Strategy and Volatility in Foreign Aid Allocation by the Council of the European Union

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Abstract

Do member states systematically exploit their position of power in the Presidency to affect EU foreign aid policy and make it reflect their own preferences? If so, what consequences does this have on the volatility of EU aid allocation? I build on Carnegie and Marinov (2017) who show that, on average, aid recipients who are former colonies of the EU member holding the Presidency are allocated greater amounts of EU aid; and then estimate the effect of such exogenous aid increase on democracy and human rights in those recipient countries via an instrumental variable design that leverages the rotating Presidency. However, breaking down the analysis by year, former colonizer, and recipient, I find that EU members might not systematically allocate more aid to their former colonies when in power. I propose a broader conceptualization of donor preferences and find evidence that EU member states holding the Presidency do indeed affect the EU aid budget so as to make it reflect their own allocation preferences. First, these findings inform the interpretation and generalizability of the local average treatment effect (LATE) estimate from the two-stage least squares analysis that leverages the rotating Presidency to instrument for endogenous aid allocation. Secondly, when estimating the second stage based on the revised instrumental variable that takes into account donors' allocation preferences more broadly, I find that the positive and significant effect on human right protections as captured by the CIRI Human Empowerment Index measure persists, but no significant effect on the Polity IV measure of democracy; nor on the Fariss Human Rights Protection Scores. Third, this work speaks to whether an institutional design feature of the EU—i.e., the rotating Presidency of its Council—impacts the volatility of EU policy. I compare the effect of the rotating Presidency on EU aid allocation before and after the 2007 implementation of "trio presidencies," whereby members holding three consecutive presidencies cooperate on a common platform. Presidency holders after 2007 are arguably more restricted in their ability to unilaterally influence the aid budget. I investigate whether this change in institutional design lessened the influence of the member state holding the Presidency on the EU aid budget; and consequently made EU foreign aid policy less volatile. These findings are preliminary and additional analyses will be needed, especially as it pertains to understanding the effect of the 2007 institutional design changes on the relationship between the Presidency and EU aid policy.

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1 Introduction

Development scholars and practitioners alike have long been interested in assessing the effects of foreign aid. Traditionally, this has meant studying aid's effects on economic growth as well as on development more broadly: including the quality of life, health and education of citizens in recipient countries; the type and strength of political institutions and governance; as well as the spread of democratic norms and the protection of human rights. The inherent challenge to these research questions is of course the endogeneity of aid allocation to most of the outcomes we care about. The foreign aid that a country receives is surely related to its own level of economic and institutional development, to its regime type and degree of democracy and freedom, to its respect for international norms and human rights, and to its relations to donors—who have been found to target recipient on both on the basis of need, and on the basis of donor strategic interest (e.g., Alesina and Dollar 2000; Dudley and Montmarquette 1976; Heinrich 2013; Maizels and Nissanke 1984; McKinlay and Little 1977, 1978; Schraeder, Hook, and Taylor 1998).

Carnegie and Marinov (2017) propose a clever strategy to address the challenge that endogenous aid allocation poses to the empirical study of the effectiveness of foreign aid. Focusing on foreign aid from European Union (EU) institutions, they exploit the arguably exogenous rotation of the Presidency of the Council of the EU to isolate the effect of aid on human rights and democracy promotion in recipient countries. Specifically, leveraging the fact that when a former colonizer holds the Presidency it is able to allocate more EU aid to its former colonies, Carnegie and Marinov (2017) employ an instrumental variable analysis to estimate the local average treatment effect of receiving an additional amount of aid. First, they show that being a former colony of the EU member state holding the Presidency results in a small aid increase. Then, they find that such arguably exogenous aid increase has positive, if perhaps short-lived, effects on human rights and democracy outcomes in recipient countries.

In this paper, I pursue three objectives: first, I investigate whether EU members states holding the Presidency indeed affect the EU aid budget to reflect their broader strategic priorities in aid allocation. I initially follow a conceptualization of donor strategic interest limited to the pro-colony bias; and then expand it to reflect the influence of all factors donors take into consideration when allocating aid. In other words, I ask whether EU member states holding the Presidency influence the EU aid budget so to make it look more like their own bilateral aid allocation preferences. Second, I present expectations and preliminary empirical tests for whether the institutional design changes that the Council of the EU underwent starting in 2007 had an impact on the relationship between the Presidency and the EU aid budget. Third, I argue that the findings about the degree to which strategic aid allocation behavior on the part of the EU member holding the Presidency is systematic and widespread should inform the interpretation and generalizability of the

local average treatment effect (LATE) estimate that comes from the two-stage least squares analysis that leverages such phenomenon to instrument for endogenous aid allocation.

I find little evidence to support the idea that the average increase in EU aid following the Presidency of recipients' former colonizer is the result of systematic behavior. Specifically, I show that (i) not all former colonizers allocate more aid to their former colonies when holding the Presidency; and (ii) not all former colonies experience an aid increase when their former colonizer holds the Presidency. However, I do find support for the hypothesis that EU members holding the Presidency affect the EU aid budget to make it reflect their own allocation preferences more broadly.

I further investigate whether the institutional design changes that the EU—and in particular the Presidency of the Council—underwent starting in 2007 affected the relationship between the Presidency and the EU aid budget. Such changes, formalized in the 2009 Treaty of Lisbon, arguably reduced the importance of the Presidency. I therefore expect that the effect of the member holding the Presidency on EU aid allocation would decrease or disappear; and that EU aid disbursements would become less unpredictable as a result. While I have yet to thoroughly test these expectations empirically, preliminary analyses do not show evidence consistent with these expectations.

In the two-stage least squares analysis to estimate the effect of the exogenous increase in EU aid that former colonies receive following their former colonizer's Presidency on their levels of democracy and respect for human rights, I confirm the positive effect on human rights measured with the CIRI Human Empowerment Index. However, I do not find evidence of significant improvements in human rights as measured with the Fariss Human Rights Protection Scores; nor on democracy measured with the Polity IV combined scores. The interpretation and generalizability of this LATE estimate is also questioned by the fact that the first-stage effect this instrumentation relies on is shown not to be reflecting a systematic, widespread phenomenon.

Lastly, I estimate the second stage on the basis of a broadened instrumental variable that takes into account donors' allocation preferences more broadly, to determine the effect of the exogenous change in EU aid that recipient receive as a consequence of the Presidency holder's own allocation preferences. I find that the positive and significant effect on human right protections as captured by the CIRI measure persists, but no significant effect on the Polity IV measure of democracy; not on the Fariss measure of human rights protections (with coefficients often going in the negative direction).

Taken together, these analyses allow us to learn two things: first, substantively, that EU member states do not seem to be systematically exploiting the Presidency of the Council to redirect more aid toward their former colonies; yet they do influence the EU aid budget so to make it reflect their broader aid-giving preferences. Second, this limits the generalizability—not the internal validity!—of a second-stage LATE estimate obtained when instrumenting via the colonial-relationship variable. When instead we instrument

via the broader allocation preferences variable, evidence of a meaningful impact of the exogenous change in EU aid on measures of democracy and human rights become more tenuous.

2 Foreign Aid Effectiveness

While foreign aid typically consists of less than one percent of a donor country's gross national income, it adds up to a considerable pot of money: the amount of Official Development Assistance (ODA) disbursed by OECD countries every year is fast approaching two hundred billion (OECD 2015). Thus, it is no surprise that development scholars have been particularly interested in the question of aid effectiveness: *does foreign aid work?* Traditionally, this has meant studying aid's effects on economic growth, as well as development more broadly—including the quality of life, health, and education of citizens in recipient countries, the type and quality of political institutions, the spread of democratic norms, and the protection of human rights.

The majority of the efforts to study aid effectiveness take the form of cross-country panel-data analyses of the impact of aid on country-level indicators of growth and development. However, the results of these studies have been historically widely divergent, finding (i) that aid positively influences growth on average across all countries, with diminishing returns as its amount increases (Arndt, Jones, and Tarp 2010; Clemens et al. 2012; Galiani et al. 2014; Levy 1988; Mekashaa and Tarp 2013; Over 1975; Papenek 1973; Sachs et al. 2004); (ii) that aid has a conditional effect on growth—it *can* positively affect growth, but only under specific circumstances (Burnside and Dollar 2000; Isham, Narayan, and Pritchett 1995; Wright 2008); (iii) that aid either has no effect on growth, or that it might actually undermine it (Bauer 1972; Boone 1994, 1996; Dowling and Hiemenz 1982; Easterly 2001; Griffin and Enos 1970; Mosley 1980; Mosley, Hudson, and Horrell 1987; Rajan and Subramainan 2008; Singh 1985). While an early review of macro studies of aid's effect on growth found little evidence in support of a positive effect (Mosley 1987), a subsequent review (Doucouliagos and Paldam 2008) suggested that there is a small positive effect, but not a statistically significant one. More recently, though, a review of the post-2008 macro literature (Arndt, Jones, and Tarp 2016) concludes that a consensus seems to be emerging:

> "The large majority of up-to-date empirical studies in the economics literature have found positive impacts. More precisely, the full range of independent studies published since 2008 based on cross-country growth regressions report comparable results for the marginal effect of aid on growth. These studies suggest that receipt of foreign aid equal to 2.5 percent of GDP over a sustained period is expected to boost growth by approximately 0.25 percentage points on average" (446).

The literature has also investigated the (more or less intended) consequences of aid on other outcomes, such as regime type and regime transition, institutions and governance, respect for human rights. For example, Svensson (2000) found that the mere expectation of aid cause a reduction in public goods provisions in

the recipient country; Knack (2001) found that high levels of aid erode the quality of governance; yet Tavares (2003) reports that aid decreases corruption; Finkel, Perez-Linan, and Seligson (2007) showed that aid given in the context of democracy promotion programs has a positive impact on democratization; Djankov, Montalvo, and Reynal-Querol (2008) investigated the "curse of aid" and found it to have an even worse impact on institutions than oil does; Busse and Gröning (2009) also find a direct negative effect of aid on institutions; Bueno de Mesquita and Smith (2009) proposed that aid transfers improve the survival of political leaders in both donor and recipient country, while leaving the average citizens in recipient states worse off; Morrison (2009) showed that greater nontax revenue—including aid—has a stabilizing effect on all regime types; while Wright (2009) showed that the likelihood of aid positively affecting democratization is conditional on the size of the regime's distributional coalition.

It is not uncommon for foreign aid donors to be explicit about their intention to use foreign aid as a tool to foster democratization and human rights protections in recipient countries, sometimes conditioning their allocations on a recipient's respect for human rights and level of democracy (Dunning 2004). However, many of the theoretical mechanisms in the aid effectiveness literature predict *negative* effects of aid on institutions. For example, some scholars have focused on the fiscal contract between governments and citizens: when foreign aid makes up a sizable amount of a government's budget, the government is less dependent on taxation, and may thus become less accountable to its citizens and less interested in nurturing effective and representative institutions (Booth 2011; Moss, Pettersson, and Van de Walle 2006). Alternatively, it is possible that aid could erode the government-citizen relationships starting from the side of the citizens: citizens whose needs are satisfied by aid-funded programs and interventions (e.g., healthcare, education, subsidies for farmers) might become less adamant in urging that those public goods be provided by the government, and make fewer demands on how taxpayer money is spent.

Given the lack of consensus in the in the foreign aid literature on the effects of aid on institutions in recipient countries, and the number of mechanisms predicting negative effects, the question of whether aid can foster human rights and democracy in recipient countries is still extremely relevant today.

3 A Two-Stage Least Squares Instrumental Variable Approach

While it is common for aid effectiveness studies to rely on regression analysis with control variables, Carnegie and Marinov (2017) address the endogeneity of aid allocation with a two-stage least squares instrumental variable approach, an identification strategy that allows them to isolate the causal effect of foreign aid on democracy and human rights in recipient countries. This is an important contribution, as aid allocation often reacts to recipients' levels of democracy and human rights and can be even used to reward or punish

them on the basis of their records of human rights protections and democratic practices (e.g., Schoultz 1981; Cingranelli and Pasquarello 1985; Poe 1992; Trumbull and Wall 1994; Berthélemy 2006; Nielsen 2013).

Focusing on foreign aid from EU institutions, Carnegie and Marinov (2017) propose to leverage the fact that, when EU member states take their turn holding the Presidency of the Council of the EU, they have the ability to direct more aid to their former colonies. Countries who are former colonies of a EU member state might thus receive an increase in EU aid when their former colonizer happens to hold the Presidency. If such aid increase depends only on the as-if random rotation of the Presidency and is indeed exogenous, we can study its effects on democracy and human rights in recipient countries without worrying about other possible confounding variables. To summarize Carnegie and Marinov's (2017) approach,[1] the goal of the analysis is to estimate, for each recipient country i, the effect of aid allocation in year $t-1$ on a measure of democracy and human rights DHR in year t:

$$DHR_{it} = \beta_0 + \beta_1 log(Aid)_{i(t-1)} + \sum_{k \in K} \beta_k I(i = k) + \sum_{j \in J} \beta_j I(t = j) + \epsilon_{it}$$

Where the two summations represent country and year fixed effects, respectively, and ϵ_{it} is the unobserved error term. However, aid allocation is not only not random, but possibly a function of the recipient country's human rights and democracy records—even conditional on the country and year fixed effects. In other words, the error term ϵ_{it} in the equation above is likely to contain variables that are correlated with the main explanatory variable, $log(Aid)_{i(t-1)}$. This violates the one of the key assumption in OLS, the independence of regressors from the disturbance term (i.e., $cor(log(Aid), \epsilon) = 0$), and leads to biased estimates. It follows that the effect estimate β_1 from the equation above is likely biased. As discussed, Carnegie and Marinov (2017) propose to solve this problem by removing the selection bias through a two-equation instrumental variable model, exploiting the as-if random nature of the rotating Presidency of the Council of the EU.

The Council of the European Union[2] serves as one half of the bicameral legislative branch of the Union. It is meant to represent the executive governments of the EU member states, and meets in different configurations of 28 national ministers (one per state) depending on the matter under consideration. This is the body that concludes international agreements, coordinates the broad economic and social policy, and sets out guidelines for the Common Foreign and Security Policy (CFSP). It also shares the budgetary power with the other half of the EU's bicameral legislature, the European Parliament (Hix and Høyland 2011).

The Presidency of the Council is not a position held by an individual, rather by the national government

[1] The remainder of this section summarizes Carnegie and Marinov (2017) and draws heavily it.

[2] The Council of the European Union should not be confused with the European Council, a EU institution composed of heads of state or government of all members, the President of the European Council, and President of the European Commission; or with the Council of Europe, an international organization completely separate from the EU, and devoted to the promotion of human rights, democracy, and the rule of law in the continent.

of one of the EU member states. Specifically, the Presidency rotates among the member states of the EU every six months—a country holding the Presidency from January to June, another from July to December. The order of rotation is established ahead of time in EU treaties or official decisions.[3]

The EU member state who holds the Presidency during the Council's budget-making process (July to December) is able to allocate more EU foreign aid to its former colonies.[4] This is a reasonable expectation given that the literature on foreign aid allocation has found both (i) that former colonizers tend to give more aid to their former colonies (Alesina and Dollar 2000; Holland 2002), and (ii) that member states of intergovernmental organizations are able to use their power to direct more of the organization's aid toward those recipients whom they favor (Thacker 1999; Dreher, Sturm, and Vreeland 2009).

If the rotation of the Presidency is as-if random, then (i) restricting the analysis to recipients who are former colonies of a EU member, and (ii) employing year fixed effects to account for the enlargement of the EU, ensures that the set of recipients who are, at a given time, former colonies of the country holding the Presidency is also as-if random. In other words, any time a former colonizer holds the Presidency, an as-if random group of recipients is allocated a little extra foreign aid from EU institutions. This analysis estimates the local average treatment effect (LATE) of such exogenous increase in aid on human rights protections and democracy levels in the countries who receive it.

The two-stage least square approach consists in, first, using the as-if random instrument to predict the endogenous explanatory variable—in this case, using a recipient's colonial status relative to the EU member holding the Presidency to predict the amount of aid that such recipient is allocated:

$$log(Aid)_{i(t-1)} = \gamma_0 + \gamma_1 Colony_{i(t-2)_{Jul-Dec}} + \sum_{k \in K} \gamma_k I(i = k) + \sum_{j \in J} \gamma_j I(t = j) + u_{i(t-1)}$$

Where the instrumental variable $Colony_{i(t-2)_{Jul-Dec}}$ is an indicator equal to one if recipient i is a former colony of the state holding the presidency in the July-December period of year $t-2$, and zero otherwise. Again, the two summations represent country and year fixed effects, respectively, and u_{it} is the unobserved

[3]Starting with the first Presidency held by Belgium in 1958, the original six member states rotated in alphabetical order, according to country names as spelled in their own languages (e.g., Germany as Deutschland, Greece as Elláda, etc.). After five fully-completed rounds, Denmark, Ireland, and the United Kingdom were added onto the rotation following their accession to the Union in 1973, still following alphabetical order; the same happened following the accession of Greece in 1981, and of Spain and Portugal in 1986. The first change in the rotating principle for the Presidency happened in 1993, after ten fully-completed rounds, when it began alternating between ascending and descending alphabetical order—so to avoid that a country might hold the Presidency twice in the same half of the year. A second revision was adopted following the joining of Austria, Finland, and Sweden in 1995, when a system of "balanced rotation" was instituted to ensure that at least one out of every three presidencies was held by a large state. Lastly, starting in 2007 and formally laid down by the 2009 Treaty of Lisbon, an effort to foster coordination between each successive six-month Presidency resulted in the so-called "troika" or "presidency trio," whereby groups of three successive presidencies are supposed to cooperate on a common political program.

[4]The budget originates in the Parliament but, in the second half of the year, is passed on to the Council, who has the last word on compulsory expenditures—including aid. The country holding the Presidency during that period is able to influence aid allocation by virtue of scheduling and controlling budgetary meetings, using—or threatening to use—its budgetary veto power, and shaping the agenda for the Council (Hix and Høyland 2011). During the following year, the aid so allocated is then disbursed by the European Commission.

error term—uncorrelated with the instrument. Then, in the second stage, the fitted values obtained in the first stage (i.e., the predicted amount of aid recipient i would receive) are used to predict the outcome of interest, democracy and human rights, DHR:

$$DHR_{it} = \beta_0 + \beta_1 log(\widehat{Aid})_{i(t-1)} + \sum_{k \in K} \beta_k I(i = k) + \sum_{j \in J} \beta_j I(t = j) + \epsilon_{it}$$

Where $log(\widehat{Aid})_{i(t-1)}$ are the fitted values obtained in the first stage, the two summations represent country and year fixed effects, and ϵ_{it} is the unobserved error term. If the instrument is strong and valid, this approach forces the independence of regressors from the disturbance term in the second-stage equation ($cor(\widehat{log(Aid)}, \epsilon) = 0$), thus eliminating the bias and producing unbiased estimates.

A strong and valid instrument (i) is random or as-if random, (ii) predicts the endogenous explanatory variable, and (iii) is not related to the outcome in any way other than through the endogenous explanatory variable (e.g., Angrist, Imbens, and Rubin 1996; Angrist and Pischke 2008; Sovey and Green 2009). In this case, the first condition—known as the *independence assumption*—is met in that a recipient's colonial status relative to the EU member holding the Presidency is determined through the as-if random rotation of the Presidency. The second condition can be tested for empirically in the first stage, where recipient colonial status should be shown to be able to positively and significantly predict aid allocation from EU institutions. Lastly, the third condition—the so-called *exclusion restriction*—hinges on the fact that the Presidency of the Council does not give its holder much room to influence human rights and democracy in recipient countries (and in particular, in its former colonies) in ways other than through the allocation of foreign aid. In fact, the influence of the Presidency in areas other than the foreign aid budget is fairly limited.

With data on net aid from EU institutions (endogenous explanatory variable), a binary variable indicating whether the recipient country is a former colony of the EU member holding the Presidency at the time of budgeting (instrumental variable), and average measures of democracy and human rights in the four years following aid disbursement (outcome variables), Carnegie and Marinov (2017) perform the analysis described above for EU aid budgeted by twenty Presidencies, from 1986 to 2005. Table 1 lists the Presidency rotations for the second semester (i.e., the budgetary period) in those twenty years, along with the number of former colonies of each EU member state holding the presidency.[5] Democracy levels are measured with the Polity IV combined scores;[6] and respect for human rights is measured the Cingranelli-Richards (CIRI) Human

[5]Note that Vanuatu is a former colony of both the UK and France, so it is coded as 0.5 both when the UK holds the Presidency and when France does.

[6]The Polity IV combined score ranges from +10 (strongly democratic) to -10 (strongly autocratic). The combined score is obtained by subtracting a scale constructed by codings of the competitiveness of political participation, the regulation of participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive to another scale derived from codings of the competitiveness of political participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive (Marshall, Gurr, Jaggers 2015). The 2015 version of the Polity IV data set

Empowerment Index.[7] Both of these outcome variables are averaged over four years, from t to $t+3$. Lastly, Carnegie and Marinov (2017) estimates their models both with and without covariates measuring recipient-country average years of education, exports, imports, FDI, religiosity, oil revenues, democracies in the region, GDP, and GDP per capita. Figure 1 is a copy of Carnegie and Marinov's results (2017).

Table 1: Presidency Rotation for the Twenty-Year Period of Interest

$\text{Year}_{(t-2)}$	Second Semester Presidency	Number of Former Colonies
1986	United Kingdom	56.5
1987	Denmark	-
1988	Greece	-
1989	France	26.5
1990	Italy	2
1991	Netherlands	2
1992	United Kingdom	56.5
1993	Belgium	3
1994	Germany	-
1995	Spain	19
1996	Ireland	-
1997	Luxembourg	-
1998	Austria	-
1999	Finland	-
2000	France	26.5
2001	Belgium	3
2002	Denmark	-
2003	Italy	2
2004	Netherlands	2
2005	United Kingdom	56.5

Figure 1: Result Table from Carnegie and Marinov (2017, 679)

TABLE 1 Two-Stage Least Squares Estimates of Effects of Logged Foreign Aid (in Year $t-1$) from the European Community on Dependent Variables Averaged over Years t through $t+3$

Dependent Variable (4-Year Average)	CIRI Human Empowerment Index		Polity IV Combined Score	
Effect of Aid	1.885	1.705	2.031	1.337
(Standard Error)	(0.946)	(0.811)	(0.708)	(0.500)
Countries	115	115	95	95
Years	20	20	20	20
Covariates	No	Yes	No	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Country Fixed Effects	Yes	Yes	Yes	Yes
N	1,792	1,792	1,818	1,818

Note: In column 2, the following covariates are not shown: Average Years Education, Log Exports, FDI, Log Imports, Religiosity, Petroleum Revenues, Democracies in Region, Log GDP, and Log GDP per Capita. Dummies indicating missing values are also not shown. Fixed effects held for country and year. Robust standard errors (accounting for multi-way clustering at the levels of country and year) are shown in parentheses. First-stage coefficient on $Colony_{i(t-2)2}$ for CIRI regression is 0.160 (SE = 0.049, p = .004, F = 10.85). First-stage coefficient on $Colony_{i(t-2)2}$ for Polity IV Combined Score regression is 0.170 (SE = 0.054, p = .005, F = 9.87).

covers all independent countries with total population greater than 500,000, from 1800 to 2015.

[7] This is an additive index constructed from measures of government respect for foreign movement, domestic movement, freedom of speech, freedom of assembly and association, workers' rights, electoral self-determination, and freedom of religion. It ranges from 0, indicating no government respect for these seven rights, to 14, full government respect for these seven rights (Cingranelli, Richards, Clay 2014). The most recent version of this data set covers only three decades, from 1981 to 2011.

4 First-Stage Analysis

Following Carnegie and Marinov (2017), I expect that EU member states holding the Presidency of the Council during the budgetary period will influence the EU aid budget and direct more aid toward their former colonies. Replicating their analysis,[8] as reported in Table 2, I confirm that over the 1987–2006 time period[9] a recipient's colonial status relative to the EU member holding the Presidency during the budgetary period (July-December) in year $t-2$ predicts the amount of aid that it receives during the following year, $t-1$. I find this positive and statistically significant increase in aid even when I expand the analysis to include Presidencies from 1960 to 2005.

Table 2: First-Stage Results

	$\boldsymbol{Aid}_{(t-1)}$			
	1987–2006		**1961–2006**	
	(1)	(2)	(3)	(4)
$\boldsymbol{Colony}_{i(t-2)_{Jul-Dec}}$	**0.15***	**0.12***	**0.23***	**0.23***
	(0.059)	(0.058)	(0.057)	(0.056)
Covariates	No	Yes	No	Yes
N	2,505	2,505	4,027	4,027
R^2	0.0026	0.038	0.0044	0.038

** p-value < 0.05. Standard errors in parenthesis.*
All models include recipient and year fixed effects.

As a placebo test, I perform the same first-stage analysis, this time using a binary variable indicating whether the recipient country is a former colony of the EU member holding the Presidency in the first semester, rather than the second. Since the first-semester Presidency would not have the ability to influence the EU aid budget, I do not expect to find an effect in this case. In fact, as reported in Table 3, I do not.

Table 3: First-Stage Placebo Results

	$\boldsymbol{Aid}_{(t-1)}$			
	1987–2006		**1961–2006**	
	(5)	(6)	(7)	(8)
$\boldsymbol{Colony}_{i(t-2)_{Jan-Jul}}$	-0.011	-0.019	-0.085	-0.077
	(0.072)	(0.071)	(0.058)	(0.058)
Covariates	No	Yes	No	Yes
N	2,505	2,505	4,027	4,027
R^2	0.000010	0.036	0.00054	0.034

** p-value < 0.05. Standard errors in parenthesis.*
All models include recipient and year fixed effects.

[8] For the analyses presented in this paper, I use Carnegie and Marinov's (2017) original data set as downloaded from their replication materials. I have previously performed the same analyses on a data set I put together myself, finding analogous results. Note also that I perform all analyses in R, without using a pre-programmed two-stage least squares function; while according to their replication materials, Carnegie and Marinov (2017) use the `ivreg2` function in STATA.

[9] EU aid budgeted in years 1986–2005.

The replication of Carnegie and Marinov's (2017) first-stage analysis confirms that, when a recipient country's former colonizer holds the Presidency, that country receives a bump in aid—on average. This effect is estimated with models including both recipient and year fixed effects, and holds to the inclusion of nine covariates accounting for a recipient's development over time.[10] What I listed as the second condition for a strong and valid instrument—i.e., the instrument must predict the endogenous explanatory variable—is met: the colonial history of a recipient in relation to the EU member holding the Presidency is a good predictor of EU aid disbursed in the following year. Thus, the EU aid amounts predicted through these first-stage models can be used in the second stage to estimate the local average treatment effect of EU aid on democracy and human rights in recipient countries.

However, it is important to investigate just how "local" such estimated local average treatment effect (LATE) really is. What results from the complete two-stage least squares analysis—i.e., the positive and significant effect of EU aid on democracy and human rights as found by Carnegie and Marinov (2017) and reported in Figure 1—can be described as the effect of the exogenous bump in aid that certain recipients get only by virtue of being a former colony of the EU member state who happened to hold the Presidency during the budgetary period. But do all recipients who are former colonies experience this increase in EU aid when their former colonizer calls the shots? And do all EU members with former colonies exploit the Presidency of the Council to direct more EU aid toward them, every time they hold the Presidency role? If the first-stage results are indeed the product of a systematic phenomenon that characterizes most Presidencies and affects most recipients, then the LATE is quite generalizable and informs us about the effects of EU aid on democracy and human rights in a variety of contexts. On the other hand, if the first-stage results are the product of a few influential observations, then the LATE is limited to describing those particular contexts.

To address this possibility and determine whether the average increase in aid found in the first-stage analysis is representative of the whole sample—or whether it is driven by the behavior of a particular set of EU members, toward a particular set of recipients—I break down the first-stage analysis (i) by year, (ii) by former colonizer, and (iii) by recipient.

4.1 First-Stage Analysis by Year

As Table 1 illustrates, six former colonizers held the Presidency in our time period of interest (aid allocated from 1986 to 2005): the United Kingdom did so three times, four others did so twice (Belgium, France, Italy, the Netherlands), and Spain held it once. I re-estimate first-stage analyses in each of those eleven years, to assess whether all Presidencies direct more aid toward their former colonies, or whether this behavior is

[10] As discussed, these nine covariates are recipient-country average years of education, exports, imports, FDI, religiosity, oil revenues, democracies in the region, GDP, and GDP per capita.

typical of only some of them. In these analyses, I compare the amount of aid received by countries who are former colonies of the member state who held the Presidency to the amount of aid received by countries who are not former colonies of that state, in the same year. In other words, these are random-effect models that estimate their coefficients off of comparisons across recipient countries, within the same year.

Table 4 reports the results of these analyses. With few exceptions, both controlling for the same usual covariates and not, I find no support for the hypothesis that former colonies of the member state holding the Presidency *systematically* receive significantly different amounts of aid than do recipients who are not former colonies of the country holding the Presidency.

Table 4: First-Stage Results by Year

Aid Budgeted in:	***British Years***						***Spanish Year***	
	1986		**1992**		**2005**		**1995**	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Colony	-0.40	NA	**-0.62***	0.032	**-0.78***	-0.011	-0.0033	-0.29
	(0.24)	NA	(0.25)	(0.22)	(0.28)	(0.24)	(0.35)	(0.30)
Intercept	2.05*	NA	2.67*	-3.05	3.04*	0.95	2.55*	-10.10*
	(0.17)	NA	(0.18)	(2.03)	(0.20)	(3.27)	(0.14)	(3.49)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.024	NA	0.050	0.58	0.063	0.63	7.64e-07	0.58

Aid Budgeted in:	***Dutch Years***				***Italian Years***			
	1991		**2004**		**1990**		**2003**	
	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Colony	-0.15	-0.43	0.32	-0.077	-0.38	1.00	-0.89	-0.48
	(1.04)	(0.65)	(1.10)	(0.65)	(1.03)	(1.01)	(1.09)	(0.86)
Intercept	2.42*	1.34	2.57*	-0.38	2.24*	-0.78	2.59*	0.15
	(0.14)	(3.07)	(0.14)	(2.02)	(0.14)	(2.70)	(0.14)	(1.81)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.00017	0.67	0.00077	0.71	0.0012	0.60	0.0059	0.67

Aid Budgeted in:	***French Years***				***Belgian Years***			
	1989		**2000**		**1993**		**2001**	
	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)
Colony	**1.09***	0.30	**0.94***	0.28	0.79	-0.32	**1.53**†	0.28
	(0.30)	(0.23)	(0.31)	(0.26)	(0.79)	(0.58)	(0.80)	(0.58)
Intercept	1.95*	1.66	2.10*	-1.18	2.48*	-0.31	2.19*	0.70
	(0.14)	(1.95)	(0.15)	(3.14)	(0.13)	(1.88)	(0.13)	(1.96)
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.10	0.64	0.076	0.60	0.0087	0.59	0.032	0.62

** p-value < 0.05, † p-value < 0.1. Standard errors in parenthesis. All are random-effect models.*

The only statistically significant effects are found in models that do not control for covariates, in four out of the twelve years. The two years in which France held the Presidency resulted in a higher allocation to France's former colonies than to former colonies of other EU member states (Models 25 and 27). Conversely, following two of the three British Presidency years the UK's former colonies experienced a reduction in aid, as compared to former colonies of other EU member states (Models 11 and 13).[11]

[11] When going back to aid allocated from 1960, the first-stage analysis by year produces similar results. The Netherlands held

Figure 2: Coefficient Plot for First-Stage Results by Year, No Covariates (1986–2005 Presidencies)

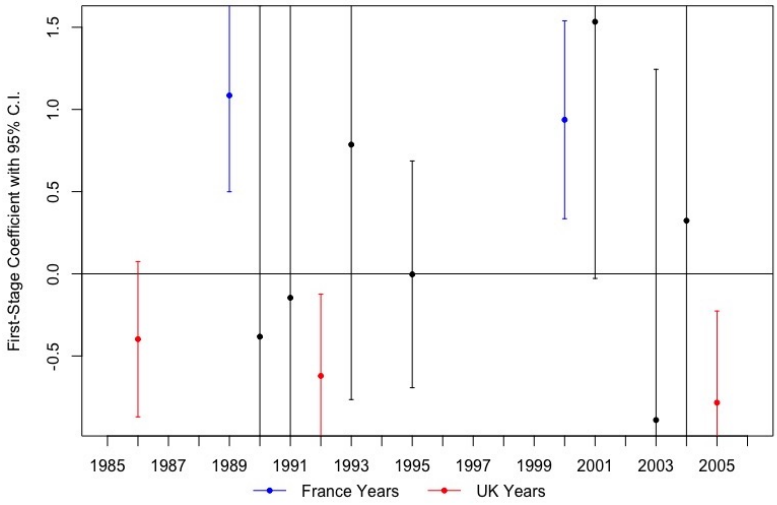
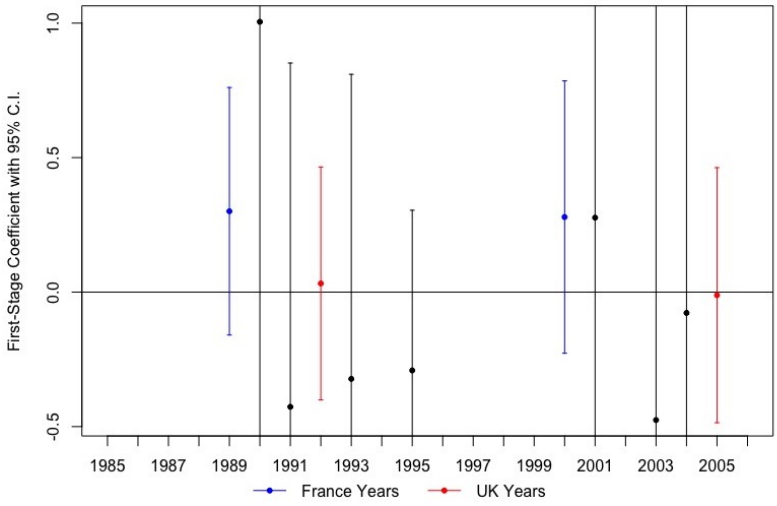


Figure 3: Coefficient Plot for First-Stage Results by Year, With Covariates (1986–2005 Presidencies)



When looking at one year at a time, evidence of "favoritism" on the part of the EU member state holding the Presidency toward its former colonies is found only in the two years when France held the Presidency. This is remarkable, as in the foreign aid literature France is generally regarded to have an aid policy that greatly favors its former colonies (e.g., Schraeder, Hook, and Taylor 1998). This finding also starts to put Carnegie and Marinov's (2017) original finding into perspective—along with the findings of any empirical strategy that relies on the rotating Presidency as an instrument for aid. If it is the case that only a few

the second-semester Presidency six times between 1960 and 1986, and Italy did so five times: none of those eleven Presidencies resulted in statistically significant bumps or cuts in aid for their respective former colonies. However, the one French Presidency in 1974 resulted in an aid increase (coeff. 1.79, st. err. 0.37, significant at the 0.1 percent level); and perhaps so did the one Belgian Presidency in 1977 (coeff. 1.78, st. err. 0.90, significant at the ten percent level). These models also do not control for covariates.

Presidencies leverage their position to direct more aid toward their former colonies (or as it may seem, if only one member state engages in this behavior), the instrument is *not* invalid. However, this prompts a more careful interpretation of the second-stage results, which might be limited to representing the effect of a small exogenous aid increase that former French colonies have received following the two French Presidencies. The analyses by former colonizer and by recipient paint a more complicated picture.

4.2 First-Stage Analysis by Former Colonizer

The first-stage analysis by year shows evidence that not all EU members exploit the Presidency of the Council to redirect more foreign aid to their former colonies. It is important to remember that the random-effect first-stage models by year presented in Table 4 compare the aid received by the former colonies of the EU member holding the Presidency at the time of allocation to that received by former colonies of other EU member states. However, the main "cumulative" first-stage models presented in Table 2 include both recipient and year fixed effect—thus, they compare *within* recipient, asking whether a country receives more or less aid following its former colonizer's Presidency than it does following the Presidency of a EU member other than its former colonizer.[12]

Table 5: First-Stage Results by Former Colonizer

	$\boldsymbol{Aid}_{(t-1)}$			
	British Group		***French Group***	
	(33)	(34)	(35)	(36)
$\boldsymbol{Colony}_{i(t-2)\,Jul-Dec}$	-0.74	-0.69	**1.16**†	0.78
	(0.71)	(0.70)	(0.66)	(0.65)
Covariates	No	Yes	No	Yes
N	1,254	1,254	594	594
R^2	0.00092	0.055	0.0057	0.096

** p-value < 0.05, † p-value < 0.1.*
Standard errors in parenthesis.
All models include recipient and year fixed effects.

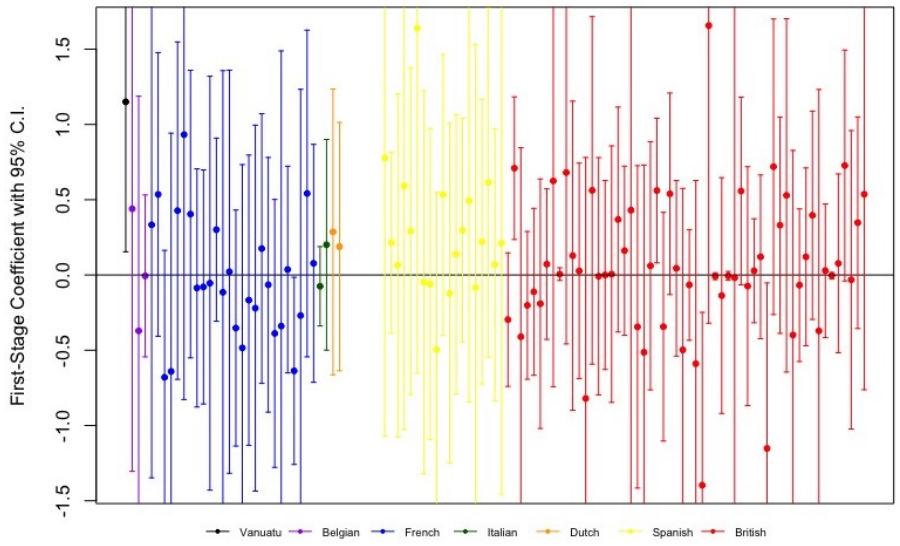
As reported in Table 5, I estimate the two-ways-fixed-effect first-stage model on two subsamples of the data: first, including only observations of British former colonies (Models 33 and 34); second, including only observations of French former colonies (Models 35 and 36). While these models do not provide definitive evidence, they do loosely support the idea that it is French former colonies who particularly benefit from having their former colonized hold the Presidency.

[12] In addition, Carnegie and Marinov (2017) argue that the year fixed effects account for the enlargement of the EU, ensuring that the set of recipients who are at a given time former colonies of the country holding the Presidency is as-if random.

4.3 First-Stage Analysis by Recipient

I continue addressing the possibility that the main first-stage results might be driven by a few specific observation by re-estimating the analysis by recipient country. This way, the comparisons remain again within country and across time, asking whether each individual recipient experiences a bump or cut in aid following its former colonizer's Presidency—as compared to what it receives following any other Presidency. Once again, the results of this analysis do not indicate that all former-colony recipients in the data set systematically experience aid increases when their former colonizer holds the Presidency. Rather, when estimating models without covariates, only three recipients experience a statistically significant aid increase: Bahrain (British), Kiribati (British), and Vanuatu (both British and French);[13] while one recipient experiences a statistically significant aid decrease: Namibia (British).[14] Of the models for each of the 109 recipients who are former colonies of the six EU members holding the Presidency during the 1986–2005 period, 49 (or roughly 45 percent) produce a *negative* coefficient for aid following the Presidency of the recipient's former colonizer.

Figure 4: Coefficient Plot for First-Stage Results by Recipient, No Covariates (1986–2005 Presidencies)



Moreover, when estimating models with covariates, only five recipients experience a statistically significant aid increase: Argentina (Spanish), Nicaragua (Spanish), Iraq (British), the Ivory Coast (France), and Singapore (British);[15] while two recipients experience a statistically significant aid decrease: Fiji and Myan-

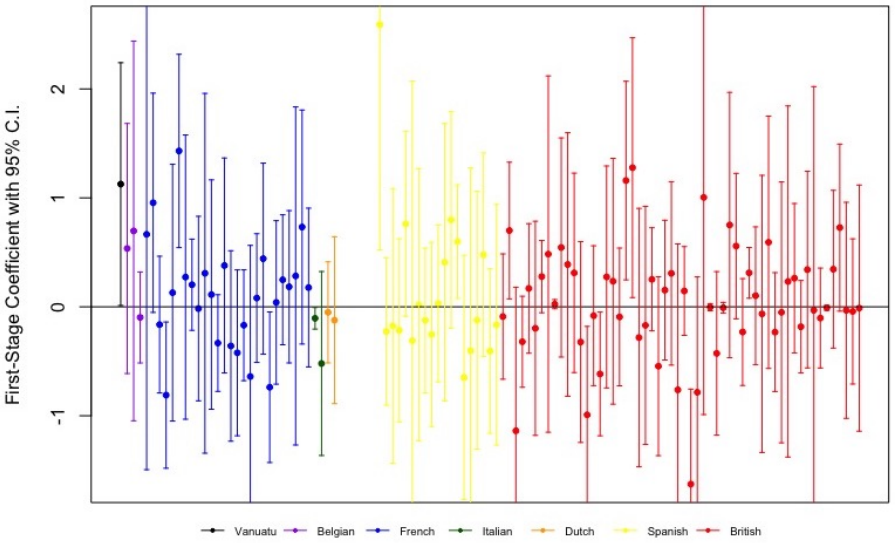
[13] An additional two recipients experience aid increases significant at the ten percent level: Seychelles and Samoa, both former British colonies.

[14] An additional two recipients experience aid decreases significant at the ten percent level: Senegal (French) and St. Lucia (British).

[15] An additional four recipient experience aid increases significant at the ten percent level: Bahrain (British), Benin (French), Jamaica (British), and Vanuatu (both British and French).

mar, both former British colonies.[16] This time, about 49 percent of the models produce a *negative* coefficient for aid following the Presidency of the recipient's former colonizer.

Figure 5: Coefficient Plot for First-Stage Results by Recipient, With Covariates (1986–2005 Presidencies)



While the first-stage analyses by year and by former colonizer point in the direction of France as the EU member who is mostly responsible for the behavior of favoritism toward its former colonies when holding the Presidency of the Council, the analysis by recipient does not provide much support for that conclusion. Taken together, these three sets of analyses do *not* find evidence that the average aid increase found in the "cumulative" first-stage analysis (Table 2) is a product of a systematic phenomenon involving most Presidencies and most recipients; rather, significant results are rarely found when the analysis is split in the various ways. However. these these three sets of analyses also do not bring us very far in identifying exactly what (group of) observations is most responsible for the positive and significant average effect found in the cumulative first-stage analysis. I continue to address this question by investigating the way in which the cumulative first-stage regression with covariates (Model 2) weights its observations.

4.4 First-Stage Analysis: Regression Weights

Are some observations in the cumulative first-stage regression with covariates (Model 2) weighted higher than others? As most recently stated by Aronow and Samii (2016), a multivariate linear regression fitted via OLS induces a weighting scheme whereby contributions from sample members are used differentially. In short, "more weight goes to units whose treatment values are not well explained by the covariates" (255).

[16] An additional three recipients experience aid decreases significant at the ten percent level: Ghana (British), Libya (Italian), and Mali (French)

In this case, the recipient countries whose status of former colony of the EU member holding the presidency is most difficult to predict given the covariates[17] are the observations which will more heavily influence the effect estimates. Lastly, the observations with the least weight are on average those of Vanuatu—which as explained is a former colony of both the UK and France, so it is coded as 0.5 both when the UK holds the Presidency and when France does.

Figure 6: First-Stage OLS: Nominal Sample

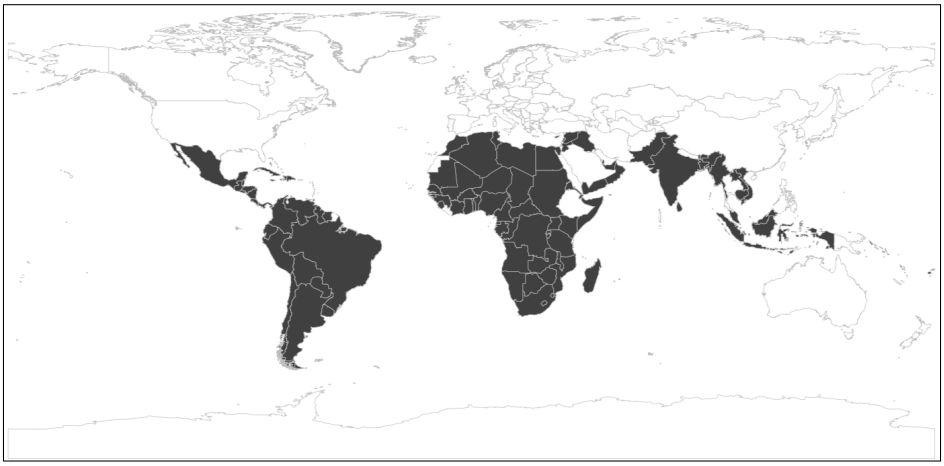


Figure 7: First-Stage OLS: Effective Sample

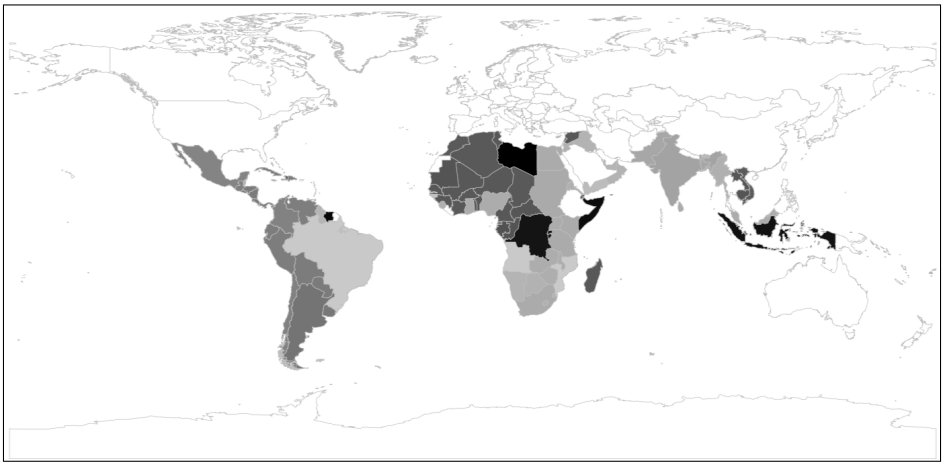


Figure 6 shows all of the recipient countries in our sample; Figure 7 ranks them by weight, with the darkest recipients weighing the most. The first tier (i.e., the most influential observations) includes Libya, Suriname, Indonesia, the Democratic Republic of the Congo, Rwanda, Burundi, and Somalia; this is an exhaustive list of the former colonies of Italy, the Netherlands, and Belgium. The second tier is an exhaustive list of French

[17]Recipient-country average years of education, exports, imports, FDI, religiosity, oil revenues, democracies in the region, GDP, and GDP per capita.

former colonies.[18] The third tier is an exhaustive list of Spanish former colonies.[19] The fourth tier is an exhaustive list of British former colonies.[20] The fifth and last tier is an exhaustive list of Portuguese former colonies,[21] which makes intuitive sense: Portugal never holds the Presidency in the budgetary semester in the 1986–2005 period, so the treatment indicator for its former colony is always equal to zero.

Of the 52 most influential recipients, the first-stage analysis by recipient found a positive coefficient for 27 (Figure 5), including three positive and significant coefficients for Argentina (Spanish), Nicaragua (Spanish), and the Ivory Coast (France); and no negative and significant coefficient. The fact that recipients for which the relationship between colonial status relative to the Presidency holder and EU aid is mostly positive and at times significant are overrepresented among the ones which the regression model weights the highest can help explain the cumulative result in Model 2.

In conclusion to this investigation of the first-stage analysis, what emerges is that the cumulative positive effect of colonial status relative to the Presidency holder on EU aid allocation does not appear to result from a systematic phenomenon involving most former colonizers and most recipients. Rather, it is likely that only a few Presidencies result in an aid increase for their holder's former colonies; and that only a few recipient experience significant aid increases following their former colonizer's Presidency. This does *not* invalidate the instrumental variable approach used by Carnegie and Marinov (2017) or its results. However, it makes it challenging to know precisely to what contexts the estimated local average treatment effect applies to.

5 Alternative First-Stage Analysis

5.1 A Broader Conceptualization of Donor Aid Giving Preferences

The analyses presented in Section 4 confirm that there is evidence of a positive and significant effect of being a former colony of the EU member state holding the Presidency of the Council on the allocated amount of EU aid—that is, in the aggregate, in a model with both recipient and year fixed effects. At the same time, (i) when the analysis is estimated separately by former colonizer, what emerges is limited evidence of such relationship holding for former French colonies, but not for former British colonies; (ii) when the analysis is estimated within each year and across recipients, there is little evidence that countries whose former colonizer

[18] Comoros, Gabon, Haiti, Djibouti, Congo, Mauritania, Cambodia, Central African Republic, Lebanon, Togo, Guinea, Benin, Laos, Senegal, Niger, Tunisia, Cameroon, Syria, Mali, Madagascar, Burkina Faso, Ivory Coast, Algeria, Chad, Morocco, and Vietnam.

[19] Argentina, Uruguay, Equatorial Guinea, Panama, Costa Rica, El Salvador, Paraguay, Honduras, Nicaragua, Dominican Republic, Cuba, Bolivia, Ecuador, Peru, Venezuela, Guatemala, Chile, Colombia, and Mexico.

[20] India, Bangladesh, Pakistan, Sudan, Kenya, Egypt, Tanzania, South Africa, Sri Lanka, Ghana, Uganda, Nigeria, Malaysia, Malawi, Zambia, Zimbabwe, Tuvalu, Sierra Leone, Lesotho, Myanmar, Eritrea, Jordan, Solomon Islands, Jamaica, Singapore, Trinidad and Tobago, Botswana, Iraq, Gambia, Mauritius, Swaziland, Qatar, Oman, Maldives, Fiji, Namibia, Guyana, Cyprus, United Arab Emirates, Bahrain, Bahamas, Belize, St. Lucia, Tonga, Kiribati, Seychelles, Antigua & Barbuda, Bhutan, Barbados, St. Vincent and the Grenadines, Dominica, St. Kitts and Nevis, Grenada, Samoa, Yemen Arab Republic, and Brunei.

[21] Cape Verde, Guinea-Bissau, Sao Tome and Principe, Brazil, Angola, and Mozambique.

holds the Presidency consistently receive more aid; and lastly (iii) when the analysis is estimated within each recipient and across years, there is also little evidence that recipients systematically get more EU aid in the years when their former colonizer is in power.

These results inform our interpretation and the generalizability of the second-stage local average treatment effect (LATE) estimate produced with the instrumentation of EU aid via this colonial-ties mechanism related to the rotating Presidency of the Council of the EU. However, they do not allow us to fully answer the question of whether EU member states systematically exploit the Presidency in order to make the allocation of EU aid closer to their own preferences. As discussed, donor countries' priorities in aid giving are not solely determined by a history of colonial ties with certain recipients. Rather, many other factors determine donors' allocation preferences.

While aid allocation is in part explained by reasons of recipient need—with poorer, larger, disaster-stricken countries being expected to receive more aid—strategic considerations have been understood to play an important role since the very beginning of modern aid giving (e.g. Morgenthau 1962). Scholars have argued that donors give disproportionate amounts of aid to recipients who are strategically important to them, and who can help them advance their own security and economic interests. Empirical evidence supports these claims, showing that bilateral military assistance, aid transfers, trade relationships, and colonial ties are reliable determinants of aid allocation (e.g. Alesina and Dollar 2000; Berthélemy 2006; Heinrich 2013; Milner and Tingley 2009; Schraeder, Hook, and Taylor 1998). More recently, Bermeo (2017) proposed that, in the increasingly connected post-9/11 world, donors use aid to protect themselves from spillovers caused by underdevelopment abroad—and therefore strategically target recipients who are nearby, poor, large, and with whom they share economic and migration ties.

Taken together, this scholarship indicates that donors' aid allocation preferences are shaped by more factors than just colonial ties, and that the pro-colony bias in aid giving is only one manifestation of their strategic considerations. Therefore, it is sensible to ask whether EU member states systematically exploit the Presidency of the Council in order to move EU aid allocation closer to their own aid-giving preferences—and to do so by conceptualizing donors' preferences as encompassing more than just the pro-colony bias. This phenomenon would be in line with other instances in which IO member states have been shown to exploit their positions of power in order to affect the IO's policy toward their preferences. In the context of aid-giving, scholars have argued that both World Bank and IMF lending is affected by the strategic consideration of their most powerful members (e.g. Dreher, Sturm, and Vreeland 2009; Thacker 1999).

I expect that, when an EU member state holds the Presidency of the Council, it will affect the EU aid budget so to make it reflect its own allocation preferences. I test this hypothesis by following the same model specification of the original first-stage analysis in Section 4, but with an alternative key independent variable:

I use the bilateral aid allocation of each EU member state as a measure of their revealed allocation preferences. Each donor's bilateral aid allocation reflects the influence of all factors—strategic and otherwise—that said donor takes into consideration when allocating aid. Thus, it allows me to test whether EU aid allocation tends to reflect the aid-giving priorities of the country holding the Presidency.

Specifically, I create a variable that measures the percent of the Presidency holder's bilateral aid budget that is allocated to each recipient ($\%President's\ Aid_{i(t-2)_{Jul-Dec}}$). I use this variable as a predictor of the amount of EU aid that that recipient is allocated following that Presidency holder's turn in office ($Aid_{(t-1)}$). Table 6 reports the result of this analysis, again over the 1987–2006 time period.[22]

Table 6: Alternative First-Stage Results

	$\boldsymbol{Aid}_{(t-1)}$	
	1987–2006	
	(37)	(38)
$\boldsymbol{\%President's\ Aid}_{i(t-2)_{Jul-Dec}}$	**7.80***	**7.15***
	(1.71)	(1.69)
Covariates	No	Yes
N	1,451	1,451
R^2	0.016	0.053

** p-value < 0.05. Standard errors in parenthesis.*
All models include recipient and year fixed effects.

This analysis shows preliminary evidence of a positive and significant effect of the allocation preferences of the EU member holding the Presidency—measured as its bilateral aid allocation—on EU aid allocation, which it oversees during the budgetary period. In other words, countries who receive a greater percentage of the Presidency holder's bilateral aid also receive more EU aid. The results are robust to the inclusion of the same covariates as the original first-stage analysis;[23] and the model includes recipient and year fixed effects.

Yet, we should again wonder whether this alternative first-stage result is the product of a systematic phenomenon that includes most—if not all—EU member states influencing the EU aid budget to reflect their own preferences when they have the opportunity to do so, through the Presidency of the Council. Thus, following the analyses in Section 4, I estimate separate within-year, across-recipient models for aid budgeted in the 1986–2005 twenty-year period. Note that this broader conception of aid-giving preferences allows us to take into consideration the effect of Presidencies held by all EU members who are bilateral aid donors—as opposed to focusing only on the six who have former colonies.[24] The coefficient plots in Figures 8–9 depict the results of these yearly models.

[22] EU aid budgeted in years 1986–2005

[23] As a reminder, these are recipient-year average years of education, exports, imports, FDI, religiosity, oil revenues, democracies in the region, GDP, and GDP per capita.

[24] This means that I can estimate the model on each separate year 1986–2005, with the exclusion of 1988: that year would reflect the budgeting under the Presidency of Greece, who was not a bilateral aid donor at the time.

Figure 8: Coefficient Plot for Alternative First-Stage Results by Year, No Covariates (1986–2005 Presid.)

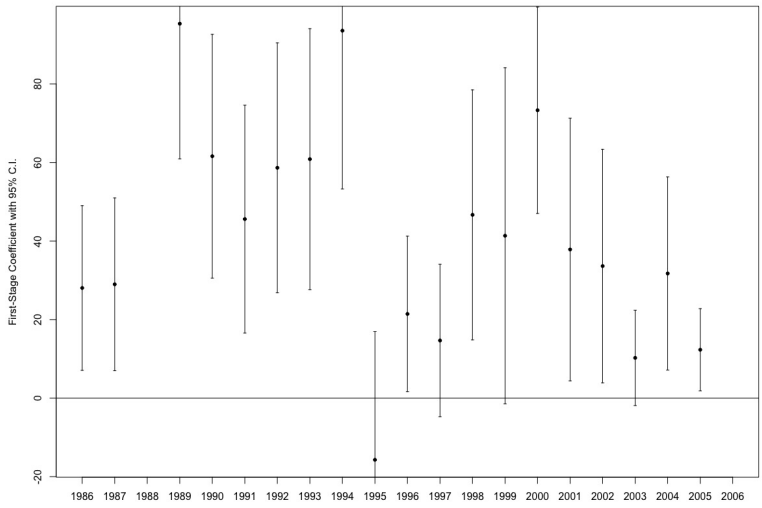
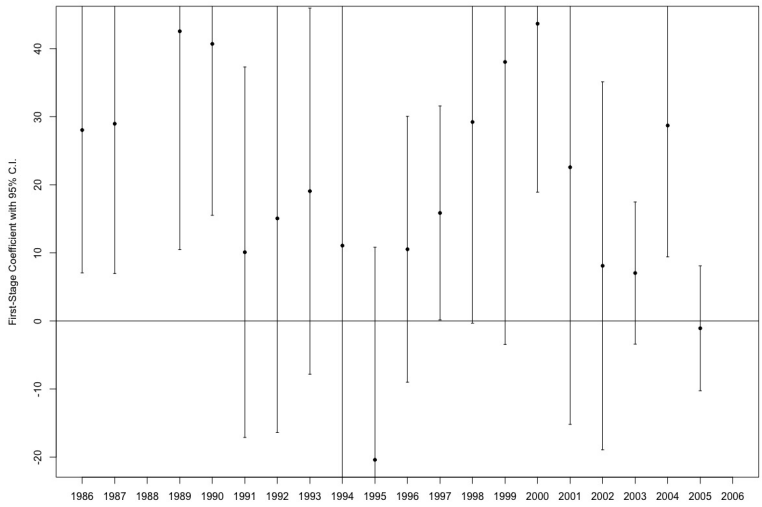


Figure 9: Coefficient Plot for Alternative First-Stage Results by Year, With Covariates (1986–2005 Presid.)



Taken together, these analyses provide further evidence in favor of the idea that EU member states indeed consistently influence EU aid budget to res amble their own bilateral aid allocation preferences when they hold the Presidency of the Council. The coefficients for the effect of the Presidency holder's bilateral aid budget that is allocated to each recipient ($\%President's\ Aid_{i(t-2)_{Jul-Dec}}$) on the amount of EU aid that said recipient is then allocated ($Aid_{(t-1)}$) are consistently positive throughout the years, with the exception of the Spanish presidency in 1995.

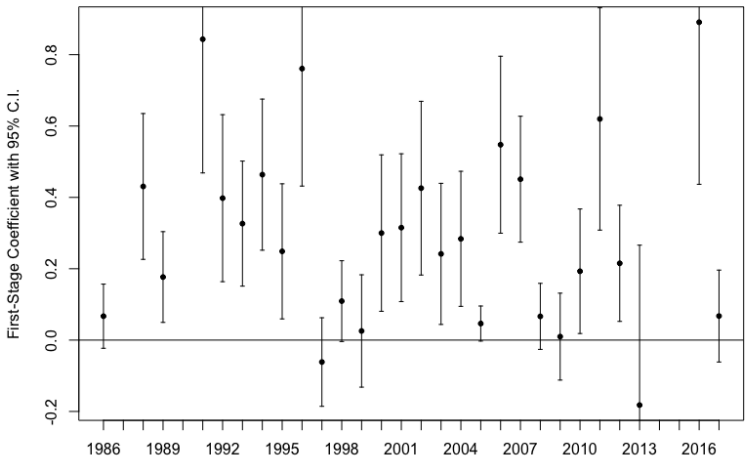
5.2 A Look Beyond 2006

The EU—and in particular the Presidency of the Council—underwent some noteworthy structural changes starting in 2007, which were then formally instituted in the 2009 Treaty of Lisbon. These changes include the formal separation of the European Council from the Council of the European Union; the split of the foreign affairs Council configuration from the General Affairs configuration; and the creation of the position of High Representative of the Union for Foreign Affairs and Security Policy. It is particularly noteworthy that the High Representative for Foreign Affairs and Security Policy is tasked with chairing the Council's Foreign Affairs Committee (FAC), which manages development cooperation, among other issue areas.

In addition, starting in 2007 groups of three EU members scheduled to hold the Presidency of the council in successive terms are made to cooperate on a common political program as part of a Presidency "trio." Taken together, these changes arguably reduced the importance of the Presidency. Therefore, I expect that they also made it more difficult for any individual member holding the Presidency in a given (second) semester to influence the EU aid budget to the extent that it was able to before 2007.

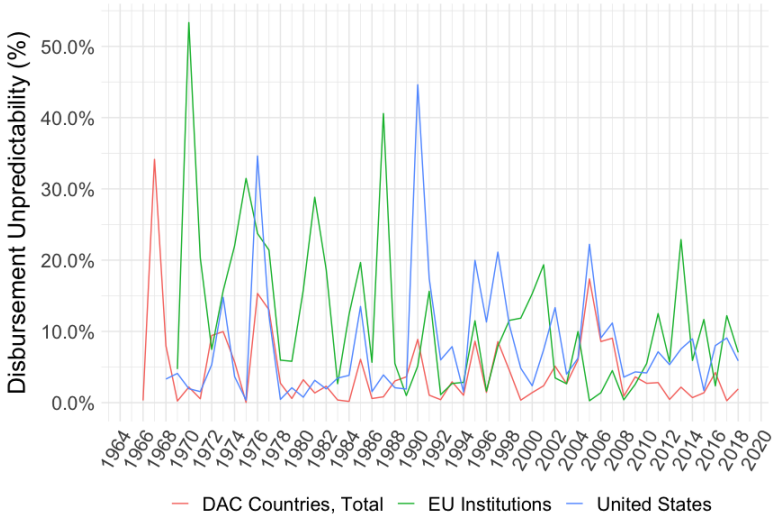
An empirical implication of such expectations is that the coefficients for first-stage models estimated by year (such as those presented in Figure 8) might be smaller and more likely to be null after 2006 as compared to before. Figure 10 presents the results of such analysis. I find little evidence to assert that the effect of the bilateral allocation preferences of the member state holding the Presidency on EU aid allocation experienced a noticeable decrease following the 2007 institutional design changes in the EU.

Figure 10: Coefficient Plot for Alternative First-Stage Results by Year, No Covariates (1986–2016)



Another empirical implication of the expectation that the 2007 changes reduced the ability of the Presidency to affect EU aid policy is that EU disbursement might become more predictable—since they would be less susceptible to the whims of the members rotating into the Presidency. Figure 11 depicts a measure of the unpredictability of aid disbursement over time, for foreign aid from EU institutions as well as from the United States and from all OECD DAC donors. I measure unpredictability as the absolute residuals of a predictive model of aid disbursements, as a percentage of actual disbursements. Imagine that one wanted to predict how much aid a given donor—or, in our case, all DAC donors combined—will disburse in a given year. A model of aid disbursements, taking into account past disbursement and commitment information, makes a prediction about this quantity. The absolute difference between the model's predicted quantity and the actual amount of aid that is eventually disbursed, scaled over the latter, measures aid unpredictability.[25]

Figure 11: Unpredictability of Aid Disbursements Over Time



The series depicted in Figure 11 do not suggest that aid disbursements from the EU became less unpredictable following the 2007 changes. Further work will be needed to determine whether and how the institutional design changes to the Presidency formalized in the Treaty of Lisbon had any impact on the ability and/or willingness of EU members holding the Presidency to affect the EU aid budget.

6 Second-Stage Analysis

The analyses in Sections 4–5 point at two conclusions with regard to the possibility of leveraging the rotating Presidency of the Council of the EU as a source of exogeneity for an instrumental variable estimation of the

[25]Specifically, I use a ten-time, five-fold cross-validated linear regression model predicting disbursements at time t with only two regressors: commitments for time t and disbursements at time $t-1$.

effects of EU aid. First, they confirm that there is an average positive and significant effect of being a former colony of the EU member state holding the presidency. However, this effect does not seem to be the product of a systematic behavior on the part of all EU member states. This complicates our ability to generalize any second-stage LATE estimates.

Secondly, I showed evidence that, while EU member states might not be systematically redirecting EU aid to their former colonies when they are in power, they might indeed be influencing EU aid to resemble their own bilateral aid allocation preferences. This alternative conceptualization of the first stage provides us with a stronger, more robust instrument; and arguably makes for easier generalizability of the second-stage results that would follow from it. In this section, I conduct second-stage analyses for both the original and the proposed alternative first stage, and compare the results.

I start by following Carnegie and Marinov (2017) and expect to find a positive effect of aid on democracy levels and respect for human rights by instrumenting for the endogenous aid allocation through the fitted values produced by the first-stage models in Table 2 (Models 1 and 2). Carnegie and Marinov (2017) measure democracy levels with the Polity IV combined scores, and respect for human rights with the CIRI Human Empowerment Index. In addition, I measure respect for human rights with the Fariss Human Rights Protection Scores. These latent scores are a relatively new way to measure human rights protection, generated by Christopher Farris using a dynamic ordinal item-response theory model. Fariss' main argument is that, as time progresses, the way in which we evaluate respect for human rights changes: "the standard of accountability used to assess state behaviors becomes more stringent as monitors look harder for abuse, look in more places for abuse, and classify more acts as abuse" (Farris 2014). When accounting for the fact that the standards have become more stringent, Farris shows that respect for human rights has improved over time—a trend that contradicts what shown by more traditional measures. The Fariss data also span a broader time period than the CIRI, covering 1949–2013. Table 7 reports the results of this replication.[26]

Note that, while the coefficients for the effect of aid on all three outcomes remain positive throughout all model specifications, they are only significant in the case of the human rights outcome operationalized with the CIRI Human Empowerment Index. As discussed, the Fariss Human Rights Protection Scores are arguably a more reliable measure—especially as it pertains to making comparisons across time. For this measure, like for the Polity IV measure of democracy, I do *not* find a significant effect of the exogenous increase in EU aid resulting from the favoritism of EU members holding the Presidency toward their former colonies. The discrepancy in the Polity IV finding indicates that the original results are not robust to this specification—which does not cluster standard errors at the levels of country and years.

[26] Again, I perform the analysis on Carnegie and Marinov's (2017) data set but with a different statistical software. According to their replication materials, Carnegie and Marinov use the `ivreg2` function in STATA. I perform the two-stage least squares analysis in R, without a pre-programmed function.

Table 7: Second-Stage Results

	$\boldsymbol{CIRI}_{(t:t+3\ avg.)}$		$\boldsymbol{Polity}_{(t:t+3\ avg.)}$		$\boldsymbol{Fariss}_{(t:t+3\ avg.)}$	
	(39)	(40)	(41)	(42)	(43)	(44)
$\boldsymbol{Aid}_{(t-1)}$	**2.08***	**2.13***	2.38	1.89	0.082	0.11
	(0.88)	(1.01)	(1.62)	(1.86)	(0.22)	(0.25)
Covariates	No	Yes	No	Yes	No	Yes
N	1,792	1,792	1,818	1,818	2,456	2,456
R^2	0.0034	0.075	0.0013	0.078	0.000062	0.021

** p-value < 0.05. Standard errors in parenthesis.*

Next, I estimate second-stage results using the alternative first stage—i.e., instrumenting the endogenous aid variable through the revealed allocation preferences of the EU member state holding the Presidency. While the positive and significant effect on human right protections as captured by the CIRI measure persists, the coefficient for the effect on the Polity IV measure of democracy is again not statistically significant—and even turns negative in the specification with covariates. I find no evidence of an effect on the Fariss measure of human rights protections, with negative coefficients in both specifications.

Table 8: Second-Stage Results with Alternative First Stage

	$\boldsymbol{CIRI}_{(t:t+3\ avg.)}$		$\boldsymbol{Polity}_{(t:t+3\ avg.)}$		$\boldsymbol{Fariss}_{(t:t+3\ avg.)}$	
	(45)	(46)	(47)	(48)	(49)	(50)
$\boldsymbol{Aid}_{(t-1)}$	**1.08***	**0.73**†	0.17	-0.13	-0.083	-0.098
	(0.41)	(0.43)	(0.81)	(0.87)	(0.11)	(0.12)
Covariates	No	Yes	No	Yes	No	Yes
N	1,248	1,248	1,299	1,299	1,448	1,448
R^2	0.0060	0.12	0.000036	0.058	0.00045	0.028

** p-value < 0.05, † p-value < 0.1. Standard errors in parenthesis.*

7 Conclusion

One of the main challenges in assessing the effectiveness of foreign aid has been linked to the endogeneity of its allocation to the outcome variables which aid might affect. We know that donor countries and organization make aid allocation decisions based on a variety of factors, including recipients' level of economic development, their type of government and institutional quality, their strategic importance in relation to the donor, and so on. In the case of democracy and human rights, if donors target recipient who struggle in an effort to help them, it might look like higher aid amounts are correlated with lower democracy levels and human rights protection—when in fact the aid might be improving upon those records. Conversely, if donors use aid to reward virtuous recipient who already do quite well, it might look like higher aid amounts are correlated with high democracy levels and human rights protection—when in fact might not be helping at all, and the improvements would have happened in those virtuous recipients nonetheless.

Carnegie and Marinov (2017) propose an innovative and clever idea to overcome the endogeneity of aid allocation and assess the effect of foreign aid on human rights and democracy. Focusing on foreign aid from European Union (EU) institutions, they exploit the arguably exogenous rotation of the Presidency of the Council of the EU and leverage the fact that, when a former colonizer holds the Presidency, it is able to allocate more EU aid to its former colonies; and employ an instrumental variable analysis to estimate the local average treatment effect of receiving an additional amount of aid. First, they show that being a former colony of the EU member state holding the Presidency results in a small aid increase. Then, they find that such arguably exogenous aid increase has positive, if perhaps short-lived, effects on human rights and democracy outcomes in recipient countries.

This estimated positive local average treatment effect is that of the small exogenous aid increase that recipients experience when their former colonizer holds the Presidency of the Council of the EU. However, it is important to assess whether this LATE is representative of most aid recipients who are former colonies; or if it is only a few former colonies of a few EU members who drive this finding. To address this question, I re-estimate the first-stage analysis by year, by former colonizer, and by recipient. When looking at one year at a time and comparing aid received across countries, I find that only French Presidency seem to result in significant increases for former colonies. This conclusion is not contradicted by the analysis by former colonizer, which shows that the positive effects remains when sub-setting the sample to only include French former colonies, but not when sub-setting the sample to only include British former colonies.

However, the analysis by recipient does not provide much support for the idea that France is mostly responsible for the behavior of favoritism toward its former colonies when holding the Presidency of the Council. Rather, taken together my analyses find little evidence that the average aid increase found in the cumulative first-stage analysis (Table 2) is a product of a systematic phenomenon involving most Presidencies and most recipients. It is reasonable to conclude that (i) not all former colonizers allocate more aid to their former colonies when holding the Presidency; and (ii) not all former colonies experience an aid increase as a result of their former colonizer holding the Presidency.

Yet these results do not refute the possibility that EU member states holding the Presidency influence the EU aid budget to move it closer to their own aid-giving priorities. In fact, the pro-colony bias is only one of many strategic determinants of aid allocation documented in the foreign aid literature. I test this broader strategic allocation hypothesis by following the same model specification of the original first-stage analysis in Section 4, but with an alternative key independent variable: I use the bilateral aid allocation of each EU member state as a measure of their revealed allocation preferences. Each donor's bilateral aid allocation reflects the influence of all factors—strategic and otherwise—that said donor takes into consideration when allocating aid. Thus, it allows me to test whether EU aid allocation tends to reflect the aid-giving priorities

of the country holding the Presidency. I show evidence that, while EU member states do not seem to be systematically redirecting EU aid to their former colonies when they are in power, they might indeed be influencing EU aid to resemble their own bilateral aid allocation preferences. This alternative conceptualization of the first stage provides us with a stronger, more robust instrument; and arguably makes for easier generalizability of the second-stage results that would follow from it.

I further investigate whether the institutional design changes that the EU—and in particular the Presidency of the Council—underwent starting in 2007 affected the relationship between the Presidency and the EU aid budget. Such changes, formalized in the 2009 Treaty of Lisbon, arguably reduced the importance of the Presidency. I therefore expect that the effect of the member holding the Presidency on EU aid allocation would decrease or disappear; and that EU aid disbursements would become less unpredictable as a result. While I have yet to thoroughly test these expectations empirically, preliminary analyses do not show evidence consistent with these expectations.

In the second stage of the analysis, I first replicate Carnegie and Marinov's (2017) specification and find positive coefficients for the effect of aid on all three outcomes; however, they are only significant in the case of the human rights outcome operationalized with the CIRI Human Empowerment Index. I do not find a significant LATE for the Polity IV measure of democracy, nor for the arguably more accurate measure of human rights, the Fariss Human Rights Protection Scores. When I move to using the alternative first stage—i.e., instrumenting the endogenous aid variable through the revealed allocation preferences of the EU member state holding the Presidency, the positive and significant effect on human right protections as captured by the CIRI measure persists. However the coefficient for the effect on the Polity IV measure of democracy is again not statistically significant—and even turns negative in the specification with covariates. I find no evidence of an effect on the Fariss measure of human rights protections, with negative coefficients in both specifications.

Taken together, these analyses allow us to learn two things: first, substantively, that EU member states do not seem to be systematically exploiting the Presidency of the Council to redirect more aid toward their former colonies; yet they do influence the EU aid budget so to make it reflect their broader aid-giving preferences. Second, this limits the generalizability—not the internal validity!—of a second-stage LATE estimate obtained when instrumenting via the colonial-relationship variable. When instead we instrument via the broader allocation preferences variable, evidence of a meaningful impact of the exogenous change in EU aid on measures of democracy and human rights become more tenuous.

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Appendix

A1 Former Colonial Status of Recipient Countries

Table A1: Recipient Countries and their Former Colonizers. Asterisks denote discrepancies between the classification used in Carnegie and Marinov (2017) and the ICOW Colonial History data.

Recipient	Colonizer, CM 2017	Colonizer, ICOW
Algeria	France	France
Angola	Portugal	Portugal
Antigua and Barbuda	United Kingdom	United Kingdom
Argentina	Spain	Spain
Bahamas	United Kingdom	United Kingdom
Bahrain	United Kingdom	United Kingdom
Bangladesh	United Kingdom	United Kingdom
Barbados	United Kingdom	United Kingdom
Belize	United Kingdom	United Kingdom
Benin	France	France
Bhutan	United Kingdom	United Kingdom
Bolivia	Spain	Spain
Botswana	United Kingdom	United Kingdom
Brazil	Portugal	Portugal
Brunei Darussalam	United Kingdom	United Kingdom
Burkina Faso	France	France
Burundi	Belgium	Belgium
Cabo Verde	Portugal	Portugal
Cambodia	France	France
Cameroon	France	France
Central African Republic	France	France
Chad	France	France
Chile	Spain	Spain
Colombia	Spain	Spain
Comoros	France	France
Congo	France	France
Costa Rica	Spain	Spain
Cote d'Ivoire	France	France
Cuba	Spain	Spain
Cyprus	United Kingdom	United Kingdom
Democratic Republic of the Congo	Belgium	Belgium
Djibouti	France	France
Dominica	United Kingdom	United Kingdom
Dominican Republic	Spain	Spain
Ecuador	Spain	Spain
Egypt*	United Kingdom	Turkey
El Salvador	Spain	Spain
Equatorial Guinea	Spain	Spain
Eritrea*	United Kingdom	Italy
Fiji	United Kingdom	United Kingdom
Gabon	France	France
Gambia	United Kingdom	United Kingdom
Ghana	United Kingdom	United Kingdom
Grenada	United Kingdom	United Kingdom
Guatemala	Spain	Spain
Guinea	France	France
Guinea-Bissau	Portugal	Portugal
Guyana*	United Kingdom	Spain
Haiti	France	France
Honduras	Spain	Spain
India	United Kingdom	United Kingdom
Indonesia	Netherlands	Netherlands
Iraq*	United Kingdom	Turkey
Jamaica	United Kingdom	United Kingdom
Jordan*	United Kingdom	Turkey
Kenya	United Kingdom	United Kingdom
Kiribati	United Kingdom	United Kingdom
Lao People's Democratic Republic	France	France
Lebanon*	France	Turkey
Lesotho	United Kingdom	United Kingdom
Libya*	Italy	Turkey
Madagascar	France	France
Malawi	United Kingdom	United Kingdom
Malaysia	United Kingdom	United Kingdom
Maldives	United Kingdom	United Kingdom
Mali	France	France
Mauritania	France	France
Mauritius*	United Kingdom	France
Mexico	Spain	Spain
Morocco	France	France
Mozambique	Portugal	Portugal
Myanmar	United Kingdom	United Kingdom
Namibia	United Kingdom	United Kingdom
Nicaragua	Spain	Spain
Niger	France	France
Nigeria	United Kingdom	United Kingdom
Oman*	United Kingdom	Portugal
Pakistan	United Kingdom	United Kingdom
Panama	Spain	Spain
Paraguay	Spain	Spain
Peru	Spain	Spain
Qatar	United Kingdom	United Kingdom
Rwanda	Belgium	Belgium
Saint Kitts and Nevis	United Kingdom	United Kingdom
Saint Lucia	United Kingdom	United Kingdom
Saint Vincent and the Grenadines	United Kingdom	United Kingdom
Samoa*	United Kingdom	New Zealand
Sao Tome and Principe	Portugal	Portugal
Senegal	France	France
Seychelles	United Kingdom	United Kingdom
Sierra Leone	United Kingdom	United Kingdom
Singapore	United Kingdom	United Kingdom
Solomon Islands	United Kingdom	United Kingdom
Somalia	Italy	Italy
South Africa	United Kingdom	United Kingdom
Sri Lanka	United Kingdom	United Kingdom
Sudan	United Kingdom	United Kingdom
Suriname*	Netherlands	Spain
Swaziland	United Kingdom	United Kingdom
Syrian Arab Republic*	France	Turkey
Tanzania	United Kingdom	United Kingdom
Togo	France	France
Tonga	United Kingdom	United Kingdom
Trinidad and Tobago	United Kingdom	United Kingdom
Tunisia*	France	Turkey
Tuvalu	United Kingdom	United Kingdom
Uganda	United Kingdom	United Kingdom
United Arab Emirates	United Kingdom	United Kingdom
Uruguay	Spain	Spain
Vanuatu*	France & UK	United Kingdom
Venezuela	Spain	Spain
Viet Nam	France	France
Yemen	United Kingdom	United Kingdom
Zambia	United Kingdom	United Kingdom
Zimbabwe	United Kingdom	United Kingdom

A2 Rotation Calendar for the Presidency of the EU Council

Table A2: Rotation Calendar for the Presidency of the EU Council, 1958–2021.

Year	Semester	President	Original Name	Cycle	Rotation
1958	1st	Belgium	België/Belgique/Belgien	1	Alphabetical
1958	2nd	West Germany	Deutschland	1	Alphabetical
1959	1st	France	France	1	Alphabetical
1959	2nd	Italy	Italia	1	Alphabetical
1960	1st	Luxembourg	Lëtzebuerg	1	Alphabetical
1960	2nd	Netherlands	Nederland	1	Alphabetical
1961	1st	Belgium	België/Belgique/Belgien	2	Alphabetical
1961	2nd	West Germany	Deutschland	2	Alphabetical
1962	1st	France	France	2	Alphabetical
1962	2nd	Italy	Italia	2	Alphabetical
1963	1st	Luxembourg	Lëtzebuerg	2	Alphabetical
1963	2nd	Netherlands	Nederland	2	Alphabetical
1964	1st	Belgium	België/Belgique/Belgien	3	Alphabetical
1964	2nd	West Germany	Deutschland	3	Alphabetical
1965	1st	France	France	3	Alphabetical
1965	2nd	Italy	Italia	3	Alphabetical
1966	1st	Luxembourg	Lëtzebuerg	3	Alphabetical
1966	2nd	Netherlands	Nederland	3	Alphabetical
1967	1st	Belgium	België/Belgique/Belgien	4	Alphabetical
1967	2nd	West Germany	Deutschland	4	Alphabetical
1968	1st	France	France	4	Alphabetical
1968	2nd	Italy	Italia	4	Alphabetical
1969	1st	Luxembourg	Lëtzebuerg	4	Alphabetical
1969	2nd	Netherlands	Nederland	4	Alphabetical
1970	1st	Belgium	België/Belgique/Belgien	5	Alphabetical
1970	2nd	West Germany	Deutschland	5	Alphabetical
1971	1st	France	France	5	Alphabetical
1971	2nd	Italy	Italia	5	Alphabetical
1972	1st	Luxembourg	Lëtzebuerg	5	Alphabetical
1972	2nd	Netherlands	Nederland	5	Alphabetical
1973	1st	Belgium	België/Belgique/Belgien	6	Alphabetical
1973	2nd	Denmark	Danmark	6	Alphabetical
1974	1st	West Germany	Deutschland	6	Alphabetical
1974	2nd	France	France	6	Alphabetical
1975	1st	Ireland	Ireland/Éire	6	Alphabetical
1975	2nd	Italy	Italia	6	Alphabetical
1976	1st	Luxembourg	Lëtzebuerg	6	Alphabetical
1976	2nd	Netherlands	Nederland	6	Alphabetical
1977	1st	United Kingdom	United Kingdom	6	Alphabetical
1977	2nd	Belgium	België/Belgique/Belgien	7	Alphabetical
1978	1st	Denmark	Danmark	7	Alphabetical
1978	2nd	West Germany	Deutschland	7	Alphabetical
1979	1st	France	France	7	Alphabetical
1979	2nd	Ireland	Ireland/Éire	7	Alphabetical
1980	1st	Italy	Italia	7	Alphabetical
1980	2nd	Luxembourg	Lëtzebuerg	7	Alphabetical
1981	1st	Netherlands	Nederland	7	Alphabetical
1981	2nd	United Kingdom	United Kingdom	7	Alphabetical
1982	1st	Belgium	België/Belgique/Belgien	8	Alphabetical
1982	2nd	Denmark	Danmark	8	Alphabetical
1983	1st	West Germany	Deutschland	8	Alphabetical
1983	2nd	Greece	Ellada	8	Alphabetical
1984	1st	France	France	8	Alphabetical
1984	2nd	Ireland	Ireland/Éire	8	Alphabetical
1985	1st	Italy	Italia	8	Alphabetical
1985	2nd	Luxembourg	Lëtzebuerg	8	Alphabetical
1986	1st	Netherlands	Nederland	8	Alphabetical
1986	2nd	United Kingdom	United Kingdom	8	Alphabetical
1987	1st	Belgium	België/Belgique/Belgien	9	Alphabetical
1987	2nd	Denmark	Danmark	9	Alphabetical
1988	1st	West Germany	Deutschland	9	Alphabetical
1988	2nd	Greece	Ellada	9	Alphabetical
1989	1st	Spain	España	9	Alphabetical
1989	2nd	France	France	9	Alphabetical
1990	1st	Ireland	Ireland/Éire	9	Alphabetical
1990	2nd	Italy	Italia	9	Alphabetical
1991	1st	Luxembourg	Lëtzebuerg	9	Alphabetical
1991	2nd	Netherlands	Nederland	9	Alphabetical
1992	1st	Portugal	Portugal	9	Alphabetical
1992	2nd	United Kingdom	United Kingdom	9	Alphabetical
1993	1st	Denmark	Danmark	10	Alternating Alphabetical
1993	2nd	Belgium	België/Belgique/Belgien	10	Alternating Alphabetical
1994	1st	Greece	Ellada	10	Alternating Alphabetical
1994	2nd	Germany	Deutschland	10	Alternating Alphabetical
1995	1st	France	France	10	Alternating Alphabetical
1995	2nd	Spain	España	10	Alternating Alphabetical
1996	1st	Italy	Italia	10	Balanced Rotation
1996	2nd	Ireland	Ireland/Éire	10	Balanced Rotation
1997	1st	Netherlands	Nederland	10	Balanced Rotation
1997	2nd	Luxembourg	Lëtzebuerg	10	Balanced Rotation
1998	1st	United Kingdom	United Kingdom	10	Balanced Rotation
1998	2nd	Austria	Österreich	10	Balanced Rotation
1999	1st	Germany	Deutschland	10	Balanced Rotation
1999	2nd	Finland	Suomi	10	Balanced Rotation
2000	1st	Portugal	Portugal	10	Balanced Rotation
2000	2nd	France	France	10	Balanced Rotation
2001	1st	Sweden	Sverige	10	Balanced Rotation
2001	2nd	Belgium	België/Belgique/Belgien	10	Balanced Rotation
2002	1st	Spain	España	10	Balanced Rotation
2002	2nd	Denmark	Danmark	10	Balanced Rotation
2003	1st	Greece	Ellada	10	Balanced Rotation
2003	2nd	Italy	Italia	10	Balanced Rotation
2004	1st	Ireland	Ireland/Éire	10	Balanced Rotation
2004	2nd	Netherlands	Nederland	10	Balanced Rotation
2005	1st	Luxembourg	Lëtzebuerg	10	Balanced Rotation
2005	2nd	United Kingdom	United Kingdom	10	Balanced Rotation
2006	1st	Austria	Österreich	10	Balanced Rotation
2006	2nd	Finland	Suomi	10	Inverted by 2002/105/EC
2007	1st	Germany	Deutschland	Trio 1	Inverted by 2002/105/EC
2007	2nd	Portugal	Portugal	Trio 1	Trios
2008	1st	Slovenia	Slovenija	Trio 1	Trios
2008	2nd	France	France	Trio 2	Trios
2009	1st	Czech Republic	Česká Republika	Trio 2	Trios
2009	2nd	Sweden	Sverige	Trio 2	Trios
2010	1st	Spain	España	Trio 3	Trios
2010	2nd	Belgium	België/Belgique/Belgien	Trio 3	Trios
2011	1st	Hungary	Magyarország	Trio 3	Trios
2011	2nd	Poland	Polska	Trio 4	Trios
2012	1st	Denmark	Danmark	Trio 4	Trios
2012	2nd	Cyprus	Kúpros	Trio 4	Trios
2013	1st	Ireland	Ireland/Éire	Trio 5	Trios
2013	2nd	Lithuania	Lietuva	Trio 5	Trios
2014	1st	Greece	Ellada	Trio 5	Trios
2014	2nd	Italy	Italia	Trio 6	Trios
2015	1st	Latvia	Latvija	Trio 6	Trios
2015	2nd	Luxembourg	Lëtzebuerg	Trio 6	Trios
2016	1st	Netherlands	Nederland	Trio 7	Trios
2016	2nd	Slovakia	Slovensko	Trio 7	Trios
2017	1st	Malta	Malta	Trio 7	Trios
2017	2nd	Estonia	Eesti	Trio 8	Trios
2018	1st	Bulgaria	Bălgariya	Trio 8	Trios
2018	2nd	Austria	Österreich	Trio 8	Trios
2019	1st	Romania	România	Trio 9	Trios
2019	2nd	Finland	Suomi	Trio 9	Trios
2020	1st	Croatia	Hrvatska	Trio 9	Trios
2020	2nd	Germany	Deutschland	Trio 10	Trios
2021	1st	Portugal	Portugal	Trio 10	Trios
2021	2nd	Slovenia	Slovenija	Trio 10	Trios