportée par les gouvernements contributeurs et la probabilité qu'un pays bénéficiaire puisse recevoir de l'aide extérieure. Les résultats suggèrent qu'en matière d'aide, le fractionnement entraîne une augmentation des budgets des pays contributeurs, constituant ainsi la

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variable dans le temps de notre instrument. La probabilité de recevoir de l'aide, quant à elle, introduit la variable parmi les pays bénéficiaires. Cette mise en interaction, tenant compte des effets fixes spécifiques au niveau des pays et des périodes, et reflétant le niveau des variables dépendantes, offre un outil puissant et exclusif. Grâce à cet instrument, nos résultats indiquent que l'aide extérieure exerce une relation positive mais négligeable sur la croissance. Dans cet article, nous étudions également l'effet de l'aide étrangère sur la consommation, l'épargne, l'investissement et les exportations nettes, ainsi que l'hétérogénéité à l'aune de la qualité des politiques économiques, du niveau démocratique et de la période de guerre froide. Nous constatons que l'aide étrangère permet d'augmenter l'investissement et la consommation, mais à tendance à diminuer les exportations nettes. Hors modèle de régression, nous constatons que l'aide extérieure exerce une incidence sur la croissance. Néanmoins, les coefficients issus des régressions à variables instrumentales ne sont pas statistiquement différents des estimations positives et significatives réalisées par la méthode des moindres carrés ordinaire.

JEL classification: O19, O11, F35, F53

1. Introduction

IN A PREVIOUS PAPER, we began with an apology for adding yet another paper investigating the effect of foreign aid on economic growth to what is already a long list of articles (Dreher et al. 2018a). We frankly admitted that we were unable to provide an unbiased estimate of aid's effect on growth—as is true for most of the preceding literature. Since then, a number of innovative contributions have added to our understanding of whether and to what extent aid causes growth. Jackson (2014) suggests using natural disasters in countries receiving aid from the same donor as an instrument. Galiani et al. (2017) instrument aid flows with the International Development Association's (IDA) threshold for receiving concessional aid. While interesting and innovative, we remain unconvinced of these identification strategies. Jackson's suggestion of increased short-term aid for countries unaffected by disaster as a consequence of disasters in other aid recipient countries from the same donor, while empirically powerful, lacks a theoretical foundation and is thus potentially spurious. Galiani et al.'s (2017) instrument could be correlated with growth for reasons other than aid, as countries' rates of growth might be influenced by factors other than aid at the time they exceed the IDA's income threshold.¹ The lack of a plausibly excludable instrument for aid in

1 This would hold even if the decision to pass the IDA's income threshold could not be manipulated by aid-receiving governments. Consider a reform-oriented government that achieves substantially higher growth rates for some years that eventually lead to passing the exogenous threshold. Growth dynamics will be different in these years compared to the years in which the country does not grow, even with an exogenous income threshold. What is more, governments can manipulate GDP data, which makes reaching the threshold potentially endogenous (see, Kerner et al. 2017, who show this for aid-dependent countries). Galiani et al. (2017) test for these possibilities. Using a smoothed

a large sample of donor and recipient countries continues to plague the aid effectiveness literature at large. The question of whether aid affects recipient countries' economic growth thus remains wide open.²

In this paper, we aim to fill this gap. We are inspired by the identification strategies of Werker et al. (2009), Nunn and Qian (2014) and Ahmed (2016). These studies rely on plausibly excludable variables that do not vary at the recipient country level and interact it with a proxy for the probability of receiving aid. We borrow from Ahmed (2016) who exploits variation in the composition of the United States' House of Representatives to instrument US aid in explaining recipient country democracy. To the extent that fractionalization leads to larger government budgets and larger overall budgets lead to an increase in the aid budget, fractionalization can serve as a powerful instrument. In line with Nunn and Qian (2014) and Ahmed (2016), we introduce variation at the recipient country level by interacting fractionalization with the share of years in our sample that a country receives aid from its donors.³ To the extent that variables correlated with donor fractionalization do not affect recipients' rates of growth differently in regular and irregular recipients of aid, controlled for country and period fixed effects, the resulting instrument is excludable. Contrary to Nunn and Qian (2014) and Ahmed (2016), we focus on growth rather than democracy or conflict and on aid from a group of major donors rather than (food) aid from the United States exclusively. Other than Werker et al. (2009), we focus on a broad set of donor countries. As we outline in more detail in section 2, we investigate the link between donor government fractionalization and the effectiveness of aid as a chain of cause-and-effect relationships. Starting with the effect of fractionalization on government budgets, we further illustrate the relation between overall budgets and aid budgets.

In addition to investigating the effect of aid on growth, this paper's contribution is the introduction of an instrument for aid from a large number of donors and years that can be used to address a substantial number of questions in the aid effectiveness literature. Though still relatively new, the instrument that we introduce in this paper has already been used, for instance,

income trajectory to rule out the effect of shocks they find results that are similar to their main analysis. They find no evidence of data manipulation. However, their sample covers only 35 countries. Dreher and Lohmann (2015) focus on regional growth within countries. Their instrument for aid is an interaction of the IDA income threshold with a region's probability to receive aid, in a sample of 21 countries.

- 2 Among prominent recent attempts to investigate the effect of aid, Clemens et al. (2012) do not use instruments and Brückner (2013) relies on rainfall and commodity price shocks, which can easily violate the exclusion restriction. See Werker (2012), Dreher et al. (2018b) and Doucouliagos (2019) for recent surveys.
- 3 Werker et al. (2009) focus on aid from Arab donors and rely on a binary indicator identifying Muslim recipient countries, which are more likely to receive such aid compared to non-Muslim countries.

in Ziaja (2017) in the context of democracy, Bluhm et al. (2020) on conflict, Doucouliagos et al. (2019) on infant mortality or Dreher et al. (2019) on refugee flows.⁴ We suggest a number of additional research questions where we think our instrument helps overcoming the endogeneity of aid in the conclusion.

We describe our data and method in more detail in section 3. To foreshadow our results (shown in section 4), we find that the interaction of government fractionalization and a country's probability of receiving aid is a powerful instrument for aid. Using this instrument, we find a positive but insignificant effect of aid on growth in the overall sample. Comparing our instrumental variables estimates to simple OLS regressions with a Hausman test, we, however, cannot reject the hypothesis that the instrumental variables estimate equals the positive and significant estimate of the OLS regression. According to the more efficient OLS results, an increase in lagged aid by one percentage point of GDP is accompanied by higher growth of a magnitude of 0.24 percentage points. We also investigate the effect of aid on components of GDP rather than growth (in section 5) and find that it significantly increases investment and (private) consumption, while it decreases net exports. Section 6 investigates heterogeneity along a number of potentially important dimensions—the quality of economic policy, democracy and the Cold War—but finds no differential impact in aid's effect on growth across these groups. The final section summarizes and concludes the paper.

2. The argument

Most of the previous literature pursues one of three strategies to identify the effect of aid on growth. One group of papers relies on instruments that relate to the size of the recipient country's population (as a proxy for the ease to exercise power, e.g., Rajan and Subramanian 2008). A second group of papers focuses on bilateral political relations, for example employing voting coincidence in the United Nations General Assembly to instrument for aid (Bjørnskov 2019). The third uses internal instruments and estimates difference or system GMM regressions (Minoiu and Reddy 2010). Each of these strategies is misguided. Population size can affect growth through many channels that researchers cannot control for and is thus not excludable (Bazzi and Clemens 2013). Lagged levels and differences of aid are also hardly excludable to growth, invalidating them as (internal) instruments. Political-relations based variables might be excludable, but to the extent that the motive for granting aid affects the outcome, the resulting local average treatment effect (LATE) reflects the effects of politically motivated aid rather than those of all aid (Dreher et al. 2018a).

4 Variants of our instrument have also been used to instrument International Monetary Fund loans (Lang 2016, Gehring and Lang 2020, Stubbs et al. 2020) and World Bank lending (Dreher et al. 2017, Jensen et al. 2019, Gehring et al. 2019).

A couple of recent papers suggest alternative identification strategies, based on interactions between an excludable instrument and a potentially endogenous variable (Werker et al. 2009, Nunn and Qian 2014, Ahmed 2016). Of these, only Werker et al. (2009) investigate the question that we address in this paper—the effect of foreign aid on economic growth. Werker et al. (2009) make use of oil price fluctuations that substantially increase the aid budgets of oil-producing Arab donors, in particular to Muslim countries. Specifically, their instrument for Arab aid is the interaction of the oil price with a binary indicator for Muslim recipient countries, which receive the bulk of Arab donors' aid. They find recipient country growth to be unrelated to aid. While we are convinced of Werker et al.'s (2009) identification strategy, their results can hardly be generalized to represent the effects of aid more broadly. As they point out, their results show the LATE for oil-price-induced increases in aid to Muslim countries, which might be unrepresentative of aid from a broader set of donors to a broader set of recipients. In particular, the modalities of aid delivery as well as the political motivations of this aid might reduce its effectiveness, as might the specific set of policies and institutions in the largely authoritarian recipient countries of aid from Arab donors (Werker et al. 2009, Dreher et al. 2018a). We build on Werker et al.'s (2009) identification strategy, closely following Nunn and Qian (2014) and, in particular, Ahmed (2016), but focusing on aid's effect on growth for a large set of aid donors and recipients, over a long period of time.

We rely on two additional strands of previous literature to motivate our instrument for aid. The first investigates the effect of government fractionalization on their budgets. Roubini and Sachs (1989) propose that coalition governments will be more reluctant to reduce expenditures compared with single-party governments, as each party of the coalition will resist pressure to cut expenditure in its own area, even if they are in favour of overall spending cuts. Volkerink and de Haan (2001) and Scartascini and Crain (2002) show that legislature fragmentation increases governments' expenditures. We make use of the relationship between fractionalization and government budgets, hypothesizing that the larger budgets arising due to fractionalization increase aid budgets, which in turn affect aid disbursements at the recipient country level. Most importantly, controlling for period and recipient fixed effects, government fractionalization in donor countries is arguably excludable in growth regressions at the recipient country level.

One might consider two alternative instruments resulting from our hypothesized transmission channels: government expenditures and aid budgets. These instruments are, however, not necessarily excludable, given that growth shocks in recipient countries could directly affect donors' aid budgets (and thus their overall budgets), while growth shocks in non-recipient countries might not. For example, Rodella-Boitreaud and Wagner (2011) show that donors' total aid budgets increase with natural disasters in developing countries, indicating that donors adjust their total aid budget in response to shocks rather than

merely reallocating aid while holding budgets constant. We therefore do not use government expenditures and aid budgets as instruments.⁵

3. Method and data

Our growth models broadly follow the approach in Clemens et al. (2012). However, Clemens et al. do not use instruments, but claim to address the endogeneity of aid by differencing the regression equation and lagging aid, so that it can reasonably be expected to cause growth rather than being its effect. Their estimates could still be biased in either direction. For example, donors might grant more aid to an incoming reform-oriented government. Increased growth resulting from reforms could then spuriously be attributed to the increases in aid. On the other hand, donors might give more aid to countries where they anticipate that shocks will reduce future growth rates (Dreher et al. 2018a). This is in line with Roodman (2015), who finds that Clemens et al. (2012) fail to remove contemporaneous endogeneity, which is why we see the need for using a new instrumental variables strategy.

We broadly base our analysis on Clemens et al.'s (2012) permutations of Burnside and Dollar (2000), the study that has arguably gained most attention in the literature on aid and growth.⁶

In terms of timing, our preferred specifications follow Clemens et al. (2012) and assume that disbursed aid takes one four-year period to become effective in increasing or decreasing economic growth (but we also report contemporaneous effects of aid on growth within the same four-year period for comparison). We estimate OLS regressions with country fixed effects,⁷ at the recipient–period level:

5 Some previous papers rely on aid budgets as an instrument for aid. One example is Hodler and Raschky's (2014) analysis of how aid affects nightlight at the regional level. See Temple and Van de Sijpe's (2017) analysis of how aid affects various components of GDP for a discussion on how endogeneity can be alleviated by filtering out common factors that have heterogeneous effects on the variable of interest. In Chauvet and Ehrhart's (2018) analysis of aid's effect on firm growth in 29 developing countries they instrument for aid using fiscal revenue as a share of donors' GDP (interacted with joint religion or colonial history).

6 We rely on Minasyan's (2016) update of these data until 2009 and further updated the data until 2013, which is the most recent four-year period available as of September 2018. When we include more recent data as an additional three-year rather than four-year period, our results are unchanged, though the power of our instrument is lower due to insufficient variation in government fractionalization in the more recent years.

7 Clemens et al. (2012) seem to prefer a measure of early impact aid over all aid. This measure has been shown not to be a robust predictor of growth (Rajan and Subramanian 2008, Bjørnskov 2019, Roodman 2015). What is more, a major drawback with this measure is that disaggregated aid disbursements are

$$Growth_{i,t} = \beta_1 Aid_{i,t-1} + \beta_2 Aid_{i,t-1}^2 + \mathbf{X}'_{i,t} \beta_3 + \beta_4 \eta_i + \beta_5 \tau_t + \epsilon_{i,t}. \tag{1}$$

$Growth_{i,t}$ is recipient country i 's average yearly real GDP per capita growth over a four-year period t .⁸ $Aid_{i,t-1}$ denotes the amount of net aid (in percent of GDP) disbursed by the 28 bilateral donors of the OECD's DAC in the previous period. Some specifications also include aid squared to test for decreasing returns to aid, following Clemens et al. (2012). Our OLS regressions include the set of control variables used in Burnside and Dollar (2000), which we denote as $\mathbf{X}_{i,t}$: (log) GDP per capita in the first year of each period (Log initial GDP pc),⁹ Assassinations, Ethnic Fractionalization*Assassinations and M2/GDP (lagged).¹⁰ η_i represent recipient country fixed effects, τ_t period fixed effects and $\epsilon_{i,t}$ the error term. Standard errors are clustered at the recipient country level.

While we report these conditional correlations between aid and growth for comparison, aid and growth are jointly determined by a large set of variables that no analysis can reasonably hope to control for. We therefore rely on instrumental variables regressions rather than OLS to test the causal effects of aid. Our instrument for bilateral aid is an interaction of a variable that varies over time for each donor j —donor-government fractionalization $FRAC_{j,t}$ —and a time-invariant variable that varies at the donor-recipient

not available for the entire period, so that disbursements have to be estimated based on commitments. Data on commitments in the earlier periods also suffer from severe under-reporting, which is not addressed in Clemens et al. (2012) (see OECD/DAC CRS Technical Guide, Coverage Ratios, accessed on March 3, 2014, at www.oecd.org/dac/stats/crsguide.htm). The lack of data extends to so-called Country Programmable Aid, which is only those parts of aid that reach the recipient country. We therefore prefer to focus on overall aid.

8 We include recipient countries that have been on at least one “DAC List of ODA Recipients” between 1997 and 2013. Appendix A1 shows these countries.

9 A skeptical reader might be concerned about the Nickell bias arising from the inclusion of initial GDP per capita. When we exclude initial GDP per capita, our results remain robust. When we correct for the bias by applying the procedure developed by Bruno (2005a, 2005b) for unbalanced dynamic panel models using the Anderson–Hsiao and Arellano–Bond estimators, our results are equally unchanged, irrespective of whether we include the remaining covariates.

10 Burnside and Dollar (2000) in addition include time-invariant measures of Ethnic Fractionalization and Institutional Quality, as well as dummies for sub-Saharan Africa and East Asia. As in Clemens et al. (2012), these time-invariant variables are removed here through the inclusion of country fixed effects. Also note that we do not control for Burnside and Dollar's (2000) measure of good policy, given that we lose about 200 observations when we include it. As we show in appendix table A3, our results do not depend on its exclusion. The same holds when we include (log) population, Trade/GDP or FDI inflows/GDP (either with the whole world or with DAC donors only). Appendix A2 reports the sources and definitions of all variables, while we show descriptive statistics in appendix A3.

level—the probability of receiving aid from a particular donor $p_{i,j}$, so that the resulting interaction varies across both time and space. We proxy a country's probability of receiving aid with the percentage of years the country received aid from a particular donor over the sample period, following Ahmed (2016) and Nunn and Qian (2014). Specifically, the probability of receiving aid from a particular donor j is $p_{i,j} = \frac{1}{40} \sum_{y=1}^{40} p_{i,j,y}$, with $p_{i,j,y}$ indicating whether recipient i received positive amounts of aid from donor j in year y .¹¹

In a nutshell, our instrument exploits the exogenous variation resulting from a differential effect of donor-government fractionalization on aid for regular and irregular recipient countries. We motivate the components of our interacted instruments in turn.

One of our instrument's two components is the probability to receive aid $p_{i,j}$. We expect that the extent to which changes in donors' aid budgets affect aid receipts depends on a country's probability of receiving aid. Both Nunn and Qian (2014) and Ahmed (2016) show that the probability of receiving aid is indeed significantly correlated with the amount of US (food) aid a country receives. The same holds for our sample, for a broad set of donors, as can be seen in panel A of figure 1. The figure plots the average probability of receiving aid (i.e., recipient i 's probability of receiving aid from any donor over the sample period) on the horizontal axis and the average aid received from all donors as a percentage of GDP on the vertical axis. The correlation between the two is 0.30, significant at the 1% level. For example, the figure shows that Afghanistan received aid in 66% of the years in the 1974–2013 period, amounting to about 33% of its GDP. On the lower end of the scale, Kuwait received 0.0076% of its GDP as aid and received aid in 7% of the years in the sample.

The second component of our instrument is political fractionalization in the donor country. A priori, it is unclear whether legislature or government fractionalization is more suitable as an instrument. As Ahmed (2016) points out for the United States, the “funding and allocation of bilateral economic aid involves both the executive branch and Congress” (p. 184) and the same is

11 To test robustness, we alternatively included the probability to receive aid over each four-year period (and its interaction with fractionalization) rather than those over the whole sample period. Our results do not depend on this choice, but the instrument lacks power. One might also think of using the amount of aid each country receives from a donor in the recipient's aid budget rather than the probability to receive aid as part of the interacted instrument. However, the first-stage F-statistic is again too low for this alternative to be feasible, and the same holds when we focus on the importance of each recipient country in a donor's aid budget. When we calculate the probability to receive aid based on the number of years a recipient received aid in the 1970–1973 period (i.e., in the years prior to our estimation sample), the instrument is sufficiently strong in the first stage (with F-statistics of 13.88 for contemporaneous aid and 12.04 for aid lagged by one period). Appendix table A4 shows that the coefficient of aid in the second stage is negative and insignificant.

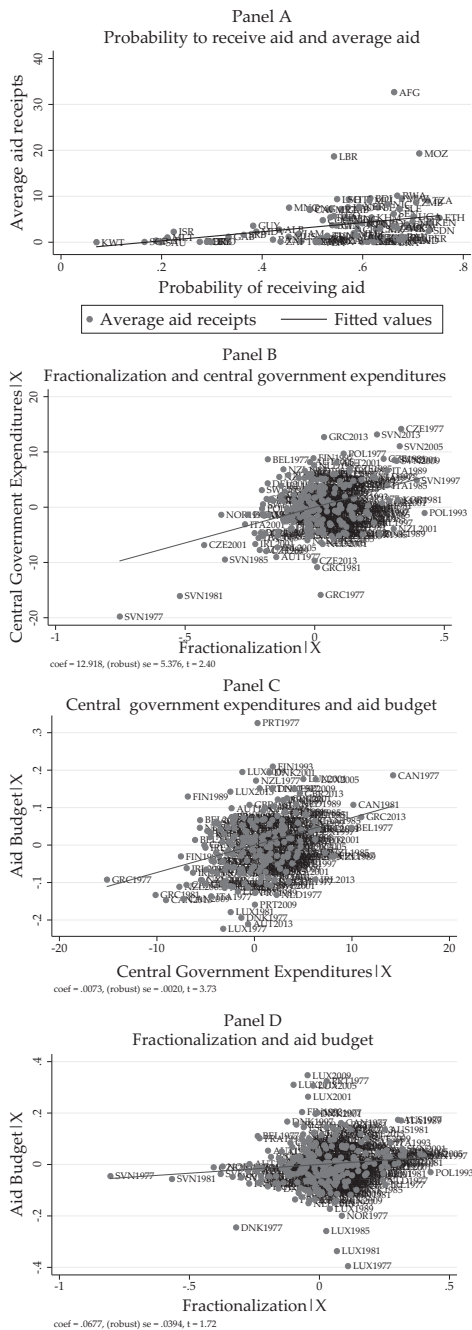


FIGURE 1
NOTES: All panels are based on the 1974–2013 period. Panel B is based on table A6, column 1. Panel C is based on table A7, column 1. Panel D is based on table A8, column 1.

true for the other donor countries in our sample. As it is the government that drafts the budget plan and not the legislature, we measure donor fractionalization as the probability that two randomly chosen deputies from among the parties forming the government represent different parties (Beck et al. 2001). This would come at the disadvantage that there is no variation in government fractionalization for the United States and Canada across our period of observation. We therefore replace government fractionalization with legislature fractionalization for these countries.¹² Our results are unchanged when we: (i) do not replace these values, (ii) omit the two countries and (iii) use legislature instead of government fractionalization for all countries.

In what follows, we explain in more detail how we expect government fractionalization to increase donor government expenditure and expect government expenditure to lead to higher aid budgets (and, in turn, larger allocations at the recipient country level). As is well established in the political economy literature, donor-government and donor-legislature fractionalization are important determinants of overall government expenditures, due to the logrolling involved when more parties govern in concert. To establish the link between fractionalization and aid disbursements in our sample, we proceed with re-estimating specifications from the previous literature, illustrating this link with our data, at the recipient-period level.

Among others, Roubini and Sachs (1989) and Scartascini and Crain (2002) show that legislature fragmentation increases governments' expenditures. Appendix table A6 closely follows their regressions (but includes our measure of fractionalization rather than theirs). The dependent variable is the donor country's annual central government expenditure as a share of GDP over the 1974–2013 period, focusing on four-year averages, as in our main regressions. As can be seen, government expenditures increase significantly with fractionalization, at the 5% level of significance. The estimated effect of an increase in fractionalization from zero to one is in the range of a 2.2 to 12.9 percentage point increase in central government expenditures (with a sample average of 34.7%). Panel B of figure 1 shows the partial leverage plot for fractionalization corresponding to the regression of column 1 in table A6. The figure shows that the results are not driven by obvious outlying observations.

The second well-established strand of literature we draw from addresses the relationship between overall government budgets and their aid budgets. Brech and Potrafke (2014) and Round and Odedokun (2004) show that overall

12 Unsurprisingly, government fractionalization in Canada and the United States is constant. While most DAC donor countries have parliamentary systems with proportional representation, there are exceptions (e.g., plurality voting system in Canada and presidential elections in the United States). The United Kingdom and France also differ from the remaining donors as they lack proportional representation. However, in both countries government fractionalization varies. In a robustness test, we also replace government fractionalization with legislature fractionalization for the United Kingdom and France. Results remain unchanged.

expenditures as a share of GDP significantly determine aid budgets. Table A7 shows how an increase in central government expenditures translates into larger aid budgets in our sample, broadly following the regressions of Fuchs et al. (2014). Column 1 shows that an increase in central government expenditures by one percentage point increases governments’ aid budgets by about 0.007 percentage points, at the 1% level of significance. For the average country in our sample this amounts to a maximum increase of 1.75% of its government’s aid budget. Put differently, a one standard deviation increase in expenditures translates into a 0.06 percentage point increase in the aid budget to GDP ratio, which represents 24% of its standard deviation.

Panel C of figure 1 shows the partial leverage plot between government expenditures and aid budgets, based on column 1 of table A7, suggesting a high positive correlation between central government expenditures and aid budgets.

We finally explore the direct link between fractionalization and aid budgets. Column 1 of table A8 tests this link for our sample, using averages over four-year periods and controlling for donor-country and period fixed effects. As can be seen, the correlation is positive, at the 10% level of significance (also see the partial leverage plot in panel D of figure 1). Interestingly, in line with our hypothesis in this paper, Round and Odedokun’s (2004) regressions excluding government expenditures show that government fractionalization increases aid budgets, “apparently to satisfy the various interests of the coalition” (p. 308). To the contrary, fractionalization does not turn out as robust determinant of aid budgets in the large-scale robustness analysis in Fuchs et al. (2014). Their regressions, however, include various measures of fractionalization and fiscal policy at the same time, setting a high bar on the identification of the individual effects. When we replicate their regressions using their original data and method controlling for (lagged and logged) per capita GDP only, government fractionalization turns out to be significant at the 10% level at least (see columns 2 to 4 in table A8).

Armed with this chain of causes and effects from donor-government fractionalization to aid volumes at the recipient-country level, our instrumental variable (IV) is

$$IV_{i,t} = \sum_j FRAC_{j,t} * p_{i,j}. \tag{2}$$

We thus estimate a first-stage regression at the recipient–period level as follows:

$$Aid_{i,t} = \beta_1 IV_{i,t} + \mathbf{X}'_{i,t} \beta_2 + \beta_3 \eta_i + \beta_4 \tau_t + \epsilon_{i,t}. \tag{3}$$

We predict aid at the recipient–period level $Aid_{i,t}$ with the sum over all donors of the interaction of donor government fractionalization $FRAC_{j,t}$ and the probability that recipient i receives aid from donor j — $p_{i,j}$ —which varies across donor–recipient pairs.¹³ In our setting this approach is equivalent to

13 Given that we use a constructed instrument, we bootstrap standard errors with 500 replications (and again cluster at the recipient-country level). Instead of

using a “zero stage procedure” that runs a zero stage regression at the donor–recipient–period level where bilateral aid from all donors j to all recipients i is predicted with the instrumental variable $FRAC_{j,t} * p_{i,j}$. This is because we have only one dyadic instrumental variable in our setting, such that both approaches lead to identical results.

One might be concerned about potential direct effects of the probability of receiving aid on economic growth. However, our growth regressions control for the effect of the probability of receiving aid as well as the level of donor fractionalization through the inclusion of recipient country and period fixed effects. Given that the effect of the potentially endogenous variable is controlled for, the interaction of the endogenous variable with an exogenous one is exogenous under mild assumptions (Nunn and Qian 2014, Nizalova and Murtazashvili 2016, Bun and Harrison 2018 and appendix S.4 in Dreher et al. 2018a).

Though the “treatment” in our setting is continuous, the intuition of our regressions is that of a difference-in-differences approach, where we investigate a differential effect of donor-government fractionalization on the amount of aid to countries with a high compared to a low probability of receiving aid. The identifying assumption is that growth in countries with differing probabilities of receiving aid will not be affected differently by changes in fractionalization, other than via the impact of aid, controlling for recipient country and period fixed effects. In other words, as in any difference-in-differences-like setting, we rely on an exogenous (continuous) treatment and the absence of different pre-trends across groups. Controlled for period fixed effects, donor-government fractionalization cannot be correlated with the error term and is thus clearly exogenous to aid. In order for different pre-trends to exist, these trends across countries with a high compared to a low probability to receive aid would have to vary in tandem with period-to-period changes in donor-government fractionalization. Given that donor-government fractionalization follows no obvious trend in our data, we consider this unlikely.

In order to probe the plausibility of the assumption that our results are not driven by omitted variables that affect regular and irregular recipients of aid differently, we estimate a dyadic regression with aid from donor j to recipient i in a period t as dependent variable. Table 1 shows that the first-stage F-statistic on our instrument is strong, in particular when we add our set of control variables (see panel B of column 1). The coefficient in panel A

exploiting the contemporaneous variation of our instrumental variable, we could as well lag fractionalization (and its interaction) to allow for some time to pass between aid commitments and disbursements. We prefer to focus on contemporaneous values, in line with the previous literature. When we lag fractionalization by one four-year period, our second-stage results are unchanged, and the instrument remains strong. As a falsification test, we also used fractionalization one period in the future interacted with the probability of receiving aid. As one would expect, the first-stage F-statistic is below one, indicating the lack of power of future fractionalization.

TABLE 1
Dyadic (first-stage) regressions, aid/GDP and alternative outcomes

	(1) Aid/GDP	(2) Trade/GDP	(3) FDI/GDP	(4) Refugees	(5) Asylum seekers
Panel A: Without covariates					
IV	0.206*** (0.066)	1.187 (1.035)	−1.063 (0.732)	0.050 (0.144)	0.090 (0.131)
Number of obs.	35,803	35,531	11,526	34,258	34,258
K–P F-stat.	9.70	1.24	2.11	0.12	0.47
Panel B: With covariates					
IV	0.189*** (0.046)	1.299 (1.212)	−0.290 (0.325)	0.030 (0.155)	0.019 (0.152)
Number of obs.	21,974	21,966	7,297	20,587	20,587
K–P F-stat.	16.45	1.07	0.80	0.04	0.02

NOTES: Data are averaged over four years at the donor–recipient–period level. Period and donor–recipient fixed effects included. Columns 2 to 5 show placebo regressions with outcomes that our instrument should not be able to predict. Panel B includes the Burnside & Dollar set of covariates as in table A3, column 1. Standard errors are in parentheses (clustered at the donor–recipient level). Refugees and asylum seekers are net flows in logarithms. All variables are at the dyadic level. Significance levels: * 0.10, ** 0.05, *** 0.01.

of column 1 implies that a change in government fractionalization in a donor country from zero to one increases aid by 0.206 percentage points of GDP in countries that received aid through the entire sample period. This implies that a change in government fractionalization by one standard deviation (i.e., 0.249) increases aid in percent of GDP by 14% of its mean (or 2.5% of a standard deviation in aid/GDP). As we show below, the average effects are substantially larger if aggregated over all 28 donors, however. When we add recipient country characteristics such as economic freedom and trade (in percent of GDP) in addition, both in levels and interacted with the probability of receiving aid, respectively, our instrumental variable remains highly significant.¹⁴ What is more, a skeptical reader might think that our instrument could be related to other donor–recipient features that are in turn correlated with aid. However, when we explain dyadic donor–recipient trade, FDI or the number of either refugee or asylum seekers from the recipient in the donor country, our instrument is weak in all regressions (see columns 2 to 5 in table 1).

Following Christian and Barrett (2017), we also plot the variation in government fractionalization in tandem with the variation in aid and growth for two different groups that are defined according to the mean of the probability

¹⁴ These additional results are available on request.

to receive aid. Appendix figure A1 plots these graphs. They give no reason to believe that the parallel trends assumption is violated in our case.¹⁵

4. Main results

Table 2 shows the OLS results. As can be seen, GDP per capita growth is not significantly correlated with contemporaneous aid (column 1). There is also no evidence of a non-linear relationship, as indicated by the insignificant squared term in column 2. In line with Clemens et al. (2012), the impact of aid on growth turns stronger when aid is lagged, as can be seen in columns 3 (without aid squared) and 4 (including aid squared). In column 3, the coefficient for lagged aid is more than twice the estimate in the comparable regressions in Clemens et al. (2012).¹⁶ The regression shows that an increase in lagged aid by one percentage point of GDP is accompanied by higher growth of a magnitude of 0.22 percentage points. The squared term in column 4 is significant at the 1% level, indicating that the correlation of aid with growth is decreasing in aid.¹⁷ These estimates are not necessarily causal, however, as omitted variables could easily explain the correlations.

Table 3 shows results for the instrumental variables regressions, excluding the control variables of table 2. As in any instrumental variables setting, we face a trade-off between increasing the efficiency of the estimator and introducing bias via the potential endogeneity of the control variables and their correlation with the variables of interest. Given that the excludability of our instrumental variable does not rely on the inclusion of control variables, we thus exclude them from the main analysis. Our results are, however, robust to their inclusion.¹⁸

15 More precisely, the probability-specific trends in aid and growth, respectively, seem rather parallel across the regular recipients (those with a probability to receive aid that is above the mean) and the irregular recipients (with the probability to receive aid being below the mean). There is also no obvious non-linear trend in regular compared to irregular recipients that is similar for aid and growth. What is more, these trends do not overlap with the trend in government fractionalization. In analogy to Christian and Barrett (2017), our identification strategy would be at risk in the presence of a non-linear trend in government fractionalization that is similar to the trends in aid and growth for the group of regular recipients. A common trend in all three variables that is not different for regular and irregular recipients would, to the contrary, be captured by our time fixed effects. On the importance of parallel trends for identification with Bartik-style instruments also see Goldsmith-Pinkham et al. (2020).

16 Specifically, their estimated coefficient is 0.096 (in column 4 of their table 7), which is, however, not significant at conventional levels.

17 In the comparable regression in Clemens et al. (2012) the coefficient for the linear aid term is 0.361 (i.e., identical to ours) and for aid squared -0.008 , both significant at the 5% level (in column 7 of their table 7).

18 See appendix table A3.

TABLE 2
Aid and growth, 1974–2013, OLS

	(1)	(2)	(3)	(4)
Aid/GDP	0.045 (0.057)	−0.053 (0.116)	0.220*** (0.058)	0.361*** (0.120)
Aid/GDP squared		0.003 (0.004)		−0.004*** (0.002)
Log initial GDP pc	−2.833*** (0.640)	−2.879*** (0.693)	−2.930*** (0.773)	−2.838*** (0.746)
Assassinations	−0.017 (0.173)	−0.014 (0.169)	−0.161 (0.157)	−0.161 (0.159)
Ethnic*Assassinations	−0.680 (0.765)	−0.680 (0.754)	−0.118 (0.543)	−0.132 (0.547)
M2/GDP (t−1)	−0.012 (0.012)	−0.012 (0.012)	−0.019* (0.011)	−0.019* (0.011)
Aid lagged	No	No	Yes	Yes
Number of obs.	801	801	710	710
Adj. R-Squared	0.152	0.153	0.203	0.207

NOTES: Data are averaged over four years at the recipient–period level. Recipient and period fixed effects are included. Standard errors are in parentheses (clustered at the recipient–country level). Significance levels: * 0.10, ** 0.05, *** 0.01.

Column 1 of table 3 focuses on contemporaneous aid, instrumented with $\sum_j FRAC_{j,t} * p_{i,j}$. According to the results, the coefficient is positive but not significant at conventional levels. As can be seen, the Kleibergen–Paap first-stage F-statistic is above the Staiger and Stock (1997) rule-of-thumb threshold of 10.¹⁹ Panel B shows the corresponding first-stage results. The coefficient implies that a one standard-deviation-increase in the instrumental variable increases aid from all DAC donors by 43% of a standard deviation in aid at the recipient level.

Column 2 includes aid squared and is estimated with a control function approach (CFA, using bootstrapped standard errors with 500 replications). This approach is more efficient compared to including the square of predicted aid to GDP of the first stage as a second instrument in the 2SLS setting

19 Stock and Yogo (2005) propose more specific sets of critical values for weak identification tests based on the number of endogenous regressors, the number of instruments and the acceptable maximum bias of the 2SLS relative to an OLS regression or the maximum Wald test size distortion. For example, a 20% 2SLS size distortion of a 5% Wald test is associated with a critical value of 6.66 and a lower value of 4.42 for a 20% limited information maximum likelihood size distortion. The under-identification test (Kleibergen–Paap LM statistic) clearly rejects the null hypothesis that the equation is under identified, and the coefficients are within the Anderson–Rubin 95% confidence interval. The Olea–Pflueger test for weak instruments for a 10% confidence level and a desired weak-instrument threshold τ of 30% rejects the null hypothesis of a weak instrument.

TABLE 3
Aid and growth, 1974–2013, 2SLS and CFA

	(1)	(2)	(3)	(4)
Panel A: Second stage				
Aid/GDP	0.222 (0.554)	0.194 (0.735)	0.232 (0.411)	0.325 (0.457)
Aid/GDP squared		0.002 (0.007)		−0.006 (0.006)
Aid lagged	No	No	Yes	Yes
Number of obs.	832	832	727	727
K–P F-stat.	10.965		18.136	
K–P LM-stat. p-val.	0.000		0.000	
Anderson–Rubin 95% CI	[−0.664,1.076]		[−0.478,0.843]	
Panel B: First stage				
IV	1.347*** (0.407)		1.980*** (0.465)	

NOTES: Data are averaged over four years at the recipient–period level. Recipient and period fixed effects are included. Columns 1 and 3 are estimated with 2SLS; columns 2 and 4 use a control function approach (CFA). Pairs cluster bootstrap standard errors with 500 replications (over both stages, in 2SLS and CFA) are in parentheses in the second-stage regressions (clustered at the recipient-country level). Standard errors are in parentheses in the first-stage regressions (clustered at the recipient-country level). Columns 2 and 4 include the residual of the first-stage regressions from columns 1 and 3, respectively. Significance levels: * 0.10, ** 0.05, *** 0.01.

(Wooldridge 2015).²⁰ That is, we control for the endogeneity of aid by including the residual from the first-stage regression (which is identical to the first stage of column 1, with just one endogenous variable and instrument). As can be seen, the coefficient of aid squared is not significant at conventional levels.

We next turn to the results for our preferred specification, replacing contemporaneous aid with lagged aid. Column 3 excludes aid squared. While the coefficient of aid and its significance hardly change compared to column 1, the strength of the instrument increases substantially.²¹ In column 4, we

20 Of course, the increase in efficiency comes at the cost of an additional assumption, that is, we need to assume that the bias is constant for different values of the variable we interact aid with (which is the amount of aid itself in our case). All coefficients remain insignificant, however, when we instrument aid squared with the square of predicted aid to GDP of the first stage, with a reasonably strong first-stage F-statistic, as can be seen in table A5. When we use (log) Aid instead of Aid/GDP to allow for a more flexible functional form, the first-stage F-statistics are low.

21 The first-stage F-statistic is above 18, the coefficient of the instrument in the first stage increases in magnitude and the Olea–Pflueger test for weak instruments rejects the null hypothesis with a τ of 20% and a confidence level of 5%. The coefficient in column 3 implies that a one standard deviation increase

again include the lag of squared aid (and again estimate the regression with a control function approach). The results show that neither aid nor its square are significant at conventional levels.

We offer a number of explanations for these insignificant results. First, aid or growth might not be measured precisely enough to capture the effects of aid in a rather small sample of around 800 observations. Second, even if aid would be measured precisely, the small number of observations implies that our tests are under powered. This is an unfortunate feature that we share with the aid effectiveness literature at large (Ioannidis et al. 2017).²² Third, the effects of aid might be spread over different horizons, and our four-year averages might be inadequate to capture these effects.²³ Fourth, aid might be effective in some groups of countries but not in others, and our pooled sample could hide such effects. We turn to this in section 6. Finally, of course, aid might simply not increase growth. Unfortunately, there is no bullet-proof way to distinguish a precisely estimated “zero effect” from an “imprecise estimate.” Though our instrument is strong, it causes only parts of overall aid in any particular period and country. As is well known, there is a trade-off between bias and efficiency, with the OLS estimator always being more precise than the instrumental variables estimator (e.g., Andrews et al. 2019). When we test whether the (insignificant) coefficient from the 2SLS estimates is different from the (significant) estimate we receive with OLS, the hypothesis of equal coefficients cannot be rejected.²⁴ We thus conclude that our 2SLS estimate is not significantly different from zero, but also not significantly different from the OLS estimate (according to which an increase in aid by one percentage point increases growth by 0.22 percentage points).

5. Where does the aid go?

This section investigates the effects of aid on components of GDP, with the aim of testing where aid is spent. We investigate the components of GDP,

in the instrumental variable increases aid from all DAC donors by 64% of a standard deviation in aid at the recipient level.

- 22 According to Ioannidis et al. (2017), only about 1% of the 1,779 estimates in the aid and growth literature surveyed have adequate power (see also Doucouliagos 2019).
- 23 A detailed analysis of longer lags is beyond the scope of this paper. When we include further lags of our aid variables, the second lag stays insignificant (8 years), but there is some evidence that growth might increase with even longer lags (from 12 years on). The number of observations in these regressions is, however, comparably low, and we did not investigate the robustness of these results.
- 24 This holds independent of whether we include control variables in these regressions.

focusing on the lagged linear specification to reduce clutter. We investigate the effect of aid on investment, overall consumption, private sector consumption, government consumption and net exports. We also investigate the effect of aid on domestic savings, testing whether aid inflows are substituted by equivalent decreases in savings. Specifically, we focus on gross capital formation (in percent of GDP), net exports (in percent of GDP), household final consumption expenditure (in percent of GDP) and government final consumption expenditure (in percent of GDP), with overall consumption being the sum of the two, and gross domestic savings (in percent of GDP).

Table 4 shows the results, holding the sample constant across the different dependent variables and using the same instrument for aid as in the main analysis.²⁵ As can be seen from column 1, aid has no significant effect on growth in the reduced sample, with a coefficient similar in size compared to those in table 3. To the contrary, aid increases investment at the 10% level of significance, and to a sizable extent. According to the estimated coefficient, a one percentage point increase in aid as a percent of GDP increases investment by 1.6 percentage points. Overall consumption increases by 2.39 percentage points, driven by private consumption, with coefficients being significant at the 5% to 10% level. While there is no significant effect of aid on savings and government consumption, aid decreases net exports significantly, by an amount almost as large as the combined effect on investment and consumption. The effect on consumption is in line with Boone (1996), Werker et al. (2009) and Temple and Van de Sijpe (2017);²⁶ the negative effect on net exports most likely reflects that much of the aid is spent on imports. The effect of aid on investment might imply that growth substantially increases in future years; however, when we lag aid by an additional period the coefficient of aid in the growth regression stays small and insignificant. It thus seems that the effect of aid on investment is either too low to be sizable in terms of short-term growth, that the investment caused by aid is unproductive or that aid affects other variables (such as corruption or the exchange rate) that offset the positive effect of aid on growth via investment. At the same time, the results of table 4 show that it is possible to obtain significant coefficients on aid with the instrumental variables strategy that we introduce here, with results that are broadly in line with previous work (Boone 1996, Werker et al. 2009, Temple and Van de Sijpe 2017).

25 The corresponding first-stage F-statistic (of 10.86) is thus the same for all regressions.

26 More specifically, Boone (1996) reports that aid increases consumption, but not savings and investment. Werker et al. (2009) find that household and government consumption both increase with aid, that savings decrease with aid and that investment is unaffected (all focusing on Arab donors and the recipients of their aid exclusively). Temple and Van de Sijpe (2017) confirm the positive impact of aid on total consumption, which seems also to be driven mainly by household consumption.

TABLE 4
Aid, other outcomes, 1978–2013, CFA

	(1) GDP pc growth	(2) Investment	(3) Savings	(4) Overall	(5) Consumption Gov.	(6) Private	(7) Net exp.
Aid/GDP (t–1)	0.213 (0.552)	1.633* (0.910)	0.633 (1.338)	2.392** (0.985)	0.365 (0.558)	2.021* (1.071)	–3.39*** (1.001)
Number of obs.	592	592	592	592	592	592	592

NOTES: The dependent variables—all in % of GDP—are indicated in the column headings. All regressions are estimated with a CFA. Recipient and period fixed effects are included. Pairs cluster bootstrap standard errors over both stages of the regression with 500 replications are used (clustered at the recipient-country level). Significance levels: * 0.10, ** 0.05, *** 0.01.

6. Heterogeneous effects of aid

Our instrumental variables regressions make use of variation in bilateral aid flows that go disproportionately to regular and irregular recipients of aid as a result of differences in donor government fractionalization. We have no reason to believe that the local average treatment effect (LATE) cannot be generalized to be representative of bilateral aid more broadly. However, the previous literature suggests that the effects of aid vary across a recipient country’s policies and institutions. What is more, the high standard errors for our estimate of the effect of aid in the overall sample in tandem with a positive average coefficient could hide significant effects in a subset of countries.

In table 5, we therefore interact aid with a number of variables to test heterogeneity, again estimated with a control function approach to control for the endogeneity of aid.²⁷ It has been suggested that aid is effective in countries with good economic policies (Burnside and Dollar 2000), in democracies (Svensson 1999) or after the end of the Cold War (Headey 2008), but not otherwise. All of these interactions have been shown to be fragile (e.g., Doucouliagos and Paldam 2009), but none of these earlier studies investigates causal relationships. Column 1 interacts aid with Burnside and Dollar’s (2000) good policy index (based on inflation, the budget balance and openness to trade). We further introduce an equivalent good-policy index, where we replace the original Sachs–Warner index of trade-openness with

27 Again, we implement this approach by controlling for the residual of the first-stage (aid) regression shown in equation 3 in the second stage. As pointed out in Wooldridge (2015), an alternative to this approach is 2SLS employing the interaction of the instrument with the variable we interact aid with as second instrument, but this approach treats the interaction with the endogenous variable as separate endogenous variable and thus “can be quite inefficient relative to the more parsimonious CF approach” (Wooldridge 2015, p. 429). Again, the increase in efficiency comes at the cost of the additional assumption that the bias does not depend on the variables we interact aid with.

TABLE 5

Aid, other outcomes and heterogeneous effects, 1978–2013, CFA

	(1) SW	(2) Fraser	(3) KOF	(4) Democracy	(5) After Cold War
GDP pc growth					
Aid/GDP (t–1)	–0.092 (1.363)	–0.086 (1.126)	–0.258 (1.560)	0.163 (0.994)	0.536 (0.694)
Aid/GDP*X (t–1)	0.091 (0.073)	0.142 (0.110)	0.115 (0.127)	0.055 (0.144)	–0.270*** (0.095)
Investment					
Aid/GDP (t–1)	0.466 (2.215)	0.824 (2.605)	0.847 (2.626)	1.542 (1.397)	1.848 (1.240)
Aid/GDP*X (t–1)	0.110 (0.108)	0.153 (0.118)	0.139 (0.132)	0.129 (0.199)	–0.180 (0.159)
Savings					
Aid/GDP (t–1)	–1.832 (3.864)	–1.511 (3.708)	–1.417 (2.996)	0.730 (1.967)	0.664 (1.626)
Aid/GDP*X (t–1)	0.181 (0.140)	0.160 (0.145)	0.114 (0.142)	–0.209 (0.247)	–0.026 (0.335)
Overall consumption					
Aid/GDP (t–1)	3.973 (22.982)	3.372 (5.454)	3.314 (4.751)	3.047 (2.405)	2.718 (1.766)
Aid/GDP*X (t–1)	–0.029 (0.127)	0.198 (0.195)	0.222 (0.177)	–0.607* (0.359)	–0.273 (0.340)
Government consumption					
Aid/GDP (t–1)	0.244 (1.578)	0.166 (1.879)	0.339 (1.484)	0.237 (0.786)	0.545 (0.656)
Aid/GDP*X (t–1)	0.117 (0.280)	0.171 (0.230)	0.140 (0.216)	0.064 (0.135)	–0.151 (0.241)
Private consumption					
Aid/GDP (t–1)	3.814 (5.486)	3.192 (6.076)	2.961 (4.497)	2.801 (2.421)	2.165 (1.759)
Aid/GDP*X (t–1)	–0.116 (0.319)	0.027 (0.282)	0.082 (0.283)	–0.669** (0.341)	–0.121 (0.380)
Net exports					
Aid/GDP (t–1)	–3.548 (4.947)	–3.180 (4.661)	–3.160 (3.374)	–3.782 (2.321)	–3.863** (1.889)
Aid/GDP*X (t–1)	–0.127 (0.160)	–0.350* (0.187)	–0.343* (0.177)	0.346 (0.427)	0.395 (0.323)
Number of obs.	446	463	439	592	592

NOTE: The number of observations is constant across panels given that the regressions are run for the same sample. The dependent variables—all in % of GDP—are indicated in the panel headings. All estimations include interactions of Aid/GDP with X as indicated in the column headings and are estimated with a CFA. Recipient and period fixed effects are included. Policy is defined according to the Burnside–Dollar good policy index, including: (i) the Sachs–Warner index (SW) in column 1, (ii) freedom to trade from Fraser in column 2 and (iii) the de facto KOF index of economic globalization in column 3 as measure of trade openness. Democracy is measured with the binary indicator of Cheibub et al. (2010), updated by Bjørnskov and Rode (2019). Cold War takes a value of 1 if the period is after the Cold War. Pairs cluster bootstrap standard errors over both stages of the regression with 500 replications are used (clustered at the recipient-country level). Significance levels: * 0.10, ** 0.05, *** 0.01.

Fraser's Freedom to Trade (Gwartney et al. 2018) in column 2 or, respectively, the KOF index of de facto economic globalization (Dreher 2006, Gygli et al. 2019) in column 3. Column 4 uses Cheibub et al.'s (2010) binary indicator of democracy,²⁸ and column 5 the years after 1990.

The results show no positive effects of aid on growth across the range of the interacted variables.²⁹ According to column 5, aid is significantly less effective in the post-Cold war period, but the effect of aid during the Cold War period remains insignificant.³⁰

The further results in table 5 show that the effect of aid on overall and private consumption is lower in democracies, while the effect of aid on net exports is driven by countries with "better" policies (when openness to trade is measured by the Fraser and KOF indices). On balance, these results give us no reason to believe that heterogeneity of the countries in our sample drives the insignificant average coefficient of aid on growth.

7. Conclusion

This paper has proposed an excludable instrument to identify whether and to what extent foreign aid affects economic growth. Cross-sectional variation arises due to changes in aid disbursements following differences in donor countries' government fractionalization. Temporal variation is introduced by interacting fractionalization with the probability of a country to receive aid. Our regressions resemble a difference-in-differences approach, with a continuous treatment variable (fractionalization) instead of a binary indicator.

Using aid disbursement data for all bilateral donors of the OECD's DAC to a maximum of 97 recipient countries over the 1974–2013 period, we find our instrument to be powerful. Applying the instrument to our growth models, we find a positive but insignificant effect of bilateral aid on economic growth, independent of the recipients' quality of economic policies, democracy and the Cold War period. We also investigate the effect of aid on savings as well as on the main components of GDP and find that aid significantly increases (private) consumption and investment, but reduces net exports.

Our results show that bilateral aid has no robust effect on short-term growth. We would like to stress that this finding does not imply that aid

28 We use the update of Bjørnskov and Rode (2019) for the most recent period.

29 The corresponding first-stage F-statistics are 7.07 in column 1, 6.64 in column 2, 4.46 in column 3 and 10.86 in columns 4 and 5.

30 Table A9 shows additional interactions. The results show no differential effects of aid on growth in countries that are larger in terms of population or GDP or that are located in sub-Saharan Africa or East Asia. While we find that aid is significantly less effective in countries that receive aid levels above the sample median, the coefficient of aid for recipients receiving below-median aid stays insignificant.

is necessarily ineffective. One might argue that aid is measured imprecisely and standard errors are too large. Statistical power might be too low for the estimators to find a significant effect, even if it would be there (Ioannidis et al. 2017). We agree that these are two possible explanations for our insignificant results. We still believe that it is important to show, and publish, these results as the published literature on the effectiveness of aid tends to be over-optimistic due to institutional biases of the authors in the aid effectiveness literature and the well-known bias of journals to publish (only) significant results (Doucouliagos and Paldam 2009, Doucouliagos 2019). As the lack of power pertains independent of the significance of the results, there is arguably no reason to dismiss ours on the grounds of large standard errors, compared to a number of recent papers finding significant (and positive) results. We therefore urge readers to evaluate this paper on its methodological improvements over the previous literature, rather than its results.

At least one other important reason can explain our results: Donors pursue a multitude of objectives when granting aid, with economic growth being just one of them. To the extent that donors prioritize geo-strategic goals over developmental ones, the effects of “true” developmental aid will be higher than those of all aid (Dreher et al. 2018a). Aid would then need to be evaluated based on progress towards its “true” goals. The effects of aid on a number of alternative outcomes have been documented, including terror (Azam and Thelen 2008), voting behaviour in international organizations (Vreeland and Dreher 2014) and conflict (Nunn and Qian 2014). While we did not investigate such outcomes here, our results document significant effects on investment and private consumption. To the extent that aid increases consumption it might contribute to reducing poverty and make recipient country populations better off. Given the potential indirect effects of aid on growth via a plethora of different channels and over different periods of time, robustly identifying the effects of aid on growth might be all but impossible in a country-level analysis. Focusing on alternative outcomes that are more directly affected by aid would then be more fruitful.

We would thus like to conclude this paper by pointing to a number of important questions that could be addressed with our instrumental variables strategy, for a large number of donors and years. The effect of aid on formal and informal institutions, economic freedom, conflict, terrorism, migration, population health and education and the size of the shadow economy, among others, has been investigated in a large number of papers. All of these questions face the problem of endogeneity between aid and the variable of interest. Our instrument is well suited to address this problem, as has been demonstrated in Bluhm et al. (2020) for conflict, Ziaja (2017) for democracy, Doucouliagos et al. (2019) for infant mortality and Dreher et al. (2019) for refugees. In providing an instrumental variable that is suitable to address the endogeneity of aid in a broad setting of questions, we hope to contribute in providing a more nuanced understanding of the various causal effects the aid might have.

Appendix A1: Sample

Included donor countries, in alphabetical order

Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Korea, Luxembourg, Netherlands, New Zealand, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, United Kingdom, United States.

Included recipient countries, in alphabetical order

Afghanistan, Albania, Algeria, Angola, Argentina, Barbados, Benin, Bolivia, Botswana, Brazil, Burkina Faso, Burundi, Cambodia, Cameroon, Central African Rep., Chad, Chile, China, Colombia, Dem. Rep. Congo, Rep. Congo, Costa Rica, Côte d'Ivoire, Cyprus, Dominican Republic, Ecuador, Egypt, El Salvador, Ethiopia, Gabon, Gambia, Ghana, Guatemala, Guinea, Guyana, Haiti, Honduras, India, Indonesia, Iran, Iraq, Israel, Jamaica, Jordan, Kenya, Korea, Kuwait, Laos, Lebanon, Lesotho, Liberia, Libya, Madagascar, Malawi, Malaysia, Maldives, Mali, Malta, Mauritania, Mauritius, Mexico, Mongolia, Morocco, Mozambique, Myanmar, Nepal, Nicaragua, Niger, Nigeria, Pakistan, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Rwanda, Saudi Arabia, Senegal, Serbia, Sierra Leone, Singapore, South Africa, Sri Lanka, Sudan, Syria, Tanzania, Thailand, Togo, Trinidad and Tobago, Tunisia, Turkey, Uganda, Uruguay, Venezuela, Yemen, Zambia, Zimbabwe.

Appendix A2: Definitions and sources

Variable	Description	Data source
Above median aid	Dummy 1 if recipient receives more aid than the median recipient (per period)	Own construction
Aid agency	Dummy 1 if there are national aid agencies operating independently from the ministry of foreign affairs (donor)	Fuchs et al. (2014), updated 2018
Aid/GDP	ODA total net, current prices (USD) in % of recipient GDP, aggregated over all 28 bilateral DAC donors	OECD, World Bank (2014, 2018)
Aid budget/GDP	Donor ODA total net, current prices (USD)—to all recipients in % of donor GDP in current prices	OECD, World Bank (2014, 2018)
After Cold War	Dummy 1 for years equal or higher than 1990–1993 period	Own construction
Central gov. expenditure/GDP	Central government expenses (% of donor GDP)	IMF/GFS (2014, 2018)

(continued)

(Continued)

Variable	Description	Data source
Closed lists	When proportional representation is 1, closed list gets a 1 if voters cannot express preferences for candidates within a party list, 0 if not (donor)	Database of Political Institutions (Beck et al. 2001)
Democracy	Dummy 1 if recipient country is a democracy	Cheibub et al. (2010), Bjørnskov and Rode (2019)
Donor GDP pc growth	GDP per capita growth (annual %) (donor)	World Bank (2014, 2018)
Economic freedom	Economic freedom, chain linked index	Gwartney et al. (2018)
Donor GDP growth FDI/GDP	GDP growth (annual %) (donor)	World Bank (2014, 2018)
FDI/GDP with DAC	Foreign direct investment inflows (% GDP) (recipient)	UNCDATStat (2017)
	Foreign direct investment inflow and outflow with all DAC donor countries (% GDP) (recipient); in bilateral regressions, we use the bilateral flows	OECD (2018)
Fractionalization (FRAC)	The probability that two deputies picked at random from among the government parties will be from different parties	Database of Political Institutions (Beck et al. 2001)
Government cons./GDP	General government final consumption expenditure (% of GDP)	World Bank (2014, 2018)
Investment/GDP	Investment—gross capital formation (% of GDP)	World Bank (2014, 2018)
Log asylum seekers	Log of net bilateral flows of asylum seekers (difference between stock in t and $t-1$) from a recipient to a donor country	OECD (2016)
Log colony	Log of the population of former colonies on DAC list of ODA recipients (1997–2013), 0 if no colonial history (donor)	Own calculations (2018) based on Fuchs et al. (2014)
Log GDP pc	Log of GDP per capita, both measures in constant 2005 USD (donor/recipient)	World Bank (2014, 2018)
Log population	Log of population total (donor/recipient)	World Bank (2014, 2018)
Log refugees	Log of net refugee flows (difference between refugee stock in t and $t-1$) from a recipient to a donor country, bilateral flows	UN/UNHCR (2015)
Net exports/GDP	Difference between exports and imports (% of GDP)	Own construction based on World Bank (2018)
Overall consumption/GDP	Overall consumption: sum of private and government consumption (% of GDP)	Own construction based on World Bank (2014, 2018)

(continued)

<i>(Continued)</i>		
Variable	Description	Data source
Political globalization	KOF political globalization index composed of embassies in country (25%), membership in international organization (27%), participation in UN Security Council missions (22%), international treaties (26%)	Dreher (2006), Gygli et al. (2019)
Population (share>64)	Population ages 65 and above (% of total) (donor)	World Bank (2014, 2018)
Presidential	Dummy 1 for a presidential country (donor)	DPI (Beck et al. 2001)
Private consumption/GDP	Household final consumption expenditure (% of GDP)	World Bank (2014, 2018)
Probability of receiving aid	The probability of receiving aid from a particular donor <i>j</i> within the whole observation period from 1974–2013	Own construction based on ODA total net data from OECD (2014, 2018), table DAC2a
Savings/GDP	Gross domestic savings (% of GDP)	World Bank (2014, 2018)
Total seats	Total seats in the legislature or in the case of bicameral legislatures, the total seats in the lower house (donor)	Database of Political Institutions (Beck et al. 2001)
Trade/GDP	Trade with the world (% of GDP) (recipient)	World Bank (2018)
Trade/GDP with DAC	Trade with all DAC donors countries (% of GDP) (recipient); in bilateral regressions, we use the bilateral flows	Calculation based on IMF (DOTS) (2018), World Bank (2018)
Trade openness	Trade (% of GDP) (donor)	World Bank (2014, 2018)
Unemployment	Unemployment, total (% of total labour force) (national estimate) (donor)	World Bank (2014, 2018)
Burnside and Dollar (2000) specification (4-year periods)		
Assassinations	Average number of assassinations in a given period	Banks and Wilson (2007, 2012, 2014) ^a
East Asia	Dummy 1 if recipient is an East Asian country	World Bank (2014)
Ethnic* Assassinations	Interaction between assassinations and ethnolinguistic fractionalization	Banks and Wilson (2007, 2012, 2014), Easterly and Levine (1997), Roeder (2001) ^a
Budget balance	Overall budget balance, including grants; measured as cash surplus/deficit (% of GDP)	World Bank (2005, 2007, 2018), IMF (IFS) 2005 ^a
Ethnolinguistic fractionalization GDP pc growth	Ethnolinguistic fractionalization in a country in a given period GDP per capita growth (%) based on constant local currency	Easterly and Levine (1997), Roeder (2001) ^a World Bank (2007 ^a , 2018)
Inflation	Natural log of (1 + consumer price inflation)	World Bank (2005, 2007, 2018), IMF (2005) ^a

(continued)

(Continued)

Variable	Description	Data source
Log initial GDP pc	Logarithm of GDP per capita in the first year of each period in international prices	Calculation based on World Bank (2014, 2018)
M2/GDP	Money and quasi-money (% of GDP)	World Bank (2007 ^a , 2018)
Openness	Wacziarg and Welch (2008) extension of the initial Sachs and Warner (1995) openness index	Wacziarg and Welch (2008), Clemens et al. (2012) ^a
Policy	Good policy index based on budget balance/GDP, inflation and trade openness (cf. Burnside and Dollar, 2000). We construct three different policy indices given that the original openness measure is not available in the last period. The first measure is based on Clemens et al. (2012) and Burnside and Dollar (2000), using the Sachs–Warner (SW) index to measure trade openness. In the second index, we measure trade openness with de facto trade globalization from KOF, and in the third index, with freedom to trade internationally from the Fraser Institute.	Clemens et al. (2012), Dreher (2006), Gygli et al. (2019), Gwartney et al. (2018)
SSA	Dummy 1 if recipient is in sub-Sahara Africa	World Bank (2014)

NOTES: ^aOur source is Clemens et al. (2012). More details can be found in the technical appendix to “Counting Chickens When They Hatch: Timing and the Effects of Aid on Growth.” Data for the 2006–2009 period are from Minasyan (2016) and for the last period from our own updates. The variables listed below the Burnside and Dollar (2000) specification are recipient-specific characteristics.

TABLE A1

Descriptive statistics: Tables 1, 2, 3, 5, A3, A4, A5, A9

	Obs.	Mean	SD	Min.	Max.
Panel A: Table 1					
Aid/GDP	35,803	0.17	1.06	−0.92	42.28
Bilateral trade/GDP	35,435	1.56	9.17	0.00	729.84
Bilateral FDI/GDP	11,498	0.27	19.50	−1342.42	1437.14
Log refugees	33,472	2.00	2.20	0.00	10.07
Log asylum seekers	33,472	1.63	1.84	0.00	9.01

(continued)

TABLE A1
(Continued)

	Obs.	Mean	SD	Min.	Max.
Panel B: Tables 2, 3, 5, A3, A4, A5, A9					
GDP pc growth	832	1.72	3.73	−32.42	17.05
Aid/GDP	832	3.50	4.82	−0.15	47.91
Probability of receiving aid	832	0.55	0.14	0.07	0.75
Log initial GDP pc	822	7.59	1.24	4.83	10.81
M2/GDP	817	39.54	29.75	0.36	239.83
Assassinations	829	0.29	0.98	0.00	11.50
Ethnolinguistic fractionalization	832	0.46	0.29	0.00	0.93
Democracy (Cheibub)	831	0.42	0.48	0.00	1.00
After Cold War	832	0.63	0.48	0.00	1.00
Policy (BD)	541	1.67	0.99	−1.76	4.20
Policy (Fraser)	592	1.72	0.75	−1.55	5.14
Policy (KOF)	567	1.71	0.65	−1.27	5.40
Log population	832	16.36	0.40	15.58	16.85
Trade/GDP	791	71.42	47.41	12.28	407.92
Trade/GDP with DAC	798	42.23	116.50	2.68	2317.06
FDI/GDP	819	3.17	11.37	−5.96	251.07
FDI/GDP with DAC	667	1.87	12.86	−9.91	215.85
Above median aid	832	0.47	0.50	0.00	1.00
SSA	832	0.41	0.49	0.00	1.00
East Asia	832	0.08	0.27	0.00	1.00

NOTES: Sample in panel A based on column 1 in table 1. Sample in panel B based on column 1 in table 3.

Appendix A3: Descriptive statistics

TABLE A2
Descriptive statistics: Tables 4, A6, A7

	Obs.	Mean	SD	Min.	Max.
Panel A: Variables table 4					
Government consumption/GDP	744	14.38	6.16	3.96	80.74
Private consumption/GDP	739	68.64	16.14	14.60	182.19
Overall consumption/GDP	739	82.97	15.37	31.67	196.45
Investments/GDP	745	22.20	8.23	0.00	72.49
Savings/GDP	675	20.10	11.22	−16.52	67.78
Net exports/GDP	786	−5.36	13.87	−111.38	49.33

(continued)

TABLE A2
(Continued)

	Obs.	Mean	SD	Min.	Max.
Panel B: Variables table A6					
Central government expenditure/GDP	230	34.72	9.93	3.55	63.26
Fractionalization	230	0.34	0.27	-0.67	0.82
Log population	230	15.90	1.37	12.30	18.23
Trade openness	230	80.95	45.42	27.74	332.68
Population (Share>64)	230	13.52	3.00	3.82	21.01
Log GDP pc	230	10.19	0.70	7.95	11.58
Closed lists	230	0.58	0.49	0.00	1.00
Total seats	230	219.73	152.68	0.00	669.75
Presidential	230	0.07	0.25	0.00	1.00
Unemployment	230	6.09	4.35	-16.30	23.03
Panel C: Variables table A7					
Aid budget/GDP	210	0.40	0.25	-0.13	1.01
Central government expenditure/GDP	210	34.34	8.86	14.60	55.16
Log GDP pc	210	10.46	0.42	9.30	11.58
Aid agency	210	0.42	0.48	0.00	1.00
Log colony	210	10.70	8.64	0.00	21.57
Political globalization	210	86.17	10.15	59.75	99.42

NOTES: Government fractionalization is replaced with legislature fractionalization for the United States and Canada. The sample in panel A is based on column 1 in table 4, the sample in panel B on column 1 of table A6 and the sample in panel C in column 1 of table A7.

Appendix A4: Additional regressions

TABLE A3

Aid and growth, 1978–2013, 2SLS, additional covariates

	(1) Baseline	(2) SW	(3) Log pop.	(4) Trade World	(5) DAC	(6) World	(7) FDI DAC
Panel A: Second stage							
Aid/GDP (t-1)	0.061 (0.323)	-0.058 (2.562)	0.061 (0.323)	-0.023 (0.403)	0.029 (0.381)	0.067 (0.341)	-0.221 (0.489)
Log initial GDP pc	-3.551** (1.717)	-3.466 (9.965)	-3.551** (1.717)	-3.812** (1.713)	-3.695 (2.373)	-3.254* (1.793)	-5.470** (2.637)
Assassinations	-0.135 (0.284)	-0.205 (1.332)	-0.135 (0.284)	-0.173 (0.275)	-0.110 (0.272)	-0.114 (0.273)	0.172 (0.292)
Ethnic*Assass.	-0.157 (1.026)	0.206 (1.754)	-0.157 (1.026)	-0.046 (0.907)	-0.192 (0.979)	-0.182 (0.951)	-0.927 (0.831)
M2/GDP (t-1)	-0.014 (0.019)	-0.007 (0.056)	-0.014 (0.019)	-0.016 (0.018)	-0.014 (0.023)	-0.020 (0.022)	0.026 (0.033)
BD policy		0.977 (2.352)					
Log population			0.000 (0.000)				
Trade/GDP				0.014 (0.016)	-0.002 (0.018)		
FDI/GDP						0.055 (0.097)	-0.110 (0.068)

(continued)

TABLE A3
(Continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	SW	Log pop.	Trade World	DAC	FDI World	DAC
Panel A: Second stage							
Number of obs.	710	484	710	674	708	705	643
K-P F-stat.	14.705	5.566	14.705	10.031	12.975	18.333	9.101
Panel B: First stage							
IV	1.948*** (0.508)	1.386** (0.588)	1.948*** (0.508)	1.824*** (0.576)	1.848*** (0.513)	2.035*** (0.476)	1.596*** (0.529)

NOTES: Data are averaged over four years at the recipient-period level. Recipient and period fixed effects are included. Pairs cluster bootstrap standard errors with 500 replications are in parentheses in the second-stage regressions (clustered at the recipient country level). Standard errors are in parentheses in the first-stage regressions (clustered at the recipient country level). Significance levels: * 0.10, ** 0.05, *** 0.01.

TABLE A4
Aid and growth, 1974–2013, 2SLS and CFA, alternative IV (pre-period)

	(1)	(2)	(3)	(4)
Panel A: Second stage				
Aid/GDP	−0.557 (0.426)	−0.592 (0.534)	−0.077 (0.390)	0.068 (0.403)
Aid/GDP squared		0.002 (0.007)		−0.006 (0.006)
Aid lagged	No	No	Yes	Yes
Number of obs.	832	832	727	727
K-P F-stat.	13.882		12.042	
K-P LM-stat. p-val.	0.000		0.000	
Panel B: First stage				
IV	5.165*** (1.387)		4.956*** (1.429)	

NOTES: Data are averaged over four years at the recipient-period level. The instrument in columns 1 and 3 is the interaction of donor-government fractionalization and the number of years a recipient has received aid in the 1970–1973 period (i.e., prior to our regression sample). Recipient and period fixed effects are included. Columns 1 and 3 are estimated with 2SLS; columns 2 and 4 use a control function approach (CFA). Pairs cluster bootstrap standard errors with 500 replications (over both stages, in 2SLS and CFA) are in parentheses in the second-stage regressions (clustered at the recipient country level). Standard errors are in parentheses in the first-stage regressions (clustered at the recipient country level). Columns 2 and 4 include the residual of the first-stage regressions from columns 1 and 3, respectively. Significance levels: * 0.10, ** 0.05, *** 0.01.

TABLE A5
Aid and growth, 1974–2013, 2SLS

	(1)	(2)	(3)	(4)
Panel A: Second stage				
Aid/GDP	0.222 (0.554)	0.469 (1.483)	0.232 (0.411)	0.656 (1.222)
Aid/GDP squared		−0.010 (0.013)		0.007 (0.013)
Aid lagged	No	No	Yes	Yes
Number of obs.	832	832	727	727
K–P F-stat.	10.965	10.038	18.136	9.099
K–P LM-stat. p-val.	0.000	0.002	0.000	0.003
Panel B: First stage				
IV	1.347*** (0.407)	1.347*** (0.407)	1.945*** (0.463)	1.945*** (0.463)
Squared predicted Aid/GDP		3.090*** (0.847)		2.051*** (0.622)

NOTES: Data are averaged over four years at the recipient–period level. Recipient and period fixed effects are included. We instrument aid squared with the square of predicted aid to GDP from the first stage. All regressions are estimated with 2SLS. The first-stage statistics reported in columns 2 and 4 refer to the squared aid term. The statistics for the linear term in columns 2 and 4 are identical to columns 1 and 3, respectively. Pairs cluster bootstrap standard errors with 500 replications (over both stages) are in parentheses in the second-stage regressions (clustered at the recipient country level). Standard errors are in parentheses in the first-stage regressions (clustered at the recipient country level). Significance levels: * 0.10, ** 0.05, *** 0.01.

TABLE A6
Fractionalization and central government expenditures, 1974–2013, OL

	(1) Scartascini & Crain (2002)	(2) Roubini & Sachs (1989)
Fractionalization	12.918** (5.376)	2.159** (0.879)
Log population	38.532* (19.483)	
Trade openness	−0.011 (0.048)	
Population (share>64)	2.788*** (0.737)	
Log GDP pc	2.559 (6.036)	
Closed lists	−8.090 (4.724)	
Total seats	−0.009 (0.012)	
Presidential	−9.053** (3.920)	

(continued)

TABLE A6
(Continued)

	(1) Scartascini & Crain (2002)	(2) Roubini & Sachs (1989)
Central gov. exp./GDP (t−1)		0.803*** (0.033)
GDP growth (annual %)		−0.118* (0.066)
Unemployment		−0.005 (0.080)
Number of obs.	230	270
Adj. R-squared	0.399	0.879

NOTES: Data are averaged over four years at the donor-period level. Donor and period fixed effects are included. Missing values for control variables are linearly interpolated. Model (1) is based on Scartascini and Crain (2002), model (2) on Roubini and Sachs (1989). Standard errors are in parentheses (clustered at the donor-country level). Significance levels: * 0.10, ** 0.05, *** 0.01.

TABLE A7
Central government expenditures and aid budgets,
1974–2013, OLS

	(1)	(2)
Central gov. exp./GDP	0.0073*** (0.0020)	0.0067** (0.0024)
Log GDP pc	0.4409** (0.1766)	0.4598** (0.1693)
Aid agency	0.0735 (0.0456)	0.0716 (0.0424)
Log colony	−0.3027*** (0.1023)	−0.2932*** (0.0872)
Political globalization	−0.0042 (0.0038)	−0.0065 (0.0045)
Lagged	No	Yes
Number of obs.	210	210
Adj. R-squared	0.448	0.456

NOTES: Data are averaged over four years at the donor-period level. Donor and period fixed effects are included. Models are based on Fuchs et al. (2014), with contemporaneous values of government expenditures and all other covariates in column 1 and its lagged values in column 2. Missing values for control variables are linearly interpolated. Standard errors are in parentheses (clustered at the donor-country level). Significance levels: * 0.10, ** 0.05, *** 0.01.

TABLE A8
Fractionalization and aid budgets, 1974–2013, OLS

	(1) Our data	(2)	(3) Fuchs et al. (2014) data	(4)
Fractionalization (t–1)	0.0677* (0.0394)	0.0004* (0.0002)	0.0003* (0.0002)	0.0005** (0.0002)
Log GDP pc		0.3858*** (0.1261)	0.3715*** (0.1218)	0.6866*** (0.0577)
Lagged	No	Yes	Yes	Yes
Number of observations	280	587	591	760

NOTES: Donor and period fixed effects are included. In column (1), data are averaged over four years at the donor-period level, with standard errors clustered at the donor-country level. Missing values for control variables are linearly interpolated. Columns 2 to 4 use the samples and method of Fuchs et al. (2014)—table 2, columns 1 to 3, which include different sets of control variables, all with Driscoll and Kraay standard errors and yearly data. Significance levels: * 0.10, ** 0.05, *** 0.01.

TABLE A9
Aid and growth, 1978–2013, CFA, additional interactions

	(1) Above median aid	(2) Log population	(3) Log GDP	(4) SSA	(5) East Asia
Aid/GDP (t–1)	0.698 (0.555)	1.143 (1.123)	0.504 (1.502)	0.316 (0.451)	0.227 (0.454)
Aid/GDP*X (t–1)	–0.566* (0.329)	–0.055 (0.060)	–0.014 (0.065)	–0.126 (0.133)	2.797 (1.904)
Number of obs.	727	727	715	727	727

NOTES: Data are averaged over four years at the recipient–period level. Recipient and period fixed effects are included. All regressions include interactions of aid/GDP (t–1) with X (t–1) as indicated in the column headings. Above median aid is defined as a binary indicator that is one if the recipient received more aid than the median over all recipients (per period). Pairs cluster bootstrap standard errors over both stages of the CFA with 500 replications are used (clustered at the recipient-country level). Significance levels: * 0.10, ** 0.05, *** 0.01.

Appendix A5: Parallel trends

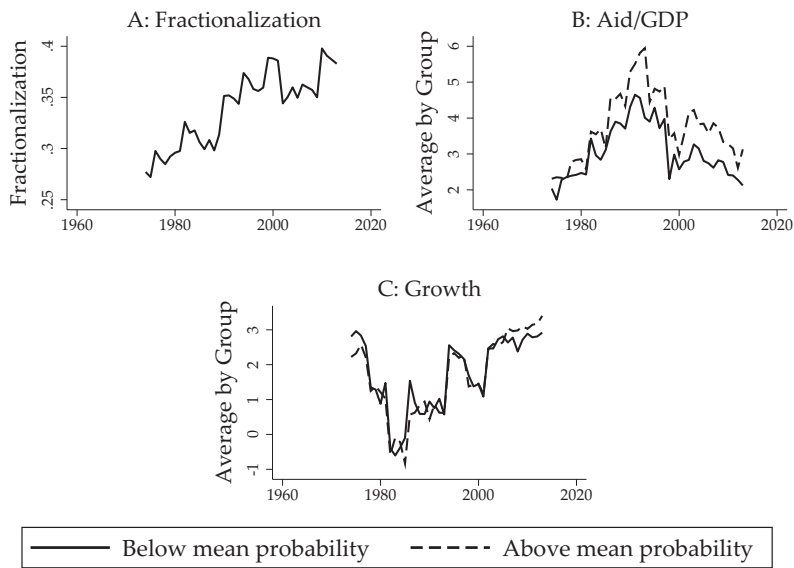


FIGURE A1
NOTES: Panel A shows how government fractionalization (replaced by legislature fractionalization for the United States and Canada) varies over time. Panel B is the average aid to GDP ratio within the group that is below the mean of the probability to receive aid (solid line) and the group that is above the mean (dashed line) over time. Panel C is the average real GDP per capita growth rate within these two groups over time. For the construction of the averages, we use observations from the sample of column 1 in table 2.

Supporting information

Supporting information is available in the online version of this article.

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