Aid Impact and Effectiveness

Editors

Rachel M. Gisselquist and Finn Tarp

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Editorial

Aid Impact and Effectiveness: Introduction and Overview

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Abstract

This editorial provides an introduction to and overview of the thematic issue on "Aid Impact and Effectiveness". The guest editors put the specific contributions of the nine articles in perspective referring to the wider literature on foreign aid and its allocation, impact, and efficiency, as well as the political and economic processes in which aid operates. They discuss the historical and present-day context for foreign aid and provide summaries of the individual articles, highlighting policy implications and future research needs.

Issue

This editorial is part of the issue "Aid Impact and Effectiveness", edited by Rachel M. Gisselquist and Finn Tarp (UNU-WIDER, Finland).

1. Introduction and Overview

The questions of whether aid has impact and is effective have been the subject of a considerable literature, including attention to the aggregate impact of aid on growth across countries (Arndt, Jones, & Tarp, 2010, 2015, 2016; Burnside & Dollar, 2000; Easterly, 2003; Hansen & Tarp, 2001; Jones & Tarp, 2016; Juselius, Møller, & Tarp, 2014; Rajan & Subramanian, 2008). In this thematic issue, we build upon this literature, paying special attention to the ways in which aid affects development outcomes, including—but not limited to—growth. We pay special attention to work that speaks to aid impact and effectiveness in fragile states and situations, where development outcomes have been the poorest and domestic capacity weakest.

While decreasing in relative terms, foreign aid remains an important financial flow for many developing countries. The topic of aid's impact on growth, therefore, remains central to the development literature. Overall, a consensus has emerged over the last decade that aid does seem to promote aggregate growth. At the same time, estimates of the impact vary and many studies focus on different factors that diminish aid's potential impact. In their study, Mekasha and Tarp (2019) turn to meta-analysis to provide an overall assessment, building on previous work (Mekasha & Tarp, 2013). They are careful to point out the potential weaknesses of this methodology as applied to the impact of foreign aid. That said, their new and updated results confirm the increasing consensus about the positive evidence of aid's impact on growth. They also show that this result is robust to including more recent studies and for different time horizons.

In turn, Martínez-Zarzoso (2019) considers the impact of aid on recipient income through international trade. She applies a structural gravity model of trade where aid is included, and estimates a set of trade variables for a cross-section of 33 donor countries and 125 recipient countries over the period 1995 to 2016. She furthermore uses a control function approach and instrumental variable techniques to estimate the indirect effect of aid on income. Her results are in line with emerging consensus about aid's impact on recipient income, highlighting that effects are heterogeneous and vary by region.

Aid is provided for many reasons and to achieve many objectives. Promoting growth and income is only one dimension. Attention to the provision of basic needs has been a recurring theme in development debates for decades. Many have argued that satisfying basic needs is a precondition for broad-based growth. See for example the famous quote by the architect of the Human Development Reports, Professor Mahbub ul Haq: "We were taught to take care of our GNP as this will take care of poverty. Let us reverse this and take care of poverty first as the GNP can take care of itself" (Haq, 2018). For sure, from 1995 onwards, attention to the provision of health and education was central to the Millenium Development Goal agenda. As Banchani and Swiss (2019) note, the G8 placed in 2010 renewed focus on maternal health via the Muskoka Initiative, with increased commitment to support aid interventions in this area, and the Sustainable Development Goals similarly prioritize maternal health. These authors take as their starting point that there is little analytical evidence on the impact of foreign aid on maternal mortality in developing countries. They analyse aid's impact on maternal health in a sample of 130 low- and middle-income countries from 1996 to 2015. Results show limited effects of total aid, but significant reductions in maternal mortality related to aid allocated to the reproductive health sector. The policy implication is that targeting aid to specific sectors has significant potential.

Climate change has over the last two decades pushed itself into a central position in international development and discussions about the future of the globe. In parallel, climate related aid is on the rise (see Arndt & Tarp, 2014). It is therefore natural to ask, as Kono and Montinola (2019) do, what the relationship is between climate aid and recipient climate policy. They find no evidence that the former is systematically related to the latter. They also qualify their conclusion with reference to the poor quality of both climate aid and climate policy data. It is well established in the literature that great care has to be exercised in avoiding to overextend the use of insignificant statistical parameters in aid debates. To be sure, an insignificant parameter reflects our lack of evidence. Temple (2010) spells this out in the following way: "An insignificant coefficient should usually be seen as absence of evidence, not evidence of absence, at least until the economic implications of a confidence interval have been explored." (p. 4448). Kono and Montinola (2019) conclude by specifying what is required in the climate change area to arrive at firm conclusions.

Aid is regularly justified with reference to the needs of recipient countries. The group of fragile and conflict-affected states is therefore a clear priority. Carment and Samy (2019) ask whether aid to this group is targeted to where it is most needed. Using the Country Indicators for Foreign Policy (CIFP) fragility index, together with data on aid flows from the Organisation for Economic Co-Operation and Development's Creditor Reporting System and six country cases, they extend recent and forthcoming work (Carment & Samy, 2017, forthcoming). Considering the types of aid received against the CIFP framework, the authors conclude that aid is poorly targeted in fragile states. They argue that aid's impact would be improved through better targeting to address core challenges of legitimacy and authority that are important to understanding why states are fragile.

Domestic ownership of foreign aid programmes is a longstanding topic in relation to foreign aid (see e.g., Tarp & Roland-Holst, 2004), and local ownership is set out as a fundamental principle for aid effectiveness in the Paris Declaration, Accra Agenda for Action, and Busan Partnership. Despite the existing rhetoric among both donor and recipient countries on this issue, Chasukwa and Banik (2019) find that aid continues to be disbursed by donors without proper coordination with national institutional structures. While the early literature on this topic justified such practice with reference to efficiency, more recent justifications have shifted to refer to corruption and weak implementation capacity consistent with Chasukwa and Banik's findings. The authors study this in the context of Malawi and show that that a variety of modalities are used to circumvent national institutions. The implication is fragmentation of aid and lack of coordination that leads to lower potential impact than would otherwise be possible (see also Bigsten & Tengstam, 2015).

Winters (2019) pursues a parallel topic addressing the question of the number of funding streams involved in World Bank projects. He combines data from World Bank projects with project performance ratings and studies within country variation across projects to establish any evidence for reduced aid effectiveness when more participants are engaged. As such, he points to yet another avenue that limits the potential impact of aid. This is very much in line with the traditional debates about the large transactions costs often inherent in providing aid to countries in need (see Paul & Vandeninden, 2012). Too many cooks do indeed often lead to less quality.

Aid effectiveness also may be influenced by donor motivations, and a significant literature on donor motivations in aid allocation exists. A particular strand is focused on how aid is used to buy influence through aid allocation to the Bretton Woods institutions and the United Nations (see e.g., Andersen, Harr, & Tarp, 2006). Reinsberg (2019) examines whether multi-bilateral aid is used to promote countries' interest in becoming a temporary member of the UN Security Council (see also Dreher, Lang, Rosendorff, & Vreeland, 2018). His analysis draws on new data using media reports to assess donor interest in winning seats in the UN security council, along with data on multi-bilateral aid flows. He demonstrates that multi-bilateral aid is indeed used for geopolitical purposes.

The demand and supply for aid is often couched in economic terms. At the same time, it is widely understood that development assistance is in many ways a political project by donor countries (see Lancaster, 2006). It is also clear that the politics of aid recipient coun-

tries matter to aid effectiveness. Over the past several decades, practitioners have sought to improve aid effectiveness by better taking politics into account, with growing interest and explicit reference to 'thinking and working politically' (TWP) in development (Carothers & de Gramont, 2013). Dasandi, Laws, Marquette and Robinson (2019) speak to the need for more systematic attention to the the evidence base on TWP and its impact on aid effectiveness. Although there is not yet a 'strong enough' evidence base, they argue, this is not surprising given that TWP is relatively recent in development programming. They discuss evidence in a variety of areas and suggest where future research should be focused.

While much remains to be learned about aid, as is true in other areas of social policy, we argue that future progress needs to take account of what is already known and hope that this thematic issue provides stimulating reading in this regard. At the same time, our ambition is to inspire further study and research on the need, supply, and provision of foreign aid and how to improve aid effectiveness.

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Conflict of Interests

The authors declare no conflict of interests.

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Article

A Meta-Analysis of Aid Effectiveness: Revisiting the Evidence

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Abstract

As research on the empirical link between aid and growth continues to grow, it is time to revisit the accumulated evidence on aid effectiveness. This study extends previous meta-analyses, noting that the increased availability of data enables us to conduct a sub-group analysis by disaggregating the sample into different time horizons to assess whether there are temporal shifts in aid effectiveness. The new and updated results show that the previously reported positive evidence of aid's impact is robust to the inclusion of more recent studies and this holds for different time horizons as well. The authenticity of the observed effect is further confirmed by results from funnel plots, regression-based tests, and a cumulative meta-analysis for publication bias.

1. Introduction

Analyzing the aid-growth nexus continues to be an area of focus in development economics. The empirical research on the effect of aid on growth goes back as far as the early 1970s. Though the methodological rigour varies, the profession has made numerous efforts since then to empirically analyze the effectiveness of aid in promoting growth. Results range from 'aid works' to 'aid does not work' and yet in other cases 'aid works but only under certain conditions'. Until 2007, the empirical evidence from individual studies varied but the past decade has witnessed convergence towards a positive assessment regarding the potency of aid in spurring economic growth (see, among others, Arndt, Jones, & Tarp, 2010, 2016). Over the years a variety of efforts have been made in the aid effectiveness literature to scrutinize and critically analyze the nature of the existing mixed aid growth evidence with the aim of showing where the balance of evidence lies. For instance, Hansen and Tarp (2000) carefully analyzed three generations of the aid effective-

ness literature, and more recently, Arndt et al. (2010) discussed a fourth generation. Our aim here is to complement these efforts, by synthesizing the existing empirical results from the accumulated evidence on aid and growth. In particular, we are interested in knowing what the range of findings (negative, zero, or positive) that have been evolving over the years, on average, tell us about aid's impact on growth.

Mekasha and Tarp (2013) addressed this issue relying on aid and growth empirical studies carried out over the period from 1970 to 2004. The accumulated evidence showed a positive impact of aid on growth during the 34-year period in question, and the authors documented that this effect is authentic, rather than an artefact of publication selection.

As the sample period in the work of Mekasha and Tarp (2013) only stretches until 2004, and given that more than a decade has passed since then, we present an update of the accumulated evidence here by including aid and growth empirical articles produced after 2004. Apart from enlarging the sample coverage and

hence working with a larger sample size, this also deepens the analysis in two important ways: (i) we now cover a longer time period and so are able to conduct a more disaggregated analysis, mainly by splitting the sample into different time periods (sub-groups); and (ii) we are able to assess whether there are temporal shifts in aid effectiveness.

In this line of thinking, the present study answers the following questions. First, does the addition of new studies have any impact on the results documented by Mekasha and Tarp (2013)? Second, has aid effectiveness changed over time and if so, is the change genuine or an artefact of publication bias? Third, is there heterogeneity between studies and if so, what explains the observed heterogeneity? To address these questions, we use a data set of 141 empirical studies on aid and growth that were conducted over the 1970–2011 period. This gives a total of 1,778 estimates for the meta-analysis.

The article is structured as follows. Section 2 first updates the aid effectiveness meta-analysis evidence documented by Mekasha and Tarp (2013) and then proceeds to present a sub-group analysis by disaggregating the data by year of publication. Section 3 presents a cumulative meta-analysis to establish how the weight of the evidence has shifted over time. This is followed by an in-depth investigation of publication bias in Section 4. In Section 5, we present a multivariate meta-regression analysis to understand the source of heterogeneity in effect estimates across studies. Finally, concluding remarks are given in Section 6.

2. Revisiting the Accumulated Evidence

2.1. Overall Effect

One of the main objectives of meta-analysis is to obtain an overall effect estimate (weighted average) from a body of literature by combining the appropriate summary statistics from each study. The choice of an appropriate model to combine the summary statistics extracted from each study is a major step in any meta-analysis and this choice depends on the degree of heterogeneity in effect sizes. In this regard, there are two alternative models: a fixed-effects model, which assumes away heterogeneity between studies and hence only uses within-study variances as study weights, and a random-effects model, which takes the across-study variation in the true effect estimates into account and uses both the within and between-study variances as weights.

Denoting the number of studies considered for the meta-analysis by k and the corresponding effect size estimates by $x_1, x_2, x_3 \dots x_k$, the overall effect estimate is:

$$\hat{\theta} = \frac{\sum_1^k \hat{w}_i x_i}{\sum_1^k \hat{w}_i} \quad (1)$$

where $\hat{w}_i$ in the case of the random and fixed-effects model is respectively given by $1/(\sigma_i^2 + \tau_i^2)$ and $1/\sigma_i^2$ where σ_i^2 and τ_i^2 are within and between-study variance of effect estimates respectively.

As can be seen from Equation 1, the random-effects model accounts for both within and between study variance to calculate the weighted average effect. Compared to the fixed-effects model, which only accounts for the within-study variance, the random-effects model gives a wider confidence interval for the overall effect and hence conservative estimates compared to the fixed-effects model (see also Kontopantelis, Springate, & Reeves, 2013). The assumption of effect homogeneity by the fixed-effect model is often criticized. In practice, a certain degree of variation in the true effect is expected. This is due to differences in the study populations as well as in the type, duration, and intensity of interventions (see Thompson & Pocock, 1991).

In this study, we rely on a random-effects model to obtain an overall average effect from the aid effectiveness literature using estimates from empirical aid-growth articles that became available over the 1970–2011 period. This choice is motivated by the apparent between-study heterogeneity in aid-growth empirical studies. This can easily be checked using statistical tests and graphical tools as shown in Mekasha and Tarp (2013) which discusses in detail why it is that the random-effects model is more appropriate in conducting a meta-analysis of aid and growth empirical studies.

The Bootstrapped DerSimonian–Laird (BDL) model was used to estimate the random-effects model. This is a non-iterative moments-based estimator which improves upon the DerSimonian–Laird model, a commonly used random-effects model, by estimating the between-study variance and other heterogeneity parameters applying a non-parametric bootstrap method. The BDL model has proven to be the best method in terms of detecting any heterogeneity, particularly for large-scale meta-analysis (see Kontopantelis et al., 2013).

Against this background, Table 1 presents the weighted average overall effect estimate from the aid-growth literature. We first disaggregated the sample into 'old period' and 'new period', where the former is the same as the sample period used in Mekasha and Tarp (2013) and the latter is a new sample focusing on the years added in this study. We finally report an overall effect estimate for the full sample period by combining the old and new periods indicated above. Such a sub-group analysis is useful in assessing whether the effect size has shifted over time (see Borenstein, Hedges, Higgins, & Rothstein, 2009). Factors such as improvement in data quality, changes in donor priorities, and the evolution of better estimation techniques, among others, are the likely explanations for potential changes in research findings within the aid effectiveness literature.

As can be seen from Table 1, the overall effect is found to be positive and statistically significant at 5 per cent level of significance. This is true both in the full

Table 1. Meta-analysis of the aid and growth literature.

Impact of aid on growth	Overall effect (BDL)	[95% CI]	Heterogeneity value (I^2) %	[95% CI]	Between study variance (τ^2)	N
Old period (1970–2004)	0.095	[0.083 0.107]	71.49	[69.31 73.51]	0.016	731
New period (2005–2011)	0.039	[0.032 0.047]	79.78	[78.62 80.88]	0.009	1,047
Full sample (1970–2011)	0.058	[0.052 0.064]	77.31	[76.28 78.30]	0.011	1,778

Notes: BDL refers to Bootstrapped DerSimonian-Laird random-effects model. Bootstrap of 10,000 repetitions is used in all cases. I^2 ranges from 0–100 per cent where a larger score shows a higher level of heterogeneity. Source: authors' estimates.

and the disaggregated samples. Even if the magnitude of the effect varies across periods and shows some decline over time, the overall conclusion regarding the potency of foreign aid in spurring growth remains the same. Regarding the practical relevance of the effect size estimate from meta-analysis, as such, no standard cut-off value exists to label an effect estimate as 'small', 'medium', or 'large'. However, according to a preliminary guideline in the literature that suggests a cut-off for economics meta-analysis, the effect sizes (the partial correlations) from our meta-analysis reported in Table 1, fall in the small to medium range. However, given that this is a preliminary guideline, one needs to be cautious about drawing firm conclusions. Further discussion is available in Mekasha and Tarp (2018).

As well as the above analysis, we have also estimated the overall effect at study level, i.e. by taking a single estimate from each study. The results from this exercise are presented in Table A2, which shows that the combined effect remains positive, statistically significant, and is higher compared to the case where the estimation is done based on study by regression level data. Moreover, as a further robustness check, we report in the Appendix a weighted average overall effect using a sample disaggregation based on the discussion in the aid effectiveness literature regarding the different generations of aid-growth empirical studies (see Arndt et al., 2010). As can be seen from Table A3 in the Appendix, our result remains robust.

Apart from showing the average effect size from studies included in the meta-analysis, the results presented in Table 1 show the level of heterogeneity as indicated by the I^2 statistics. In particular, the I^2 statistic shows the percentage of the between-study heterogeneity that can be attributed to the variability in the true treatment effect instead of sampling variation. An I^2 value of more than 50 per cent is normally considered to be high (see, for example, Kontopantelis et al., 2013).

In Table 1, there is, in all the cases, considerable heterogeneity (in the true effect of aid) across studies, suggesting that the effect homogeneity assumption implied by the fixed-effects model is not valid. In other words, the use of a random-effects model, which allows the

true effect of aid to vary between studies, is an appropriate choice.

To put our results into perspective, our finding stands in contrast to the results reported in Doucouliagos and Paldam (2015). These authors mainly focus their analysis on the 2007–2011 period and particularly argue that the 2007–2008 years are 'dark years' in aid effectiveness. They further add that the effect estimates in the 2009–2011 period show presence of an 'upward kink' which, according to these authors, is purely a result of publication bias rather than a real improvement in aid effectiveness.

We use the same dataset as Doucouliagos and Paldam (2015), so checking the assertions made by the authors makes our analysis more complete. We do so by answering the following four questions: (i) is there any reasonable justification behind the classification of the different periods?; (ii) is the 2007–2008 period really a dark period in aid effectiveness?; (iii) is the 'upward kink' real and is there any theoretical/intuitive reason to expect an upward kink in the 2009–2011 period?; (iv) can the concern regarding publication bias be justified by the data at hand?

To begin, we find that the decision to categorize the years 2005 and 2006 as 'old-period' is arbitrary and actually matters for the results. As indicated in Doucouliagos and Paldam (2015):

The period covered by Doucouliagos and Paldam (2008) is taken as the old period and *two more years with broadly similar results are added* [emphasis added], so the old period (1) stretches until the end of 2006. The article concentrates on the new period (2) commencing in 2007. (p. 6)

However, given that the sample in Doucouliagos and Paldam (2008) is from 1970 to 2004, there is no clear and convincing reason to categorize 2005 and 2006 as old period. As shown in the replication table (Table 2), comparing row 2 and row 3 in the middle section, this choice matters for the results; i.e., when one includes years 2005 and 2006 in the 'new-period', the effect of aid is positive (albeit small) and statistically significant, but

Table 2. Replication of Table 1 in Doucouliagos and Paldam (2015).

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Observations			Arithmetic mean						FAT-PET MRA			
	Period	N	Papers	Mean	(t1)	(t2)	P	PET	(t)	(trc)	FAT	(t)	(trc)
						Top section: All 1,779 estimates							
All	1970–2011	1,779	141	.066	.9	14.6	15.25	0.031	5.96	2.12	0.378	4.56	1.38
					Middle section: All estimates divided into old and new								
(1)	1970–2006	904	88	.098	1.1	13.8	13.67	0.036	5.27	3.56	0.586	5.86	3.37
(2)	2007–2011	875	53	.034	.6	6.3	16.88	**0.036**	4.50	**1.36**	0.000	0.00	0.00
(3)	2005–2011	1,047	68	.037	.699	7.4	17.9	**.0416**	7.22	**2.54**	−.0448	−0.41	−0.11
				Bottom section: The new estimates starting in 2007 divided into two sub-periods									
(A)	2007–2008	534	28	.002	.2	.27	15.95	**0.039**	3.06	**1.01**	−.423	−2.21	−0.67
(B)	2009–2011	341	25	.084	1.3	11.0	18.32	**0.019**	1.92	**0.91**	.915	**4.59**	1.75
					New classification for period A and period B								
(A)	2005–2007	430	32	0.029	.580	3.47	20.4	**.039**	5.03	**1.99**	−.214	−1.14	−0.41
(B)	2008–2011	617	36	0.043	.785	6.93	16.17	**.055**	5.75	**1.94**	−.098	**−0.61**	−0.15

Notes: FAT: funnel asymmetry test; PET: precision estimate test; MRA: meta regression analysis; trc: robust cluster corrected t-statistics, where the clustering is done at the paper level. t1 is the average t-statistics of the estimates, t2 is t-statistics given by the ratio of the mean and standard error of the N estimates and p is the average of the precision of the estimates. Source: authors' estimates.

this would not have been the case had the new period started from 2007.

We also believe there is no clear and convincing reason to pick 2009 as a starting year for period B (2009–2011), and the results and main conclusion of Doucouliagos and Paldam (2015) are sensitive to a change in the starting year of period B. Following the discussion above, we redefine periods A and B by including 2005 and 2006 in period A and 2008 in period B, and the results are presented in the last panel of Table 2. As can be seen from the last panel of this table, the effect of aid on growth remains positive and statistically significant in both the 2005–2007 and 2008–2011 periods. And if one starts period B from 2008 instead of 2009 (last row of Table 2), the result appears to be contrary to what Doucouliagos and Paldam (2015) found. That is, in the 2008–2011 sample period, the impact of aid on growth is, on average, positive (0.05) and is precisely estimated. On the other hand, the bias coefficient is negative and statistically indistinguishable from zero. Moreover, the Doucouliagos and Paldam (2015) claim of an 'upward kink' in the 2009–2011 period is not robust to how one defines periods A and B. Given that there is no clear reason why one should expect any jump in this period, the 'upward kink' reported in Doucouliagos and Paldam (2015) does not seem to reflect real changes. As it will become clear in what follows, this jump is exclusively due to the inclusion of a large set of observations from one single study.

The 0.084 mean estimated in Doucouliagos and Paldam's (2015) classification of period B (2009–2011) is almost twice as large as the 0.043 mean estimated in an alternative classification of period B covering the years 2008–2011. This clearly shows that the results reported in Doucouliagos and Paldam (2015) vary a lot depending on whether one puts observations from year 2008 in ei-

ther period A or period B. A closer look at the data shows that this is due to the influence of a large set of estimates from the article by Rajan and Subramanian (2008), which contributes 138 estimates (observations) out of the total 276 estimates coded for 2008. Observations taken from Rajan and Subramanian (2008) account for about 25 per cent of the total observations used in the 2007–2008 period. Thus, Doucouliagos and Paldam's (2015) labelling of 2007–2008 as a dark period for aid effectiveness is mainly driven by the large number of observations taken from Rajan and Subramanian (2008). It is important to highlight that estimating the effect of aid on growth by excluding estimates from Rajan and Subramanian (2008) gives a positive and statistically significant effect of aid on growth for the 2007–2008 period.

2.2. Patterns of Evidence over Time—Cumulative Meta-Analysis

Another question of interest to both researchers and policymakers is whether there are temporal changes in aid effectiveness. The article presented here has made effort to assess whether the magnitude and precision of the impact of aid on growth changes with the passage of time or following the addition of newer studies. To this end, the work of Lau et al. (1992) was followed and cumulative meta-analysis was conducted with studies being sequentially added to the analysis according to a variable of interest, and a new-pooled estimate recalculated every time a new study was added to the analysis. Since the objective is to uncover the pattern of evidence over time and to see how the conclusions may have shifted, the variable of interest is the year of publication for each study. Thus, in doing the cumulative meta-analysis, studies were sorted in chronological order for the 1970–2011 period. In cases where studies report multiple estimates,

the data were pooled by study and an overall effect estimate calculated for each study.

Figure 1 and Table A4 in the Appendix present the results from cumulative random-effects meta-analysis of the aid-growth literature. In Figure 1, the circles show the estimates from the cumulative meta-analysis and the horizontal lines show the 95 per cent confidence interval. Moreover, the vertical dotted line in the middle of the figure shows the combined estimate. The value for each row shows the summary estimate for a meta-analysis based on all studies up-to and including that row. The point estimate in the last row is the same as the effect estimate shown in the summary line as the analysis in the last row includes data from all the 141 studies.

As can be seen from the results in Figure 1 and Table A4, there is evidence of the positive impact of aid on growth since the early 1980s with a magnitude of 0.206. As one moves further down the plot, the effect size shows some decline and stabilizes around a combined effect equal to 0.074 with a confidence interval from 0.051 to 0.097. Over the years, the addition of new studies does not substantially change the aid effectiveness conclusion. In general, even if the answers to the aid effectiveness question in terms of growth impact have evolved over the years, the balance of evidence, on average, points to a positive (albeit small to moderate) and statistically significant impact of aid on growth.

3. Assessing Publication Bias

One issue that can jeopardize the credibility of results from meta-analysis is the issue of publication bias. It arises if there is a tendency to only publish research findings with statistically significant treatment effect (Sterne, Gavaghan, & Egger, 2000). That is, if studies included in the meta-analysis are a biased sample of the target population of studies (for example, if small studies with statistically insignificant findings remain unpublished/in the grey literature), the meta-analysis may overestimate the true effect (see Borenstein et al., 2009). In the following section, using various methods we, assess whether publication bias is a concern within the aid effectiveness literature.

3.1. Funnel Plot

One way to assess the issue of publication bias in a body of literature is to use funnel plots that relate the precision of studies (study size) to the size of the effect estimate. In the absence of publication bias, smaller studies are expected to scatter widely at the bottom of the graph with the spread getting narrower as study precision increases. Thus, if publication bias is not a problem, the plot takes the shape of a symmetrically inverted funnel.

Figure 2 presents a funnel plot of the aid effectiveness literature. The vertical line at the centre of the plot shows the combined effect estimate from the aid effectiveness literature. As can be seen from the figure, the estimates appear randomly distributed around the combined effect estimate, and the plot exhibits symmetry showing lack of evidence to suggest the existence of publication bias in the aid-growth literature. Particularly note that smaller studies with statistically insignificant results are not missing.
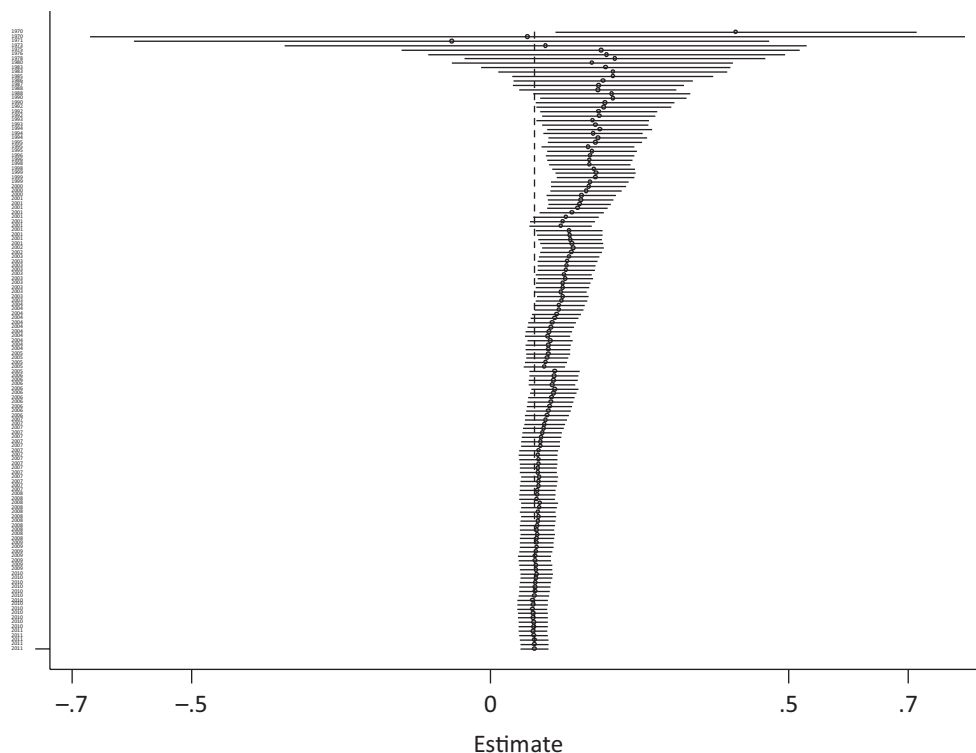

Figure 1. Cumulative random effects meta-analysis. Source: authors' computation.
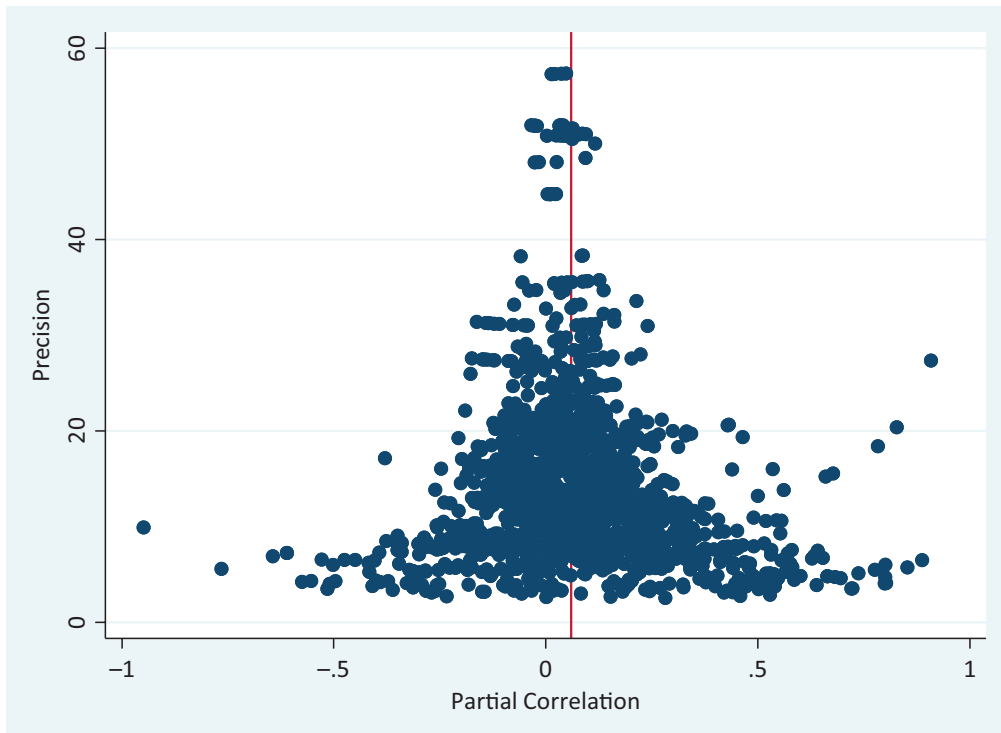
Figure 2. Funnel plot from the aid-growth literature. Source: authors' computation.

A further check for publication bias relies on contour enhanced funnel plots. This approach uses the idea that the main reason for studies to remain unpublished is lack of statistical significance, with studies that cannot achieve standard levels of statistical significance left out of mainstream publications (Dickersin, 1997).

To check whether this is the case in the aid effectiveness literature, we add contours of statistical significance on the funnel plot shown in Figure 1. This makes it easier to assess the statistical significance of hypothetically missing studies. That is, we can check whether the areas where studies are likely to be missing are areas of low statistical significance and whether areas, where studies are more visible, are areas of high statistical significance.

Publication bias is likely to exist if the areas where studies are missing are areas of low statistical significance. As shown in the contour enhanced funnel plot depicted in Figure 3, this is not the case for the aid effectiveness literature studied here. Overall, the distribution of the estimates is reasonable in the regions of both low and high statistical significance, and there is no evidence that studies with insignificant results have been repressed.

3.2. Cumulative Meta-Analysis and Publication Bias

Cumulative meta-analysis can also be used to investigate whether the combined effect estimate presented in Section 2 suffers from publication bias in the literature. This is done first by sorting studies based on their level of precision (from the most precise to the least precise) and then by sequentially adding studies to the analysis.

That is, in the cumulative meta-analysis, the first estimate represents an estimate of the most precise study, and the second estimate represents meta-analysis of the first two precise studies, and so on. The assumption here is that precise studies are less likely to suffer from publication bias, and it is the less precise studies that are more prone to overstating their effect estimates to compensate for their large standard errors in order to achieve a statistically significant effect.

This approach helps us to see if the effect estimates of the less precise studies that are likely to report biased (larger) effect estimates to increase their chances of publication influence the combined effect estimate. Thus, if the effect size increases, as less precise studies are included in the analysis, it is likely that there is a bias from small studies (see Borenstein et al., 2009).

Figure 4 presents the cumulative meta-analysis of studies conducted over the 1970–2011 period. Here studies are sorted from most to least precise, and the vertical reference line represents the combined effect estimate based on the random-effects model. While the circles show the cumulative effect estimates, the horizontal lines show the 95 per cent confidence intervals. On the vertical axis, study names ordered based on their level of precision are shown and the horizontal axis shows the partial effect estimate. Since the names of these 141 studies and respective cumulative effect estimates are not visible in this plot, we have also presented the same cumulative meta-analysis in a table format (see Table A5).

As shown in Figure 4 and Table A5, there is no as such consistent pattern of an increase in the cumulative effect
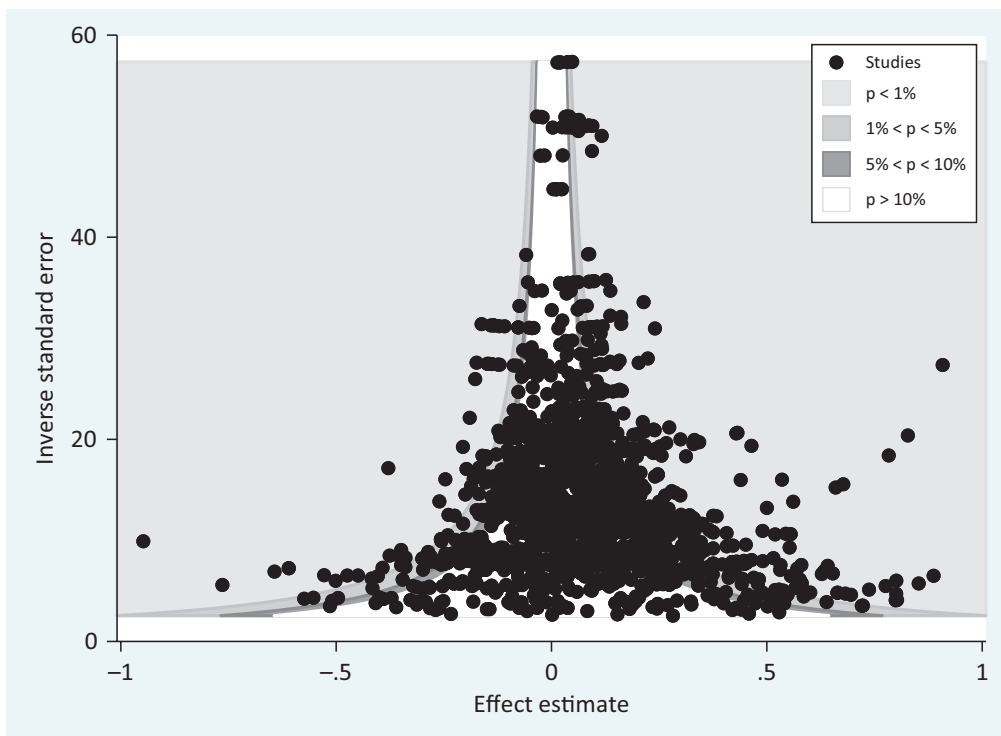
Figure 3. Contour enhanced funnel plot. Source: authors' computation.
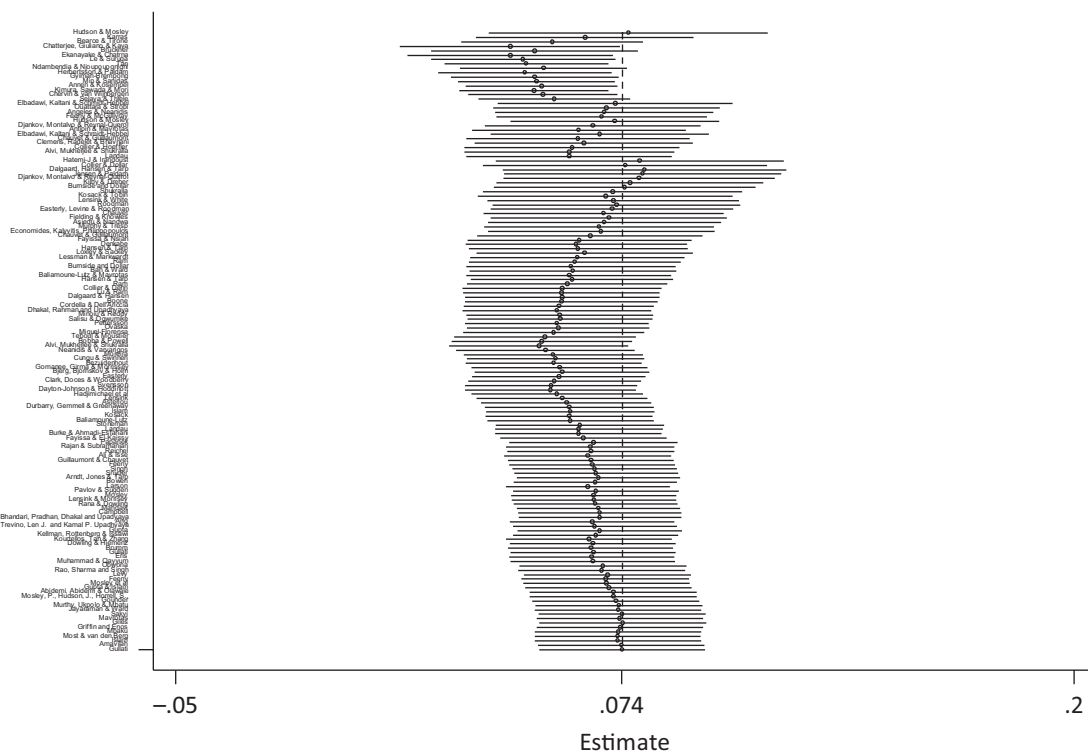
Figure 4. Cumulative meta-analysis: 1970–2011. Source: authors' computation.

estimate as less and less precise studies are added to the analysis. For instance, the most precise study has an effect estimate of 0.076 with a confidence interval from 0.037 to 0.115, while the cumulative meta-analysis of the ten most precise studies shows an estimate of 0.05. After that, the combined effect estimate starts to increase, reaching 0.07 and 0.08 with the top 20 and 30 most pre-

cise studies added, respectively. As more and more (relatively less precise) studies are added, the cumulative effect rather shows a decline reaching 0.05 and gradually converging at 0.074.

In general, further addition of the less and less precise studies does not reveal a steadily increasing clear pattern of the cumulative effect estimates to suggest the

existence of publication bias in the literature. It is also worth noting that the confidence intervals from the cumulative meta-analysis of the least precise studies do overlap with that obtained from the cumulative effect estimates of the most precise studies; i.e. comparing the confidence interval from the least precise studies (final rows) with the confidence interval when the 1st, 10th, 20th etc. most precise studies are added to the analysis. This shows that the effect estimates from the most and least precise studies are not statistically significantly different, making the issue of publication bias less of a concern here.

3.3. Regression-Based Test

Since visual inspection of a funnel plot is subjective, we also conducted a regression-based test to objectively assess the presence or absence of publication bias. Egger, Smith, Sceider and Minder (1997) is the most commonly used test to assess asymmetry in funnel plots. It regresses the standardized effect from each study on precision (inverse of standard error). The regression to be estimated takes the following form:

$$t_i = \beta_0 + \beta_1 \frac{1}{SE_i} + v_i \qquad (2)$$

where t_i is the standardized effect and $1/SE_i$ is the measure of precision. The parameters of interest are β_0 and β_1 which capture bias and genuine effect respectively. A detailed discussion of the test, the importance of doing a multivariate analysis and the choice of covariates can be found in Mekasha and Tarp (2013).

The result from the Egger et al. (1997) funnel asymmetry test is reported in Table 3. As can be seen from the results in both the bivariate and multivariate regressions, the bias coefficient is found to be statistically indistinguishable from zero, confirming the absence of publication bias in the aid-growth literature, in line with the funnel plot analysis. Moreover, in both the bivariate and multivariate results, the coefficient of precision (the estimate of the impact of aid on growth) is found to be positive and statistically significant. Note that when we look at our preferred estimation, controlling for all study characteristics (Columns 2, 5 and 6), the estimated effect of

aid from the existing literature is 0.13, 0.05, and 0.05 for the 'old period', 'new period', and the 'full sample', respectively, with the coefficients being statistically significant in all cases. This is in stark contrast to the finding of Doucouliagos and Paldam (2015) who reported that this coefficient was insignificant in both a statistical and an economic sense.

Overall, based on graphical tools and the regression-based tests, publication bias is not found to be a concern in the aid-growth empirical literature. This confirms that the overall effect estimate obtained from the aid effectiveness literature is not an artefact of publication bias.

4. Meta-Regression Analysis

As seen in Table 1, there is considerable heterogeneity in the aid effectiveness literature. In this section, we explore whether this observed heterogeneity could be attributed to one or more of the study characteristics. To this end, we employ a random-effects meta-regression analysis. In this regression, following estimation of the between-study variance τ^2 using methods of moments, the coefficient estimates are estimated using weighted least squares where $1/(\sigma_i^2 + \tau^2)$ is the weight.

The results from the meta-regression are presented in Table A6 in the Appendix. According to the statistics reported at the bottom of the table, 72 per cent of the residual variance is due to heterogeneity of the true effect, with the remaining 18 per cent attributed to sampling variability. Moreover, the proportion of between-study variance explained by the covariates can be seen from the adjusted R^2. This is calculated by comparing the estimated between-study variance with its value when no covariates are included. We note that 25 per cent of the between-study variance is explained by the covariates and the remaining between-study variance is found to be 0.008.

Turning to the role of the study characteristics in explaining the variation in reported effects, it appears that more than 20 covariates are important. However, caution needs to be exercised in interpreting the results from this regression. According to Higgins and Thompson (2004), testing several covariates without adjusting for multiplicity will lead to increased false positive rates in

Table 3. Funnel asymmetry test (FAT) meta-regression analysis (MRA) (dependent variable: standardized effect (t-stat)).

	Old period		New period		Full sample	
	Bivariate	Multivariate	Bivariate	Multivariate	Bivariate	Multivariate
Precision	0.05	0.13***	0.04**	0.05**	0.03**	0.05**
	(0.03)	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)
_cons	0.54*	0.37	−0.05	−1.42	0.38	−0.09
	(0.31)	(0.75)	(0.40)	(1.0)	(0.27)	(0.59)
N	731	715	1,047	1,047	1,778	1,762

Notes: Standard errors in parentheses. * p < .1, ** p < .05, *** p < 0.01. Old period (1970–2004), new period (2005–2011) and full sample (1970–2011). Source: authors' estimates.

meta-regression. To deal with this issue, these authors suggest a permutation test to assess statistical significance in meta-regression and warn researchers not to make claims about statistical significance before conducting such a test. Thus, following the suggestion of Higgins and Thompson (2004), we conduct the permutation test on the meta-regression reported in the Appendix.

The results are reported in Table 4. The first column shows permutation p-values without adjustment for multiplicity and the second column shows p-values adjusted for multiplicity. While Table 4 reveals which study characteristics are, statistically speaking, important in explaining the variation in reported effect estimates within the aid-growth literature, Table A6 shows in which direction (how) each particular study characteristic affects the reported estimates. After adjusting for multiple testing, only 10 of the included covariates appear to have a role in explaining the heterogeneity in effect size, shown in bold within Table 4. We highlight that the type of publication outlet, data type (structure), and type of controls included in the growth regression are found to be important in explaining the observed heterogeneity in reported effect estimates of the impact of aid on economic growth. For instance, the positive and statistically significant coefficient on the variable 'Panel' (from Table A6 and Table 4) implies, ceteris paribus, that studies using panel data, on average report higher (positive) partial correlations. Another point worth noting from the results in the tables is that the coefficients of the decade dummies are statistically indistinguishable from zero. This implies that the sample period covered by the original studies does not have a role in explaining the reported variation in research findings on aid and growth.

5. Conclusion

The main aim of this study was to update the aid effectiveness meta-analysis evidence in Mekasha and Tarp (2013), adding newly available studies which emerged from 2004 to 2011. To this end, we employed a random-effects model. This is the appropriate choice in the presence of considerable heterogeneity in the true effects, which is the case in the aid effectiveness literature. The positive impact of aid on growth in Mekasha and Tarp (2013) is shown here as being robust to the inclusion of new studies in the meta-analysis and this appears to be true for different time horizons.

Having established this result, we carefully assessed whether publication bias has any impact on the observed effect estimates. Results from funnel plots, a regression-based test, and a cumulative meta-analysis for publication bias all suggest that publication bias is not a concern within the aid-growth literature and the observed effect is not an artefact hereof. Finally, given the considerable heterogeneity observed in the data, we conducted a meta-regression analysis to explain the heterogeneity in reported effect estimates. After adjusting the p-values for multiple testing, it is found that only ten out

Table 4. Monte Carlo permutation test for meta-regression p-values unadjusted and adjusted for multiple testing.

Number of obs. = 1,761 Permutations = 20,000		
Partial	Unadjusted	Adjusted
Gender	0.891	1.000
Working paper	0.963	1.000
Cato	0.293	1.000
JDS	0.494	1.000
JID	0.498	1.000
EDCC	0.000	**0.000**
AER	0.654	1.000
Applied economics	0.039	0.829
Sub-sample	0.000	**0.007**
Low income	0.019	0.581
World Bank	0.519	1.000
Influence	0.112	0.991
Theory	0.004	0.174
Gap model	0.088	0.977
Panel	0.000	**0.005**
No. of countries	0.000	**0.008**
No. of years	0.488	1.000
Average	0.026	0.696
y1960s	0.006	0.238
y1970s	0.064	0.941
y1980s	0.006	0.238
y1990s	0.099	0.985
y2000	0.312	1.000
Outliers	0.820	1.000
Single country	0.000	**0.008**
EDA	0.080	0.968
Asia	0.122	0.995
Latin	0.813	1.000
Aid-institutions interaction	0.002	**0.078**
Aid-policy interaction	0.003	0.137
Aid square	0.010	0.391
Lag used	0.287	1.000
System growth and aid	0.064	0.941
System growth and capital	0.179	0.999
Capital	0.700	1.000
Human capital	0.077	0.958
FDI	0.402	1.000
Policies	0.030	0.750
Instability	0.423	1.000
Inflation	0.000	**0.001**
Fiscal	0.029	0.725
Size of government	0.000	**0.001**
Region dummy	0.031	0.753
Ethnic fractionalization	0.000	**0.002**
Financial development	0.000	**0.004**
Openness	0.219	1.000
Population	0.316	1.000
Per capita income	0.051	0.886
OLS	0.516	1.000
Africa	0.582	1.000

Note: see Table A1 for a detailed description of the variables used in Table 4. Source: authors' estimates.

of the 50 study characteristics appear to be important in explaining the observed heterogeneity. These include the type of publication outlet, data types, and the type of controls used in the growth regression.

In sum, careful meta-analysis, including more recent studies do not suggest any material changes in the previously established insight that aid promotes growth in a statistically significant manner. The results presented here coupled with the previously documented evidence in Mekasha and Tarp (2013) provide a systematic and objective (quantitative) assessment of the current body of findings within the literature and hence give a clear answer to the question raised by Cassen and Associates (1994): *Does Aid Work?* Having drawn this conclusion, the following points need attention in future evaluations of aid effectiveness.

First, the evidence presented here is clearly not the full story of aid effectiveness. Promoting economic growth is often not the primary objective of foreign aid, and neither should it be. Following the adoption of the Millennium Development Goals (MDGs) back in 2000 and the Sustainable Development Goals (SDGs) in 2017, donors tend to channel most of their assistance to social sectors such as health and education as well as to poverty reduction interventions in general. With multifaceted objectives, aid effectiveness meta-analysis needs to move beyond examining the role of aid on economic growth. A meta-analysis of aid and poverty reduction would be an interesting future avenue to explore, once sufficient empirical evidence from individual studies has accumulated. Furthermore, on top of the aid effectiveness analysis, careful attention should also be given to the increasing focus on the concept of development effectiveness that covers rather broader outcomes.

Second, there is a need to complement the existing empirical evidence on aid and growth with country-specific success/failure stories, which we believe are a valid and yet often neglected aspect in the discourse surrounding aid effectiveness. For instance, Arndt, Jones and Tarp (2007) have shown how a high level of sustained aid to Mozambique helped the country establish peace, manage the difficulties of post-war stabilization, and embark on widespread reconstruction. In addition, the experiences of Vietnam and South Korea are also examples regarding the role that aid can play in facilitating the development process of a country.

Last, but by no means least, future aid effectiveness studies need to deal with data and methodological concerns associated with the current aid-growth empirical studies. These concerns include, but are not limited to, the need to control important factors such as export price (terms of trade) shocks, exports and private capital flows, the need for comparing aid effectiveness results using alternative aid data such as Country Programmable Aid which better reflect actual aid flows to countries and which have increasingly become available in recent years. Moreover, in assessing aid effectiveness, it is crucial to look for the longer-term impact of aid as a large propor-

tion of aid goes to social sectors like health and education following global development commitments such as the MDGs and SDGs.

Conflict of Interests

The authors declare no conflict of interests.

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Appendix

Table A1. Variables and their descriptions.

Variables	Description	Variables	Description
Working paper	Binary dummy (BD) for unpublished paper	No. of countries	Number of countries included in the sample
Cato	BD for Cato journal	No. of years	Number of years covered in the analysis
JDS	BD for Journal of Development Studies	Africa	BD if countries from Africa included
JID	BD for Journal of International Development	Asia	BD if countries from Asia included
EDCC	BD for Economic Development and Cultural Change	Latin	BD if countries from Latin America included
AER	BD for American Economic Review	Single Country	BD if data from single country
Applied Economics	BD for Applied Economics	y1960s	BD if data for the 1960s
World Bank	BD for authors affiliated with the World Bank	y1970s	BD if data for the 1970s
Gender	BD if at least one of the authors is female	y1980s	BD if data for the 1980s
Expectations	BD for authors with realized expectations about aid-growth relation	y1990s	BD if data for the 1990s
Influence	BD for authors who acknowledge feedback from other authors in aid effectiveness literature	Sub-sample	BD if data relate to sub-sample of countries
Panel	BD for use of panel data	Low income	BD if data related to sub-sample of low-income countries
EDA	BD for use of Effective Development Assistance Data	Financial development	BD for control of financial development
Aid square	BD if aid square term added	Ethnic fractionalization	BD for control of ethnic fractionalization
Interaction policy	BD for aid interacted with policy	Region dummy	BD for regional dummies
Interaction institutions	BD for aid interacted with institutions	Human capital	BD for control of human capital
Capital	BD for control of domestic savings or investment	Openness	BD for control of trade openness
FDI	BD for control of foreign capital flows other than aid	Population	BD for control of population size
Gap model	BD for two gap model	Per capita income	BD for control of per capita income
Theory	BD for paper developing a theory	Policy	BD for control of policies
Average	Number of years involved in data averaging	OLS	BD for use of OLS
Lag used	BD for use of lagged value of aid	Growth and aid	BD for equation system with a growth and an aid equation
Inflation	BD for control if inflation	Growth and capital	BD for equation system with a growth and a saving equation
Instability	BD for control of political instability		
Fiscal	BD for control of fiscal stance		
Size of govt.	BD for control of government size		

Source: based on Doucouliagos and Paldam (2008).

Table A2. Meta-analysis of aid and growth literature: Using observations at study level.

Impact of aid on growth	Overall effect (BDL)	[95% CI]	Heterogeneity value (I^2)%	[95% CI]	Between-study variance (τ^2)	N
Old period (1970–2004)	0.097	[0.061 0.134]	60.90	[49.57 69.69]	0.013	73
New period (2005–2011)	0.058	[0.027 0.088]	77.53	[71.80 82.09]	0.010	68
Full sample (1970–2011)	0.074	[0.051 0.098]	71.28	[66.01 75.73]	0.011	141

Notes: BDL refers to Bootstrapped DerSimonian-Laird random-effects model. Bootstrap of 10,000 repetitions used in all cases. I^2 = a heterogeneity measure ranging from 0–100 per cent where a larger score shows a higher level of heterogeneity. Source: authors' estimates.

Table A3. Meta-analysis of aid and growth literature.

Impact of aid on growth	Overall effect (BDL)	[95% CI]	Heterogeneity value (I^2)%	[95% CI]	Between study variance (τ^2)	N
Full sample	0.058	[0.052 0.064]	77.39	[76.36 78.37]	0.011	1,778
Period I: 1st Generation (1970–1979)	0.292	[0.142 0.442]	90.48	[87.41 92.79]	0.139	28
Period II: 2nd Generation (1980–1995)	0.108	[0.083 0.133]	46.44	[35.78 55.33]	0.009	169
Period III: 3rd Generation (1996–2007)	0.055	[0.047 0.064]	80.43	[79.26 81.53]	0.012	964
Period IV: 4th Generation (2008–2011)	0.049	[0.040 0.058]	71.24	[68.83 73.46]	0.007	617

Notes: BDL refers to Bootstrapped DerSimonian-Laird random-effects model. Bootstrap of 10,000 repetitions used in all cases. I^2 = a heterogeneity measure ranging from 0–100 per cent where a larger score shows a higher level of heterogeneity. Source: authors' estimates.

Table A4. Cumulative random-effects meta-analysis of 141 studies: Pattern of aid effectiveness over time (Studies sorted in chronological order).

Trial	Cumm. Est.	[95% Conf. Interval] Lower	Upper	Trial	Cumm. Est.	[95% Conf. Interval] Lower	Upper	Trial	Cumm. Est.	[95% Conf. Interval] Lower	Upper
1970	0.411	0.110	0.713	2001	0.138	0.084	0.191	2007	0.090	0.058	0.122
1970	0.062	−0.672	0.795	2001	0.136	0.084	0.188	2007	0.092	0.061	0.124
1971	−0.065	−0.597	0.467	2002	0.139	0.088	0.190	2007	0.091	0.059	0.122
1973	0.093	−0.344	0.530	2002	0.135	0.085	0.186	2007	0.089	0.058	0.120
1975	0.185	−0.148	0.518	2003	0.139	0.089	0.189	2007	0.088	0.058	0.119
1976	0.195	−0.103	0.493	2003	0.137	0.088	0.186	2007	0.085	0.054	0.116
1978	0.209	−0.042	0.460	2003	0.134	0.087	0.182	2007	0.084	0.054	0.115
1980	0.171	−0.063	0.406	2003	0.131	0.084	0.179	2007	0.083	0.053	0.114
1983	0.193	−0.015	0.401	2003	0.133	0.086	0.180	2007	0.081	0.051	0.111
1983	0.206	0.015	0.397	2003	0.132	0.086	0.179	2007	0.080	0.050	0.110
1985	0.206	0.038	0.373	2003	0.129	0.083	0.174	2007	0.080	0.050	0.109
1986	0.189	0.040	0.339	2003	0.126	0.081	0.171	2008	0.079	0.049	0.108
1987	0.182	0.039	0.324	2003	0.126	0.082	0.169	2008	0.078	0.049	0.107
1988	0.180	0.049	0.311	2003	0.122	0.079	0.165	2008	0.077	0.048	0.106
1988	0.203	0.072	0.334	2003	0.119	0.077	0.162	2008	0.081	0.052	0.111
1990	0.189	0.066	0.312	2004	0.115	0.073	0.157	2008	0.079	0.050	0.109
1990	0.193	0.077	0.308	2004	0.111	0.069	0.152	2008	0.078	0.049	0.107
1992	0.190	0.078	0.302	2004	0.115	0.073	0.156	2008	0.078	0.049	0.106
1992	0.191	0.083	0.298	2004	0.112	0.072	0.153	2008	0.079	0.050	0.107
1992	0.182	0.088	0.276	2004	0.109	0.069	0.149	2008	0.078	0.049	0.107
1993	0.172	0.078	0.265	2004	0.105	0.065	0.145	2008	0.079	0.051	0.107
1993	0.176	0.088	0.265	2004	0.101	0.061	0.141	2008	0.078	0.051	0.106
1994	0.184	0.097	0.272	2004	0.099	0.060	0.138	2009	0.076	0.048	0.104
1994	0.174	0.091	0.256	2004	0.099	0.061	0.137	2009	0.076	0.048	0.103
1994	0.180	0.098	0.262	2004	0.099	0.062	0.136	2009	0.077	0.049	0.105
1995	0.168	0.087	0.248	2004	0.097	0.060	0.133	2009	0.077	0.050	0.104
1995	0.174	0.096	0.252	2005	0.094	0.058	0.130	2009	0.076	0.049	0.103
1995	0.170	0.096	0.245	2005	0.092	0.058	0.127	2009	0.077	0.050	0.103
1996	0.169	0.097	0.241	2005	0.090	0.056	0.124	2009	0.077	0.050	0.103
1996	0.167	0.096	0.237	2005	0.107	0.066	0.149	2010	0.074	0.047	0.101
1998	0.167	0.099	0.234	2005	0.108	0.066	0.149	2010	0.075	0.048	0.101
1998	0.174	0.105	0.242	2006	0.105	0.065	0.146	2010	0.074	0.048	0.100
1999	0.164	0.096	0.231	2006	0.104	0.064	0.145	2010	0.074	0.048	0.099
1999	0.167	0.101	0.233	2006	0.102	0.064	0.141	2010	0.073	0.047	0.098
1999	0.167	0.103	0.231	2006	0.100	0.062	0.138	2010	0.073	0.047	0.098
2000	0.162	0.102	0.223	2006	0.099	0.061	0.137	2010	0.073	0.048	0.097
2000	0.160	0.101	0.220	2006	0.097	0.059	0.134	2010	0.071	0.047	0.096
2000	0.153	0.095	0.210	2006	0.096	0.059	0.133	2010	0.070	0.046	0.095
2001	0.152	0.097	0.207	2006	0.100	0.063	0.137	2010	0.071	0.047	0.095
2001	0.142	0.085	0.198	2006	0.097	0.060	0.134	2010	0.071	0.047	0.095
2001	0.131	0.072	0.190	2006	0.095	0.059	0.132	2010	0.071	0.048	0.095
2001	0.127	0.070	0.184	2007	0.095	0.059	0.131	2010	0.072	0.049	0.096
2001	0.128	0.073	0.182	2007	0.093	0.058	0.129	2011	0.072	0.049	0.095
2001	0.122	0.068	0.175	2007	0.092	0.058	0.125	2011	0.072	0.049	0.095
2001	0.136	0.079	0.193	2007	0.090	0.057	0.122	2011	0.074	0.050	0.097
2001	0.133	0.078	0.188	2007	0.090	0.057	0.122	2011	0.074	0.051	0.097
2001	0.136	0.081	0.191	2007	0.090	0.058	0.122	2011	0.074	0.051	0.097

Source: authors' computation.

Table A5. Cumulative random-effects meta-analysis of 141 studies: Assessing publication bias (studies sorted from most to least precise).

Study	Cumm. Est.	[95% Conf. Interval]		Study	Cumm. Est.	[95% Conf. Interval]	
		Lower	Upper			Lower	Upper
Hudson & Mosley	0.076	0.037	0.115	Burnside and Dollar	0.060	0.031	0.089
Karras	0.064	0.034	0.094	Bah & Ward	0.061	0.032	0.089
Bearce & Tirone	0.055	0.029	0.080	Baliamoune-Lutz & Ma	0.059	0.031	0.088
Chatterjee, Giuliano	0.043	0.013	0.074	Hansen & Tarp	0.060	0.032	0.088
BrŸckner	0.050	0.021	0.078	Ram	0.059	0.031	0.087
Ekanayake & Chatrna	0.043	0.015	0.072	Collier & Dehn	0.057	0.030	0.085
Le & Suruga	0.047	0.021	0.072	Lu & Ram	0.057	0.030	0.085
Tan	0.048	0.025	0.070	Dalgaard & Hansen	0.058	0.031	0.085
Ndambendia & Njoupou	0.052	0.029	0.076	Boone	0.057	0.031	0.084
Herbertsson & Paldam	0.047	0.023	0.071	Cordella & Dell'Aric	0.057	0.030	0.083
Gyimah-Brempong	0.050	0.027	0.073	Dhakal, Rahman and U	0.056	0.030	0.082
Min & Sanidas	0.050	0.029	0.072	Minoiu & Reddy	0.057	0.031	0.083
Annen & Kosempel	0.052	0.031	0.073	Salisu & Ogwumike	0.057	0.031	0.083
Kimura, Sawada & Mor	0.050	0.029	0.070	Pettersson	0.056	0.031	0.082
Chervin & van Wijnbe	0.052	0.032	0.073	Ovaska	0.056	0.031	0.082
Selaya & Thiele	0.055	0.034	0.076	Miquel-Florensa	0.055	0.030	0.080
Elbadawi, Kaltani &	0.072	0.040	0.105	Teboul & Moustier	0.053	0.028	0.078
Ouattara & Strobl	0.070	0.038	0.101	Bobba & Powell	0.052	0.027	0.077
Angeles & Neanidis	0.069	0.039	0.100	Alvi, Mukherjee & Sh	0.051	0.026	0.076
Feeny & McGillivray	0.069	0.039	0.098	Neanidis & Varvarigo	0.053	0.028	0.078
Hudson & Mosley	0.072	0.043	0.101	Moreira	0.055	0.030	0.080
Djankov, Montalvo &	0.066	0.036	0.096	Cungu & Swinnen	0.056	0.031	0.080
Antipin & Mavrotas	0.062	0.033	0.092	Bezuidenhout	0.055	0.031	0.080
Elbadawi, Kaltani &	0.068	0.038	0.098	Gomanee, Girma & Mor	0.057	0.032	0.081
Chauvet & Guillaumon	0.062	0.031	0.093	Bjerg, Bjornskov & H	0.058	0.033	0.082
Clemens, Radelet & B	0.064	0.033	0.094	Easterly	0.057	0.033	0.081
Collier & Hoeffler	0.060	0.030	0.090	Clark, Doces & Woodb	0.055	0.031	0.079
Alvi, Mukherjee & Sh	0.060	0.030	0.089	Svensson	0.055	0.031	0.078
Landau	0.059	0.031	0.088	Dayton-Johnson & Hod	0.054	0.031	0.078
Hatemi-J & Irandoust	0.079	0.039	0.119	Hadjimichael et al.	0.056	0.032	0.080
Collier & Dollar	0.075	0.036	0.114	Lensink	0.058	0.034	0.081
Dalgaard, Hansen & T	0.080	0.041	0.120	Asteriou	0.059	0.035	0.082
Jensen & Paldam	0.080	0.041	0.118	Durbarry, Gemmell &	0.060	0.036	0.083
Djankov, Montalvo &	0.079	0.041	0.117	Islam	0.060	0.036	0.083
Kilby & Dreher	0.076	0.039	0.113	Kosack	0.059	0.036	0.083
Burnside and Dollar	0.075	0.039	0.111	Baliamoune-Lutz	0.060	0.037	0.083
Shukralla	0.072	0.036	0.108	Stoneman	0.062	0.039	0.086
Kosack & Tobin	0.070	0.034	0.105	Landau	0.062	0.039	0.085
Lensink & White	0.072	0.037	0.107	Burke & Ahmadi-Esfah	0.062	0.039	0.085
Roodman	0.073	0.038	0.107	Fayissa & El-Kaissy	0.063	0.040	0.086
Easterly, Levine & R	0.071	0.038	0.105	Papanek	0.066	0.043	0.090
Chauvet	0.069	0.036	0.102	Rajan & Subramanian	0.065	0.042	0.089
Fielding & Knowles	0.071	0.038	0.103	Reichel	0.066	0.042	0.089
Asiedu & Nandwa	0.069	0.037	0.102	Ali & Isse	0.065	0.042	0.088
Murphy & Tresp	0.068	0.036	0.100	Guillaumont & Chauve	0.066	0.042	0.089
Economides, Kalyviti	0.068	0.037	0.100	Feeny	0.066	0.043	0.089
Chauvet & Guillaumon	0.065	0.034	0.097	Singh	0.067	0.044	0.089
Fayissa & Nsiah	0.062	0.031	0.094	Snyder	0.067	0.044	0.090
Denkabe	0.061	0.031	0.092	Arndt, Jones & Tarp	0.068	0.045	0.090
Hansen & Tarp	0.062	0.032	0.092	Bowen	0.067	0.044	0.089
Loxley & Sackey	0.064	0.034	0.094	Larson	0.065	0.042	0.087
Lessman & Markwardt	0.062	0.032	0.092	Pavlov & Sugden	0.067	0.044	0.090
Ram	0.061	0.032	0.090	Mosley	0.066	0.043	0.089

Table A5. (Cont.) Cumulative random-effects meta-analysis of 141 studies: Assessing publication bias (studies sorted from most to least precise).

Study	Cumm. Est.	[95% Conf. Interval]		Study	Cumm. Est.	[95% Conf. Interval]	
		Lower	Upper			Lower	Upper
Lensink & Morrisey	0.066	0.044	0.089	Feeny	0.070	0.046	0.093
Rana & Dowling	0.067	0.044	0.090	Mosley et al	0.070	0.047	0.093
Mahdavi	0.068	0.045	0.090	Gupta & Islam	0.070	0.047	0.094
Campbell	0.068	0.045	0.091	Abidemi, Abidemi & O	0.072	0.049	0.095
Bhandari, Pradhan, D	0.068	0.046	0.091	Mosley, P., Hudson,	0.072	0.049	0.095
Ang	0.066	0.043	0.089	Gounder	0.072	0.049	0.096
Trevino, Len J. and	0.067	0.044	0.089	Murthy, Ukpolo & Mba	0.073	0.050	0.097
Gupta	0.068	0.045	0.091	Jayaraman & Ward	0.073	0.050	0.096
Kellman, Rottenberg	0.067	0.044	0.090	Sakyi	0.074	0.051	0.097
Kourtellos, Tan & Zh	0.065	0.042	0.088	Mavrotas	0.074	0.051	0.097
Dowling & Hiemenz	0.066	0.043	0.089	Giles	0.074	0.051	0.098
Brumm	0.065	0.043	0.088	Griffin and Enos	0.074	0.051	0.097
Gullati	0.066	0.043	0.089	Mbaku	0.073	0.050	0.096
Eris	0.066	0.043	0.089	Most & van den Berg	0.073	0.050	0.096
Muhammad & Qayyum	0.066	0.043	0.089	Islam	0.073	0.050	0.096
Obwona	0.069	0.046	0.092	Amavilah	0.074	0.051	0.097
Rao, Sharma and Sing	0.069	0.045	0.092	Gullati	0.074	0.051	0.097
Levy	0.070	0.047	0.093				

Source: Authors' computation

Table A6. Meta-regression analysis (dependent variable: Partial correlation).

	Partial		Partial
Gender	−0.004	Aid-Institutions Interaction	−0.061***
	(0.011)		(0.019)
Working paper	0.003	Aid-Policy Interaction	−0.036***
	(0.010)		(0.013)
Cato	−0.044	Aid square	0.029***
	(0.041)		(0.010)
JDS	0.018	Lag used	0.012
	(0.021)		(0.010)
JID	−0.011	System growth and aid	−0.033
	(0.017)		(0.021)
EDCC	−0.178***	System growth and capital	−0.037
	(0.034)		(0.030)
AER	−0.016	Capital	0.007
	(0.033)		(0.014)
Applied Economics	−0.053*	Human capital	0.028*
	(0.029)		(0.016)
Sub-sample	−0.047***	FDI	0.014
	(0.014)		(0.019)
Low income	0.037**	Policies	−0.032**
	(0.018)		(0.015)
World Bank	−0.011	Instability	−0.008
	(0.019)		(0.011)
Theory	0.027**	Inflation	−0.063***
	(0.011)		(0.015)
Gap model	0.041	Fiscal	0.036**
	(0.026)		(0.015)
Panel	0.093***	Size of government	0.056***
	(0.024)		(0.014)
No. countries	−0.001***	Region dummy	0.019*
	(0.000)		(0.010)
No. years	−0.001	Ethnic fractionalization	−0.049***
	(0.001)		(0.013)
Average	0.003**	Financial development	0.042***
	(0.001)		(0.011)
y1960s	−0.037**	Openness	0.014
	(0.014)		(0.012)
y1970s	0.026	Population	0.012
	(0.016)		(0.013)
y1980s	−0.057***	Per capita income	−0.020
	(0.020)		(0.013)
y1990s	−0.033*	OLS	−0.006
	(0.019)		(0.009)
y2000	−0.010	Africa	−0.011
	(0.011)		(0.021)
Outliers	−0.002	Constant	0.146***
	(0.011)		(0.043)
Single country	0.140***	Number of Obs.	1,761
	(0.036)	F-stat	9.2
EDA	−0.018	Between study variance	0.01
	(0.012)	Heterogeneity Measure (%)	0.72
Asia	−0.029	Adj R-squared	25.39
	(0.021)		
Latin	0.009		
	(0.021)		

Notes: standard errors in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Source: authors' estimates.

Table A7. List of original articles used in the meta-analysis.

Year	Authors	Title	Journal Title
1970	Gupta, K. L.	Foreign capital and domestic savings: A test of Haavelmo's hypothesis with cross-country data: A comment.	*Review of Economics and Statistics*
1970	Griffin, K. B., & Enos, J. L.	Foreign assistance: Objectives and consequences.	*Economic Development and Cultural Change*
1971	Kellman, M.	Foreign assistance: Objectives and consequences: Comments (to Griffin and Enos, 1970).	*Economic Development and Cultural Change*
1973	Papanek, G.F.	Aid, foreign private investment, savings, and growth in less developed countries.	*Journal of Political Economy*
1975	Stoneman, C.	Foreign capital and economic growth.	*World Development*
1976	Gulati, U. C.	Foreign aid, savings and growth: Some further evidence.	*Indian Economic Journal*
1978	Gulati, U. C.	Effects of capital imports on savings and growth in less developed countries.	*Economic Inquiry*
1980	Mosley, P.	Aid, savings and growth revisited.	*Bulletin of the Oxford University Institute of Economics and Statistics*
1983	Gupta, K. L., & Islam, M. A.	Foreign capital, savings and growth. An international cross-section study.	*Dordrecht, Reidel Publishing Company*
1983	Dowling Jr, J. M., & Hiemenz, U.	Aid, savings, and growth in the Asian region.	*Developing Economies*
1985	Singh, J. M.	State intervention, foreign economic aid, savings and growth in LDCs: Some recent evidence.	*Kyklos*
1986	Landau, D.	Government and economic growth in the less developed countries: An empirical study for 1960–1980.	*Economic Development and Cultural Change*
1987	Mosley, P., Hudson, J., & Horrell, S.	Aid, the public sector and the market in less developed countries.	*The Economic Journal*
1988	Levy, V.	Aid and growth in Sub-Saharan Africa: The recent experience.	*European Economic Review*
1988	Rana, P. B., & Dowling, J. M.	The impact of foreign capital on growth: Evidence from Asian developing countries.	*Developing Economies*
1990	Landau, D.	Public choice and economic aid.	*Economic Development and Cultural Change*
1990	Mahdavi, S.	The effects of foreign resource inflows on composition of aggregate expenditure in developing countries: A seemingly unrelated model.	*Kyklos*
1992	Islam, M. A.	Foreign aid and economic growth: An econometric study of Bangladesh.	*Applied Economics*
1992	Gyimah-Brempong, K.	Aid and economic growth in LDCs: Evidence from Sub-Saharan Africa.	*Review of Black Political Economy*
1992	Mosley, P., Hudson, J., & Horrell, S.	Aid, the public sector and the market in less developed countries: A return to the scene of the crime.	*Journal of International Development*
1993	Lensink, R.	Recipient government behavior and the effectiveness of development aid.	*De Economist*

Table A7. (Cont.) List of original articles used in the meta-analysis.

Year	Authors	Title	Journal Title
1993	Mbaku, J. M.	Foreign aid and economic growth in Cameroon.	*Applied Economics*
1994	Giles, J. A.	Another look at the evidence on foreign aid led economic growth.	*Applied Economics Letters*
1994	Murthy, V. N. R., Ukpolo, V., & Mbatu, J. M.	Foreign aid and economic growth in Cameroon: Evidence from cointegration tests.	*Applied Economics Letters*
1994	Boone, P.	The impact of foreign aid on savings and growth.	*WP London School of Econ.*
1995	Reichel, R.	Development aid, savings and growth in the 1980s: A cross-section analysis.	*Savings and Development*
1995	Hadjimichael, M. T., Ghura, D., Mühleisen, M., Nord, R., & Ucer, E. M.	Sub-Saharan Africa: Growth, savings, and investment, 1986–93.	*IMF Occasional Paper*
1995	Bowen, J. L.	Foreign aid and economic growth: An empirical analysis.	*Geographical Analysis*
1996	Most, S. J., & De Berg, H. V.	Growth in Africa: Does the source of investment financing matter?	*Applied Economics*
1996	Snyder, D. W.	Foreign aid and private investment in developing economies.	*Journal of International Development*
1998	Durbarry, R., Gemmell, N., & Greenaway, D.	New evidence on the impact of foreign aid on economic growth.	*Credit research paper*
1998	Amavilah, V. H.	German aid and trade versus Namibian GDP and labour productivity.	*Applied Economics*
1999	Campbell, R.	Foreign aid, domestic savings and economic growth: Some evidence from the ECCB area.	*Savings and Development*
1999	Svensson, J.	Aid, growth and democracy.	*Economics and Politics*
1999	Fayissa, B., & El-Kaissy, M.	Foreign aid and the economic growth of developing countries (LDCs): Further evidence.	*Studies in Comparative International Development Fall*
2000	Burnside, C., & Dollar, D.	Aid, policies and growth.	*American Economic Review*
2000	Lensink, R., & Morrisey, O.	Aid instability as a measure of uncertainty and the positive impact of aid on growth.	*Journal of Development Studies*
2000	Hansen, H., & Tarp, F.	Aid effectiveness disputed.	*Journal of International Development*
2001	Lu, S., & Ram, R.	Foreign Aid, government policies, and economic growth: Further evidence from cross-country panel data for 1970–1993.	*Economia Internazionale/ International Economics*
2001	Larson, J. D.	An updated analysis of Weisskopf's savings-dependency theory.	*Review of Development Economics*
2001	Gounder, R.	Aid-growth nexus: Empirical evidence from Fiji.	*Applied Economics*
2001	Obwona, M. B.	Determinants of FDI and their impact on economic growth in Uganda.	*African Development Review*
2001	Lensink, R., & White, H.	Are there negative returns to aid?	*Journal of Development Studies*
2001	Dalgaard, C. J., & Hansen, H.	On aid, growth and good policies.	*Journal of Development Studies*
2001	Guillaumont, P., & Chauvet, L.	Aid and performance: A reassessment.	*Journal of Development Studies*

Table A7. (Cont.) List of original articles used in the meta-analysis.

Year	Authors	Title	Journal Title
2001	Collier, P., & Dehn, J.	Aid, shocks, and growth.	*World Bank Policy Research*
2001	Hansen, H. & Tarp, F.	Aid and growth regressions.	*Journal of Development Economics*
2001	Tebouel, R., & Moustier, E.	Foreign aid and economic growth: The case of the countries south of the Mediterranean.	*Applied Economics Letters*
2001	Hudson, J., & Mosley, P.	Aid policies and growth: In search of the Holy Grail.	*Journal of International Development*
2002	Mavrotas, G.	Aid and growth in India: Some evidence from disaggregated aid data.	*South Asia Economic Journal*
2002	Gomanee, K., Girma, S., & Morrissey, O.	Aid, investment and growth in Sub-Saharan Africa.	*Paper prepared for the 10th General Conference of EADI*
2003	Dayton-Johnson, J., & Hoddinott, J.	Aid, policies and growth, redux.	*WP Dalhousie Univ*
2003	Moreira, S. B.	Evaluating the impact of foreign aid on economic growth: A cross-country study (1970–1998).	*WP for 15th Annual Meeting on Socio-Economics, Aix-en-Provence, France*
2003	Brumm, H. J.	Aid, policies and growth: Bauer was right.	*Cato Journal*
2003	Cungu, A., & Swinnen, J.	The impact of aid on economic growth in transition economies: An empirical study.	*LICOS Discussion Papers, Leuven*
2003	Ram, R.	Roles of bilateral and multilateral aid in economic growth of developing countries.	*Kyklos*
2003	Easterly, W.	Can foreign aid buy growth?	*Journal of Economic Perspectives*
2003	Cordella, T., & Dell'Ariccia, G.	Budget support versus project aid.	*IMF WP/03/88*
2003	Islam, M. A	Political regimes and the effect of foreign aid on economic growth.	*The Journal of Developing Areas*
2003	Trevino, L. J., & Upadhyaya, K. P.	Foreign aid, FDI and economic growth: Evidence from Asian countries.	*Transnational Corporations*
2003	Ovaska, T.	The failure of development aid.	*Cato Journal*
2003	Kosack, S.	Effective aid: How democracy allows development aid to improve the quality of life.	*World Development*
2004	Roodman, D.	An Index of Donor Performance.	*Center for Global Development Working Paper*
2004	Easterly, W., Levine, R., & Roodman, D.	Aid, policies, and growth: Comment.	*American Economic Review*
2004	Denkabe, P.	Policy, aid and growth: A threshold hypothesis.	*Journal of African Finance and Economic Development*
2004	Clemens, M., Radelet, S., & Bhavnani, R.	Counting chickens when they hatch: The short-term effect of aid on growth.	*Center for Global Development WP 44*
2004	Collier, P., & Hoeffler, A.	Aid, policy and growth in post-conflict societies.	*European Economic Review*
2004	Burnside, C., & Dollar, D.	Aid, policies and growth: Reply.	*American Economic Review*
2004	Ram, R.	Recipient country's "policies" and the effect of foreign aid on economic growth in developing countries: Additional evidence.	*Journal of International Development*
2004	Dalgaard, C. J., Hansen, H., & Tarp, F.	On the empirics of foreign aid and growth.	*Economic Journal*

Table A7. (Cont.) List of original articles used in the meta-analysis.

Year	Authors	Title	Journal Title
2004	Chauvet, L., & Guillaumont, P.	Aid and growth revisited: Policy, economic vulnerability and political instability. In B. Tungodden, N. Stern, I. Kolstad (Eds.), *Toward pro-poor policies—Aid, institutions and globalization* (pp. 95–109).	*World Bank/Oxford UP*
2004	Collier, P., & Dollar, D.	Development effectiveness: What have we learnt?	*Economic Journal*
2004	Shukralla, E. K.	Aid, incentives, policies, and growth: Theory and a new look at the empirics.	*WP Western Michigan Univ.*
2005	Ali, A. M., & Isse, H. S.	An empirical analysis of the effect of aid on growth.	*International Advances in Economic Research*
2005	Le. M. V., & Suruga, T.	Foreign direct investment, public expenditure and economic growth: The empirical evidence for the period 1970–2001.	*Applied Economics Letters*
2005	Hatami-J, A., & Irandoust, M.	Foreign aid and economic growth: New evidence from panel cointegration.	*Journal of Economic Development*
2005	Chauvet, L.	Can foreign aid dampen external political shocks?	*EPCS-2005*
2005	Feeny, S.	The impact of foreign aid on economic growth in Papua New Guinea.	*The Journal of Development Studies*
2006	Murphy, R. G., & Tresp, N. G.	Government policy and the effectiveness of foreign aid.	*WP 399. Economics Department, Boston College*
2006	Burke, P. J., & Ahmadi-Esfahani, F. Z.	Aid and growth: A study of South East Asia.	*Journal of Asian Economics*
2006	Djankov, S., Montalvo, J. G., & Reynal-Querol, M.	Does foreign aid help?	*Cato Jourral*
2006	Pavlov, V., & Sugden, C.	Aid and growth in the Pacific Islands.	*Asia-Pacific Economic Literature*
2006	Jayaraman, T. K., & Ward, B. D.	Economic growth in a vulnerable island nation: An empirical study of the aid-growth nexus in Vanuatu.	*Asia-Pacific Development Journal*
2006	Kosack, S., & Tobin, J.	Funding self-sustaining development: The role of aid, FDI and government in economic success.	*International Organizations*
2006	Antipin, J. E., & Mavrotas, G.	On the empirics of aid and growth. A fresh look.	*UN-WIDER WP 2006/05*
2006	Clark, W. R., Doces, J. A., & Woodberry, R. D.	Aid, protestant missionaries, and growth.	*Prepared for presentation at the University of Illinois at Urbana-Champaign*
2006	Jensen, P. S., & Paldam, M.	Can the two new aid-growth models be replicated?	*Public Choice*
2006	Karras, G.	Foreign aid and long-run economic growth: Empirical evidence for a panel of developing countries.	*Journal of International Development*
2007	Hudson, J., & Mosley, P.	Aid volatility, policy and development.	*Sheffield Economic Research Paper Series (SERP)*

Table A7. (Cont.) List of original articles used in the meta-analysis.

Year	Authors	Title	Journal Title
2007	Asiedu, E., & Nandwa, B.	On the impact of foreign aid in education on growth: How relevant is the heterogeneity of aid flows and the heterogeneity of aid recipients?	*Kiel Institute*
2007	Minoiu, C., & Reddy, S. G.	Aid does matter, after all. Revisiting the relationship between aid and growth.	*Challenge*
2007	Chatterjee, S., Giuliano, P., & Kaya, I.	Where has all the money gone? Foreign aid and the quest for growth.	*IZA DP*
2007	Bobba, M., & Powell, A.	Aid and growth: Politics matters.	*IDB Research Department WP*
2007	Herbertsson, T. T., & Paldam, M.	Does development aid help poor countries to converge to our standard of living?	*Danish Journal of Economics/ Nationaløkonomisk Tidsskrift*
2007	Dhakal, Rahman, & Upadhyaya	Foreign Direct Investment and Economic Growth in Asia.	*Indian Journal of Economics and Business*
2007	Miquel-Florensa, J. M.	Aid effectiveness: A comparison of tied and untied aid.	*WP 2007-2. Department of Economics*
2007	Elbadawi, I. A., Kaltani, L., & Schmidt-Hebbel, K.	Post-conflict aid, real exchange rate adjustment, and catch-up growth.	*Post-conflict transitions working paper*
2007	Rao, B. B., Sharma, M., & Singh, R.	Estimating aid-growth equations: The case of Pacific Island countries.	*MPRA paper no*
2007	Fielding, D., & Knowles, S.	Measuring aid effectively in tests of aid effectiveness.	*University of Otago, Economic WP*
2007	Pettersson, J.	Foreign sector aid fungibility, growth, and poverty reduction.	*Journal of International Development*
2007	Feeny, S.	Impacts of foreign aid to Melanesia.	*Journal of the Asia Pacific Economy*
2007	Economides, G., Kalyvitis, S., & Philippopoulos, A.	Does foreign aid distort incentives and hurt growth? Theory and evidence from 75 aid-recipient countries.	*Public Choice, W.P.*
2007	Kimura, H., Sawada, Y., & Mori, Y.	Aid proliferation and economic growth: A cross-country analysis.	*RIETI WP*
2007	Upadhyaya, K. P., Pradhal, G., Dhakal, D., & Bhandari, R.	Foreign aid, FDI and economic growth in East European countries.	*Economics Bulletin*
2007	Kourtellos, A., Tan, .M., & Zhang, X.	Is the relation between aid and economic growth nonlinear?	*Journal of Macroeconomics*
2008	Alvi, E., Mukherjee, D., & Shukralla, E. K.	Aid, policies, and growth in developing countries: A new look at the empirics.	*Southern Economic Journal*
2008	Alvi, E., Mukherjee, D., & Shukralla, E. K.	Foreign aid, growth, policy and reform.	*Economics Bulletin*
2008	Elbadawi, I. A., Kaltani, L., & Schmidt-Hebbel, K.	Foreign aid, the real exchange rate, and economic growth in the aftermath of civil wars.	*The World Bank Economic Review*
2008	Rajan, R. G., & Subramanian, A.	Aid and growth: What does the cross-country evidence really show?	*The Review of Economics and Statistics*
2008	Djankov, S., Montalvo, J. G., & Reynal-Querol, M.	The curse of aid.	*J Econ Growth*
2008	Eris, M.	Foreign aid and growth.	*Economics Bulletin*
2008	Loxley, J., & Sackey, H. A.	Aid Effectiveness in Africa.	*Journal compilation*

Table A7. (Cont.) List of original articles used in the meta-analysis.

Year	Authors	Title	Journal Title
2008	Fayissa, B., & Nsiah, C.	The impact of remittances on economic growth and development in Africa.	*WP Department of Economics and Finance. Middle Tennessee State University*
2008	Asteriou, D.	Foreign aid and economic growth: New evidence from a panel data approach for five South Asian countries.	*Journal of Policy Modeling*
2008	Tan, K. Y.	A pooled mean group analysis on aid and growth.	*Applied Economic Letters*
2008	Ouattara, B., & Strobl, E.	Aid, policy and growth: Does aid modality matter?	*Weltwirtschaftliches Archiv/Journal of World Economics*
2009	Annen, K., & Kosempel, S.	Foreign aid, donor fragmentation, and economic growth.	*The B.E. Journal of Macroeconomics*
2009	Chauvet, L., & Guillaumont, P.	Aid, volatility, and growth again: When aid volatility matters and when it does not.	*Review of Development Economics*
2009	Chervin, M., & van Wijnbergen, S.	Economic growth and volatility of foreign aid.	*Tinbergen Institute Discussion Paper*
2009	Neanidis, K. C., & Varvarigos, D.	The allocation of volatile aid and economic growth: Theory and evidence.	*European Journal of Political Economy*
2009	Bezuidenhout, H.	A regional perspective on aid and FDI in Southern Africa.	*North West University WP*
2009	Baliamoune-Lutz, M., & Mavrotas, G.	Aid effectiveness: Looking at the aid-social capital-growth nexus.	*Review of Development Economics*
2009	Baliamoune-Lutz, M.	Policy reform and aid effectiveness in Africa.	Icer working paper series
2010	Feeny, S., & McGillivray, M.	Aid and growth in small island developing states.	*The Journal of Development Studies*
2010	Brückner, M.	On the simultaneity problem in the aid and growth debate.	*Universitat Pompeu Fabra WP*
2010	Lessman, C., & Markwardt, G.	Decentralization and foreign aid effectiveness: Do aid modality and federal design matter in poverty alleviation?	*CESifo working paper Fiscal Policy, Macroeconomics and Growth, No. 3035*
2010	Kilby, C. ,& Dreher, A.	The impact of aid on growth revisited: Do donor motives matter?	*Economics Letters*
2010	Ndambendia, H., & Njoupouognigni, M.	Foreign aid, foreign direct investment and economic growth in Sub-Saharan Africa: Evidence from pooled mean group estimator (PMG).	*International Journal of Economics and Finance*
2010	Selaya, P., & Thiele, R.	Aid and sectoral growth: Evidence from panel data.	*The Journal of Development Studies*
2010	Ekanayake, E. M., & Chatrna, D.	The effect of foreign aid on economic growth in developing countries.	*Journal of International Business and Cultural Studies*
2010	Sakyi, D.	Trade openness, foreign aid and economic growth in post-liberalisation Ghana: An application of ARDL bounds test.	*Journal of Economics and International Finance*
2010	Ang, J. B.	Does foreign aid promote growth? Exploring the role of financial liberalization.	*Review of Development Economics*
2010	Arndt, C., Jones, S., & Tarp, F.	Aid, growth, and development: Have we come full circle?	*UNU-WIDER WP*

Table A7. (Cont.) List of original articles used in the meta-analysis.

Year	Authors	Title	Journal Title
2010	Salisu, A. A., & Ogwumike, F. O.	Aid-macroeconomic policy environment and growth: Evidence from Sub-Saharan Africa.	*Pakistan Journal of Applied Economics*
2010	Neanidis, K. C.	Humanitarian aid, fertility, and economic growth.	*CGBCR Discussion Paper Series*
2010	Bearce, D. H., & Tirone, D. C.	Foreign aid effectiveness and the strategic goals of donor governments.	*The Journal of Politics*
2011	Javid, M., & Qayyum, A.	Foreign aid and growth nexus in Pakistan: The role of macroeconomic policies.	*PIDE Working Papers*
2011	Bah, E. M., & Ward, J.	Effectiveness of foreign aid in small island developing states.	*Munich Personal RePEc Archive Paper*
2011	Bjerg, C., Bjornskov, C., & Holm, A.	Growth, debt burdens and alleviating effects of foreign aid in least developed countries.	*European Journal of Political Economy*
2011	Min, K., & Sanidas, E.	The impact of foreign aid's 7 functional categories on economic development in recipient countries.	*Korea and the World Economy*
2011	Abidemi, O. I., Abidemi, L., & Olawale, A. L.	Foreign aid, public expenditure and economic growth: The Nigerian case	*The Journal of Applied Business Research*

Article

Effects of Foreign Aid on Income through International Trade

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Abstract

This article presents a review of recent studies that estimate the trade effects of foreign aid. It also provides new results obtained using panel data techniques to estimate the direct effects of aid on international trade accounting for countries' participation in free trade agreements, and the indirect effects that aid exerts on income through trade. A structural gravity model of trade augmented with aid and free trade agreement variables is estimated for a cross-section of 33 donor countries and 125 recipient countries over the period 1995 to 2016. In a second step, the indirect effect of aid on income is estimated using a control function approach and instrumental variable techniques. The main results indicate that development aid has a robust direct effect on donor exports (the effect on recipient exports, however, is not robust). It also has an indirect positive effect on income levels in the recipient countries. The effects are heterogeneous and vary by region.

Keywords

bilateral aid; development aid; exports; foreign aid; free trade agreements; income; international trade

Issue

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1. Introduction

Rich countries have traditionally used foreign aid as a means of advancing their foreign policy aims in developing countries. Although each donor country has its own policies concerning aid distribution, donors tend to give bilateral aid to countries with which they have past or current colonial links, to countries that have the same official language and to those with which they have cultural and historical links (Nilsson, 1997). Political and economic interests have also influenced donors' aid policies and in many cases these strategic interests have been related to commercial aims (Arvin & Baum, 1997).

Over the years, the link between foreign aid and trade has generated significant academic interest and has been analyzed in a number of different contexts (Cadot, Fernandes, Gourdon, Matto, & de Melo, 2014). In general, the existing literature points towards a positive relationship between trade and foreign aid; this relationship is robust to various controls in the case of recipient imports, but not in the case of recipient exports (Martínez-Zarzoso, Nowak-Lehmann, & Klasen, 2014;

Nowak-Lehmann, Martínez-Zarzoso, Herzer, Klasen, & Cardozo, 2013; Wagner, 2003; Pettersson & Johansson, 2013; Silva & Nelson, 2012). Most related studies focus on foreign aid and its link with trade, but do not address the link with bilateral or regional trade policies; there is thus scope for investigating the latter association. Therefore, this article, after presenting a review of recent studies that estimate the trade effects of foreign aid, examines the extent to which aid policies help promote recipient countries' imports from and exports to donors, thereby contributing to the development process. Moreover, it examines whether bilateral aid and trade policies are complementary and explores the indirect effects that aid exerts on economic development through trade. This article makes two novel contributions with respect to the previous literature, which has mainly focused on the trade-aid nexus (Nowak-Lehmann, Martínez-Zarzoso, Herzer, Klasen, & Cardozo, 2013; Martínez-Zarzoso et al., 2014). First, in addition to the aid-trade link, it explores the interaction between trade agreements and bilateral aid. To that end, this article estimates a gravity model of trade using data for a 22-year period and for trade be-

tween developed and developing countries, augmented with the interaction between bilateral aid and free trade agreements (FTAs). Second, it estimates and discusses the effect of aid on developing countries' total exports, and presents estimates of the indirect effects that aid exerts on income through international trade.

The main results indicate that bilateral aid has a direct effect on donor exports and an indirect positive effect on the income levels in the recipient countries. With respect to FTAs, the results indicate that the direct effect of aid on donor exports is mainly observed for recipient countries that do not have an FTA with the donor.

The rest of the article is structured as follows. Section 2 presents a summary of the related literature. Section 3 presents the main empirical strategy used to evaluate the links between foreign aid and donor exports, recipient exports, FTAs and, in turn, recipient output. Section 4 discusses the main results and Section 5 concludes.

2. Literature Review

This section reviews the recent literature on the link between development aid, international trade and FTAs.

There are several channels through which foreign aid can foster exports from donors to recipients at the bilateral level: First, donors can use foreign aid as an 'opening-door policy' to establish or reinforce official relationships and to present the country as a trustworthy exporter. Second, when a donor gives aid for trade that is dedicated to infrastructure, to enhancing production capacity or to trade facilitation in general, these measures should reduce trade costs and hence boost exports. Third, under the premise that aid promotes trade, and trade influences income, aid can be seen as having an indirect effect on income. Tied aid has also been used to promote donor exports by linking the transfer to the purchase of goods and services from the donor (Arvin & Baum, 1997; Arvin & Choudhry, 1997). Finally, a long-term aid relationship can foster goodwill towards the donor, incentivizing firms in the recipient country to buy goods from the donor country (Arvin & Baum, 1997).

Recipient countries perceive aid as additional income that will eventually lead to an increase in demand and in imports (Temple & Van de Sijpe, 2017). For instance, development aid can be used to overcome financing constraints (Chenery & Strout, 1966). Aid transfers might also affect the recipient country's income in the medium to long term. In particular, private domestic savings could be substituted by external savings that come in the form of foreign aid (Doucouliagos & Paldam, 2006, 2008; Griffin & Enos, 1970; Griffin & Enos, 1970; White, 1992). Development aid could also be used by political leaders to substitute public revenue with external savings, in order to gain voter support (Crivelli & Gupta, 2017; Morrisey, 2001, 2005; White, 1992; among others).

Turning to the empirics, the gravity model of trade provides a suitable theoretical framework to evaluate the determinants of bilateral trade and, more specifically,

to evaluate the trade-aid relationship. First used to estimate the determinants of bilateral trade by Tinbergen (1962), this model holds that bilateral trade is directly proportional to the Gross Domestic Products (GDPs) of the trading countries and inversely proportional to the distance between them. The model has been widely used in the empirical trade literature to estimate the effect of a number of trade policies on bilateral trade. Starting with Anderson (1979), the theoretical literature has shown that gravity models can be derived from a range of trade theories (Anderson & van Wincoop, 2003; Bergstrand, 1985, 1989; Head & Mayer, 2014). This model is today considered a workhorse for empirical analysis of the international trade effects of policy measures, such as trade agreements, trade facilitation initiatives, tariff and non-tariff barriers reductions, etc. Head and Mayer (2014) summarize the recent literature and state that the estimation of theoretically-based gravity models requires the inclusion of proxies for the relative trade costs between a given country and its potential trading partners—the so-called multilateral resistance terms (MRT). The research that uses the gravity model to examine the effect of development aid on trade is summarized below. Other modeling frameworks have already been reviewed in Zarin-Nejadan, Monteiro and Noormamode, (2008, Table 3.1).

Jepma (1991), Arvin and Baum (1997) and Arvin and Choudhry (1997) analyzed the relationship between bilateral aid and bilateral exports, distinguishing between tied and untied aid, and found that both have a similar effect on promoting exports. In more recent years, there has been a gradual reduction in the tying of aid, partly due to pressure from the Development Assistance Committee of the OECD (OECD-DAC).

Nilsson (1997) was the first author to use the gravity model framework to investigate the relationship between bilateral aid and EU exports to developing countries. Estimating the traditional gravity model with data from 1975 to 1992, he showed that US$1 of aid increased EU exports by an average of US$2.60. Other authors have found similar effects for other countries (Pettersson & Johansson, 2013; Silva & Nelson, 2012; Wagner, 2003), while smaller effects have been found when applying panel data techniques and estimating a theoretically-based gravity model that accounts for MRT (Martínez-Zarzoso et al., 2014; Nowak-Lehmann et al., 2013; Silva & Nelson, 2012). Silva and Nelson (2012) used the *bonus vetus OLS* method proposed by Baier and Bergstrand (2009) to model multilateral resistance. Martínez-Zarzoso et al. (2014) investigated whether bilateral aid promoted bilateral exports to recipient countries during the period 1988–2007. The authors applied advanced panel data techniques considering time-variant MRT and endogeneity controls, and providing donor-specific export/aid elasticities. Overall, the findings showed a positive effect of bilateral aid on exports, which varied over time and across donors, and which depended on the extent to which donors tied aid to exports.

The effect appeared to have decreased substantially over the period of study and was no longer statistically significant by the 2000s, indicating that donors had responded to the OECD-DAC's recommendations concerning the untying of aid. Pettersson and Johansson (2013) used bilateral exports between 180 countries to investigate third-country effects and the effects of aid on recipient exports, but did not control for the endogeneity of the aid variable and for time-varying MRT.

Examples of single-donor studies are Martínez-Zarzoso, Nowak-Lehmann, Klasen and Larch (2009), Nowak-Lehmann, Martínez-Zarzoso, Klasen, and Herzer (2009) and Martínez-Zarzoso, Nowak-Lehmann, Klasen and Johannsen (2016) for Germany; Hansen and Rand (2014) for Denmark; Martínez-Zarzoso, Nowak-Lehmann and Klasen (2017) for the Netherlands; Zarin-Nejadan et al. (2008) for Switzerland; Otor (2017) for Japan; and Liu and Tang (2018) for China and the US. The main results obtained in those studies are summarized in Table A.1 in the Appendix, which is a more up-to-date and comprehensive version of Table 1 in Hansen and Rand (2014, p. 19).

A few of the abovementioned articles disaggregated exports in some way (Nowak-Lehmann et al., 2013; Martínez-Zarzoso, Nowak-Lehmann, & Klasen, 2017; Pettersson & Johansson, 2013). The findings indicated that the effects of aid on trade also differ by sector and seem to be more pronounced in sectors where the exporter has a comparative advantage.

Regarding the effect of aid on recipient exports, thus far only Pettersson and Johansson (2013) and Nowak-Lehmann et al. (2013) have investigated this effect. The first study found a positive and significant effect of aid on recipient exports, whereas the second found that the long-term impact of bilateral aid on recipient exports is not statistically significant. Pettersson and Johansson (2013) did not use bilateral fixed effects, which capture time-invariant pair heterogeneity, and reported that using bilateral fixed effects instead of country dummies yielded much weaker though still significant effects of aid.

The use of a full gravity model (Silva & Nelson, 2012) rather than just donor-recipient trade flows (Martínez-Zarzoso et al., 2014) to study the effects of bilateral aid on trade does not significantly change the results of the export/aid elasticity. Moreover, the results appear to be only slightly affected by not including zero trade or zero aid flows in the estimations. Notably, the way in which time-invariant unobserved heterogeneity is controlled for in the models seems to be the main source of differences in the results. In fact, the inclusion of trading-pair fixed effects (FE) to control for this type of endogeneity weakens the relationship between aid and recipient exports, but not the one between aid and donor exports.

In the last decade, more attention has been given to how aid can be used to promote exports from developing countries—the so-called 'aid for trade' principle (Morrisey, 2006). Aid for trade research has been at the forefront of the trade-and-aid literature since the mid-2000s, with most such studies using aid for trade data to investigate the effect of aid on recipient exports. Cadot et al. (2014) presents a summary of this growing literature, the main findings of which are mixed. For instance, this literature reports small effects of aid on trade for recipient countries that receive specific types of aid, mainly aid assigned to economic infrastructure or aid for building production capacity; moreover, these effects are found only for medium to large exporters (Martínez-Zarzoso, Nowak-Lehmann, & Rehwald, 2017).

The bilateral relationship between donor and recipient countries could also be used to promote FTAs. FTAs can reduce or eliminate artificial trade barriers between member countries, particularly tariffs and non-tariff barriers. Since the 1970s, most aid recipients have benefited from lower tariffs due to their Most Favored Nation status and their participation in the Generalized System of Preferences; however, these trade preferences are non-reciprocal and apply only to exports but not imports of capital goods. Moreover, there are other ways, besides the elimination of tariffs, in which being a signatory to a trade agreement can stimulate trade. FTAs and Customs Unions (CUs) are of particular interest in this regard, because they eliminate all tariff and non-tariff barriers between members and resolve uncertainty with respect to trade preferences. The difference between an FTA and a CU is that in the former members maintain their own trade policies with respect to third countries, whereas in the latter the members have a common external policy. For this reason, FTA member exporters must comply with the rules of origin for goods that originate in third countries and are in turn traded within the area.

Some donors have common external policies that simultaneously incorporate bilateral trade and aid policies, and treat them as complementary. In some cases, donors give aid to countries with which they have weak trade links, with the aim of establishing closer relations. The nexus between giving aid and forming FTAs has only been investigated in specific contexts, namely in the aid for trade literature (Vijil, 2014) and in research on trade flows between EU and North African countries (Martínez-Zarzoso, Nowak-Lehmann, & Johannsen, 2012). Vijil (2014) found complementarities between aid for trade and regional economic integration, while Martínez-Zarzoso et al. (2012) found that both aid and FTA/CU agreements promote trade in North African countries, and that the two measures complement each other.

In this article, we extend this literature by focusing on North-South bilateral aid and regional trade agreements (RTAs), including FTAs and CUs, to investigate whether the complementarities found in previous literature are more generally applicable.

Finally, it should be noted that the body of research on the effect of trade and foreign aid on economic growth and economic development is very large, and a comprehensive review of the entire literature is beyond the scope of this article. Therefore, the main arguments are outlined here and a number of highly influential ar-

ticles are highlighted. This literature has followed two parallel paths. On the one hand, authors that have focused on the effect of openness on economic growth have tended not to include foreign aid in the growth regressions (Alcalá & Ciccone, 2004; Dollar & Kraay, 2003; Frankel & Romer, 1999; Singh, 2010, for a review, among others). On the other hand, a number of articles investigating the effect of foreign aid on economic growth have included openness as a control and in many cases as part of an index that included several policy variables (Burnside & Dollar, 2000; Collier & Dollar, 2001; Dalgaard & Hansen, 2000). We refer readers to Addison, Morrisey and Tarp (2017) for an overview of the macroeconomics of aid, in which they describe five generations of aid research and the main controversies surrounding the aid-growth debate. Starting in the late 1990s and early 2000s, the aid and growth literature mostly focused on analyzing whether aid was effective only when accompanied by a number of "good" economic policies in the recipient countries—the so-called conditionality argument. After the seminal article by Burnside and Dollar (2000), many scholars focused on validating the findings of that research, obtaining mixed evidence at best, as summarized in McGillivray, Feeny and Hermes and Lensink (2006, Table A.2). In the 2010s, research showed that despite the shortcomings and complexities involved in the development aid process, foreign aid has been effective when an extended time frame is considered (Arndt, Jones, & Tarp, 2010, 2015, 2016).

For a more in-depth discussion of the aid-growth debate in recent decades, we refer readers to Hansen and Tarp (2000, 2001) and to the literature reviews in Dalgaard, Hansen and Tarp (2004), Doucouliagos and Paldam (2008, 2015), Edwards (2005); Rajan and Subramanian (2008) and Arndt et al. (2015, 2016).

3. Empirical Strategy

This section describes the data, sources and variables and presents the main results concerning the bilateral trade and aid link, the complementarity of aid and trade policies, as well as the links between aid and total exports from recipients to donors and between aid and recipients' income level.

3.1. Data, Sources and Variables

The data and variables used cover the period 1995 to 2016 for a cross-section of 33 donors and 125 recipients (see Table A.2 for a list of variables and sources and Table A.3 for a list of countries). Official Development Assistance (ODA) data are from the OECD[1]. We consider net ODA disbursements, in current USD, because we are interested in the funds that were actually dis-

bursed to the recipient countries in a given year. Disbursements record the actual international transfer of financial resources, or the transfer of goods or services, valued at the cost to the donor. Aid commitments are also used as proxies for the willingness to give aid. Bilateral exports are obtained from the UN-COMTRADE database[2]. Data on income and population variables are drawn from The World Bank (World Development Indicators Database, WDI-2018). Gravity variables such as distance between capital cities, common language, colonial relationship and common border are from the Centre d'Études Prospectives et d'Informations Internationales (CEPII). The variable RTA and currency unions are constructed from De Sousa (2012) and updated using data from the World Trade Organization (WTO) and Central Banks.

The additional variables used in the aggregate exports and income models—namely, population, consumer price index, gross capital formation, foreign direct investment and remittances—are also from the WDI-2018. Summary statistics of the main variables are presented in Table 1.

3.2. Model Specification

The main modeling framework is the gravity model of trade, and in this context we use a control function approach to investigate the effect of aid on donor and recipient exports. This approach shares some features with the standard approaches based on Instrumental Variables (IV), which are also used as robustness checks. Most of the panel data applications we reviewed used models that are linear in the parameters (log-linearized version of the gravity model) and were estimated using IV methods with two-stage least squares (2SLS), Generalized Methods of Moments (GMM) or dynamic Ordinary Least Squares (OLS) to account for the endogeneity of the aid variable. The control function approach is an alternative proposed by Wooldridge (2010), which relies on similar identification conditions to the IV approach. The main advantage of the control function approach is that, unlike IV methods, it can be used in combination with the most recent techniques proposed to estimate gravity models of trade with panel data, which require the inclusion of three sets of multidimensional fixed effects (Correia, 2017).

In our specification, exports from country i to country j at year t in natural logs (lx_{ijt}) is the response variable, bilateral aid in natural logs from country i to country j ($laid_{ijt}$) is the endogenous explanatory variable and Z is the $1 \times L$ vector of exogenous variables (Z_1 is a $1 \times L_1$ strict sub-vector of Z). This can be specified as:

$$lx_{ijt} = Z_1\delta_1 + \alpha_1 laid_{ijt} + u_{ijt}, \qquad (1)$$

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[1] The countries selected are all those for which the OECD-DAC reports data on ODA, and which have been giving aid over the analyzed period. All recipient countries in the sample engage in bilateral trade with the donors although there are 3,815 non-reported data on exports, which could be potential zero trade flows. Those represent only 10 percent of the observations used in the regressions.

[2] UN-COMTRADE has incomplete data for 2017 as some countries report with a lag of 2 years. For this reason our sample ends in 2016.

Table 1. Summary statistics.

Variable	Obs	Mean	Std. Dev.	Min	Max
Recipient Exports*:					
Lexp	35,710	9.143	3.547	−5.521	19.517
Laid	35,710	0.717	2.492	−4.605	9.326
Laidcom	35,710	0.654	2.603	−4.605	9.186
Lgdp_don	33,849	27.328	1.397	23.376	30.523
Lgdp_rec	32,947	23.381	1.888	16.395	28.592
WTO	35,710	0.7541	0.4306	0	1
Comcur	35,710	0.0021	0.0458	0	1
Ldist	35,710	8.7478	0.6231	4.710	9.846
Landlock	35,710	0.4112	0.5659	0	2
Lang	35,710	0.1642	0.3704	0	1
Comcol	35,710	0.0075	0.0861	0	1
Border	35,710	0.0032	0.0567	0	1
Smctry	35,710	0.0016	0.0399	0	1
RTA	35,710	0.116	0.320	0	1
RTA_Europe	35,710	0.096	0.294	0	1
RTA_Asia	35,710	0.004	0.059	0	1
RTA_Africa	35,710	0.002	0.047	0	1
RTA_America	35,710	0.010	0.098	0	1
RTA_Pacific	35,710	0.001	0.030	0	1
Donor Exports**:					
Lexp	37,356	10.051	2.670	−5.809	19.093
Laid	37,356	0.621	2.519	−4.605	9.326
Laidcom	37,314	0.556	2.611	−4.605	9.186
Lgdp_don	35,457	27.308	1.405	23.376	30.523
Lgdp_rec	34,590	23.341	1.881	16.395	28.592
WTO	37,356	0.770	0.421	0	1
Comcur	37,356	0.002	0.045	0	1
Ldist	37,356	8.754	0.617	4.7104	9.850
Landlock	37,356	0.398	0.561	0	2
Lang	37,356	0.168	0.374	0	1
Comcol	37,356	0.008	0.088	0	1
Border	37,356	0.003	0.056	0	1
Smctry	37,356	0.002	0.039	0	1
RTA	37,356	0.114	0.318	0	1
RTA_Europe	37,356	0.091	0.288	0	1
RTA_Asia	37,356	0.003	0.058	0	1
RTA_Africa	37,356	0.002	0.047	0	1
RTA_America	37,356	0.013	0.112	0	1
RTA_Pacific	37,356	0.001	0.030	0	1

Notes: * Dataset used in Tables 2 and A.5 and first part of Tables A.7 and A.8. ** Dataset used in Tables 3 and A.6 and second part of Tables A.7 and A.8. L denotes natural logs.

where *aid* denotes net bilateral official development aid (disbursements). The Z_1 variables are the natural logs of GDPs for the donor and recipient countries as well as the standard gravity variables; namely, distance between trading countries and dummy variables for common language, past or current colonial relationship and RTA (we omit subscripts for simplicity). In the preferred panel data specification, the effect of the bilateral time-invariant gravity variables will be subsumed in the dyadic fixed effects and the effect of GDPs on the time-variant MRT.

First, consider the exogeneity assumption:

$$E(Z_1' u_{ijt}) = 0. \tag{2}$$

The reduced form for *laid* is:

$$laid_{ijt} = Z\pi_2 + \varepsilon_{ijt}, \tag{3}$$

where Z includes (in addition to the exogenous variables in Z_1) aid commitments (aid commitments were lagged two periods to avoid endogeneity concerns), and country-specific fixed effects as exclusion variables.

The linear projection of u_{ijt} on ε_{ijt} is:

$$u_{ijt} = \rho_2 \varepsilon_{ijt} + e_{ijt}. \qquad (4)$$

Now plugging (4) into (1), we obtain:

$$lx_{ijt} = Z_1 \delta_1 + \alpha_1 laid_{ijt} + \rho_2 \varepsilon_{ijt} + e_{ijt}. \qquad (5)$$

The two-step procedure consists of first regressing bilateral aid on all the exogenous variables to obtain the reduced form residuals $\hat{\varepsilon}_{ijt}$, and then regressing exports on a subset of the exogenous variables, bilateral aid and $\hat{\varepsilon}_{ijt}$. We use the same two-step procedure for recipient exports and for donor exports (recipient imports).

The OLS estimate from the second step in (5) is a *control function* estimate and gives consistent estimates. A simple test for the null of exogeneity is a t-statistic on the statistical significance of $\hat{\varepsilon}_{ijt}$.

We combine this control function approach with the use of panel data and three sets of fixed effects. These are bilateral fixed effects that control for the unobservable heterogeneity attached to each trade flow (*ij*) and donor-and-time (*it*) and recipient-and-time (*jt*) fixed effects as controls for MRT, which have to be considered when estimating theoretically-based gravity models using panel data.

We use a first-step reduced form regression with aid as the response variable (based on Equation 3). The reduced form is a bilateral aid equation estimated with country fixed effects. For aid flows, the donor dummies reflect, in part, the effect of common aid policies that govern the way in which aid is distributed, while the recipient dummies are proxies for the political and institutional environment in the recipient countries.

Reduced form estimations are presented separately for donor and recipient exports. Since we are also interested in the effect of trade policies in combination with aid policies, we add a number of RTA dummies and the interaction between RTA variables and aid to the empirical specification (based on equation 5). For instance, we show estimates for RTA agreements signed between recipient countries (developing countries) and donors in the following regions: Asia, America (North and South America), Africa, Europe and Pacific. The inclusion of interaction terms between the RTA dummies and development aid will allow us to investigate the extent to which trade and aid policies are complementary.

The related literature has recognized the importance of evaluating the effects of foreign aid on the trade and economic growth of the recipient countries (Doucouliagos & Paldam, 2006, 2015). One of the main issues in such an analysis is the endogeneity of aid in the trade and growth equations. We tackle this issue as follows: First, we use the results from the estimations of bilateral exports (1) from recipient to donor countries (donor to recipient countries, see Frankel and Romer, 1999) and bilateral aid (3) to obtain the corresponding residuals. Then, we take the exponential of the residuals and aggregate them over all donors to obtain an estimate

for each recipient and time period:

$$resx_{jt} = \sum_i Exp(\hat{u}_{ijt}). \qquad (6)$$

$$resaid_{jt} = \sum_i Exp(\hat{\varepsilon}_{ijt}). \qquad (7)$$

Finally, these residuals in natural logs are used in a second-step estimation in which the dependent variables are the natural logs of total recipient exports, *lxrec*, and the natural log of recipient GDP per capita, *lgdppc*. The corresponding specifications are given by equations (7) and (8):

$$lxrec_{jt} = \delta_j + \alpha_1 laid_{jt} + \alpha_2 lgdppc_{jt} + \alpha_3 CPI_{jt}$$
$$+ \alpha_4 lxdon_{jt} + \rho_1 lresaid_{jt} + \rho_2 lresxd_{jt} \qquad (8)$$
$$+ \theta_t + e_{jt}.$$

$$lgdppc_{jt} = \delta_j + \alpha_1 laid_{jt} + \alpha_2 lpop_{jt} + \alpha_3 lxrec_{ht}$$
$$+ \alpha_4 lxdon_{jt} + \rho_1 lresaid_{jt} + \rho_2 lresxd_{jt} \qquad (9)$$
$$+ \rho_3 lresxr_{jt} + \theta_j + e_{jt}.$$

where *l* denotes natural logs, *aid* is bilateral aid, *CPI* denotes the consumer price index, *xdon* denotes donors' exports and *xrec* recipients' exports, θ_t denotes time fixed effects and δ_j denote country fixed effects. *Lresxr* and *lresxd* refer to the log of the aggregated exponential residuals from the corresponding gravity models for recipients' exports and donors' exports according to (6).

The models are also estimated with IV using the second and third lag of aid commitments as instruments. Moreover, dynamic models that include the lagged dependent variables are also estimated as a robustness check.

4. Main Results

A gravity model of trade with bilateral fixed effects and MRT is used to estimate the effects of bilateral aid on donor exports and recipient exports. The bilateral fixed effects control for unobservable country-pair heterogeneity as a source of the endogeneity of the aid variable, and the MRT allow us to estimate a theoretically-based structural gravity model, as described in the previous section. The results using this approach are shown in Table 2 for recipient exports and in Table 3 for donor exports.

Regarding the target variable, bilateral aid, results indicates that it has a positive but small significant effect on recipient exports to donors (Table 2) and on donor exports to recipients (Table 3). This is also the case when aid is taken as endogenous in columns (4)–(6). The point estimate is 0.022 (Table 2, column 4) for recipient exports and 0.026 (Table 3, column 4) for donor exports, indicating that the effect is stronger for the latter. The estimates for donor exports are similar to those obtained in Nowak-Lehmann et al. (2013) for the period 1988 to 2007 using Dynamic FGLS without MRT, but with leads and lags of the variables in first differences. Similar estimates were

Table 2. Gravity results for recipient exports.

Dep. Variable: Ln Rec. Exports	(1) HDFE	(2) HDFE	(3) HDFE	(4) CF	(5) CF	(6) CF
Indep. Variables:						
Laid	0.0164** [0.00717]	0.0187** [0.00748]	0.0193*** [0.00750]	0.0224** [0.00910]	0.0245*** [0.00908]	0.0252*** [0.00969]
RTA	0.140*** [0.0390]	0.161*** [0.0426]		0.142*** [0.0495]	0.163*** [0.0457]	
Laid*RTA		−0.0188** [0.00880]			−0.0185** [0.00922]	
RTA_Europe			0.113** [0.0495]			0.114** [0.05411]
RTA_Asia			0.236** [0.106]			0.238** [0.111]
RTA_Africa			0.571* [0.309]			0.602* [0.3337]
RTA_America			0.135 [0.117]			0.134 [0.123]
RTA_Pacific			−0.235 [0.258]			−0.225 [0.322]
Laid* Europe			−0.0274*** [0.00955]			−0.0270*** [0.0101]
Laid*Asia			−0.0516** [0.0235]			−0.0514** [0.0253]
Laid*Africa			−0.0199 [0.110]			−0.0403 [0.123]
Laid*America			0.0640* [0.0335]			0.0647* [0.0365]
Laid*Pacific			−0.0665 [0.199]			−0.0569 [0.269]
Residuals from Bilateral Aid Equation				−0.0157 [0.0112]	−0.0153 [0.0114]	−0.0156 [0.0117]
BFE, XT, MT	Yes	Yes	Yes	Yes	Yes	Yes
Observations	35,748	35,748	35,748	37,710	35,710	35,710
R-squared	0.914	0.914	0.914	0.917	0.914	0.914

Notes: Robust standard errors in brackets clustered by donor-recipient (default). Method: High-dimensional fixed effects (HDFE) linear regression. Fixed effects include: donor-year (XT), recipient-year (MT), donor-recipient (BFE). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. CF denotes Control Function Approach. All models estimated with the Stata command *reghdfe* from Correia (2017). Bootstrapped standard errors in columns 3–6 (1000 replications).

also obtained for donor exports using GMM in a dynamic setting (as shown in Martínez-Zarzoso et al., 2014). In contrast to Nowak-Lehmann et al. (2013), we also obtain statistically significant coefficients for recipient exports. However, only the results concerning donor exports are robust when a PPML (Poisson pseudo-maximum likelihood) estimator—a technique that tackles several econometric issues, including zero flows, selection bias and heteroskedasticity—is used (see the results in Tables A.5 and A.6). In the case of recipient exports, bilateral aid turns out to be non-statistically significant when PPML is used (Table A.5), in line with previous literature.

We add to the model the average effect of RTAs and its interaction with bilateral aid in column (2) and the effects of specific trade agreements and their interactions with bilateral aid in column (3) of Tables 2 and 3. The results show that the interaction between the RTA variable and bilateral aid is negative and statistically significant, indicating that the positive effect found for the aid variable vanishes for countries that have common RTAs. In particular, the partial effect of aid on recipient exports when RTA = 1, calculated using the results in column (2) of Tables (2) and (3)[3], is not statistically significant. Therefore aid is only statistically significant on average when

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[3] The marginal effect of aid on trade has been calculated using a test of joint statistical significance (*lincom* in Stata).

Table 3. Gravity results for donor exports.

Dep. Variable: Ln donor exports	(1) HDFE	(2) HDFE	(3) HDFE	(4) CF	(5) CF	(6) CF
Indep. Variables:						
Laid	0.0297*** [0.00396]	0.0338*** [0.00413]	0.0344*** [0.00414]	0.0259*** [0.00488]	0.0296*** [0.00499]	0.0302*** [0.00501]
RTA	0.173*** [0.0242]	0.204*** [0.0257]		0.176*** [0.0242]	0.207*** [0.0258]	
Laid*RTA		−0.0329*** [0.00493]			−0.0335*** [0.00494]	
RTA_Europe			0.218*** [0.0307]			0.222*** [0.0307]
RTA_Asia			0.0801 [0.0797]			0.0803 [0.0797]
RTA_Africa			0.244 [0.183]			0.248 [0.186]
RTA_America			0.121*** [0.0453]			0.121*** [0.0453]
RTA_Pacific			−0.903*** [0.242]			−0.905*** [0.241]
Laid*Europe			−0.0409*** [0.00551]			−0.0414*** [0.00551]
Laid*Asia			0.0271 [0.0190]			0.0272 [0.0190]
Laid*Africa			0.0307 [0.0825]			0.0276 [0.0844]
Laid*America			−0.0276** [0.0118]			−0.0283** [0.0118]
Laid*Pacific			−0.695*** [0.188]			−0.699*** [0.188]
Residuals from Bilateral Aid Equation				0.0101* [0.00610]	0.0110* [0.00610]	0.0111* [0.00610]
Observations	37,356	37,356	37,356	37,314	37,314	37,314
R-squared	0.952	0.952	0.952	0.952	0.952	0.952

Notes: Robust standard errors in brackets clustered by donor-recipient (default). Method: High-dimensional fixed effects (HDFE) linear regression. Fixed effects include: donor-year, recipient-year, donor-recipient. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. CF denotes Control Function Approach. All models estimated with the Stata command *reghdfe* from Correia (2017). Bootstrapped standard errors in columns 3–6 (1000 replications).

there are no RTAs between the donor and the recipient country. The estimated coefficient for pairs of countries without RTAs indicates that a 10 percent increase in bilateral aid raises recipient exports by about 0.24 percent (column 5, Table 2), whereas for donor exports the corresponding effect is around 0.3 percent (column 5, Table 3). Moreover, the coefficient estimated for the RTA variable indicates that RTAs increase exports by around 17 percent[4] for recipients and by 22 percent for donors for country pairs without aid link. These effects decrease with the amount of aid given, as indicated by the negative effect of the interaction variable (Laid * RTA).

Results in column (3) of both tables show that the effect is heterogeneous and varies by agreement. For instance, the bilateral RTAs signed mostly between the EU and EFTA (Europe) and recipient countries have a positive and significant effect on recipient exports—and also on donor exports—but this effect decreases with the amount of aid given. This is also the case for RTAs in Asia (see Table A.4 for a list of agreements included). In terms of the RTAs signed by American countries, they seem to exert a statistically significant effect on donor exports only, and this effect also decreases with the amount of aid given. For the agreement involving the Pacific region (Australia–Singapore and TPP: Trans-Pacific Partnership Agreement), no significant effect of the RTAs on recipient exports is found, whereas the effect is negative and significant for donor exports.

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[4] The effect is calculated as Exp(0.163 − 1)*100 using the coefficient of the RTA variable in column (5) of Table 2.

Next, we estimate the effect of aid on aggregate recipient exports and on income per capita in the recipient countries by using the control function approach and alternative IV methods. The main results are presented in Tables 4 (for exports) and 5 (for income). Column (1) shows the FE results and column (2) the results of the control function approach, when the aggregated residuals from the first step estimations of bilateral aid and bilateral exports are added as regressors. In columns (3)–(4), the models are estimated using IV for aid, while column (5) presents the results of a dynamic model that uses IV for aid and for the lagged dependent variable.

The results in Table 4 indicate that greater amounts of aid received and more imports from all donors lead to an increase in recipient exports, given that significant and positive effects are shown for foreign aid and for donors' exports. In particular, a 10 percent increase in ODA raises recipient exports by around 0.6 percent when using the control function approach, and the point estimate increases to 1.6 when using IV; however, the effect is statistically significant only at the 10 percent level. In addition, for each 10 percent increase in donor exports, recipient exports increase by around 2.6 percent (column 2, Table 4). These results are robust to the addition of control variables (column 4) and the lagged dependent variable (column 5) to the model. The long-run effects in

column (5) can be calculated by dividing the point coefficients by (1–0.77), with 0.77 being the coefficient of the lagged dependent variable. Tests for the validity of the instruments are included in the last two rows of Table 4. The Hansen test indicates that we cannot reject the validity of the instruments and the Kleibergen-Paap statistic indicates that the instruments are not weak.

Column (1) in Table 5 shows the effect of trade on the recipient's income per capita. A one percent increase in exports from recipients to donors raises the income per capita in the recipient country by around 0.12 percent. Moreover, the same increase in donors' exports increases that income level by around 0.2 percent. As in other studies (Nowak-Lehmann et al., 2013), the aid coefficient is not statistically significant in Table 5. However, aid is found to exert an indirect effect on income through trade, given that aggregate aid and imports from the donors are associated with higher recipient exports (Table 4), and higher recipient exports have a positive effect on income (Table 5). The estimated coefficients in columns (1) and (2) of Table 5 are robust to changes in the specification and to the addition to a number of control variables. In particular, column (3) presents the results when population, foreign direct investment, remittances, and gross capital formation are added to the model; the main difference is the reduction in the coeffi-

Table 4. Regression results for aggregate recipient exports.

Dep. Variable: ln Rec. Exports of Goods and Services	(1) CTFE	(2) CTFE-CF	(3) CTFE-IV	(4) CTFE-IV	(5) IV-Dyn
Indep. Variables:					
Lgdppc	1.049***	1.041***	1.115***	1.113***	0.218**
	[0.118]	[0.121]	[0.130]	[0.138]	[0.0877]
Laid_Total	0.0652**	0.0693**	0.157*	0.156*	0.0458**
	[0.0319]	[0.0327]	[0.0807]	[0.0860]	[0.0219]
Lxdon_Total	0.246***	0.257***	0.191***	0.198***	0.0414*
	[0.0807]	[0.0826]	[0.0698]	[0.0707]	[0.0238]
CPI		−8.73e-06***		−8.56e-06***	−6.56e-06***
		[2.32e-06]		[2.58e-06]	[9.85e-07]
Lresaid_s	−0.00101	0.00187			
	[0.0175]	[0.0166]			
Lresxd_s	−0.0254	−0.0248			
	[0.0372]	[0.0354]			
Lxrec (T-1)					0.771***
					[0.0432]
Observations	1,785	1,666	1,762	1,646	1,388
R-squared	0.693	0.696	0.683	0.687	0.911
Number of countries	115	108	100	96	93
Hansen st. (prob.)			0.448	0.430	0.545
Kleibergen-Paap st.			18.34	15.71	18.34

Notes: Robust standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. CFE denotes country fixed effects, CTFE denotes country and time fixed effects, CF Control Function Approach and IV instrumental variables. The definition of the variables can be found in Table A2 and lresaid_s and lresxd_s are obtained from the estimated residuals of the aid and the donors' exports models, respectively. Hansen st. (prob.) is the probability associated to the Hansen test, which indicate that we cannot reject the validity of the instruments. Kleibergen-Paap statistic is a test for weak instruments, which result indicates that the instruments are not weak.

Table 5. Regression results for recipient income per capita.

Dep. Variable: lgdppc	(1) CTFE	(2) CTFE-CF	(3) CTFE_CF	(4) CTFE-IV	(5) CTFE-IV	(6) IV-Dyn
Indep. Variables:						
Laid_Total	−0.0165	−0.0145	−0.00436	0.00130	0.0261	0.00646
	[0.0144]	[0.0156]	[0.0106]	[0.0323]	[0.0218]	[0.00694]
Lxrec_Total	0.121***	0.119***	0.0939***	0.121***	0.108***	0.0285***
	[0.0291]	[0.0289]	[0.0331]	[0.0301]	[0.0333]	[0.00491]
Lxdon_Total	0.208***	0.207***	0.0855***	0.223***	0.0782***	0.0176*
	[0.0422]	[0.0424]	[0.0286]	[0.0362]	[0.0288]	[0.0106]
Lpop			−0.911***		−0.960***	−0.245***
			[0.129]		[0.142]	[0.0293]
Lfdi			0.00168		0.00210	
			[0.00469]		[0.00449]	
Lrem			0.00183		0.00223	
			[0.00876]		[0.00872]	
Lgcf			0.110***		0.120***	0.0372***
			[0.0283]		[0.0274]	[0.00811]
Lresaid_s		0.00485	0.00879			
		[0.0105]	[0.00599]			
Lresxr_s		−0.0135	0.000895			
		[0.0106]	[0.00662]			
Lresxd_s		−0.0452**	−0.0239			
		[0.0196]	[0.0144]			
Lgdppc(T-1)						0.799***
						[0.0168]
Observations	2,248	2,241	1,447	2,235	1,438	1,697
R-squared	0.667	0.670	0.843	0.663	0.836	0.961
Number of countries	126	121	100	122	96	110
Hansen st. (prob.)				0.283	0.0239	0.793
Kleibergen-Paap st.				19.39	12.54	20.59

Notes: Robust standard errors in brackets. *** p < 0.01, ** p < 0.05, * p < 0.1. CFE denotes country fixed effects, CTFE denotes country and time fixed effects, CF Control Function Approach and IV instrumental variables. The definition of the variables can be found in Table A2. lresaid_s, lresxr_s and lresxd_s are obtained from the estimated residuals of the aid, the recipients' exports and the donors' exports models, respectively. Hansen st. (prob.) is the probability associated to the Hansen test, which indicate that we cannot reject the validity of the instruments. Kleibergen-Paap statistic is a test for weak instruments, which result indicates that the instruments are not weak.

cient of donor exports, which is in part due to the smaller sample of countries for which data are available (121 in column 2 compared to 100 in column 3). In columns (4) and (5), the aid variable is instrumented with the first and second lag of aggregate aid commitments and the results remain similar to those in columns (2) and (3) using the control function approach. Finally, in column (5), the lagged dependent variable of income per capita is added to the model to incorporate dynamics. The coefficient of the lagged income variable is positive and significant, as expected, and the results for recipient exports and donor exports remain positive and significant: the long-run effects are 0.14 and 0.08 for each one percent increase in recipient and donor exports, respectively. As in Table 4, the last two rows of Table 5 include tests for the validity of the instruments and for weak instruments.

The control function approach allows us to test for the endogeneity of aid and trade variables in the recipient exports and income equations estimated in Tables 4 and 5.

The corresponding t-tests on the residuals from the first-step equations (for exports and for development aid) indicate that the coefficients of the residuals are generally not statistically significant when the model is estimated with country and time fixed effects, suggesting that the use of panel data mitigates potential endogeneity.

5. Robustness Checks

As a first robustness check, we have estimated the gravity models for recipient exports and imports using the usual gravity controls; namely, income in the trading countries and dummy variables for common language, common border, colonial relationship and belonging to the same country in the past. The results are shown in Table A.7. In general, the aid coefficient is positive and significant, and higher in magnitude than in the main results. This is as expected since the models in Table A.7 do not control for time-variant MRT, nor for all the bilateral unobserved

heterogeneity in the gravity model.

We have also run separate models for different regions. Using the World Bank classification, we divide the world into regions as indicated in Table A.8. The results for recipient exports shown in column (1) indicate that it is mainly aid sent to the Latin American and Caribbean region and to South Asian countries that has been effective in increasing recipient exports. Concerning donor exports, the results are shown in column (2) and indicate that aid to East Asia & Pacific, to Europe & Central Asia, to South Asia, and to Sub-Saharan Africa increase exports, whereas the aid coefficient for Latin America & Caribbean and Middle East & North Africa is not statistically significant.

The income per capita model was also estimated in first differences with IV to avoid potential issues with spurious correlations, and the results hold (see Table A.9). Finally, the model was also estimated for several lags of the aggregate aid variable and the results indicate that the aid was statistically significant in the income model when using the fifth lag as regressor and the sixth-to-tenth lags as instruments. This is in line with recent reviews of the aid-growth literature (Table A.10).

6. Conclusion

This article reviews the recent literature on the bilateral trade and aid link that uses the gravity model as the main analytical framework. Existing studies find a robust positive effect of bilateral aid on bilateral exports from donor to recipient countries. The findings also indicate that there is a small but non-robust effect of bilateral aid on recipient exports. The claim for causality running from aid to exports is supported by use of methods that account for the endogeneity of aid in the bilateral trade equation.

This article confirms the abovementioned findings and adds trade policy variables, specifically RTA dummy variables, to the main setting. It has been argued that in some cases, donors will seek to combine closer trade relations with more aid, whereas in other cases, aid and trade regional policies are unrelated. The results of this article support the view that donors give aid to countries with which they have weak trade links with the aim of establishing closer relations.

Finally, when studying the effect of total aid on total recipient exports and GDP per capita, we find that the effect of aid on recipient exports is statistically significant and that aggregate exports and imports seems to have a positive and significant effect on the GDP per capita of the recipient countries. Hence, the part of trade that has been incentivized by foreign aid appears to foster economic development.

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Conflict of Interests

The author declares no conflict of interests.

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Appendix

Table A.1. Overview of studies on the effects of ODA on donor exports.

Authors	Countries	Period	Method	Export/aid Elasticity	Average $-return per $1 Aid
Nilsson (1997)	EU-15 donors to 108 recipients	1975–1992	OLS	0.230 sr	2.6
Wagner (2003)	20 donors to 109 recipients	1970–1992	Bilateral FE/NLS	0.062 sr	.35 direct .95 indirect
Zarin-Nejadan, Monteiro and Noormamode (2008)	Switzerland to almost 100 recipients	1966–2003	Country FE/FD OLS	0.044 sr	.84–.96 (Swiss Fr.)
Martínez-Zarzoso, Nowak-Lehmann, Klasen and Larch (2009)	Germany to 138 recipients	1962–2007	Bilateral FE/Sys-GMM	0.051 sr 0.220 lr	0.64 1.10–1.52
Nowak-Lehmann, Martínez-Zarzoso, Klasen and Herzer (2009)	Germany to 77 recipients	1962–2007	DOLS/DGLS	0.090 lr	1.04–1.50
Silva and Nelson (2012)	Bilateral exports between 180 countries	1962–2000	Bilateral FE	0.094 sr Neg. multil. effect	Not comparable
Pettersson and Johansson (2013)	Exports among 180 countries	1990–2005	OLS /HMR Country FE	0.09 sr	Not comparable
Martínez-Zarzoso, Nowak-Lehmann, Klasen and Johannsen (2016)	Germany to 132 recipients (sectoral exports)	1988–2009	Bilateral FE/DOLS	Sectoral Elast. 0.06 lr	—
Hansen and Rand (2014)	Denmark to 144 recipients	1981–2010	Bilateral FE/GMM	0.059 sr 0.057 lr	0.30
Martínez-Zarzoso, Nowak-Lehmann and Klasen (2017)	The Netherlands to 130 recipients	1973–2009	Bilateral FE/GMM/DOLS	0.06 sr 0.10 lr	0.29 sr 0.84 lr
Martínez-Zarzoso, Nowak-Lehmann and Klasen (2014)	DAC donors to 130 recipients	1988–2007	Bilateral FE/Sys-GMM	0.04 sr 0.12 lr*	0.50 sr 1.80 lr
Martínez-Zarzoso (2015)	22 donors to 132 recipients	1988–2007	Control Function Approach	0.052 sr	—
Otor (2017)	Japan to 15 Asian countries	1972–2008	DOLS		1.30–1.50 sr 1.41–2.62 lr
Temple and Van de Sijpe (2017)	Net imports for 88 aid recipients	1971–2012 three-year averages	FE and CCE	Aid/GDP increase net donor exports	Not comparable
Liu and Tang (2018)	USA and China to 26 and 30 African countries	2003–2012	FE/Dif-GMM	US ns China FE/0.06sr	

Notes: See Zarin et al (2008) for studies in the 1990s and for studies on time-series bivariate models. NLS denotes non-linear least squares. FE denotes fixed effects, Sys-GMM denotes System Generalized Method of Moments, DOLS/DGLS denotes dynamic OLS and dynamic generalized least squares. SR denotes short run (from a static model) and LR long run estimates (from a dynamic model). HMR denotes Helpman, Melitz and Rubinstein (2008). *Calculated as an average of the LR coefficients of three periods.

Table A.2. List of variables, definitions and sources.

Variable	Variable description	Source
Aid	Bilateral Official Development Aid net disbursements in current USD	OECD
Aidcom	Bilateral Official Development Aid commitments in current USD	OECD
Xrec	Donor imports from the recipient in current USD	UNCTAD
Xdon	Recipient imports from the donor in current USD	UNCTAD
GDP_don	GDP of reporter country in current USD	WDI
GDP_rec	GDP of partner country in current USD	WDI
Pop_don	Population of reporter country in millions of inhabitants	WDI
Pop_rec	Population of partner country in millions of inhabitants	WDI
Dist	The distance in kilometers between the capital cities of reporter i and partner j	CEPII
Landlock	Variable that takes the value of 1 if the reporter country is landlocked (meaning that it does not have access to a sea or coastline), 2 if the partner country is also landlocked, and 0 otherwise	CEPII
Comcol	Binary variable that takes the value of 1 if the countries have ever had a colonial relationship, and 0 otherwise	CEPII
Border	Binary variable that takes the value of 1 if the reporter country i and partner country j share a common border and 0 otherwise	CEPII
Lang	Binary variable that takes the value of 1 if the trading countries have a common official language, and 0 otherwise	CEPII
Smctry	Binary variable that takes the value of 1 if both countries were part of the same country in the past and 0 otherwise	CEPII
Comcur	Binary variable that takes the value of 1 if the countries have a common currency, and 0 otherwise	De Sousa (2012)
RTA	Binary variable that takes the value of 1 if the countries belong to the same free trade agreement, and 0 otherwise	De Sousa (2012) and WTO
WTO	Binary variable that takes the value of 1 if the countries are WTO members and 0 otherwise	WTO
Gkf	Gross Capital Formation in current USD	WDI
Lgdppc	Recipient GDP per capita in 2011 constant USD	WDI
CPI	Consumer price index	WDI
Exp_gs	Exports of goods and services in current USD	WDI
Fdi	Foreign direct investment in current USD	WDI
Rem	Remittances in current USD	WDI

Table A.3. List of countries.

Donors	Recipients		
Australia	Afghanistan	Gabon	Pakistan
Austria	Albania	Gambia	Palau
Belgium	Algeria	Georgia	Panama
Canada	Angola	Ghana	Papua New Guinea
Czech Republic	Antigua and Barbuda	Guatemala	Paraguay
Denmark	Argentina	Guinea	Peru
Estonia	Armenia	Guinea-Bissau	Philippines
Finland	Azerbaijan	Guyana	Rwanda
France	Bahrain	Haiti	Samoa
Germany	Bangladesh	Honduras	Sao Tome and Principe
Greece	Belarus	Indonesia	Saudi Arabia
Hungary	Belize	Iraq	Senegal
Iceland	Benin	Israel	Seychelles
Ireland	Bhutan	Jamaica	Sierra Leone
Israel	Bolivia	Jordan	Slovenia
Italy	Bosnia and Herzegovina	Kazakhstan	Solomon Islands
Japan	Botswana	Kenya	Somalia
Kuwait	Brazil	Kiribati	South Africa
Lithuania	Burkina Faso	Kyrgyzstan	Sri Lanka
Luxembourg	Burundi	Lebanon	Sudan
Netherlands	Cambodia	Lesotho	Suriname
New Zealand	Cameroon	Liberia	Swaziland
Norway	Central African Republic	Libya	Syrian Arab Republic
Poland	Chad	Madagascar	Tajikistan
Portugal	Chile	Malawi	Thailand
Slovenia	Colombia	Malaysia	Togo
Spain	Comoros	Maldives	Tonga
Sweden	Congo	Mali	Tunisia
Switzerland	Costa Rica	Malta	Turkey
Turkey	Croatia	Mauritania	Turkmenistan
United Arab Emirates	Cuba	Mauritius	Tuvalu
United Kingdom	Cyprus	Mexico	Uganda
United States	Djibouti	Mongolia	Ukraine
	Dominica	Morocco	Uruguay
	Dominican Republic	Mozambique	Uzbekistan
	Ecuador	Myanmar	Vanuatu
	Egypt	Namibia	Venezuela
	El Salvador	Nepal	Viet Nam
	Equatorial Guinea	Nicaragua	Yemen
	Eritrea	Niger	Zambia
	Ethiopia	Nigeria	Zimbabwe
	Fiji	Oman	

Source: OECD-DAC.

Table A.4. List of free trade agreements.

Europe	Asia	America
EU-South Africa	ASEAN-Australia-New Zealand	Canada-Chile
EU-Albania	ASEAN-Japan	Canada-Colombia
EU-Bosnia	Japan-Indonesia	Canada-Costa Rica
Turkey-Bosnia&Herz.	Japan-Malaysia	Canada-Honduras
EU-Slovenia	Japan-Peru	Canada-Jordan
EU-Chile	Japan-Philippines	Canada-Panama
EU-Cameroon	Japan-Vietnam	Canada-Peru
EU-Colombia	Malaysia-Australia	Chile-Australia
Croatia-Turkey	Malaysia-New Zealand	Chile-Japan
EU-Algeria	Thailand-Japan	Mexico-Chile
EFTA-Albania	Thailand- Australia	Mexico-Japan
EFTA-Bosnia&Herz.	Thailand-New Zealand	US-Mex-Can
EFTA-Chile		US-Chile
EFTA-Colombia	**Africa**	US-Colombia
EFTA-Costa Rica	Egypt-Turkey	USA-Israel
EFTA-Colombia	Morocco-Turley	US-Jordan
EFTA-Egypt	South Africa CU	US-Morocco
EFTA-Israel	Syria-Turkey	US-Oman
EFTA-Jordan	Tunisia-Turkey	US-Panama
EFTA-Libya		US-Peru
EFTA-Morocco		USA-CAFTA-Dominican Republic
EFTA-Mexico		
EFTA-Panama		**Pacific**
EFTA-Peru		Australia-Singapore
EFTA-Tunisia		Trans-Pacific EPA
EFTA-Turkey		
EFTA-Ukraine		
EU-Egypt		
EU-East Africa		
EU-Canada		
EU-Fiji		
EU-Georgia		
EU-Jordan		
EU-Libya		
EU-Morocco		
EU-Mexico		
EU-Peru		
EU-Singapore		
EU-Syria		
EU-Tunisia		
EU-Turkey		
Turkey-Israel		
EU-Ukraine		
EU-CARIFORUM		

Source: WTO.

Table A.5. PPML estimates for recipient exports.

Dep. Variable: Rec. Exports (levels)	(1) HDFE	(2) HDFE	(3) HDFE	(4) CF	(5) CF	(6) CF
Indep. Variables:						
Laid	0.00720 [0.00713]	0.00996 [0.00787]	0.00834 [0.00776]	0.00748 [0.00904]	0.0112 [0.0101]	0.00907 [0.0100]
RTA	−0.000415 [0.0499]	0.0355 [0.0548]		0.0196 [0.0449]	0.0601 [0.0568]	
Laid*RTA		−0.0118 [0.0135]			−0.0216 [0.0158]	
RTA_Europe			0.0217 [0.0727]			0.0223 [0.0809]
RTA_Asia			0.444*** [0.122]			0.306*** [0.0923]
RTA_Africa			−0.398*** [0.154]			−0.263** [0.132]
RTA_America			0.0727 [0.0972]			0.111 [0.102]
RTA_Pacific			0.0400 [0.292]			−0.0381 [0.317]
Laid*Europe			0.00927 [0.0145]			0.00615 [0.0211]
Laid*Asia			−0.125*** [0.0300]			−0.108*** [0.0276]
Laid*Africa			0.0923 [0.110]			0.0108 [0.0909]
Laid*America			0.0119 [0.0252]			−0.000121 [0.0254]
Laid*Pacific			−0.213 [0.257]			−0.142 [0.217]
Residuals from Bilateral Aid Equation				0.00672 [0.00735]	0.00692 [0.00725]	0.00733 [0.00719]
Observations	36,089	36,089	36,089	36,051	36,051	36,051
R-squared	0.997	0.997	0.997	0.996	0.997	0.997

Notes: Standard errors in brackets. Method: PPML for structural gravity with high-dimensional fixed effects (HDFE). FE included: donor-year, recipient-year, donor-recipient. Clustered standard errors, clustered by donor-recipient (default). *** $p < 0.01$, ** $p < 0.05$. CF denotes Control Function Approach. The definition of the variables can be found in Table A2.

Table A.6. PPML estimates for donor exports.

Dep. Variable: Donor Exports (levels)	(1) HDFE	(2) HDFE	(3) HDFE	(4) CF	(5) CF	(6) CF
Indep. Variables:						
Laid	0.00916**	0.0187***	0.0201***	0.00447	0.0182***	0.0211***
	[0.00453]	[0.00511]	[0.00500]	[0.00601]	[0.00614]	[0.00584]
RTA	0.171***	0.250***		0.181***	0.253***	
	[0.0322]	[0.0355]		[0.0367]	[0.0377]	
Laid*RTA		−0.0270***			−0.0293***	
		[0.00721]			[0.00843]	
RTA_Europe			0.168***			0.179***
			[0.0364]			[0.0377]
RTA_Asia			0.447***			0.315***
			[0.115]			[0.116]
RTA_Africa			−0.00941			−0.0742
			[0.156]			[0.151]
RTA_America			0.429***			0.470***
			[0.0513]			[0.0551]
RTA_Pacific			−0.391**			−0.682***
			[0.163]			[0.191]
Laid*Europe			−0.0234***			−0.0277***
			[0.00693]			[0.00857]
Laid*Asia			−0.0681**			−0.0425*
			[0.0266]			[0.0248]
Laid*Africa			−0.0974			−0.0658
			[0.0830]			[0.0606]
Laid*America			−0.0455***			−0.0598***
			[0.0142]			[0.0146]
Laid*Pacific			−0.334**			−0.412***
			[0.143]			[0.115]
Residuals from Bilateral Aid Equation				0.00948*	0.00949*	0.00992**
				[0.00488]	[0.00498]	[0.00482]
Observations	37,879	37,879	37,879	37,837	37,837	37,837
R-squared	0.999	0.999	0.999	0.998	0.999	0.999

Notes: Standard errors in brackets. Method: PPML for structural gravity with high-dimensional fixed effects (HDFE). FE included: donor-year, recipient-year, donor-recipient. Clustered standard errors, clustered by donor-recipient (default). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. CF denotes Control Function Approach. The definition of the variables can be found in Table A2.

Table A.7. Gravity model with additional controls.

Dep. Variable:	Recipient exports		Donor exports	
	(1)	(2)	(3)	(4)
Ln Exports	OLS-TFE	OLS-TCFE	OLS-TFE	OLS-TCFE
Ind. Variables:				
Lgdp_rec	1.206***	0.778***	0.967***	0.822***
	[0.0329]	[0.117]	[0.0157]	[0.0370]
Lgdp_don	1.229***	0.685***	0.925***	0.0416
	[0.0231]	[0.0775]	[0.0218]	[0.0717]
Laid	0.0330**	0.106***	0.118***	0.159***
	[0.0161]	[0.0140]	[0.00967]	[0.00928]
RTA	0.386***	0.240***	0.363***	0.183***
	[0.0886]	[0.0770]	[0.0535]	[0.0459]
Laidrta	0.0268	−0.0211	−0.0104	−0.0556***
	[0.0294]	[0.0227]	[0.0172]	[0.0139]
WTO	0.329***	0.206**	0.0875	0.0966*
	[0.0990]	[0.0838]	[0.0541]	[0.0529]
Comcur	1.279**	−0.270	1.130	0.328
	[0.604]	[0.474]	[1.057]	[0.554]
Ldist	−0.728***	−1.481***	−0.988***	−1.396***
	[0.0585]	[0.0803]	[0.0378]	[0.0515]
Landlock	−0.618***		−0.387***	
	[0.0764]		[0.0469]	
Lang	0.670***	0.479***	0.513***	0.421***
	[0.112]	[0.102]	[0.0711]	[0.0665]
Comcol	1.009***	−0.0278	0.690***	0.265
	[0.266]	[0.266]	[0.223]	[0.206]
Border	0.932**	0.255	0.533	0.289
	[0.443]	[0.466]	[0.442]	[0.581]
Smctry	2.280***	1.094**	0.906***	0.760*
	[0.414]	[0.431]	[0.325]	[0.443]
Observations	33,253	33,253	35,052	35,052
R-squared	0.628	0.757	0.767	0.844

Notes: Robust standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. TFE denotes time fixed effects. TCFE denotes time and country fixed effects. The definition of the variables can be found in Table A2.

Table A.8. Regional specific coefficients for aid.

Dep. variable:	(1) Ln Recipient Exports	(2) Ln Donor Exports
Indep. Variables:		
Laid_EAP	0.0233	0.0328**
	[0.0161]	[0.0142]
Laid_ECA	0.0333	0.0398***
	[0.0211]	[0.0119]
Laid_LAC	0.0436***	0.00816
	[0.0111]	[0.00662]
Laid_MENA	0.00976	0.0123
	[0.0208]	[0.00925]
Laid_SAS	0.0699***	0.0399**
	[0.0215]	[0.0159]
Laid_SSA	0.0135	0.0404***
	[0.0134]	[0.00721]
RTA	0.162***	0.201***
	[0.0429]	[0.0256]
Laid*RTA	−0.0173*	−0.0295***
	[0.00945]	[0.00510]
Residuals from Bilateral Aid Equation	−0.0164	0.0120**
	[0.0110]	[0.00607]
Observations	35,710	37,314
R-squared	0.914	0.952

Notes: Robust standard errors in brackets clustered by donor-recipient (default). Method: High-dimensional fixed effects (HDFE) linear regression. Fixed effects include: donor-year, recipient-year, donor-recipient. *** $p < 0.01$, ** $p < 0.05$. Control Function Approach. All models estimated with the Stata command *reghdfe* from Correia (2017). Bootstrapped standard errors in columns (1000 replications). EAP = East Asia & Pacific; ECA = Europe and Central Asia; LAC = Latin America & Caribbean; MENA = Middle East & North Africa; SAS = South Asia; SSA = Sub-Saharan Africa. The definition of the variables can be found in Table A2.

Table A.9. Income per capita model in first differences.

Dep. Variable: D.lgdppc	(1) CTFE-IV	(2) CTFE-IV	(3) IV-Dyn
Indep. Variables:			
D.laid_total	−0.0540	−0.00860	−0.000816
	[0.0381]	[0.0205]	[0.00279]
D.lxrec_total	0.0408***	0.0203***	0.0232***
	[0.00953]	[0.00778]	[0.00814]
D.lxdon_total	0.0905***	0.0716***	0.0526***
	[0.0149]	[0.0130]	[0.0128]
D.lpop		−0.648***	−0.429***
		[0.131]	[0.0612]
D.lfdi		0.00233**	0.00115
		[0.00106]	[0.000934]
D.lrem		0.000573	0.00282
		[0.00246]	[0.00213]
LD.lgdppc			0.649***
			[0.0560]
Observations	2,112	1,659	1,441
R-squared	0.054	0.227	0.273
Number of countries	122	115	112
Hansen st. (prob.)	0.358	0.294	0.403
Kleibergen-Paap st.	5.850	3.119	21.24

Notes: Robust standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$. CTFE denotes country and time fixed effects, IV denotes instrumental variables and Dyn dynamic model. D. denotes variables in first differences. Hansen st. (prob.) is the probability associated to the Hansen test, which indicate that we cannot reject the validity of the instruments. Kleibergen-Paap statistic is a test for weak instruments, which result indicates that the instruments are not weak. The definition of the variables can be found in Table A2.

Table A.10. Income per capita model with aid in previous periods.

Dep. Variable: Lgdppc	(1) CTFE_CF	(2) CTFE-IV
Indep. Variables:		
Laid_total(t-5)	0.0158**	0.0252***
	[0.00642]	[0.00963]
Laid_total(t-10)	0.0173*	
	[0.00939]	
Lxrec_total	0.105***	0.0965***
	[0.0221]	[0.0206]
Lxdon_total	0.129***	0.134***
	[0.0465]	[0.0391]
Lpop	−0.792***	−0.811***
	[0.120]	[0.109]
Lfdi	−0.00488	−0.00208
	[0.00508]	[0.00459]
Lgcf	0.0693***	0.0677***
	[0.0213]	[0.0200]
Lresaid_s	−0.00725	
	[0.00610]	
Lresxr_s	−0.00876	
	[0.00848]	
Lresxd_s	−0.0301	
	[0.0187]	
Observations	1,541	1,538
R-squared	0.746	0.738
Number of countries	109	106
Hansen st. (prob.)	.	0.314
Kleibergen-Paap st.	.	30.52

Notes: Robust standard errors in brackets. *** p < 0.01, ** p < 0.05, * p < 0.1. CTFE denotes country and time fixed effects, CF denotes control function approach, IV denotes instrumental variables and Dyn dynamic model. Hansen st. (prob.) is the probability associated to the Hansen test, which indicate that we cannot reject the validity of the instruments. Kleibergen-Paap statistic is a test for weak instruments, which result indicates that the instruments are not weak. The definition of the variables can be found in Table A2. lresaid_s, lresxr_s and lresxd_s are obtained from the estimated residuals of the aid, the recipients' exports and the donors' exports models, respectively.

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Article

The Impact of Foreign Aid on Maternal Mortality

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Abstract

In 2010, the G8 placed renewed focus on maternal health via the Muskoka Initiative by committing to spend an additional $5 billion on maternal, newborn, and child health before 2015. Following the end of the Millennium Development Goals and the advent of the Sustainable Development Goals, maternal health issues have continued to feature prominently on the global health agenda. Despite these substantial investments of foreign aid over the past decade, there is limited evidence on the effectiveness of foreign aid in reducing maternal mortality in low- and middle-income countries (LMICs). Using data from the Organisation for Economic Cooperation and Development, the World Development Indicators and the Institute of Health Metrics and Evaluation, this study analyzes the effects of aid on maternal health in a sample of 130 LMICs from 1996 through 2015. Our results show that the effects of total foreign aid on maternal mortality are limited, but that aid allocated to the reproductive health sector and directly at maternal health is associated with significant reductions in maternal mortality. Given these targeted effects, it is important to channel more donor assistance to the promotion of reproductive health and contraceptive use among women as it serves as a tool towards the reduction of maternal mortality.

Keywords

family planning; foreign aid; maternal mortality; Muskoka Initiative; reproductive health

1. Introduction

Recent debates concerning the effectiveness of aid in improving development outcomes have been inconclusive (Tilburg, 2015). Aid critics (Easterly, 2006; Moyo, 2009; Winters, 2010) have voiced their concerns that aid is "dead". They maintain that billions of dollars have been transferred to poor economies with the aim of improving living conditions, but the results have always been catastrophic, leaving more than a billion people still living in abject poverty. Despite these concerted efforts, there has been limited academic research on the links between foreign aid and maternal mortality reduction in low- and middle-income countries (LMICs; Taylor, Hayman, Crawford, Jeffery, & Smith, 2013).

In the case aid committed to maternal health, the Muskoka Initiative on Maternal, Newborn and Child Health was one such commitment adopted at the G8 summit in 2010. This initiative saw a commitment of $7.3 billion through 2015 to improve maternal and child health in the world's poorest countries and to contribute to the achievement of Goal 5 of the Millennium Development Goals (MDGs). The presumption that aid can combat maternal mortality, however, seemed to be based on limited evidence, and this relationship has rarely featured in the global health research agenda.

Given the Muskoka commitments, and support for the MDGs and Sustainable Development Goals (SDGs), over the past decade, the donor community has committed sizeable financial resources to the reduction of

maternal deaths in developing countries. Between 1990 and 2017 and estimated $11.6 billion has been invested in maternal health (Institute of Health Metrics and Evaluation [IHME], 2018). Yet, high levels of maternal mortality are still prevalent in many parts of the world. It is estimated that in 2015 99% (302,000) of maternal deaths were recorded in LMICs compared to other developed regions of the world (World Health Organization [WHO], UNICEF, UNFPA, & The World Bank, 2015). Given the seeming role for international development assistance in combatting this development challenge, it is important to assess the evidence of aid's efficacy in reducing maternal mortality. As such, this study examines the effect of foreign aid on maternal mortality in LMICs using two-way fixed effects panel regression over the period from 1996 through 2015.

2. Background

Evidence suggests most LMICs were not able to meet the targets of the health-related MDGs of reducing maternal mortality ratio by 75% between 1990 and 2015 (WHO et al., 2015). Indeed, by 2015, the WHO reported an estimated decline in global maternal mortality rate (MMR) of 45% in that period to 210 deaths per 100,0000 live births, far short of the 75% reduction goal. Following the MDGs, the SDGs set a target of lowering MMR to 70 per 100,000 live births, as part of SDG 3's goal to "ensure healthy lives and promote wellbeing for all at all ages". To this end, several donor countries have pledged their support to increase funding towards the reduction of maternal health levels to the countries with the poorest health indicators (Proulx, Ruckert, & Labonté, 2017).

Previous foreign aid research has mainly focused on economic development and poverty reduction with mixed results. For example, Bornschier, Chase-Dunn and Robinson (1978), Dalgaard, Hansen and Tarp (2004), and Arndt, Jones and Tarp (2015) all show that foreign aid has a positive impact on economic growth. In contrast, Durbarry, Gemmel and Greenaway (1998), and Annen and Kosempel (2009) and Easterly (2003) show that foreign aid has no impact on economic growth. Ekanayake, Cookman and Chatrna's (2000) study on the effect of foreign aid in developing countries show that there is no impact. Given the complex relationship between health and development, there is an interest in exploring how investments in people's overall health in a country contribute to economic development. It is argued that if the productive workforce is healthy, they can work meaningfully towards higher productivity translating into a higher economic growth and development.

While these studies provide an important step in obtaining empirical evidence of the role of foreign aid on development outcomes, few studies to date have examined the impact of foreign aid on health outcomes such as mortality (Kotsadam, Østby, Rustad, Tollefsen, & Urdal, 2018). Early studies point to a harmful effect of aid on mortality and health outcomes, specifically in the

case where aid increased the indebtedness of recipient countries (Bradshaw, Noonan, Gash, & Sershen, 1993; Sell & Kunitz, 1986). Shen and Williamson (1999) find that greater indebtedness—in some cases aid-related—indirectly increases maternal mortality, but conclude their study with a rallying call to donors, arguing: "It is likely that even a modest increase in aid could substantially improve maternal mortality rates if it were spent on improving the access of poor women to health services" (p. 211).

More recent studies on the impact of foreign aid on mortality have mainly focused on infant or child mortality (Burguet & Soto, 2012; Kotsadam et al., 2018; Mishra & Newhouse, 2009; Pandolfelli, Shandra, & Tyagi, 2014; Winkleman & Adams, 2017). Similarly to the economic literature, empirical evidence suggests that the effects of foreign aid on mortality are inconclusive. Many studies highlight the inefficacy or negative effects of aid. For example, Williamson (2008) find that foreign aid is ineffective in improving overall health. Likewise, Pandolfelli et al. (2014) find that International Monetary Fund loans and structural adjustment contribute to higher maternal mortality in Sub-Saharan Africa. These deleterious effects of structural adjustment on child and maternal mortality are echoed by Thomson, Kentikelenis and Stubbs (2017), Powell-Johnson, Borghi, Mueller, Patouillard and Mills (2006) also find a positive relationship between mortality and Official Development Assistance (ODA). Other research is mixed: Mishra and Newhouse (2009) show that total overall aid had no impact on infant mortality, while health aid reduced mortality levels. Still other studies find beneficial effects of aid on mortality rates: Kotsadam et al. (2018) show that aid programming reduces infant mortality for marginalized communities, while Yogo and Mallaye (2015) demonstrate that increased health aid is linked to significant decreases in child mortality.

While few studies have touched on aid's effect on maternal mortality, there has been a concerted effort to track aid spending in this area. Greco, Powell-Jackson, Borghi and Mills (2008) tracked the flow of health-related aid from 2003 through 2006 and found that aid to maternal health did not always go to the most affected countries. This tracking was part of a series of *Lancet* articles which mapped ODA spending on maternal health but did not analyze its effects on maternal mortality (Arregoces et al., 2015; Grollman et al., 2017; Hsu, Pitt, Greco, Berman, & Mills, 2012; Powell-Johnson et al., 2006). These studies provide a strong basis upon which to examine the effects of the flow of aid to maternal health.

Considering the significant international attention paid to the maternal mortality issue by the international community and donor agencies in recent years, the relative absence of empirical evidence linking aid and reduced mortality is surprising. This study aims to provide some of this evidence and examine the impact of several categories of foreign aid spending on maternal mortality over time. This evidence is important, not only to better understand the health effects of aid, but also to ex-

pand the growing literatures linking aid to gender equality outcomes (Grown, Addison, & Tarp, 2016; Pickbourn & Ndikumana, 2016; Tiessen, 2015).

3. Data and Methods

3.1. Data

Data for this study are drawn from the Organisation for Economic Cooperation and Development (OECD) Creditor Reporting System (CRS) database, the World Development Indicators (WDI) from the World Bank, the IHME database, and Grollman et al.'s (2017) ODA + data set on aid to maternal health.

Our main sample consists of 130 LMICs that were eligible to receive the various categories of aid between 1996 and 2015. In total, the sample consists of 2,093 country-year observations over that period for which all data was available. Descriptive statistics for our sample are shown in Table 1.

The dependent variable in this study is MMR: the number of maternal deaths in a given period per 100,000 women of reproductive age during the same time pe-

riod (WHO, UNICEF, UNFPA, & The World Bank, 2012). We test the relationship between aid and MMR using two different data sources for the dependent variable. The MMR measure in our main analysis consists of MMR data from the WHO and housed in the World Bank's WDI dataset. As a robustness check, we also repeat our analysis using MMR data from the IHME "Maternal Mortality Estimates and MDG 5 Attainment by Country 1990–2011" dataset (IHME, 2011). The WHO defines maternal death as:

> The death of a woman while pregnant or within 42 days of termination of pregnancy, irrespective of the duration and site of pregnancy from any cause related to or aggravated by the pregnancy or its management but not from accidental or incidental causes. (WHO ICD-10, 2011, p. 156)

The causes of maternal death according to the WHO can be direct or indirect causes. The direct causes are those resulting from complications of the pregnant state, from interventions, omissions, incorrect treatment, or from a chain of events resulting from any of the above.

Table 1. Sample descriptive statistics, 130 countries, 1996–2015.

	Min	Mean	Median	Max	SD	N	Source
Dependent variable							
Maternal mortality ratio (MMR)—primary analysis	4.00	289.32	148.00	2650.00	321.69	2093	WDI
Maternal mortality ratio (MMR)—robustness analysis	6.80	294.00	113.50	2592.50	335.29	1709	IHME
Aid measures (per capita)							
Total aid (constant 2011 USD)	0.00	68.66	39.95	1257.09	98.73	2093	OECD CRS
Total aid to Health (constant 2011 USD)	0.00	4.14	1.57	170.19	8.94	2093	OECD CRS
Total population/reproductive policy and programming (constant 2011 USD)	0.00	2.87	0.80	133.76	7.56	2093	OECD CRS
Aid to reproductive health (constant 2011 USD)	0.00	0.34	0.09	11.83	0.76	2093	OECD CRS
Aid to family planning (constant 2011 USD)	0.00	0.16	0.00	5.75	0.41	2093	OECD CRS
Total maternal and newborn health aid (constant 2013 USD)	0.00	0.57	0.09	12.36	1.10	2093	Grollman et al., 2017
Controls							
GDP per capita, (constant 2010 USD)	186.66	5414.95	2357.40	72670.96	9666.15	2093	WDI
Births attended by skilled health personnel, percentage, percent	5.60	72.28	81.00	100.00	27.06	2093	WDI
Adolescent fertility rate (births per 1,000 women ages 15–19)	3.82	72.17	63.98	218.77	47.62	2093	WDI
Contraceptive prevalence, modern methods (percent of women ages 15–49)	1.20	35.76	34.50	86.20	20.92	2093	WDI
Instrument							
Donor fractionalization-recipient aid probability interaction	0	0.22	0.25	0.38	0.08	2055	OECD/ WB DPI

The indirect causes are those not due to direct obstetric causes. Not surprisingly, there is a close association between economic development in a country and its rates of maternal mortality. Figure 1 highlights this relationship for our sample countries in 2015, showing that wealthier countries are likely to have lower rates of mortality. Mean MMR in our sample is approximately 289 deaths per 100,000 women, while median MMR is approximately 148. MMR varies significantly across different geographic regions within our sample and over time. Figure 2 shows this variability, revealing that overall MMR has declined significantly over time, but remains high in certain regions.

Our main independent variables are the annual ODA flows for six categories of aid in millions of constant 2011 USD. The source from the OECD is the net bilateral ODA commitments by the Development Assistance Committee (DAC) donors reported from the CRS. This study considers all forms of aid commitments allocated by the DAC donor countries. We consider the effects of six categories of bilateral aid: total aid, total health-related aid, total aid to population/reproduction policy and programming,
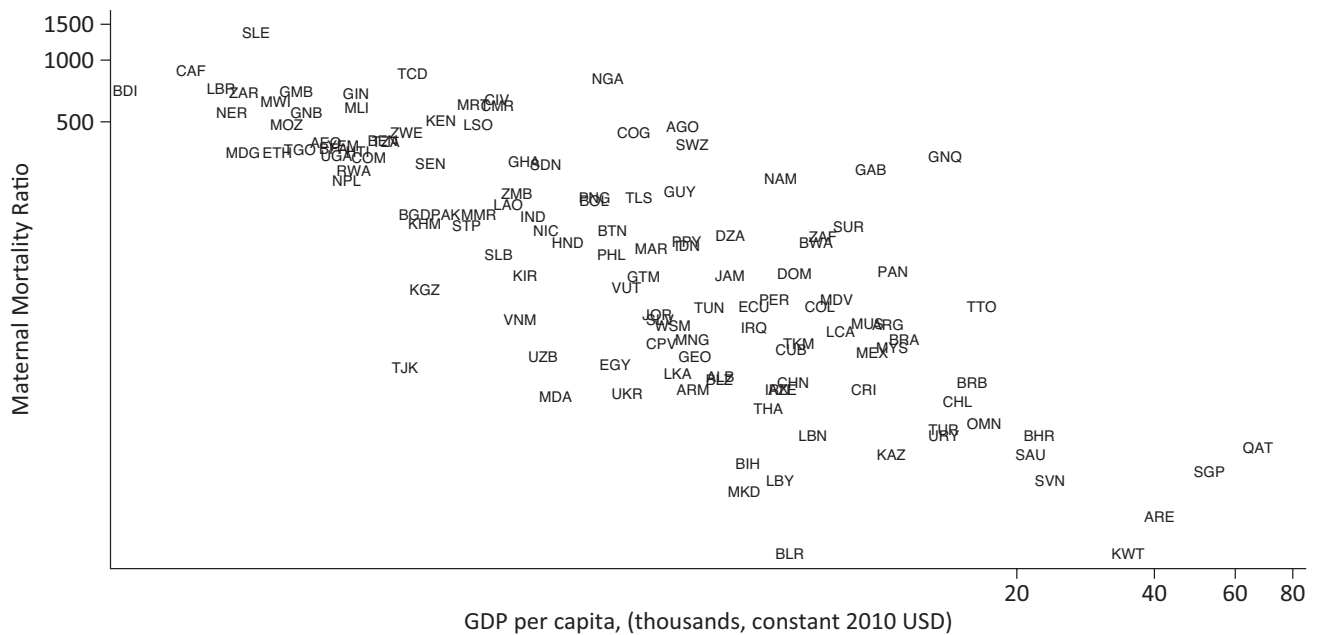
Figure 1. Relationship of sample country GDP per capita and maternal mortality, 2015.
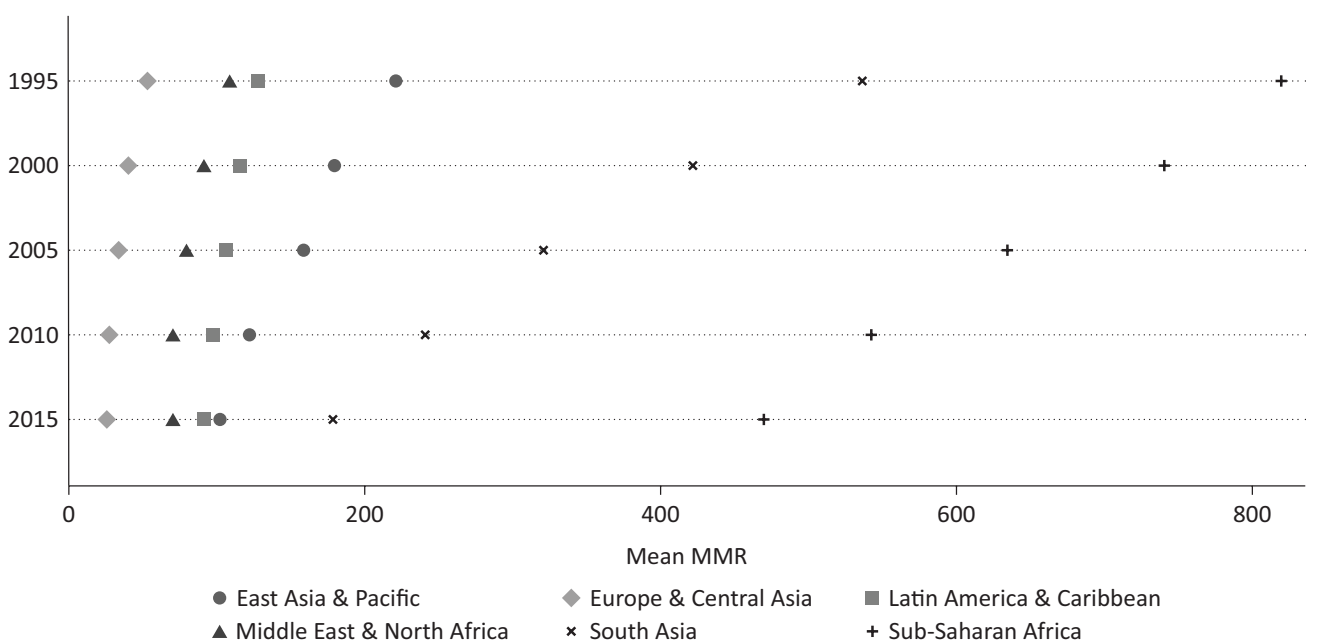
Figure 2. Mean maternal mortality ratio by region, 1995–2015.

reproductive health aid, family planning aid, and total aid to maternal and newborn health.[1] Figure 3 shows how the first five of these categories maps onto DAC aid codes. To account for variation in population size between countries, we convert these ODA data into per capita measures. Our analysis uses the log (base 2) of these measures to account for skewness, meaning that the coefficients for each measure can be interpreted as the marginal effect of a doubling of that type of aid.

Our analysis also accounts for other variables that have an impact on maternal mortality. The other variables included are Gross Domestic Product (GDP) per capita, births attended by a skilled birth attendant, adolescent fertility rate, and population using any method of contraception and the total population. Each of these variables is drawn from the World Bank's WDI databank. To address missing values in this data we replaced missing data with the most recent year's non-missing data. These independent variables are explained below.

GDP per capita: There is a strong negative correlation between a country's level of national income and maternal mortality ratio (Bishai et al., 2016). This relationship has been shown to be robust over time and is evident in Figure 1. Mean GDP per capita in our sample is $5415. In our models, GDP per capita is measured in constant 2010 US dollars and is logged to account for skewness.

Skilled birth attendant: According to a statement by WHO, International Confederation of Midwives (ICM),

and the International Federation of Gynecology and Obstetrics (FIGO), the term 'skilled attendant' refers to:

> An accredited health professional—such as midwife, doctor or nurse—who has been educated and trained to proficiency in the skills needed to manage normal (uncomplicated) pregnancies, childbirth and the immediate postnatal period, and in the identification, management or referral of complications in women and newborns. (WHO, ICM, & FIGO, 2004, p. 1)

Traditional birth attendants either trained or not, are excluded from this category of skilled health workers (WHO et al., 2004 as cited in Nanda, Switlick, & Lule, 2005, p. 9). This measure reflects the percentage of births attended by skilled health personnel, with a mean of 72% of births in countries in our sample over time.

Adolescent fertility rate: The association between maternal mortality and the age at birth of mothers is well-established in the literature (Conde-Agudelo, Belizan, & Lammers 2005; Nove, Mathews, Neal, & Camacho, 2014; WHO, 2018). In our models, adolescent fertility is measured by the rate of births per 1,000 women aged 15–19 years, and averages 72 births per 1,000 women in our sample.

Modern contraceptive use: We account for contraceptive use in our analysis using a measure of the percentage of women ages 15–49 using at least one mod-
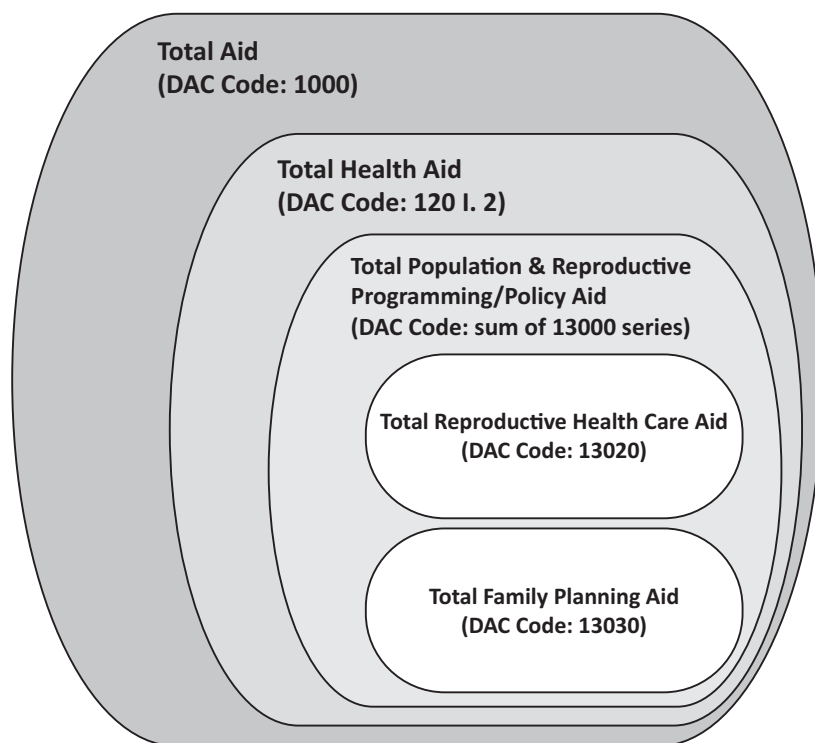

Figure 3. Aid independent variables and corresponding DAC codes.

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[1] The first five categories correspond to the following DAC Sector Codes in the CRS: Total Aid (1000); Health Total (120 I. 2); Total Population and Reproductive Programming and Policy (total of 13000s); Reproductive Health Care (13020); and Family Planning (13030). The final category, total aid to maternal and newborn health, is drawn from the ODA plus dataset presented in Grollman et al. (2017).

ern form of birth control. In our models we use this measure to serve as a proxy measure of reproductive health services and women's empowerment (WHO, 2011). The mean of modern birth control use in our sample is approximately 34%.

3.2. Analysis

We use a two-way fixed effects panel regression model with both year- and country-fixed effects to analyze the impact of foreign aid on maternal mortality. Including both fixed effects components in our models allows us to account for the influence of correlation within countries over time and the effect of global time trends on maternal mortality ratios and all other co-variates. As a result, our models help us predict the effect of aid on change in MMR within countries over time and control for all time-invariant characteristics of a given country. We lag all of our independent measures one-year behind the dependent variable to allow for a temporal gap in which the effects of aid might take hold.[2] For example, in our analy-

sis we are predicting the effects of all independent measures in 2000 on MMR in 2001, or the effects of independent variables in 1996 on MMR in 1997. Finally, we run separate sets of nested models for each of the four aid measures because they are too highly correlated to provide meaningful results if included in a single model.

4. Results

We ran a series of nested models for each aid measure, but in Table 2 we present only the full models for each for the sake of parsimony. Each model includes one of our aid measures, as well as the controls for country-level characteristics. Each of the aid measures is negatively associated with MMRs, but in the case of the Total Aid measure we fail to reject the null hypothesis. The results represent the effect of a doubling of a given type of aid. The strongest effects are seen in total maternal and newborn health aid (from the ODA plus source) and in ODA committed under the reproductive health category, where a doubling predicts a more than 33 death reduction and 26

Table 2. Two-way fixed effects panel regression of maternal mortality on total foreign aid, 1996–2015.

	(1)	(2)	(3)	(4)	(5)	(6)
Aid measures (logged)						
Total aid	−1.34					
Total aid to health		−7.12***				
Aid to reproductive health			−26.07***			
Aid to family planning				−13.10*		
Total aid to population/reproductive policy and programming					−16.41***	
Total Maternal and Newborn Health Aid (ODA plus dataset)						−33.46***
Controls						
Logged GDP per capita, (constant 2010 USD)	−42.31***	−42.83***	−44.84***	−41.72***	−43.74***	−47.16***
Births attended by skilled health personnel, percentage, percent	−1.06***	−1.08***	−0.93***	−1.04***	−1.10***	−1.03***
Adolescent fertility rate (births per 1,000 women ages 15–19)	3.66***	3.63***	3.53***	3.59***	3.44***	3.41***
Contraceptive prevalence, modern methods (percent of women ages 15–49)	−1.89***	−1.86***	−1.83***	−1.89***	−1.55***	−1.77***
Constant	649.76***	661.10***	679.78***	644.47***	683.94***	716.45***
Observations	2093	2093	2093	2093	2093	2093
Countries	130	130	130	130	130	130
R-Squared	0.70	0.70	0.69	0.70	0.69	0.70
Country FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes

Notes: * $p < 0.05$, *** $p < 0.001$.

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[2] We also tested 2-, 3-, 4-, and 5-year lags and the results were comparable except in the case of one aid measure. Due to the nature of our dataset, the one-year lag maximizes our sample size.

death reduction in MMR respectively. These marginal effects are shown in Figure 4, and indicate that, apart from total aid's non-significant relationship to MMR, the most modest effect on MMR is for total health aid. Increases in family planning aid and total population/reproductive policy aid also predict reduced MMR.

Our controls for country and society characteristics are all correlated with MMRs at the $p < 0.001$ level. A doubling of GDP per capita predicts the sharpest reduction in MMR in all models, while more modest reductions in MMR are associated with increased rates of birth attendance by skilled health professionals and contraceptive prevalence. In contrast, adolescent fertility rates are associated with increases in MMR in all models. The results of these models show that countries with growing economies, improving health systems, more readily available contraception, and decreasing teen birth rates all stand to see reductions in their national MMR over time.

When comparing our main results to those in our robustness checks included in the appendix, we note two differences worth discussing. First, with the change in sample introduced via the instruments in the instrumental variable analysis (see Appendix Table A1), via the longer lag period (see Appendix Table A2), or via the use of the IHME MMR data which is restricted to the 1996–2011 period, the robustness of our estimate for the effect of family planning related aid on MMR is challenged. In each of the robustness check models, we see that the family planning aid parameters no longer allow us to reject the null hypothesis. The second difference,

seen in Tables A1 and A3, are that with the shorter time-frame and alternate specifications, the total effect of aid on MMR does meet the $p < 0.05$ level in our robustness checks, suggesting that overall aid is correlated with reductions in MMR.

4.1. Robustness Checks

We also conducted a set robustness check models using: (1) instrumental variable models; (2) instrumental variable regressions with five-year averaged aid flows; (3) five different lag periods for our independent variables; and (4) the alternative measure of MMR from the IHME. Our first robustness check was to reanalyze our data using an instrumental variable approach (see Appendix, Table A1). Following Dreher and Langlotz (2017) and Doucouliagos, Hennessy, and Mallick (2019), we use an excludable instrument based upon the fractionalization of governments in donor countries interacted with the probability of recipient countries receiving aid in a given year. We construct this instrument measure using a dyadic donor-recipient aid dataset based on OECD figures used in Swiss and Longhofer (2016). Because foreign aid levels are endogenous to some measures of development and the other independent variables in our model, we control for endogeneity by using a two-stage approach in our instrumental variable models.

In a zero-stage regression, we use OLS to regress our various aid measures on a lag of each aid measure and the five-year lag of donor government fractionaliza-
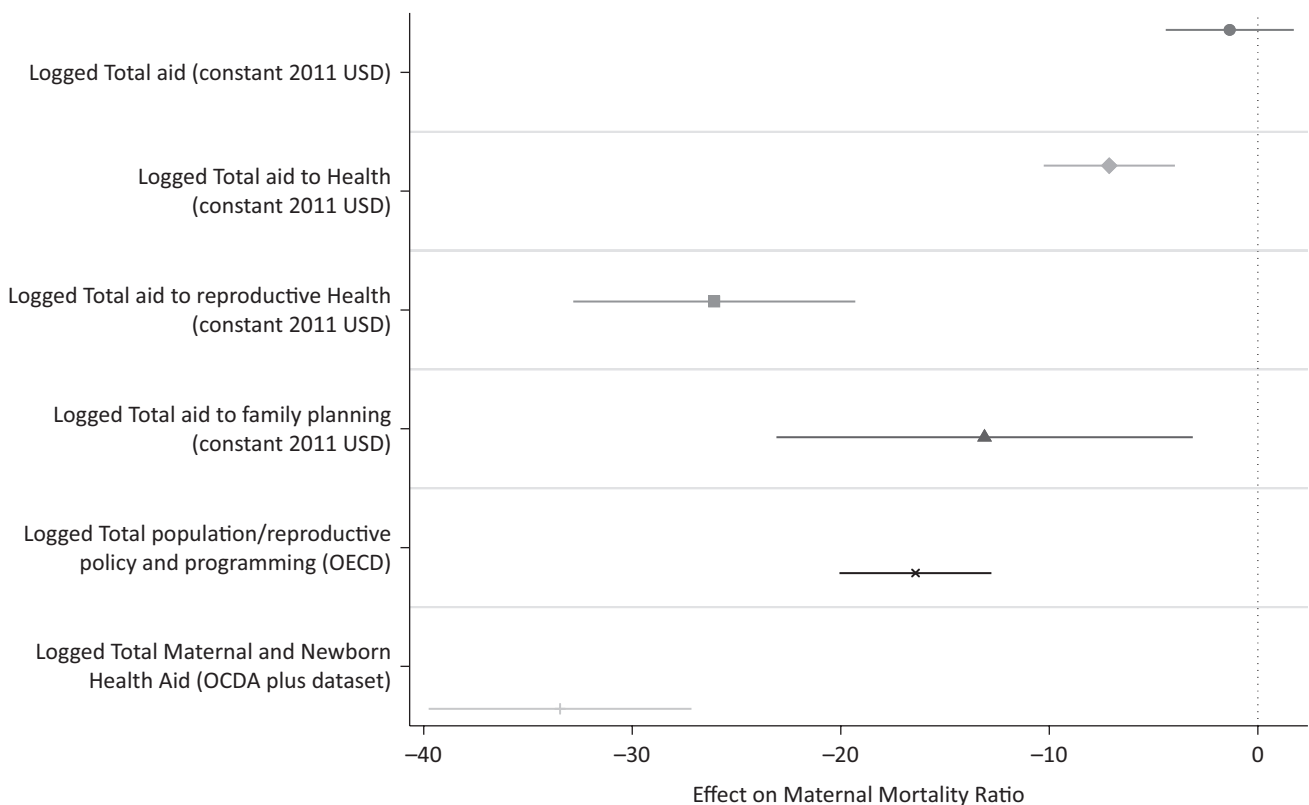

Figure 4. Marginal effect of logged aid on maternal mortality with 95% confidence intervals.

tion interacted with the probability of the recipient country receiving aid from all donors. Because aid levels are closely linked to government competition within donor states (Dreher & Langlotz, 2017), this instrument the level of global aid is fluctuates independently of the conditions in recipient countries. Following this zero-stage model, we predict a fitted aid measure that serves as the excludable instrument in the first-stage model. By interacting the mean donor fractionalization with the mean probability of each recipient country to receive aid from all donors in a given year, the instrument remains exogenous to the MMR variable in the second stage.

In the two-stage IV regression, the aid measure is instrumented on the fitted aid predicted in the zero-stage models. The IV models also include year and country fixed-effects. The results of the IV models in Table A1 show a similar pattern to our main results below.

The second robustness check tested an alternate specification of our aid measures using a five-year moving average (see Appendix, Table A2). These results are consistent with our main analysis, but with the five-year averages, each of the predicted aid measures is associated with reductions in maternal mortality.

Our third robustness check tested the effect of different lag periods between our dependent and independent variables (see Appendix, Table A2). These results are consistent with our main analysis, but with a longer lag period, the predicted effects of family planning aid no longer attain p-values below the commonly accepted 0.5 threshold.

Our final robustness check was to repeat our analysis using the alternative MMR measure discussed earlier (see Appendix, Table A3). These results closely echo our main analysis but, as in the case of Table A1, there are some minor differences of note.

5. Discussion

Our findings show clearly that aid—depending on the sector in which it is spent—has the potential to help reduce maternal mortality. As Figure 3 highlights, the effects of reproductive health-focused aid or aid targeted specifically at maternal health are stronger than that of total aid or total health aid. Given the narrowed focus of reproductive health-focused aid, it is not unexpected it might reduce maternal mortality more directly. If, for instance, reproductive health aid is specifically channeled to the promotion of prenatal and postnatal care including deliveries (which are crucial in elements in the reduction of maternal mortality), an increase in reproductive health aid will have a greater likelihood of diminishing MMR.

With an equally narrow focus as reproductive health-related aid, what might explain the counterintuitive finding we see in the mixed effects of family planning-focused aid between our main analysis and the robustness checks? Comparing the relationship between reproductive health aid and family planning aid in Figure 5 reveals relatively low correlation between the two types of aid (Pearson's R of 0.28 in our sample). This suggests that the same countries receiving significant amounts of reproductive health aid are not necessarily also in receipt of family planning aid and vice versa. Likewise, the bivariate relationship of family planning aid to each of adolescent fertility, birth control, and MMR reveal very low levels of correlation < 0.1 in each case. This implies that, regardless of the intent of family planning-related aid to make contraceptives more widely available, these programs are not necessarily associated with reducing MMR either directly or indirectly through reduced fertility or contraceptive use. Cleland et al. (2006) suggest that un-
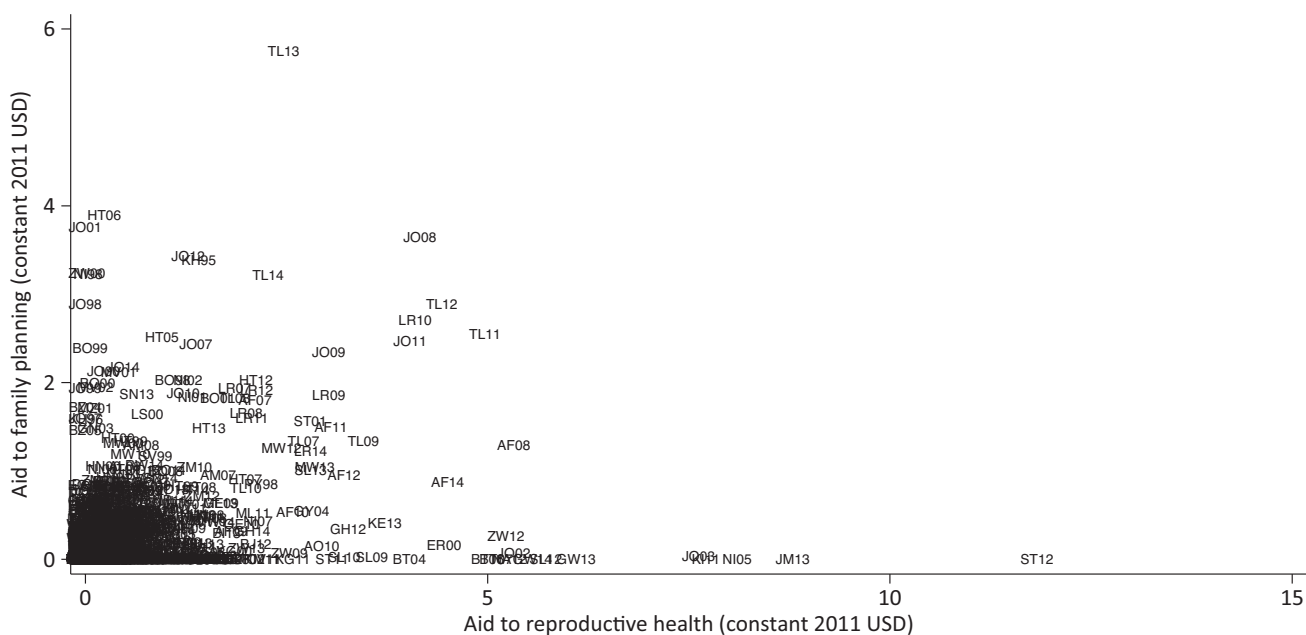

Figure 5. Scatterplot of per capita family planning vs. reproductive health-related aid, sample countries 1996–2014.

even and at times inconsistent uptake of the most effective contraceptive methods, erosion of donor and government support for family planning, and the reallocation of funds towards HIV/AIDS programming are all factors in the reduced efficacy of family planning programs in recent years. Given these challenges, it is not, perhaps, surprising that our results reveal an association between family planning aid that is inconsistent. If family planning programs are increasingly limited, have less political support, and are being sidetracked by resources reallocated to other priorities, it is not unimaginable that they might not reduce maternal mortality.

In contrast to the narrower focus of reproductive health aid and aid to maternal health, our main analysis shows that total aid has no statistically significant effect on maternal mortality once other factors are controlled for. This may be due to the fact that the entirety of a country's ODA is expansive, and the amount allocated for maternal health is marginal. This is clear in our sample, where the mean level of total aid was $68.66 per capita, while mean aid focused on reproductive health amounted to only slightly less than half a percent of that amount at $0.34 per capita. It is not surprising, then, that total aid might not contribute directly to reductions in maternal mortality. Indeed, as has been indicated in much research, MMRs tend not to influence the amount of aid that is allocated to the health sector, whereas, in the case of HIV/AIDS, prevalence rates are closely linked to the amount of foreign funding for HIV/AIDS programs (Shiffman, 2006; Youde, 2010). It is as a result of the threat of the disease globally, which donors believe may be a threat to their own citizens and, therefore, to commit more resources to reduce the prevalence rates (Shiffman, 2006). A report from the OECD indicates that between 2006 and 2007, the amount of health-related aid allocated for HIV/AIDs programs constituted 39% as compared to 13% of aid allocated to the reproductive health sector (OECD, 2008). Maternal mortality may have seized the attention of the international aid community, but it is clear that, even with efforts like the Muskoka Initiative and the SDGs, funding perhaps does not yet match the development challenges posed by maternal mortality.

Our other results are in keeping with what is known about maternal mortality. Each factor shows the type of association with MMR that we would expect to see based on the research literature on maternal mortality. In our main analysis higher rates of adolescent fertility are associated with higher rates of maternal death. Likewise, our findings show that increasing access to modern methods of contraception reduces maternal mortality. The results of this study are consistent with Ahmed, Li, Liu and Tsui (2012) study which found that increased access to contraception in countries with low prevalence of contraceptive use averted 272,040 maternal deaths. This is because people can make choices regarding their reproductive health issues and will also avoid unintended pregnancies and to space the number of children they have.

Women with high parity are more likely to have high maternal mortality as compared to women who have timed and spaced their children. In addition, contraceptives lower the risk of unwanted and unintended pregnancies which often lead to abortion, considered to be the leading cause of maternal mortality in most developed countries (Haddad & Nour, 2009; Okonofua, 2006; Rosmans & Graham, 2006). Despite our potentially contradictory finding regarding the impact of family planning related aid funds, the effects of birth control use suggest it may well remain important to ensure that donor assistance is channeled towards the provision of contraceptives as it is a substantial and effective strategy of reducing maternal mortality in developing countries. Cleland et al.'s (2006) argument that family planning receives more international priority within the context of the SDG post-2015 might be worth heeding in this case.

6. Conclusion

Since total aid is overly broad, there is insufficient evidence to suggest that overall ODA levels lead to a reduction in maternal mortality. However, once aid is targeted at the health sector generally, and at reproductive health, population programming, and maternal health more specifically, there is likely to be accelerated progress towards the achievement of the SDG target for maternal mortality. Still, despite increased efforts under Muskoka, there is a need to increase more resources not only to the health sector but in a more targeted way towards maternal health. Our results show that, despite the potential inefficacy of family planning-focused aid programs, access to contraceptives has a significant effect on the reduction of maternal mortality. It would, therefore, be important to channel more donor assistance to the promotion of contraceptive use among women as it serves as a tool to empower them and to take decisions that influence their reproductive behavior.

One limitation of this study was that it only analyzed bilateral ODA from the DAC donors and did not capture multilateral aid or aid from other non-traditional donors such as the WHO, NGOs, private foundations, businesses, among others. By tracking the amount committed from these other donors, a clearer picture of the effects of donor assistance on maternal mortality might emerge. Future research should track the amount of resources from the other donors not reported by the DAC so that the true effect of foreign assistance on maternal health could be established. Research is also needed to do a comparison between the DAC and the non-traditional donors to compare the behavior of these groups of donors and their impact on maternal mortality.

A second limitation of this study is that it does a cross-country analysis of donor funding to various countries and the results may not be in context for all countries. A possible extension of this study could focus on individual countries and the amount of donor assistance received respectively, with more attention paid to what

services, expertise, and reforms aid money is actually funding. Likewise, multilevel models studying the maternal health outcomes of individuals nested in national contexts could help deepen our understanding of the effects of aid further. A detailed case study of individual country is necessary in order to establish a more nuanced picture of the effect of foreign aid on maternal mortality. Donor decisions on the level of maternal health assistance provided, the nature of those programs, and how they are implemented in individual countries likely vary widely and it would be important to treat each country as a unique case.

The results of this study should be interpreted with caution since the data on the DAC reporting system broken down at the sector level are commitments from the donor community rather than actual disbursements, so actual aid flows to each country might depart significantly from what donors committed. Still, given these data limitations, our study is one of the first to clarify the relationship between aid and maternal mortality over time, and makes a contribution to both the research literature on maternal mortality specifically, and to the literature on the effects of aid more generally.

The Muskoka Initiative in 2010 drew significant donor attention to the issue of maternal mortality and encouraged an intensification of efforts towards supporting recipient countries in achieving MDG 5 and reducing the burden of maternal mortality. These efforts now continue under the SDG framework of the 2030 Agenda. Our results suggest that this international agenda-setting exercise is not without merit. Foreign aid narrowly focused on issues of reproductive and maternal health is strongly associated with declining maternal mortality. As the implementation of Agenda 2030 unfolds, these results suggest that the international community would do well to continue to invest its development assistance resources in ongoing efforts to counter maternal mortality wherever it remains a significant threat to women's lives.

Acknowledgments

Conflict of Interests

The authors declare no conflict of interests.

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Appendix

Table A1. Instrumental variable two-stage fixed effects regression of maternal mortality on foreign aid, 1996–2014.

	(1)	(2)	(3)	(4)	(5)	(6)
Second stage regression results (two-way fe)						
Aid measures (logged)						
Total aid	−4.41					
Total aid to health		−54.28**				
Aid to reproductive health			−104.97***			
Aid to family planning				−10.08		
Total aid to population/reproductive policy					−54.73***	
Total MNH Aid (ODA plus dataset)						−76.65***
Observations	2054	2054	2054	2054	2054	2054
Countries	128	128	128	128	128	128
R-Squared	0.50	0.30	0.39	0.51	0.42	0.49
Controls	yes	yes	yes	yes	yes	yes
Country FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Cragg-Donald Weak Identification	617.45	24.72	79.90	334.66	164.92	609.00
Anderson Underidentification Test	471.83	24.69	77.60	288.05	153.60	466.93
First stage regression results (two-way fe)						
Fitted aid (correspond to aid measures above)	0.52***	0.15***	0.34***	0.54***	0.34***	0.68***
Observations	2054	2054	2054	2054	2054	2054
Countries	128	128	128	128	128	128
R-Squared	0.30	0.10	0.13	0.23	0.29	0.60
Controls	yes	yes	yes	yes	yes	yes
Country FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Zero order regression results (OLS)						
One-year lagged aid measure	0.93***	0.73***	0.59***	0.67***	0.81***	0.76***
Donor fractionalization-aid receipt probability	0.58**	0.89***	0.44***	0.28***	0.69***	0.10
Observations	2054	2054	2054	2054	2054	2054
Countries	128	128	128	128	128	128
R-Squared	0.88	0.54	0.36	0.48	0.65	0.59
Controls	no	no	no	no	no	no
Country FE	no	no	no	no	no	no
Year FE	no	no	no	no	no	no

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The donor fractionalization-aid receipt probability measures in the zero order regression is the interaction of the mean donor-state government fractionalization (World Bank DPI) for all donors in a given recipient country and the mean probability of a recipient country to receive any aid from all possible donors in the Swiss and Longhofer (2016) dataset. The interaction term is lagged five years to account for the aid project cycle, providing time for changes in donor governments and aid levels to take effect.

Table A2. Two-way fixed effects panel regression of maternal mortality on five-year average foreign aid flows, 1999–2015.

	(1)	(2)	(3)	(4)	(5)	(6)
Aid measures (logged five-year average)						
Total aid	−6.54**					
Total aid to health		−18.09***				
Aid to reproductive health			−47.97***			
Aid to family planning				−17.96*		
Total aid to population/reproductive policy					−27.04***	
Total MNH Aid (ODA plus dataset)						−45.58***
Observations	1919	1919	1919	1919	1919	1919
Countries	130	130	130	130	130	130
R-Squared	0.68	0.68	0.67	0.68	0.66	0.68
Controls	yes	yes	yes	yes	yes	yes
Country FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A3. Two-way fixed effects panel regression of maternal mortality on foreign aid, different lags.

	(1) 1-year lag	(2) 2-year lag	(3) 3-year lag	(4) 4-year lag	(5) 5-year lag
Aid measures (logged)					
Total aid	−1.34	−1.85	−1.85	−1.83	−1.57
Total aid to health	−7.12***	−5.37***	−5.52***	−5.59***	−4.65***
Aid to reproductive health	−26.07***	−23.78***	−21.58***	−17.42***	−17.10***
Aid to family planning	−13.10*	−9.76*	−2.82	−0.24	3.12
Total aid to population/reproductive policy	−16.41***	−16.72***	−16.25***	−15.44***	−14.90***
Total MNH Aid (ODA plus dataset)	−33.46***	−38.67***	−35.04***	−30.73***	−26.81***
Observations	2093	1965	1837	1709	1582
Countries	130	130	130	128	127
Controls	yes	yes	yes	yes	yes
Country FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes

Notes: * $p < 0.05$, *** $p < 0.001$.

Table A4. Two-way fixed effects panel regression of MMR on foreign aid, 1996–2011, (IHME MMR measure).

	(1)	(2)	(3)	(4)	(5)	(6)
Aid measures (logged)						
Total aid	−13.79***					
Total aid to health		−13.64***				
Aid to reproductive health			−28.30***			
Aid to family planning				3.33		
Total aid to population/reproductive policy					−45.16***	
Total MNH Aid (ODA plus dataset)						−23.18**
Observations	1582	1582	1582	1582	1582	1582
Countries	127	127	127	127	127	127
R-Squared	0.06	0.11	0.07	0.11	0.02	0.08
Controls	yes	yes	yes	yes	yes	yes
Country FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes

Notes: ** $p < 0.01$, *** $p < 0.001$.

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Article

Foreign Aid and Climate Change Policy: What Can('t) the Data Tell Us?

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Abstract

Climate-related foreign aid is on the rise, with signatories to the Paris Climate agreement pledging $100 billion annually to promote mitigation and adaptation in recipient countries. While this seems like a welcome development, we have little evidence that climate aid actually encourages recipients to adopt climate legislation. In this article, we examine the relationship between climate aid and recipient climate policy. Using multiple measures of each, we find no evidence that the former is systematically related to the latter. Although this suggests that climate aid is ineffective, this conclusion must be qualified due to the poor quality of both climate aid and climate policy data. More definitive conclusions will require more accurate coding of climate aid as well as better climate policy measures that distinguish truly consequential policies from less consequential ones.

Keywords

adaptation; climate change; climate policy; environmental policy; foreign aid; mitigation

1. Introduction

Concerns about climate change continue to mount, along with scientific evidence of its risks. A recent report by the United Nations Intergovernmental Panel on Climate Change concluded that a "business as usual" scenario would lead global temperatures to rise by 1.5 degrees Celsius above pre-industrial levels by 2040 (Masson-Delmotte & et al., 2018). The likely consequences include rising sea levels, worsening droughts and wildfires, food shortages, species extinctions and extreme weather events. Preventing these changes will be difficult, requiring the virtual elimination of carbon emissions over the next two decades. This presents particular challenges for developing countries, which will need to undertake costly mitigation measures even as they struggle to adapt to these climatic effects.[1]

One way to help poor countries respond to the climate challenge is to increase climate-related foreign aid. Aid could reduce the economic costs of mitigation measures, help developing countries adapt to climate-related threats, and incentivize recipients to adopt such policies. For these reasons, aid has become a central plank in efforts to combat climate change. During the 2009 Copenhagen climate conference, developed countries pledged $100 billion annually to assist developing countries with mitigation and adaptation, and this commitment was formalized in Article 9 of the Paris Climate Agreement (United Nations Framework Convention on Climate Change [UNFCCC], 2015). The rising prominence of climate aid raises a perennial question: Is foreign aid effective? Does it really help recipients combat climate change?

This question is important, given the mixed overall track record of foreign aid. As Qian (2015, p. 280) notes,

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[1] This is not to suggest that developing countries are primarily responsible for solving the climate problem: only a few are major emitters, and developed countries account for the bulk of historical carbon emissions. Our point is that the costs of mitigation and adaptation are particularly burdensome to countries that are already poor.

research on the effects of aid has yielded no firm conclusions: "Depending on the measures used, the empirical strategy employed, and the context of the study, the results can vary widely, from finding that aid can be quite beneficial to being harmful." Moreover, this research focuses mostly on economic growth and development: we have no systematic evidence on whether aid influences recipient environmental policy. Case studies suggest that appropriately designed aid programs can facilitate climate policy adoption (Barnett, 2008; Chen & He, 2013). However, the broader literature on aid makes clear that potential benefits are not always realized. We thus need more systematic research on how climate aid affects recipient climate policies.

Such research requires good data on both variables. Unfortunately, we lack such data. Although the OECD Development Assistance Committee (DAC)'s "Rio markers" are meant to identify climate-related aid, a number of studies demonstrate that these markers lack both validity and reliability (Donner, Kandlikar, & Webber, 2016; Hicks, Parks, Roberts, & Tierney, 2008; Junghans & Harmeling, 2012; Michaelowa & Michaelowa, 2011; Roberts & Weikmans, 2017; Weikmans, Roberts, Baum, Bustos, & Durand, 2017). Extant measures of climate policy exhibit even more serious problems. The mere ratification of international climate agreements such as the Kyoto Protocol or the UNFCCC tells us little about how seriously signatories are addressing climate change through domestic policies. The Grantham Institute's *Climate Change Laws of the World* database (n.d.) provides a wealth of information on national climate-related laws. But without coding this legislation for scope, depth and implementation, we cannot aggregate these laws into a meaningful measure of climate action. Of extant measures, Steves and Teytelboym's (2013) Climate Laws, Institutions and Measures Index (CLIMI) comes closest to accurately measuring national climate policy. But as we discuss below, this measure also has serious limitations.

Although these data limitations are well-known, they require emphasis because, in the absence of good alternatives, scholars increasingly employ these data to draw conclusions about both climate aid and climate policy. For example, Bagchi, Castro and Michaelowa (2016), Betzold and Weiler (2017), Halimanjaya and Papyrakis (2015), Klöck, Molenaers and Weiler (2018), and Weiler, Klöck and Dornan (2018) all employ Rio markers to study the allocation of climate aid, while Schmidt and Fleig (2018) employ counts of Grantham climate laws to study the evolution of national climate policies. Without addressing the abovementioned measurement concerns, such studies may provide a misleading picture of developments in both policy domains.

To illustrate this point, we examine the relationship between extant measures of climate aid and climate pol-

icy. We obtain climate aid data from Tierney et al. (2011), which includes the Rio markers, and Michaelowa and Michaelowa (2011), who provide an alternative and arguably more accurate coding of mitigation and adaptation aid. We employ climate policy data from the Grantham Institute and Steves and Teytelboym (2013). Given the questionable validity of both aid and climate policy measures, we do not expect to find any associations between the two beyond what might result from random measurement error. This is exactly what we find. Of 66 estimated aid coefficients, only three (4.5 percent) are significant—which, at the .05 significance level, is about what random measurement error would produce. These null results are robust to various measures, samples and estimation techniques. Moreover, even the three significant coefficients are anomalous: for example, mitigation aid predicts adaptation policies but not mitigation policies. There seems little reason to attribute these results to anything but random chance.

Of course, we do not know that our null results reflect measurement error. It is also possible that climate aid really does not affect recipient climate policies, or that endogenous aid allocation—which we have been unable to adequately address—biases our analyses toward null results. Without knowing the true relationship between climate aid and climate policy—which we cannot know without better data—we cannot reject these alternative interpretations. It is worth noting, however, that our control variables also fare poorly: only a handful of coefficients are significant, none of them robustly across the various models. This also suggests that we are trying to predict variation that is more random than systematic.[2] Given the known limitations of extant climate aid and policy measures, the simplest explanation for our results is "garbage in, garbage out." That is, we cannot expect to find robust, significant relationships among variables that are largely unrelated to the concepts of interest.

For this reason, we do not conclude that climate aid is ineffective at combating climate change. Rather, our central message is that we cannot even begin to study this question without a much larger investment in measurement techniques and data collection. With foreign aid, the central challenge is identifying aid that is clearly climate-relevant. Previous studies provide some guidelines for doing so (Donner et al., 2016; Junghans & Harmeling, 2012; Michaelowa & Michaelowa, 2011; Roberts & Weikmans, 2017; Weikmans et al., 2017), but implementation remains a formidable task. With climate policy, the task is more difficult still. Diverse national policies—carbon taxes, cap-and-trade systems, clean energy subsidies, regulatory requirements, petrol taxes, feed-in tariffs, energy efficiency measures, and so on—must somehow be aggregated into a single cross-nationally comparable metric, ideally after weighting these policies according to not only their importance on

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[2] Steves and Teytelboym (2013) are more successful at predicting variation in the CLIMI index. This is probably because their sample includes developed countries, which we do not include due to our focus on aid recipients. Our different results suggest that CLIMI is better at measuring variation between developed and developing countries than within the latter group alone.

paper but also their implementation. Although this is a daunting task, it is similar to that faced by trade-policy scholars who must aggregate diverse nontariff barriers into a single ad valorem tariff equivalent (Kee, Nicita, & Olarreaga, 2009). Their success at doing so suggests that climate scholars may also find ways to construct, for example, an ad valorem carbon tax equivalent. In any case, progress toward this goal is essential if we are to say with any confidence whether and how foreign aid affects climate policy.

2. Climate Aid and Climate Policy

For concreteness, we begin by clarifying the scope of our study. First, we are interested in the effects of climate aid, i.e., aid meant to promote either climate change mitigation or adaptation. Such aid has risen dramatically in recent years. In 2001, donors established several funds to increase poor-country participation in the Kyoto Protocol: an adaptation fund, a fund to finance the transition to greener technologies, and a Clean Development Mechanism to finance greenhouse-gas reducing projects (Hicks et al., 2008, pp. 258–259). At the 2009 Copenhagen conference, donors pledged $100 billion annually to promote mitigation and adaptation, and this commitment was formalized in the Paris Climate Agreement. Although disbursements have lagged behind these commitments, there seems little doubt that climate aid is on the rise. According to the OECD (n.d.), total climate-related aid rose from around $700 million in 2000 to $19 billion in 2010 to $52 billion in 2016.

Second, we focus on bilateral rather than multilateral aid. This is partly because we rely on Michaelowa and Michaelowa's (2011) coding, which covers only bilateral aid. However, it is also true that most climate aid is bilateral. Victor (2013, p. 5) estimates that "bilateral climate change assistance is more than twenty times larger than multilateral funds," and Marcoux, Parks, Peratsakis, Roberts and Tierney (2013) show that this trend toward bilateralism is increasing over time. Bilateral aid is thus substantively more important and more likely to have discernible effects.

Third, we are interested in how this aid affects recipient climate policies: i.e., policies that either mitigate climate change (by reducing greenhouse gas emissions) or adapt to its effects (by taking measures to deal with rising sea levels, droughts, crop failures, etc.). Such policies have also proliferated in recent years. For example, in 2008, Brazil passed the "National Plan on Climate Change"; Chile passed the "National Climate Change Action Plan"; India passed the "National Action Plan on Climate Change," and so on (Grantham Institute, n.d.). Although some of these policies are of questionable significance, it is clear that a growing number of poor countries are making at least a cosmetic effort to address climate change.

This focus excludes some important questions. Our focus on *climate* aid means that we do not consider the effects of development aid more broadly. Similarly, our focus on climate *policies* means that we do not consider direct economic links between aid and climate change, e.g., aid-fueled growth causing higher carbon emissions. Although these issues are worth exploring, it is important to look specifically at the climate aid-policy link. Climate aid is donors' key policy lever for helping poor countries cope with climate change. If it does not work, we must ask whether these funds could be more effectively used in other ways. And while aid could affect climate outcomes through non-legislative channels, large-scale mitigation and adaptation seem unlikely without public policies that alter private-sector incentives.

How might climate aid affect recipient climate policies? Perhaps the simplest mechanism is that aid can relax the recipient government's budget constraint, allowing it to spend more on climate mitigation or adaptation. This is the intuition in Chao and Yu (1999) and Hatzipanayotou, Lahiri and Michael (2002): in these models, an increase in foreign aid reduces pollution by increasing resources for pollution abatement. The increased-spending link is clearly relevant to adaptation measures, which typically require governments to spend money on projects such as dikes, dams, wells, irrigation, and so on. However, fiscal resources can also play an important role in mitigation policy in the form of subsidies or tax breaks to encourage clean energy production and consumption. An important caveat to this argument, as Eyckmans, Fankhauser and Kverndokk (2016) note, is that climate aid may simply allow recipients to reallocate their current climate finance to other ends, yielding no net increase in climate-related spending. They recommend giving climate aid in the form of matching grants, in which aid is proportional to domestic climate spending.[3]

Even if aid incentivizes climate action via increased resources, we need to ask why it would lead to new climate legislation—an important question because our empirical analysis focuses on such legislation rather than spending per se. Much aid is project-specific: it is earmarked for projects such as dams, wind turbines, and rural infrastructure projects. Why would external funding for such projects lead recipients to pass new legislation?

One answer is that aid could boost the need for implementing legislation, i.e., laws that implement projects wholly or partially funded by aid. For example, Vietnam's "Decisions No. 37/2011/QD-TTg and 39/2018/QD-TTg on the support for Wind Power Projects" (Vietnam Law & Legal Forum, 2011) provides detailed rules for the establishment and operation of wind power projects, including licensing requirements, electricity purchase prices per kilowatt-hour, subsidies per kilowatt-hour, and so on. Although aid probably did not "cause" this legislation, the latter would not exist without wind power projects.

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[3] Matching grants would constitute a form of aid conditionality. This is another possible channel through which aid donors could influence recipient climate policy. At present, however, this possibility is largely hypothetical, since climate aid is generally not conditional.

To the extent that aid makes such project possible, it also generates a need for the implementing legislation.

Perhaps more importantly, much climate aid funds "capacity building" projects. Capacity building is a broad concept that, in this context, largely boils down to a society's ability to formulate and implement mitigation and adaptation policies. As such, it encompasses such diverse elements as training public and private-sector actors, building bureaucracies to formulate and implement policies, and educating the public (Victor, 2013). Although capacity building involves both the public and private sector, the government plays a central role. It is typically government agencies that manage and coordinate activities like scientific research, planning and building infrastructure, taxing fossil fuels and subsidizing clean energy, disaster relief, and so on. For this reason, a large fraction of climate aid is targeted toward government capacity building (Victor, 2013, p. 4).

Building government capacity typically involves legislation. In fact, of the 1,512 climate laws currently listed in the Grantham database, 127 (8 percent) are wholly dedicated to building institutions and administrative arrangements, and another 700 (46 percent) are partially dedicated to this goal. An example is Mali's "Decree No. 2011-107-PM-RM of March 11, 2011 establishing the National Climate Change Committee of Mali" (Republic of Mali, 2011). This created a National Climate Change Committee responsible for implementing UNFCCC and other international obligations, securing funding for this purpose, preparing Mali's participation in international climate conferences, and providing the government with information on climate-related issues. The linkages with aid are clear: this legislation was passed in part to help Mali administer aid-financed projects, as well as to secure future funding. More generally, if aid increases the fiscal feasibility of climate measures, it also creates a need for administrative institutions and personnel, and hence for capacity-building legislation.

Despite these plausible linkages, there are reasons to doubt the efficacy of climate aid. One is the mixed record of foreign aid more generally (Qian, 2015). If development aid often doesn't work—due to waste, corruption, bad governance, etc.—it is not clear why climate aid should fare any better. Studies on environmental aid specifically reinforce this point. For example, Connolly (1996, p. 333) notes that "donors do not always provide aid in order to solve environmental problems…donor governments…sometimes care more about the appearance of doing something…than about finding genuine solutions." Michaelowa and Michaelowa (2011) support this view, showing that (1) much climate aid has nothing to do with climate-related projects, and (2) governments have domestic political motives to attach climate markers to aid. If donors simply want to show that they are "doing something," they may not monitor recipients closely—and the latter may do little themselves. This un-

derscores the need to examine the aid-climate policy link empirically.

Although there is a growing body of research on climate aid allocation (Bagchi et al., 2016; Betzold & Weiler, 2017; Halimanjaya & Papyrakis, 2015; Hicks et al., 2008; Klöck et al., 2018; Weiler et al., 2018), we know little about its effects. A study of World Bank-financed projects found that the track record of environmental projects is poor: only 26 percent of environmental project outcomes were deemed satisfactory, compared with an average success rate of 80 percent in eight other sectors (World Bank, 2005, p. 12). We do not know how many of these projects were climate-related, however, or whether any of them involved policy changes. Case studies that focus on climate aid show that well-designed programs can facilitate mitigation and adaptation (Barnett, 2008; Chen & He, 2013). However, due to their narrow scope, these studies reveal more about what is possible than what is typical.

Mak Arvin, Dabir-Alai and Lew (2006) examine the relationship between development aid and carbon dioxide (CO_2) emissions. They find causality in both directions: aid causes emissions and emissions cause aid. However, the sign of this relationship (positive or negative) varies across countries, and "no obvious grouping of developing countries with common characteristics emerges with respect to a particular causal finding" (Mak Arvin et al., 2006, p. 76). Their results are thus mixed. Moreover, these emissions-based results probably reflect the association between CO_2 emissions and growth: that is, aid causes higher (lower) emissions when it promotes (hinders) growth, and emissions cause increased (decreased) aid when donors reward recipients for good (bad) growth performance. Hence, while these results are interesting, they also tell us little about the aid-climate policy link. Given the dearth of direct evidence on this relationship, there is a pressing need for further empirical research.

3. Data Sources

We obtain data on climate aid and climate policy from well-established and widely used sources. Our aid data are from AidData's project-level database (Tierney et al., 2011), which provides data on aid commitments and disbursements from the OECD Creditor Reporting System as well as individual donor agencies (AidData, n.d.).[4] Our climate policy data are from the Grantham Research Institute on Climate Change and the Environment and from Steves and Teytelboym (2013), which introduces the CLIMI index. Although these are, to our knowledge, the best available sources of data on climate aid and climate policy, they raise a number of measurement concerns. We discuss these concerns below, beginning with climate aid.

In 1995, donors began coding aid projects with "Rio markers," which indicate whether projects relate to bio-

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[4] PLAID version 1.9.2 was accessed on 19 October 2010. We use this release of the data because it employs the same project codes as Michaelowa and Michaelowa (2011).

diversity, desertification or climate change. In 2011, donors also began classifying climate aid according to its purpose (mitigation or adaptation). In principle, these markers make it easy to identify climate-related bilateral aid. In practice, scholars have questioned how accurately these markers convey the true purpose of aid.

Assessments of the original (pre-2011) Rio climate marker—which, strictly speaking, only pertained to mitigation projects—were skeptical. For example, Roberts, Starr, Jones and Abdel-Fattah (2008) examined a random sample of 115,000 aid projects and found that only 25 percent of projects with Rio climate markers were actually relevant to climate change. Michaelowa and Michaelowa (2011) examined all 636,962 aid activities in the DAC database and came to similar conclusions. Of the 10,414 projects with Rio markers, only 2,798 (27 percent) had any relevance to mitigation. Another 1,277 projects (12 percent) were relevant to adaptation. Even if we broaden the DAC's definition of climate relevance to include both mitigation and adaptation, nearly two-thirds of projects with Rio markers had nothing to do with climate. As examples of such miscoding, Michaelowa and Michaelowa (2011, p. 2010) cite the following (donors in parentheses):

- Savannah elephant vocalization (US);
- Uniforms for park guardians in Central America (Spain);
- Tobacco control (New Zealand);
- Lead reduction in transport fuels in Pakistan (UK);
- Earthquake safety (Switzerland);
- Monetary climate in Democratic Republic of Congo (Belgium);
- Love movie festival (Belgium).

Michaelowa and Michaelowa (2011) conclude that, while some errors are probably accidental, donors also have incentives to deliberately miscode data to give the appearance of addressing climate change.

Studies on the post-2010 Rio Markers, which code projects for both mitigation and adaptation, have been no less critical. Junghans and Harmeling (2012) conclude that 65 percent of projects with the adaptation marker are unrelated to adaptation. Moreover, the degree of over-reporting varies across donors: hence the Rio marker lacks reliability as well as validity. Weikmans et al. (2017, p. 458) reach similar conclusions, arguing that "the absence of independent quality control makes the adaptation Rio marker data almost entirely unreliable." These coding irregularities have made their way into real-world policy debates: for example, during the Paris climate negotiations, India argued that developed countries provided only $2.2 billion of climate aid in 2014, rather than the $62 billion claimed by donors (Roberts & Weikmans, 2017, p. 130). For our purposes, the central problem is that these inaccuracies make it difficult to credibly estimate the effects of climate aid.

Although we employ Rio markers in our analysis, we also use Michaelowa and Michaelowa's alternative (2011) coding. We do this for two reasons. First, because the latter data have been coded by disinterested third parties, they should be more accurate than donor-reported data. Second, Michaelowa and Michaelowa's (2011) data allow us to distinguish mitigation and adaptation aid prior to 2011. For both reasons, these data should permit us to estimate more accurately the relationship between climate aid and climate policy.

If the climate aid data are questionable, climate policy data raise even greater measurement concerns. As Bernauer (2013, p. 435) observes, existing datasets "offer aggregate measures of ambition levels of climate policies but no information on the adoption of specific climate policy instruments." The measurement problem is twofold. First, it is difficult to assess the contribution each policy makes to climate mitigation or adaptation. How, for example, can we compare a national carbon tax, a cap-and-trade program, a set of subsidies for clean energy, and a law mandating energy efficiency? In principle, each policy could be converted into a comparable metric such as a carbon tax equivalent or an estimated emissions reduction. In practice, this econometric task has yet to be achieved, making it difficult to compare the depth and scope of diverse national policies. Second, without accomplishing this first task, we cannot aggregate diverse policies into a single national index of climate mitigation or adaptation. Some scholars circumvent these problems by treating international commitments—for example, ratification of the Kyoto Protocol or the UNFCCC—as proxies for climate orientation (Bättig & Bernauer, 2009; Neumayer, 2002; von Stein, 2008). However, because these agreements impose no binding emissions commitments on most aid recipients, and because membership is now nearly universal, ratification alone tells us little about signatories' climate policies.

In the absence of better alternatives, some scholars have begun to analyze national climate policies using a simple count of climate-related laws. For example, Schmidt and Fleig (2018) document the evolution of national climate policies using the Grantham Institute's *Climate Change Laws of the World* database. This is a comprehensive catalog of climate-related laws passed in all countries of the world from 1963 to 2018. Besides providing verbal descriptions, it classifies each law by function: energy supply, energy demand, institutions/administrative arrangements, adaptation, REDD+ and LULUCF, research and development, transportation, and carbon pricing. The appeal of this dataset is clear: it provides an off-the-shelf source of data on various dimensions of climate policy. However, while the Grantham database is an invaluable resource, it does not yet provide enough information to compare the various laws or to aggregate them into a single climate policy index.

To begin with the first point: Grantham provides no information on how consequential the various laws are. For example, Australia's Carbon Farming Initiative Amendment Act of 2014 creates an economy-wide emissions trading system (Parliament of Australia, 2014). This seems more consequential than Israel's "Energy Resources Regulations (Energy labeling of electric heating furnaces), 1993" (State of Israel, 1993), which requires that manufacturers of electric heating furnaces and induction motors provide energy efficiency labels. Similarly, India's National Action Plan on Climate Change (Government of India, n.d.)—which contains specific targets, tax provisions, and subsidies—seems more substantial than Gambia's National Climate Change Policy (Urquhart, 2016), which is a strategy document with no concrete provisions. Because we do not know how important each law is, we have no basis for weighting them before combining them into a summary index. Without obtaining further information, we must rely on a simple count approach, as in Schmidt and Fleig (2018). The drawbacks of this approach are apparent: it is entirely possible that one well-devised climate law—say, a national carbon tax—could have a deeper impact on emissions than dozens of small-bore or aspirational policies. In terms of substantive significance, a simple count of laws could well be meaningless.

In saying this, we do not mean to criticize either the Grantham Institute or Schmidt and Fleig (2018). Assessing these laws' substantive impact would require not only fluency in many languages but also expertise in predicting the country-specific environmental impact of diverse policies. This is a herculean task. Our point, rather, is that relying on a simple count measure—however understandable—probably tells us little about climate policy. We employ this approach here because it is feasible and has been employed elsewhere. However, because this measure probably captures no meaningful variation in climate policy, we do not expect it to be systematically related to climate aid—or, for that matter, to our controls. In other words, we employ it mainly to illustrate the hazards of relying on extant climate policy measures.

In addition to the Grantham measure, we employ Steves and Teytelboym's (2013) CLIMI index. CLIMI is based on policies reported to the UNFCCC between 2005 and 2011. Policies are first sorted into four classifications—international cooperation, domestic climate framework, sector-specific measures, and cross-sectoral measures—then given scores of 0, .5, or 1 depending on how close they come to worldwide best practice. Average scores in each classification are then weighted according to the contribution each makes to worldwide emission reductions, and the weighted scores are summed. The resulting measure ranges from 0 to 1, with higher values implying more ambitious climate policy.

CLIMI remedies some weaknesses of our Grantham measure by weighting policies according to their likely impact. However, CLIMI has serious limitations as well. Perhaps most importantly, it does not incorporate information on policy implementation: hence, CLIMI scores may reflect aspirations as much as substantive policy steps. Second, the weighting scheme is very coarse and cannot capture much of the variation in policy impact. Third, it is a pure cross-section of 95 countries, many of which are developed. This sample limitation is a particular problem when analyzing developing-world aid recipients: in our CLIMI regressions, observations range from 44 to 55. Finally, because the CLIMI index incorporates all policies notified between 2005 and 2011—many of which were passed before 2005—we cannot match the years in which aid was received to the years in which policies were passed.

Again, the point is not to criticize Steves and Teytelboym (2013), who have taken the first steps toward aggregating diverse national policies into a single climate policy index. Rather, it is that the CLIMI index may tell us little about aid recipients' actual climate orientation or its relationship to climate aid. As with the Grantham measure, CLIMI's various sources of measurement error may mask the true relationship between climate aid and climate policy.

In sum, extant measures of climate aid and climate policy lack validity, reliability, or both. Much Rio marker "climate aid" is not actually related to climate, while available measures of climate policy may be weakly related or unrelated to governments' actual efforts to mitigate or adapt to climate change. We therefore do not expect these measures to be related to one another, beyond the occasional significance that can arise from random measurement error.

4. Analysis and Results

Our analysis includes all aid-eligible countries and years for which data were available for all variables. The resulting sample sizes vary widely, from a minimum of 44 in some cross-sectional CLIMI regressions to a maximum of 951 in our panel analyses. The number of countries ranges from 44 to 100, depending on the analysis, while the longitudinal coverage is 1996 to 2009 for the Grantham analyses and 2005 to 2011 for the CLIMI analyses.[5]

We employ three dependent variables based on Grantham data. *Climate Policy*$_{it}$ is a cumulative count of country i's climate laws in year t. *Mitigation Policy*$_{it}$ is a cumulative count of country i's mitigation laws, and *Adaptation Policy*$_{it}$ is a cumulative count of i's adaptation laws.[6] Because we include a lagged dependent variable in all panel analyses, we are predicting the likelihood that country i will pass additional laws from time $t-1$ to time t. As a robustness check, we also employ *CLIMI*$_i$, country i's CLIMI score, as a dependent variable.

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[5] These years refer to the dependent variable observations. Aid, which is lagged by one year, covers 1995 to 2008.
[6] Grantham has an explicit category for adaptation. We treat all remaining laws as mitigation-related.

We employ six climate aid variables, all from Aid-Data's PLAID 1.9.2 database (Tierney et al., 2011). In the original data, the Rio markers code projects as 0 if they have no climate relevance, 1 if climate is a significant objective, and 2 if climate is the main objective. We combine the latter two outcomes to create a dichotomous indicator of whether aid projects are climate-related or not. We sum all climate aid for each recipient country and year, divide by population to obtain aid per capita, and log this measure to reduce skewness.[7] We construct measures for both aid commitments and disbursements in case these two variables have distinct effects.[8] $Ln(Climate\ Aid\ Commitments_{it-1})$ and $Ln(Climate\ Aid\ Disbursements_{it-1})$ are our first two measures of climate aid. We lag these and all other right-hand side variables by one period to ensure that the independent variables are realized before the dependent variable.

Michaelowa and Michaelowa (2011) additionally classify aid projects as mitigation-related and adaptation-related. Using their coding, we construct four additional aid variables: $Ln(Mitigation\ Aid\ Commitments_{it-1})$, $Ln(Mitigation\ Aid\ Disbursements_{it-1})$, $Ln(Adaptation\ Aid\ Commitments_{it-1})$, and $Ln(Adaptation\ Aid\ Disbursements_{it-1})$.

We include a number of controls. First, we include $Ln(Other\ ODA_{it-1})$, recipient i's non-climate aid in year $t-1$. This is simply the log of i's total per capita ODA minus climate aid. We include this so our climate aid variables do not spuriously capture the effects of development aid more generally.[9] We include $Ln(GDP\ Per\ Capita_{it-1})$ to control for economic development, and its quadratic term, $Ln(GDP\ Per\ Capita_{it-1}^2)$, in case there exists an environmental Kuznets curve for climate policies.[10] We include $Growth_{it-1}$, the GDP per capita growth rate, in case faster growth makes it politically easier to pass climate legislation.[11] We include $Ln(Government\ Spending_{it-1})$, logged government consumption spending as a percent of GDP, as a proxy for government ideology (left-leaning governments should spend more than right-leaning ones).[12] We include $Ln(Fossil\ Fuels_{it-1})$, the logged percent of electricity provided by oil, gas and coal, in case fossil-fuel dependency makes it harder to pass climate laws.[13] We include $Ln(Openness_{it-1})$, logged trade as a percent of GDP, in case trade openness affects cli-

mate politics by exposing carbon-intensive industries to international competition.[14] We include $Polity_{it-1}$, country i's Polity score, in case domestic regime type affects the provision of climate policies.[15] Finally, we include a lagged dependent variable in all panel analyses.

Because our primary dependent variable is a count of climate laws, we employ Poisson regressions, which are more appropriate for count data than linear regression. We employ recipient fixed effects in all panel analyses, which eliminates unobserved cross-national variation and allows us to focus on the within-country relationship between aid and climate policies. We also include year fixed effects to control for unobserved year-specific shocks or trends. To reduce the influence of outliers, we perform jackknife regressions in all models.[16] To address serial correlation, we employ robust country-clustered standard errors. Aid coefficients based on annual panels are shown in Figure 1.

To save space, we present our climate aid results graphically and relegate control-variable results to the Appendix. Figure 1(a) shows coefficients for the six climate aid variables when the dependent variable is all climate policies, with 95 percent confidence intervals indicated by the error bars. Figures 1(b) and 1(c) present the same information when the dependent variables are mitigation and adaptation policies, respectively. In nearly all cases, the error bars include the value of zero, indicated by the red vertical lines. Our measures of climate aid are thus, for the most part, not significantly related to climate policies. Of the 18 aid coefficients, only three are statistically significant. Climate aid commitments, as defined by the Rio marker, predict significantly more climate policies overall as well as more adaptation policies. One possible interpretation of these results is that they are driven by adaptation aid, which should predict adaptation but not mitigation policy. However, the results for mitigation and adaptation aid commitments cast doubt on this interpretation. Mitigation commitments predict adaptation policies but not mitigation policies, while adaptation commitments do not predict adaptation policies. This is the opposite of what we would expect if our variables were well-measured and aid was achieving its stated goals. In sum, our most consistent predictor is Rio-marked climate aid—which has well-documented mea-

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[7] We add 1 before logging to keep observations with zero aid.

[8] Commitments should matter more if recipients are motivated by the promise of aid, while disbursements should matter more if recipients require aid in hand to pass legislation.

[9] Correlations between climate and non-climate aid range from .13 to .33.

[10] A number of studies find evidence of an EKC for CO_2 emissions. See Apergis and Ozturk (2015) for a recent example.

[11] Ward and Cao (2012) find that high unemployment leads to lower "green taxes," implying that recessions reduce support for environmental regulations. We employ growth rates rather than unemployment because data on the latter are unavailable for many aid recipients.

[12] Ward and Cao (2012) find that left governments adopt higher levels of "green taxes," including taxes on emissions. We employ government spending as a proxy because direct ideology data are unavailable for many aid recipient.

[13] Steves and Teytelboym (2013) find that a larger carbon-intensive sector leads to less stringent climate mitigation, presumably because carbon-intensive industries oppose such policies. Our measure captures consumers' interest in cheap fossil fuels as well as the interests of fossil fuel energy providers.

[14] Systematic research on this point is lacking, but international competitiveness concerns are common in debates about climate mitigation. For example, in 2009, two U.S. Senators wrote in a *New York Times* op-ed that "climate change is real and threatens our economy and national security…[but] we cannot sacrifice another job to competitors overseas" (Kerry & Graham, 2009).

[15] Bättig and Bernauer (2009) find that democracies have a higher output of climate-friendly policies. Polity scores are from the Polity IV Project (Center for Systemic Peace, n.d.). All other controls are from the World Bank's World Development Indicators (n.d.).

[16] The jackknife procedure sequentially drops each country from the data and reports the average of the estimates.
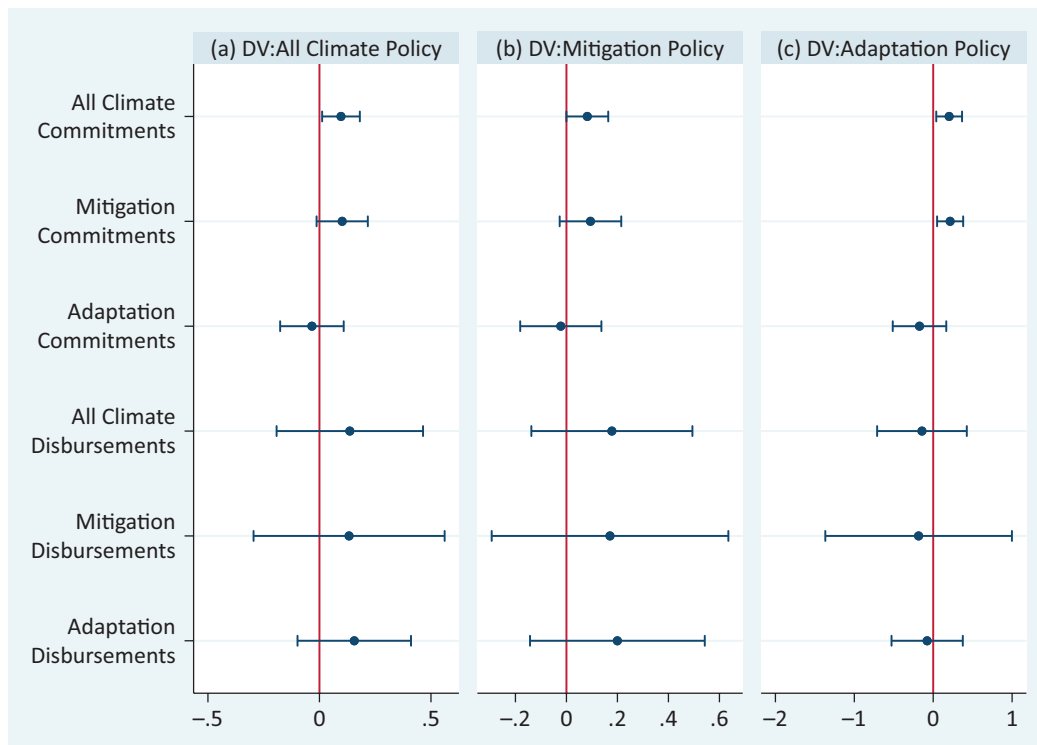
Figure 1. Climate aid and Grantham climate policy, annual panels.

surement problems—while the only other significant coefficient is anomalous. These results, along with the 15 insignificant coefficients, raise doubts about whether the few significant coefficients are substantively meaningful.

Climate policies are "sticky," in that they are not passed frequently. Regressing year-to-year changes in climate policy against year-to-year changes in foreign aid may thus not constitute a fair test. To allow for the possibility that aid affects climate policy with long and varying lags, we perform two additional analyses. First, following Knack (2004), we regress the entire-period change in climate policies—that is, the change between the first and last periods—against the whole-period average for climate aid and the controls. Both the dependent and the independent variables are thus collapsed into a single cross-section. The idea is that, while it may be difficult to match climate policy in year t with aid in any particular $t - n$, countries that receive more aid over the entire period may exhibit larger changes in climate policy. Results of this analysis are shown in Figure 2.

Figure 2's structure is the same as Figure 1's, with aid variables presented on the left and sub-graphs (a), (b) and (c) showing coefficients and confidence intervals for all climate, mitigation and adaptation policies, respectively. The results are easily summarized: of the 18 coefficients, none are significant.

Second, we collapse the data into four-year panels, with each variable taking the average for each period. We thus regress average climate policy in four-year pe-

riod t against average aid in four-year period $t - 1$. In addition to being less noisy, the four-year averages allow for longer lags in the effects of aid. Because the averaged dependent variables no longer take on integer values, we employ ordinary least-squares rather than Poisson regression for this analysis. Otherwise, it is identical to our earlier panel regressions. Results are shown in Figure 3. Again, climate aid is never significantly related to climate policy.[17]

As noted earlier, our Grantham measure treats all climate laws as equal, regardless of how much they matter in practice. A simple count of climate laws probably misstates the degree to which governments are addressing climate change. We thus perform additional analyses using the CLIMI measure, which weights government policies according to their likely impact. One challenge in using this measure is that it is a cross-section based on policies reported from 2005 to 2011. Not only does it incorporate multiple reporting years, but many reported policies were passed before reporting began in 2005. This makes it difficult to say what years should be included in the cross-sectional measure of climate aid. Because the appropriate period is not obvious, we employ two for robustness, averaging climate aid for 2000–2005—the five years before reporting began—and 2005–2008, the years in which reporting occurred and for which we have aid data. Results are shown in Figure 4. The CLIMI results tell the same story as previous ones: of the 12 climate aid coefficients, none are significant.

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[17] Because the number of four-year periods in each panel is small, combining a lagged dependent variable and recipient fixed effects raises concerns about Nickell bias. We note that we obtain very similar results both with and without the lagged dependent variable and country fixed effects.
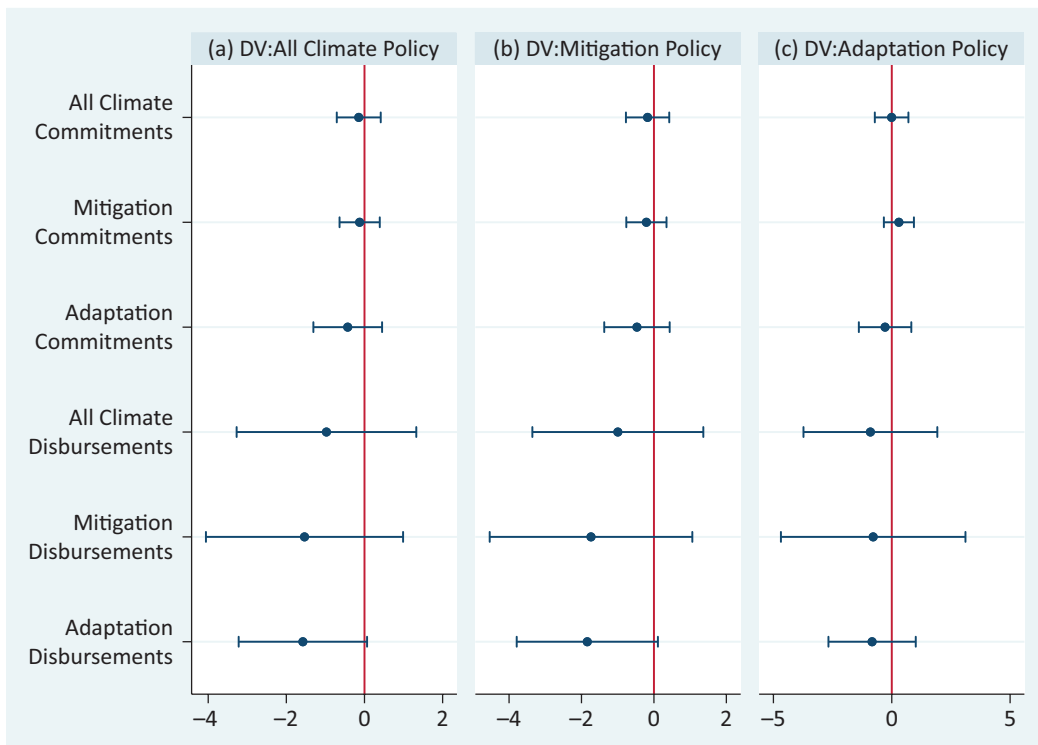
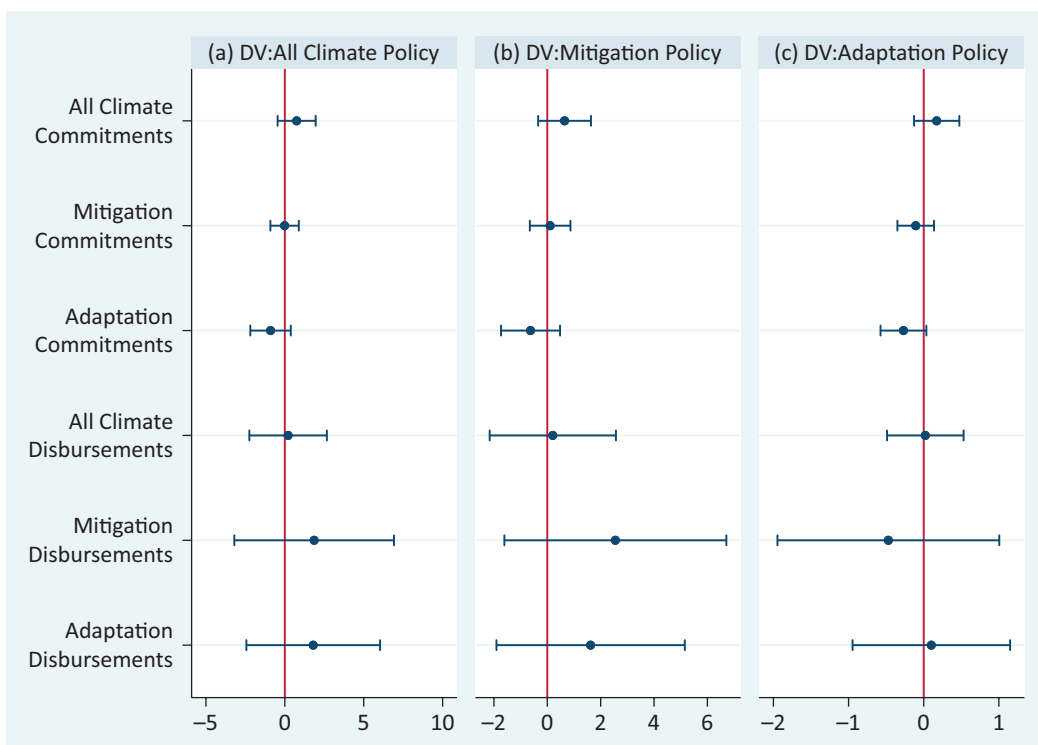**Figure 2.** Climate aid and change in Grantham climate policies.

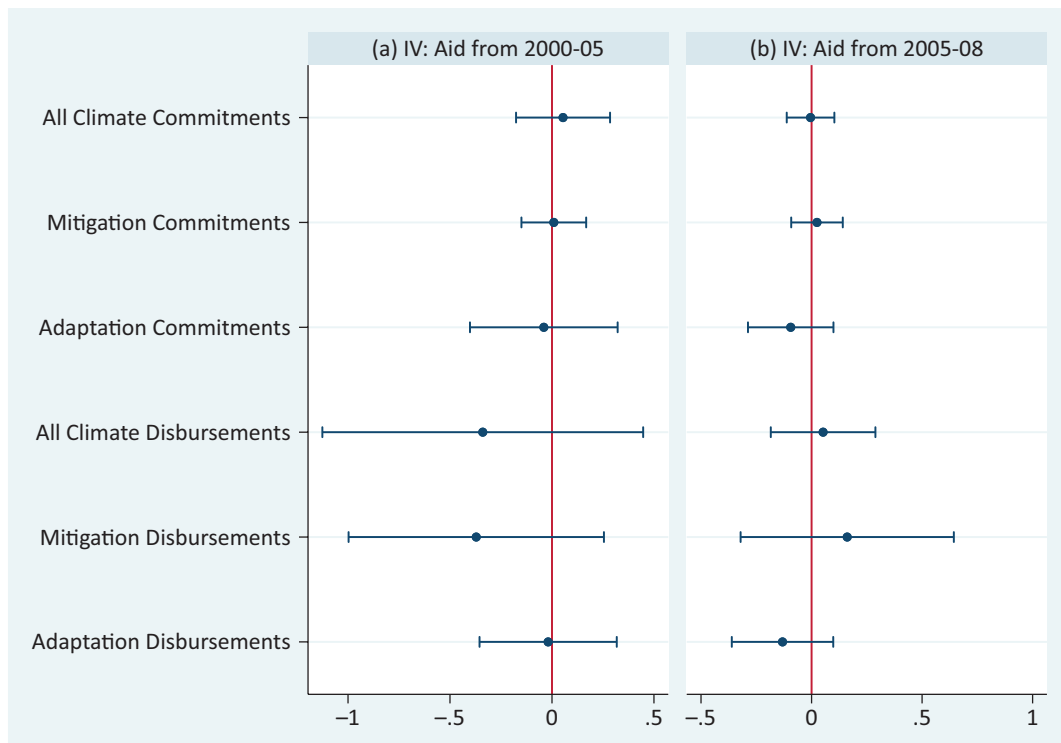**Figure 3.** Climate aid and Grantham climate policy, 4-year panels.

Figure 4. Climate aid and CLIMI.

In all, we estimate 66 climate aid coefficients. Of these, only three (4.5 percent) were significant. This could mean several things. First, climate aid may simply not affect climate policy. Second, the impact of aid may be masked by endogeneity bias. Research on climate aid allocation suggests that it is influenced by recipient-country characteristics (Bagchi et al., 2016; Betzold & Weiler, 2017; Halimanjaya & Papyrakis, 2015; Hicks et al., 2008; Klöck et al., 2018; Weiler et al., 2018), and this non-random allocation could bias our results in either a positive or negative direction. As we have been unable to find an instrument that strongly predicts recipient-year variation in climate aid, we cannot rule such bias out.

Finally, we may be unable to find a relationship because our variables are poorly measured. We are inclined toward this explanation. The Grantham measure lacks validity for reasons discussed above, and while the CLIMI index is arguably more valid and reliable, its multi-year cross-sectional structure does not allow us to match it with aid (or other variables) from any particular year. The Rio markers for climate aid are also known to lack validity and reliability, and while Michaelowa and Michaelowa's (2011) aid data are arguably more accurate, this may not matter when employing dependent variables of questionable validity. In this context, it is worth noting that our control-variable results (shown in the Appendix) are nearly all insignificant as well. Since previous research suggests that these controls should matter for climate policy, this also suggests that our broadly insignificant results reflect measurement error in our dependent variable.

5. Conclusion

Foreign aid for climate mitigation and adaptation has risen dramatically in recent years, reflecting widespread concerns about climate change. As with all aid, it is important to assess whether climate aid "works" in the sense of actually promoting mitigation and adaptation. The central message of this article is that we are not yet in a position to make this assessment due to the poor quality of climate aid and climate policy data. Climate aid measured with Rio markers appears to lack both validity and reliability, as donors over-report their efforts to varying degrees, and there is no system in place for verifying their claims. Extant measures of climate policy also lack validity, as they do not meaningfully aggregate diverse policies into a single cross-nationally comparable metric. We wish to draw attention to these data limitations, not only because they impede our study of this important issue, but also because these data are increasingly employed in studies of both climate aid allocation (Bagchi et al., 2016; Betzold & Weiler, 2017; Halimanjaya & Papyrakis, 2015; Klöck et al., 2018; Weiler et al., 2018) and climate policy (Schmidt & Fleig, 2018). We must ask whether research based on these data paint a misleading picture of developments in both policy domains.

Of these measurement problems, those involved with climate aid data should be easier to remedy. Michaelowa and Michaelowa (2011) have already made progress toward more accurately coding such aid, and other studies on this topic offer helpful suggestions (Donner et al., 2016; Junghans & Harmeling, 2012;

Michaelowa & Michaelowa, 2011; Roberts & Weikmans, 2017; Weikmans et al., 2017). Without minimizing the difficulties involved, the challenge here is essentially just identifying aid whose core purpose is clearly climate-relevant. In contrast, constructing more accurate measures of climate policy will be exceedingly difficult. Doing so will require scholars to identify the relevant provisions of diverse national policies, to estimate their likely impact on carbon emissions or other climate-related outcomes, and to aggregate these estimated effects into summary indices of climate policy. Although this task will be difficult, it is encouraging to note that scholars have accomplished similar feats in other policy domains: for example, aggregating diverse non-tariff barriers to trade into ad valorem tariff equivalents (Looi Kee et al., 2009). To reach meaningful conclusions about the aid-climate policy relationship—or climate policy more generally—scholars in this area will need to undertake similar efforts.

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Conflict of Interests

The authors declare no conflict of interests.

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Appendix

Table A0. Summary statistics.

Variable	Obs.	Mean	S.D.	Min.	Max.
Climate Policy$_{it}$	951	3.32	4.37	0	34
Mitigation Policy$_{it}$	951	2.91	3.93	0	29
Adaptation Policy$_{it}$	951	0.41	0.80	0	6
CLIMIi	55	0.30	0.18	0.02	0.70
Ln(Climate Aid Commitments$_{it}$)	756	0.42	0.62	0	3.80
Ln(Mitigation Aid Commitments$_{it}$)	862	0.34	0.61	0	3.80
Ln(Adaptation Aid Commitments$_{it}$)	862	0.21	0.44	0	3.91
Ln(Climate Aid Disbursements$_{it}$)	741	0.08	0.17	0	1.36
Ln(Mitigation Aid Disbursements$_{it}$)	831	0.04	0.12	0	1.11
Ln(Adaptation Aid Disbursements$_{it}$)	852	0.07	0.21	0	3.61
Ln(Other ODA$_{it}$) (Commitments)	862	3.78	0.95	0.65	6.99
Ln(Other ODA$_{it}$) (Disbursements)	813	1.89	1.16	0.03	6.61
Ln(GDP Per Capita$_{it}$)	951	7.84	1.01	5.35	10.19
Ln(GDP Per Capita$^2_{it}$)	951	62.40	15.62	28.58	103.74
Growth$_{it}$	951	3.13	4.54	−19.06	33.03
Ln(Government Spending$_{it}$)	950	2.60	0.39	0.72	4.24
Ln(Fossil Fuels$_{it}$)	947	3.62	1.43	−3.83	4.61
Ln(Openness$_{it}$)	951	4.23	0.43	2.75	5.30
Polity$_{it}$	951	3.64	5.82	−9	10

Table A1(a). Climate aid and Grantham climate policy, annual panels.

	Climate Aid Variable					
	All Climate Commitments (Rio)	Mitigation Commitments (Michaelowa)	Adaptation Commitments (Michaelowa)	All Climate Disbursements (Rio)	Mitigation Disbursements (Michaelowa)	Adaptation Disbursements (Michaelowa)
Climate Policy$_{it-1}$	0.06**	0.04	0.04	0.05**	0.05**	0.05*
	(0.01)	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)
Ln(GDP Per Capita$_{it-1}$)	−1.02	−3.24	−3.02	−1.41	−1.72	−2.64
	(3.29)	(3.45)	(3.54)	(2.93)	(3.85)	(3.70)
Ln(GDP Per Capita$^2_{it-1}$)	0.12	0.24	0.22	0.16	0.16	0.22
	(0.21)	(0.22)	(0.22)	(0.19)	(0.25)	(0.24)
Growth$_{it-1}$	−0.00	0.00	0.00	−0.00	0.00	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Ln(Government Spending$_{it-1}$)	−0.31	−0.19	−0.19	−0.37	−0.35	−0.22
	(0.23)	(0.31)	(0.31)	(0.23)	(0.26)	(0.28)
Ln(Fossil Fuels$_{it-1}$)	−0.04	−0.02	−0.02	−0.05	−0.02	−0.02
	(0.07)	(0.08)	(0.08)	(0.06)	(0.08)	(0.08)
Ln(Openness$_{it-1}$)	0.02	0.33	0.34	−0.06	0.20	0.21
	(0.27)	(0.31)	(0.32)	(0.27)	(0.28)	(0.28)
Polity$_{it-1}$	−0.00	0.01	0.01	−0.01	0.01	0.01
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Ln(Other ODA$_{it-1}$)	0.05	−0.00	0.01	−0.04	−0.04	−0.02
	(0.06)	(0.09)	(0.09)	(0.04)	(0.04)	(0.03)
Ln(Climate Aid$_{it-1}$)	0.10*	0.10	−0.03	0.14	0.13	0.16
	(0.04)	(0.06)	(0.07)	(0.16)	(0.21)	(0.13)
Observations (Countries)	775 (74)	951 (83)	951 (83)	751 (74)	845 (75)	875 (75)
F (Prob > F)	25.0 (.000)	15.7 (.000)	13.9 (.000)	42.8 (.000)	29.5 (.000)	20.3 (.000)

Notes: Dependent variable: Climate Policy$_{it}$; Robust (country-clustered) standard errors in parentheses, **p < .01 * p < .05; All models include country and year fixed effects.

Table A1(b). Climate aid and Grantham mitigation policy, annual panels.

	Climate Aid Variable					
	All Climate Commitments (Rio)	Mitigation Commitments (Michaelowa)	Adaptation Commitments (Michaelowa)	All Climate Disbursements (Rio)	Mitigation Disbursements (Michaelowa)	Adaptation Disbursements (Michaelowa)
Climate Policy$_{it-1}$	0.06**	0.03	0.04	0.05**	0.06**	0.05*
	(0.01)	(0.04)	(0.04)	(0.01)	(0.02)	(0.02)
Ln(GDP Per Capita$_{it-1}$)	−0.73	−2.77	−2.59	−0.88	−1.11	−2.06
	(2.92)	(3.19)	(3.29)	(2.61)	(3.39)	(3.28)
Ln(GDP Per Capita$^2_{it-1}$)	0.12	0.21	0.20	0.13	0.12	0.19
	(0.19)	(0.20)	(0.21)	(0.17)	(0.22)	(0.22)
Growth$_{it-1}$	−0.01	−0.00	−0.00	−0.01	−0.00	−0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Ln(Government Spending$_{it-1}$)	−0.27	−0.14	−0.13	−0.34	−0.29	−0.16
	(0.22)	(0.31)	(0.31)	(0.22)	(0.25)	(0.28)
Ln(Fossil Fuels$_{it-1}$)	−0.03	−0.01	−0.02	−0.03	−0.01	−0.01
	(0.07)	(0.08)	(0.08)	(0.06)	(0.09)	(0.09)
Ln(Openness$_{it-1}$)	0.17	0.47	0.47	0.05	0.30	0.33
	(0.27)	(0.33)	(0.34)	(0.27)	(0.28)	(0.28)
Polity$_{it-1}$	−0.00	0.01	0.01	−0.01	0.01	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Ln(Other ODA$_{it-1}$)	0.05	−0.00	0.01	−0.04	−0.04	−0.02
	(0.06)	(0.09)	(0.09)	(0.04)	(0.04)	(0.03)
Ln(Climate Aid$_{it-1}$)	0.08	0.09	−0.02	0.18	0.17	0.20
	(0.04)	(0.06)	(0.08)	(0.16)	(0.23)	(0.17)
Observations (Countries)	767 (73)	942 (82)	942 (82)	743 (73)	836 (74)	867 (74)
F (Prob > F)	24.9 (.000)	14.6 (.000)	13.0 (.000)	26.4 (.000)	24.7 (.000)	21.2 (.000)

Notes: Dependent variable: Mitigation Policy$_{it}$; Robust (country-clustered) standard errors in parentheses, **$p < .01$ * $p < .05$; All models include country and year fixed effects.

Table A1(c). Climate aid and Grantham adaptation policy, annual panels.

	Climate Aid Variable					
	All Climate Commitments (Rio)	Mitigation Commitments (Michaelowa)	Adaptation Commitments (Michaelowa)	All Climate Disbursements (Rio)	Mitigation Disbursements (Michaelowa)	Adaptation Disbursements (Michaelowa)
Climate Policy$_{it-1}$	0.24 (0.13)	0.20 (0.13)	0.23 (0.12)	0.27* (0.13)	0.30* (0.13)	0.25 (0.14)
Ln(GDP Per Capita$_{it-1}$)	−5.64 (7.21)	−7.93 (7.31)	−7.04 (7.25)	−8.72 (6.98)	−7.15 (7.86)	−7.55 (7.75)
Ln(GDP Per Capita$^2_{it-1}$)	0.32 (0.45)	0.48 (0.46)	0.41 (0.46)	0.52 (0.44)	0.43 (0.49)	0.46 (0.49)
Growth$_{it-1}$	0.02 (0.02)	0.02 (0.02)	0.02 (0.02)	0.03 (0.03)	0.02 (0.02)	0.02 (0.02)
Ln(Government Spending$_{it-1}$)	−1.01* (0.42)	−1.15* (0.49)	−1.19* (0.47)	−0.94* (0.41)	−1.18* (0.48)	−1.20* (0.50)
Ln(Fossil Fuels$_{it-1}$)	−0.11 (0.10)	−0.09 (0.13)	−0.12 (0.11)	−0.17 (0.09)	−0.11 (0.12)	−0.12 (0.12)
Ln(Openness$_{it-1}$)	−1.31 (0.70)	−0.93 (0.64)	−0.83 (0.66)	−1.15 (0.71)	−0.83 (0.71)	−0.77 (0.65)
Polity$_{it-1}$	−0.01 (0.06)	0.00 (0.06)	0.00 (0.06)	−0.04 (0.06)	−0.00 (0.06)	0.01 (0.06)
Ln(Other ODA$_{it-1}$)	−0.04 (0.12)	−0.00 (0.11)	0.03 (0.10)	−0.06 (0.08)	−0.07 (0.08)	−0.03 (0.09)
Ln(Climate Aid$_{it-1}$)	0.20* (0.08)	0.21* (0.08)	−0.17 (0.17)	−0.14 (0.28)	−0.19 (0.59)	−0.08 (0.22)
Observations (Countries)	531 (50)	635 (53)	635 (53)	509 (50)	578 (50)	598 (50)
F (Prob > F)	12.0 (.000)	20.4 (.000)	17.6 (.000)	14.8 (.000)	11.6 (.000)	19.7 (.000)

Notes: Dependent variable: Adaptation Policy$_{it}$; Robust (country-clustered) standard errors in parentheses, * $p < .05$; All models include country and year fixed effects.

Table A2(a). Climate aid and change in Grantham climate policies.

	Climate Aid Variable					
	All Climate Commitments (Rio)	Mitigation Commitments (Michaelowa)	Adaptation Commitments (Michaelowa)	All Climate Disbursements (Rio)	Mitigation Disbursements (Michaelowa)	Adaptation Disbursements (Michaelowa)
Ln(GDP Per Capita$_i$)	1.21	1.07	1.09	2.34	2.46*	2.16
	(1.20)	(0.95)	(0.96)	(1.22)	(1.14)	(1.14)
Ln(GDP Per Capita$_i^2$)	−0.08	−0.07	−0.07	−0.16	−0.17*	−0.15
	(0.08)	(0.06)	(0.06)	(0.08)	(0.07)	(0.07)
Growth$_i$	0.04	0.03	0.03	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Ln(Government Spending$_i$)	−0.21	−0.21	−0.22	−0.09	−0.02	−0.05
	(0.31)	(0.28)	(0.28)	(0.32)	(0.31)	(0.31)
Ln(Fossil Fuels$_i$)	−0.01	0.01	0.01	−0.02	−0.02	−0.00
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)
Ln(Openness$_i$)	−0.31	−0.30	−0.31	−0.35	−0.32	−0.34
	(0.30)	(0.29)	(0.29)	(0.33)	(0.32)	(0.31)
Polity$_i$	0.06**	0.06**	0.06**	0.06**	0.06**	0.06**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Ln(Other ODA$_i$)	−0.18*	−0.20*	−0.18	−0.27*	−0.36**	−0.28*
	(0.09)	(0.09)	(0.11)	(0.11)	(0.10)	(0.11)
Ln(Climate Commitments$_i$)	−0.15	−0.12	−0.43	−0.97	−1.53	−1.58
	(0.28)	(0.26)	(0.44)	(1.16)	(1.27)	(0.83)
Constant	−0.32	0.10	0.15	−4.51	−5.10	−3.93
	(4.83)	(3.97)	(3.95)	(4.88)	(4.55)	(4.50)
Observations	91	100	100	89	91	92
F (Prob > F)	3.60 (.001)	3.86 (.000)	4.50 (.000)	5.12 (.000)	5.95 (.000)	6.25 (.000)

Notes: Dependent variable: Change in Climate Policy from 1995 to 2009; Robust standard errors in parentheses, **$p < .01$ * $p < .05$.

Table A2(b). Climate aid and change in Grantham mitigation policies.

	Climate Aid Variable					
	All Climate Commitments (Rio)	Mitigation Commitments (Michaelowa)	Adaptation Commitments (Michaelowa)	All Climate Disbursements (Rio)	Mitigation Disbursements (Michaelowa)	Adaptation Disbursements (Michaelowa)
Ln(GDP Per Capita$_i$)	1.00	0.90	0.85	2.31	2.37*	2.01
	(1.29)	(1.02)	(1.02)	(1.28)	(1.16)	(1.18)
Ln(GDP Per Capita$_i^2$)	−0.07	−0.06	−0.06	−0.16	−0.16*	−0.14
	(0.09)	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)
Growth$_i$	0.04	0.04	0.03	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Ln(Government Spending$_i$)	−0.09	−0.11	−0.11	0.05	0.11	0.09
	(0.32)	(0.28)	(0.29)	(0.33)	(0.31)	(0.31)
Ln(Fossil Fuels$_i$)	−0.01	0.01	0.01	−0.02	−0.02	−0.01
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)
Ln(Openness$_i$)	−0.28	−0.28	−0.30	−0.34	−0.30	−0.32
	(0.31)	(0.29)	(0.30)	(0.34)	(0.32)	(0.31)
Polity$_i$	0.06**	0.06**	0.06**	0.06**	0.06**	0.06**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Ln(Other ODA$_i$)	−0.21*	−0.22*	−0.20	−0.31**	−0.40**	−0.32**
	(0.09)	(0.10)	(0.11)	(0.12)	(0.10)	(0.11)
Ln(Climate Commitments$_i$)	−0.17	−0.21	−0.47	−1.00	−1.74	−1.84
	(0.30)	(0.28)	(0.46)	(1.19)	(1.41)	(0.98)
Constant	0.01	0.38	0.71	−4.79	−5.17	−3.82
	(5.17)	(4.29)	(4.25)	(5.19)	(4.73)	(4.73)
Observations	91	100	100	89	91	92
F (Prob > F)	2.93 (.004)	3.05 (.003)	3.70 (.001)	4.72 (.000)	6.07 (.000)	6.05 (.000)

Notes: Dependent variable: Change in Mitigation Policy from 1995 to 2009; Robust standard errors in parentheses, **p < .01 * p < .05.

Table A2(c). Climate aid and change in Grantham adaptation policies.

	Climate Aid Variable					
	All Climate Commitments (Rio)	Mitigation Commitments (Michaelowa)	Adaptation Commitments (Michaelowa)	All Climate Disbursements (Rio)	Mitigation Disbursements (Michaelowa)	Adaptation Disbursements (Michaelowa)
$Ln(GDP\ Per\ Capita_i)$	2.61 (1.83)	2.23 (1.57)	2.68 (1.58)	2.63 (1.79)	3.18 (1.87)	3.17 (1.80)
$Ln(GDP\ Per\ Capita_i^2)$	−0.16 (0.12)	−0.14 (0.10)	−0.17 (0.10)	−0.16 (0.11)	−0.20 (0.12)	−0.20 (0.11)
$Growth_i$	0.03 (0.06)	0.00 (0.05)	0.02 (0.05)	0.03 (0.06)	0.03 (0.06)	0.02 (0.06)
$Ln(Government\ Spending_i)$	−0.90* (0.42)	−0.80* (0.39)	−0.82* (0.39)	−0.85* (0.42)	−0.78 (0.42)	−0.83 (0.43)
$Ln(Fossil\ Fuels_i)$	0.01 (0.08)	0.02 (0.08)	0.02 (0.08)	0.01 (0.08)	0.01 (0.09)	0.02 (0.08)
$Ln(Openness_i)$	−0.51 (0.40)	−0.44 (0.39)	−0.41 (0.39)	−0.44 (0.40)	−0.43 (0.41)	−0.47 (0.41)
$Polity_i$	0.06* (0.03)	0.06* (0.02)	0.06* (0.02)	0.06* (0.03)	0.06* (0.02)	0.05* (0.02)
$Ln(Other\ ODA_i)$	−0.04 (0.17)	−0.12 (0.15)	−0.05 (0.18)	−0.07 (0.17)	−0.15 (0.17)	−0.10 (0.18)
$Ln(Climate\ Commitments_i)$	−0.01 (0.36)	0.30 (0.32)	−0.28 (0.56)	−0.90 (1.42)	−0.78 (1.96)	−0.83 (0.93)
Constant	−6.05 (6.84)	−4.90 (6.03)	−6.73 (6.05)	−6.46 (6.78)	−8.40 (7.05)	−8.17 (6.83)
Observations	91	100	100	89	91	92
F (Prob > F)	2.78 (.006)	2.67 (.008)	2.92 (.004)	2.79 (.006)	2.70 (.008)	2.86 (.005)

Notes: Dependent variable: Change in Adaptation Policy from 1995 to 2009; Robust standard errors in parentheses, * $p < .05$.

Table A3(a). Climate aid and Grantham climate policy, 4-year panels.

	Climate Aid Variable					
	All Climate Commitments (Rio)	Mitigation Commitments (Michaelowa)	Adaptation Commitments (Michaelowa)	All Climate Disbursements (Rio)	Mitigation Disbursements (Michaelowa)	Adaptation Disbursements (Michaelowa)
Climate Policy$_{it-1}$	0.75**	0.78**	0.77**	0.69**	0.72**	0.72**
	(0.22)	(0.17)	(0.17)	(0.21)	(0.17)	(0.16)
Ln(GDP Per Capita$_{it-1}$)	−3.63	−5.81	−5.63	−4.70	−1.93	1.90
	(11.09)	(9.50)	(9.44)	(11.51)	(10.81)	(11.18)
Ln(GDP Per Capita$^2_{it-1}$)	0.29	0.47	0.45	0.40	0.18	−0.05
	(0.73)	(0.59)	(0.58)	(0.77)	(0.71)	(0.74)
Growth$_{it-1}$	0.16	0.11	0.11	0.10	0.09	0.07
	(0.10)	(0.08)	(0.08)	(0.10)	(0.10)	(0.09)
Ln(Government Spending$_{it-1}$)	0.16	0.06	0.15	0.58	0.34	0.21
	(1.66)	(1.51)	(1.50)	(1.49)	(1.49)	(1.47)
Ln(Fossil Fuels$_{it-1}$)	−0.05	−0.08	−0.09	0.14	0.09	0.12
	(0.41)	(0.41)	(0.41)	(0.46)	(0.46)	(0.42)
Ln(Openness$_{it-1}$)	0.55	0.41	0.35	0.05	0.24	0.20
	(1.96)	(1.89)	(1.83)	(1.85)	(2.20)	(2.05)
Polity$_{it-1}$	0.08	0.09	0.08	0.08	0.08	0.07
	(0.08)	(0.08)	(0.08)	(0.07)	(0.08)	(0.07)
Ln(Other ODA$_{it-1}$)	−0.43	−0.16	−0.13	−0.87	−0.69	−0.83
	(0.47)	(0.40)	(0.38)	(0.60)	(0.51)	(0.56)
Ln(Climate Aid$_{it-1}$)	0.75	−0.02	−0.90	0.21	1.86	1.80
	(0.61)	(0.45)	(0.65)	(1.24)	(2.55)	(2.13)
Constant	12.32	18.26	17.79	14.91	6.34	−8.38
	(42.42)	(38.91)	(38.73)	(43.90)	(41.27)	(42.02)
Observations (Countries)	228 (86)	266 (97)	266 (97)	228 (86)	238 (89)	240 (90)
R-Squared	0.67	0.70	0.70	0.69	0.69	0.70

Notes: Dependent variable: Climate Policy$_{it}$; Robust (country-clustered) standard errors in parentheses, **p < .01; All models include country and year fixed effects.

Table A3(b). Climate aid and Grantham mitigation policy, 4-year panels.

	Climate Aid Variable					
	All Climate Commitments (Rio)	Mitigation Commitments (Michaelowa)	Adaptation Commitments (Michaelowa)	All Climate Disbursements (Rio)	Mitigation Disbursements (Michaelowa)	Adaptation Disbursements (Michaelowa)
Climate Policy$_{it-1}$	0.78**	0.79**	0.78**	0.71**	0.72**	0.72**
	(0.25)	(0.18)	(0.18)	(0.24)	(0.18)	(0.17)
Ln(GDP Per Capita$_{it-1}$)	−0.63	−4.36	−4.37	−1.56	0.05	3.51
	(9.76)	(8.53)	(8.51)	(10.07)	(9.48)	(9.80)
Ln(GDP Per Capita$^2_{it-1}$)	0.07	0.36	0.36	0.17	0.03	−0.18
	(0.64)	(0.53)	(0.52)	(0.67)	(0.62)	(0.65)
Growth$_{it-1}$	0.15	0.11	0.11	0.11	0.09	0.07
	(0.10)	(0.08)	(0.08)	(0.09)	(0.09)	(0.08)
Ln(Government Spending$_{it-1}$)	0.30	0.23	0.31	0.62	0.42	0.38
	(1.43)	(1.27)	(1.26)	(1.25)	(1.24)	(1.18)
Ln(Fossil Fuels$_{it-1}$)	−0.04	−0.07	−0.07	0.13	0.10	0.13
	(0.37)	(0.37)	(0.37)	(0.41)	(0.42)	(0.37)
Ln(Openness$_{it-1}$)	0.56	0.41	0.37	0.15	0.29	0.23
	(1.83)	(1.77)	(1.72)	(1.75)	(2.07)	(1.89)
Polity$_{it-1}$	0.06	0.07	0.07	0.06	0.06	0.05
	(0.06)	(0.06)	(0.06)	(0.05)	(0.06)	(0.05)
Ln(Other ODA$_{it-1}$)	−0.33	−0.11	−0.08	−0.73	−0.63	−0.76
	(0.39)	(0.33)	(0.32)	(0.47)	(0.41)	(0.43)
Ln(Climate Aid$_{it-1}$)	0.65	0.11	−0.63	0.21	2.55	1.62
	(0.50)	(0.38)	(0.56)	(1.19)	(2.09)	(1.78)
Constant	1.32	12.67	12.86	3.80	−0.70	−14.08
	(37.60)	(35.10)	(35.11)	(38.61)	(36.25)	(37.00)
Observations (Countries)	228 (86)	266 (97)	266 (97)	228 (86)	238 (89)	240 (90)
R-Squared	0.67	0.69	0.69	0.68	0.69	0.70

Notes: Dependent variable: Mitigation Policy$_{it}$; Robust (country-clustered) standard errors in parentheses, **p < .01; All models include country and year fixed effects.

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Table A3(c). Climate aid and Grantham adaptation policy, 4-year panels.

	Climate Aid Variable					
	All Climate Commitments (Rio)	Mitigation Commitments (Michaelowa)	Adaptation Commitments (Michaelowa)	All Climate Disbursements (Rio)	Mitigation *Disbursements* (Michaelowa)	Adaptation Disbursements (Michaelowa)
Climate Policy$_{it-1}$	0.39**	0.47**	0.47**	0.43**	0.49**	0.45**
	(0.14)	(0.13)	(0.13)	(0.15)	(0.16)	(0.16)
Ln(GDP Per Capita$_{it-1}$)	−2.74	−1.09	−0.93	−3.02	−1.86	−1.49
	(2.12)	(1.85)	(1.85)	(2.29)	(2.16)	(2.23)
Ln(GDP Per Capita$^2_{it-1}$)	0.22	0.10	0.09	0.24	0.15	0.13
	(0.14)	(0.11)	(0.11)	(0.15)	(0.14)	(0.15)
Growth$_{it-1}$	0.01	0.00	0.00	0.00	−0.00	−0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Ln(Government Spending$_{it-1}$)	−0.06	−0.07	−0.04	0.03	−0.05	−0.07
	(0.31)	(0.31)	(0.31)	(0.33)	(0.34)	(0.34)
Ln(Fossil Fuels$_{it-1}$)	−0.04	−0.03	−0.04	−0.01	−0.02	−0.02
	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)
Ln(Openness$_{it-1}$)	−0.05	−0.03	−0.05	−0.13	−0.07	−0.07
	(0.14)	(0.14)	(0.13)	(0.13)	(0.16)	(0.17)
Polity$_{it-1}$	0.02	0.03	0.02	0.02	0.02	0.02
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Ln(Other ODA$_{it-1}$)	−0.11	−0.05	−0.06	−0.14	−0.06	−0.08
	(0.10)	(0.08)	(0.08)	(0.14)	(0.12)	(0.13)
Ln(Climate Aid$_{it-1}$)	0.17	−0.11	−0.27	0.02	−0.47	0.10
	(0.15)	(0.12)	(0.15)	(0.26)	(0.74)	(0.53)
Constant	9.45	3.63	3.05	10.09	6.13	4.63
	(8.03)	(7.69)	(7.75)	(8.85)	(8.59)	(8.76)
Observations (Countries)	228 (86)	266 (97)	266 (97)	228 (86)	238 (89)	240 (90)
R-Squared	0.51	0.50	0.51	0.51	0.49	0.50

Notes: Dependent variable: Adaptation Policy$_{it}$; Robust (country-clustered) standard errors in parentheses, **p < .01; All models include country and year fixed effects.

Table A4(a). Climate aid and CLIMI (aid averaged over 2000–2005).

	Climate Aid Variable					
	All Climate Commitments (Rio)	Mitigation Commitments (Michaelowa)	Adaptation Commitments (Michaelowa)	All Climate Disbursements (Rio)	Mitigation Disbursements (Michaelowa)	Adaptation Disbursements (Michaelowa)
$\text{Ln(GDP Per Capita}_i)$	0.38	0.18	0.20	0.63*	0.54	0.53
	(0.36)	(0.32)	(0.29)	(0.31)	(0.36)	(0.32)
$\text{Ln(GDP Per Capita}_i^2)$	−0.02	−0.01	−0.01	−0.04	−0.03	−0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Growth_i	0.01	0.01	0.01	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$\text{Ln(Government Spending}_i)$	−0.04	−0.04	−0.03	−0.01	−0.03	−0.03
	(0.10)	(0.10)	(0.09)	(0.08)	(0.08)	(0.08)
$\text{Ln(Fossil Fuels}_i)$	−0.00	0.00	0.00	−0.01	−0.00	−0.01
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
$\text{Ln(Openness}_i)$	−0.04	−0.00	0.00	−0.02	−0.01	−0.01
	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)
Polity_i	0.01*	0.02**	0.02**	0.01**	0.01**	0.01**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$\text{Ln(Other ODA}_i)$	−0.05	−0.05	−0.05	−0.08*	−0.07*	−0.07*
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
$\text{Ln(Climate Commitments}_i)$	0.05	0.01	−0.04	−0.34	−0.37	−0.02
	(0.11)	(0.08)	(0.18)	(0.39)	(0.31)	(0.17)
Constant	−1.03	−0.46	−0.58	−2.04	−1.66	−1.66
	(1.34)	(1.29)	(1.21)	(1.11)	(1.36)	(1.16)
Observations	46	55	55	46	47	48
R-squared	0.43	0.48	0.48	0.50	0.43	0.42

Notes: Dependent variable: CLIMI_i; Robust standard errors in parentheses, **$p < .01$ *$p < .05$.

Table A4(b). Climate aid and CLIMI (aid averaged over 2005–2010).

	Climate Aid Variable					
	All Climate Commitments (Rio)	Mitigation Commitments (Michaelowa)	Adaptation Commitments (Michaelowa)	All Climate Disbursements (Rio)	Mitigation Disbursements (Michaelowa)	Adaptation Disbursements (Michaelowa)
Ln(GDP Per Capita$_i$)	0.45	0.44	0.42	1.02**	1.01**	1.01**
	(0.49)	(0.47)	(0.47)	(0.34)	(0.35)	(0.32)
Ln(GDP Per Capita$_i^2$)	−0.03	−0.03	−0.02	−0.06**	−0.06**	−0.06**
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
Growth$_i$	0.00	0.00	0.00	−0.00	−0.00	−0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Ln(Government Spending$_i$)	−0.05	−0.04	−0.04	0.04	0.03	0.03
	(0.11)	(0.11)	(0.11)	(0.12)	(0.11)	(0.11)
Ln(Fossil Fuels$_i$)	0.00	0.00	0.00	−0.01	−0.01	−0.01
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Ln(Openness$_i$)	0.01	0.01	0.02	−0.04	−0.03	−0.03
	(0.09)	(0.09)	(0.09)	(0.07)	(0.07)	(0.07)
Polity$_i$	0.01*	0.01*	0.01*	0.01**	0.01**	0.01**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Ln(Other ODA$_i$)	−0.04	−0.05	−0.03	−0.08**	−0.08**	−0.06**
	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)	(0.02)
Ln(Climate Commitments$_i$)	−0.00	0.02	−0.09	0.05	0.16	−0.13
	(0.05)	(0.06)	(0.10)	(0.12)	(0.24)	(0.11)
Constant	−1.49	−1.48	−1.47	−3.54**	−3.51*	−3.53**
	(1.73)	(1.65)	(1.67)	(1.27)	(1.32)	(1.18)
Observations	46	46	46	44	44	44
R-squared	0.35	0.35	0.37	0.57	0.57	0.58

Notes: Dependent variable: CLIMIi; Robust standard errors in parentheses, ** $p < .01$ * $p < .05$.

Article

Aid Targeting to Fragile and Conflict-Affected States and Implications for Aid Effectiveness

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Abstract
While significant amounts of foreign aid have been allocated to the group of so-called fragile and conflict-affected states in recent years, it is not clear whether that aid is targeted to where it is most needed. This article extends recent work by Carment and Samy (2017, in press), and focuses on aid targeting in fragile states by using the Country Indicators for Foreign Policy fragility index together with sectoral aid flows from the OECD Creditor Reporting System. Specifically, it considers six country-cases from a three-fold typology of states and evaluates the performance of these countries in terms of their fragility relative to the types of aid that they have received. The article argues that aid is poorly targeted in fragile states and by considering the sectoral allocation of aid it also contributes indirectly to the related issue of aid effectiveness.

Issue
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1. Introduction

Fragile and conflict-affected states (FCAS) have attracted a significant amount of donor attention and resources in recent years.[1] According to the latest *States of Fragility* report by the OECD (2018), donors spent US$ 68 billion or more than 65% of their earmarked funding in 58 fragile contexts in 2016, and hence more than in other developing countries. Given that poverty will become increasingly concentrated in fragile states in the next decade, and that progress in these countries has been slow historically, it is likely that aid flows in FCAS will continue their steady increase. We also know that the amounts spent in these fragile countries are not evenly spent, giving rise to the phenomenon of aid darlings and aid orphans. A few papers have used large-N analysis to examine aid allocation and aid effectiveness in fragile countries. For

example, McGillivray (2006) and McGillivray and Feeny (2008) examine aid allocation, and aid and growth, in fragile states respectively. They find that fragile countries face difficulties in absorbing the amounts of aid they receive when compared to other countries and that growth would have been lower in the absence of aid. Carment, Prest and Samy (2008) find that aid is allocated to fragile states on the basis of their capacity and authority structures but not according to legitimacy, which could be problematic if countries remain trapped or face challenges in overcoming fragility as a result of lack of legitimacy. However, these empirical studies consider fragile states as a group and do not fully exploit the different ways in which countries are fragile. Indeed, few are the studies that focus on the policy implications of fragility persistence by considering specific country-cases. An early study by Chauvet and Collier (2008) found that the

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[1] We use the term fragile states, fragile contexts, fragile countries and FCAS interchangeably in this article.

average duration of a failing state is a prolonged period of five decades because external financing for resource exports and aid tend to embolden and support rent seeking elites and retard reforms. Andrimihaja and his colleagues in their 2011 World Bank study cited a combination of weak property rights enforcement, corruption, insecurity and violence, which conspire to create a low growth equilibrium (Andrimihaja, Cinyabuguma, & Devarajan, 2011). But what neither of these studies clarify is how these factors work together to generate fragility persistence.

To address this deficiency in theorising, in recent years we have begun to see the emergence of a multidimensional approach to the identification and classification of fragile states (Carment, Prest, & Samy, 2009; Call, 2011; Gravingholt, Ziaja, & Kreibaum, 2015; OECD, 2015). In this article, we draw from the Country Indicators for Foreign Policy (CIFP) fragile states framework proposed by Carment et al. (2009) to conduct our analysis. According to the CIFP framework, for a state to function properly it needs to exhibit three fundamental properties: authority (A), legitimacy (L) and capacity (C). Fragility, again according to the CIFP framework, refers to the extent to which the actual characteristics of a state differ from their ideal situation. Viewed in this way, all states are to some extent fragile and weaknesses in one or more of these three fundamental properties will have a negative impact on their fragility. The CIFP framework thus includes an overall index of fragility and these three different characteristics of stateness, also known as the ALC framework. Authority refers to the legislative power of the state and its ability to control its territory, to provide core public goods, stability and security to its people. Legitimacy refers to how much a particular government commands loyalty to the governing regime and how much domestic support it generates for its legislation and policy. Capacity refers to the ability of the state to mobilize and employ resources towards productive ends.[2] To obtain composite scores for authority (A), legitimacy (L) and capacity (C), various indicators are converted to a nine-point score based on the performance of a country relative to a global sample of countries. A higher score indicates that a country is performing poorly relative to other countries. Whether a state is strong or vulnerable is relative, and while certain states are strong or resilient by certain measures, they are weak by others. As recently pointed out by the Fund for Peace, fragility affects even the world's richest and developed countries, as both the United States and the United Kingdom are facing unprecedented internal political divisions, with the main difference, however, being that they have stronger capacity and resilience than more fragile countries (see Messner, 2018).

This article extends recent work by Carment and Samy (2017, in press) to consider aid targeting in fragile states. Its core argument is that aid is poorly targeted in fragile states; in particular, current aid allocations in the chosen country-cases do not pay sufficient attention to issues of authority and legitimacy that are both important for understanding why countries are fragile. Since Burnside and Dollar (2000), much attention has been devoted to the allocation of aid to countries with good policies. However, as pointed out by Hansen and Tarp (2000), the Burnside-Dollar policy selectivity result is sensitive to data and model specification, which makes the selectivity argument less robust. Furthermore, despite the evidence of non-linearities in the aid-growth relationship, it may well be the case that those countries that do not have good policies in place are the ones that need aid the most (Hansen & Tarp, 2000). We are well aware of this dilemma in the case of fragile states, which by definition are characterized by poor policy environments. Our argument is not that aid should not be allocated to countries that face authority and legitimacy challenges. Instead, we argue that aid could be better targeted to these weaknesses in order to improve its effectiveness. The remainder of the article proceeds as follows. In the next section, we present a typology of states using the fragile states framework (Carment et al., 2009) discussed above. Section 3 summarizes the evolution of fragility using both quantitative analysis and the examination of six country-cases drawn from the typology discussed in Section 2. Section 4 analyses how aid is spent in the six identified country-cases using the OECD Creditor Reporting System (CRS) database. Section 5 concludes with the policy implications of our analysis.

2. A Typology of States

Just like Tolstoy's unhappy family, each fragile state is unique in its own way. However, we argue that we can categorize countries according to three types of states when one considers their performance over time: states that are stuck in a fragility trap, states that move in and out of fragility, and states that have exited fragility for a significant period of time. To be sure, these states are not identical in every aspect and neither are they fragile for the same reasons. However, their trajectories have followed particular patterns that allow them to be classified under one of these three types. As fragility rankings such as those from Fund for Peace and CIFP have shown, some countries have remained among the worst performers over time, with no sign of improvement. However, there are others who were once fragile and that have been able to build resilience and are no longer classified among current lists of fragile states. These are countries that we characterize as having exited fragility. There is also another category of in-between countries that have moved in and out of fragility, that is, where improvements in their situations did not last very long.

In order to identify countries for each type (trapped, in and out, and exited), we use the CIFP fragility index discussed earlier. The advantage of this index when com-

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[2] In addition to information about authority, legitimacy and capacity, countries are also scored according to various clusters that include governance, economics, security and crime, human development, demography, the environment, and gender as a cross-cutting theme.

pared to others is that it is available over a few decades going back to 1980.[3] Given the persistence of fragility, this allows us to observe movements over an extended period of time that may be less visible with shorter time series such as the Fragile States Index of the Fund for Peace that is available since 2005 only. Countries that are trapped in fragility are those that are ranked among the top 20 fragile countries for more than half of the period from 1980 to 2015, and whose long-term trajectory according to fragility scores did not improve. This means that we consider both the ranking and fragility scores in order to avoid the possibility that the rank of countries could be negatively affected if other countries are doing better, even as the former are also improving. Trapped countries include the likes of Afghanistan, Chad, the Democratic Republic of the Congo, Pakistan, Somalia and Yemen.

Countries that moved in and out of fragility are those that were only able to exit the top 40 fragile countries for a short period of time and whose fragility scores were relatively flat over the long-term. While the choice of the top 40 might appear arbitrary, it is within the range of contexts identified as fragile—the OECD (2018) currently identifies 58 such situations—and if anything, imposed a more constraining condition on those countries that were deemed to have exited, albeit temporarily. In this group, one can find countries such as Cameroon, Laos, Mali, Rwanda and Senegal. Some of these countries are currently classified as fragile (OECD, 2018) but our analysis using CIFP's data indicates that they have been in and out of fragility. A third type of country consists of those that have exited fragility. These are countries that were among the most fragile countries in earlier years and exited the top 40 rankings for a period of ten years or more. Countries in this category display improvement in their fragility scores over the full period and include the likes of Algeria, Bangladesh, Guatemala and Mozambique. Although there is always a possibility that some of these countries might go back into fragility, and that some of them are still classified as fragile (OECD, 2018), the CIFP data indicates that they have been able to exit the top 40 rankings.

Classifications and typologies are challenging. Other than the more obvious cases that we consider trapped, different indices tend to rank and thus categorize countries differently and we are aware that our categorization of states as being in and out of fragility, or exited, may be disputed. However, for the purposes of this article, we are interested in categorizing so that we can then trace the evolution of these states over time using the ALC framework, which we address in the next section, and also to examine the allocation of aid to them.

3. Evolution of States Using the ALC Framework

3.1. Trapped, In and Out, and Exit

Why do countries end up being trapped, moving in and out of fragility, or exiting fragility altogether? The existing empirical literature on this issue remains fairly nascent. In their UNU-WIDER Research Working Paper, Carment and Samy (2017) find that there is strong evidence that capability and legitimacy traps are significant correlates of countries trapped in fragility, which when mapped onto the ALC framework indicate that authority and legitimacy are significant.[4] They also find evidence of a conflict trap (that is, authority challenges) as a contributing factor and no evidence for the poverty trap when country fixed effects are taken into account. Overall, countries are trapped for reasons that are more related to authority and legitimacy, instead of capacity. Hence, their analysis, even if based on a small sample of ten countries using data collected for the period 1980–2015, and drawing from the CIFP ALC framework, indicates that focusing on capacity alone will not allow countries to escape the fragility trap when there are insufficient improvements to authority and legitimacy.

In an extension of this analysis to "in and out" states, Carment and Samy (in press) find that the conflict trap is important for that particular group. While the governments of countries in that category became more capable in some periods, their fragility scores only improved temporarily as a result of failure to exercise control over people and territories. On the other hand, the poverty and legitimacy traps are not as significant for them. Linking back to the ALC construct, authority seems to be the most important driver that prevented them from building resilience and stability more permanently. Finally, those countries that were able to exit fragility avoided the recurrence of large scale conflict (overcoming the conflict trap is significant in their case) but this was possible when they succeeded in building legitimacy as well. So, overall, the evidence indicates that authority and legitimacy are important drivers of fragility, with capacity playing a smaller role.

3.2. Country-Cases[5]

Building on these observations from the various types of fragile states identified above, we now turn our attention to six country-cases (two from each type) and how they have evolved, or not, over time, once again linking their evolution to the ALC framework. We should note that the selected cases are exemplars or typical countries

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[3] Although the entire CIFP dataset is not made public, the methodology is fully transparent (see Country Indicators for Foreign Policy, n.d.). Data for the most recent year is publicly available each time the dataset is updated.

[4] Capability traps (Pritchett & de Weijer, 2010, and Pritchett, Woolcock, & Andrews, 2010) prevent fragile states from implementing basic functions such as service provision, the maintainance of law and order, and security. These factors are more in line with how authority is defined in the CIFP framework. In the case of legitimacy traps, they refer to countries that suffer from weak legitimacy due to high inequalities and authoritarian management (Takeuchi, Murotani, & Tsunekawa, 2011).

[5] For an extensive discussion of these cases, please see Carment and Samy (in press). Here we focus mostly on how fragility has evolved with respect to the ALC framework across these different cases.

found under each type (trapped, in and out, and exited). We selected two under each category because we want to show that while these countries can be classified as belonging to specific types, they can be fragile for different reasons. There is no reason, for example, to expect Pakistan and Yemen to be fragile for similar reasons, and yet both are classified as countries trapped in fragility. In choosing our cases, we also wanted to make sure that they were in different regions of the world, as opposed to all, if not most, coming from the same region.

Let us therefore first consider Pakistan and Yemen as our two trapped states. Both are middle-income countries and are thus not among the poorest fragile countries. Their trajectories are different, with Yemen a collapsed state in the midst of a horrific civil war and Pakistan still deeply fragile but improving economically. Since unification in 1990, Yemen's fragility ranking has deteriorated, compared to fluctuating but moderate levels of fragility in the 1980s. Although there was a brief period of stability from 1995 to 1998, Yemen has been among the top 10 most fragile countries since 2001 according to CIFP's fragility index; following the outbreak of civil war and Saudi intervention in 2015, it was ranked 4th. 85,000 children under five are estimated to have died from extreme hunger or disease since the beginning of the civil war according to a recent report by Save the Children (2018). Authority, which has been very volatile as a result of conflict and terrorist events, and legitimacy, resulting from lack of support for Saleh's regime leading to his removal and the takeover of the government by the Houthis in 2015, have been the most important drivers of fragility in the case of Yemen.

Pakistan's average fragility rank was around 15 according to CIFP's fragility index, and it has remained in the top 20 most fragile states for most years over the 1980 to 2015 period. Both its authority and legitimacy scores have deteriorated in the last 20 years while capacity has remained stable. The country's inability to control internal conflict and the highly unequal nature of its society has meant that civilian and military governments have not been able to build legitimacy while developmental aid has reinforced a centralized authority structure, regardless of its legitimacy.

The fragility trap for Pakistan and Yemen are both related to problems of legitimacy that further undermine their authority structures, thus allowing for a negative interaction between legitimacy and authority. In general terms, strengthening authority structures without appropriate resource distribution goes hand in hand with declining legitimacy. Capacity is skewed to maintaining control over the distribution of resources and rents in favour of entrenched and unelected elites. Fissures based on ethnic cleavages, elite capture and rent seeking behaviour are met with coercive measures to maintain stability but come at the costs of further declines in legitimacy.

In examining the importance of a legitimacy feedback loop for Yemen, we see that deposed head of state

Saleh's leadership ideology had essentially been one of regime survival, as the system of governance under his rule was a savvy mixture of Islamic, conservative, and liberal economic policies (Carment & Samy, 2017). Furthermore, his legitimacy stemmed primarily from his regime's ability to maintain stability and to provide rewards to his clients. Much of that stability stemmed from the aid Yemen received in support of its contribution to the Global War on Terror (Carment & Samy, in press). Thus Yemen's fragility trap is a function of mutually reinforcing structural constraints. Saleh carefully constructed a patronage system that provided benefits to a select few clients (Clark, 2010). But that narrow support base also constrained his ability to improve the country's economy, for example through structural adjustment, and improved social services. As long as resources were available, such as rents from oil revenues, development assistance and military aid, the regime was secure and did not need to reform though the country itself remained deeply fragile. When those narrowly distributed benefits began to diminish, and as oil output declined, so too did Saleh's hold on power. Ultimately, his concentration of personal power and the neglect of the periphery left the field open to new challengers, such as disaffected southerners, Islamist groups, and the northern Houthi movement. Yemen's legitimacy feedback loop was lethal because Saleh's regime survival was tied to a declining rent economy leading to reduced capacity and control over territory and ultimately collapse.

Like Yemen, Pakistan's fragility challenges appear to be linked to problems of legitimacy which further undermine its authority structures. But here the legtimacy feedback loop is pernicious and so far non-lethal. Systemic social fissures, which pit ethnic and sectarian groups against each other, form the unsteady foundation upon which Pakistan's political institutions are built (Carment & Samy, in press). Inequality between ethnic groups, in particular, has highlighted poor legitimacy as various calls for self-government by provincial regions seeking autonomous control over their resources clearly demonstrate a loss of confidence in the capacity of Pakistan's regimes to act in their interests. In addition, continued elite capture of power and resources has contributed to a depreciation in the quality of Pakistan's institutions. The result is a governance system that explicitly favours networks of unelected ruling elites, and a public with little trust for ruling regimes.

The negative reinforcement of Pakistan's authority structures is achieved through an institutional system, political structure and popular media in Pakistan that collectively reinforces the identity of state-centric nationalism. The Pakistani state is not so much a subordinate to dominant ethnic groups but works in partnership with them. This partnership is reinforced when the state is challenged by regional minority groups, itself a response generated by assimilative pressures, policies on in-migration, economic competition and more recently political threats of secession. Simply put, Pakistan's

fragility begins with a weakness in legitimacy structures which rather than being adaptively modified in a positive way are negatively reinforced, with the consequence of increasing instability over the short run.

The feedback loop is pernicious because when Pakistan experiences internal legitimacy challenges there is an effort to reinforce oppressive authority structures, no matter how weak they are as a bulwark against further decline. Such an emphasis, exemplified in the United States' long-term aid program for Pakistan (as a result of its support for allies in the Global War on Terror), led to a potential distortion in both the selection of aid recipients, and the type of aid provided. A large amount of aid, including billions of dollars in military aid from the United States, has been given to Pakistan, regardless of the legitimacy of the regime in power. The results are deeply unpopular, and contribute to the persistence of nearly illegitimate regimes dependent on external aid, and that are unstable over the long term.

For the in and out cases, let us consider Mali and Laos. In the case of Mali, CIFP's data indicates that Mali has been in and out of the top 40 fragile states at least four times, with a deteriorating trend in the last few years after the Tuareg rebellion and military coup in 2012. The main source of Mali's fragility has to do with challenges to authority, partly as a result of lack of control over its territory and people. Conversely, recovery from fragility occurs when challenges to authority are terminated, even if only temporarily. Capacity problems resulting from a weak aid-dependent economy have kept the country close to the cut-off for exiting fragility and very vulnerable to shocks.

Mali's spectacular collapse in recent years has sparked a re-examination of its characterization as a model of stability in North Africa. In reality, Mali has never been such a model (Carment & Samy, in press) and its recent conflict is the rule rather than the exception. The picture of Mali is of a country that continually exits fragility, only to re-enter it further down the road. This underperformance is concomitant with an increase in international development assistance. Mali has generally been an aid-dependent country, but aid increased substantially in real terms since its attempts to transition to a hybrid form of democracy in the 1990s. The effects of increasing aid, driven in part by promises to reform economically and politically, have been staggeringly negative. That is because Mali's elite benefit from aid by 'brokering' its distribution. In order to operate effectively, internationally funded NGOs rely on the cooperation (or at least benign neglect) of local government institutions (Carment & Samy, in press). In Mali, this has resulted in political alliances between influential politicians cum aid brokers and key NGOs. Far from representing the interests of the people by establishing and adhering to institutional performance measures under a democratic system, the goal is to establish a patronage network and push for additional zones around areas over which they have more control.

More generally, Mali's aid economy offers a viable option to the thin veneer of democratic reform on display to the donor community. For example, partnering with a development NGO allows local politicians an opportunity to skim funds, either directly, or indirectly by influencing where the money is spent. Additionally, NGOs need to engage the population. In a country where the majority are illiterate, well-educated elites are hired. These elite constitute the bridge between regions and the state but operate from within the state apparatus and therefore are uncritical of it, and the legitimacy that underpins it (Carment & Samy, in press). It becomes self-evident why democratic institutions have little purpose under such an arrangement, other than providing the cloak of international legitimacy through which state and regional elites can ensure resource distribution for the people of Mali. However, such a system has the potential to under-resource key sectors of society which do not factor into the maintenance of this economic system, such as the military (Carment & Samy, in press).

Unlike Mali, Laos (Laos People's Democratic Republic—Laos PDR) has never really been considered a model of stability. It is a country of only 6.2 million people, surrounded by larger, more powerful neighbours. Though both it and Mali have been afflicted by environmental calamity such as droughts, Laos has shown that it can recover in the face of adversity. The communist takeover in 1975 provided regime stability by bringing an end to years of civil war, but since then, Laos's leaders have been unable to bring meaningful reform and economic growth to their people. Until recently, the country's primary weaknesses were authority followed by capacity; when either deteriorate, the country moves back into fragility. Laos has been in and out of fragility five times over the last 30 years. The entries by Laos into the top 40 are consistent with the historical analysis coinciding with severe drought and border wars, conflict with Hmong insurgents, severe flooding and the Asian Financial Crisis of 1997. Laos' subsequent improvement from about 2009 onward is a result of the regime's slow but positive reforms to address security and economic stabilisation.

Economic growth has proved a double edge sword for Laos. With a number of trading partners to work with, Laotian elites have proved adept at insinuating their own economic interests into trade and investment frameworks that Laos has with established regional powerhouses. Though there are immense dependencies within these relationships, it has also meant that Laos has benefitted from a much greater political latitude that imposed structural adjustment programmes and aid programmes normally generate. In essence, Laos' improvement over the last 10 years can be understood from the perspective of spillover of economic success from its larger neighbours without the democratic baggage. But the country's weakness also emanates from the same source. There are relatively few reform-minded elites who are able to shrug off the mantle of risk averse communist style Politburo decision-making.

The result of such a political economy is one which is entirely dependent on the ruling regime for rent redistribution. Indeed, the few sectors which have experienced liberalization have been those primarily devoted to resource extraction/export, which are dominated by party or military officials. Thus, if there is a weakness in Laos's trajectory it is simply that its leaders are slow to make policy decisions that could improve the lives of average citizens. These outputs in turn should create expectations, raise accountability and open up the political system. Instead, the regime concentrates on liberalizing and expanding only those sectors which it fully controls for fear they could lose power, influence and income.

In comparing Laos with Mali, we see there are two different paths. One (Mali) is the possibility of regression and succumbing eventually to lethal feedback loops, the other (Laos) is a potentially stable exit. The likelihood of a stable exit assumes that reformists within government eventually succeed in expanding resource distribution. Conversely, a lethal feedback loop awaits those countries with poor economic performance, coupled with an economy based on aid dependence. Under such conditions, as we have seen with the trapped states, a focus on security issues at the expense of stronger state-society relations undermines further economic growth.

We turn now to Bangladesh and Mozambique both of which have built resilience and exited fragility. In the case of Bangladesh, as one of the most fragile countries in the world in 1980, it left the top 40 ranking in 1991 and has only gone back into that category twice (in 2004–2005 and 2007–2008). According to CIFP's data, this transformation is the result of improvements in authority and legitimacy, and also a gradual improvement in capacity. A key contributing factor was the replacement of military control in domestic crisis management through opportunities for political mobilization and reform. With the help of international donors, political reform was followed by improved economic performance and improvements in authority. Even moderate commitments to reform have made a big difference. Bangladesh realized a respectable level of civilianization of its military leaders and a nascent, if not dysfunctional, multi-party political organization (Carment & Samy, in press). The leaders of Mozambique's Frelimo showed flexibility and pragmatism in the aftermath of protracted war. Instead of focusing on revenge, they focused on economic growth. For Bangladesh, the country's strong improvement in capacity fuelled by rapid economic growth was reinforced by powerful deep-rooted patron-client relations, resilience in the face of adversity, and a strong civil society presence. Mozambique in contrast, though its economic growth is strong, is run by a rent seeking political party that appears unwilling to relinquish control.

Just as Pakistan and Yemen remain trapped because of legitimacy issues that further undermine authority structures, Bangladesh was able to create space for civil society and new political parties. The resulting legitimacy enabled export-led manufacturing to drive the economy,

and allowed the country to exit from being a fragile state. Mozambique, for its part, was able to exit fragility at the end of the civil war in 1992. The largest improvements towards the end of the civil war and thereafter were recorded in legitimacy, followed by authority. And as in the case of Bangladesh, export driven economic growth helped Mozambique to exit. Both countries exited from fragility through two stages. First by overcoming the adversities of war as well as meeting the challenges of natural and man-made disasters (flooding in the case of Bangladesh, demining in the case of Mozambique) and second by focusing on economic growth through significant reforms implemented with support from the international community. During the first phase, we see decreasing volatility in both legitimacy and authority and only later in the second phase improvements in capacity based on economic growth and more diversified economies.

These transitions have been far from perfect. Both aid and extractive industry rents have aggravated Mozambique's fragility by undermining regime legitimacy and effectiveness due to poor resource distribution. Maputo's growth is not matched by equivalent gains in the hinterland. The absence of accountability is key here. For example, Perez Nino and Le Billon (2013) argue that Mozambique will ultimately fall back into fragility because of its low tax burden on elites which in turn puts minimal pressure on these elites to provide social spending for all Mozambicans.

Politically, as hybrid democracies both countries have substantial difficulties in managing political transitions without violence and political unrest. Mozambique introduced multi-party elections in 1989 though Frelimo has ruled ever since. Bangladesh is a multi-party democracy but one consistently undermined by cronyism, corruption and dynasticism. To be fair, much of Bangladesh and Mozambique's corruption might be reinvested in their respective economies creating a kind of virtuous feedback loop. But other virtuous feedback loops are present, including investments in human capital projects, to some degree gender empowerment (in the case of Bangladesh) and spontaneous forms of privatization. These all serve to indirectly improve legitimacy and authority by reducing social unrest and improving legitimacy outputs.

These two cases show that it is possible for countries to exit fragility with the right mix of political and economic reforms in the presence of independent political institutions and support from the international community. Whether both cases remain out of fragility will depend not simply on continued economic growth and capacity improvements, but on whether state-society relations remain strong. Mozambique's future is less clear-cut than Bangladesh's. If there is a major constraint for Mozambique, it is its inability to match poverty reduction with rapid economic growth especially as new sectors such as mining show lower levels of productivity.

Like Bangladesh, Mozambique's political and economic interdependencies were most evident in its for-

mative years after the war. Where catalysing negative effects had the potential to shift the country downward, political leadership moved the country in a more positive direction with the support of the international community. When stressed, as in a time of political upheaval, Mozambique's leaders revealed a modest ability to recuperate.

To summarize, the statistical analysis by Carment and Samy (2017) showed that capability, legitimacy and conflict (to a certain degree) are key correlates of the fragility trap. The capability and conflict traps are equivalent to authority according to CIFP's framework, and the legitimacy trap with CIFP's legitimacy cluster. While conflict is a factor that was present in all of the above cases at one point or another in their recent history, it was not a leading reason for how fragile they are or have been. Yemen is the only case where conflict has arguably kept it trapped. The dominance of the military in the case of Pakistan has prevented it from succumbing to a lethal feedback loop but the result is deteriorating state legitimacy and limited political freedoms. In the case of Bangladesh and Mozambique, both recovered from large scale civil conflicts as a result of political and economic processes that were both legitimate and sustainable. For the two in and out countries, Mali and Laos, low intensity conflicts have made it more difficult to assert authority and build legitimacy, especially for Mali, though less so in the case of Laos.

The inherent difficulty of a fragile state attempting to exit the trap is straightforward. Leaders of deeply fragile states are able to survive with a small but powerful support base by tying private welfare to their own welfare (Carment & Samy, in press). Even though the state is the primary instrument of power and may even indeed possess overwhelming coercive capacity, its leaders lack the autonomy to affect concessions for reform. Since a necessary ingredient for implementing reform is public support for such policies, elites that are unaccountable to the larger population (in which the possibility of overturning the government is always present) have little incentive to pursue change. Legitimacy is weakened even further when elites are forced to expend greater resources on coercive means in order to ensure they are obeyed. Under these conditions, elites express only a minimal commitment to reform. This is because the centralization of state authority and the pursuit of development policies aimed at maximizing revenues and rents, rather than social welfare, produce a process which has non-elected institutions and elites dominating.

States remain trapped or fall back into fragility when they fail to provide public goods that benefit large parts of the population, even in the face of improved capacity. Situations where there is a decline in the provision of public goods is often followed by decreasing volun-

tary compliance, such as tax payment, which can in turn reduce government effectiveness further. Given the importance of legitimacy and authority for the evolution of states, and the lack of evidence supporting the poverty trap, in the next section we examine how aid is allocated to these various countries.

4. Sectoral Aid Allocations and the CIFP Framework

In this section, we use gross official development assistance (or foreign aid) disbursements from all Development Assistance Committee (DAC) donors to compare aid allocation across the six country-cases discussed earlier.[6] Of the six countries, Mozambique is currently the most aid dependent country with an aid to gross national income ratio of 14% in 2016, followed by Mali (9%) and Yemen (7%) according to data from the World Bank's World Development Indicators dataset. On the other hand, the numbers for Bangladesh and Pakistan are around 1%, and 2–3% in the case of Laos. However, even in the case of Bangladesh and Laos, aid represents a fairly significant percentage of central government expenditure (which arguably is a better measure since it shows the extent to which states rely on foreign aid for their budgets) at 12% and 18% respectively, while in the case of Mali it is an incredible 70%, for 2016. Therefore, the countries that we examine are quite dependent on foreign aid. We calculate the average sectoral aid allocation using the CRS aid activity of the OECD for the period 2003–2016 during which detailed sectoral level data is available for our six country-cases.

Of most relevance to us is aid allocated to social infrastructure and services which comprises anywhere from 41 to 51% of aid allocated (see column (a) of Table 1) and is by far the most important sector.[7] This sector is relevant because it includes aid allocated towards education, health, population policies and programmes, water supply and sanitation, government and civil society, and other social infrastructure. Except for government and civil society, all the other categories are mostly related to capacity in the ALC framework. As can be seen in column (b) of Table 1, they comprise a significant proportion of column (a); for example, in the case of Pakistan, they account for 75% of aid allocated towards social infrastructure and services, while government and civil society accounts for the remaining 25%. This is in fact a pattern that repeats itself across all the six country-cases, with most aid going towards capacity-related issues.

The difference between columns (a) and (b) is aid allocated to government and civil society (column (c)). The latter can be further broken down as government and civil society—general (column (d)) and conflict, peace and security (column (e)). Government and civil society—general, includes funding for democratic participation

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[6] Our analysis excludes military aid because we focus on official development assistance. However, we are aware that this is significant for countries such as Pakistan. For example, for several years after the 9/11 attacks, United States security assistance to Pakistan exceeded economic assistance (see Center for Global Development, n.d.).

[7] Other sectors include economic infrastructure, production sectors, multi-sector/cross-cutting, commodity aid, action related to debt, humanitarian assistance, administrative costs of donors, refugees in donor countries and unallocated/unspecified.

Table 1. Aid allocation to social infrastructure and services as a % of total aid, 2003–2016. Source: OECD DAC CRS (OECD, n.d.).

Country	Total	Education, Health, Population, Water Supply, Other Social Infrastructure	Government and Civil Society		
			Total	General	Conflict, Peace and Security
	(a)	(b)	(c)	(d)	(e)
Pakistan	41.1	30.6	10.5	9.4	1.1
Yemen	51.1	41.4	9.7	8.4	1.3
Mali	47.4	36.4	11.0	9.3	1.7
Laos	43.6	31.6	12.0	7.4	4.6
Bangladesh	41.3	33.4	7.9	7.7	0.1
Mozambique	50.9	42.2	8.7	8.0	0.7

and civil society, elections and human rights.[8] These are related to legitimacy under the ALC framework and represent between 7.4% to 9.4% on average (column (d)). Aid allocated towards conflict, peace and security (column (e))—which under the ALC framework fits under authority—is even smaller, accounting for less than 2% for most of the country-cases. Part of the explanation for why most aid goes to capacity and less to authority and legitimacy may have to do with the short time horizon of donors that are often in search of quick wins. Indeed, it is easier to see results when one builds a school or invests in healthcare but, for example, political stability (building authority) and improving the quality of democracy (building legitimacy) are more difficult to achieve even if long-term impacts may be far more important in helping states become resilient.

Two trends are clear from the above analysis. First, a very high proportion of aid is allocated towards capacity but less so towards legitimacy and authority. We are not arguing that capacity is not important. Issues such as education and health, which have been prioritized by donors, and especially after the adoption of the Millennium Development Goals, attracted a lot of funding in the years that followed. On the other hand, to the extent that authority and legitimacy matter for the evolution of states, particularly those that are trapped or are falling back into the trap, they are, comparatively, areas that have not received the same level of attention. We should note that even if we report averages in Table 1, examining variation over time does not change our overall conclusions. Furthermore, in countries such as Pakistan, if we think beyond development aid to consider other forms of assistance, large amounts of aid from the United States, including significant military assistance, have undermined rather than promote democracy (Ali, 2009).

While the low percentages allocated towards "conflict, peace and security" make sense in the case of Bangladesh and Mozambique, our two exit states, the same cannot be said about Mali or Yemen. We should note that the results reported here differ from those in

Carment et al. (2008), in part because the current article focuses on a narrow set of countries, whereas the former examines aid allocation for all countries that include both fragile and non-fragile countries. However, they still confirm that legitimacy is not a factor that seems to be taken into account by donors. Second, the sectoral allocation of aid towards legitimacy (see column (d)) does not seem to vary much across the six country-cases, even if the situation of these countries with respect to fragility and its evolution is quite different.

5. Conclusions

While issues of aid allocation and aid effectiveness have received a lot of attention in the academic and policy literature, comparatively less has been written about these issues with respect to fragile states. Of the few papers that have done so and discussed at the beginning of this article, none have, as far as we are aware, combined the findings of quantitative analysis with specific country-cases to examine whether aid is targeted towards the characteristics that explain the evolution of states. This is what we set out to do in this article. By comparing the sectoral allocation of aid with the CIFP framework, we have shown that aid allocation does not pay sufficient attention to issues of legitimacy and authority that are both important for understanding why countries are fragile. While measuring aid effectiveness (i.e. the impact of aid) is beyond the scope of the current article, we think that a first step is making sure that aid is targeted where it is needed. And for this, it is paramount that donors pay more attention to the multiple ways in which countries are fragile, and the conditions that prevent them from exiting fragility permanently such as lethal and pernicous feedback loops, rent seeking and failure to buy reform.

In this context, for those states where fragility is persistent we find that elites who are resistant to change engage in damaging and self-interested behaviour such as corruption and rent seeking. With a focus on symptoms rather than causes, policies are rarely successful

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[8] This includes funding towards public management and domestic revenue mobilization, which can help countries build authority through taxation. As pointed out by Tilly (1975) and Herbst (2000), taxation is fundamental for statebuilding and creates a fiscal pact between citizens and the state. Unfortunately, as noted by Culpeper and Bhushan (2010), for example, a very small fraction of technical assistance is devoted to public sector financial management in sub-Saharan Africa.

because they do not get to the core of the fragility trap problem. International and domestic incentives for leaders of trapped states to embrace reforms that affect their personal interests are often too weak. Indeed, policies intended to induce reform are not only misplaced, they are often counterproductive (Chauvet & Collier, 2008). In addition to the dilemma that selectivity poses for fragile states (see introduction), there is an extensive literature on conditionality associated with aid programs reinforcing our point that aid conditionality fails more often than not (see, for example, Collier, 1997, and Dijkstra, 2002). That is because they rely on causal explanations focusing on one structural factor such as economic development or political development, which by themselves are insufficient. In many of these interpretations, fragility is usually associated with poor policy environments, aid absorption problems, conflict and poverty.

We find that policies focused purely on structure will be misplaced if there is limited willingness for leaders of trapped states to reform. We emphasize the importance of state-society relations, specifically the role of legitimacy in underpinning the behaviour of political, social and economic elites, in the formation of undergoverned spaces and a coercive state apparatus, rent seeking behaviour, and in building a less resilient society overall. These assumptions are premised on claims regarding interactions between the superordinate elements of state authority, capacity, and legitimacy and not just economic development and democracy. For a state that has exited fragility, positive changes in authority that address societal well-being not only provide valuable guidance for government policy, they also reduce literal barriers to commerce and economic development (measures of capacity) such as restrictions on citizen movement and assembly (measures of legitimacy). Responsiveness also induces governments to produce policies addressing popular concerns that are not growth-focused, such as wealth distribution and social programming, and which by extension increase state legitimacy.

Our overall conclusion is that trapped states are most prone to lethal and pernicious feedback loops. In general terms, strengthening authority structures without appropriate resource distribution goes hand in hand with declining legitimacy. Capacity is skewed to maintaining control over the distribution of resources and rents in favour of entrenched and often, unelected elites. Fissures based on ethnic cleavages, elite capture and rent seeking behaviour are met with coercive measures to maintain stability but come at the costs of further declines in legitimacy. Lethal feedback loops occur when regime survival is tied to a declining rent economy leading to reduced capacity and control over territory and ultimately collapse. Undergoverned spaces increase over time, as patron-client politics and resources weaken simultaneously. Under these conditions, elites express only a minimal commitment to reform. This is because the centralization of state authority and the pursuit of development policies aimed at maximizing revenues and rents, rather than so-

cial welfare, produce a process which has non-elected institutions and elites dominating. There is only a limited opportunity for elites to pursue reforms.

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Conflict of Interests

The authors declare no conflict of interests.

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Article

Bypassing Government: Aid Effectiveness and Malawi's Local Development Fund

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Abstract
Many practical and action-oriented international roadmaps to improve the quality of aid and its delivery and impact on development—including the Paris Declaration, Accra Agenda for Action, and Busan Partnership—emphasize a more active involvement of domestic institutions and procedures. Despite widespread agreement among both donor and recipient countries on this issue, we find that aid often tends to bypass national institutional structures. This practice is sometimes justified on grounds of high levels of political and administrative corruption and weak implementation capacity in recipient country bureaucracies. We examine how and to what extent multilateral and bilateral development agencies bypass national and local government institutions while channeling aid and the impact of such practices on aid effectiveness in Africa. Based on an empirical study of project aid and budget support provided to Malawi by the World Bank, the African Development Bank, and the German Economic Group, we argue that earmarked funding, specialized procurement arrangements, and the proliferation of Project Management Units are among the mechanisms used to circumvent the involvement of national institutions. We conclude that while such practices may achieve short-term gains by displaying successful and visible 'donorship', the long-term impact is more uncertain. The bypassing of local institutions results in fragmentation of aid, lack of coordination among aid industry actors, and a general weakening of policy space and domestic capacity to formulate and implement development policy.

1. Introduction

Ever since the end of WWII, and the provision of capital by the United States to Europe, there has been considerable academic and policy interest in whether and to what extent foreign aid promotes economic development. Even among the most ardent supporters of foreign aid there is a recurring desire to ensure that disbursed funds are used efficiently and effectively to meet project and program goals. Thus, we have witnessed in recent years a growing interest among aid agencies and con-

sultants to monitor and evaluate what is often termed 'aid effectiveness.' And several international roadmaps to improve the quality of aid and its delivery and impact on development—including the Paris Declaration (OECD, 2005), Accra Agenda for Action (AAA; OECD, 2008), and Busan Partnership (OECD, 2011)—emphasize a more active involvement of domestic institutions and procedures. Despite widespread agreement among both donor and recipient countries on the issue of 'ownership,' we argue that a large amount of disbursed aid continues to bypass national institutional structures. Such

practices are often justified by donors as necessary due to high levels of political and administrative corruption and weak implementation capacity in recipient country bureaucracies.

While all actors in the aid industry demand that aid should be effective, they disagree on how it should be delivered to ensure effectiveness. In this article, we examine how and to what extent multilateral and bilateral development agencies bypass national and local government institutions while channeling aid and the impact of such practices on aid effectiveness in Africa. Our empirical focus is Malawi, heavily aid-dependent and recently ranked the third poorest country in the world. We study project aid and budget support provided to the country's Local Development Fund (LDF) by the World Bank, the African Development Bank (AfDB), and the German Economic Group (KfW). Designed as an inter-governmental fiscal transfer mechanism, the LDF was established by the Government of Malawi in 2009 to mobilize resources for poverty reduction interventions in accordance with the provisions of international agreements that promoted greater harmonization of aid and recipient government ownership. The initiative functioned as a basket fund, pooling resources for development and governance projects at the local level, with the Government of Malawi given the power to spend resources at its own discretion but based on the national policies such as the Malawi Growth and Development Strategy. The government envisioned that the LDF, as a pooled fund, would enable better coordination of the numerous projects that donors were implementing at the local level either on their own or through civil society organizations. The goal was to stop donors bypassing the country's public administration and ensure greater effectiveness of disbursed development aid to local councils by actively making use of government structures.

We examine the functioning of the LDF in practice and the extent to which it has promoted harmonized policies and aid aligned to the needs and goals of the national government. In particular, we critically analyze how earmarked funding, specialized procurement arrangements, and the creation of a specialized Project Management Unit (PMU) have been used by donors to bypass the involvement, and curtail the power, of national institutions. Our study is based in fieldwork conducted in Malawi in the period 2015–2018. In addition to visiting LDF projects in four districts—Lilongwe, Zomba, Thyolo, and Mangochi—we conducted forty-five in-depth interviews with local and national government officials as well as those representing the three donor agencies—the World Bank, AfDB and KfW German Economic Bank. The research districts were purposively sampled based on their performance and rating regarding LDF projects. The in-depth interviews were semi-structured to allow detailed probing of themes being examined. At the local council level, respondents were selected from the directorates of Planning and Development, Public Works, and Finance—agencies that are sig-

nificantly involved in LDF projects and hence conversant with issues that were being examined. We also interviewed traditional authorities (chiefs), local councilors, and Members of Parliament (MPs). In-depth interviews were conducted with officials from the Ministry of Local Government and Rural Development, the Ministry of Finance, the National Local Government Finance Committee, and the LDF-Technical Support Team (hereafter LDF-TST). The informants were identified on the basis of being key players in the aid industry either as donors or policymakers in Malawi. They represented a variety of public institutions and aid agencies, thus allowing us to achieve triangulation and solicit diverse views. With the agreement of our informants, all interviews have been anonymized given the sensitive nature of the topic.

In Section 2, we briefly examine the relationship between aid and development, with a particular focus on the evolution and growing popularity of the international aid effectiveness agenda in recent decades. In Section 3, we undertake a discussion of aid dependency in Malawi, setting the scene for our empirical focus. Following this, we introduce the LDF in Section 4 before analyzing its functioning and impact in relation to four sets of issues: earmarked funding and procurement procedures; coordination and project management; aid and patronage politics; and institutional design and competition for scarce resources. We end with some concluding reflections on the effectiveness of bypass strategies.

2. Foreign Aid for Development

The historical foundations of foreign aid are numerous. In addition to political ideology, foreign policy, commercial interests, and national security, there are large elements of altruism and a desire to reduce global poverty. The world of foreign aid is complex and includes Official Development Assistance (concessional flows with a grant element of at least 25%) and development, humanitarian, and emergency aid provided by non-governmental organizations, civil society organizations, bilateral donors, and multilateral agencies.

Over the past couple of decades, there have been interesting exchanges on the conceptual foundations and effectiveness of providing aid. Some, like Calderisi (2006, p. 163), argue that 'foreign aid, as a whole, has not worked,' particularly in Africa. Such conclusions strengthen the arguments of scholars like Easterly (2006), who has famously made the case for 'searchers,' i.e. those who not only find things that work but also accept responsibility for their actions, in contrast to 'planners,' who are supposedly more interested in applying 'global blueprints' than in adapting to local conditions. Jeffrey Sachs has responded by claiming that by not making careful distinctions across countries and types of programs, such conclusions suffer from a major methodological error and end up conveying 'a misplaced sense of helplessness in the face of massive but solvable problems' (Sachs, 2006, p. 1309).

The arguments in favor of aid typically emphasize the positive and significant impact that it has on growth—even though the magnitude of such an effect may be low (Clemens, Radelet, & Bhavnani, 2004). Some argue that aid finances crucial public investments required for infrastructure and physical and human capital, and improves fiscal discipline (e.g. tax collection and reform). Sustained donor–recipient dialogue with flexible conditionality also increases aid effectiveness (Dalgaard, Hansen, & Tarp, 2004; Hansen & Tarp, 2001). The empirical literature, however, does not offer a clear theoretical model on how aid affects economic growth (Easterly, 2003). Indeed, many influential quantitative studies do not find an independent effect of foreign aid on economic growth (Burnside & Dollar, 2000; Collier & Dollar, 2002). Thus, those who argue the case against aid typically point to the large number of African countries that have experienced little economic growth despite being major recipients of aid for decades. They highlight several explanations for this lack of success: large areas with poor infrastructure networks and over-dependence on primary commodity exports; poor governance and weak political commitment to market reform; and inadequate resources for financial investment. With such factors in mind, some conclude that it is not possible to test the 'absolute effectiveness' of aid but rather only the 'marginal effectiveness' (Morrissey, 2006).

Other voices, some from Africa (e.g. Moyo, 2009), claim that trade, and not aid, is crucial for economic growth. Although aid is often conditioned on recipient countries respecting democratic rights, donors themselves often do not abide by such values. The argument is that aid often results in a large and ineffective public sector and is an impediment for private sector development. Without fiscal and budgetary discipline, large public bureaucracies, funded with aid money, breed corruption (Ayittey, 2005). Moreover, some studies show that aid to a repressive regime helps consolidate its power through corruption and exploitation (Bueno de Mesquita, Smith, Siverson, & Morrow, 2003), while others argue that aid should only be given to countries already implementing good policies (Dollar & Levin, 2004).

2.1. The Aid Effectiveness Agenda

Since the Monterrey International Conference on Financing for Development (in 2002), there been an increased focus on the type and quality of foreign aid that can most effectively reduce poverty. The movement for aid effectiveness received a further boost with the Paris Declaration (2005), which recommended *local ownership* of policies by recipient countries; *alignment* of development programs in accordance with developing country priorities; *harmonization* of practices among donors in order to avoid fragmentation, duplication, and high transaction costs; creation of a *results framework* for measuring impact; and increased *mutual accountability* and transparency of aid flows. The commitments made

in Paris were followed by the signing in 2008 of the AAA, which reiterated the importance of strengthening three key aspects of aid: ownership, inclusive partnerships, and delivery of results (OECD, 2008). The AAA also served as a forum for reviewing progress in meeting targets achieved since the Paris Declaration. The direction and volume of aid flows are historically contingent, and some studies have highlighted how voting patterns in the UN, colonial ties, and political and strategic considerations continue to shape global aid flows (Alesina & Dollar, 2000; Bermeo, 2018; Dietrich, 2013, 2018; Easterly, 2006; Knack, 2004). The growing role of emerging donors including India, China and Brazil was also discussed at AAA, and all actors—donors and recipients—were encouraged to abide by the Paris principles in promoting coordinated and effective aid. The Paris and Accra meetings, however, did not result in major changes in the nature, type and predictability of aid flows and some (e.g. Woods, 2008) argued that aid remained dysfunctional, fragmented and duplicative. China, in particular, has become very assertive in foreign aid policies, and through the Forum on China–Africa Cooperation, the Chinese government has increased its support for the activities of Chinese state-owned enterprises and private sector actors in Africa. It thus interacts in numerous ways with poorer countries, including through so-called 'South–South' dialogues, which give access to natural resources and new and growing markets, and reduce the dependence of African countries on traditional forms of aid. Rather than providing budget support to governments, and conditioning aid on support for democracy and gender equality, the Chinese model emphasizes the principles of 'win–win,' 'mutual respect,' 'friendship,' and 'non-interference' (Banik, 2013; Banik & Chasukwa, 2016).

Key debates on aid effectiveness have centred on selectivity and modalities. The major proponents of aid selectivity for increased effectiveness argue that aid should be given to countries that have good policies because they will deliver on what aid ought to achieve (Burnside & Dollar, 1997, 2004). Thus, Burnside and Dollar (2004) claim that good policies will not only make aid more effective, but that they will also succeed in facilitating economic growth. In practice, donors use such arguments when providing tied aid aimed at improving the policy environment of recipient countries. However, aid selectivity remains controversial. For example, Dietrich, 2013, p. 208) notes how donors continue to give aid to countries with not so good policies:

> In 2008, Haiti, a developing country with an abysmal record of governance, received more than 700 million US dollars in bilateral development assistance from OECD donor countries, amounting to roughly 70 dollars of aid per capita. In the same year, Tanzania, whose institutions of intermediate strength bode well for effective aid implementation, received around 2 billion US dollars in bilateral assistance, equivalent to approximately 47 dollars in per capita aid.

Despite the controversy surrounding aid selectivity, donors continue to implement reforms aimed at reducing fragmentation of aid and improving the policy environment in recipient countries. Oh and Kim (2015, p. 92) argue that aid proliferation occurs when donor budgets increase, which in turn gives rise to fragmented aid. Thus, the proliferation of aid can harm economic growth in recipient countries. Moreover, such proliferation is an obstacle to effective aid as interventions are uncoordinated and primarily tend to address the interests of donors and not those of recipient countries. When donors split their aid, the result is often an increase in mechanisms by which they can choose to bypass local government structures citing institutional and human capacity inadequacies and corruption (Dietrich & Winters, 2016; Quibria, 2014). A typical bypass involves 'donors channeling aid through intergovernmental or nongovernmental organizations (IGOs and NGOs) and providing in-kind aid like food and medicine directly to citizens in recipient countries' (Dietrich, 2013, p. 208). In this age of competitive bilateralism and multilateralism, donors also engage the private sector in circumventing local government structures. In a typical bypass, donors are in control of the aid, and retain control over most decisions on how aid is allocated and spent. Such bypass mechanisms give donors the discretion to make decisions that have direct or indirect impact on government policies.

The fast-changing international aid landscape and the growing role of civil society and private organizations as well as emerging countries as providers of aid was the focus of the Busan Partnership for Effective Development Cooperation (OECD, 2011). A major outcome was agreement on a two-tier approach (voluntary for emerging donors) that committed signatories to continued commitment to development and greater accountability of their own actions and inactions (Burnell, 1997; Leiderer, 2013; Oya, 2006; Dreher et al., 2014). The modalities for achieving such results, however, remain unclear. Some studies have identified structural bottlenecks in the implementation of the Paris principles, including the absence of donor accountability and credible penalties for non-compliance (Rogerson, 2005) and the dominance of a 'planning' mind-set that downplays the role of actors with diverse and competing interests (Barder, 2009). Others have argued that the principles of 'harmonization' and 'ownership' are incompatible (Hyden, 2008).

Wolfensohn (1999, p. 9) describes partnership in development aid as a relationship 'led by governments and parliaments of the countries, influenced by the civil society of those countries, and joined by the domestic and international private sectors, and by bilateral and multilateral donors.' A common feature of the declarations in Paris, Accra, and Busan is the importance of treating donors and recipients as equals. These declarations thus re-vitalized previous international discussions that emphasized partnership, including the 1969 Pearson Commission report on development partners and the 1980 Brandt Commission report, 'North–South: A Program for

Survival' (Maxwell & Riddell, as cited in Crawford, 2003, p. 140). Since the Paris Declaration, the aid community has attached growing importance to budget support, particularly in Africa. For example, 20% of donor flows in Mozambique in the year the Paris Declaration was signed was budget support (Renzio & Hanlon, 2009, p. 258), while in Tanzania around 40% of aid in 2009 was channeled through the national budget as compared to 30% in 2002 (Harrison, Mulley, & Holtom, 2009, p. 281). Budget support comes in different forms and includes so-called 'pooled development funds'—which are 'arrangements where donors provide financial contributions towards a common set of broad objectives and where allocations for specific activities are decided by a joint governing mechanism' (Ball & Beijnum, 2010, p. 3). Others highlight that pooled funds are 'a financing arrangement where donors pool their resources using a special account…managed by one of the participating donors or by the respective line ministries' (Bandstein, 2007, p. 3). Thus, a key issue appears to be close collaboration, with the recipient country taking the lead in determining the policies that are to be supported and deciding the policy instruments and interventions to be used in pursuing policy objectives. In other words, donors agree to contribute resources to pooled funds to support the policies that have been determined by the national governments of aid-receiving countries. By supporting such national policies and priorities, donor agencies aim to fulfil their pledge to promote harmonization, mutual accountability, results management, and respect for country-owned policies and structures, as espoused in the 2005 Paris Declaration.

While donors, development agencies and aid-recipient countries may agree on enhancing overall effectiveness of development aid, operationalizing such agreements in practice has always been a challenge. Parties to aid agreements and policies have frequently reneged on their promises and responsibilities in pursuit of individual goals. Thus, despite promises made in Paris, Accra and Busan on the importance of strengthening global partnerships for increased harmonization of aid, bypass mechanisms—as exemplified by the proliferation of PMUs—have continued to flourish (Abdel-Malek, 2015; Li, 2017). The reluctance to phase out PMUs is perhaps also a reflection of the new donor landscape, with the entrance of emerging countries and philanthropic organizations. And current assessments of the quality of aid pays little or no attention to the existence of PMUs. The emphasis on 'value for money' and 'quantification of results/outputs' by most major donors has contributed to the increased usage of bypass arrangements and reversion to project modality in aid delivery as donors show increased interest in balancing score sheets and ticking the right boxes (Yanguas, 2018). Indeed, Bermeo (2018) argue that although 'targeted development' is meant to reach out to specific vulnerable groups, the continued use of project aid and PMUs show that such efforts fall into the same trap that of the 'results agenda'. The aid industry appears to have been reduced to chasing

matrices, since mobilization of resources by donors and development agencies is dependent on the available proof to have successfully completed 'value for money' interventions that can document high impact on beneficiaries. With both donors and recipients under pressure to identify and report projects that can document 'value for money', the current global partnership framework has the potential of compromising aid effectiveness.

3. Malawi: Development in a Highly Aid-Dependent Context

With growing climate-related threats to agricultural production, millions of Malawians regularly go to bed hungry every night. Moreover, the country does not perform well in international rankings such as those on human development and perceptions of corruption. For example, with a Human Development Index value of 0.477 in 2017 (below the average of 0.504 for countries in the Low Human Development group and below the average of 0.537 for countries in Sub-Saharan Africa), Malawi is ranked 171 out of 189 countries in a newly released UNDP statistical update. Indeed, many Western analysts have even begun to propagate the idea that Malawi is unique—'There are developing countries, and then there is Malawi'—implying that a sense of hopelessness characterizes the country's development trajectory (Banik, 2018). All of this points to a paradox: despite peace, political stability, and consistent support for democracy, a large majority of the country's rapidly growing population (estimated to be over 18 million) has not witnessed radical improvement in their living standards (Banik, 2018).

Malawi is particularly interesting as it is heavily dependent on bilateral and multilateral donors to meet the cost of implementing its national budget. This has made the country highly vulnerable to external influence on matters of economic development. Indeed, the proxy influence of donors on development policies can be gauged from the extent of budgetary support they provide. A focus on Malawi to study the aid effectiveness agenda is also interesting as the country has implemented several bold policies—on occasion against the wishes of influential foreign aid donors—that have worked for short periods in promoting food security, but have subsequently been overshadowed by political patronage, corruption, and leakages.

According to Van der Meer, Tostensen, Slob and Jerve (2008, p. 17), foreign aid as a proportion of the government budget averaged 38% over the period 1994–2006, the bulk of which was grants (averaging 72% of all foreign aid). The situation today is not much different, with donor funds accounting for 38% of the total budget in the 2013–14 fiscal year (Van der Meer et al., 2008). Between 2004 and 2010, Malawi consistently received less in loans than in grants. For instance, DAC statistics indicate that the country received 7% of total Official Development Assistance (ODA) as loans and 93% as grants in 2010 (Amundsen, 2017). The trend of grants dominating the aid flows continued between 2012 and 2015 with 83% of assistance provided as grants in 2012–13, 77% in 2013–14, and 80% in the 2014–15 (Malawi Government, 2015b). Although grants have continues to dominate, loans have increased slightly over the years, with the World Bank providing the majority of loans. The percentage of assistance provided as loans increased from 14% in 2012–13 to 19% in 2013–14, and then decreased slightly (still a net increase from 2012–13 to 2014–15) to 17% in 2014–15. In 2014–15, the World Bank provided 61% of all assistance delivered through loans. Among bilateral donors, the United States of America and the United Kingdom are the largest funding partners (Malawi Government, 2015b).

Malawi's external debt stock was US$1,783 million as of December 2015, with multilaterals accounting for US$1,323 million, while the debt portfolios for bilateral and commercial creditors were US$428.8 million and US$114.5 million, respectively. The International Development Association of the World Bank, the International Monetary Fund, and the AfDB are the major multilateral creditors, accounting for 47%, 20%, and 18%, respectively.

Since 2013, the World Bank has been the largest source of financing to Malawi, providing about 45% of all loan disbursements (Malawi Government, 2015a). It is anticipated that the organization will continue to play a major role, as its current Country Assistance Strategy shows that US$300 million has been committed to Malawi for a five-year period (Malawi Government, 2015a). The AfDB has been the second largest source of foreign loans, with 13% of all loan disbursements made since 2013 (Malawi Government, 2017). According to the AfDB's Country Strategy Paper, the planned disbursements amount to US$820 million over a five-year period, of which US$266 million is on non-concessional terms.

China and India are the major bilateral creditors, with 54% and 35% of debts, respectively. China is a relatively new development partner, which has been offering semi/non-concessional loans since 2008. Currently, China represents 15% of disbursements on all loans made since 2012. India, which is another relatively new development partner, has accounted for 15% of all loan disbursements received by the Government of Malawi since 2012. Other non-Paris bilateral lenders, including Kuwait Fund, Saudi Fund and the United Arab Emirates, are expected to provide US$205 million over five years starting in 2016. Poland, a new non-traditional donor, has indicated willingness to support Malawi's development agenda on a non-concessional basis (Malawi Government, 2017).

In theory, all development aid, including bilateral and multilateral assistance, is governed by the Malawi Development Cooperation Strategy launched in 2014.[1] In practice, however, external actors hold the upper hand. The

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[1] The current Malawi Development Cooperation Strategy succeeds the previous Development Assistance Strategy (2006–2011).

relationship between successive Malawian governments and donors has been largely cordial, although there have been several disagreements and confrontations. All five regimes[2] that have governed the country since 1994 have experienced either withdrawal or suspension of aid. For example, aid was withdrawn during the reign of Hastings Kamuzu Banda on account of bad governance and violation of human rights (Meinhardt & Patel, 2003; Resnick, 2012). Similar reasons were cited when aid was withdrawn during subsequent administrations (Von Hagen & Schulz, 2009; Wroe, 2012). Budget support has been suspended on numerous occasions, including 2002, 2003, 2011, and 2012–13 (Amundsen, 2017). The punitive actions on the aid front have often resulted in successive Malawian governments accusing donors of neo-colonization and meddling in domestic politics with the aim of initiating regime change. Following a major corruption scandal in 2013—dubbed 'Cashgate'—major donors including the United Kingdom, Norway, and the European Union suspended their budget support, although they continued to deliver aid through projects, programs, and pooled funds. This decision by donors, which continues today, has raised questions on the preferred aid modalities of traditional aid-givers and the future scenarios under which aid will be provided to Malawi.

4. The Local Development Fund

Aid is channeled to developing countries through either on-budget or off-budget categories. On-budget aid modalities are generally preferred by recipient countries as they, not donors, control the resources and decide how these should be allocated. In many situations, donors prefer to deliver aid through off-budget modalities, where they retain greater control of the agenda and can more directly try to design and implement interventions in recipient countries that are key to their interests.

Motivated by the need to promote increased harmonization of aid and closer alignment with national development priorities[3] following major international declarations and agreements in Paris, Accra, and Busan, the Malawian government established the LDF in 2009 as an inter-governmental fiscal transfer mechanism aimed at mobilizing finance for poverty reduction. The LDF—which evolved from the Malawi Social Action Fund (MASAF) and was established in 1995 with seed funds from the World Bank and the Government of Malawi—is designed to function as a discretionary public financing grant facility that can quickly disburse funds for development projects to local governments. The LDF was established as a basket fund where the government and all donors could pool their resources for development and governance projects at the local level. The pooled funds were aimed at improving aid governance by en-

suring that there were only a few players in the local development sector, who could easily coordinate and work with each other. The government was given the mandate to spend LDF funds at its discretion, as long as these decisions were based on national policies and priorities.

The LDF pools and disburses funds through four funding windows—community, urban, local authority, and performance. Donors (or 'development partners') mainly contribute with financial aid and technical support. The combined contributions of the World Bank, KfW, and AfDB for the period 2009–2014 was approx. US$126 million, which constituted 62% of the total funds received by the LDF in this period. The World Bank, with US$107.8 million, was the major financier, followed by the AfDB (USD 17 million) and KfW (US$1.8 million) (LDF-TST, 2015a). In accordance with the agreement signed with donors, the Government of Malawi provided the remaining funds in cash and kind (e.g. providing office space and paying salaries of selected staff). The government's cash contribution was highly unstable, and payments were often less than the agreed amounts. During fieldwork, the significant role of donors in funding the LDF was acknowledged by a government official, who remarked, 'LDF is much about donors when it comes to making the finances available to roll out the activities.'[4] The key point is that LDF is heavily donor dependent. This heavy reliance on donors has weakened the ability of the government to improve aid coordination. As of June 2014, the LDF funding portfolio was at US$245 million, including commitments by donors and the government of Malawi for projects until 2019 (LDF-TST, 2015a, p. 3).

4.1. Earmarked Funding and Procurement Procedures

The LDF itself entails an administrative bypass, as funds are centrally managed by the LDF-TST instead of the National Local Government Finance Committee—the constitutional body mandated to manage all development funds that target local councils. Staff at LDF-TST sign contracts with the Ministry of Finance giving the impression that they are part of the civil service and using government salary scale. However, LDF-TST employees have different conditions of service from those of civil servants and their salaries are above those of Malawian civil servants. Since its operational costs are covered by the World Bank, the LDF-TST is often referred to as a 'super-NGO that implements activities of donors other than those of the central government and local councils.'[5] Contrary to the rules of the LDF pooled funding, donors earmark their resources. For example, the funding windows are attached to specific donors, which has allowed these actors to profile themselves as promoting specific developmental agendas. Thus, the World Bank funds three windows (local authority, perfor-

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[2] Hastings Kamuzu Banda (July 1964–May 1994), Bakili Muluzi (May 1994–May 2004), Bingu wa Mutharika (May 2004–April 2012), Joyce Banda (April 2012–May 2014), and Peter wa Mutharika (2014–present).
[3] These include the Malawi Growth and Development Strategy (MGDS II), the Decentralization Policy and the Local Government Act.
[4] Key informant interview, Ministry of Finance, Lilongwe, 4 December 2015.
[5] Key informant interview, Ministry of Local Government and Rural Development, Lilongwe, 8 December 2015.

mance, and community), which are used to implement public works programs and community demand-driven projects, mainly related to the construction of school blocks and teachers' houses.

A senior official of the LDF-TST observed:

> When we receive funding from the World Bank, we actually know what it is supposed to be spent on. Any expenditure outside the identified project is queried. The next thing are threats on suspension and withdraw of funding as well as penalties.'[6]

It is important to note that these are the very same activities that the Bank previously funded (1995–2007) during the final three stages of the MASAF. The KfW and AfDB fund and identify closely with the urban window. And KfW funds are earmarked for two activities: 'construction of socio-economic infrastructure (bus stations, stadia, markets, administrative buildings) and urban planning' (LDF-TST, 2015b, p. 15). Just as the World Bank's record of funding activities reflects its previous LDF priorities, the KfW had previously funded and prioritized construction activities (through the Secondary Centers Development Program) for over two decades in the period 1985–2007. In the LDF, KfW activities are aimed at boosting the local revenue base of urban councils. For this purpose, it has provided earmarked funds for the construction of markets, bus stations, and halls (LDF-TST, 2015a). Even in the LDF where councils 'identify' their projects and the KfW provide funds, the projects are similar to those that the KfW funded under Secondary Centers Development Program (LDF-TST, 2015b). And just like the World Bank and KfW, the AfDB continues to fund LDF activities in local economic development that are very similar to the Poverty Alleviation Program it funded in 1994.

Given such trends, a local government official claimed that the:

> LDF has been an earmarked fund from the start. The 'pooling' of funds is to the convenience of donors because they want to be seen to be using government structures although that is only on paper and not in practice. Government does not determine which projects are to be implemented.[7]

Such earmarked funding in the LDF was justified by donors as a necessary strategy to ensure that aid is allocated to strategic interventions. As an official of the AfDB noted:

> We do our assessments that tell us which projects and activities to fund. Above all, we also do have our own

areas of interest indicated in our Country Development Assistance Plan that we have to stick to. Earmarking is in the best interest of donors and stakeholders we serve—government and community members.[8]

While the government provides, in principle, matching funds to all LDF windows, donors have continued to fund their pet projects but give the impression of aligning their interests with the financing arrangements and priorities of the government. Indeed, the impression given is that previously prioritized funding mechanisms are now 'surrendered' to the LDF for the sake of improved effectiveness and in the name of good 'donorship.' One of the implications of this arrangement is that each funding agency (donor) requires the LDF-TST to follow their own specific procedures which means the Malawi Procurement Act is not applied when procuring goods and services with funds from donors. An example is the advert on a technical audit for beneficiary verification of the World Bank-funded social cash transfer program in Malawi. In the advert, it was highlighted that 'the selection Procedures shall be in accordance with the World Bank Procedures' (LDF-TST, 2018, p. 2). Similarly, the shortlisting and selection of audit services for the AfDB-funded projects were based on the 'the selection procedure…in accordance with the AfDB's Procurement Policy Framework' (LDF-TST, 2015b, p. 2). Such diversity in procurement procedures creates numerous challenges—including increased costs, delays in decision-making, an overwhelmed and overburdened administrative staff, and delays in project completion—which in turn impact the effectiveness of aid.

Managing aid is not easy and there are numerous challenges on the ground. A respondent from the LDF-TST noted: 'We work under pressure and get confused. We have several forms to complete and reports for individual donors to write. We are told they need to have their impact isolated from other donors.'[9] Indeed, the dominant narrative was that the LDF-TST and local council officials spend a considerable amount of their time on paperwork.

4.2. Coordination and Project Management

Realizing that the country in 2009 had over 48 PMUs that involved 12 donors and functioned parallel to national institutions and policies, the government banned the creation of new PMUs (Nilsson & Nkhoma, 2014). Many of the existing PMUs were in the health sector. Despite the presence of a relatively well-organized Sector-Wide Approach (SWAp), around 100 projects from more than 20 donors provided funding to the health sector outside of the SWAp arrangement (OECD, 2012).[10] The

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[6] Key informant interview, LDF-TST, Lilongwe, 7 January 2016.
[7] Key informant interview, local government official, Directorate of Planning and Development, Mangochi, 19 January 2016.
[8] Key informant interview, AfDB, Lilongwe, 22 March 2016.
[9] Key informant interview, LDF-TST, Lilongwe, 25 March 2016.
[10] SWAp refers to mechanisms 'by which Governments and donors can support the development of a sector in an integrated fashion through a single sector policy and expenditure programme, under Government leadership, using common management and reporting procedures and progressing towards the use of Government procedures to disburse and account for all funds' (Foster & Fozzard, 2000, p. 55).

government indicated that its intention was to phase out 27 PMUs by 2011 (OECD, 2012). However, by 2015, Malawi still had 33 PMUs—six more than the 27 parallel PMUs that the Government was aiming to have by 2011. Moreover, parallel PMUs were still being established upon the insistence of donors, who made the establishment of parallel PMUs a condition for providing aid. Thus, of the 33 parallel PMUs that existed in 2015, six were affiliated to donors participating in the LDF—World Bank (5) and AfDB (1).

In our interactions with the World Bank and KfW, we were often told that the LDF is a pooled fund managed by the Government of Malawi. However, government officials claimed that the LDF is managed by a parallel PMU—termed the LDF-TST. As an official in the Ministry of Local Government put it:

> The LDF-TST operates outside government machinery…[O]n paper they appear to be part of government but in practice they are not—they do their own project planning, handle finances and hold discussions with donors on their own.'[11]

Indeed, having investigated the organizational set-up of the LDF, we find considerable evidence that the LDF-TST is a parallel bypass rather than an embedded PMU. For an entity to qualify as an embedded PMU, it must meet three of the following four criteria: (a) the terms of reference for externally appointed staff are determined by the country implementation agency, not by the donor; (b) the most senior staff member is to be appointed by the country implementation agency, not by the donor; (c) the salary structure of national staff (including benefits) must not be higher than that of civil service personnel; and (d) the entity must be accountable to the country's implementing agencies, and not to external funding agencies (Ministry of Finance, cited in Said, McGrath, Grant, & Chapman, 2011, pp. 27–28). We find that the LDF does not meet three of the criteria above. In fact, the LDF only meets one criterion—the most professional staff member is to be appointed by the country implementation agency, and not by the donor. Even this criterion has to be qualified because donors have a say in the recruitment of professionals as they vet all applications. A senior AfDB official, however, disagreed with this view: 'We have always believed that the LDF-TST is government institution because employees have their job contracts with government.'[12]

What then is the problem with PMUs such as those in the LDF? Many argue that PMUs are actively used to bypass government structures by virtue of the enormous amount of funds committed to project support (under donor control) as opposed to general budget support and sector support, which constitute pooled funds under the control of the government. For example, in 2011, 67% of all foreign aid resources in Malawi were allocated to project support and a mere 10% to budget support and 23% to sector support (Malawi Government, 2015b). Thus, a large chunk of aid is disbursed through channels that are not preferred by the government. The major concern is that such active bypass of public institutions neither contributes to capacity building of weak institutions nor improves the already fragile legitimacy of political and administrative structures. The circumvention denies public institutions the opportunity to acquire experience by working with the best practices and policies that donors would like them to follow in order to deliver effective aid. The result appears to be 'amplification effects' (Ndulo, 2014) in that institutions that were weak remain weak while those that were strong become stronger. For example, the LDF has fortified institutions like the LDF-TST that are outside the public machinery as donors have equipped such entities with the best and latest technology, in addition to adequate funding for attracting and retaining well-qualified staff, who in turn have been effective in lobbying influential actors both in the donor community and in government. A respondent from the Ministry of Local Government indicated, 'We see a trend of variation regarding councils responding to interventions. Performing councils are improving their operations. The struggling councils are still struggling.'[13]

The assertion by Booth (2011) that capacity will not be built in developing countries if donors continue to avoid working with existing institutions, is applicable in the context of the LDF. Instead of avoiding institutions, weak as they may be, the 'thinking and working politically' (TWP) literature on the integration of politics in development theory and practice (Dasandi, Marquette, & Robinson, 2016) calls for donors to work with local institutions in order to understand the factors blocking effective delivery of aid and to jointly devise solutions to problems. TWP emphasizes the point made by critics of bypass structures that aid modalities will have to change from 'best practice' to 'best fit' to align with donors' interests and programs of the recipient country. TWP also insists that both donors and aid recipients are active political players in the aid industry and as such the 'best fit' modalities need to be informed by the politics of institutional design between the givers and recipients of aid.

4.3. Aid and Patronage Politics

One of the main reasons donors bypass the government machinery is to avoid political and administrative mismanagement and resulting delays, as well as misallocation of aid. The LDF is a major financing mechanism for local development and decentralization projects at the local council level in Malawi. The power enjoyed by the PMU (LDF-TST) rests mainly on its ability to prevent aid being used for unintended purposes. The country's po-

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[11] Key informant interview, Rural Development Officer, the Ministry of Local Government, Lilongwe, 19 April 2016.
[12] Key informant interview, Senior Technical Officer, AfDB, Lilongwe, 12 April 2016.
[13] Key informant interview, Ministry of Local Government and Rural Development, Lilongwe, 27 March 2016.

litical elites have historically shown considerable interest in using aid for personal economic gain as well as political survival. Given the massive funds that go into the LDF, successive ruling parties have tried to ensure that their chosen candidates are appointed to the most senior positions. This helps to make policies and strategies more palatable to the government, especially in relation to the distribution of LDF resources. Political appointees ensure that their political patrons can continue to access resources that would otherwise be difficult to gain control over. These officials tend to support projects championed by political parties. As an LDF official noted, 'those appointed based on political colors are willing to help the political party to get resources with or without technical justification.'[14] An example was the executive director of the LDF (2009–2010) who ensured that two successive ministers of local government were able to establish so-called Rural Growth Centers in their home districts despite those not originally meeting the selection criteria.[15] An official in the Ministry of Local Government claimed that the incumbent minister had 'influenced allocation of funds to a stadium under the urban window to his constituency when it did not qualify using the set criteria.'[16]

Another way for politicians to promote their personal interests is to encourage temporary ('acting') appointments at the LDF. Such interim officers offer less resistance than permanent staff and are more likely to redirect resources according to the preferences of influential political leaders. The local communities also highlighted the role of Members of Parliament in diverting resources for projects that were not earmarked: 'The MP interferes a lot with our work in the Village Development Committee. When the MPs fails to get a project at the district council, he/she approaches us with directives'[17] Thus, we find considerable evidence of political pressure on the LDF-TST to allocate projects in the constituencies of political elites, including serving Cabinet ministers. And a popular view among officials in the ministries of local government and finance was that each minister was able to influence projects in their respective constituencies that would not have been possible if proper procedures were followed.

The LDF experience demonstrates that even when there is an attempt to ring-fence and protect aid, recipient country politicians continue to find ways of accessing and influencing the distribution of such resources. Both public officials and donors routinely break resource allocation procedures when faced with pressure from elected representatives in order not to be seen as sabotaging a country's political agenda and demonstrating loyalty to the wishes of the electorate. In diverting aid to personal projects and rewarding individuals and groups for political support and loyalty, both formal and informal rules of the game are applied. Informal procedures are particularly popular when the interests of donors differ from those of elected representatives, MPs and council members. The case of diverting Rural Growth Centers to districts that are politically important is an example. The Malawian government borrowed US$14 million and US$3.04 million from the AfDB and the World Bank, respectively, to fund the construction of these centers (AfDB, 2008). When the criteria to allocate such centers to particular districts were manipulated by political leaders, the AfDB did not raise any concern, possibly for fear of jeopardizing the credit agreement with the Malawian Government relating to the loan.

The Malawian case is not unique in this regard. Using regional data on aid projects financed by the World Bank and the AfDB, Ohler and Nunnenkamp (2014) analyze the motives governing these two multilateral donors' aid allocation in 27 African countries. They do not find any evidence supporting the donors' claim that more aid was given to the neediest regions, defined on the basis of infant mortality, maternal health, and undernutrition. Rather, regions in which political leaders were born were likely to receive more multilateral aid, in particular for physical infrastructure projects, which reinforces the belief that political favoritism plays an important role in aid allocations (Nunnenkamp, Ohler, & Andres, 2017). Thus, aid deepens patronage politics as political elites find ways of using aid for their political purposes.

Donors can hence play Ostrich politics in that they may deliberately ignore local malpractices, turning a blind eye to the messy realities of development projects as long as their own interests are served (Reltien, 2001). Our review of the LDF experience in Malawi suggests that there is a possibility of donors becoming clients. This is particularly the case when a donor or a group of donors face competition from others for the attention of recipient country policymakers. The growing influence of new development actors such as China is putting pressure on more established actors to compromise their standards in order to stay relevant. Such an understanding departs from the mainstream literature, particularly on public sector reforms, which indicates that it is only recipient countries that practice clientelism and patronage. While Hyden (2008) suggests that donors are complicit in promoting patronage in aid-receiving countries, we argue that donors can be patrons or clients—or what Yanguas (2014) calls 'spoilers'—depending on the context. Development agencies are under pressure to perform to the satisfaction of their constituents and the head office. The head office is in turn interested in seeing the

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[14] Interview, LDF-TST official, Lilongwe, 13 April 2016.

[15] LDF-TST has had four Executive Directors since its inception: Sam Kakhobwe, Edward Sawerengera, Ted Kalebe, and Charles Mandala.

[16] Interview, Ministry of Local Government official, Lilongwe, 9 April 2016. In the same interview, the respondent narrated how another minister appointed by the People's Party administration influenced the construction of a community secondary school and a market in her constituency. The projects remained unfinished as the contractor and the Ministry of Local Government are currently fighting a court case related to corruption as part of the Cashgate scandal.

[17] Interview with member of the Village Development Committee chairperson, Thyolo, 7 March 2017.

impact and attaining value for money for the resources pumped into a particular country. The processes and procedures required to achieve positive impact and value for money may be of less concern to the head office, hence the temptation for the country staff of aid agencies to act as clients (abiding by the wishes of local politicians) as they focus on delivering what is expected of them by the head office. The emerging consensus in the context of the 'messy politics' of development aid (Yanguas, 2018) is that aid has small but positive effect on economic growth (Arndt, Jones, & Tarp, 2011). The diverging views on about aid effectiveness originate from difficulties in measuring the effectiveness of aid (Bourguignon & Sundberg, 2007).

4.4. Institutional Design and Competition for Scarce Resources

The LDF is designed as a collective financing mechanism for local development realized through the harmonization of donor-funded projects under the stewardship of the Malawian Government. All district councils should thus ideally interact with the government rather than with individual donors. In 2008, the government also made a commitment to harmonize its institutional procedures relating to local development financing and established a single institution—the National Local Government Finance Committee—with the mandate to interact with local councils and other players on matters of local development (GOPA, 2005). However, in practice we find that this mandate is shared with the LDF. And the current institutional design allows local councils and others to interact directly with the LDF-TST, contrary to the original design. This is a major problem because while the Ministry of the Local Government and Rural Development and the National Local Government Finance Committee are bypassed in decision-making, these are in reality the institutions with the official mandate to guide local councils. Thus, the donors use the LDF-TST to interact with local councils and implement their projects rather than engaging with the national government.

Since the LDF offers four financing windows, there is now increased competition in the local development sector to access multiple opportunities for funding. The windows that are heavily competed for are those funded by donors—the community, local authority, and urban windows. Because of its perceived pressure to fund local council activities as well as serve donor and government interests, the LDF has extended its mandate to become a grants provider. Thus, the LDF now plays a double role—as a grants-issuing institution as well as a PMU. For example, it awards projects to local councils in the urban window. And for all such projects funded by the KfW, allocation is based on an expression of interest through a donor-influenced application process which differs from the constitutionally approved inter-governmental fiscal formula. Thus, in the KfW urban

window-funded projects, local councils submit their applications and compete among themselves for the grants. The essence of awarding projects on a competitive basis is that councils have to prove that they have both the technical and financial capacity to complete projects within an agreed timeframe. While such competition in awarding projects may promote innovative solutions (e.g. construction of stadia and bus depots for local revenue generation), implementation is often problematic as councils tend to undercut practices among themselves in a bid to win the projects (e.g. district versus town councils). By becoming a grants-issuing unit, the LDF is searching for ways to survive the government-led restructuring process and yet doing a disservice to the aid effectiveness agenda.

The competition to attract the resources that come with aid is also intense among elected representatives at both central government and local council levels. Such competition among councilors and MPs occurs view of other local development financing mechanisms under the control of the central government (e.g. the Constituency Development Fund and the District Development Fund). The increase of earmarked funds for a specific group (such as the Constituency Development Fund for MPs) leads to increased competition among different stakeholders. A local council official made a critical observation: 'We implement many projects that are outside the development plans hence sometimes not really responding to the immediate needs of our local communities.'[18] The implication is that councils often experience allocative inefficiency as resources are invested in wrong sectors/areas.

Scholars have previously argued that MPs and councils in Malawi routinely tussle over financial resources to develop their local constituencies (Cammack, Kanyongolo, Gooloba-Mutebi, & O'Neil, 2007; Chinsinga, 2005; Tambulasi, 2009). This competition for resources has a negative impact on aid effectiveness as both administrators and elected officials aim to use the resources for career development and for gaining political mileage, which do not necessarily address the needs of local individuals and households.

5. Conclusion

While the international aid discourse routinely highlights the importance of harmonized policies and aid disbursements aligned to the needs and goals of recipient governments, there are numerous disagreements on how this can best be implemented on the ground. The aim of our study was to examine how and to what extent multilateral and bilateral development agencies bypass national and local government institutions while channeling aid, and the impact of such practices on aid effectiveness in Africa. A focus on Malawi, which is heavily dependent on bilateral and multilateral aid, is particularly illustrative of the challenges many countries face in achieving devel-

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[18] Key informant interview, Thyolo District Council, Thyolo, 18 March 2017.

opment outcomes when their policy space is limited by the dominant role and influence of external actors in national policymaking and implementation.

Donors often choose to bypass national institutions while implementing development projects citing inadequate administrative capacity, unhealthy political interference, and widespread corruption. In turn, political leaders in aid-dependent countries often choose not to challenge donor practices and reforms that are aimed at bypassing national institutions in order to improve aid effectiveness. Rather, they may choose to focus their energy on devising ways to redirect such resources to themselves and their preferred constituents.

The LDF was established to address the growing problem of uncoordinated aid targeting local development. In order to check the uncontrolled proliferation of aid agencies and projects, a basket fund was created that would pool resources and strengthen the ability of the government to better devise and implement development and governance projects at the local level in line with its national policy priorities. In practice, however, the LDF has ended up largely bypassing administrative structures, which in turn has created frustration among officials and slowed down the aid effectiveness agenda in Malawi. Earmarked funding, specialized procurement arrangements, and the establishment of specialized PMUs are among the mechanisms used by donors to circumvent the involvement of national institutions. Thus, we find that aid sometimes weakens the very institutions it aims to build and strengthen (Booth, 2011).

The growing use of bypass mechanisms to circumvent the public administration in aid-receiving countries is often justified on the grounds of weak public financial management system, low local capacity to absorb aid, low capacity to formulate and implement programs, high risk of using aid for unintended purposes (patronage and clientelism), and mixed priorities on the part of both donors and aid recipients. But donors also have their own reasons that compel them to create bypasses— visibility/publicity of their efforts, documenting attribution and impact of their projects, and pursuing specific goals such as policy influence. However, rather than promoting the principles of greater local ownership, capacity building, and harmonization of available external resources, we find that bypass mechanisms are fragmenting aid delivery and negatively affecting its effectiveness.

Our findings support the call by Eichenauer and Reinsberg (2017) for a re-examination of the hypothesis that bypasses are always the fastest way of delivering aid to local communities. Indeed, not all bypasses will be successful in delivering quick and efficient aid to local communities. Rather, the effectiveness of bypass strategies will depend on the type of institutional design adopted and the existing rules of the game that incentivize actors to make quick decisions and implement such policies effectively in line with original intentions. The reality on the ground is that recipient country governments continue to prefer budget support, as this gives them more control of resources and allows them to make decisions regarding expenditure and allocation. In contrast, many donors are showing a growing preference for project and program aid, which they can more easily control. The aid architecture in Sub-Saharan Africa has, in recent decades, witnessed a pendulum swing from project/program aid to budget support and then back to project aid.

The Malawian case demonstrates that aid effectiveness is compromised by the reluctance of both donors and recipients to more aggressively confront problems that arise from the practice of patronage politics. We conclude that while bypass practices may achieve short-term gains by displaying successful and visible 'donorship,' the long-term impact is more uncertain. The overall conclusion is that the bypassing of local institutions results in fragmentation of aid, lack of coordination among aid industry actors, and a general weakening of policy space and domestic capacity to formulate and implement development policy.

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Conflict of Interests

The authors declare no conflict of interests.

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Article

Too Many Cooks in the Kitchen? The Division of Financing in World Bank Projects and Project Performance

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Abstract
The total funding envelope for World Bank projects is often divided among various state and non-state actors, each of which can have competing ideas about or interests in the project. How does the division of financing relate to overall project effectiveness? I argue that too many funding streams in a project can reduce project effectiveness by creating delays, increasing transaction costs, and blurring lines of accountability. I combine original data on the number and concentration of financial collaborators in World Bank projects with the World Bank's ratings of project performance, looking at within-country variation across projects to explore whether or not there is evidence of reduced aid effectiveness in projects with more participants. The results suggest that projects with significant cofinancing receive somewhat worse project ratings.

Issue
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1. Introduction

Major foreign-financed development projects often involve cooperation on the part of multiple actors. Many projects involve initial negotiations between a donor and a national government, followed by donor monitoring of the project's implementation by an agency of the national government, subnational government entities, private contractors, or non-governmental organizations (Winters, 2010). For a given project, there can be multiple implementers, responsible for different project components, and the success of certain components might depend on activities happening in other parts of the project that are administered by different entities. Beyond a single foreign donor and an implementer, cooperation in a development project may include cofinancing by multiple foreign and/or domestic sources. In many cases, the reality of a development project is much more complex that a straightforward principal-agent relationship in which a single foreign donor provides funding to an aid-receiving government for project implementation by a single relevant ministry.

In this paper, I argue that involving more actors in the design and implementation of development projects risks less satisfactory outcomes. Having more actors cooperating in a single development project increases transaction costs, increases the likelihood of implementation delays, and reduces the clarity of lines of accountability within a project. Each of these problems risks undermining development impact.

I explore this hypothesis using outcome ratings produced for World Bank projects by the World Bank's Independent Evaluations Group and data from Winters and Streitfeld (2018) about the division of financing in those projects. I find that the presence of multiple financers correlates with slight decreases in the likelihood of a project receiving a satisfactory rating. This is particularly true for the small number of projects that use funding from non-governmental organizations in the aid-receiving country. Other patterns related to particular types of cofinancers are less robust.

2. Project-Level Determinants of Aid Effectiveness

While the most common approach in the literature to understanding aid effectiveness is to study the macroeconomic impacts of aid on growth (see Qian, 2015, for a recent review), another stream of research studies project-level assessments of development outcomes, trying to understand the characteristics of successful projects and/or the contexts in which development projects are most successful. In some of the earliest entries in this literature, scholars use cross-project data to assess whether or not local participation in development projects improves development outcomes (Finsterbusch & Van Wicklin, 1987; Isham, Narayan, & Pritchett, 1995). Other studies focus on how the political or economic characteristics of aid-receiving countries affect project success (Denizer, Kaufmann, & Kraay, 2013; Dollar & Levin, 2005; Guillaumont & Laajaj, 2006; Isham & Kaufmann, 1999; Isham, Kaufmann, & Pritchett, 1997), although a key finding of Denizer et al. (2013) is that development project success varies more within countries than it does across countries. Some studies focus on particular actions taken by aid agencies in preparing projects or during their implementation (Deininger, Squire, & Basu, 1998; Denizer et al., 2013; Kilby, 2000, 2015). Recent work has situated aid agencies within donor governments more broadly and argued that greater aid agency autonomy leads to better development outcomes in challenging contexts (Honig, 2018, in press).

In an article that undertakes an analysis closely related to the one presented here, Shin, Kim and Sohn (2017) analyze the relative performance of World Bank projects implemented by the borrowing government versus those implemented by non-governmental actors or by a combination of governmental and non-governmental actors. Although the number of World Bank projects implemented by non-governmental actors is small—only 29 out of the 647 projects that they study—they consistently estimate that these projects have better evaluation scores, controlling for the total size and duration of the projects and the country, year, and sector of their implementation. They additionally find that the likelihood of having a positive project evaluation is increasing in the number of non-governmental organizations involved in project implementation: a finding that runs counter to the argument and evidence presented below.

3. The Challenges of Multiple Principals in Development Projects

On the one hand, involving multiple actors in development projects suggests the possibility of important benefits. In terms of financing, the involvement of more actors can lead to a greater resource envelope, making it plausible that a development project will be able to do more of what it is supposed to do (e.g., build schools, train bureaucrats, finance microlending). In addition, if

the counterfactual scenario to having multiple principals collaborating on a single development project is that the different development industry actors would undertake separate, overlapping projects, then there should be efficiency gains to be made by coordinating aid and avoiding duplication (Acharya, Lima, & Moore, 2006). If some of the actors that become involved in a development project are local to the project sites, the existing evidence suggests that this will improve project performance (Isham et al., 1995).

On the other hand, expanding the set of actors that are involved in designing and implementing a single development project can raise challenges that might prevent that development project from meeting with success.

First, additional actors imply more transaction costs. Project resources that could be spent on concrete outputs may instead be lost to mundane administrative tasks because of the increased need for meetings and reporting. As Knack and Rahman (2007), drawing on the observations of Van de Walle and Johnston (1996), describe, donors like to send expert missions, and those missions like to meet with key government officials and obtain comments for their reports. And different donors may want information provided in specific formats. Citing a World Bank press release, Knack and Rahman (2007) refer to a survey in Bolivia sponsored by five different donors, each of which had specific financial and technical reporting, "leading the government official assigned to the project to spend nearly as much of her time meeting these requirements as in undertaking the actual survey" (p. 178).

The friction association with increased transaction costs may lead to suboptimal design or implementation decisions. For instance, if multiple principals need to sign off on changes to the project during implementation, this likely means that project implementation will be less flexible and less responsive to changing conditions on the ground, mirroring the problems that Honig (2018, in press) identifies when the headquarters office of aid agencies exerts too much control.

Second, a project that is reliant on funding from multiple sources is more likely to encounter project delays. A review of World Bank Implementation Completion and Results Reports reveals that funding committed by government entities in aid-receiving countries often does not arrive, delaying project implementation and creating new transaction costs related to reallocating budgets or finding substitute financing (Winters, 2014). Entire project components may be dropped if one of the project funders proves unable to provide financing, and this may have knock-on effects for other project components that were supposed to build on the now-eliminated one. Once again, end-of-project reviews make clear that the delay or absence of counterpart funds is often blamed for project shortcomings (Winters, 2014).

Third, having multiple actors involved in development project implementation may reduce the clarity of

the lines of accountability within the project. It can become less clear to whom contractors and subcontractors are responsible and to whom project beneficiaries should report a concern about project implementation. If different project beneficiaries identify different actors as being in charge of the project, this may lead to reports of problems or other information not being aggregated in such a way that project principals can take action. The literature already identifies a "broken feedback loop" (Martens, 2002; see also Winters, 2010) in which project beneficiaries face great challenges in making their voices heard by aid donors; having multiple donors at play in a project may exacerbate this issue.

At the macro-level, the problems of having many donors operating in a single aid-receiving country have been studied in the literature on aid fragmentation, which typically associates more donors in a country with less positive economic or institutional outcomes (e.g., Djankov, Montalvo, & Reynal-Querol, 2009; Kimura, Mori, & Sawada, 2012; Knack & Rahman, 2007). Some of the most recent literature in this field suggests that more donors are not necessarily problematic for macro-level outcomes—a finding that may carry over to the current project-level study. Ziaja (in press), for instance, argues that fragmented democracy aid can be useful for the institutionalization of democracy because it presents a "marketplace of ideas" from which the aid-receiving country can draw. As compared to the argument above about multiple principals leading to friction that drives suboptimal decisions, this framework would suggest that more entities involved in an aid project may bring more ideas that can help increase the efficiency of the project. Gehring, Michaelowa, Dreher and Spörri (2017) argue that donor fragmentation will be less problematic to the extent that donors are willing to coordinate. At the project level, if there is substantial coordination, there is the possibility of minimizing transaction costs, delays, and unclear accountability chains.

4. Research Design

To study how complexity in project financing relates to project outcome ratings, I combine together information about financing in World Bank projects with project-specific outcome ratings. I use simple linear models and build on existing analyses in the literature. In the absence of an instrument predicting exogenous variation in the complexity of project financing, my findings should be understood as correlational and preliminary.

By "World Bank project," I refer to investment projects financed by the World Bank's two main lending arms, the International Bank for Reconstruction and Development (IBRD), which lends at near-market rates to middle-income countries, and the International Development Association (IDA), which provides concessional loans or grants to the world's poorest countries. In a small number of cases—known as "blend" projects—the financing comes from both the IBRD and the IDA. As de-

velopment policy lending (formerly known as structural adjustment lending) does not include cofinancing, that type of World Bank lending is not included in the analysis.

World Bank projects are designed by World Bank staff in collaboration with officials from the borrowing country. Unlike some other forms of foreign assistance, where the funding is provided to non-state actors—i.e., "bypass aid" (Dietrich, 2013)—World Bank assistance flows directly to the member governments that borrow from the Bank. Where there is cofinancing from other donors, it is likely that those other donors also have been involved in the design of the project. On the other hand, as detailed in Winters and Streitfeld (2018), cofinancing from within-country sources may be something that the government agrees to secure without actually incorporating the cofinancing entity into conversations about project design.

4.1. Operationalizing Development Project Funding Complexity

In order to study the extent to which having more principals involved in a development project correlates with project success or failure, I use data from Winters and Streitfeld's (2018) study of counterpart funding in World Bank projects. This data describes the number of entities making financial commitments to particular World Bank projects at the time of project approval.

The data in Winters and Streitfeld (2018) was collected in two waves from the World Bank Projects Database. Originally, the authors hand-coded the financing in 2,631 World Bank (IBRD/IDA) investment projects that were approved in the 2000–2010 period using the financing tables found in the Project Appraisal Document (PAD) or other publicly available documents. Later, the authors webscraped the "financing tab" of the World Bank Projects Database for 1,676 projects listed as having been approved during the 2011–2017 period.

The authors then recoded the funding information into a set of 13 categories reflecting different within-borrower entities and a set of 32 codes reflecting either specific (e.g., "United Kingdom") or generic (e.g., "Unidentified Bilteral Donors") international sources. In doing so, the authors collapsed together some information. Specifically, for the international codes, funding from any of the World Bank-administered trust funds was included in a general trust fund category; funding from the regional development banks was included in a generic regional fund category; and funding from other multilateral lenders was included in a generic multilateral donor category. In some cases, it may be possible that the collapsing of information has led to multiple funders being combined together in a single category. There also are generic local and international categories for cases where the documentation did not precisely identify the source of financing. The finance coding categories are listed in Table 1.

I use this underlying data to create a series of explanatory variables meant to proxy for project complexity in

Table 1. Coding categories for funding in World Bank projects.

Domestic Financing Categories	
Borrower	Local Communities
Borrowing Agency	Local Farmer Organizations
Borrowing Country's Financial Intermediaries	(Unidentified) Local Sources of Borrowing Country
Local Governments of Borrowing Country	Private Sector
Municipalities of Borrowing Country	Sub-Borrowers
NGOs of Borrowing Country	(Unidentified) Others
Local Beneficiaries	

International Financers	
Australia	The Netherlands
Austria	New Zealand
Belgium	Norway
(Unidentified) Bilateral Donor(s)	Private Sources
Canada	Regional Fund
Denmark	Russia
European Union	Saudi Arabia
Finland	South Africa
France	Spain
Germany	St. Kitts and Nevis
Ireland	Sweden
Italy	Switzerland
Japan	Trust Fund
Korea	Unidentified International Source
Kuwait	United Kingdom
Multilateral Fund	United States

Note: Categories are developed in Winters and Streitfeld (2018).

terms of the number of different principals involved in project design and implementation. First, I create a series of indicator variables for whether there was cofinancing from (1) any government entity on the borrower side, (2) any community-level entity on the borrower side, (3) any non-government organization in the borrower country, and (4) any foreign donor besides the World Bank. Second, I create a variable that counts the number of non-World Bank sources of financing in each project. In the data, 13 percent of projects feature only World Bank funding; 51 percent involve funding from the World Bank and one other source; and 36 percent involve funding from two or more sources in addition to the World Bank. Third, I create a Hirschman-Herfindahl concentration index in which I sum the squared financing shares of all categories of entities contributing to the project. Higher values of this variable indicate that a small number of financing entities provide the majority of project financing; when all funding comes from the World Bank, this index takes the value 1.

4.2. Measuring Project Success

As with a number of previous studies of project-level aid effectiveness, I take the outcome variable from the World Bank Internal Evaluation Group's Project Performance Ratings dataset (World Bank, 2011). I down-

loaded the version of the data dated 26 July 2018[1]. This data includes ratings from World Bank project assessments conducted by the Independent Evaluation Group (IEG) and is described in Independent Evaluation Group (2015). In line with previous studies, I use the "overall project outcome" variable from the dataset for my outcome variable.

I use the most recent assessment available in the data. For most projects, this is the overall project outcome rating found in IEG's review of the Implementation Completion Results (ICR) report submitted by the project team to IEG. Note that this document, known as either an Evaluation Summary or an ICR Review, is prepared by IEG staff based on the review of the project previously completed by operational staff. In an ICR Review, IEG staff base their review of the project on the criteria stated in the ICR, and they have the ability to overrule the rating provided by the operational staff in the ICR.

In addition to ICR Reviews, IEG undertakes in-depth, field-based project reviews for a small subset of projects, releasing a Project Performance Assessment Report (PPAR) based on original data collection about project performance. For 10 percent of the cases in the data, the PPAR rating supersedes the ICR Review rating.

The ratings are on a six-point scale: highly unsatisfactory, unsatisfactory, moderately unsatisfactory, moderately satisfactory, satisfactory, and highly satisfactory.

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[1] https://ieg.worldbankgroup.org/sites/default/files/Data/Data/MasterDataFile20180726.xlsx

The modal category in the data is moderately satisfactory (44 percent of projects). Slightly more than one-in-four projects (28 percent) is given one of the three unsatisfactory ratings with more than half of those being the mildest of the three. The highly satisfactory rating is reserved for only a few projects (2.2 percent). Because of the limited use of the extreme categories on the six-point scale, I also study an indicator variable that distinguishes between projects receiving any one of the three satisfactory ratings and projects receiving any one of the three unsatisfactory ratings.

I present summary statistics for both the outcome and explanatory variable in Table 2.

4.3. Specification

I use a linear model to study the relationship between the measures of project complexity and the project outcome ratings. Although I lack a source of exogenous variation in financing complexity that would help identify the causal effects of having more financing entities involved in a project, I control for some potential confounding variables. I control for the total project size to avoid introducing a potential spurious correlation in which larger projects are both more likely to involve more funding entities and more or less likely to be rated as satisfactory. I also control for sector fixed effects for similar reasons (i.e., that projects in certain sectors may be more or less likely to involve multiple actors and more or less likely to be rated as satisfactory). I include fixed effects for the year of the evaluation to account for changing standards within IEG over time that may also vary with temporal trends in the prevalence of cofinancing. I include indicator variables for whether the project funding comes from the IBRD, the IDA, or both (with IDA, the modal category in the data being the omitted category) in case there is a propensity for greater cofinancing in one branch of the Bank or the other and also a propensity for differential kinds of evaluations. The data include projects financed under eight different World Bank financing mechanisms: Adaptable Program Loans, Emergency Recovery Loans, Financial Intermediary Loans, Investment Project Financing, Learning and Innovation Loans, Sector Investment

and Maintenance Loans, Specific Investment Loans, and Technical Assistance Loans. Once again, given the possibility of variation in the likelihood of cofinancers and variation in the baseline likelihood of a satisfactory project across types of financing, I include a set of lending instrument fixed effects. I include an indicator variable for the type of evaluation on the grounds that PPARs may be more likely for more complex projects and also more likely to return harsher project ratings. Finally, and perhaps most importantly, I include country fixed effects to account for the possibility that certain countries may be more likely to have projects involving more or fewer partners and (for plausibly distinct reasons) may be more or less likely to have successful or unsuccessful projects. The point estimates on the project-level variables of interest are therefore based on within-country, within-year, within-sector, and within-lending-instrument variation in project complexity and project outcomes. I cluster the standard errors on country.

5. Results

Table 3 presents the results. Columns (1) through (4) use the six-point evaluation ratings scale as the outcome and columns (5) through (8) use the dichotomized outcome variable. According to the results in columns (1) and (5), the within-country variation in whether or not World Bank projects involve cofinancing is such that the presence of any non-World Bank funding in a project negatively predicts project outcome ratings. The effect sizes, however, are modest. The presence of cofinancing reduces the project rating only by 0.16 points on the six-point scale or, alternatively, the likelihood of the project being rated satisfactory falls by seven percentage points (against a baseline likelihood of 72 percent that a project will have a satisfactory rating).

In columns (2) and (6), I break down the presence of cofinancing by including a set of indicator variables for the different types of actors that might provide funding to the project: any government entity from the borrowing country, any local-level entity, any domestic NGO, and any other external donors. In column (2), the involvement of local government entities in financing the

Table 2. Summary statistics.

	N	Mean	SD	Min	Max
IEG Rating (Six-Point Scale)	2024	3.91	1.01	1	6
IEG Rating (0/1)	2024	0.72	0.45	0	1
Cofinancing (0/1)	2024	0.87	0.33	0	1
Borrower Gov't (0/1)	2024	0.83	0.38	0	1
Community/Local (0/1)	2024	0.12	0.33	0	1
Domestic NGO (0/1)	2024	0.00	0.07	0	1
Other Donors (0/1)	2024	0.29	0.46	0	1
Count of Cofinancers	2024	1.48	1.14	0	8
Concentration of Financing	2024	0.67	0.20	0.14	1
Log (Project Size)	2024	4.00	1.43	0.26	10.20

Table 3. Effect of project funding complexity on development project outcomes.

Outcome:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Six-Point Satisfaction Scale				Satisfactory/Unsatisfactory			
Any Cofinancing (0/1)	−0.16**				−0.07*			
	(0.08)				(0.04)			
Borrower Gov't (0/1)		−0.11				−0.04		
		(0.08)				(0.04)		
Community/Local (0/1)		0.14**				0.05		
		(0.07)				(0.04)		
Domestic NGO (0/1)		−0.51				−0.43**		
		(0.33)				(0.18)		
Other Donors (0/1)		−0.04				−0.01		
		(0.05)				(0.02)		
Count of Cofinancers			−0.00				−0.01	
			(0.02)				(0.01)	
Concentration of Financing				0.23				0.15**
				(0.14)				(0.06)
Log(Project Size)	0.10***	0.10***	0.09***	0.10***	0.05***	0.05***	0.05***	0.05***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	2,024	2,024	2,024	2,024	2,024	2,024	2,024	2,024
Adjusted R^2	0.12	0.12	0.12	0.12	0.06	0.07	0.06	0.07

Notes: All models are linear regression models that include indicators for IBRD and blend funding, an indicator for PPAR ratings, and country, year, sector, and lending instrument fixed effects. Robust standard errors clustered on country in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

project correlates with worse performance ratings (although not significantly so), whereas the presence of funding from community organizations correlates with better performance ratings, conditional on the other variables in the model. The signs of these variables remain consistent in column (6), but the uncertainty in their estimation increases. There is a large and negative coefficient in column (2) on the indicator for the presence of funding from a domestic NGO in the project; in column (6), with the dichotomized outcome, this effect becomes statistically significant at conventional levels. According to column (6), the presence of funding from a domestic NGO decreases the probability of a project getting a satisfactory rating by a striking 43 percentage points. There are only 10 projects in all of the data, however, where domestic NGOs provide funding. Given the numerous fixed effects in the model, this estimate may be based on the performance of an even smaller number of projects. The presence of foreign donors other than the World Bank does not significantly correlate with project performance in either column (2) or column (6); this variable produces coefficient estimates close to zero. Removing the indicators other than for the borrower government from the model and comparing those projects with borrower government involvement to all other projects produces coefficient estimates that are approximately the same magnitude and significance as those in Table 3 (results not reported).

In columns (3) and (7), the coefficient on the raw count of the number of actors providing funding to the project is negative but small in magnitude and not statistically significant. In columns (4) and (8), on the other hand, the concentration of project funding as measured by the Hirschman-Herfindahl Index is a positive predictor of project success (and a statistically significant one for the dichotomous outcome): where project funding is more concentrated, we can expect to see more satisfactory project outcomes. This variable ranges from 0.14 to 1.0 with a standard deviation of 0.2. The substantive effect of a one standard deviation change in the concentration of funding is therefore relatively small: a one standard deviation increase in the concentration of funding correlates only with a 0.05 point increase on the six-point satisfaction scale or a 3.0 percentage point increase in the probability of any kind of satisfactory rating, conditional on the other variables in the model. Although small, the result is consistent with the result presented in columns (1) and (5) that overall project ratings are lower, ceteris paribus, in the presence of cofinancing.

In all of the regressions reported in Table 3, total project size is a positive predictor of the project ratings. Denizer et al. (2013) find the opposite result, that larger projects receive less positive ratings, whereas Shin et al. (2017) report a similar finding as here, that larger projects are more highly rated. Denizer et al. (2013) study a much larger set of projects, going further back in history, whereas Shin et al. (2017) study projects from a time period similar to the one studied here. Therefore, this may be an association that has changed over time. Looking only at the bivariate correlation between log total project size and the IEG rating, the correlation is positive and highly significant.

For some of the indicator variables used in Table 3, some of the categories indicating the presence or absence of a particular type of actor mask the fact that multiple entities from that category were involved in the project. For instance, of the 1,688 projects in the data involving a domestic government actor, 79 of those involve multiple government actors. Likewise, of the 553 projects that involve financing from other foreign donors, 129 involve financing from two other donors; 49 involve financing from three other donors; and 30 involve financing from four other donors. In Table 4, therefore, I replace the 0/1 indicators for these types of financing with count variables for the number of financing entities under each of these categories. Column (1) uses the six-point scale as the outcome, while column (2) uses the satisfactory/unsatisfactory distinction.

The results in Table 4 largely follow those in Table 3. Using the six-point scale, we see a marginally significant positive correlation between community funding and project success; although we see a negative correlation between the count of domestic government agencies contributing to the project and the project outcome,

it is not statistically significant (as compared to the binary predictor in column (1) of Table 3). Using the dichotomous satisfactory versus unsatisfactory outcome, the presence of a domestic NGO remains a large and significant negative predictor of project success, and none of the other funding source variables are significant.

5.1. Project Delay Mechanism

As described above, cofinancing frequently is delayed, and project evaluations frequently complain about this delay in cofinancing and attribute problems in the project to the delay. Although it is challenging to find proxies for the transaction cost mechanism or the accountability mechanism described above, I can look for evidence of the project delay mechanism by studying the relationship between cofinancing and project length.

In column (1) of Table 5, I regress project duration (closing date minus approval date) on cofinancing, total project size, and the set of indicator variables for country, year, and project characteristics included in the main specification above. The results show that cofinancing

Table 4. Effect of project funding complexity on development project outcomes (additional count variables).

Outcome	(1) Six-Point Satisfaction Scale	(2) Satisfactory/Unsatisfactory
Count of Borrower Government	−0.05	−0.01
	(0.07)	(0.03)
Community/Local (0/1)	0.14**	0.05
	(0.07)	(0.04)
Domestic NGO (0/1)	−0.51	−0.42**
	(0.34)	(0.18)
Count of Other Donors	−0.03	−0.01
	(0.03)	(0.01)
Log (Project Size)	0.10***	0.05***
	(0.02)	(0.01)
Observations	2,024	2,024
Adjusted R^2	0.12	0.06

Notes: All models are linear regression models that include indicators for IBRD and blend funding, an indicator for PPAR ratings, and country, year, sector, and lending instrument fixed effects. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 5. Project funding complexity, project duration, and project ratings.

Outcome	(1) Project Duration (Years)	(2) Six-Point Satisfaction Scale
Any Cofinancing (0/1)	1.35***	−0.18**
	(0.13)	(0.09)
Project Duration (Years)		−0.02
		(0.02)
Log (Project Size)	−0.03	0.11***
	(0.05)	(0.02)
Observations	2,019	2,019
Adjusted R^2	0.22	0.11

Notes: both models are linear regression models that include indicators for IBRD and blend funding, an indicator for PPAR ratings, and country, year, sector, and lending instrument fixed effects. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

clearly leads to project delays. Controlling for project size, the presence of cofinancing is associated with an increased project duration of 1.35 years.

Longer projects, however, do not imply worse ratings. Column (2) shows that the measure of project duration is not significantly associated with project outcome ratings when we add it to the specification from column (1) of Table 3. The coefficient on cofinancing is of the same magnitude as above and remains statistically significant. Therefore, insofar as we are finding evidence that cofinancing negatively affects project ratings, the pathway does not seem to be through simply extending the duration of a project.

5.2. Additional Analyses

As described above, the main estimating equation controls for various project characteristics, such as the arm of the World Bank providing the lending and the specific category of loan. In addition, the evaluation data comes from two types of evaluations: the more superficial ICR Reviews and the field-based PPARs. In Table 6, I use these project and evaluation characteristics to subset the data and study the correlation between cofinancing and project ratings across various types of projects.

Columns (1) and (2) look at projects where the World Bank funding included in the project comes from either the IBRD or the IDA. ("Blend" projects that include funding from both are not reported because of their small number.) In both IBRD and IDA projects, the presence of cofinancing negatively correlates with project ratings, conditional on the other variables included in the model. The relationship is only statistically significant for IDA projects, but the two coefficients are not statistically distinguishable.

In column (3), I look only at specific investment projects. For these projects, the negative relationship between cofinancing and project ratings is reduced in magnitude and does not achieve conventional levels of statistical significance. Conversely, across the remaining types of World Bank lending, the relationship is larger and estimated more precisely than the overall relationship reported in Table 3. Column (6) suggests that cofinancing may be particularly deleterious in Emergency Recovery Loans: the coefficient indicates that the presence of cofinancing in these loans is associated with a project rating that is over half a point less than those found in otherwise similar Emergency Recovery Loans without cofinancing. The estimate in column (7) is far from statistical significance, but it provides some evidence that Technical Assistance Loans might receive higher project ratings when there is cofinancing. This would make sense, as government cofinancing might truly be a sign of government buy-in in these projects.

Columns (8) and (9) look to see if the results change when we subset the data to only the desk-based ICR Reviews or the field-based PPARs. The coefficients are indistinguishable, although given the relatively smaller number of PPARs, there is much more uncertainty in the estimate when we only use projects with that kind of evaluation.

Overall, these results reinforce the patterns identified in Table 3, although they suggest that problems brought about by cofinancing might be particularly deleterious in Emergency Recovery Loans and that cofinancing might actually be beneficial in Technical Assistance Loans.

6. Conclusions

I study whether or not multiple financing sources in World Bank projects are associated with undesirable project outcomes, combining data on project cofinancing with the IEG Project Ratings Database that has been used in previous work. I hypothesize that a diversity of funding flows may correlate with less satisfactory project performance because of friction associated with increased transaction costs, the possibility of delayed implementation, and the issue of blurred lines of accountability. Overall, I find indications in favor of the theory: World Bank projects with any cofinancing and with less concentrated financing receive less positive evaluations. The estimated correlations, however, are generally small in size, conditioning on other project characteristics and using country and year fixed effects.

Table 6. Project complexity and project ratings within subsets of projects.

Subset	(1) IBRD Projects	(2) IDA Projects	(3) Specific Investment Loans	(4) All Other Types of Loans	(5) Adaptable Program Loans	(6) Emergency Recovery Loans	(7) Technical Assistance Loans	(8) ICRRs	(9) PARs
Any Cof. (0/1)	−0.20 (0.23)	−0.17* (0.09)	−0.12 (0.12)	−0.29*** (0.11)	−0.15 (0.28)	−0.64** (0.31)	0.28 (0.56)	−0.19** (0.08)	−0.19 (0.55)
N	734	1,236	1,244	780	302	154	179	1,828	196
Adj. R^2	0.28	0.20	0.25	0.25	0.45	0.75	0.60	0.21	0.51

Notes: all models are linear regression models that include log(total project size) and country, year, and sector fixed effects. Columns (1) and (2) include an indicator for PPAR ratings and lending instrument fixed effects. Columns (3) through (7) include indicators for IBRD and blend funding and an indicator for PPAR ratings. Columns (8) and (9) include indicators for IBRD and blend funding and lending instrument fixed effects. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

When I try to identify if cofinancing streams from specific types of partners are particularly problematic, I find some evidence that local-level participation may lead to better project performance, a result that is in line with the existing literature (Isham et al., 1995). The partial correlation, however, is estimated with a fair amount of uncertainty, depending on the outcome variable, and is small in magnitude.

When local NGOs provide financing, however, I identify a large, negative correlation with project satisfaction ratings, which is significant for one of the two outcome measures. This finding contrasts with Shin et al.'s (2017) finding that World Bank projects with non-government implementers receive higher project ratings than those that are implemented by a government agency. In both that article and this one, the number of observations that include NGO involvement is small, such that it may be the case that a few good or bad projects drive the results. I was not able to obtain replication data from the Shin et al. (2017) article to explore the overlap in the projects that we are studying or our coding of NGO participation. While I look at whether or not domestic NGOs make a financial contribution to the project, they ostensibly identify World Bank-funded projects that are implemented by NGOs.

The study of project-level outcomes remains crucial for the overall study of aid effectiveness. Whereas overall aid flows result from a variety of geopolitical and domestic political processes—giving development-oriented practitioners less control, project-level design decisions are typically made by bureaucratic agents with sincere interests in development (Iannantuoni, Waeiss, & Winters, 2018; Winters & Streitfeld, 2018). Continuing to build up our knowledge about which project design elements facilitate development impact is an important endeavor for concretely making aid more effective.

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Conflict of Interests

The author declares no conflict of interests.

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Article

Do Countries Use Foreign Aid to Buy Geopolitical Influence? Evidence from Donor Campaigns for Temporary UN Security Council Seats

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Abstract
In recent years, donor countries have increasingly used different aid allocation channels to boost aid effectiveness. One delivery channel that has grown tremendously is 'multi-bi aid'—contributions to multilateral organizations earmarked for specific development purposes. This article examines whether donors use multi-bi aid to further their selfish goals—specifically, to garner political support for their ambition to become a temporary member of the UN Security Council. In this context, multi-bi aid is particularly beneficial to countries with limited experience as foreign aid donors; whose governance quality is weak; and which are more internationalized. Using a sample of OECD/DAC donor countries in 1995–2016, time-series cross-section analysis corroborates these arguments. The analysis draws on a new dataset of media reports proxying for donor interest in winning a temporary seat in the UN Security Council and extended data on multi-bi aid flows. The findings demonstrate that multi-bi aid may be a tool for geopolitical influence, with yet unexplored consequences for aid effectiveness.

1. Introduction

A growing body of work demonstrates that donor motivations matter for aid effectiveness (Dreher & Kilby, 2010; Dreher, Klasen, Vreeland, & Werker, 2013; Headey, 2008). Many observers infer donor motivations from different aid channels, arguing that bilateral aid advances selfish goals, while multilateral aid promotes collective goals by helping donors coordinate aid efforts, enhancing the credibility of aid commitments, and exploiting the expertise of multilateral bureaucracies (Hawkins, Lake, Nielson, & Tierney, 2006).

The traditional distinction between bilateral aid and multilateral aid, however, is no longer tenable. Multi-bi aid—donor contributions to multilateral organizations earmarked for specific purposes—has emerged as a 'third way' of allocating foreign aid beside traditional

channels (Reinsberg, 2017; Reinsberg, Michaelowa, & Eichenauer, 2015; Reinsberg, Michaelowa, & Knack, 2017). Inferring donor motivations is difficult for multi-bi aid because it combines motivational elements of both traditional modalities—the quest for control and the preference for cooperation.

Therefore, the motivations underlying multi-bi aid likely depend on additional circumstances. In some cases, multi-bi aid is purely developmental as donors use it to push multilaterals toward interventions yielding tangible results and efficiency-enhancing reforms (Reinsberg, 2017; Reinsberg et al., 2015; Sridhar & Woods, 2013). In other cases—which are relatively underappreciated in the current literature—multi-bi aid may further the foreign policy goals of the donors.

As a test case for foreign-policy uses of multi-bi aid, I examine aid growth in the context of donor campaigns

for a temporary seat in the United Nations Security Council (UNSC)—the only global institution to authorize the use of force and as such an important venue for wielding policy influence. Donor countries may therefore use foreign aid to win support for their UNSC campaign among UN member states. However, not all aid types are equally beneficial and uniformly available to all donors. In particular, donor countries with less experienced aid agencies are unable to muster capacities for short-term aid increases and should therefore be particularly likely to increase multi-bi funding, as a sufficiently targeted and quickly disbursable aid channel. Furthermore, donors with weak governance should use multi-bi aid to advance their UNSC campaign, as bilateral assistance would raise public concerns about non-development uses of aid. Finally, multi-bi aid provides internationally-oriented donors with an instrument to undergird their preference for international cooperation while creating the regional co-benefits that multilateral aid is unlikely to provide.

I test my theoretical arguments using data for up to 35 Organization for Economic Cooperation and Development (OECD)/Donor Assistance Committee (DAC) donor countries over 1995–2016. I analyze whether the campaign activity of a donor for a temporary UNSC seat predicts contemporaneous aid increases. I proceed with fixed-effect linear panel regressions. In further analysis, I also take potential endogeneity of campaigning with respect to aid growth into account. In both cases, I find that donors support UNSC campaigns with multi-bi aid specifically when their own aid agencies are less experienced, when their governance frameworks are weak, and when they are more internationalized. To demonstrate the plausibility of these findings, I provide quotes from interviews with government officials.

My study contributes to the literature on the political economy of international organizations and aid allocation. Within the broad literature on the UNSC (e.g., Malone, 2000; Mikulaschek, 2018; Prantl, 2005; Voeten, 2001; Vreeland & Dreher, 2014a), some research focuses on who gets elected to the institution (Dreher, Gould, Rablen, & Vreeland, 2014; Schmitz & Schwarze, 2012; Vreeland & Dreher, 2014b), whereas other research explicitly links aid allocation and multilateral lending decisions to temporary UNSC membership of recipient countries (Dreher, Lang, Rosendorff, & Vreeland, 2018; Dreher, Sturm, & Vreeland, 2009; Kuziemko & Werker, 2006; Lim & Vreeland, 2013; Vreeland & Dreher, 2014a). In contrast to these studies, I focus on the strategies that donors use to enhance their chances of getting themselves elected to the institution.

In addition, I complement existing research on the determinants of multi-bi aid (Eichenauer & Reinsberg, 2017). My analysis establishes conditions under which multi-bi aid is attractive for geopolitical purposes, as the posited effect is only present for small donors without entrenched aid bureaucracies. By focusing on one specific aspect of geostrategic use of aid, I extend previous

aid allocation research, which often uses broad proxies for donor interest (Alesina & Dollar, 2000; Berthélemy, 2006; Hoeffler & Outram, 2011). Indeed, the rise of multi-bi aid has created new avenues for donor influence—with yet unknown consequences for aid effectiveness. Previous research suggests that geopolitically motivated aid is less effective, for instance due to sloppier project preparation under time pressure, the approval of lower-quality projects, and lower compliance with conditionality (Dreher et al., 2013; Kilby, 2013; Stone, 2009). Whether this holds also for multi-bi aid is an open question though, given that donor motivations cannot easily be inferred for this type of aid.

2. Argument

Countries vie for UNSC membership for three main reasons: gaining international prestige and reputation for being a responsible nation in the pursuit of global peace; advancing the national position on an upcoming issue; and promoting broader objectives and collective norms (Malone, 2000, pp. 6–7). Official statements reflect that developed countries consider UNSC membership to be valuable to them. For instance, a Canadian representative, leading his country toward a bid for a non-permanent seat in 2011–12, argued, "[g]enerally speaking, you can see that the big issues do get discussed at the UN Security Council…[w]hen you take yourself out of that game, you lose one of the vehicles you have for having some influence" (The Canadian Press, 2014). In an earlier bid, Canada had announced to use the influence afforded by a UNSC seat to promote "human security," following up on its earlier success in pushing for an anti-personnel landmine convention and the International Criminal Court (Malone, 2000, p. 7).

The UNSC consists of the five permanent members (P5)—China, France, Russia, the United Kingdom, and the United States—and ten non-permanent members which are elected in periodic ballots at the UN General Assembly (UNGA) to serve two-year tenures. Besides "contributions to the maintenance of international peace" (United Nations, 1945, art. 23), the UN Charter requires equitable geographical representation of non-permanent members, which is ensured by allotting the temporary seats to five regional groups in the following manner: three seats for the African group, two seats for the Asia-Pacific group (APG), two seats for the Latin American and Caribbean states group (GRULAC), one seat for the Eastern European group (EEG), and two seats for the Western European and Others Group (WEOG).[1] Half of the temporary seats are up for grabs each year, which implies different schedules of UNGA elections across caucus groups. Every even year, two seats for WEOG, one for the APG, one for GRULAC, and one for the African Group come up for election, whereas every odd year, states vie for two seats for the African Group, one for the APG, one for the EEG, and one for GRULAC.

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[1] According to a gentlemen agreement, Africa and Asia make available one seat for an Arab country in alternating order.

The selection of the non-permanent UNSC members follows a two-stage process. First, countries self-nominate for candidacy in their respective regional group. They simply declare their candidacy to their regional chair in New York, which then adds the country to the list of candidates (Security Council Report, 2009, p. 9). Most candidates inform UN members of their candidacy afterwards. While some regional groups have established informal practice of rotation-based nominations, other groups have no established procedures to coordinate the nomination process. In the former groups, the sole candidate for an open seat typically has regional endorsement. In the latter groups, however, competition for seats is intense, as the number of candidates exceeds the number of available seats. For example, WEOG follows an "open-market practice" (Malone, 2000, p. 5) in which members compete over seats freely. For example, the current WEOG seats are split among three countries—Sweden for a full tenure and Italy and the Netherlands serving subsequent one-year terms (United Nations, 2016). As a result, countries aspiring for UNSC membership have come to announce their candidature far in advance, in the hope to remain uncontested. For example, Switzerland has announced its candidature for 2023–24 in 2011 (Federal Department of Foreign Affairs, 2018). Competition for the Eastern European seat is similarly high. Contested ballots are not uncommon: 9 out of the 12 elections in 1990–2015 were initially contested. For example, four countries—Armenia, Azerbaijan, Hungary, and Slovenia—declared interest to represent the region in 2012–13, but given regional pressures to avoid contested elections, several countries dropped their aspirations later in the process.

Following the nomination phase, the UN member states vote for candidates in the UNGA. The election is a secret ballot and a candidate requires two-third majority approval of all countries participating in the ballot in order to be elected as temporary UNSC member. If no candidate obtains the required majority, run-off votes continue until one of the candidates is elected. UNGA elections typically take place in the last quarter of a year, with new tenures beginning in the first month of the subsequent year. After a country has served its two-year term in the UNSC, it cannot immediately re-apply for candidature for two years (United Nations, 1945, art. 23).

To further their chances of election for a temporary UNSC seat, states recur to several tools of foreign policy. Based on an inductive analysis of media reports of UNSC candidates, I have identified at least four such tools. First, states can win over other UN members by rhetorically supporting policy positions that are important to them. For example, the Czech Republic, vying for the 2008–09 Eastern European seat, also claimed by Croatia, attempted to win the votes of Morocco by shunning the cartoons of the prophet Mohammed (CTK Daily News, 2006). Second, states can mutually support candidatures for lucrative posts in international bodies.

Support for a UNSC bid may be reciprocated by later support for candidature in another international organization. Australia provides an example, which secured support for its 2013–14 bid from Rwanda in exchange for supporting Rwanda in its membership application to the Commonwealth (BBC Monitoring Africa, 2009). Third, states can curry favor by promising an expansion of bilateral trade and foreign direct investment. For instance, Japan committed to have its businesses invest USD 5.7 billion in infrastructure projects in Bangladesh, while winning support for its bid for a temporary seat on the UNSC (Reuters, 2014).

Fourth, states can use foreign aid to persuade other countries to vote for them in a UNGA ballot. Although aid-giving—in contrast to the previous tools—is limited to less developed countries, it is relatively more common. Descriptive analysis of media reports on UNSC campaigns over the past 20 years—available from Factiva—yields 15 news reports that explicitly mention pledges of aid in the context of UNSC candidatures—either generally to specific causes, or (more commonly) to specific countries. Ten pledges were from WEOG states, 4 from Asian-Pacific states, and one from the EEG. Obviously, this is merely a lower bound of the true extent of the phenomenon, given that some aid may be given privately in an expectation for votes in return. In contrast, the data entail only 12 new reports in which deeper economic cooperation was offered (explicitly)—six by WEOG donors, three by the APG, one from EEG, and two from GRULAC.

2.1. Can Aid Buy Support for UNSC Candidatures?

Donors can further their chances of winning a temporary seat by increasing foreign aid, especially to poorer countries, which depend on foreign aid for economic development and which may be willing to support a donor vying for a seat in return for aid—an important example of an "aid-for-policy deal" (Bueno de Mesquita & Smith, 2009).[2] Some donors are open about how aid supports their foreign policy goals, especially in contested races. For example, the foreign affairs minister of a small donor said:

> The main thing is: Do we get a return on that investment? Given what we spend on the Commonwealth, and campaigning for a seat on the Security Council, this is just one more opportunity to engage with leaders of other countries and convince them to support New Zealand.

The country paid almost USD 1 million to the Commonwealth secretariat in 2011–12 and provided technical assistance over USD 4 million to Commonwealth countries, while seeking support for its bid for temporary UNSC membership (Dominion Post, 2003).

Whereas more aid can increase the chances of winning, less aid can decrease them—an unpleasant lesson

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[2] These deals occur because developing countries are assumed to care more about material benefits than (most) policy issues given their low income.

learnt by Australia, Iceland, Spain, and Sweden. For instance, when Iceland was hit by the Global Financial Crisis in 2008, its chances for the 2009–10 spot for which it had vied were hurt according to Western diplomats (CNN, 2008). Concerns were also growing in Spain that the stagnating aid budget—a result of the Global Financial Crisis—would adversely affect the chances of winning a UNSC seat in 2015–16. In Sweden, where pressures on aid budgets mounted due to refugee costs, similar concerns were raised (Jacobsen, 2015). The above discussion implies the following hypothesis:

> Hypothesis 1: Donors will increase foreign aid to advance their campaigns for a temporary UNSC seat.

2.2. Choice of Aid Channels

Donors face a decision of which aid channel to use for advancing their UNSC campaign. The decision to alter the distribution of aid across delivery channels may be particularly important if the donor is unable to increase total aid. First, a donor can use *bilateral aid*, which is typically given to a recipient government. It is attractive to both sides as it furthers the foreign policy goal of the donor while being valuable to the recipient government, especially if provided as general budget support. In contrast, *multilateral aid* involves the pooling of resources among several donors and delegation to a multilateral aid agency that uses the funds at its own discretion. By diluting the influence of any individual donor and due to its long commitment cycles, multilateral aid is unlikely to be used for buying support for UNSC candidatures. Finally, *multi-bi aid*—donor contributions to multilateral organizations earmarked for specific development purposes—affords donors a third channel of aid delivery. It is a flexible mechanism whereby donors can support specific regions, countries, themes, or sectors through special funds, whose aid allocation decisions are not generally overseen by the formal governing bodies of the host multilaterals. For instance, Australia took advantage of this flexibility and provided multi-bi aid through a UN organization to garner support for its UNSC campaign in Ethiopia (Australian Associated Press, 2008).

Not all aid channels are equally attractive to all donors and under all circumstances given that donors vie for a temporary UNSC seat. Focusing on multi-bi aid, I derive three hypotheses on when this channel is particularly attractive. First and foremost, I expect donors with limited experience and thus capacity to stem short-term aid increases to be particularly likely to increase multi-bi aid to support their UNSC campaign. To these donors, multi-bi aid is the only way to direct funding that is sufficiently targeted and quickly disbursable. Alternative channels are not appropriate: While multilateral aid cannot be targeted, bilateral aid is not a feasible option due to capacity limitations that these donors face. An observable implication is that experienced donors are more likely to use bilateral aid in support of a UNSC campaign.

> Hypothesis 2: As donor experience increases, a donor will increase bilateral aid but decrease multi-bi aid in the context of a UNSC campaign.

Second, donors with relatively higher levels of domestic corruption are more likely to use multi-bi aid. A sizeable literature argues that donor characteristics not only matter for aid effectiveness (Bourguignon & Sundberg, 2007; Dreher & Kilby, 2010; Wright & Winters, 2010) but also affect aid channel choices (Dietrich, 2013; Dietrich & Wright, 2015; Reinsberg et al., 2017). Public opinion can constrain donor choices in that a more skeptical public forces donor governments to re-route larger shares of their aid budget through multilateral organizations which publics generally trust more to act as pro-development actors (Milner, 2006). Similarly, publics are easily alienated by aid fraud—whether in recipient countries (Bauhr, Charron, & Nasiritousi, 2013) or in their own government, which implies that donor governments with weak governance may need to channel aid multilaterally to reassure domestic audiences of aid being well spent. An observable implication is that donors with "good governance" are more likely to use bilateral aid to support their campaigns.

> Hypothesis 3: As governance quality increases, a donor will increase bilateral aid but decrease multi-bi aid in the context of a UNSC campaign.

Third, donor preferences for international cooperation matter. Some donors traditionally use aid more to advance narrow foreign policy goals (Bueno de Mesquita & Smith, 2009), while others conceive aid as a vehicle to build partnerships and to demonstrate international solidarity (Lumsdaine, 1993). Sweden is an example of a country that embraces the latter view. Having faced skyrocketing domestic expenses due to the refugee crisis, the Swedish government needed to consider significant cuts to the aid budget in recent years. The Swedish foreign ministry wrote that in such an event Sweden would damage its reputation and that it "would also have to give up its candidacy for a seat in the United Nations Security Council, as [its] high development aid contribution is seen as its most important trump card" (Jacobsen, 2015). Hence, specifically donors with an ideological commitment to aid will use multi-bi aid if prompted by the opportunity to win a UNSC seat to increase overall aid. An observable implication is that less outward-oriented donors should prefer bilateral aid to advance their campaigns.

> Hypothesis 4: As a donor becomes more internationalized, it will increase multi-bi aid but decrease bilateral aid in the context of a UNSC campaign.

In the empirical analysis, I also consider heterogeneous effects with respect to constituency. As sufficient time-series information on aid flows is available only for states

that report to the OECD, specifically its DAC, I consider three constituencies—WEOG, EEG, and APG.

3. Data and Methods

I construct a panel dataset of initially 40 donors over the 1995–2016 period in the Northern hemisphere—23 WEOG donors, 12 EEG donors, and the two APG donors, Japan and Korea. Due to missing data, the panel is unbalanced. For three permanent UNSC members (France, United Kingdom, and the United States), the posited mechanism does not apply and I thus exclude these donors from the sample. The results are unchanged when including them nonetheless, as shown in a robustness test.[3] I chose the sample period to allow for sufficient time after the dissolution of the Soviet Union and in view of concerns over data quality as some donors only then joined the OECD/DAC and began reporting their aid flows.

3.1. Dependent Variables

I disaggregate total aid into its three main channels— bilateral aid, multilateral aid, and multi-bi aid. I rely primarily on OECD/DAC data for that purpose (OECD, 2018). For multi-bi aid, OECD/DAC data only cover the 2006–16 period; to extend the time series to prior sample years, I rely on the multi-bi aid data (Eichenauer & Reinsberg, 2017). The multi-bi aid data are available for 23 OECD/DAC donors and are based on the Creditor Reporting System, which ensures that both data sources are consistent.[4] For each aid flow, I apply a logarithmic transformation to mitigate outliers.[5] Using aid changes (rather than levels) helps de-trending the time series. This is also suggested by diagnostic tests.[6] Furthermore, aid changes correspond closely to my theoretical interest in short-term effects. Following the aid allocation literature, I use aid commitments in constant USD in the main analysis (e.g., Dietrich & Wright, 2015; Dreher, Langlotz, & Marchesi, 2017; Hoeffler & Outram, 2011), while using disbursements for robustness tests. Finally, in accordance with OECD/DAC definitions, aid flows do not include military aid, although military assistance was given once to support a UNSC campaign.[7]

3.2. Predictors

The key predictor of this study is the campaign activity for a temporary UNSC seat of a given donor in a given year. As no such indicator—to the best of my knowledge—was

available, I sourced newspaper articles covering UNSC candidacies during 1994–2016 from Factiva. Appendix B explains the selection of media reports and the coding procedure in detail. The typical news article in my database is a statement of support by one country for the UNSC candidature of another country. I discarded duplicates (covering the same event) and campaigns for permanent memberships and UNSC reform. I then aggregated the data to the country-year format.

My main predictor—Campaign—is a binary indicator of whether there is at least one media report in which a donor is reported to bid for a temporary UNSC seat in a given year. The measure is not ideal, given that some donors may lobby non-publicly for seats and data sources are biased toward English-speaking outlets, but it successfully addresses the challenge that both the willingness to run for a seat and the timing of a campaign are otherwise unobservable. I also verify that donors release statements in close temporal proximity to their aspired tenure (Figure B1), with the mode of that distribution being one year prior.

To examine effect heterogeneity, I include multiplicative interaction terms between campaign activity and donor characteristics. To capture Donor Experience, I compute the natural logarithm of the difference $(t + 1) - f$, where f is the founding year of the (first) bilateral aid agency of a donor, and t is the current year. For all sample years prior to foundation, I set its value to zero. For robustness tests, I use an alternative data source of the aid agency founding year (Fuchs & Müller, 2018, p. 41). To measure donor governance, I use the ICRG Index of quality of government (The PRS Group, 2015), while using the VDem Corruption Index (Coppedge et al., 2016) for robustness. To measure preferences for cooperation, I use the Political Globalization index of the KOF institute (Dreher, 2006), which includes information on the number of embassies globally, active (I)NGOs, membership in international organizations, participation in peacekeeping missions, and treaties signed. In further analyses, I consider interactions of campaigns with binary indicators of three regional UN constituencies.

3.3. Control Variables

To mitigate the impact of potential confounders, I include several time-varying control variables. Without a mainstay model of donor-year aid growth at hand, I choose control variables with a view to maximize observations and thus the power of my statistical tests. First, macroeconomic shocks may affect the opportu-

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[3] Excluding the P5 is consistent with the 'possibility principle' (Mahoney & Goertz, 2004), because the purported mechanism is not applicable for these donors.

[4] Eichenauer and Reinsberg (2017) show that both datasets yield virtually identical aggregate multi-bi aid flows in 2006–12 and that the OECD/DAC data severely underreport multi-bi aid in earlier years. Any remaining discrepancies in aid levels are mitigated due to differencing.

[5] Specifically, $\ln(\Delta y)$ if $\Delta y \geq 0$ and $-\ln(-\Delta y)$ if $\Delta y < 0$.

[6] Specifically, I conduct a Dickey-Fuller tests for stationarity for all transformed aid variables, rejecting the null hypothesis of unit roots in all panels (p < 1E-4). A Wooldridge test indicates mild autocorrelation (p < 0.05), which can be remedied by including lagged levels of the dependent variable.

[7] Based on publicly available media reports, Australia announced military assistance over approximately USD 4 million to the Philippines in 2009–10 (BusinessWorld, 2011).

nity cost of aid provision *and* the willingness to campaign for a UNSC seat. I therefore include a binary indicator of financial crisis in the donor country (Laeven & Valencia, 2013), as well as the percentage rate of GDP Growth. Second, domestic politics may affect aid policies and UNSC campaign decisions. For example, the Australian opposition leader Tony Abbott (Liberal party) challenged Kevin Rudd (Labor party) for his decision to run for a UNSC seat. Skeptical of foreign aid increases in general, Abbott promised to abandon the UNSC campaign and cut aid if elected prime minister (The Age, 2010). In other cases, governments may have avoided pursuing certain foreign policies as those policies were controversial among coalition parties. I therefore include binary indicators for Executive Elections, Left-wing government ideology, as well as a continuous measure of Government Fractionalization—all available from the Database of Political Institutions (Beck, Clarke, Groff, Keefer, & Walsh, 2001).

All regressions further include country-fixed effects (accounting for unobserved heterogeneity) and year-fixed effects (accounting for common shocks). To control for donor size and mean reversion—the tendency of (stationary) variables to fluctuate around their average—I include the log-transformed lagged value of the respective aid flow.

In robustness tests, I consider additional controls relating to events in the neighborhood of a donor country—financial crises, armed conflicts, and natural disasters—which may prompt donors to reconsider aid policies and UNSC campaign decisions. For example, a donor may respond to emergent conflicts by providing more aid to address emergent needs while at the same time vying for more influence in the UNSC to help resolve these conflicts. I count the respective number of these types of events in all contiguous countries and neighboring countries no farther away than 1,500 kilometers and normalize by the total number of countries in the neighborhood to eliminate scaling effects.

Table 1 presents descriptive statistics and data sources for all variables. It shows that (non-P5) donors campaign for a temporary UNSC seat in around 6.4% of all years and 7.0% when excluding years of tenure and the two years following it. In addition, campaigning is widespread. The media reports identify 24 donors with at least one campaign in 1995–2016.[8] This represents 65% of all eligible donors. This ensures that results are unlikely to be driven by individual countries with frequent campaigns.

3.4. Methods

To adequately capture interdependencies across aid channels, I conduct seemingly unrelated regression (SUR) analysis. The SUR model controls for factors jointly affecting all aid channels by allowing for cross-equation

correlations that appear as parameters in the cross-equation variance-covariance matrix (Roodman, 2012). The model is especially suitable under the assumption of a fixed aid budget, implying that donors which want to boost one type of aid must decrease another type of aid to not overspend the budget. Even if aid budgets are freely determined, SUR estimates are consistent (albeit not necessarily efficient). Finally, I cluster standard errors by country, thereby mitigating temporal auto-correlation.

4. Results

4.1. Correlational Analysis

I examine the unconditional relationship between UNSC campaigning and aid growth graphically. The full regression results are available in the supplemental appendix but I do not show them here as I do not find a statistically significant unconditional relationship for various sets of control variables and measures of campaigning.

Figure 1 shows for all donors vying for a temporary UNSC seat the evolution of their total aid in a time window around the respective UNGA election. Each point shows for a given campaign for each year relative to the UNGA election (x-axis) the logged absolute amount of aid dollars by which a donor has increased (or decreased) aid with respect to the previous year (y-axis). The solid line is the average aid change across all campaigning donors, along with the 95%-CI. I find no statistically significant change in aid growth, as the confidence intervals include zero.

Figure 2 tracks aid growth around UNGA elections separately for different aid channels. Multi-bi aid growth accelerates in the year before the UNGA election, but this finding does not hold in multivariate analysis, as shown in the appendix. For the remaining aid channels, there is no statistically significant growth effect in the context of UNSC campaigning. A possible interpretation is that donors are heterogeneous in the ways they use the various aid channels to support their UNSC candidature, which I will explore now.

Table 2 presents estimation results for the relationship between campaigning for a temporary UNSC seat and aid growth under different donor characteristics. I present results for all three estimating equations as a stacked vector, using a baseline set of control variables and referring to the supplemental appendix for robustness to other controls.

In Model 1, I find a marginally significant effect in the difference of campaign effects between experienced donors and inexperienced donors. For multi-bi aid, I find the effect to be in the expected direction, but not statistically significant. For bilateral aid, everything else equal, 25 years of donor experience are related to an increase in bilateral aid growth by about USD 200,000—compared

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[8] These donors include: Australia, Azerbaijan, Bulgaria, Canada, Croatia, Czech Republic, Denmark, Finland, Greece, Iceland, Israel, Ireland, Italy, Japan, Korea, Lithuania, Norway, New Zealand, Poland, Romania, Slovak Republic, Slovenia, Spain, and Turkey.

Table 1. Descriptive statistics and variable definitions.

	Observations	Mean	Sd	Min	Max	Description
Dependent variables						
Bilateral aid growth	567	2.24	18.35	−22.08	22.59	$\ln(B_{it} - B_{i,t-1} + e)$ if $B_{it} - B_{i,t-1} \geq 0$, $-\ln(-[B_{it} - B_{i,t-1}])$ otherwise; B_{it} refers to bilateral ODA net of multi-bi aid, sourced from Table DAC1 (OECD, 2018); e is an offset to ensure a non-missing value if growth is zero
Multilateral aid growth	567	2.31	17.81	−21.57	21.58	$\ln(M_{it} - M_{i,t-1} + e)$ if $M_{it} - M_{i,t-1} \geq 0$, $-\ln(-[M_{it} - M_{i,t-1}])$ otherwise; M_{it} refers to multilateral ODA, sourced from Table DAC1 (OECD, 2018)
Multi-bi aid growth	420	2.65	17.04	−22.13	22.17	$\ln(MB_{it} - MB_{i,t-1} + e)$ if $MB_{it} - MB_{i,t-1} \geq 0$, $-\ln(-[MB_{it} - MB_{i,t-1}])$ otherwise; MB_{it} refers to multi-bi aid, sourced from Table DAC1 for 2006–16 (OECD, 2018) and from the multi-bi aid data for 1990–12 (Eichenauer & Reinsberg, 2017)
Key predictor						
Campaign	999	0.06	0.24	0.00	1.00	Binary indicator for Media Reports > 0 (the procedure for identifying media reports on temporary UNSC seat campaigns is laid out in Appendix B), coded from Factiva
Control variables						
Financial crisis	945	0.05	0.22	0.00	1.00	Binary indicator for (contemporaneous) incidence of financial crisis (Laeven & Valencia, 2013)
GDP growth (%)	928	2.64	4.22	−23.10	34.50	Real (contemporaneous) annual GDP growth from World Economic Outlook, sourced from QoG database (Teorell et al., 2018)
Executive election	944	0.07	0.26	0.00	1.00	Binary indicator for (contemporaneous) executive election from the Database of Political Institutions, sourced from QoG database (Teorell et al., 2018)
Left-wing government	999	0.29	0.46	0.00	1.00	Binary indicator for left-wing government (largest governing party or the state executive—depending on the political system), from the Database of Political Institutions, sourced from QoG database (Teorell et al., 2018)
Government fractionalization	899	0.35	0.27	0.00	0.83	Government fractionalization is defined as the probability that any two legislators are not from the same party, from the Database of Political Institutions, sourced from QoG database (Teorell et al., 2018)
Regional financial crises	918	0.05	0.11	0.00	1.00	Number of financial crises (Laeven & Valencia, 2013) in the neighborhood of a donor country, divided by the number of countries in the neighborhood. A neighborhood comprises all contiguous countries and all states within 1,500 kilometers. Distance data from the CEPII dataset (Mayer & Zignago, 2011)
Regional armed conflicts	918	0.15	0.13	0.00	1.00	Number of armed conflicts based on UCDP/PRIO definition, available from the QoG database (Teorell et al., 2018), divided by neighborhood size as above
Regional natural disasters	918	0.98	2.03	0.00	15.44	Natural logarithm of people affected by natural disasters (including offset) in the neighborhood divided by neighborhood size (EM-DAT, 2018)
Moderators						
Agency age	837	1.98	1.46	0.00	4.01	Natural logarithm of number of years of existence of the aid agency (formally, 2016-*f*, with *f* the founding year), based on own search
ICRG index	912	0.79	0.17	0.36	1.00	ICRG index of quality of government from the PRS Group, sourced from QoG database (Teorell et al., 2018)
Political globalization	897	77.68	19.67	15.04	98.41	Sub-index on political globalization from the KOF index, sourced from QoG database (Teorell et al., 2018)
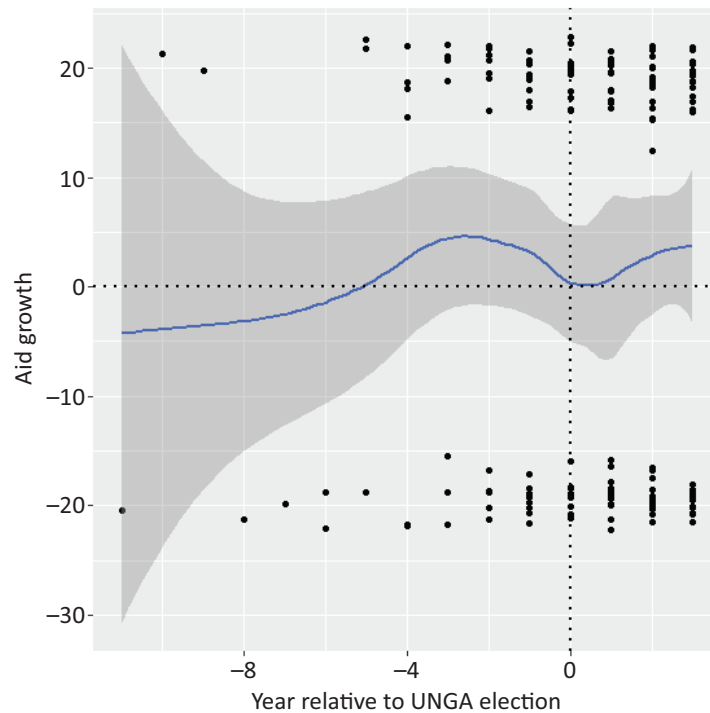
Figure 1. Absolute aid growth in the context of UNSC campaigns. Note: UNSC campaign start year is endogenously determined by the first media report, whereas the figure tracks aid growth up to three years after the UNGA election year.
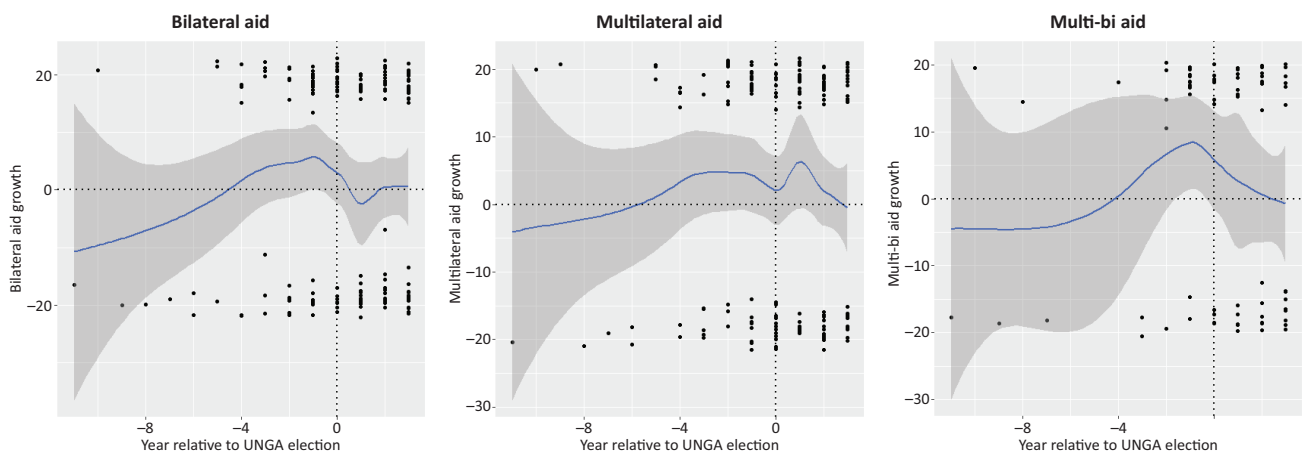
Figure 2. Absolute aid growth in the context of UNSC campaigns for different aid channels. Note: UNSC campaign start year is endogenously determined by the first media report, whereas the figure tracks aid growth up to three years after the UNGA election year.

to a fully inexperienced donor—in the context of a UNSC campaign (p < 0.1).[9] Figure 3 plots the marginal effect of campaigning on bilateral aid growth conditional on logged years of donor experience. Consistent with theoretical expectations, the marginal effect of campaigning on bilateral aid growth increases with donor experience and becomes positively significant only for the most experienced donor.

Model 2 explores the moderating impact of donor governance. Specifically, decreases in governance quality let donors increase their multi-bi aid growth to support a UNSC campaign. Above the mean of governance

quality, the effect of UNSC campaigning on multi-bi aid growth is not statistically significant, but the effect increases and becomes positively significant at lower governance values. For example, multi-bi aid growth increases by about USD 15,000 when reducing governance quality by one standard deviation ($\sigma = 0.17$). Figure 4 shows the conditional marginal effect across the entire range of governance quality.

In Model 3, I find that political globalization is significantly related to an acceleration of campaign-induced multi-bi aid growth. As the globalization index is never zero for any donor, the effect is best illustrated in an in-

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[9] Multiply ln(25) with $\beta = 3.807$ and exponentiate the result.

Table 2. UNSC campaign and aid growth with basic control variables.

	Donor experience (1)		Governance quality (2)		Political globalization (3)	
Bilateral aid growth						
Campaign	−8.093	(6.285)	−2.378	(18.218)	−0.336	(41.956)
Campaign × X	3.807*	(2.208)	5.376	(21.474)	0.019	(0.488)
X	0.277	(1.950)	11.556	(19.876)	0.110	(0.294)
Financial crisis	−10.045**	(4.637)	−10.387**	(4.643)	−9.436**	(4.631)
GDP growth	0.222	(0.439)	0.190	(0.444)	0.570	(0.527)
Executive election	−3.884	(3.574)	−4.477	(3.487)	−3.958	(3.580)
Left-wing government	0.583	(1.897)	0.717	(1.849)	1.001	(1.890)
Government fractionalization	−4.141	(5.357)	−4.306	(5.332)	−3.059	(5.671)
Lagged aid level	−15.816***	(1.734)	−16.053***	(1.717)	−17.707***	(1.997)
Multilateral aid growth						
Campaign	−8.815	(8.696)	4.789	(23.410)	−65.591*	(37.205)
Campaign × X	2.676	(2.860)	−6.941	(26.743)	0.749*	(0.433)
X	2.677	(1.997)	−17.106	(22.132)	0.115	(0.289)
Financial crisis	2.782	(5.385)	3.113	(5.292)	2.995	(5.389)
GDP growth	0.584**	(0.279)	0.567**	(0.279)	0.622*	(0.324)
Executive election	−3.891	(3.810)	−4.256	(3.741)	−4.232	(3.670)
Left-wing government	1.548	(1.781)	2.258	(1.766)	2.309	(1.804)
Government fractionalization	1.798	(5.350)	1.779	(5.483)	1.670	(5.872)
Lagged aid level	−13.512***	(2.007)	−13.242***	(1.941)	−13.719***	(2.037)
Multi-bi aid growth						
Campaign	1.316	(9.406)	51.894***	(18.888)	−141.226***	(53.114)
Campaign × X	0.483	(3.059)	−56.127**	(22.490)	1.590***	(0.603)
X	−3.518*	(2.084)	−19.821	(24.171)	0.055	(0.440)
Financial crisis	−3.709	(3.283)	−3.496	(3.367)	−1.940	(3.727)
GDP growth	0.510	(0.404)	0.495	(0.408)	0.241	(0.460)
Executive election	6.248	(4.079)	6.135	(3.868)	6.420	(4.071)
Left-wing government	1.373	(1.961)	0.759	(1.917)	1.244	(1.960)
Government fractionalization	4.667	(7.994)	2.978	(7.995)	7.451	(8.316)
Lagged aid level	−4.481***	(0.746)	−4.251***	(0.716)	−4.099***	(0.719)
Country-fixed effects	yes		yes		yes	
Year-fixed effects	yes		yes		yes	
N_B/R_B^2	481	0.25	481	0.24	452	0.25
N_M/R_M^2	481	0.23	481	0.23	452	0.22
N_{MB}/R_{MB}^2	383	0.24	383	0.24	354	0.25
$\rho_{B,M}$	0.028	(0.046)	0.032	(0.046)	0.027	(0.048)
$\rho_{B,MB}$	0.004	(0.053)	0.005	(0.053)	−0.024	(0.054)
$\rho_{M,MB}$	0.067	(0.054)	0.068	(0.054)	0.062	(0.056)

Notes: System of equations estimated simultaneously with dependent variables shown in row heads. Campaign is a binary indicator, X is the moderator in the respective column header, and Campaign × X the multiplicative interaction term. Abbreviations in the diagnostics section refer to the number of observations across bilateral aid (N_B), multilateral aid (N_M), and multi-bi aid (N_{MB}), R-squared, and cross-equation correlations, respectively. Significance levels: * $p < .1$ ** $p < .05$ *** $p < .01$.

teraction plot. Figure 5 shows that the campaign effect is positively significant only for the most globalized donors, insignificant for the average donor, and negatively significant for inward-oriented donors. Substantively, if political globalization is increased by one standard deviation ($\sigma = 9.02$), multi-bi aid growth accelerates by more than USD 1.7 million ($p < 0.01$), while multilateral aid increases by a meager USD 867 ($p < 0.1$) in the context of a UNSC campaign.

While the effects of most control variables remain statistically insignificant, they point into the expected direction. Among the statistically significant controls, I find that financial crises negatively affect bilateral aid growth ($p < 0.05$) and that more experienced donors tend to mobilize larger increases of bilateral aid ($p < 0.1$). For multilateral aid, I find a significantly positive relationship between GDP growth and multilateral aid growth ($p < 0.1$). For multi-bi aid, none of the controls has a significant ef-

Thick dashed lines give 90% confidence interval.
Thin dashed line is a kernel density estimate of donor experience.
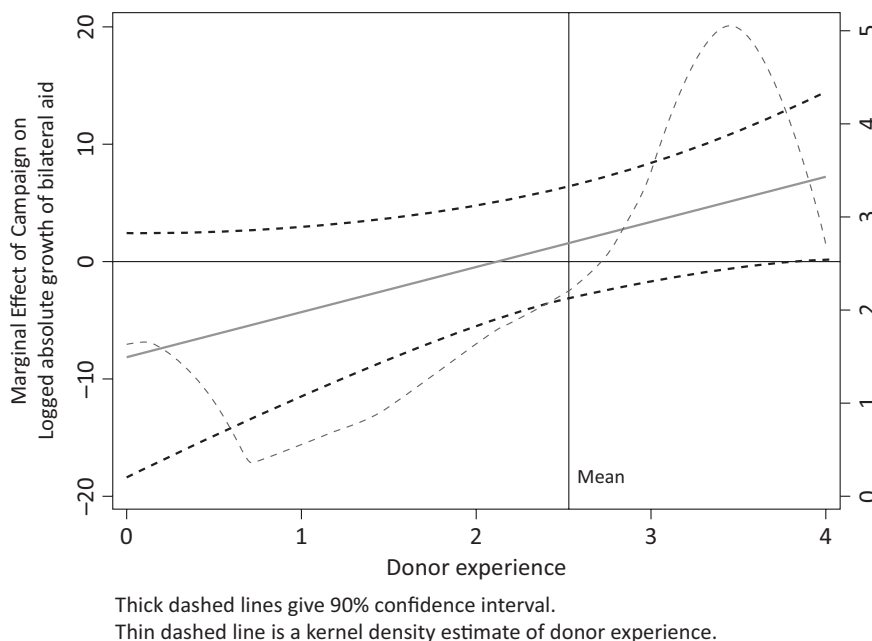
Figure 3. Marginal effect of campaigns on multi-bi aid growth conditional on donor experience.

Thick dashed lines give 90% confidence interval.
Thin dashed line is a kernel density estimate of government quality.
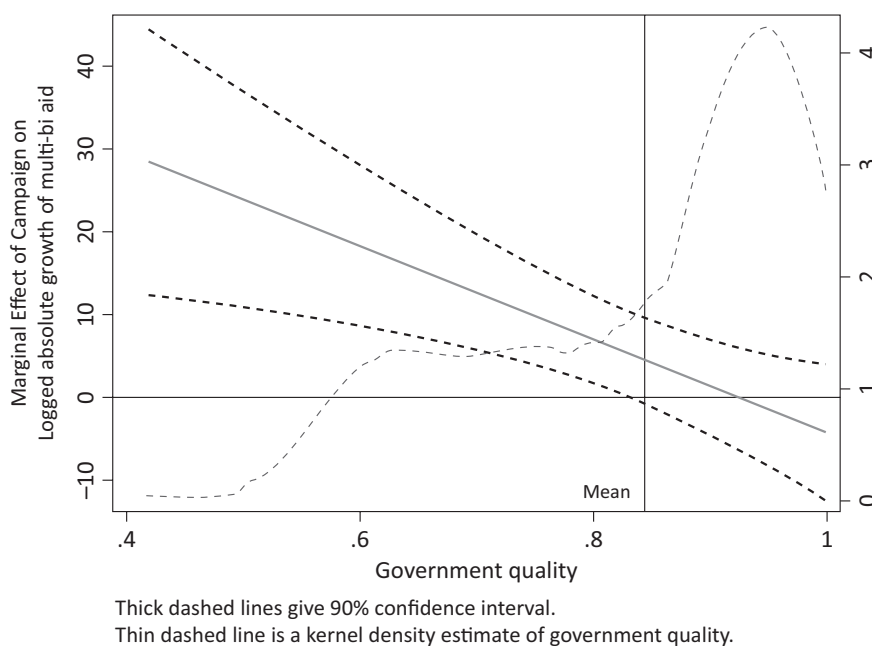
Figure 4. Marginal effect of campaigns on multi-bi aid growth conditional on governance quality.

fect. In all equations, lagged aid levels have a statistically significant effect, indicating mean-reverting time series ($p < 0.01$). Overall, model specifications are plausible as the models explain more than 20% of the variation in all aid types. Thanks to joint estimation, I can assess the extent to which aid growth across different channels is correlated. The cross-equation correlations are insignificant in most regressions.

4.2. Instrumental-Variable Design

The results thus far are based on correlations. In the remainder, I consider potential endogeneity of campaign-

ing with respect to aid growth. For example, donor preferences for aid provision and running for a seat may be affected simultaneously by unobserved factors. Therefore, I introduce an instrumental variable—the number of donors in the same constituency that are reported to vie for the same temporary UNSC seat as a given donor. This instrument fulfills the relevance criterion because a donor will need to do more campaigning if many other donors vie for the same spot. In addition, to the extent that peer campaigning affects donor-specific aid growth only through donor campaigning, the instrument fulfills excludability, conditional on control variables and fixed effects.
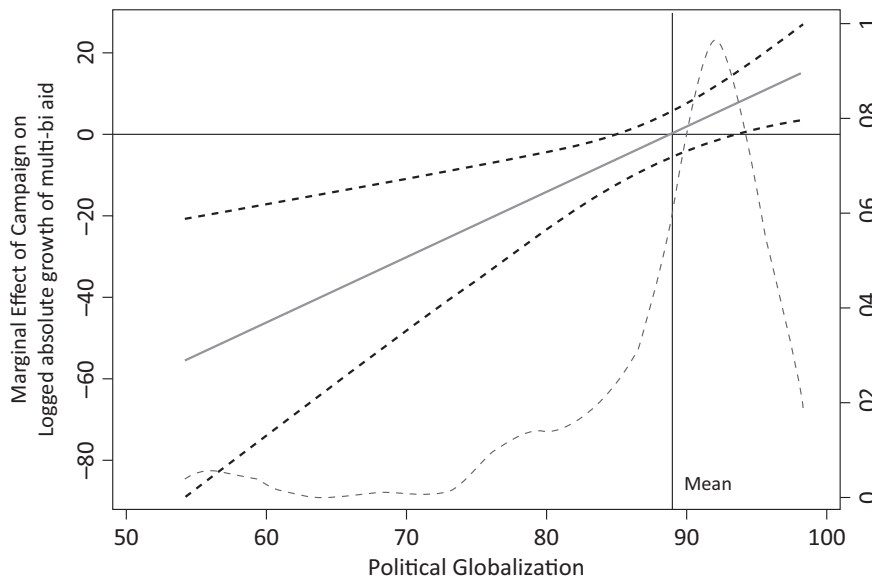
Thick dashed lines give 90% confidence interval.
Thin dashed line is a kernel density estimate of political globalization.

Figure 5. Marginal effect of campaigns on multi-bi aid growth conditional on political globalization.

Table 3 presents the results, indicating that most previous findings hold, some being more significant. Donor experience is a significantly positive predictor of bilateral aid growth in the context of UNSC campaigns. 25 years of additional experience now are related to bilateral aid growth of around USD 50 million ($p < 0.05$). To the extent that a donor becomes more internationalized by a standard deviation, it accelerates its multilateral aid growth by USD 1,100 ($p < 0.1$) and its multi-bi aid growth by about USD 140,000 ($p < 0.1$) during UNSC campaigns. Finally, campaigning significantly increases multi-bi aid growth if governance quality decreases ($p < 0.05$). As regards instrument strength, I find that peer campaigning is a positively significant predictor of individual campaigning, with the Kleibergen-Paap F-statistics being well above the conventional threshold ($F > 10$). The estimated sign implies that competition for a single UNSC seat drives donors toward intensifying their own campaigning.

4.3. Robustness Checks

In the appendix, I present the full results for the unconditional relationship between UNSC campaigning and aid growth for total aid (Table A1) and different aid channels (Table A2), before probing the robustness of my findings in several ways. First, I extend the set of controls to also include 'neighborhood variables,' which yields the additional finding that multi-bi aid growth is significantly positively related to the incidence of regional conflicts, but has otherwise no effect on the results (Table A3). My results also are qualitatively unchanged when dropping all control variables from the model, except for the

country-fixed effects and year dummies (Table A4). Furthermore, I successfully replicate all findings using alternative measures of the moderator variables (Table A5). In another robustness test, I explore an alternative lag structure. When using the one-year lagged campaign measure, I find that less experienced donors significantly increase multi-bi aid growth to support their UNSC campaigns ($p < 0.05$). Conversely, political globalization loses its significant conditioning effect on campaign-induced multi-bi aid, but now significantly accelerates campaign-induced multilateral aid ($p < 0.05$). The interaction effect of UNSC campaigning and governance quality is robust (Table A6). In another robustness test, I add the three P5 donors to the sample. This does not affect the results (Table A7). Except for donor experience, all results are significant when using aid disbursements rather than aid commitments (Table A8).[10] The relative consistency of the results implies that donors generally keep their promises to reward support for their UNSC bids with additional aid.

Yet another robustness test conducts all regressions without country-fixed effects. This alters the interpretation of the findings because estimated effects also refer to differences across donors, rather than inter-temporal differences within the same donors. For slow-moving variables, effects generally refer to cross-donor differences. Replicating the main analysis using pooled regressions, I find the main results to be remarkably robust (Table A9).

Finally, again using pooled regressions, I explore potential effect heterogeneity across three regional constituencies, notably EEG, WEOG, and APG (Table A10). I find that Asian donors increase bilateral aid to support

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[10] When lagging the campaign indicator, I obtain a significant conditional effect of donor experience on multi-bi aid growth, which may be due to delays in the actual disbursement of committed aid.

Table 3. Instrumental variable analysis.

	Donor experience (1)		Governance quality (2)		Political globalization (3)	
Bilateral aid growth						
Campaign	−18.170**	(7.799)	−19.289	(21.333)	5.941	(42.521)
Campaign × X	5.513**	(2.363)	19.773	(23.675)	−0.104	(0.498)
X	0.621	(1.945)	12.919	(19.828)	0.162	(0.296)
Multilateral aid growth						
Campaign	−10.929	(9.495)	5.847	(24.303)	−67.166*	(36.670)
Campaign × X	3.036	(2.982)	−7.839	(27.489)	0.780*	(0.426)
X	2.753	(2.008)	−17.192	(22.106)	0.103	(0.288)
Multi-bi aid growth						
Campaign	−4.192	(9.386)	44.190*	(23.090)	−118.837**	(57.898)
Campaign × X	1.008	(2.749)	−50.911**	(25.581)	1.313*	(0.675)
X	−3.534*	(2.068)	−17.401	(24.245)	0.070	(0.440)
Campaign						
Peer campaigning	0.555***	(0.042)	0.543***	(0.038)	0.540***	(0.041)
Country-fixed effects	yes		yes		yes	
Year-fixed effects	yes		yes		yes	
Control variables	yes		yes		yes	
N_B/R^2_B	481	0.25	481	0.24	452	0.25
N_M/R^2_M	481	0.24	481	0.23	452	0.22
N_{MB}/R^2_{MB}	383	0.24	383	0.24	354	0.25
$\rho_{B,M}$	0.031	(0.046)	0.032	(0.046)	0.026	(0.048)
$\rho_{B,MB}$	0.009	(0.053)	0.008	(0.053)	−0.210	(0.054)
$\rho_{M,MB}$	0.069	(0.054)	0.067	(0.054)	0.060	(0.056)
F-statistic	177.22		201.62		170.98	

Notes: System of equations estimated simultaneously with dependent variables shown in row heads. Campaign is a binary indicator, X is the moderator in the respective column header, and Campaign × X the multiplicative interaction term. Baseline set of controls included in all equations. The instrument—peer campaigning—is defined as the number of other donors in the same constituency vying for the same seat. Abbreviations in the diagnostics section refer to the number of observations across bilateral aid (N_B), multilateral aid (N_M), and multi-bi aid (N_{MB}), R-squared, and cross-equation correlations, respectively. Significance levels: * $p < .1$ ** $p < .05$ *** $p < .01$.

a UNSC campaign ($p < 0.05$), while WEOG donors tend to reduce it ($p < 0.1$). I find no significant conditional effects for EEG donors, which likely is the result of underpowered tests.

5. Conclusion

Do countries use foreign aid to support their campaigns for a temporary seat in the UN Security Council? I found some robust evidence of campaign-induced aid growth for some aid channels and under some circumstances. In particular, as donor experience increases, bilateral aid growth increases, whereas multi-bi aid growth tends to decline. Conversely, multi-bi aid growth increases with declining quality of governance and with the level of internationalization of a given donor.

A noteworthy limitation of this research is that some findings can still be interpreted in various ways. For example, while internationalization boosts multi-bi aid growth, it does not seem to affect bilateral aid growth. One interpretation is that there really is no effect on bilateral aid growth, but another one is that bilateral aid declines

but the analysis fails to produce a statistically significant effect, for example due to measurement error. Despite best efforts, the data collection might be incomplete, which would generate measurement error. Other sources of insignificant effects include under-powered tests due to short time series as well as donor heterogeneity. These are common challenges of quantitative analysis that also this article cannot defy. Nonetheless, the above analysis produced results that are remarkably robust against several modifications, including instrumental-variable estimation.

Albeit necessarily preliminary, these findings suggest interesting avenues for future research. They demonstrate that donors care about UNSC membership. Existing studies emphasize the aid rewards for recipient countries during their temporary UNSC tenure. Future research could examine how aid flows evolve over the lifetime of a UNSC campaign, given that donors (including the ones that do not get elected) are likely to lessen aid effort once uncertainty is resolved after the outcome of a UNGA election. Studying how aid evolves during UNSC tenure would also hint to donor motiva-

tions in this context and specifically which donors use aid for private gain versus fostering international solidarity. In this context, further research into donors of specific UN constituencies would be promising. For EEG donors, qualitative research would be necessary given the lack of long time-series data (Szent-Iványi, Reinsberg, & Lightfoot, 2018). Capturing the donor perspective, future research could also examine the choice of instruments other than aid for supporting UNSC campaigns. Research could also explore which recipients stand to gain most from donor campaigns for UNSC seats. Ultimately, researchers should examine the effectiveness of campaign-induced aid. Although previous research finds that aid extended to developing countries during their temporary UNSC tenure is less effective (Dreher, Eichenauer, & Gehring, 2016), this does not need to be the case for campaign-induced aid for which donor motives are less clear.

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Conflict of Interests

The author declares no conflict of interests.

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Appendix A. Robustness tests.

Table A1. Unconditional effect of UNSC campaigning on total aid growth.

	Media reports > 0			ln (1 + Media reports)			Media reports $_{t-1}$ > 0		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(Column header)	**0.159**	**0.337**	**−0.110**	**1.420**	**1.641**	**−0.971**	**1.517**	**1.666**	**1.396**
	(3.656)	**(3.622)**	**(2.929)**	**(2.948)**	**(2.812)**	**(2.359)**	**(2.264)**	**(2.284)**	**(2.839)**
Financial crisis		1.609	2.151		1.709	2.091		1.633	2.193
		(3.276)	(3.261)		(3.324)	(3.224)		(3.309)	(3.308)
GDP growth		−0.047	−0.021		−0.052	−0.022		−0.048	−0.019
		(0.157)	(0.168)		(0.157)	(0.169)		(0.158)	(0.166)
Executive election		0.033	−0.009		0.084	−0.035		−0.053	−0.082
		(2.780)	(2.747)		(2.800)	(2.734)		(2.765)	(2.758)
Left-wing government		2.261	2.084		2.288	2.033		2.276	2.149
		(1.550)	(1.736)		(1.524)	(1.739)		(1.513)	(1.680)
Government fractionalization		0.861	1.757		1.165	1.671		1.119	1.908
		(3.916)	(3.978)		(3.869)	(3.955)		(3.812)	(3.913)
Regional crises			−1.500			−1.713			−1.070
			(11.647)			(11.755)			(12.118)
Regional conflicts			2.572			2.606			2.633
			(3.996)			(3.929)			(3.908)
Regional disasters			0.304			0.263			0.355
			(0.526)			(0.519)			(0.630)
Lagged aid level	0.472***	0.472***	0.467***	0.474***	0.474***	0.465***	0.474***	0.474***	0.469***
	(0.070)	(0.071)	(0.070)	(0.069)	(0.071)	(0.070)	(0.070)	(0.071)	(0.070)
Country-fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
R-squared	0.48	0.46	0.47	0.48	0.46	0.47	0.48	0.46	0.47
Observations	814	753	709	814	753	709	814	753	709

Notes: Campaign is either a binary indicator of media reports (column 1–3), the logged number of media reports (column 4–6), or the lagged binary indicator (column 7–9). Significance level: *** $p < .01$.

Table A2. Unconditional effect of UNSC campaigning on aid growth for different aid channels.

	Media reports > 0 (1)		ln (1 + Media reports) (2)		Media reports $_{t-1}$ > 0 (3)	
Bilateral aid growth						
(Column header)	**2.289**	**(2.943)**	**2.440**	**(2.363)**	**0.441**	**(2.978)**
Financial crisis	−10.242**	(4.611)	−10.139**	(4.591)	−10.355**	(4.707)
GDP growth	0.208	(0.440)	0.194	(0.439)	0.225	(0.449)
Executive election	−4.459	(3.465)	−4.351	(3.467)	−4.259	(3.469)
Left-wing government	0.782	(1.856)	0.767	(1.851)	0.714	(1.853)
Government fractionalization	−4.331	(5.397)	−4.108	(5.387)	−4.482	(5.503)
Lagged aid level	−15.994***	(1.729)	−16.002***	(1.730)	−16.001***	(1.722)
Multilateral aid growth						
(Column header)	**−1.256**	**(3.060)**	**−0.844**	**(2.555)**	**−2.712**	**(2.832)**
Financial crisis	2.898	(5.337)	2.863	(5.315)	3.524	(5.414)
GDP growth	0.541*	(0.279)	0.541*	(0.281)	0.568**	(0.280)
Executive election	−4.290	(3.794)	−4.360	(3.787)	−4.566	(3.767)
Left-wing government	2.164	(1.757)	2.187	(1.756)	2.106	(1.763)
Government fractionalization	1.826	(5.457)	1.795	(5.489)	1.302	(5.454)
Lagged aid level	−13.306***	(1.935)	−13.317***	(1.933)	−13.450***	(1.949)
Multi-bi aid growth						
(Column header)	**2.550**	**(3.431)**	**1.190**	**(2.291)**	**0.709**	**(3.582)**
Financial crisis	−3.905	(3.380)	−3.672	(3.373)	−3.815	(3.381)
GDP growth	0.505	(0.405)	0.502	(0.407)	0.514	(0.405)
Executive election	6.111	(3.971)	6.285	(3.932)	6.395	(3.921)
Left-wing government	0.842	(1.928)	0.736	(1.916)	0.698	(1.912)
Government fractionalization	3.755	(8.006)	3.481	(8.025)	3.126	(7.968)
Lagged aid level	−4.268***	(0.716)	−4.286***	(0.714)	−4.320***	(0.707)
Country-fixed effects	yes	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes	yes
N_B/R^2_B	481	0.24	481	0.24	481	0.24
N_M/R^2_M	481	0.23	481	0.23	481	0.23
N_{MB}/R^2_{MB}	383	0.23	383	0.23	383	0.23
$\rho_{B,M}$	0.031	(0.046)	0.031	(0.046)	0.031	(0.046)
$\rho_{B,MB}$	0.002	(0.053)	0.001	(0.053)	0.003	(0.053)
$\rho_{M,MB}$	0.068	(0.054)	0.067	(0.054)	0.066	(0.054)

Notes: System of equations estimated simultaneously with dependent variables shown in row heads. Campaign is either a binary indicator of media reports (column 1), the logged number of media reports (column 2), or the lagged binary indicator (column 3). Significance levels: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table A3. UNSC campaign and aid growth with additional control variables.

	Donor experience (1)		Government quality (2)		Political globalization (3)	
Bilateral aid growth						
Campaign	**−8.158**	**(6.300)**	**−1.672**	**(18.175)**	**−3.468**	**(40.753)**
Campaign × X	**3.858***	**(2.204)**	**4.556**	**(21.407)**	**0.056**	**(0.473)**
X	**0.318**	**(1.954)**	**11.869**	**(19.847)**	**0.103**	**(0.294)**
Financial crisis	−10.141**	(4.580)	−10.597**	(4.548)	−9.475**	(4.610)
GDP growth	0.205	(0.450)	0.170	(0.455)	0.565	(0.544)
Executive election	−3.964	(3.590)	−4.553	(3.506)	−4.094	(3.604)
Left-wing government	0.455	(1.890)	0.593	(1.843)	0.867	(1.882)
Government fractionalization	−4.218	(5.362)	−4.426	(5.335)	−3.167	(5.688)
Regional crises	−4.165	(12.090)	−5.048	(12.221)	−4.699	(12.557)
Regional conflicts	5.290	(7.432)	5.437	(7.438)	8.009	(7.549)
Regional disasters	0.475	(1.151)	0.336	(1.162)	0.733	(1.201)
Lagged aid level	−15.798***	(1.731)	−16.048***	(1.715)	−17.694***	(1.983)
Multilateral aid growth						
Campaign	**−8.491**	**(8.670)**	**4.086**	**(23.136)**	**−62.985***	**(36.791)**
Campaign × X	**2.474**	**(2.824)**	**−6.412**	**(26.381)**	**0.718***	**(0.427)**
X	**2.710**	**(1.998)**	**−16.188**	**(22.004)**	**0.128**	**(0.288)**
Financial crisis	2.229	(5.367)	2.540	(5.279)	2.778	(5.391)
GDP growth	0.666**	(0.283)	0.646**	(0.283)	0.704**	(0.326)
Executive election	−3.675	(3.789)	−4.003	(3.731)	−4.059	(3.651)
Left-wing government	1.515	(1.778)	2.220	(1.758)	2.293	(1.802)
Government fractionalization	0.817	(5.395)	0.816	(5.514)	0.999	(5.923)
Regional crises	5.661	(8.975)	5.415	(8.870)	6.094	(8.880)
Regional conflicts	4.440	(7.416)	4.203	(7.452)	2.469	(7.577)
Regional disasters	−1.862*	(1.128)	−1.890*	(1.127)	−1.361	(1.202)
Lagged aid level	−13.615***	(1.991)	−13.347***	(1.927)	−13.763***	(2.028)
Multi-bi aid growth						
Campaign	**2.363**	**(9.065)**	**50.414***	**(18.254)**	**−149.786***	**(50.562)**
Campaign × X	**0.205**	**(2.961)**	**−54.216**	**(21.791)**	**1.692***	**(0.574)**
X	**−3.572***	**(2.056)**	**−18.753**	**(24.020)**	**0.014**	**(0.440)**
Financial crisis	−1.889	(3.604)	−1.755	(3.670)	0.367	(3.934)
GDP growth	0.642	(0.407)	0.622	(0.407)	0.403	(0.493)
Executive election	6.152	(4.083)	6.054	(3.869)	6.179	(4.075)
Left-wing government	1.144	(1.962)	0.509	(1.916)	1.001	(1.971)
Government fractionalization	2.257	(8.115)	0.650	(8.104)	5.553	(8.457)
Regional crises	16.746	(10.186)	15.915	(10.254)	17.055*	(9.842)
Regional conflicts	13.487**	(6.181)	13.565**	(6.083)	12.347*	(6.611)
Regional disasters	−1.689	(1.165)	−1.609	(1.133)	−0.692	(1.244)
Lagged aid level	−4.584***	(0.734)	−4.348***	(0.704)	−4.183***	(0.710)
Country-fixed effects	yes	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes	yes
N_B/R^2_B	481	0.25	481	0.24	452	0.26
N_M/R^2_M	481	0.24	481	0.23	452	0.23
N_{MB}/R^2_{MB}	383	0.25	383	0.26	354	0.26
$\rho_{B,M}$	0.030	(0.046)	0.034	(0.046)	0.029	(0.048)
$\rho_{B,MB}$	0.006	(0.053)	0.007	(0.052)	−0.026	(0.054)
$\rho_{M,MB}$	0.052	(0.054)	0.054	(0.054)	0.052	(0.056)

Notes: System of equations estimated simultaneously with dependent variables shown in row heads. Campaign is binary, X is the moderator in the respective column header, and Campaign × X the multiplicative interaction term. For the two countries for which the neighborhood is empty (Australia and New Zealand), I set the respective values for the three regional variables to zero to avoid a drop in observations. Standard errors clustered on countries. Significance levels: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table A4. UNSC campaign and aid growth without control variables.

	Donor experience (1)		Government quality (2)		Political globalization (3)	
Bilateral aid growth						
Campaign	−9.298	(5.872)	−4.470	(18.172)	2.640	(41.178)
Campaign × X	4.295**	(2.114)	7.917	(21.438)	−0.010	(0.480)
X	−0.081	(1.881)	7.376	(20.116)	0.126	(0.296)
Lagged aid level	−15.511***	(1.761)	−15.746***	(1.749)	−17.438***	(2.026)
Multilateral aid growth						
Campaign	−9.315	(8.403)	3.267	(23.585)	−54.172	(37.156)
Campaign × X	2.830	(2.802)	−5.430	(27.088)	0.617	(0.433)
X	2.720	(1.936)	−7.083	(21.526)	0.182	(0.276)
Lagged aid level	−13.894***	(1.980)	−13.637***	(1.880)	−14.075***	(2.020)
Multi-bi aid growth						
Campaign	2.044	(9.254)	56.936***	(18.921)	−146.578***	(52.279)
Campaign × X	0.202	(2.997)	−61.710***	(22.260)	1.647***	(0.593)
X	−3.387*	(2.036)	−9.094	(24.533)	0.055	(0.433)
Lagged aid level	−4.237***	(0.723)	−3.998***	(0.690)	−4.010***	(0.706)
Country-fixed effects	yes	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes	yes
N_B/R_B^2	493	0.23	493	0.22	457	0.24
N_M/R_M^2	493	0.23	493	0.22	457	0.22
N_{MB}/R_{MB}^2	391	0.22	391	0.23	358	0.24
$\rho_{B,M}$	0.021	(0.046)	0.025	(0.046)	0.031	(0.048)
$\rho_{B,MB}$	−0.004	(0.052)	−0.002	(0.052)	−0.032	(0.053)
$\rho_{M,MB}$	0.093*	(0.054)	0.094*	(0.054)	0.072	(0.056)

Notes: System of equations estimated simultaneously with dependent variables shown in row heads. Campaign is binary, X is the moderator in the respective column header, and Campaign × X the multiplicative interaction term. Standard errors clustered on countries. Significance levels: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table A5. Different operationalization of moderator variables.

	Donor experience (1)		VDem corruption (2)		Neighborhood size (3)	
Bilateral aid growth						
Campaign	**−10.359**	**(6.774)**	**1.983**	**(4.121)**	**1.842**	**(3.969)**
Campaign × X	**4.054***	**(2.174)**	**7.979**	**(24.426)**	**0.043**	**(0.244)**
X	**−0.078**	**(1.180)**	**−80.863***	**(42.169)**	**−1.296*****	**(0.248)**
Financial crisis	−9.685**	(4.675)	−7.685	(4.786)	−10.252**	(4.609)
GDP growth	0.194	(0.444)	0.148	(0.456)	0.212	(0.442)
Executive election	−3.495	(3.621)	−4.712	(3.388)	−4.567	(3.543)
Left-wing government	0.474	(1.882)	0.576	(1.842)	0.834	(1.852)
Government fractionalization	−4.073	(5.339)	−3.920	(5.237)	−4.393	(5.432)
Lagged aid level	−15.581***	(1.804)	−15.794***	(1.773)	−16.018***	(1.748)
Multilateral aid growth						
Campaign	**−6.274**	**(8.893)**	**0.863**	**(3.951)**	**−0.723**	**(4.065)**
Campaign × X	**1.435**	**(2.723)**	**−18.235**	**(30.258)**	**−0.050**	**(0.292)**
X	**0.669**	**(1.111)**	**−37.057**	**(43.678)**	**−0.480*****	**(0.163)**
Financial crisis	2.505	(5.382)	3.104	(5.542)	2.911	(5.346)
GDP growth	0.567**	(0.282)	0.676**	(0.286)	0.536*	(0.281)
Executive election	−3.890	(3.891)	−4.252	(3.792)	−4.165	(3.849)
Left-wing government	1.917	(1.771)	1.892	(1.762)	2.104	(1.773)
Government fractionalization	2.003	(5.404)	1.580	(5.482)	1.901	(5.467)
Lagged aid level	−13.134***	(1.944)	−13.291***	(2.008)	−13.271***	(1.932)
Multi-bi aid growth						
Campaign	**−1.951**	**(9.689)**	**−1.863**	**(4.236)**	**−1.554**	**(4.327)**
Campaign × X	**1.388**	**(3.012)**	**75.563****	**(29.841)**	**0.476***	**(0.274)**
X	**−1.544**	**(1.195)**	**−35.449**	**(54.308)**	**−0.123**	**(0.171)**
Financial crisis	−3.656	(3.294)	−3.405	(3.579)	−3.834	(3.448)
GDP growth	0.500	(0.402)	0.441	(0.411)	0.557	(0.407)
Executive election	6.262	(4.102)	5.638	(3.969)	5.479	(4.083)
Left-wing government	1.035	(1.930)	0.432	(1.942)	1.125	(1.939)
Government fractionalization	3.530	(7.982)	2.445	(8.194)	2.999	(8.009)
Lagged aid level	−4.410***	(0.746)	−4.250***	(0.737)	−4.255***	(0.710)
Country-fixed effects	yes	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes	yes
N_B/R_B^2	475	0.24	459	0.25	481	0.24
N_M/R_M^2	475	0.23	459	0.24	481	0.23
N_{MB}/R_{MB}^2	383	0.24	372	0.24	383	0.24
$\rho_{B,M}$	0.022	(0.046)	0.021	(0.047)	0.031	(0.046)
$\rho_{B,MB}$	0.001	(0.053)	−0.001	(0.054)	0.004	(0.053)
$\rho_{M,MB}$	0.067	(0.054)	0.065	(0.055)	0.070	(0.054)

Notes: System of equations estimated simultaneously with dependent variables shown in row heads. Campaign is binary, X is the moderator in the respective column header, and Campaign × X the multiplicative interaction term. Donor experience was taken from Fuchs and Müller (2018). Standard errors clustered on countries. Significance levels: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table A6. Lagged effect of donor campaigning.

	Donor experience (1)		Government quality (2)		Political globalization (3)	
Bilateral aid growth						
Campaign	**8.141***	**(4.837)**	**8.314**	**(17.311)**	**44.588**	**(38.068)**
Campaign × X	**−2.873**	**(1.933)**	**−9.352**	**(20.491)**	**−0.532**	**(0.450)**
X	**0.074**	**(1.944)**	**13.410**	**(19.779)**	**0.189**	**(0.300)**
Financial crisis	−10.291**	(4.766)	−10.440**	(4.756)	−9.628**	(4.815)
GDP growth	0.225	(0.452)	0.196	(0.451)	0.602	(0.542)
Executive election	−4.258	(3.513)	−4.343	(3.481)	−4.021	(3.630)
Left-wing government	0.901	(1.909)	0.714	(1.855)	0.890	(1.875)
Government fractionalization	−4.849	(5.532)	−4.456	(5.461)	−3.671	(5.758)
Lagged aid level	−15.756***	(1.734)	−15.984***	(1.710)	−17.575***	(1.963)
Multilateral aid growth						
Campaign	**−6.046**	**(6.928)**	**−14.365**	**(17.798)**	**−80.959****	**(32.406)**
Campaign × X	**1.150**	**(2.399)**	**13.858**	**(20.695)**	**0.912****	**(0.382)**
X	**2.630**	**(1.987)**	**−18.184**	**(22.000)**	**0.027**	**(0.293)**
Financial crisis	3.296	(5.390)	3.640	(5.353)	4.189	(5.347)
GDP growth	0.604**	(0.280)	0.608**	(0.282)	0.611*	(0.326)
Executive election	−4.614	(3.698)	−4.466	(3.703)	−4.190	(3.594)
Left-wing government	1.514	(1.808)	2.101	(1.775)	2.094	(1.796)
Government fractionalization	1.318	(5.407)	1.328	(5.460)	1.049	(5.835)
Lagged aid level	−13.693***	(2.015)	−13.321***	(1.964)	−13.818***	(2.053)
Multi-bi aid growth						
Campaign	**20.603***	**(7.748)**	**32.545***	**(18.125)**	**33.656**	**(64.446)**
Campaign × X	**−6.483****	**(2.724)**	**−36.594***	**(21.123)**	**−0.352**	**(0.731)**
X	**−4.084****	**(2.063)**	**−25.281**	**(23.736)**	**0.075**	**(0.443)**
Financial crisis	−2.280	(3.375)	−3.516	(3.357)	−2.667	(3.666)
GDP growth	0.523	(0.403)	0.488	(0.401)	0.204	(0.452)
Executive election	6.663*	(3.990)	6.563*	(3.875)	6.613*	(4.020)
Left-wing government	1.341	(1.932)	0.525	(1.906)	1.510	(1.967)
Government fractionalization	3.356	(7.971)	2.371	(8.019)	8.812	(8.225)
Lagged aid level	−4.594***	(0.717)	−4.373***	(0.712)	−4.141***	(0.723)
Country-fixed effects	yes	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes	yes
N_B/R_B^2	481	0.24	481	0.24	452	0.26
N_M/R_M^2	481	0.23	481	0.23	452	0.23
N_{MB}/R_{MB}^2	383	0.24	383	0.24	354	0.24
$\rho_{B,M}$	0.033	(0.046)	0.032	(0.046)	0.032	(0.048)
$\rho_{B,MB}$	−0.004	(0.053)	0.000	(0.053)	−0.020	(0.054)
$\rho_{M,MB}$	0.065	(0.054)	0.067	(0.054)	0.063	(0.056)

Notes: System of equations estimated simultaneously with dependent variables shown in row heads. Campaign is binary and lagged by one year, X is the moderator in the respective column header, and Campaign × X the multiplicative interaction term. Standard errors clustered on countries. Significance levels: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table A7. Including P5 observations in the sample.

	Donor experience (1)		Government quality (2)		Political globalization (3)	
Bilateral aid growth						
Campaign	**−9.230**	**(6.130)**	**−7.278**	**(18.357)**	**4.525**	**(41.778)**
Campaign × X	**4.226***	**(2.161)**	**11.220**	**(21.671)**	**−0.037**	**(0.486)**
X	**0.989**	**(1.785)**	**4.539**	**(18.684)**	**0.131**	**(0.287)**
Financial crisis	−11.882***	(3.852)	−11.987***	(3.836)	−11.273***	(3.842)
GDP growth	0.184	(0.420)	0.160	(0.426)	0.490	(0.512)
Executive election	−0.747	(3.202)	−1.066	(3.187)	−0.743	(3.213)
Left-wing government	−0.027	(1.782)	0.223	(1.744)	0.482	(1.778)
Government fractionalization	−4.945	(5.298)	−5.256	(5.283)	−4.196	(5.584)
Lagged aid level	−16.589***	(1.729)	−16.858***	(1.714)	−18.469***	(1.978)
Multilateral aid growth						
Campaign	**−8.969**	**(8.395)**	**5.141**	**(23.961)**	**−69.063***	**(35.260)**
Campaign × X	**2.771**	**(2.792)**	**−7.076**	**(27.469)**	**0.788***	**(0.413)**
X	**3.257***	**(1.803)**	**−33.712**	**(20.623)**	**0.074**	**(0.274)**
Financial crisis	3.992	(4.613)	4.743	(4.512)	4.346	(4.634)
GDP growth	0.701**	(0.273)	0.716***	(0.274)	0.772**	(0.316)
Executive election	2.165	(3.434)	2.149	(3.369)	2.003	(3.399)
Left-wing government	2.442	(1.660)	3.235*	(1.652)	3.390**	(1.677)
Government fractionalization	4.179	(5.187)	3.701	(5.284)	3.568	(5.599)
Lagged aid level	−14.743***	(2.038)	−14.319***	(1.964)	−14.875***	(2.042)
Multi-bi aid growth						
Campaign	**1.285**	**(9.439)**	**51.470***	**(18.294)**	**−149.111***	**(52.252)**
Campaign × X	**0.516**	**(3.045)**	**−55.608**	**(21.693)**	**1.681***	**(0.593)**
X	**−2.190**	**(1.976)**	**−29.547**	**(23.160)**	**−0.157**	**(0.468)**
Financial crisis	−0.432	(4.067)	−0.090	(4.018)	1.232	(4.192)
GDP growth	0.521	(0.403)	0.516	(0.404)	0.281	(0.461)
Executive election	2.575	(3.572)	2.499	(3.496)	2.988	(3.599)
Left-wing government	1.281	(1.820)	0.843	(1.772)	1.180	(1.820)
Government fractionalization	2.645	(7.641)	0.860	(7.665)	5.858	(7.935)
Lagged aid level	−3.967***	(0.655)	−3.902***	(0.651)	−3.796***	(0.659)
Country-fixed effects	yes	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes	yes
N_B/R_B^2	547	0.24	547	0.23	515	0.25
N_M/R_M^2	547	0.23	547	0.22	515	0.22
N_{MB}/R_{MB}^2	437	0.21	437	0.22	405	0.22
$\rho_{B,M}$	0.064	(0.043)	0.069	(0.043)	0.056	(0.045)
$\rho_{B,MB}$	0.006	(0.049)	0.008	(0.049)	−0.009	(0.05)
$\rho_{M,MB}$	0.087*	(0.051)	0.085*	(0.051)	0.086*	(0.052)

Notes: System of equations estimated simultaneously with dependent variables shown in row heads. Campaign is binary, X is the moderator in the respective column header, and Campaign × X the multiplicative interaction term. Standard errors clustered on countries. Significance levels: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table A8. Using aid disbursements rather than aid commitments.

	Donor experience (1)		Government quality (2)		Political globalization (3)	
Bilateral aid growth						
Campaign	**−2.848**	**(4.785)**	**−1.698**	**(13.497)**	**−9.278**	**(19.105)**
Campaign × X	**1.578**	**(1.807)**	**3.090**	**(16.516)**	**0.122**	**(0.231)**
X	**1.400**	**(1.747)**	**−10.354**	**(18.503)**	**0.150**	**(0.192)**
Financial crisis	−2.489	(4.191)	−2.450	(4.092)	−1.731	(4.073)
GDP growth	0.608*	(0.369)	0.535	(0.337)	0.759**	(0.351)
Executive election	−1.633	(2.639)	−2.273	(2.548)	−1.997	(2.583)
Left-wing government	−0.955	(1.728)	−0.635	(1.683)	−0.727	(1.725)
Government fractionalization	−13.103**	(5.136)	−13.983***	(5.327)	−11.858**	(5.419)
Lagged aid level	−5.369***	(1.207)	−5.521***	(1.190)	−6.122***	(1.259)
Multilateral aid growth						
Campaign	**−2.280**	**(4.546)**	**−4.778**	**(11.923)**	**−30.520***	**(18.319)**
Campaign × X	**1.312**	**(1.739)**	**7.399**	**(14.590)**	**0.377***	**(0.221)**
X	**0.649**	**(1.628)**	**−30.903***	**(18.088)**	**0.127**	**(0.185)**
Financial crisis	8.983**	(3.690)	7.860**	(3.603)	8.598**	(3.566)
GDP growth	0.493**	(0.216)	0.289	(0.204)	0.323	(0.228)
Executive election	−5.748**	(2.683)	−4.974*	(2.634)	−4.763*	(2.664)
Left-wing government	−0.062	(1.558)	0.061	(1.534)	0.050	(1.581)
Government fractionalization	2.593	(4.269)	1.962	(4.282)	3.176	(4.573)
Lagged aid level	−6.786***	(1.170)	−7.427***	(1.204)	−7.065***	(1.231)
Multi-bi aid growth						
Campaign	**12.164***	**(7.165)**	**53.331*****	**(19.296)**	**−140.347****	**(61.275)**
Campaign × X	**−2.869**	**(2.524)**	**−57.431****	**(22.965)**	**1.589****	**(0.693)**
X	**−5.632*****	**(2.101)**	**−17.606**	**(24.559)**	**0.376**	**(0.412)**
Financial crisis	0.635	(5.500)	0.501	(5.492)	0.621	(5.376)
GDP growth	0.529	(0.392)	0.520	(0.395)	0.301	(0.465)
Executive election	3.563	(4.113)	3.680	(4.087)	4.777	(4.145)
Left-wing government	3.051	(1.951)	2.019	(1.924)	1.955	(1.979)
Government fractionalization	9.848	(8.001)	8.048	(8.093)	10.839	(8.386)
Lagged aid level	−4.775***	(0.759)	−4.397***	(0.736)	−4.299***	(0.748)
Country-fixed effects	yes	yes	yes	yes	yes	yes
Year-fixed effects	yes	yes	yes	yes	yes	yes
N_B/R_B^2	590	0.18	628	0.18	594	0.19
N_M/R_M^2	590	0.19	628	0.19	594	0.18
N_{MB}/R_{MB}^2	386	0.23	388	0.23	358	0.23
$\rho_{B,M}$	0.078*	(0.043)	0.070	(0.042)	0.065	(0.043)
$\rho_{B,MB}$	−0.060	(0.051)	−0.065	(0.050)	−0.086*	(0.051)
$\rho_{M,MB}$	−0.004	(0.051)	−0.007	(0.051)	−0.016	(0.052)

Notes: System of equations estimated simultaneously with dependent variables shown in row heads. Campaign is binary, X is the moderator in the respective column header, and Campaign × X the multiplicative interaction term. Standard errors clustered on countries. Significance levels: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table A9. Pooled SUR estimations.

	Donor experience (1)		Government quality (2)		Political globalization (3)	
Bilateral aid growth						
Campaign	**−10.000**	**(6.101)**	**7.008**	**(19.466)**	**6.516**	**(34.872)**
Campaign × X	**4.112***	**(2.160)**	**−7.143**	**(22.805)**	**−0.072**	**(0.408)**
X	**0.647**	**(0.728)**	**13.583**	**(8.960)**	**0.145**	**(0.114)**
Financial crisis	−8.866*	(4.814)	−8.546*	(4.856)	−8.739*	(4.931)
GDP growth	0.272	(0.424)	0.219	(0.419)	0.693	(0.512)
Executive election	−4.459	(3.808)	−5.597	(3.789)	−4.958	(3.682)
Left-wing government	0.539	(1.751)	0.546	(1.771)	1.019	(1.842)
Government fractionalization	1.847	(3.084)	0.101	(3.384)	3.861	(3.177)
Lagged aid level	−3.167***	(0.679)	−3.181***	(0.693)	−3.306***	(0.866)
EEG	−11.755**	(4.850)	−12.245***	(4.749)	−17.752***	(5.684)
WEOG	−7.711**	(3.002)	−9.454***	(3.164)	−9.674***	(3.447)
Multilateral aid growth						
Campaign	**−7.104**	**(7.707)**	**13.674**	**(21.645)**	**−53.925**	**(36.596)**
Campaign × X	**2.829**	**(2.646)**	**−15.005**	**(24.669)**	**0.641**	**(0.427)**
X	**−0.056**	**(0.695)**	**−1.928**	**(8.589)**	**0.267****	**(0.121)**
Financial crisis	3.423	(5.781)	2.940	(5.750)	3.132	(5.910)
GDP growth	0.499**	(0.249)	0.493**	(0.250)	0.576*	(0.303)
Executive election	−5.982	(3.913)	−6.683*	(3.767)	−7.338**	(3.637)
Left-wing government	1.798	(1.684)	2.072	(1.691)	1.721	(1.760)
Government fractionalization	3.700	(3.132)	4.056	(3.447)	4.190	(3.215)
Lagged aid level	−1.975***	(0.629)	−1.963***	(0.616)	−3.321***	(0.878)
EEG	2.123	(4.430)	0.965	(4.430)	−4.002	(4.722)
WEOG	−1.408	(3.276)	−1.324	(3.345)	−5.724*	(3.456)
Multi-bi aid growth						
Campaign	**−0.266**	**(8.058)**	**50.215***	**(15.033)**	**−91.455***	**(51.564)**
Campaign × X	**1.790**	**(2.637)**	**−52.535***	**(18.481)**	**1.079***	**(0.583)**
X	**−0.098**	**(0.735)**	**16.653***	**(9.133)**	**−0.096**	**(0.137)**
Financial crisis	−4.982	(4.271)	−4.448	(4.126)	−4.377	(4.367)
GDP growth	0.370	(0.408)	0.220	(0.397)	0.188	(0.527)
Executive election	3.934	(4.170)	2.836	(3.909)	3.390	(4.081)
Left-wing government	1.842	(1.822)	1.479	(1.834)	2.425	(1.854)
Government fractionalization	6.058*	(3.273)	3.664	(3.497)	7.650**	(3.356)
Lagged aid level	−1.917***	(0.460)	−2.229***	(0.479)	−1.843***	(0.526)
EEG	−9.261*	(5.261)	−8.571*	(5.122)	−15.877***	(5.506)
WEOG	−5.602	(3.798)	−6.630*	(3.797)	−8.900**	(3.786)
Country-fixed effects	no	no	no	no	no	no
Year-fixed effects	yes	yes	yes	yes	yes	yes
N_B/R_B^2	590	0.17	628	0.18	594	0.19
N_M/R_M^2	590	0.19	628	0.19	594	0.18
N_{MB}/R_{MB}^2	386	0.23	388	0.23	358	0.22
$\rho_{B,M}$	0.080*	(0.043)	0.069	(0.042)	0.066	(0.043)
$\rho_{B,MB}$	−0.065	(0.051)	−0.073	(0.050)	−0.080	(0.052)
$\rho_{M,MB}$	−0.007	(0.051)	−0.004	(0.051)	−0.010	(0.052)

Notes: System of equations estimated simultaneously with dependent variables shown in row heads. Campaign is binary and lagged by one year, X is the moderator in the respective column header, and Campaign × X the multiplicative interaction term. Standard errors clustered on countries. Significance levels: * $p < .1$ ** $p < .05$ *** $p < .01$.

Table A10. Constituency-specific effects using pooled SUR estimations.

	EEG (1)		WEOG (2)		APG (3)	
Bilateral aid growth						
Campaign	**2.447**	**(3.246)**	**9.498***	**(5.364)**	**−1.946**	**(3.244)**
Campaign × X	**−7.736**	**(8.332)**	**−11.394***	**(6.282)**	**16.623****	**(6.507)**
X	**−2.954**	**(3.189)**	**−0.583**	**(2.112)**	**3.700**	**(3.282)**
Financial crisis	−7.568	(5.001)	−7.609	(5.088)	−8.283	(5.039)
GDP growth	0.375	(0.443)	0.346	(0.426)	0.298	(0.417)
Executive election	−3.397	(4.119)	−5.104	(4.096)	−4.430	(3.833)
Left-wing government	−0.405	(1.726)	−0.342	(1.694)	0.007	(1.726)
Government fractionalization	0.551	(2.984)	−0.157	(2.957)	0.918	(3.001)
Lagged aid level	−2.250***	(0.617)	−1.855***	(0.499)	−2.224***	(0.500)
Multilateral aid growth						
Campaign	**0.749**	**(2.958)**	**1.474**	**(5.816)**	**0.249**	**(3.064)**
Campaign × X	**0.082**	**(10.098)**	**−1.260**	**(6.637)**	**2.009**	**(8.098)**
X	**3.174**	**(2.398)**	**−2.115**	**(2.140)**	**1.131**	**(3.690)**
Financial crisis	3.409	(5.716)	2.901	(5.652)	2.796	(5.764)
GDP growth	0.502**	(0.250)	0.489**	(0.249)	0.500**	(0.248)
Executive election	−6.308	(3.988)	−6.526*	(3.797)	−6.090	(3.874)
Left-wing government	1.701	(1.674)	2.095	(1.618)	2.125	(1.653)
Government fractionalization	3.282	(3.070)	3.850	(3.023)	3.852	(3.078)
Lagged aid level	−1.869***	(0.614)	−2.048***	(0.579)	−2.241***	(0.535)
Multi-bi aid growth						
Campaign	**4.782**	**(3.181)**	**11.277***	**(5.864)**	**3.950**	**(3.215)**
Campaign × X	**1.794**	**(5.712)**	**−7.920**	**(6.750)**	**7.486**	**(8.881)**
X	**−4.168**	**(3.640)**	**−0.086**	**(2.852)**	**4.307**	**(4.143)**
Financial crisis	−6.011	(4.277)	−5.909	(4.404)	−5.094	(4.341)
GDP growth	0.379	(0.410)	0.379	(0.402)	0.368	(0.397)
Executive election	4.111	(4.335)	3.570	(4.261)	3.774	(4.086)
Left-wing government	1.378	(1.786)	1.305	(1.764)	1.634	(1.790)
Government fractionalization	4.703	(3.111)	4.380	(3.110)	5.436*	(3.238)
Lagged aid level	−1.828***	(0.453)	-1.644***	(0.421)	−1.735***	(0.409)
Country-fixed effects	no	no	no	no	no	no
Year-fixed effects	yes	yes	yes	yes	yes	yes
N_B/R_B^2	481	0.11	481	0.12	481	0.13
N_M/R_M^2	481	0.12	481	0.12	481	0.11
N_{MB}/R_{MB}^2	383	0.15	383	0.16	383	0.16
$\rho_{B,M}$	0.004	(0.047)	−0.003	(0.047)	−0.001	(0.047)
$\rho_{B,MB}$	0.040	(0.052)	0.031	(0.052)	0.027	(0.052)
$\rho_{M,MB}$	0.110**	(0.051)	0.105**	(0.051)	0.108**	(0.051)

Notes: System of equations estimated simultaneously with dependent variables shown in row heads. Campaign is binary and lagged by one year, X is the moderator in the respective column header, and Campaign × X the multiplicative interaction term. Standard errors clustered on countries. Significance levels: * $p < .1$ ** $p < .05$ *** $p < .01$.

Appendix B. Coding protocol and illustrative examples.

B1. A Glance at the Raw Data

Date	Constitu	Term	Tern Per	NonPerm	iso3A	BidA	Comp	Com	AidA	AidAt	Trac	PKOe	Requ	iso3B	Supp	Den	Supp	AidDe	TradeD	Quotes	NewsID
18/05/1994	APG	1995-96	0	1	IDN	1								NZL	1						bbcfe00020011105dq5i008rd
18/07/1994	APG	1995-96	0	1	KOR	1								BGR	1						ked0000020011105dq7i001y5
24/01/1995	WEOG	1997-98	0	1	AUS	1					1			BRB	1						lba0000020011103dr1o04fir

Description of the variables (most relevant variables highlighted in bold font):

- Date: exact date of media report
- **Constituency**: name of the UN constituency
- **Term**: term for which donor seeks UNSC membership (if not mentioned, then this value was imputed from other sources)
- Permanent: 1 if donor is seeking permanent UNSC membership (observations with Permanent = 1 discarded)
- NonPerm: 1 if donor is seeking non-permanent UNSC membership
- **iso3A**: country code of the donor seeking UNSC seat
- **BidA**: 1 if press report states that donor seeks support for its UNSC candidature (observations with BidA = 0 discarded)
- Comp1: country code of first competitor also vying for UNSC seat in the same term if the media report mentions it
- Comp2: country code of any second competitor if media report mentions it (these variables are not used as they are likely incomplete and other sources should be used)
- AidA: 1 if donor is reported to commit to aid increase to unspecified recipients in the same media report
- AidAtoB: 1 if donor is reported to commit aid increase to specified recipient (iso3B) in the same media report
- TradeAgreement: 1 if donor is reported to promise better trade relations with specified recipient (iso3B) in the same media report
- PKOengage: 1 if donor is reported to commit more engagement in UN peacekeeping in the same report
- **RequestSupport**: 1 if donor solicits support for its UNSC candidature from (un)specified recipient(s)
- **iso3B**: country code of supporting country
- **SupportA**: 1 if media report mentions iso3B supporting UNSC candidature for iso3A
- DenySupport: 1 if media report mentions iso3B denying support for UNSC candidature of iso3A
- **SupportAB**: 1 if report mentions commitment of mutual support for UNSC bids between iso3A and iso3B
- AidDemanded: 1 if report mentions iso3B asking for increased development cooperation from iso3A
- TradeDemanded: 1 if the report mentions iso3B asking for increased trade cooperation with iso3A
- Quotes: Any key quotes from the article; also codes exhaustively for instances of support for candidacies in other international venues
- NewsID: Factiva news ID
- **Media report** (derived variable): 1 if document is a relevant media report (NonPerm = 1 and BidA = 1) in which the donor requests support or receives it (RequestSupport = 1, SupportA = 1, or SupportAB = 1). Used for aggregation to the donor-year level and then called Campaigns.

B2. The Data Collection Process

A media report is relevant in the context of my research question if it contains explicit mentioning of the aspiration for temporary UNSC membership of a given country. A media report is irrelevant under any of the following conditions: 1) the report type is an opinion piece that does not present any new information previously coded; 2) the article discusses institutional reform of the UNSC; 3) a country vies for permanent membership in the UNSC.

To identify as many relevant source documents as possible, I tried several combinations of search strings and found the following to yield the best results in terms of covering many relevant media reports while discarding the irrelevant ones (based on a glance at the first hundred hits):

united nations security council *near7* (candidacy *or* candidate *or* candidature *or* non-permanent member *not* permanent *or* temporary seat *or* membership *or* election *or* campaign *or* competition *or* bid) *near5* (win *or* won *or* lose *or* lost *or* announce *or* vote *or* elect *or* select *or* choose *or* chosen *or* assume *or* support *or* assist *or* encourage)

Further search options in the Factiva menu included:

- English language
- Duplicates not included (if one event is reported by several newswires)

B3. The Coding Process: An Illustrative Example

The following media report is one document in the entire Factiva dataset (which contains 1,706 documents covering the 1994–2016 period). Factiva has highlighted the main search terms in yellow. In addition, I have underlined text passages that are used to code the variables in the campaigning spreadsheet.

BBC

```
President Museveni urges Icelandic entrepreneurs to invest in Uganda


332 words
19 September 2008 15:57
BBC Monitoring Africa BBCAP English
(c) 2008 The British Broadcasting Corporation. All Rights Reserved. No material may be
reproduced except with the express permission of The British Broadcasting Corporation.

Text of report by state-run Uganda Broadcasting Corporation (UBC) Radio on 19 September

[Presenter] President Yoweri Museveni has invited entrepreneurs in Iceland to invest in
priority areas of the Ugandan economy such as infrastructure development, harnessing energy
and agro-processing for the export market. He is in Iceland on a state visit.

Mr Museveni thanked Iceland for development and humanitarian assistance Iceland continues to
extend to Uganda, particularly in adult literacy, energy, fisheries and scientific research
to raise household income, especially among the rural poor.

He commended Iceland for her great strides in geothermal energy, hydropower, ICT and fisheries,
adding that Uganda would like to share this experience. Mr Museveni said he was impressed by
the achievements of Iceland in social, economic and political transformation, and said his
government aims to transform Uganda from a peasant society to an industrial nation.

He said Uganda will support Iceland's candidature for the United Nations Security Council
slot, saying Uganda hopes to also serve on the same council in future.

Mr Museveni invited President Grimsson to visit Uganda, which was accepted.

President Olafur Grimsson saluted the people of Uganda and hoped that the growing cooperation
between the two countries would become a model. He said President Museveni's visit was a
demonstration of the desired collaboration in geothermal resources, fishing and ICT, where
Iceland has a comparative advantage. He assured Mr Museveni that Iceland would cooperate
with Uganda.

Grimsson has said Africans, through the Organization of African Unity, the precursor to
the African Union, supported the Icelandic cause for freedom and even send a delegation of
solidarity to Iceland in support of the struggle for independence.

Grimsson, said in welcoming President Museveni and his delegation, he was paying tribute to
the vision of the African leaders, who decades ago, understood that solidarity across oceans,
was essential in building a progressive global community.

Source: UBC Radio, Kampala, in English 0400 gmt 19 Sep 08
Document BBCAP00020080919e49j000dx
```

The following variables are affected by the information in this media report:

Date	Constituency	Term	Permanent	Non Perm	iso3A	Aid AtoB	BidA	iso3B	SupportA	SupportAB	Trade Demanded
19/09/2008	WEOG	2009–10	0	1	**ISL**	0	1	**UGA**	1	1	1

Iceland (iso3a = "ISL") vies for a non-permanent UNSC seat (NonPerm=1 and BidA = 1), supported by Uganda (iso3b = "UGA"). Uganda publicly declares to support this candidature (SupportA = 1). It is also implicit in the news report that Uganda would like to intensify commercial links with Iceland in this context (TradeDemanded = 1) and is expecting support for its own future candidacy (SupportAB = 1). However, Iceland does not promise any new aid (AidAtoB = 0), while reaffirming its ongoing development support.

B4. Data Preparation for Analysis

To allow for country-year panel data analysis, the information in the media reports needs to be aggregated. The variable Media Report covers both a statement of support from another country as well as unilateral solicitation of support for UNSC candidature by a donor. I aggregate the number of lines (representing the number of media reports) for each unique country-year pair to obtain the (untransformed) measure Campaigns. The main analysis relies on the dummy variable $1(Campaigns > 0)$.

B5. Descriptive Statistics
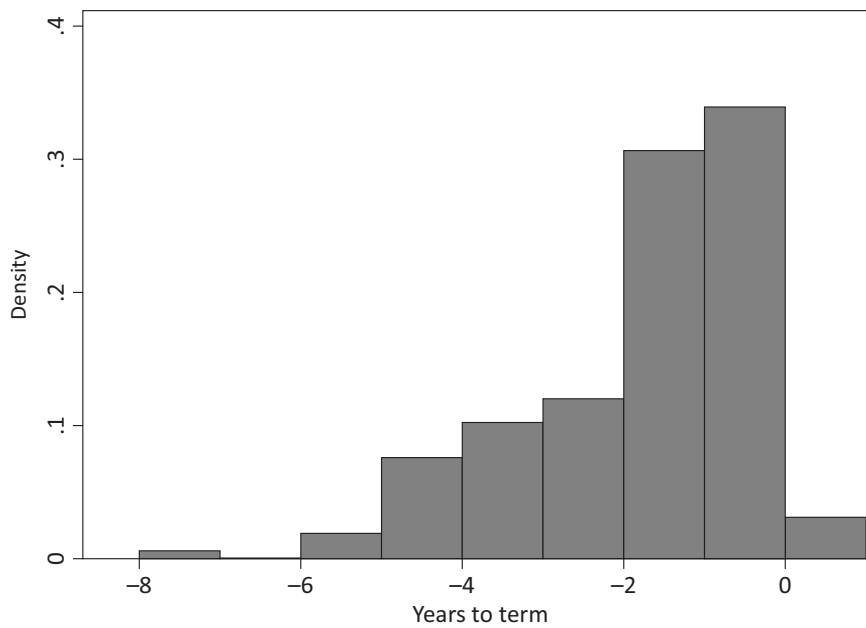
Figure B1. Distribution of campaigning events relative to the envisaged term. Note: The sample includes media reports on the same donors on which the main analysis is based.

Article

What Does the Evidence Tell Us about 'Thinking and Working Politically' in Development Assistance?

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Abstract
This article provides a critical review of the evidence on 'thinking and working politically' (TWP) in development. Scholars and practitioners have increasingly recognised that development is a fundamentally political process, and there are concerted efforts underway to develop more politically-informed and adaptive ways of thinking and working in providing development assistance. However, while there are interesting and engaging case studies in the emerging, largely practitioner-based literature, these do not yet constitute a strong evidence base that shows these efforts can be clearly linked to more effective aid programming. Much of the evidence used so far to support these approaches is anecdotal, does not meet high standards for a robust body of evidence, is not comparative and draws on a small number of self-selected, relatively well-known success stories written primarily by programme insiders. The article discusses the factors identified in the TWP literature that are said to enable politically-informed programmes to increase aid effectiveness. It then looks at the state of the evidence on TWP in three areas: political context, sector, and organisation. The aim is to show where research efforts have been targeted so far and to provide guidance on where the field might focus next. In the final section, the article outlines some ways of testing the core assumptions of the TWP agenda more thoroughly, to provide a clearer sense of the contribution it can make to aid effectiveness.

Issue
This article is part of the issue "Aid Impact and Effectiveness", edited by Rachel M. Gisselquist and Finn Tarp (UNU-WIDER, Finland).

1. Introduction

A long-standing criticism of development assistance has been its technocratic focus. This technocratic approach can be traced back to the origins of modern development assistance after the Second World War, which was in part based on the belief that 'underdevelopment is a function of a lack of resources—usually financial, but also technical or human—and that this can be tackled with a sufficient infusion of capital' (Hudson & Dasandi, 2014, p. 239). However, the growing focus

on aid effectiveness—or more specifically, the lack of aid effectiveness (see Bourguignon & Sundberg, 2007; Doucouliagos & Paldam, 2009; Tarp & Hansen, 2003)—has led to criticisms of the failure of aid donors to engage with the inherently political nature of the development process, criticisms that have come from various sources over an almost thirty year period (e.g. Easterly, 2006; Ferguson, 1990; Leftwich, 2000; Unsworth, 2009). From this perspective, the persistence of poor policies and weak institutions is believed to have less to do with a lack of knowledge or finance and more to do with the ac-

tions of powerful actors, groups or collective movements who gain from existing arrangements and resist change (Leftwich, 2000).

Over the past two decades, in a bid to improve aid effectiveness, major donors have sought to engage more explicitly with the politics of the contexts in which they operate (Carothers & de Gramont, 2013). This turn to politics by aid donors and other development organizations—which is discussed in detail by Carothers and de Gramont (2013)—has been labelled 'thinking and working politically' or 'TWP'. As Teskey (2017) points out, the exact origin of the phrase 'thinking and working politically' is uncertain. The first formal academic reference seems to be in Leftwich (2011), but there are internal Department for International Development (DFID) notes going back at least to the early 2000s that reference the key ideas (see e.g., Pycroft, 2006, 2010).[1] While there is no single agreed definition, framework or set of formal tools for 'TWP', three potentially core principles of TWP have been set out: a) strong political analysis, insight and understanding; b) a detailed appreciation of, and response to, the local context; and c) flexibility and adaptability in program design and implementation (TWP Community of Practice, 2013).

In recent years, there has been a notable increase in aid programmes that explicitly reference TWP and/or what are said to be similar ideas such as 'Doing Development Differently' (DDD), problem-driven iterative adaptation (PDIA) and adaptive management.[2] DFID's recent review of their efforts to integrate politics into programming, for example, highlighted the organisation's commitment to this way of working (Piron, Baker, Savage, & Wiseman, 2016), and it remains at the heart of its approach to governance programming. The focus on politics and power in the 2017 World Development Report (World Bank, 2017) and the introduction of applied political economy analysis (PEA) in USAID missions since 2014 (Garber, 2014; Rocha Menocal et al., 2018) are further examples of the growing interest in TWP in other donors.

Scholars have advocated for greater flexibility, learning from failure and paying attention to political context in aid programmes since at least the 1960s (Carothers & de Gramont, 2013), while calls to adopt a more adaptive, locally-led approach also have a strong precedent

in development theory, with a particular group of authors in the 1980s championing this philosophy (Korten, 1980; Rondinelli, 1983; Therkildsen, 1988). While 'TWP' does not therefore describe an entirely new set of ideas or methods, it is nevertheless clear that we are witnessing an unprecedented level of interest in engaging with power and politics in development organisations. An international TWP Community of Practice[3]—bringing together leading experts from donor agencies, NGOs, the private sector, think tanks and academia—has been meeting periodically since late 2013, with a 'sister' DDD group meeting periodically since 2014.[4] Several case studies have been published (discussed in more detail below). However, despite this growing interest in TWP among development organisations, a crucial issue that has received less attention is the extent to which adopting the ideas and practices associated with TWP have succeeded in improving the effectiveness of development programmes. In other words, does the existing evidence suggest that TWP has led to increased aid effectiveness?

This article considers this question by reviewing the current evidence base on TWP to better understand its contribution to the aid effectiveness agenda in order to inform discussions around what may constitute good practice and what future evidence needs may be. In part it uses the framework suggested by Dasandi, Marquette and Robinson (2016) to more systematically evaluate the current evidence base across three areas—*political settlement*, *sector* and *organization*—to see if different patterns emerge and if more fine-grained lessons for specific contexts can be found. To do this, we reviewed 44 case studies and compared them across their political context, sector and organisation.

The approach used to select this sample of case studies was based on identifying experts through the authors' professional networks and through the TWP Community of Practice mailing list, who were asked to provide relevant case studies. This was further supplemented by searching Google and Google Scholar using various combinations of relevant key words[5]. The sample was limited to studies that look at development practice through a lens or framework where TWP is a central part of the analysis, strategy, partnerships or design. It is not limited to a particular definition of TWP or focused only on a par-

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[1] Pycroft also refers to 'acting politically' to differentiate between activities with a specifically political objective and 'working politically', as described here. Carothers and de Gramont (2013) talk about 'thinking and acting politically'. For whatever reason, this distinction—which is important—does not seem to have been picked up in the wider literature.

[2] For useful discussions of the similarities and differences between these various initiatives, see Parks (2016) and Green (2016).

[3] See Thinking and Working Politically Community of Practice (n.d.). The authors of this article have all played some role in the TWP Community of Practice and associated groups, albeit with varying degrees of formal involvement. Heather Marquette and Mark Robinson were founding members, and Heather was the Community's Secretary from 2013 until 2018. She remains a member of the steering committee. In 2017–18, Ed Laws was appointed as a Research Fellow to undertake research and analysis for the Community of Practice. Some of the work he undertook during the fellowship has contributed to this article. Niheer Dasandi has not had a formal role with the Community but has been a participant in several workshops.

[4] While the DDD website no longer exists, details can be found on ODI and Building State Capability websites. See, for example, Building State Capability (2014) and DDD Manifesto (n.d.).

[5] While not a systematic review, we tried to be as systematic in our approach as possible. We used keyword terms and Boolean operators, as well as UK and US/Australian spelling. These search strings included, for example: 'TWP' AND 'development' OR 'aid' OR 'donor' OR 'programme' OR 'programming' OR 'program'; 'thinking and working politically'; 'thinking and working politically' AND 'development' OR 'aid' OR 'donor'; 'politically-informed' AND 'programme' OR 'programming' OR 'program'; 'politically-smart' AND 'programme' OR 'programming' OR 'program' OR 'donor' OR 'development'; 'adaptive' AND 'management' OR 'programming' OR 'program' OR 'aid' OR 'donor' OR 'development'; 'political economy' AND 'donor'; OR 'aid' OR 'development'; political economy analysis'; 'PEA'; 'PDIA'; 'doing development differently'; 'DDD'.

ticular approach and takes authors who self-identify as writing about TWP (or, in some cases, 'adaptive management') at face value. From the sampling, a database of available case studies was created (see Appendix).[6] In addition to these cases, we also refer to more conceptual literature, as well as conversations that have taken place through blogs and online commentary. This is important for trying to understand what is, as we discuss, in many ways an ongoing conversation rather than an attempt at producing a rigorous evidence exercise.

It is important to note that in limiting our sample of studies that have an explicit TWP focus, our analysis does not consider studies that in fact do fit descriptions of 'TWP', but do not self-identify as 'TWP'. Such studies have not been included due to time and budget constraints. In this same vein, we are not reviewing case studies on the effectiveness of public sector reforms and/or development interventions in general, which would be well beyond the scope of this article. Therefore, our claims regarding the state of the evidence should be understood to refer to the literature that makes a direct link to TWP rather than the wider literature on development programmes that include elements of politically informed practice, but do not explicitly label them as such. Having said this, given that the studies included in our sample have an explicit focus on TWP, we would expect these to provide the strongest evidence on how TWP impacts aid effectiveness. Furthermore, as the cumulative knowledge produced by TWP 'identifiers' is clearly influencing development practice, trying to understand the strength of this particular evidence base remains important.

Much has been written about how prevailing organisational cultures, incentives and structures in most development agencies, as well as political pressure from ministers, continue to pose significant obstacles to the implementation of more politically informed development work (Carothers & De Gramont, 2013; Unsworth, 2015; Yanguas, 2018; Yanguas & Hulme, 2015). Our argument here is not that these obstacles are directly linked to a lack of evidence, or even that a stronger evidence base will, by itself, overcome these obstacles. However, a stronger evidence base that demonstrates clearly and robustly that TWP contributes to more effective development practice and, importantly, improved outcomes would certainly strengthen the case for donors to adopt more politically informed, adaptive approaches to development assistance, and as such could contribute to efforts to overcome these challenges. Our analysis suggests, however, that this strong evidence base does not yet exist.

2. What Does the Evidence Base Currently Look Like?

In this section, we examine the evidence base on TWP. We begin by discussing the factors identified in the existing studies as contributing to increased aid effective-

ness. We then map out the evidence base on TWP. To do this, we utilise the framework proposed by Dasandi et al. (2016) that involves three levels of analysis: 1) the wider *political context* of development interventions—how the political system, leadership and the nature of the political settlement in a given context affect development programmes; 2) the *sectoral* level—how characteristics of specific sectors (e.g. health, education, or water delivery) influence programme implementation and impact; and 3) the *organisational* level—how features of an implementing organisation can support or hinder politically informed programming. In each section below, we provide an overview of the level of analysis and the overall evidence base within that, as well as an illustrative example of a programme that reflects that particular level of analysis. These cases were chosen as 'typical' examples to illustrate the wider body of literature (Gerring, 2008). Of course, this approach has limitations; as Seawright and Gerring (2008, p. 294) explain: 'the chosen case is asked to perform a heroic role: to stand for (represent) a population of cases that is often much larger than the case itself'. This is not, however, untypical in small-*N* samples, and we would not claim that the illustrative cases are somehow representative of the wider reform literature. They are, however, fairly representative of the much more limited sample of TWP case studies, the subject of this article.

Although TWP is not a formal method or operational model, the literature highlights several recurring factors that are said to contribute to the success of more politically-informed programmes. Common success factors flagged by the authors are:

- Leaders were politically smart and could use that knowledge effectively;
- Programme managers allowed local actors to take the lead;
- The programmes adopted an 'iterative problem solving, stepwise learning' process;
- Programme staff brokered relationships with major interest groups;
- Donors provided flexible and strategic funding;
- There was a long-term commitment by donors and high level of continuity in staffing;
- There was a supportive environment in the donor agency.

Each of the factors listed above maps onto what are said to be corresponding weaknesses in more conventional programming approaches. For example, the imperative to be 'politically smart' contrasts with what is seen as the failings of 'politically blind' approaches to development. Similarly, the importance of local ownership is a response to problems that have been seen to emerge from development initiatives largely driven by external actors. We discuss these factors in more detail below.

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[6] The full list of programmes can be found in Laws and Marquette (2018, pp. 37–38). We have also included a table that provides an overview of the 44 studies in the Appendix to this article.

Each of the case studies that we identified included a selection of the above factors in their explanation for the programme's success, albeit sometimes in different combinations and with differing emphasis. It is, however, also important to note that these are the factors that were identified in Booth and Unsworth's (2014) influential article looking at seven cases of successful donor-led interventions. Given, as we discuss below, that these programmes cut across different political contexts, sectors and organisations, an important question that arises is whether these factors are actually the most important elements of the programme success that is claimed to the exclusion of other aspects, or whether these factors have been identified because Booth and Unsworth's (2014) article has been so influential that authors are now primed to look out for and emphasise the same features when evaluating programme success. In other words, does the literature suffer from 'confirmation bias', which means that it draws lessons only from cases that fit a pre-existing notion of what factors lead to more successful programme implementation and outcomes (Dasandi et al., 2016, p. 6)?[7]

One of our initial aims in compiling a database of TWP case studies was to identify patterns in the success factors across programmes, across the three different levels (political context, sector, and organisation). This was based on the expectation that different combinations of the recurring factors identified in the literature would be required to improve programme effectiveness in different contexts and sectors (Hudson & Marquette, 2015, p. 74). For example, we might expect that successfully incorporating politics and adaptive practice into programme design and implementation in the justice and security sector in a fragile and conflict-affected state means something quite different than in, say, a sanitation programme in a relatively stable country. However, our review of the current literature found little in the way of guidance on how and why different aspects of TWP may be necessary and sufficient conditions for success in different scenarios. Furthermore, we found little discussion of whether and how some factors or approaches may be inappropriate in certain contexts.

This is a significant issue, because if TWP is at its heart about illuminating contextual differences in order to move away from 'cookie cutter' best practice approaches (cf. Levy, 2014), then we would expect to see variations in programme design, implementation and outcomes. However, while different case studies have been published since Booth and Unsworth's comparative study, there is very little, if any, variation between them along these factors. Indeed, given the similarities highlighted below, it makes it difficult, if not impossible, to discern if the patterns that are beginning to emerge from comparing the various cases genuinely reflect an emerging consensus or if, in fact, it reflects growing 'group think' about the necessary programme design characteristics among

TWP insiders. Given the lack of discernible difference in the factors identified in the existing literature across the different levels, our discussion of each level focuses on the nature of the evidence in each of these areas, and on providing examples of the types of studies that exist.

2.1. Political Context

There are different ways to distinguish and analyse the political context in which development interventions take place, such as by reference to the political regime, the nature of political and bureaucratic leadership and interaction, or power structures such as gender, religion, ethnicity, caste and rural-urban divides. Here, we focus on the *political settlement* in a country to identify the different political contexts in which TWP programmes have been carried out. Political settlements analysis focuses on the power and incentives that shape the actions of key decision makers (Kelsall, 2018; Khan, 2018; Laws & Leftwich, 2014). As such, it can enable development practitioners to distinguish meaningfully between different country contexts by identifying the kinds of issue areas, programming approaches, and potential partners with whom they are likely to have traction (Kelsall, 2018). Furthermore, the political settlements approach is closely related to the turn to TWP among donors (see Carothers & de Gramont, 2013).

Political settlements have been defined as:

> Informal and formal processes, agreements, and practices in a society that help consolidate politics, rather than violence, as a means for dealing with disagreements about interests, ideas and the distribution and use of power. (Laws & Leftwich, 2014, p. 1)

Several typologies have been put forward to identify different types of political settlement. Synthesizing a range of approaches, Kelsall (2016) identifies three particularly common forms: (i) developmental (ii) predatory and (iii) hybrid. While these are ideal types, they identify a broad range of features that enable states to be categorised according to settlement type. Developmental states are characterised by an inclusive settlement, a high degree of coordination amongst elites and a bureaucracy that operates on largely impersonal norms. Predatory settlements tend to have exclusive settlements, spoils-driven elites and a bureaucracy with pervasive patron-client relations. Hybrid settlements sit between the two; there is a significant degree of inclusion and political contestation is for the most part peaceful, but some elites are excluded and actors may be willing to use political violence. Similarly, some elites are coordinated while others are spoils-driven, and the norms within the bureaucracy vary between elements of patronage and high-functioning 'pockets' that are largely rule-based (Wales, Magee, & Nicolai, 2016, p. 13).

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[7] One expert we consulted flagged that the extent to which these features may be objectively verified is another challenge. What defines a politically smart or a politically unsmart leader, for example?

The existing case studies on TWP programmes are heavily weighted towards countries with hybrid political settlements (see Figure 1). This is perhaps to be expected, given that, as Kelsall (2016) points out, most countries in the developing world will have settlements of this kind, rather than exhibiting the characteristics at the other ends of the settlement spectrum (developmental or predatory). Apart from a report on the Strategic Capacity Building Initiative in Rwanda (AGI, n.d.), there are no studies on how TWP programmes have operated in political contexts characterised by developmental political settlements. Given that inclusive, coordinated, developmental states are highly rare in the developing world, this is not particularly surprising.

One example from a hybrid political settlement is the Enabling State Programme (ESP) in Nepal, a 13-year, DFID-supported programme with a budget of £33m. It sought to address issues of weak governance and social and political exclusion that research had identified as underlying causes of conflict and poor development outcomes. Booth and Unsworth (2014, p. 11) refer to a series of independent evaluations that point to ESP having been a major player in helping to shift the 'rules of the game' in the direction of greater social and political inclusion, as well as achieving more specific, quantifiable results. Specific examples of ESP impact cited include piloting of single treasury accounts in 38 districts (now rolled out in all 75 districts); support for the Public Service Commission that contributed to modest but positive increases in appointments of women and other excluded groups; and provision of disaggregated data and other evidence to the National Planning Commission.

During the lifespan of the programme the DFID office in Nepal is said to have taken considerable effort to become involved in and informed about local politics. This involved analysis of the underlying causes of the conflict dynamics that were unfolding at the time, including the political, economic, gender and ethnic dimensions and the impact of DFID programming. This research helped to refocus the work of the ESP team away from good governance and towards the critical conflict issues. In addition to this research and analysis, the team was able to recruit several well-informed, well-networked elite Nepali staff; the authors argue that these staff were not

only politically well-informed but also skillful in navigating a charged political environment and in seizing opportunities to advance programme objectives (Booth & Unsworth, 2014).

Predatory, exclusive, spoils-driven settlements are the second most common context in which TWP programmes have been studied. Trying to think and work in more politically engaged, experimental or entrepreneurial ways may be particularly appropriate for interventions in these kinds of challenging political contexts, given the uncertain change processes at play and the lack of prior accumulated evidence on what works (Wild, Booth, & Valters, 2017). However, predatory settlements do not necessarily overlap with fragile or conflict-affected states, and a closer look at the spread of the evidence indicates a notable gap here in terms of TWP case studies. Of the 44 programmes that we identified as being the subject of TWP research, only seven are based exclusively in countries that are featured on the World Bank's most recent Harmonised List of Fragile Situations. Given the growing concentration of aid from major donors, including DFID and the World Bank, in fragile and conflict-affected states, a greater emphasis of TWP research efforts in violent and unstable political contexts would seem to be important given the untested nature of these ways of working. In addition, given the argument found in many of the case studies—as in the ESP one above—that effective programmes require politically well-connected staff, there has been surprisingly little analysis about how these staff are recruited, how their activities are assessed or what this may mean in practice in politically divided societies.

2.2. Sector

The conditions for successful programme implementation are also likely to vary according to the sector in question. This is because different sectors have specific characteristics that determine their political salience, the incentives for politicians to deliver them, the main actors and interests surrounding them and the ways that citizens can mobilise around them. In particular, the extent to which a particular sector or service is targetable, 'visible', measurable and easily credited affects the like-
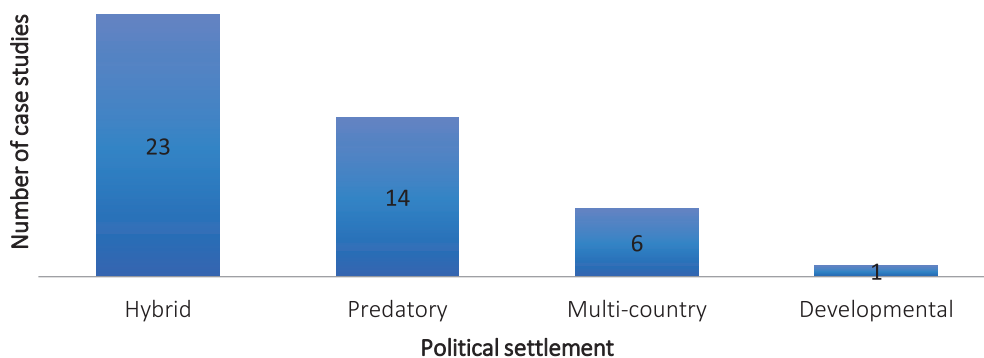

Figure 1. Case studies grouped according to political settlement type.

lihood that states will be responsive to efforts to reform it (Batley & Mcloughlin, 2015). For example, a state may have strong incentives for inclusive provision where a particular service or good has historically been a key source of state legitimacy and an expression of the social contract. Therefore, it seems reasonable to expect that programmes successfully designed and implemented with a close consideration of political dynamics would need to be aware of and responsive to the political characteristics of the sector in question.

TWP is associated closely with the governance sector, to the extent that some authors suggest that it might be trapped in a 'governance-ghetto' (Green, 2017; see also Yanguas & Hulme, 2015). Our analysis of the evidence confirms that governance is the most heavily studied sector in the TWP field by a considerable measure (see Figure 2). One example is the Department of Foreign Affairs and Trade (DFAT)-funded Governance for Growth (GfG) programme, which has been supporting economic governance and public financial management (PFM) reforms in Vanuatu for the past decade. The programme has run over two phases at a cost of around AUD90 million over the first nine years. GfG has been able to support reforms in several different areas. Flagship changes such as the liberalisation of the telecommunications industry have been accompanied by important reforms in areas such as wharf management, fiscal decentralisation, school capitation grants and taxation.

According to a review by Hadley and Tilley (2017), GfG has been able to support these reforms by working politically, a core part of which has involved building close relationships with senior and mid-level bureaucrats in government. Many features of GfG aim to encourage close working partnerships between the GfG team and their counterparts in Vanuatu, with an office co-located in the Office of the Prime Minister. In Vanuatu, there are factors which are said to make this aspect of the initia-

tive especially important. Individuals' relationships are shaped by local hierarchies and ties to family and place. Public institutions are often dominated by a particular island or church group with shared values, while status and kinship ties overlap with politics and public administration. This makes informal systems extremely important in the flow of knowledge, information and decisions (Cox et al., 2007). Building trust across groups and bridging these formal and informal systems is therefore believed to be central to supporting change in Vanuatu's public sector (Hadley & Tilley, 2017).

In terms of the overall evidence base, there are individual studies that are spread across a very wide range of other sectors: justice and security, conflict resolution, infrastructure, gender equality, reform coalitions, PFM, investment, health, community policing, rural livelihoods, economic development, legislative reform, private sector development, state capacity, human development, water, human resources, knowledge sector, solid waste management, forestry, agriculture, and service delivery. This supports the argument made by Rocha Menocal (2014) that TWP is not simply a governance solution to be applied to a narrow set of institutional issues, because incentives and power dynamics lie at the centre of most development challenges. TWP suggests principles for improving the delivery of any aid programme that involves reform and behavioural change; therefore, it should be as relevant in principle to the better delivery of health services or economic policy reform as it is to human development or water services.

While the growing breadth of individual TWP studies across a wide range of sectors is encouraging, our review found that governance, security and justice and infrastructure are the only sectors which have been the subject of three or more case studies. In addition, with few exceptions, the programmes that have been written up into case studies are all *reform* programmes. It
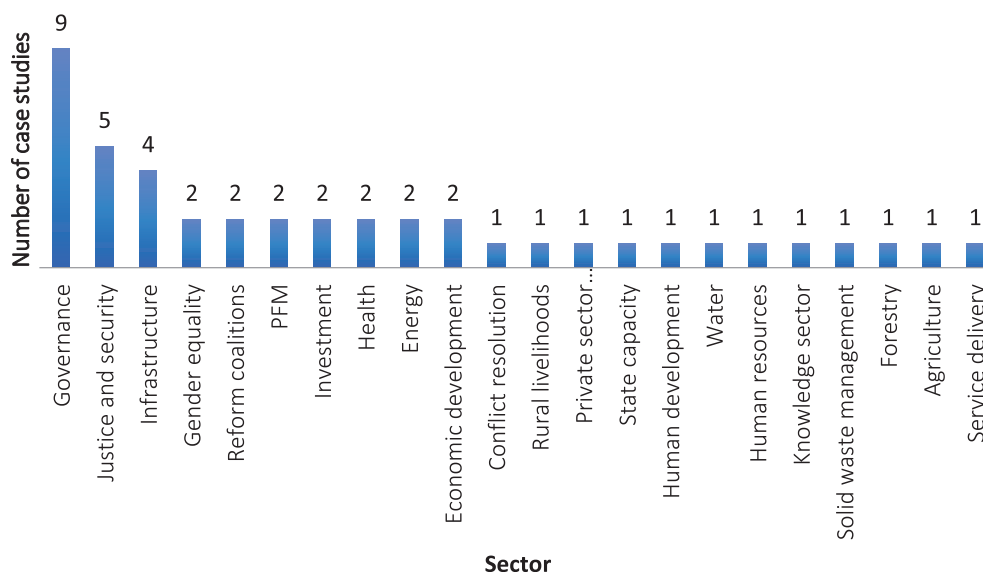

Figure 2. Case studies grouped according to sector.

may be that the conclusion we can draw from the evidence base is that TWP might look similar, in terms of programme design, for reform programmes, regardless of sector; whether or not that is useful for someone trying to design an infrastructure programme, or a service delivery one, is not clear. It is therefore not possible to draw robust conclusions about how development programmes can think and work politically in an effective way in a particular sector without a deeper and stronger evidence base to draw upon. This would require a larger body of studies that look at a number of different programming approaches in the same sector, as well as studies looking at similar kinds of programmes in different sectors (Dasandi et al., 2016).

2.3. Organisation

The third level of analysis focuses on the organisations involved in the design and implementation of TWP programmes. This includes external actors (the bilateral or multilateral donors or international NGOs [INGOs] which are usually responsible for funding and programme design) and domestic partners (the government agencies and local NGOs which are typically responsible for programme implementation and aspects of design) (Dasandi et al., 2016, p. 11).

Certain kinds of organisational characteristics are claimed by the literature to be closely associated with successful TWP. For example, the TWP literature calls for organisations—and individuals within these—that can solve problems and search for workable solutions through iterative learning, can broker relationships with key stakeholders in a specific programme area and are prepared to experiment with flexible and strategic funding modalities (Booth & Unsworth, 2014; Dasandi et al., 2016). Ideally, it is argued that organisations need to have processes in place that encourage this kind of experimentation, innovation and learning, along with a bureaucratic and managerial culture that supports staff in operating along these lines (Bain, Booth, & Wild, 2016, p. 35).

For example, in 2012 DFAT and the Asia Foundation (TAF) began work under a strategic partnership agreement which included a range of reform initiatives that aimed to work politically in practice. In Bangladesh, the team worked with local partners to support efforts to move leather tanneries out of a dangerously polluted location to a modern industrial park. The goal was to improve compliance with health and environmental protection standards and potentially lead to growth in the sector. A reform coalition supported by TAF is said to have contributed significantly to expediting the relocation process, with figures issued in 2015 indicating that of the 155 tanneries allocated plots at the new estate, 148 had begun substantive construction.

As detailed in an ODI case study (Harris, 2016), this initiative used structured learning to iterate and adapt over the course of implementation. This involved both regular reflective discussions as part of an approach called 'strategy testing' (Ladner, 2015), along with day-to-day, ad-hoc adjustments. Strategy testing offered opportunities for discussion within the team across all levels of seniority, and prompted staff to regularly consider how changes in the reform context might affect their strategy. The team reported that the strategy testing sessions provided an opportunity for them to take stock of recent events and actions. It also offered an opportunity to update documentation to reflect changes in the program and thereby provide a record of decision-making for donor accountability purposes (Harris, 2016). Micro-adjustments were also made on an on-going basis through problem solving and informally reflecting on tactics, which was encouraged by the initiative's culture.

The literature on TWP programmes focuses primarily on the role of bilateral and multilateral donors (see Figure 3). For the most part, the agencies examined are DFID, DFAT and the World Bank. Given that these donors fund a significant amount of the research that constitutes the TWP literature, this bias is not surprising, but strengthening the evidence for TWP will require researchers to look at a wider range of organisations and agents engaged in programming (Dasandi et al., 2016). There is a lack of research looking at the demands that TWP places on the internal systems, capabilities and incentive structures of the organisations implementing programmes on the ground—whether domestic or international NGOs, commercial service providers or domestic government agencies. The small number of documented cases that do focus on the experience of the implementing organisation mostly center on one INGO, the TAF, some of which were produced in collaboration with ODI, including the Bangladesh study discussed above (Denney, 2016; Faustino & Booth, 2014; Harris, 2016; Valters, 2016). Excluding one report looking at the work of Peace Direct and Centre Résolution Conflicts in the DRC (Gillhespy & Hayman, 2011), there appear to be no TWP studies that focus on cases where an INGO is the external funding organisation and a local NGO is the implementing partner. Finally, there are no cases at all in the sampled literature that look specifically at the experience of domestic government agencies in implementing TWP programmes with external donor support.

A more detailed insight into the internal processes involved in TWP in donor agencies is put forward in a collection of essays in Fritz, Levy and Ort (2014), which looks at the implementation of PEA in eight World Bank country programmes. These studies demonstrate how the findings and recommendations from political analysis were taken on board by different programmes and used in operational practice. As such, they provide an insight into some of the micro-level processes involved in TWP within the donor organisation and country teams. However, these studies are weaker on demonstrating how the implementation of the insights from PEA led to better outcomes or more successful programming decisions.
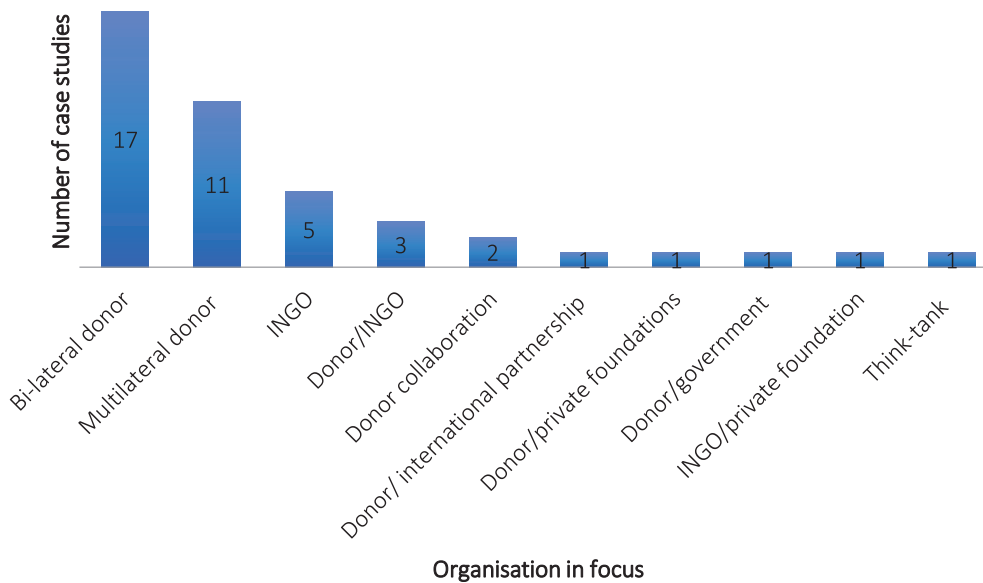
Figure 3. Case studies grouped according to organization.

3. Is There a 'Good Enough' Evidence Base on TWP and Aid Effectiveness?

Leading on from this, in addition to the content gaps that have been noted above, there are also important methodological limitations in the literature. These gaps and limitations mean that, while there are certainly interesting and engaging case studies, they do not constitute the kind of 'rigorous-enough' evidence base that is needed to support more ambitious causal and predictive claims about the role of TWP in improving aid effectiveness and securing better development outcomes, including in fragile environments.

With a few exceptions the case studies reviewed fall short of the high standards on transparency, validity, reliability and cogency that one would expect in a strong evidence base (DFID, 2014). The literature continues to be almost entirely made up of single programme case studies, with few attempts at comparison, and written for the most part by programme insiders. There have been recent improvements in terms of transparency on methods, most notably Denney (2016), Denney and McLaren (2016), Hadley and Tilley (2017), Harris (2016) and Lucia, Buckley, Marquette and McCulloch (2017, in press). However, even these rely largely on interviews and documentary analysis, or a form of action research, rather than methods more appropriate for establishing causal explanations. Moreover, approaches to triangulation are often unclear or entirely absent. Subsequently, in the case studies reviewed, it is often hard to discern a direct causal relationship between TWP and the outcomes that were said to have been achieved.

Only one study in our sample (Booth, 2014) considers counterfactuals and very few discuss challenges faced in the programmes or areas that were unsuccessful (no-

table exceptions include Denney and Maclaren (2016), Hadley and Tilley (2017) and Lucia et al. (2017, in press). A more balanced approach would look to highlight areas where TWP has failed to achieve positive results or to achieve the results that were intended. The fact that this is uncommon in the case studies reviewed may be, at least in part, because many TWP case studies have been written up either by funders themselves or by other actors who have been involved in evaluating the programme as part of its implementation.[8]

This also means that there are limitations of the existing literature in terms of its theory-building rather than theory-testing potential. As we have discussed, the TWP literature identifies several factors that are seen as improving the effectiveness of politically-informed programmes, such as programme managers allowing local actors to take the lead and programme staff brokering relationships with major interest groups. However, beyond fairly broad discussions, there is a lack of in-depth analysis of how, and importantly when, these factors lead to improved outcomes. For example, programme staff brokering relationships with major interest groups by itself will not enable programme staff to address opposition to change by these groups or contestation among these different interest groups. As such, there is also a need for more attention to causal mechanisms that connect the factors identified in the literature with increased aid effectiveness. This would be helped by greater engagement with some of the more general literature on the politics of reform processes (e.g. Ascher, 1984; Grindle, 2004). In part, this again would be helped by greater engagement with programmes that have adopted elements of TWP but failed to achieve positive results.

This would seem to be particularly relevant to TWP, which emphasises the need to test theories of change

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[8] This also raises more serious questions about the potential for conflicts of interest to arise in 'insider' driven research, especially where potential commercial benefits exist, something that has not yet been sufficiently addressed.

and adapt projects and programmes in light of some activities failing. Indeed, studies rarely focus on outcomes, instead focusing on the reform and/or programming *process* instead. Few studies discuss their criteria for 'success', including what the relevant metrics used are. All of this raises concerns about quality, which can often be ameliorated by publishing in well regarded, peer reviewed journals as a proxy for quality. However, to the best of our knowledge, only one of the case studies has been published in a peer reviewed journal (Lucia et al., in press). While one might expect a healthy balance between organizational working papers and journal articles in such a practice-oriented area, the lack of journal articles is a concern, especially when combined with the other points raised here.

4. Conclusions

This article has looked at the existing evidence base for TWP with the aim of providing guidance for future research into what works, where and why in terms of TWP programming. In short, we find that while there are certainly interesting and engaging case studies in the literature, these do not yet constitute a 'strong enough' evidence base that proves that TWP has significantly improved aid effectiveness. Since TWP is a relatively recent arrival in the development debate, gaps in the literature are to be expected. Additionally, the primary function of TWP may not in fact be aid effectiveness per se, but rather avoiding the well recorded pitfalls and negative unintended consequences of 'politically blind' aid (Carothers & de Gramont, 2013). But given the rising interest in developing more politically informed, flexible and adaptive programming, and claims that case study authors themselves make about improved effectiveness, this should be an urgent priority for funders. In addition, if one intention is to avoid the well documented consequences of 'politically blind' aid, the potential for unintended consequences to emerge from TWP 'approaches', such as relying on politically well-connected insiders, should be another urgent priority.

We suggest that—if our overall aim is to understand the effect of TWP on aid effectiveness—we need to move beyond descriptions of what are, in effect, programme designs and activities. The analysis here suggests that if we are to determine if TWP leads to greater aid effectiveness, future research should consider more rigorous and structured testing of what works, where, why and how. Ideally, this would happen while this sort of programming is still relatively 'niche' and where it does not yet make up a significant percentage of donor funding. Developing a better understanding of the approaches and strategies that work well in different political, sectoral and organisational contexts will be an important step if TWP, and its variants such as adaptive management, are going to move into more mainstream development programming. Looking at programmes in a broader range of political contexts, including in contexts that are fragile and conflict-affected, where a focus on potential unintended consequences—including from the programming approach used—and on the trade-offs and dilemmas that development organisations face, may be particularly salient in terms of engaging with the political process of development (see Dasandi & Erez, 2017).

By systematically comparing a broader range of programmes in different sectors and organisational contexts, the field may be able to draw firmer lessons about programme implementation and outcomes in different situations, testing some of the common assumptions about what works. This will help to demonstrate whether there are general lessons about when, why, and how different factors identified in TWP literature lead to programme success or failure. This may, in turn, help the field move towards a clearer understanding of the constraints that can hinder more political ways of working and to explore where and how these barriers have occurred in the context of specific strategies, programmes or country offices. Comparative analysis could then be used to test assumptions and draw out lessons about how actors have or have not been able to navigate around them in different contexts.

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Conflict of Interests

The authors declare no conflict of interests.

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Appendix. List of case studies.

Programme	Donor/lead organisation	Country
Australia–Timor-Leste Partnership for Human Development	DFAT	Timor-Leste
Budget Strengthening Initiative	DFID, AusAID, DANIDA, World Bank	South Sudan, Sierra Leone, Liberia, Democratic Republic of Congo and Uganda, the Secretariat of the g7+ based in Timor-Leste
Centre for Inclusive Growth	DFID	Nepal
Coalitions for Change	DFAT-TAF	Philippines
Community Dispute Resolution	TAF	Philippines
Community Dispute Resolution	TAF, Hewlett Foundation & later USAID	Nepal
Community Dispute Resolution	TAF	Sri Lanka
Community Policing	TAF, DFID & BHC	Sri Lanka
Community Policing	TAF	Timor-Leste
Developing Commercical Agriculture	World Bank	Ghana
Disarmament, Demobilisation and Reintegration in DRC	Peace Direct	DRC
Empowerment, Voice and Accountability for Better Health and Nutrition	DFID	Pakistan
Energy Subsidy Reform	World Bank	Morocco
EU Forest Law Enforcement, Governance and Trade Action Plan	EU, DFID	Asia, Africa, Central and South America
Facility for Oil Sector Transparency and Reform	DFID	Nigeria
Governance for Development	DFAT	Timor-Leste
Governance for Growth	DFAT	Indonesia
Governance for Growth in Vanuatu	DFAT	Vanuatu
Health sector quality improvement projects ('basket' case study)	Multiple donors	Ghana, Ethiopia
Infrastructure Reform	World Bank	Sierra Leone
Infrastructure Reform	World Bank	Zambia
Knowledge Sector Initiative	DFAT/Government of Indonesia	Indonesia
Leather Sector Initiative	DFAT, TAF	Bangladesh
Legal Assistance for Economic Reform	DFID	Kenya, Rwanda, Sierra Leone, Somaliland, Uganda, Bangladesh, Burma, Tanzania
Local government development programmes ('basket' case study)	UNCDF	Uganda
Local Infrastructure in Papua New Guinea	World Bank	PNG
Pacific Leadership Program	DFAT	Pacific region with a focus on Samoa, Solomon Islands, Tonga and Vanuatu
Pacific Women Shaping Pacific Development	DFAT	Fiji, Kiribati, Papua New Guinea and Tonga
Papua New Guinea Governance Facility	DFAT	PNG

Programme	Donor/lead organisation	Country
Pay and Attendance Monitoring Programme	DFID, Global Fund	Sierra Leone
Private Sector Development	DFID	DRC
Pyoe Pin	DFID, SIDA, DANIDA	Myanmar
Reforming Solid Waste Management	DFAT/TAF	Cambodia
Rural Water and Accountability Programme	DFID, SNV Netherlands Development Organisation	Tanzania
Shifting Incentives in the Power Sector	World Bank	Dominican Republic
State Accountability and Voice Initiative	DFID	Nigeria
State Partnership for Accountability, Responsiveness and Capability	DFID	Nigeria
Strategic Capacity Building Initiative	UNDP, World Bank	Rwanda
Strategy and Policy Unit	Various private foundations and institutional donors	Sierra Leone
Strengthening Local Service Delivery in the Philippines	World Bank	Philippines
The Enabling State Programme	DFID	Nepal
Voices for Change	DFID	Nigeria
Western Odisha Rural Livelihoods Programme	DFID	India
World Bank Country Assistance Strategy	World Bank	Mongolia

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