

The Far Right and Earmarked Funding of International Organizations

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January 15, 2024

Abstract

The Far Right is on the rise, winning elections and entering government globally. Sharing a nationalist ideology, far-right parties are skeptical toward multilateralism, with international organizations (IOs) becoming prominent targets in their campaigns. Recent research in International Relations debates whether such attacks are rhetorical or a substantial assault on IOs. Our contribution investigates the direct effect of the Far Right on IOs, focusing on their financial contributions for multilateral development cooperation. Many donor states direct their foreign aid through IOs, often earmarking funds for specific purposes to maintain control over spending. We argue that, when in government, far-right parties will reduce earmarked funding to IOs, while they will be more hesitant to cut direct bilateral aid. Reducing earmarked funding of IOs helps the Far Right to realize their promises to spend less for others and curtail the influence of international institutions. Using a new dataset on IO's earmarked funding, we study the monetary foreign aid commitments of 37 democratic OECD donor states from 1990 until 2020. To measure the Far Right, also beyond Europe, we create a dataset identifying their participation in donor governments. We use weighting techniques to balance our sample at each point in time, addressing risks from dynamic confounding and post-treatment bias. Employing country, year, and aid sector fixed effects, our evidence consistently supports our argument. The Far Right, when in government, has direct consequences for the resources IOs have for development purposes. Far-right governments cut earmarked funding, but do not change substantially direct bilateral aid. These dynamics can weaken International Organizations and their development policies in important ways.

Keywords: Far Right; International Organizations; Foreign Aid; Party Politics

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1 Introduction

Over the past decade, the Far Right reshaped domestic politics around the world. It gained parliamentary seats or even government participation in many European countries (Akkerman, de Lange, & Rooduijn, 2016; Minkenberg, 2017; Mudde, 2007), and shifted the political landscape globally, not least in the USA (James, 2012), Brazil (Bertonha, 2020), and India (Masood & Nisar, 2020). Particularly radical right parties have gained wide attention, as “political parties with a core ideology that is a combination of nativism, authoritarianism, and populism.” (Mudde, 2007, p. 26). Despite their differences on many policy issues, these parties pursue national sovereignty and are skeptical of international organizations (IOs) and their authority (Hooghe, Lenz, & Marks, 2019; Mudde, 2007; Zürn, Tokhi, & Binder, 2021)—much more so if IOs support policies detrimental to their positions. For example, the Alternative for Germany (AfD) openly rejects the European Union in its manifesto for the 2024 European Parliament elections.¹ In 2018, the Freedom Party Austria made the Austrian government retract from signing the UN Global Compact for Migration based on its “dangers for national sovereignty”.² Katalin Novák, FIDESZ-member and current president of Hungary and outspoken anti-gender activist, created a major uproar when delivering a speech at the international “Women Deliver” conference in 2023, which brings together women’s rights groups and is closely supported by UN Women, UNFPA, and the World Health Organization.³

Examples abound of how the Far Right challenges International Organizations. Yet, our knowledge about how the Far Right affects IOs, their design, policies, and functioning, remains limited. Related research is scarce and scattered. To little surprise, domestic politics have been the at the center of scholarly attention. Researchers analyzed the extent to which the rise of the Far Right shifted domestic discourse, as these groups give certain issues, such as migration, abortion or others, salience and put them on the agenda (Akkerman et al., 2016; van Spanje, 2010). Consequently, its rise directly (via government participation) or indirectly (via electoral successes) shifts discourse and subsequently policies (Minkenberg, 2001). With regard to migration policies in particular, but also beyond, we do see indirect effects and limited direct effects where far-right parties push publicly certain policies and conservative parties take over their discourse (Lutz, 2019). Relatedly, much attention for the Far Right focused on the

¹Accepted proposal at the AfD European election meeting in Madgeburg, p. 10.

²See [sueddeutsche.de](https://www.sueddeutsche.de)

³See [guardian.co.uk](https://www.guardian.co.uk).

European Parliament (EP)—where these groups were, however, not found to be overly effective in the past due to their lacking unity (Hafez, 2014; McDonnell & Werner, 2019; Startin, 2010). Indeed, the direct influence on policymaking of far-right parliamentarians in the EP has been found to be rather small (Chiru & Wunsch, 2023; Mudde, 2013). It appears, though, that the Far Right in the EP might be “learning the game” (Ben-Hur Levy & Center for European Reform, 2015) steadily.

This study contributes to the larger debate by considering one direct way of how the Far Right may affect IOs and their policies—via funding. In recent years, earmarked funding of IOs makes up the bulk of IO resources targeted for development aid (Heinzel, Cormier, & Reinsberg, 2023)—and is one of the more direct ways for states to shape IO activities. Complementing direct bilateral aid, earmarked funding commitments represent voluntary financial contributions of states to IOs. These funds are reserved for specific development purposes that states can often specify in advance. The IO then designs and manages projects and allocates the funds according to the mandate given by donor states. Earmarked funding is a double-edged sword. It gives donor states substantial influence and control over IOs’ development projects, often lowering the performance of IOs (Eichenauer & Reinsberg, 2017; Graham, 2017; Heinzel et al., 2023). On the other hand, earmarked funding is an important resource that enhances IOs’ operational capacities, often providing much needed funds to advance development policies. Many parties of the Far Right openly target multilateral development aid, particularly when IOs are involved in project management. For example, when the new conservative Swedish government came into power, backed by the far-right Sweden Democrats, it not only announced a turnaround in foreign aid policies of the country, it explicitly wanted to reduce funding for multilateral institutions and instead focus on direct civil society support. These measures were presented as a way to reduce corruption (Käppeli & Calleja, 2022).

This paper investigates the effect of the Far Right on foreign aid funding, analyzing whether and how government participation of far-right parties alters donor states’ earmarked funding commitments, the strings attached to earmarked funds, and the level of direct bilateral aid without IO intermediaries. Based on a novel dataset on earmarked funding developed by Reinsberg, Heinzel, and Siauwijaya (2023), we examine the earmarked funding commitments of 37 democratic OECD member states from 1990 until 2020. Specifically, we code for each year and state whether a far-right party participated in government, triangulating several data sources

to broaden the coding of far-right parties beyond Europe and increase its validity. To obtain more fine-grained aid data, we consider also the allocation of funds across different thematic aid sectors defined by the OECD. We address important sources of bias, stemming from time-varying confounding, by specifying Marginal Structural Models, to which we add three sets of fixed effects: at the country, year, and thematic aid sector levels.

We find that far-right government participation has a substantial negative and significant effect on earmarked funding commitments to IOs. Across all sectors for which aid is allocated within the OECD’s Development Assistance Committee (DAC), far-right parties in government provide less earmarked funds to IOs. With respect to strings attached to the earmarked funds, governments with far-right parties do not differ from those without them. Rather than formulating conditions for how IOs should spend earmarked funds, far right parties in government seem to directly cut the earmarked monetary commitments. Similarly, governing far-right parties do not reduce direct bilateral aid.

2 State of the art

Earmarked funding is one part of the foreign aid (next to core funding) states give to multilateral institutions. This differs from other types of foreign aid, which states might offer bilaterally to other states. Such earmarked funding is mostly tied to specific programs and projects (and might be channeled via funds or other types of entities) or might have other types of conditional ties attached. Much more so than core funding, it allows states to set policy preferences and to prioritize topics of specific interest. While there has been a lively debate on why states might want to channel money via international organizations at all (Bobba & Powell, 2006; McLean, 2015; Milner & Tingley, 2013), recent research has mostly explored what the effects of a rise of earmarked funding in IO resources are—overall pointing to more direct control of states on how IOs spend money (Eichenauer & Reinsberg, 2017; Goetz & Patz, 2017; Graham, 2017). However, this freedom of choice for states comes with less aid effectiveness (Heinzel et al., 2023).

Research on how the far right affects IO funding is very scarce at this point. Political parties and their role for foreign policy have only recently gained more systematic attention (Hofmann & Martill, 2021; Otjes, van der Veer, & Wagner, 2023; Rathbun, 2004; Raunio & Wagner, 2020). Some overlap also exists with research on populism and foreign policy (Destradi, Cadier, &

Plagemann, 2021; Destradi & Plagemann, 2019; Plagemann & Destradi, 2019). Centrally, this literature has shown that parties do differ systematically regarding foreign policy preferences, based on a left-right spectrum (Hofmann & Martill, 2021, p. 309)—yet, the role of a rising far right has not been widely researched. Some research focuses on far-right foreign policy ideology (de Orellana & Michelsen, 2019; Varga & Buzogány, 2021) and positions (Ostermann & Stahl, 2022; Özdamar & Ceydilek, 2020), some on the specific policies followed when in government (Verbeek & Zaslove, 2015). Specific interactions with IOs has only rarely been studied (but see Müller & Gazsi, 2023).

Scholars have analyzed how the European far-right parties position themselves on foreign aid—most often with a focus on development cooperation (Ostermann & Stahl, 2022), and how it is involved in the politicization of the field (Hackenesch, Bergmann, & Orbie, 2021). Specifically, Bergmann, Hackenesch, and Stockemer (2021) show that radical-right parties indirectly shape development policies of governments (via electoral success and parliamentary seats), interlinking them with migration issues.

Some limited research on the specific effects of strong radical-right parties on (mostly bilateral) foreign aid-spending has been published recently. For example, Suzuki (Suzuki, 2023) analyzes the effects of radical-right parties' electoral successes (indirect effect) or their government participation (direct effect) on bilateral aid based on data of 17 Western European democracies. She shows a “punitive” effect in bilateral aid towards states with high emigration flows if radical-right parties participate in government. Hammerschmidt, Meyer, and Pintsch (2022) study the indirect (parliamentary seats) and direct (government participation) effects of radical-right parties on foreign aid more generally (net commitments of official development assistance based on OECD data for 25 OECD members). They show an overall reduction of foreign aid linked to strong radical-right parties—this effect is present both for electoral success and government participation of the radical right.

IO funding has, so far, only been part of one specific study. Hackenesch, Högl, Öhler, and Burni (2022) add a more fine-grained picture to these general findings. They study the direct and indirect effects of radical-right parties in 23 European countries based on DAC data with a specific focus on different types of foreign aid. They analyze different sectors, assuming that the radical-right might shift policy priorities away from politicized topics (such as climate change) and into the direction of others (such as migration). They also assume that a strong

radical right might be more skeptical of supporting international organizations. They show that, overall, stronger radical-right parties lead to a rise in foreign aid, which aims at the curtailing of migration—yet to a decrease on aid on fighting climate change. In addition, less core funding (multilateral aid) is spent. While they show such effects to be significant for indirect effects of radical-right parties (rise of voting share and parliamentary seats), they do not find a direct effect, i.e. when radical right parties participate directly in government and thus have (partly) control over how states spend their money.

In sum, research differs on their results on direct effects of radical-right government participation on foreign aid in general. It also overwhelmingly focuses on bilateral aid. While Hackenesch et al. (2022) show a general decrease of multilateral spending (core funding) vs. bilateral spending linked to strong radical-right parties, we do not know how this affects the highly important area of earmarked funding. In addition, our knowledge is limited as to the direct effects of governments with radical right parties on earmarked funding. In fact, earmarked funding would provide radical right government with a convenient way of limiting their multilateral commitments and signaling to their support base their policy consistency of avoiding multilateral institutions and not paying for non-natives.

Lastly, all studies take the same data on radical-right parties as the basis for their studies. PopuList offers an overview of populist far-right parties as well as Eurosceptic parties more generally, which is based on expert assessments. Its specific coding of parties as Far Right or not are, however, certainly debatable in some cases and do need a very detailed “re-check” of the available data as well as a broadening beyond the European cases—one we are offering in this article.

3 Theoretical expectations

We expect that governments with far-right parties participating in them will lower the amount of earmarked funding. The Far Right shares an ideological core around nativism and authoritarianism (Pirro, 2023). Nativism prioritizes natives over other groups, particularly those outside the imagined “native” territory. The authoritarian element, in turn, may nurture a skepticism toward multilateralism in general and international institutions and their authority in particular. The interference of third parties into national sovereignty, a feature of international authority,

and their management of funds from national coffers to be spent for others, non-natives, is at odds with far-right ideology. These policy positions regarding earmarked funding can be derived directly from the ideological core of the Far Right. We make a further assumption, which is often, however, contested. We assume that the policy preferences of the Far Right are sincere in that it will realize its preferences once presented with the opportunity. That is, we do not expect the Far Right to engage in cheap talk or target just discursive changes when it comes to multilateral development aid. Instead, we expect to observe direct and immediate effects.

Unlike direct bilateral aid or IOs' core funding, earmarked funding schemes provide the Far Right with the least costly way to realize their policy preferences. As earmarked funds are voluntary contributions to multilateral institutions, they can be easily rescinded without much consequences for the donor government. In other words, the institutional opportunities provided by earmarked funding schemes gives far-right parties that participate in government a strong impetus to follow-through on their policy priorities and preferences. As far-right parties shun multilateral institutions and their authority and want to avoid to pay into programs which prioritize non-natives, they will refrain from committing money to institutions they cannot directly control. This preference has to meet opportunity: While certain types of funding cuts need time to take hold or come with potential sanctioning, such as in the case of IO core funding, earmarked funding decisions can be altered more easily by governments. They thus represent an expedient and easy route to implement policy preferences and signal policy consistency to far-right constituencies.

Accordingly, we expect the following hypothesis to hold:

Hypothesis 1. *If the Far Right participates in government, this should lower earmarked funding contributions*

Based on the above reasoning, the Far Right in government should also not be interested in time-consuming and potentially costly recalibrations of aid conditions, meticulously defining strings attached to the earmarked funds. While this provides some sort of control for donor governments over IOs, governments do still entrust IOs with carrying out the development work for non-natives. This should further fuel the Far Right's reluctance to pay into multilateral settings. Instead of incurring the costs of formulating strings attached, the Far Right in government thus would directly proceed to cut earmarked funding. A corollary that can be derived is the following hypothesis:

Hypothesis 2. *If the Far Right participates in government, this should have no effect on the stringency of multilateral foreign aid conditions*

We also argue that far right parties have a specific preference not to use international organizations as intermediaries, which is not necessarily linked to a general rejection of using foreign aid. The latter can be an instrument to implement foreign policy goals. While ideology matters for foreign aid, it does rather in the long run and more indirectly (Thérien & Noel, 2000). Similarly, while far-right parties might have different preferences on how and where to spend bilateral aid than other parties, we do not assume that the far right in government leads to a general cut in direct bilateral foreign aid. Quite to the contrary: some far-right parties demand a re-allocation of bilateral aid, away from social and human development projects toward funding off migration. Austria's formerly governing Freedom Party (FP'Ö), for example, heavily argued for increasing the country's bilateral aid budget by redirecting funds to prevent migration.⁴ The Far Right may have incentives to re-allocate bilateral aid, or change the local recipients of their development aid that are often closer in political terms to the donors. Accordingly, we expect:

Hypothesis 3. *If the far right participates in government, this should not make the use of bilateral aid less likely*

4 Research design

To empirically examine our hypotheses, we develop a research design that tackles important inferential challenges and that allows us to draw broader conclusions about the far right and foreign aid. We therefore present in this section our measurement and modeling approach, discussing the data sources and coding to identify far-right parties and determine their government participation. Next, we review the data used to code the dependent variables of interest: earmarked funding, the stringency of earmarking, and direct bilateral aid. We show notable descriptive patterns and dynamics in the data that need to be taken into account when statistically modeling the relationship between the far right and foreign aid.

⁴Proposal brought to Parliament by FPÖ in September 2023. parlament.gv.at

4.1 Sample

Before we discuss the coding of Far Right parties, we present our sample of countries. To determine the appropriate sample, we start from the set of all countries that report their aid activities to the OECD’s Development Assistance Committee (DAC). As of 2020, 47 states reported their earmarked commitments to IOs for development purposes to the DAC. However, not all of these 47 states are eligible to be included in our sample. This is because at least ten of them are non-democratic countries and as such there is no variation to be expected in the probability of having a far-right party voted into government. This probability is either zero if the authoritarian government is non-radical right or one if the far right governs, such as in Russia.

Accordingly, we exclude all non-democratic governments and are left with 37 states that report to the DAC their multi-bi aid funding activity (see Eichenauer & Reinsberg, 2017). Our sample of states covers democracies, both established and new ones, with regular and competitive elections. We use data from Cheibub, Gandhi, and Vreeland (2010) and updates therefore to determine if a state is democratic or not.⁵ The overall time frame we investigate spans the years from 1990 until 2020. However, because several countries were not reporting to the DAC, either because they were not independent (e.g., Lithuania in 1990) or not participating in the OECD (e.g., Poland before 1996 or Greece before 1999), the begin of the observation period varies per country, with the first occurrence being the first year of DAC reporting (irrespective of whether the state was member of the DAC). The final year of observation is 2020.

4.2 The Far Right in government

To identify and code far-right parties, we follow the conceptualization proposed by Pirro (2023) and Mudde (2007). The conceptualization is used to identify the Far Right, consisting of radical right and extreme right parties. The Far Right differs from other party families by two distinct dimensions: nativism and authoritarianism. Nativism captures the idea that “states should be solely inhabited by members of the native group” (Pirro, 2023, p. 105) and, by implication, that politics should serve this native group. The authoritarian dimension, in turn, rejects political and social equality, and instead emphasizes the prevalence of (enduring power) hierarchies and

⁵A borderline case is Turkey from 2016 onward. Yet, excluding or including Turkey in our analysis has no effect on our main findings (see Appendix).

societal inequalities. Authoritarian ideology can both relate to the organization of the state, such as the avoidance of external and internal constraints on the exercise of executive authority, and to the social realm. For example, rejecting gender equality on the grounds that women have a particular role to play in society is an instance of authoritarianism.

Within the Far Right, comparativists distinguish between the radical and the extreme right. While the latter seeks to overthrow democratic politics, often with violent means, the former seeks to partake in democratic politics and change policies “from within”. In other words, the extent of nativism and authoritarianism, both conceptualized as continuous dimensions, are less pronounced in the case of radical right parties (RRP) than in the case of the extreme right. Examples of RRP include Hungary’s Fidesz or Italy’s Fratelli d’Italia (both governing currently), while Hungary’s Jobbik party or Italy’s former Movimento Sociale Italiano (MSI) would count as extreme right parties.⁶ As these two examples suggest, it is radical right parties that, if at all, participate in governments, and not the extreme right.⁷

We use three different sources to identify and code far-right parties and their government participation. These are: the expert-reviewed PopuList on populist parties (Rooduijn et al., 2019), the Comparative Manifesto Project (Lehmann et al., 2023), and the PPEG data on government compositions and national elections (PPEG, 2022).

The PopuList identifies populist parties in 32 European countries that have won at least one seat in the national legislature or at least 2% in national parliamentary elections since 1992. Using a broad definition of populism, the data includes parties with a left, right, and centrist ideology as well as those that position themselves critically toward the European Union without a specific ideological leaning. While most radical right parties (RRP) “can also be described as populist (in the sense of anti-elite ideology and rhetoric), this is not the case of all parties which are part of the far right.” (Mudde, 2007, pp. 22–27). To obtain the set of relevant parties, we first excluded all left-wing parties. Then, we went through the list of remaining parties that included extreme right (e.g., Greece’s Golden Dawn), radical right (e.g., Poland’s Law and Justice [PiS]), and other parties (e.g., Italy’s Five Star Movement), which were, however, coded as populist and/or far right or populist and/or euroskeptic in the original PopuList. We could

⁶In fact, some intra-party factions of MSI (including Giorgia Meloni) created in 2012 Fratelli d’Italia, and in the 1990s Alleanza Nazionale (Gianfranco Fini).

⁷There are few exceptions, such as Croatia’s Party of Rights that used to be a neo-fascist party participating for a certain time in the country’s government.

attribute most parties coded as far-right, but not as populist in the original data to the extreme right (with some exceptions). Those coded as both populist and far-right were mostly RRP.

However, several parties were only coded as populist without a further indication of their ideology. To be able to tell whether these parties are part of the radical right, we consulted the party family classification of the Manifesto Project. If a party was coded as belonging to the nationalist party family by the Manifesto data, we coded it as a radical right party (if it was not an extreme right party). We proceeded similarly in the case of parties coded in the PopuList as only euroskeptic (e.g., the UK's Democratic Unionist Party [DUP] or Iceland's Independence Party). In some cases, the Manifesto data helped to ascertain the party family of these parties and therefore their classification as an RRP or not. With respect to other remaining parties, some of which were conservative (like the Tories in the UK) that had either populist or euroskeptic leanings, we went through the detailed documentation and information provided by PartyFacts⁸ to ascertain if the parties were indeed radical right in their ideology and policy platform (as indicated by their stance on the role of the nation in politics, their position on migration, and their party-group affiliation in the European Parliament) or conservative. For countries in our sample that were not included in the PopuList, notably the non-European DAC participants (e.g., New Zealand or South Korea), we used Manifesto data and background information as provided by PartyFacts to determine if the parties participating in these countries' governments could be coded as RRP. The resulting list of parties includes for all countries in our sample all far right parties. That is, both RRPs and extreme right parties, while excluding centrist, mainstream right-wing parties (e.g., the UK's Conservatives), and parties without an ideological core.

To measure whether the far right participates in government, we use the PPEG. This dataset provides consistent and full data on elections and government composition for the sample of our countries and time frame. Bringing together our list of far-right parties with the PPEG, we obtain an indicator of whether such parties participated in government for all 37 countries over the entire period of observation. With one exception (Croatian Party of Rights), only radical right parties, and not extreme right ones, participated in government. Hence, we use the term RRP government participation.

⁸See <https://partyfacts.herokuapp.com>

From this combined data, we code our indicator of RRP government participation. To do so, we define RRP government participation to both include the presence of an RRP in the coalition forming the government, but also when the RRP leads the government. We neither count the number of RRP ministers, nor incorporate the relative strength of the RRP in the government. This is because we expect that irrespective of the relative strength of the RRP, it will have some leverage, particularly in coalition governments, over other governing parties and therefore try to prioritize its most preferred policies and pursue them within the government.

Electoral cycles and government inaugurations do not necessarily follow the rhythm of a calendar year. To be able to still match the participation of RRPs in governments to annual foreign aid decisions, we need to decide on the annual aggregation. This necessarily involves making assumptions about the prevalence of RRP governments in an election year or when governments change more than once within a year, such as when the government majority breaks down and a new one forms without calling new elections. For years in which the government did not change, the rule is simple: code if the government had an RRP or not. For years with several government changes, we decided to count the months in which an RRP participated in the government and then defined thresholds: if an RRP took part in the government for more than six months, we coded the entire country-year as having an RRP government. If the duration of the RRP government was less than or equal to six months within a calendar year, we code the entire country-year as having no RRP government. As a result, our main independent variable is a dichotomous annual indicator, taking a value of 1 if an RRP participates in government, and a value of zero otherwise. In total, about one-fifth of all observations in our sample had an RRP government, with significant cross-country and temporal variation (sample average: 0.21, sample standard deviation: 0.41).

Figure 1 plots our binary measure of RRP participation in government (henceforth RRP government) for our sample of 37 DAC-reporting states over time. As can be seen, the annual share of observations with RRP governments grows over time, starting from around 10% in 1990s to reach around 35% from 2015 and on. This pattern is congruent with the overall observed rise of the radical right in wealthy democracies. It appears that the increase in RRP government participation is marked by two waves: the first one occurring in the early 2000s and the second one from 2015 and on. Of note, in the late 1990s and mid-2000s many newly established democracies from Eastern Europe joined the OECD and/or started reporting their aid activities

within the DAC. Many of these had governments with RRP participation, contributing to the first rise of our indicator variable from 2000 to 2010. Indeed, around 70% of all observations in our sample from Eastern Europe had radical right parties in government (see Table A.2 in the Appendix). Starting from 2015, RRP became also increasingly involved in governments of Southern and Western Europe, adding to the further growth of their over-time prevalence among the OECD’s DAC members and participants.

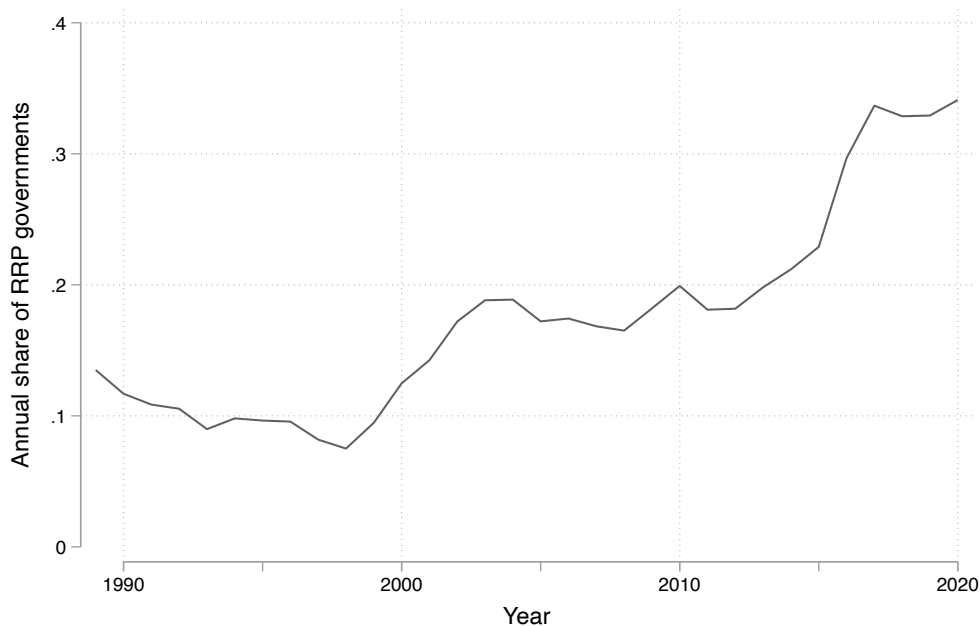


Figure 1: Annual share of RRP government participation over time. The time series represents the moving average of our binary indicator RRP government

4.3 Foreign aid

Having defined, coded, and described our main explanatory variable, we now discuss the data sources capturing earmarked funding, stringency of earmarks, and direct bilateral aid.

4.3.1 Earmarked funding

We use the earmarked funding data (EFD) developed by Reinsberg et al. (2023) to measure our main outcome variable. This is defined as the amount of monetary commitments, measured in millions constant US dollars, made by DAC donor countries to IO-managed development projects. The OECD translates the monetary commitments made by donor states to the annual level, unlike in the case of disbursements that often cannot be allocated ex post to a specific

year.⁹ Instead of looking only at the aggregate amount committed by state and year, we take also the specific thematic sectors or issue areas into account. The OECD differentiates between different sectors of aid activity, including funds allocated to strengthening recipient economies, direct relief and disaster aid, or funding for schools and health care. The OECD has several so-called sector codes that classify for which specific activity donors earmark their commitments. We use these sector codes and generate nine different issue areas to which varying amounts of monetary commitments are allocated. We do so to not only aggregate the thematic sectors/issue areas, but also to focus on some issues that are particularly relevant to researching the radical right and IO funding. Specifically, we use some sector codes that belong to larger issue areas, e.g., women’s reproductive health to Health, and create new issue areas. The issue areas we define are the following: Social (health, education, and social security), Political System (all funds for strengthening democratic institutions and civil society, including peacekeeping operations), Economy (all multi-bi aid for strengthening recipients’ economy), Public Utilities (includes infrastructure work and energy systems), Assistance (direct relief and emergency funds), Environment (aid to protect the environment, including climate change measures), and Other (all commitments made that the OECD does not further classify into its existing sector codes). These issue areas summarize the existing DAC sector codes into larger categories. We define two new issue areas by aggregating different sector codes: Migration and Human Rights. Migration includes *donor* costs for refugees along with funds expended for the facilitation of orderly migration and alien registration. Our Human Rights issue area comprises activities meant to strengthen reproductive health rights and measures, women’s right, children’s right, and strengthening human rights civil society organizations. With this classification of issue areas to which earmarked funding flows, we obtain as unit of observation the country–issue-area–year construct. That is, we observe earmarked commitments to IOs not only across states and years, but also across the specific issue areas.

Figure 2 shows the average earmarked funding commitments made across the nine issue areas.

While states commit on average 67 million USD to multi-bi aid (SD=279 million dollar), we see strong differences across issue areas. Most funds go to the Assistance issue area, summarizing

⁹Across the entire sample, the average monetary commitments amount to 67 million US dollars, while the average disbursements to 62 million USD. Per year, these two measures differ quite strongly in the data, while overall, on balance, they are similar, indicating that states on average disburse more or less what they committed.

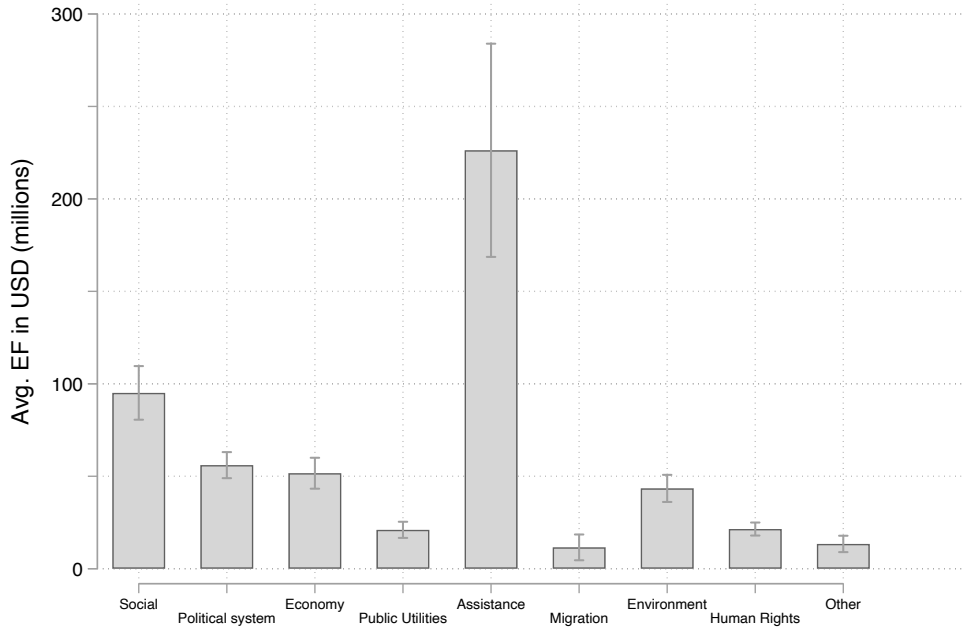


Figure 2: Average EF commitments across issue areas

all monetary commitments to direct relief and emergency assistance (states commit on average around 225 million USD to this issue area). This is followed by the funds committed to Social aid, involving education, health, and social security measures in recipient countries with average commitments of almost 100 million USD. The issue areas that have the lowest average earmarked funding (EF) commitments are Human Rights (21.45 million USD), Public Utilities (21 million USD), and Migration (11.5 million USD). In sum, Figure 2 shows that there is considerable variation in earmarked funding commitments across issue areas that should be taken into account when modeling the effect of RRP governments on EF commitments, not only to control for the unequal distribution of commitments across different aid activities, but also for capturing the possible reservations and hesitations of RRP governments to spend money for certain issue areas, such as human rights or environmental development (see Hackenesch et al., 2022).

4.3.2 Stringency and direct bilateral aid

The earmarked funding dataset offers another useful indicator to examine how RRP governments may affect international institutions. In particular, the EFD provides information on the conditions that states impose when committing funds to aid activities. States can choose to define geographic, thematic, or project-related criteria, for example, when making decisions about their donations. The EFD systematically measures these criteria and summarizes them, through

a weighted additive aggregation procedure, into a stringency index. This index informs about the extent of rules and criteria that donors specify, imposing thereby conditions on international institutions (and often recipients) on how to spend the money. We average the stringency index across our nine issue areas, per state, and year. The original index ranged from 1 to 7, where higher values indicate stronger strings attached by donor states on international agents. We rescale this indicator to range between zero and six, with zero indicating no to very low strings, and six the maximum possible number of strings. Our sample average of stringency is 2.4 (SD=0.87), indicating that strings are rather moderately distributed in our sample.

Finally, we take into account also the direct bilateral commitments made by OECD-DAC donors. As in the case of the earmarked funding, we not only record variation across states and years, but take the thematic sectors or issue areas of foreign aid spending into account. We obtain the data from the OECD’s Creditor Reporting System Aid Activity Database. After cleaning the raw data from the multi-bi aid flows that are channeled through IOs, we obtain the direct bilateral commitments made by donors per issue area and year. On average, the direct bilateral aid is much higher than earmarked funding commitments. Specifically, and across all issue areas, the average bilateral aid commitments amount to 432 million USD per state and year (standard deviation of 1024 million USD).

4.3.3 The joint distribution of RRP and foreign aid measures

We now briefly inspect the joint distribution of our RRP government participation, our main independent variable, and the earmarked funding commitments. Figure 3 shows the average amount of committed dollars for governments without radical right parties and for governments with participation of radical right parties.

The difference is remarkable. While non-RRP governments commit almost 75 million USD to earmarked funds, governments with RRP’s commit on average around 35 million USD. This difference of 40 million USD appears to be significant and applies across all nine issue areas we defined. When considering the differences between governments without and with RRP’s for each issue area, we see that in eight out of nine issue areas, RRP governments tend to commit less than governments without RRP’s (see Table A.3 in the Appendix). However, there is one exception: Migration. As it appears, RRP governments tend to spend more for refugee-related expenditures (that the donor governments incur) than non-RRP governments. This indicates

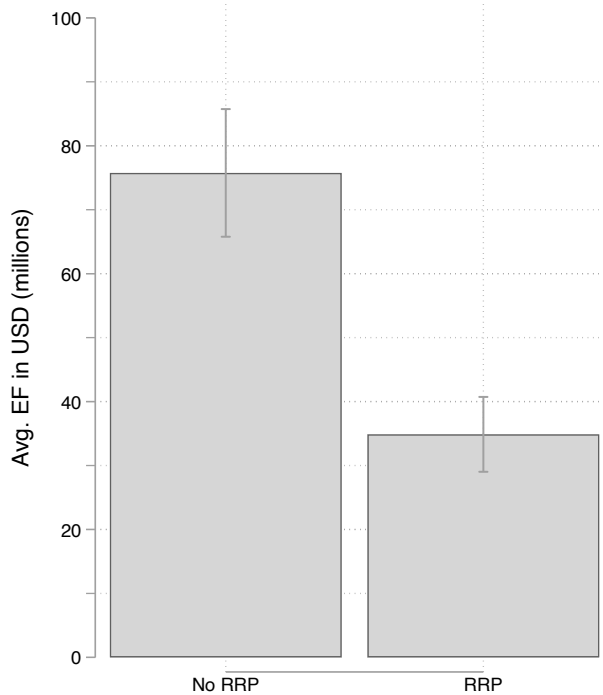


Figure 3: Average earmarked funding commitments for governments without radical right party (No RRP) and governments with RRP participation

that RRP governments declare funds they spend domestically as development aid rather than channeling them to recipients via IOs. However, the difference is not statistically significant.

Concerning descriptive data on the stringency index, we find no difference between RRP and non-RRP governments. Both appear to formulate conditions at a similar incidence, with non-RRP governments having an average stringency index of 2.38 (95% CI: 2.35, 2.41) and RRP governments one of 2.4 (95% CI: 2.34, 2.46). When we consider the unadjusted difference in direct bilateral aid between non-RRP and RRP governments, we find a very strong difference. On average, RRP governments seem to spend 270 million USD less than non-RRP governments. In particular, the estimated average bilateral aid for non-RRP governments is 483 million USD (95% CI: 448, 518), while the average bilateral aid for RRP governments is 213 million USD (95% CI: 178, 247).

While these differences appear remarkable, these descriptive statistics do not adjust for a series of confounding factors, both static and dynamic that may bias the associations. Accordingly, we invest considerable effort to correctly model the influence of RRP on foreign aid decisions. The next section discusses the specific measures we undertake.

4.4 Modeling the effect of RRPs

Whether radical right parties participate in government or not is certainly not determined by chance. Accordingly, and as typical for observational data, any differences observed between governments with and without RRPs in their foreign aid decisions may be confounded by a series of observable and unobservable factors and thus bias any comparisons. We focus on static and dynamic sources of such confounding biases and suggest an approach that enhances the comparability of governments with and without RRPs across countries, issue areas of foreign aid, and, importantly, time.

Static confounding describes the potentially biasing influence of relatively time-invariant factors on foreign aid spending at each level of our analysis. At the annual level, common shocks in a particular year to all DAC donors may raise the odds of RRP government participation and the propensity of donors to spend on foreign aid. For example, in the aftermath of the financial crisis of 2007/08, some RRPs in Europe entered government (e.g., Greece), while many governments faced severe debt crises. Similarly, the so-called European migration crisis in 2015 favored RRP government participation (e.g., Poland) and may thus have affected also foreign aid spending. At the level of thematic aid sectors or issue areas, the descriptive data already showed that some areas persistently receive much more earmarked funds (e.g., Assistance) than others (e.g., Environment). Relatedly, in direct bilateral aid, Social development projects and Public Utilities are allocated traditionally more funds than other areas. These are systematic and relatively time-invariant differences among the sectors to which foreign aid is allocated. Finally, we expect that time-invariant country differences may drive part of the variation in RRP government participation and foreign aid policies. All these static confounders, but also time-invariant factors that affect our outcomes but not necessarily RRP government participation, can be captured by respective sets of fixed effects that we include in our analyses.

While we address static sources of confounding through three-way fixed effects for the country, issue area, and year levels, we are left with another important source of confounding stemming from the longitudinal nature of our data. Our main independent variable, RRP government participation, is time-varying by definition, as is a series of time-varying potential confounders, such as GDP. These sources of dynamic confounding can imbalance our comparison groups, both over time and at each point in time, and hence bias our analyses. Concerning our time-varying treatment, the descriptive part showed that the prevalence of RRP governments varies markedly

over time. This not only indicates an imbalance between RRP and non-RRP governments at each point in time, but, perhaps more importantly, the possibility of dynamic feedback effects. These occur, for example, when past versions of the treatment affects the outcomes in t along with contemporaneous versions of it. The other, and more immediate source of dynamic confounding, are time-varying potential confounders and their relation to past and current treatments and outcomes. For example, GDP per capita is considered a potential confounding variable in many studies. In our case, it can both affect the probability of observing an RRP government, such as when falling average per-person income generates electoral frustration that favors radical right policy platforms, and by affecting the propensity of governments to mobilize domestic resources for development aid. Importantly, an RRP government affects a country's GDP per capita, which in turn has consequences for foreign aid funding decisions. In this case, GDP per capita is post-treatment. The usual recommendation to avoid post-treatment biases is to leave the affected variable out of the estimation and use only its pre-treatment values. However, as Blackwell (2013) shows, this practice can lead to omitted variable bias (OMV), reinforcing a pernicious trade-off between OMV and post-treatment bias.

Part of the dynamic confounding could be addressed by instrumental variables regression or a regression discontinuity design. However, regarding the former, finding a suitable instrument is notoriously difficult, depends on strong assumptions, and risks analyzing only a subset of the data. A regression discontinuity design would help resolve a series of endogeneity concerns. However, identifying a homogeneous cut-off value for the entire sample that would serve as threshold to differentiate treated from untreated units is challenging. Moreover, the available information from the data on governments and their composition would also be reduced as one would choose a smaller bandwidth within which to look for sufficiently comparable cases of RRP and non-RRPG governments.

To address these dynamic sources of confounding, mitigate the trade-off from OMV and post-treatment bias, and exploit the full information in our data, we use the toolkit offered by marginal structural models.

4.4.1 Marginal structural models

Marginal structural models (MSM) have been developed in epidemiology research (Hernán, Brumback, & Robins, 2000; Robins, Hernán, & Brumback, 2000) and adjusted for political

science first by Blackwell (2013). The idea is to pre-process the data in such a way as to make the probability of treatment independent of observed time-varying confounders (and of its prior treatment history). At the basis of MSMs lies the propensity score that is used to calculate inverse probability of treatment weights that balance treated (i.e., RRP governments) and untreated (i.e., non-RRP governments) units on observed (measured) variables (Rosenbaum & Rubin, 1983). These weights create a pseudo-population that mimics the data as if it would have resulted from a randomized experiment, always conditional, however, on the included observed potential confounding variables. Because MSMs balance the sample of treated and untreated units at each point in time, given the prior treatment and covariate history, the treated and untreated units are conditionally and sequentially independent at each point in time—with the conditions defined by the set of included confounders. The resulting pre-processed and weighted data can then be analyzed without adjusting for the time-varying confounders in the statistical models. In addition to leveling the observed differences between treated and untreated units, and also ensuring that they occur with comparable frequency in the data, MSMs reduce the risk of adjusting away the treatment effect by including too many controls. Moreover, MSMs reduce model dependency in important ways. Other variables, that increase precision of estimates or that capture fixed effects, can be easily included in the weighted MSMs.

MSMs allow us to tackle the time-varying nature of our “treatment” and its potential dynamic and static confounders. Although MSMs serve this purpose, while retaining the original sample size, they do not guarantee the identification of a causal effect. This is because we can only balance our data on potential confounders that are observed and measured. As a result, there is always the possibility that we omit, or cannot measure, a time-varying variable that could possibly confound the relationship between RRP government participation and foreign aid commitments. To the extent that the sample is balanced on observed potential confounding variables (and with further adjustments, such as fixed effects), we strengthen our identifying assumption, which is that RRP and non-RRP governments are sufficiently sequentially comparable (or exchangeable) given the set of observed confounders, both in the cross-section and over time. The MSMs we specify focus on the average treatment effect, which compares the hypothetical situation where all units would have been RRP governments (and their foreign aid) to the hypothetical situation where all units would have been non-RRP governments (and their foreign aid). The weights used to create our pseudo-population mimic and enable this

comparison, always conditional on the included covariates in our study (Cole & Hernán, 2008; Robins et al., 2000).

Potential confounders

We now discuss the procedure to estimate the weights that are used to balance our data and level out (some of) the observed differences between RRP governments and non-RRP governments. We begin by a brief presentation of the potential time-varying confounders included in our analysis. We focus mainly on economic and political factors. Concerning the economic factors, we take into account GDP per capita (in constant USD, with data from the World Bank), GDP (in constant USD, with data from the World Bank), and the unemployment rate (share of unemployed to total labor force, with data taken from the IMF). We assume that these variables affect both the likelihood of RRP participating in governments and decisions about development aid. While this may be clear in the case of the GDP and GDP per capita, we assume that higher shares of unemployment create a reservoir of dissatisfied voters that could be more easily mobilized by RRP. At the same time, if high unemployment occurs and persists in a country, governments (also non-RRPs ones) may have different policy priorities, focusing on domestic growth strategies and therefore eschewing foreign aid.

Concerning political factors, we suggest to distinguish between Parliamentary and Presidential democracies, to take into account the duration of OECD membership, code whether or not countries are full DAC members, and to measure the average duration of governments in a country. In general, it appears easier for RRP to partake in governments of parliamentary democracies as these typically have multi-party coalition governments. Yet, with frequently changing coalition governments, policy preferences for or against development may also change, affecting thereby overall earmarked funding commitments. We use data from Anckar and Fredriksson (2019) to determine whether the democracies in our sample are parliamentary or presidential.

How long states have been members of the OECD may affect both the incidence of RRP governments and their propensity to commit funds to development aid. The OECD has been created by established and wealthy democracies and hence its long-term members tend to have fewer governments with RPPs than younger democracies, particularly from Eastern and Southern Europe, which have a higher tendency to produce RRP governments. In addition, the older

(and often richer) OECD members give more funds to development aid than the OECD’s younger members. We measure OECD membership duration with data from the OECD.

Similarly, whether or not a country is a full DAC member can be related to both the occurrence of RRP governments and its total commitments to earmarked funding. Many Eastern European countries with pronounced RRP government participation were not full DAC members during the period of observation, and accordingly committed (and reported) less than DAC member governments without RRP.

Finally, we take an important variable into account which is closely related to the frequency of RRP governments in our sample and which may also have effects on foreign aid commitments. This is the average duration of a government within a state. The shorter the duration of a government, the more likely it is to observe a government with RRP participation. In turn, frequent government changes can also lower the ability of a state to make commitments to development aid and result in lower overall contributions.

Calculating dynamic weights for MSMs

Having discussed the potential time-varying confounders, we now estimate a set of weights that balance governments with RRP and without. To calculate these weights, we proceed in three steps. First, we estimate the propensity score for each observation. In our case, the propensity score expresses the probability of being an RRP government, conditional on time-varying confounders and past RRP government participation.¹⁰ Using the propensity score, we take its inverse to obtain the inverse probability of treatment weight. In the second step, and to minimize the range of the resulting weights, we standardize this inverse propensity score by the marginal probability of observing an RRP government.¹¹ We thus have calculated for each observation (country-year) an individual weight: $w_{ij} = \frac{\text{Marginal Probability of having RRP government}_{ij}}{\text{Propensity Score of having RRP government}_{ij}}$, with i being the country, and j being the year. In the third step, we calculate the product of this weight for each state across its period of observation. This gives us time-varying weights, supposed to balance treated and untreated units at each point in time.

¹⁰More specifically, we fit a logit model with RRP government (0/1) as dependent variable and as right-hand side variables the lagged RRP government indicator, the lagged values of all time-varying confounders as well as their baseline values (at the start of each case’s observation period).

¹¹More specifically, we estimate again a logit model with RRP government as dependent variable and its lagged value as independent variable.

Having calculated the time-varying weights, we need to inspect their distribution and if they achieve balance on potential time-varying confounders. Concerning their distribution, the average of all weights should be one or close to one and their maximum should be below 10 (Hernán et al., 2000). Our weights are very well-behaved, averaging at 1 and having a maximum value of 4. The weights distribution is also reasonable over our analysis time. Their mean across time is close to one and with the ranges increasing only at very late points in time (notably around 2018, see Figure A.4 in the Appendix).

The most important criterion to judge the suitability of the weights is whether they achieve covariate balance across treated and untreated units. Table 1 shows the balance achieved. Calculating covariate balance in longitudinal data requires a slightly different approach than just performing t-tests or checking the cross-sectional mean standardized difference. Blackwell (2013) proposed a procedure that adjusts for the history of covariates and treatment. This procedure regresses the lagged potential confounder on the other confounders at baseline and the lagged and contemporaneous treatment variables. The t-value on the treatment variable at t serves then to assess balance across the treated and untreated units. The cell entries indicate the respective t-values, where a t-value below 2 shows that the sample is balanced on that covariate. As can be seen in the column titled “weighted”, our weights balance all time-varying confounders across treated and untreated units. While the variables GDP and Parliamentary were not imbalanced before the weighting, all others were, with government duration, unemployment, and OECD membership duration being strongly imbalanced. The weights correct these systematic differences due to these observed confounders.

Table 1: Balance of time-varying covariates

Variable	Unweighted	Weighted
GDP	1.7	1.8
GDP per capita	2.1	0.6
Govt. duration	6.7	0.4
DAC	2.01	1
Unemployment	6	0.05
Parliamentary	1.5	1.4
OECD duration	4.2	0.5

With the balance achieved on time-varying confounders and the relative frequency of treated and untreated observations being made comparable over time, we now specify and fit MSMs.

In our case, these are weighted linear regressions that include our main explanatory variable and three sets of fixed effects. Because the data is weighted and balanced, there is no need to include time-varying confounders. Other variables, however, that are not necessarily potential confounders but that may affect just the outcome, can be included to enhance precision of estimates. One such variable is the extent of fiscal austerity. Austerity policies come along with major government cuts concerning public spending. It is not unreasonable to assume that tighter fiscal rules and lower government expenditures in the wake of austerity policies will also reduce development aid. We use data on states' structural fiscal balance from the IMF's International Financial Statistics and calculate their first difference to obtain a proxy for austerity. A positive first difference indicates higher austerity, while a negative first difference indicates less austere fiscal policies. While still debated, recent research also suggests that austerity may be related to the odds of RRP strength (Baccini & Sattler, 2023). We accordingly calculate a set of weights balancing also on austerity and include the analysis in the Appendix.

5 How RRPs in government affect foreign aid

We find that OECD governments with RRP participation systematically reduce earmarked funding to IOs, while they neither formulate stringent funding criteria nor cut direct bilateral aid. Our findings from the statistical analysis support our expectation that RRPs in government will directly target multilateral foreign aid channeled through IOs. This suggests that the radical right can accomplish two political goals with such action. They can expediently realize electoral promises to spend less for others and they can also curtail the influence of IOs that design and run development projects.

Tables 2 and 3 present estimates from our MSMs. While Table 2 compares estimates on the RRP variable for earmarked funding commitments and the level of stringency, the models in Table 3 contrast the effect of RRPs in government concerning earmarked funding and direct bilateral aid, respectively.

In Model 1 in Table 2 that focuses on contemporaneous effects of governing RRPs, we find that governments with RRP participation reduce earmarked funding commitments by 26%,

compared to governments without RRPs.¹² The coefficient is statistically significant. This supports hypothesis 1.

Table 2: MSMs with earmarked funding and stringency

	Earmarked Funding		Stringency Index	
	(1)	(2)	(3)	(4)
RRP	-0.30** (0.10)	-0.18* (0.11)	-0.03 (0.04)	0.03 (0.05)
RRP _{t-1}		-0.19* (0.12)		-0.11** (0.05)
Constant	1.10** (0.42)	1.14** (0.42)	1.72** (0.24)	1.75** (0.24)
R^2	0.64	0.64	0.32	0.32
F	78.90**	77.23**	31.32**	30.72**
N	4435	4435	4565	4565

Note: Marginal structural linear models with dependent variable earmarked funding commitments (Models 1 and 2) or stringency index (Models 3 and 4). All models include country, year, and issue area fixed effects. Lagged austerity included as control. Robust standard errors in parentheses. *Statistical significance:* * $p < 0.10$, ** $p < 0.05$.

Participating in government, the radical right contributes to reducing earmarked funding commitments by an important amount. This is a novel insight generated by our data and analysis that suggests that the effect of the radical right on international development is not diffuse or very narrowly confined to certain issues only (Hackenesch et al., 2022), but rather a systematic feature once RRPs have the chance to participate in government. Our results show that RRPs have a direct and sizable effect as they target earmarked funding, weakening thereby the financial and operational basis of IOs for development projects.

Model 2 in Table 2 takes into account also temporally lagged effects of RRPs. Governments with RRPs have both short-term and long-term effects on earmarked funding commitments. More specifically, RRP governments reduce in t the amount of earmarked funding commitments by 17%. In $t-1$, RRP governments reduce by a nearly equal magnitude the amount of earmarked funding commitments (17.4%). The total two-period effect is negative and statistically signifi-

¹²We obtain this percentage change by exponentiating the estimated coefficients on RRP. That is, $e^{-0.30} \approx 0.74$ and then subtracting it from 1, $1 - 0.74 = 0.26$.

cant, with an estimated coefficient of -0.37 , resulting in a total percentage reduction of 31%, distributed, though, over two periods. That is, the amounts committed by RRP governments are almost by a third lower than the earmarked amounts committed by non-RRP governments.

According to hypothesis 2, we expect that RRPs in government would not invest much time and effort to specify strings for how IOs should spend earmarked funds. We assess this hypothesis in Models 3 and 4 in Table 2. We distinguish again between the contemporaneous (Model 3) and the lagged effects RRPs in government could have on the stringency of their earmarking. We find no systematic association between RRP governments and more strings attached to IOs and their development work. While the coefficient on RRP in Model 3 is negative, it is statistically insignificant. In Model 4, the coefficient on the lagged RRP variable is statistically significant and negative, suggesting that RRPs reduce the stringency on earmarked projects. However, when we consider the total effect of RRP governments—that is, their effect in time t and $t - 1$ —we find no discernible effect as the total effect is -0.07 and statistically indistinguishable from zero.

Table 3 compares the effect of RRPs in government for earmarked funding commitments and direct bilateral aid. Models 1 and 2 present findings of RRPs' effect on the earmarked amounts committed to IOs. Echoing the findings from Table 2, we again find a substantively sizable and statistically significant negative effect of RRPs in government. According to Model 1, RRPs in government are associated with a reduction of earmarked funds by about 28%, compared to governments without RRPs. As before, the effect of RRPs is both in t and $t - 1$ negative and significant, suggesting that RRPs continue time their cuts to earmarked funds for IOs (Model 2). When we contrast these findings with patterns concerning direct bilateral aid, we find no systematic association between RRPs and funding cuts. While the coefficient of RRPs in government is negative according to Model 3 in Table 3, it is far from statistically significant.

The size of the coefficient on RRP in Model 3 is also, when compared to the corresponding estimate from Model 1 in Table 3, relatively moderate. It shows a tendency of RRP governments to cut bilateral aid by about 12% compared to governments without RRPs. However, as it is not statistically significant, this tendency could be due to chance. Tackling static and dynamic sources of confounding in the data, we find that our descriptive pattern from above, where RRP would on average pay much about 217 million USD less for direct bilateral aid than non-RRP governments, does not hold. This emphasized the importance of addressing a series of sources

Table 3: MSMs with earmarked funding and direct bilateral aid

	Earmarked Funding		Direct Bilateral Aid	
	(1)	(2)	(3)	(4)
RRP	-0.33** (0.09)	-0.19* (0.11)	-0.12 (0.07)	-0.12 (0.09)
RRP _{t-1}		-0.23** (0.11)		0.01 (0.09)
Constant	1.13** (0.43)	1.18** (0.43)	5.87** (0.32)	5.87** (0.32)
R^2	0.64	0.64	0.80	0.80
F	79.91**	78.18**	59.54**	58.03**
N	4399	4399	4399	4399

Note: Marginal structural linear models with dependent variable earmarked funding commitments (Models 1 and 2) or direct bilateral aid (Models 3 and 4). All models include country, year, and issue area fixed effects. Lagged austerity included as control. Robust standard errors in parentheses. *Statistical significance:* * $p < 0.10$, ** $p < 0.05$.

of potential biases. Findings from our MSMs clearly indicate that, across all issue areas of the OECD's foreign aid allocations, the radical right does not reduce bilateral foreign aid when in government. Concerning temporal lags, we find no systematic association between RRP governments in t or $t - 1$ and the level of direct bilateral aid. Our analysis thus does not provide enough counter-evidence against our hypothesis 3. Instead, our findings put more nuance to previous studies that suggested a direct effect of RRPs on direct bilateral aid (Hammerschmidt et al., 2022), or even in some issue areas only (Hackenesch et al., 2022).

We have made large strides to reduce dynamic and static sources of confounding, supporting our identifying assumption that governments with and without RRPs are sufficiently comparable to each other. However, as we analyze observational data, we cannot prove causality; instead we can take measures to increase confidence in our findings. To further enhance confidence in our findings, we provide in the Appendix a series of robustness checks that include the lagged dependent variable, that exclude some few outliers, and that re-estimate our MSMs with a different set of dynamic weights. Our main findings are robust to these additional checks. Finally, we run a sensitivity analysis using the E-value approach by VanderWeele and Ding (2017). We use the coefficient on RRP from Model 1 in Table 2 and calculate the E-value, which

is 2.04. This tells us that an unobserved confounder (that we ignore or omit) would need to be associated with both RRP government and earmarked funding commitments by a risk-ratio of 2 each, given the confounders we already adjust for, to explain away our estimate. This indicates that our initial analysis and MSM specification provides strong grounds to believe in the systematic nature of our findings.

Taken together, our findings from marginal structural models with three-way fixed effects clearly demonstrate that RRPs in government systematically and substantially reduce earmarked funding of IOs. It also appears that RRPs in government do not invest time and resources to specify strings for how IOs should spend earmarked funds; neither do RRPs that partake in a country's government reduce direct bilateral aid.

6 Next steps

When in government, radical right parties contribute to cut earmarked funding to international organizations. The radical right in government does not differ from government without their participation when it comes to the strings attached to earmarked funds or direct bilateral aid. This paper provides consistent and robust evidence for this association, across a diverse set of countries, thematic issue areas of foreign aid, and over time.

To research the mechanisms and identify which political rationales drive these dynamics, we will add comparative case studies. In particular, we choose country cases that represent typical cases (i.e., as predicted by our MSM). These include Austria, the UK, Denmark, Italy, South Korea, and Finland. Among these, we observe that radical right parties politicize differently foreign aid issues and accordingly adjust their electoral strategies. Some do not emphasize at all development cooperation in their electoral campaigns, but nonetheless proceed to spending cuts for earmarking when in government. Others, by contrast, promise their voters to generally reduce foreign aid. Lastly, other radical right parties very specifically target international institutions and their development projects, before they proceed to cutting earmarked funding. Elaborating on the political mechanism behind our RRPs' cuts to earmarked funding will provide additional insights into the direct effects of the Far Right on international institutions.

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Appendices

Appendix A Descriptive statistics

Table A.1: Descriptive statistics

Variable	Mean	SD	Min.	Max.	N
EF commitments (million USD)	67.2	278.8	0	6115.5	4667
Log EF commitments	2.001	2.5	-9.7	8.7	4532
RRP	0.21	0.41	0	1	4667
GDP (constant, billion USD)	1488.7	3140.6	13.2	19929	4667
GDP per capita (PPP, constant USD)	46523.9	14814.1	21470	120647.8	4667
Government duration (in months)	34	15	7	62	4667
DAC member	0.94	0.24	0	1	4667
Unemployment	7.1	3.6	1.7	27.5	4667
Parliamentary vs presidential system	0.8	0.4	0	1	4667
OECD membership duration	41.5	14.5	-8	59	4667
Austerity	-0.14	1.7	-9.7	8.4	4595
America	0.09	0.28	0	1	4667
Asia	0.05	0.2	0	1	4667
Europe	0.78	0.4	0	1	4667
Oceania	0.07	0.26	0	1	4667
Social	0.13	0.34	0	1	4667
Political System	0.13	0.34	0	1	4667
Economy	0.12	0.33	0	1	4667
Public Utilities	0.10	0.3	0	1	4667
Assistance	0.13	0.3	0	1	4667
Migration	0.028	0.16	0	1	4667
Environment	0.13	0.33	0	1	4667
Human Rights	0.11	0.31	0	1	4667
Other	0.11	0.31	0	1	4667

Table A.2: Distribution of RRP governments across detailed UN regions classification

	RRP		
	mean	sd	count
Australia and New Zealand	0.13	0.33	349
Eastern Asia	0.30	0.46	238
Eastern Europe	0.69	0.47	267
Northern America	0.00	0.00	414
Northern Europe	0.18	0.38	1452
Southern Europe	0.21	0.40	685
Western Asia	0.00	0.00	4
Western Europe	0.22	0.41	1258
Total	0.21	0.41	4667

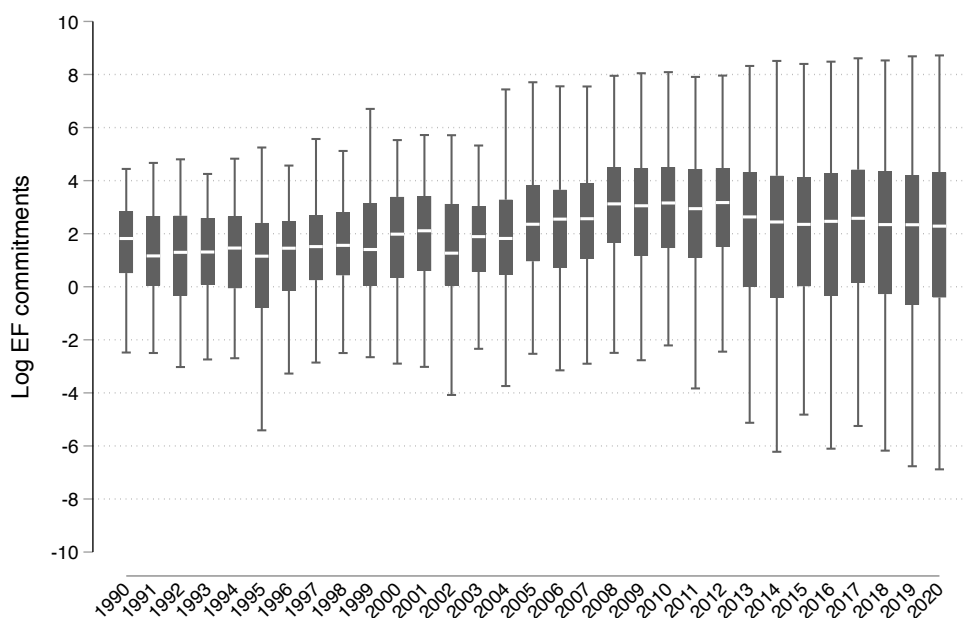


Figure A.1: Distribution of EF commitments over time

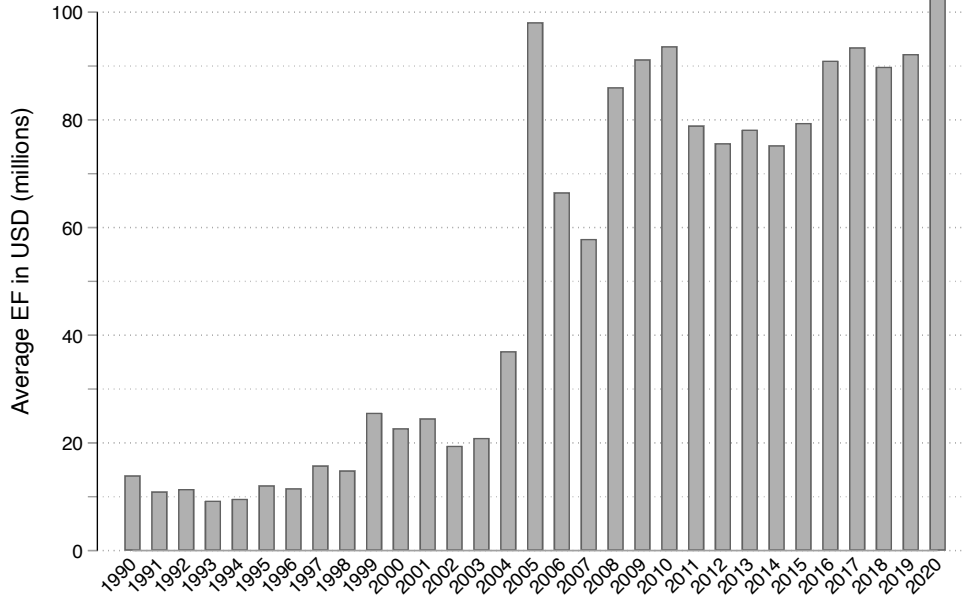


Figure A.2: Average EF commitments over time

Table A.3: T-test for difference in average EF commitments for governments without RRP and with RRP. Bold cell entries under t indicate significance at $p < 0.05$ level, italicized entries under t indicate significance at $p < 0.10$ level .

Issue area	EF No RRP	EF RRP	t	N
Social	105.37	56.27	2.72	630
Political System	60.69	38.65	2.52	612
Economy	56.54	32.14	2.31	577
Public Utilities	21.91	17.11	0.84	501
Assistance	270.28	68.38	2.86	592
Migration	10.22	16.29	-0.71	130
Environment	45.17	36.93	0.90	592
Human Rights	23.06	14.59	<i>1.86</i>	515
Other	15.25	7.47	1.47	518

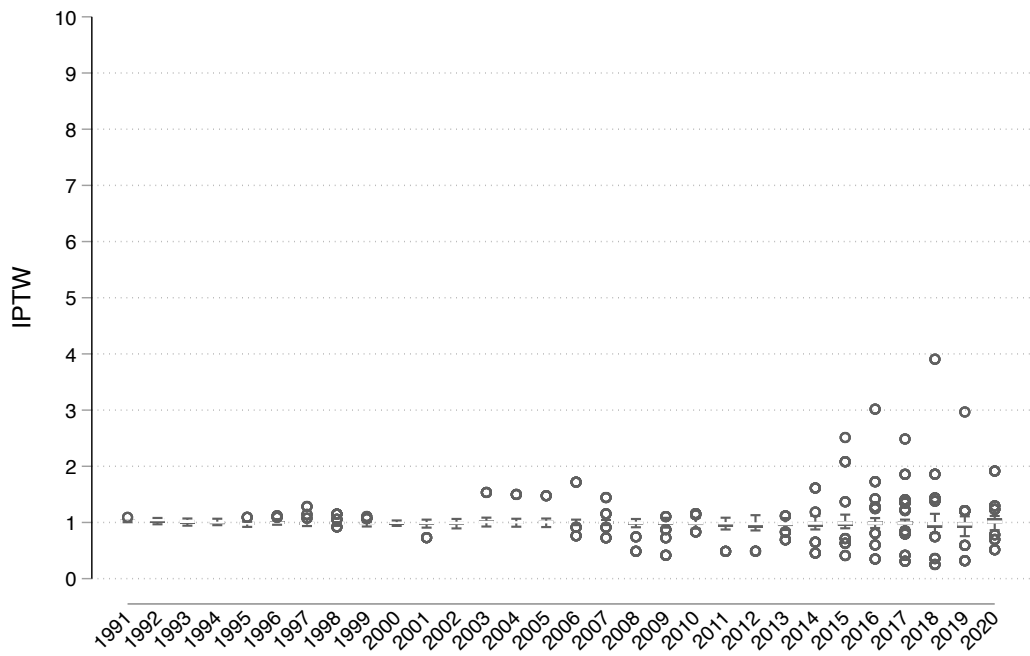


Table A.4: Distribution of Inverse Probability of Treatment Weights over time.

Appendix B Weighted analysis: robustness

In this section, we provide some additional analyses to assess the robustness of our main findings from the MSMs in the main text. First, we drop all sets of fixed effects and just run the baseline MSM. We then alter the set of fixed effects. Table B.5 shows the first set of MSM estimates. Model 1 is the baseline MSM, adding only the baseline values of the time-varying potential confounding variables and austerity. Model 2 adds fixed effects of the UN regions to which DAC-reporting states belong. Model 3 adds to these region effects also issue area fixed effects to take into account the varying EF commitments of states across thematic aid sectors.

We find across all three models that RRP government participation is significantly associated with a reduction in earmarked funding commitments. The percentage reduction is estimated to be around 25% (Model 1), and the estimates from Model 2 (24%) and 3 (23%) are very close to this.

Table B.5: MSM with region and issue area fixed effects

	(1) No FE	(2) Region FE	(3) Issue FE
RRP	-0.28** (0.13)	-0.27** (0.13)	-0.25** (0.13)
Constant	2.22** (0.16)	1.80** (0.27)	2.45** (0.26)
R^2	0.17	0.17	0.30
F	113.38**	83.65**	103.64**
N	4435	4435	4435

Linear MSMs, robust SE in parentheses.

Includes TVCs at baseline + lag of austerity

* $p < 0.10$, ** $p < 0.05$

Table B.6: Robustness 1

	(1) Lagged DV	(2) Ctr-IA FE	(3) - TR	(4) - CH	(5) Weights 2
RRP	-0.194** (0.0815)	-0.275** (0.0826)	-0.296** (0.0952)	-0.331** (0.0974)	-0.299** (0.0939)
Constant	0.614 (0.535)	0.333 (0.445)	1.100** (0.424)	1.044** (0.425)	1.211** (0.457)
R^2	0.713	0.728	0.641	0.644	0.638
F	85.22**	43.20**	78.92**	72.92**	83.00**
N	3891	4435	4431	4226	4435

Note: Linear marginal structural models with earmarked funding commitments as dependent variable. All models, except Model 2, include country, year, and issue area fixed effects. Model 2 includes year and country–issue-area fixed effects. Robust standard errors in parentheses. *Statistical significance:* ** $p < 0.05$.

Second, we change the model specification of our main MSM. In particular, we include the lagged dependent variable, specify more fine-grained country fixed effects, drop outliers, and present an alternative set of balancing weights. Table B.6 shows some robustness checks for our main models with earmarked funding commitments as dependent variable. Model 1 includes three sets of fixed effects (country, issue area, year), the lag of austerity, and the lagged dependent variable. Substantively, findings are similar to our main findings. Model 2 in Table B.6 changes the one set of fixed effects, specifying country–issue-area fixed effects. This is to control for unobserved time-invariant country and aid sector specific effects, such as when Norway traditionally spends most on the empowerment of marginalized groups in other countries. Findings are again in line with our main results in Table 2. Models 3 and 4 exclude, each, the outlier cases of Turkey and Switzerland. It is debated whether Turkey counts as a democracy after the failed coup attempt in 2016. Moreover, Turkey, despite the participation of a radical right party in government, increased remarkably its foreign aid, both direct bilateral and earmarked funding. In the case of Switzerland, the Swiss People’s Party (SVP) is the strongest political party, openly rejects multilateral development aid, and participates in the country’s all-parties government since decades. It thus represents a case with strong RRP governmental participation. Excluding Switzerland, though, contributes to making our estimated effect of RRP slightly stronger. Finally, Model 5 in Table B.6 uses a slightly different set of dynamic balancing weights. In particular, these weights also balance on austerity and its prior history.

Using this set of weights leaves our main findings intact. The coefficient estimate of -0.299 is very close to our main estimate in Model 1 of Table 2.