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The economics of the democratic deficit: The effect of IMF programs on inequality

Valentin Lang¹ 

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Abstract

Does the International Monetary Fund (IMF) increase inequality? To answer this question, this article introduces a new empirical strategy for determining the effects of IMF programs that exploits the heterogeneous effect of IMF liquidity on loan allocation based on a difference-in-differences logic. The results show that IMF programs increase income inequality. An analysis of decile-specific income data shows that this effect is driven by absolute income losses for the poor and not by income gains for the rich. The effect persists for up to 5 years, and is stronger for IMF programs in democracies, and when policy conditions, particularly those that demand social-spending cuts and labor-market reforms, are more extensive. These results suggest that IMF programs can constrain government responsiveness to domestic distributional preferences.

Keywords International Monetary Fund (IMF) · Inequality

JEL codes F53 · O19

1 Introduction

Over the course of the last decades, income inequality has been rising in many countries. While multiple factors have contributed to this development, there is now a broad consensus that changing national policies explain a substantial part of it (OECD 2011; World Bank 2016; World Inequality Lab 2017). Quite naturally, this prompts the question as to why so many countries changed their policies in a way that inequality increased. While the literature has often searched for answers by examining the pressures that economic globalization exerts on policies that ensure a more equal

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income distribution, this article turns to the *political* dimension of globalization. Its focus is on the pressures that international organizations as institutions of global governance exert on national economic policies in the globalized world. More specifically, it examines whether the activities of “the most powerful international institution in history” (Stone 2002, p. 1) – the International Monetary Fund (IMF) – contribute to the explanation for why inequality has been rising in so many countries.

The pressures that international organizations exert on national policies are rarely as strong as under the IMF’s loan programs. Since the IMF’s inception, its programs have been active in more than 130 countries. For many countries, some of the most fundamental economic reforms of their recent past were implemented under these programs (Reinsberg et al. 2019). This is largely due to the policy conditions that the IMF sets in exchange for its loans with a view to resolving balance-of-payment crises and correcting underlying macroeconomic and structural problems.

Pursuing these objectives, however, can translate into reforms with distributional effects. The actors that influence the design of IMF programs face the decision as to how to distribute the burdens of economic adjustment. As IMF decision-making has been shown to reflect the interest of its major shareholder governments and its staff (Copelovitch 2010; Dreher et al. 2015; Nelson 2014; Stone 2008), the design of IMF programs is influenced by the preferences of these actors. Not least because they are accountable to different audiences than national governments, their policy priorities can diverge from the preferences of national governments, which would decide on national policies more independently in the absence of an IMF program. To the extent that distributional policies implemented under IMF programs reflect the interests of the IMF’s major shareholders and its staff, IMF programs constrain the government’s responsiveness to the preferences of its domestic audience. As I argue in more detail below, inequality is thus more likely to rise under IMF programs than without IMF interference.

To investigate this empirically, the key challenge is to find a research design that allows comparing inequality trends under IMF programs to the counterfactual absence of an IMF program. Since IMF programs are not randomly assigned but usually take place during economic crises, simple comparisons of cases with and without programs would be plagued by severe endogeneity (Vreeland 2007a). Most approaches in the literature that address this problem rely on empirical strategies with problematic identifying assumptions. To solve this problem, I propose a new identification strategy for IMF programs inspired by recent methodological innovations (Nunn and Qian 2014). In a setting that is based on a difference-in-differences logic, I exploit the fact that changes in the IMF’s liquidity affect IMF loan allocation depending on a country’s history of participating in IMF programs. This relationship, which reflects bureaucratic incentives at the level of the Fund, is arguably excludable to country-specific economic outcomes like inequality. As the identifying assumption of this approach is likely to hold for other outcomes, the methodological section of this paper is also an attempt to provide the literature with a new tool to investigate the effects of IMF programs at large.

Foreshadowing the main results, I find IMF programs to increase inequality. Examining the persistence of the effect suggests that inequality remains heightened for up to 5 years. An additional analysis of new decile-specific income data suggests that the increase in inequality results from significant income losses for the poor, while there is no evidence for increasing absolute incomes for any decile. Consistent with the hypothesized mechanism, the effect is primarily driven by countries where IMF

programs are more likely to constrain democratic responsiveness to domestic distributional preferences. An additional analysis of IMF conditions finds evidence suggesting that inequality rises faster during programs that feature more extensive conditionality and that include social-spending cuts and labor-market conditions.

In light of these results, this article contributes to several literatures. First, it adds new findings to research on the IMF's distributional impact. Supporting previous studies that also find IMF programs to increase income inequality (Pastor 1987; Garuda 2000; Vreeland 2002; Oberdabernig 2013; Forster et al. 2019), it provides new evidence on the underlying mechanisms and shows that the increases in inequality result not only from relative but also from absolute income losses for the poor. The new evidence presented here suggests that these effects are causal. This is important because existing research on the IMF's effects often struggles with solving the problem of endogenous selection into IMF programs. The new identification strategy proposed here can thus be of help for future research on the causal effects of IMF programs.¹

Furthermore, the article links the analysis of the IMF's effects to the literature on IMF decision-making (e.g., Copelovitch 2010; Dreher et al. 2009; Schneider and Tobin 2020; Stone 2008). It argues that the distribution of decision-making power within the IMF, which this literature reveals, has direct distributional implications for the economies of the countries that the IMF influences. More generally, the paper speaks to the literature on the unintended effects of international organizations and official financial assistance. In particular, it supports scholars who point to adverse effects of international aid on governance (Knack 2000), scholars who are skeptical about the beneficial effects of aid on democratic institutions (Knack 2004), and scholars who emphasize that multilateral organizations can interfere with the functioning of domestic democracy (Gartzke and Naoi 2011). Lastly, the article adds a 'global governance' perspective to the growing literature on the causes behind increasing economic inequalities. While research often blames economic globalization for rising income inequality (Autor et al. 2013; Helpman et al. 2010), this paper shows that the political dimension of globalization also plays an important role for this contemporary trend.

The remainder of this paper proceeds as follows. The subsequent section 2 builds a theoretical argument based on the previous literature and derives testable hypotheses. Section 3 develops the new empirical strategy designed to identify the effects of IMF programs. Section 4 presents the main results and summarizes the robustness test, which are presented in more detail in Appendices A-H. Section 5 discusses the results and concludes.

2 Argument

2.1 IMF decision-making

International organizations like the IMF can be considered as sets of "nested principal-agent relationships" (Nielsen and Tierney 2003, p. 250). From this perspective, the IMF is part of a delegation chain starting with voters in member countries, the 'ultimate principal' (see also Vaubel 2006). The chain runs via national parliaments, governments, their representatives in

¹ Since an earlier version of this article became available as a working paper, several studies have borrowed the identification strategy proposed here (Forster et al. 2019; Gehring et al. 2019; Nelson and Wallace 2017; Schneider and Tobin 2020; Stubbs et al. 2020).

the IMF's executive board, and ends with the IMF's staff. There are two main reasons for why IMF decisions may reflect agent preferences that diverge from the preferences of voters in member countries that are affected by these decisions.

First, the governments of major shareholders have substantially more influence on the IMF than the governments of the countries that usually receive IMF loan programs. Empirical evidence for the disproportional influence of the US and other "G5" governments abounds (for reviews of this literature see Dreher and Lang 2019 and Vreeland 2019). The delegates of these governments have the largest formal voting power in the Executive Board, but even beyond formal votes they have a considerable impact on IMF policies through so-called "informal governance" (Stone 2008). Various channels of influence allow the US and other G5 governments to influence IMF decision-making in a way that it reflects their political (e.g., Dreher et al. 2018), geostrategic (e.g., Reynaud and Vauday 2009), and economic (e.g., Copelovitch 2010) interests. The governments of the countries that receive most IMF programs, on the other hand, tend to lack significant formal voting power, individual representatives in the Board, and substantial informal channels of influence (Kaja and Werker 2010).

Second, it is well documented that the IMF's policy decisions also reflect the particular interests of its staff. Due to high costs of information and control, and the ability of agents to exploit preference heterogeneity among multiple and collective principals, there is substantial 'agency slack' in international organizations like the IMF (Copelovitch 2010; Hawkins et al. 2006; Nielson and Tierney 2003; Vaubel 2006). This increases the ability of staff to pursue their own interests. Multiple studies observe IMF behavior that reflects staff interests like maximizing budgets, responsibilities, and autonomy, and find that IMF officials are able to push for longer programs, larger loans and more far-reaching conditionality than what is economically optimal (Barnett and Finnemore 2004; Copelovitch 2010; Lang and Presbitero 2018; Vaubel 2006). A second strand of this research shows that staff's ideological beliefs and policy preferences are also reflected in the IMF's policy decisions (Barro and Lee 2005; Chwieroth 2007a; Nelson 2014). These studies, inter alia, identify links between staff preferences for market-liberal policies and corresponding reforms in program countries.

In sum, major shareholder governments exploit their influence on the IMF to further their own political and economic interests, while staff shape the IMF's policy decisions in accordance with their material interests and ideological preferences. As will be discussed next, these preferences are often unlikely to align with preferences of voters in program countries when it comes to policy reforms with distributional implications.

2.2 Divergent priorities and distributional implications

Which policy preferences of major IMF shareholders and IMF staff can have distributional consequences in program countries? The existing literature suggests that the Fund's major shareholders have an economic interest in guarantees of debt repayments and cuts of public spending in program countries as this helps prevent financial losses for creditors from their country (e.g., Copelovitch 2010; Gould 2003).² Furthermore, to increase trade with and

² The same could arguably be achieved by raising taxes, but other preferences of major shareholders (e.g., increase investment opportunities for multinational firms) and IMF staff (e.g., limit the role of the state in the economy) make it more likely that this is achieved by cutting spending. See below.

opportunities for investments in these countries they also have an interest in other countries liberalizing their trade and financial policies (Woods 2006).³ In addition, multinational firms based in major shareholder countries have a commercial interest in less regulated labor markets, lower taxes, and privatizations in developing countries to produce more cheaply. Major shareholder governments will represent these interests if lobbied or convinced of beneficial effects for their economies. Consistent with this argument, large shareholder governments have been shown to influence the World Bank in accordance with commercial interests of multinational firms based in their countries (Dreher et al. 2019; Malik and Stone 2017). Similarly, IMF programs were found to be associated with subsequently rising flows of foreign direct investment from the United States and have been shown to benefit US commercial banks (Biglaiser and DeRouen 2010; Gould 2006).

For the IMF bureaucracy, the gradual expansion of the scope of IMF conditionality into policy areas where reforms are more ‘structural’ has often been linked to the bureaucratic incentive to expand the organization’s mission (Barnett and Finnemore 2004; Dreher and Lang 2019; Kentikelenis et al. 2016; Reinhart and Trebesch 2016). In policy areas like labor-market regulation IMF conditions go beyond setting quantitative benchmarks and instead include structural reforms (Reinsberg et al. 2019). This gives IMF staff more direct and detailed influence on policies (Babb and Buirra 2005; Kentikelenis and Babb 2019). It is consistent with this explanation that IMF staff also played an important role in strengthening the IMF’s focus on reforms in the area of social policy (Vetterlein and Moschella 2013). Furthermore, scholars have identified a strong tendency among IMF staff to favor market-liberal policies over government intervention in market processes and outcomes. As the IMF’s internal structure, hiring patterns, and organizational culture are typically described as stable, hierarchical, and monolithic (Momani 2005), scholars consider the market-liberal ideological preferences of its staff as highly stable over time (Chwieroth 2007a; Nelson 2014). As a result, there is substantial evidence suggesting that policies stipulating reduced public spending as well as trade and financial liberalization are associated to these ideological preferences of IMF staff (Barnett and Finnemore 1999; Chwieroth 2007a; Nelson 2014). It is furthermore worthwhile to add that preferences of IMF staff and major shareholders are not independent of each other. There is evidence suggesting that the United States played an important role in shaping the political orientation of the IMF bureaucracy (Kentikelenis and Babb 2019; Momani 2004).

In line with these arguments, studies show that IMF conditionality reflects these preferences and find conditions in these three areas – *cuts of public spending, trade and financial liberalization, labor-market reforms* – to be frequently included. According to Stone (2008, p. 600) “there is almost always some limit on public debt or government spending.” According to Kentikelenis et al. (2016), more than 70% of programs include conditions on trade and financial liberalization and about 50% set labor-market conditions.

But do these conditions lead to reforms in program countries? Even though not all IMF-mandated reforms are complied with, program countries implement many of the IMF’s conditions and their impact is measurable (Rickard and Caraway 2019; Stubbs et al. 2020). Through the threat to withhold loan disbursements, IMF conditionality rises costs for domestic political actors, e.g., parliaments, to block reforms under a

³ In a sense, the IMF is at the center of what Krasner (1985) called “structural conflict.”

program. As disbursement are in practice often withheld (Dreher 2006), this threat is credible. Furthermore, unpopular reforms become more likely as governments can use the IMF as a “scapegoat” and can “dilute accountability by blaming IMF conditionality” (Smith and Vreeland 2006). The IMF itself states that conditionality helps “strengthen [...] the hand of reformers” (IMF 2007, p. 8).

Empirically, IMF programs are indeed associated with policy reforms in the mentioned policy areas. They were found to come along with trade and capital account liberalization, (e.g., Chwieroth 2007b; Mukherjee and Singer 2010), cuts in the social sector and public wages (Kentikelenis et al. 2015; Nooruddin and Simmons 2006; Rickard and Caraway 2019; Stubbs et al. 2017), and less-regulated labor markets (Blanton et al. 2015; Caraway et al. 2012; Lee and Woo 2020). The latter includes minimum wage reductions, dismissals in the public sector, pension cuts, the legalization of nonpermanent labor, and the privatization of state-owned enterprises.

Which distributional effects should be expected from these reforms? The remainder of this paper examines the hypothesis that IMF programs *increase* inequality. This expectation is in line with existing evidence on the type of reforms that the IMF supports in the three discussed areas of public spending, trade and financial liberalization, and labor-market regulation: To the extent that IMF programs reduce public spending they can increase inequality by reducing the extent of redistribution and by affecting the distribution of gross income. Pension cuts or freezes, which are frequently included in IMF programs, may also increase inequality. Cuts in public wages could both increase and reduce gross inequality, depending on employment effects and the relation between public wages and median income. As regards the liberalization of trade and the capital account, most recent studies find inequality-increasing effects for capital account openness, FDI inflows, and composite measures of financial liberalization (de Haan and Sturm 2017; Furceri and Loungani 2018; Lang and Mendes Tavares 2018). The evidence on the effect of trade also points to inequality-increasing effects for many countries (e.g., Antràs et al. 2017; Autor et al. 2014; Goldberg and Pavcnik 2007). More generally, the fact that IMF programs restrict government expenditure during periods of economic liberalization limits the opportunities to ‘embed liberalism.’ As IMF conditionality often combines liberalization and austerity, vulnerable segments of society may lack the “compensations” for distributional risks that result from increasing openness (Rodrik 1998; Walter 2010). Typical IMF labor conditions like minimum wage reductions and weakening collective labor rights are, according to the literature, also likely to lead to higher gross inequality (Autor et al. 2016; Kerrissey 2015). Inequality may also rise if layoffs in the public sector and privatizations of state-owned enterprises increase unemployment.

In sum, the implementation of typical IMF conditions concerning social spending, liberalization and labor-market reform runs the risk of increasing inequality. In many countries, these reforms can mean a substantial departure from pre-program policy paths. Compared to a counterfactual scenario without IMF influence on national economic policies, inequality could thus rise if countries enter IMF programs.

The subsequent part of this paper tests the empirical implications of the theoretical argument. At the core of the analysis is the test of the overarching hypothesis that IMF programs increase inequality. In addition to standard measures of inequality, new global data of absolute income growth for different income deciles of affected countries are considered. This allows testing whether inequality rises because the poor lose or

because the rich gain in absolute terms. To investigate channels, heterogeneous effects are examined and the links between IMF conditionality and inequality are analyzed.

3 Method and data

3.1 Endogeneity of IMF programs

There is no lack of anecdotal evidence linking IMF programs to rising inequality. Many Latin American, East Asian, and former Soviet countries experienced a divergence in incomes while IMF programs were in place (Stiglitz 2002). An illustrative example is the case of Argentina, which was under one of the economically largest and longest IMF programs of all time. Democratic since 1983, Argentina received financial assistance from the Fund for almost the entire 1983–2004 period. Over the course of these two decades the country's Gini coefficient of net income rose from 38 to 45. Especially during the mass protests at the turn of the millennium many blamed this trend, as well as widespread poverty and unemployment, on reforms with origins in IMF conditions implemented by Carlos Menem's government. The IMF had demanded and supported policies such as fiscal austerity that resulted in wage and pension cuts, the privatization of state-owned enterprises leading to mass layoffs, and during the 1998–2002 recession opposed social programs for the poor and government plans such as increasing teachers' salaries (Klein 2008; Paddock 2002; Rodrik 2003). When the program ended after Argentina's last purchase of IMF resources in 2004, inequality started to decline and in 2013 the Gini coefficient reached 38 again.

While it is plausible that IMF programs contributed to rising inequality in Argentina, other simultaneous processes may explain this development just as well: The same period was also characterized by years of hyperinflation, economic crises, and high levels of debt – which, in turn, had made continued participation in IMF programs more likely in the first place. It is furthermore not excludable that Menem's government would have implemented similar free-market liberal reforms by itself in the absence of IMF influence and that the trend of decreasing inequality after 2004 is linked to the more egalitarian policies under Néstor and Cristina Kirchner's governments rather than to the end of the IMF programs.

The case of Argentina illustrates that the central challenge for any study investigating the causal effects of IMF programs on economic outcomes is nonrandom selection (Przeworski and Vreeland 2000). It is obvious that the economic and political conditions that explain selection into IMF programs are closely related to the economic and political outcomes of interest. Problematically, not all of the potentially confounding variables are observable. In addition to frequently missing data for variables that predict IMF program participation, the key problem is that many relevant conditions are intrinsically difficult, if not impossible, to measure. Vreeland (2002) lists “political will” as an example. Applied to the focus of this study, this argument suggests that governments that favor IMF programs, e.g., due to a political preference for austerity, could also be more likely to implement policies leading to higher inequality, irrespective of the presence of an IMF program.

Conceptually, there is a straightforward solution to this endogeneity problem, but to applied quantitative research on the IMF it presents a difficulty: “Instrumental variables

can address this problem, but they are not easy to come by, especially since so much of what drives selection into IMF programs also influences IMF program effects” (Vreeland 2007b, p. 82). So far, one strand of this research has either limited itself to correct for selection-on-observables (e.g., Hartzell et al. 2010), or additionally controlled for selection-on-unobservables by means of selection models without exclusion restrictions (e.g., Mukherjee and Singer 2010). The former studies do not control for unobserved confounders, while the latter have to make strong assumptions on the joint distribution of the error term and the correct specification of the participation equation.⁴

The other strand of research has incorporated exclusion restrictions in their empirical models (e.g., Barro and Lee 2005; Dreher and Walter 2010). In these studies, voting similarity with the United States in the UN General Assembly (UNGA) has become the ‘standard instrument’ for IMF programs.⁵ However, as the other IVs used in this literature, this measure is not clearly excludable to macroeconomic outcomes at the country-level.⁶ It rests on the assumption that IMF programs are the only channel through which a country’s UNGA voting behavior is linked to economic outcomes in the same country. But it is likely that a government’s foreign policy preferences articulated in UNGA voting are related to a government’s preferences in domestic policy, which in turn are clearly linked to economic outcomes.⁷ To paraphrase Moravcsik (1997), I argue that identification strategies should ‘take preferences seriously;’ especially since the authors of the most widely used UNGA voting data suggest that the data “can be interpreted as states’ positions towards the U.S.-led liberal order” (Bailey et al. 2017). The assumption that this political position is unrelated to domestic policies and the domestic economy is not plausible. Hence, a new identification strategy is needed.⁸

3.2 Identification strategy

A prominent finding in the literature on IMF loan allocation is that countries with a longer history of IMF program participation are more likely to receive IMF programs in the present. Variables that measure the time a country has spent under IMF programs in the past are robust predictors of the country’s present participation (Moser and Sturm 2011; Sturm et al. 2005). This pattern is sometimes attributed to “recidivism” since countries often come back to the

⁴ For details on problems of selection models without exclusion restrictions, see Puhani (2000).

⁵ Barro and Lee (2005) first proposed this IV.

⁶ Beyond UNGA voting, country-specific economic variables such as GDP, budget balance, inflation (Biglaiser and DeRouen 2010), growth, reserves (Bauer et al. 2012), exchange rates (Clements et al. 2013), trade with G5 countries (Barro and Lee 2005), have been used. But the assumption that such country-specific economic variables do not affect the respective country-specific economic outcome of interest other than through the presence of an IMF program is not plausible as more direct channels within the country’s economy cannot be excluded. A proposed alternative is the number of countries under an IMF program or the number of past IMF program years (Oberdabernig 2013). However, the former is correlated with global economic crises (and is multicollinear with year fixed effects), and the latter captures country-specific characteristics like weak economic governance (and is multicollinear with country fixed effects).

⁷ For theory, see Moravcsik (1997); for empirical evidence, see Mattes et al. (2015).

⁸ Of the existing studies on the IMF’s distributional effects Pastor (1987) conducts before-and-after comparisons, Garuda (2000) controls only for selection-on-observables, Vreeland (2002) addresses selection-on-unobservables without an exclusion restriction, and Oberdabernig (2013) relies on the excludability of UNGA voting. Forster et al. (2019), who cite an earlier version of this paper, use a strategy building on the IV introduced in this paper.

IMF soon after their programs end (Bird et al. 2004).⁹ Bureaucratic preferences inside the IMF are also likely to contribute to this pattern. As IMF staff have been shown to prefer working with like-minded policymakers in program countries, collaborations with countries whose policymakers the IMF is familiar with become more likely (Chwioroth 2007a; Dreher and Lang 2019; Nelson 2014).

In this paper, I detect a specific heterogeneity in the link between past IMF participation and present IMF participation and exploit it for a new identification strategy. I find that this relationship crucially depends on the IMF's liquidity. In years in which IMF liquidity is relatively low, IMF programs frequently go to countries that have received them more often in the past. But this link is substantially weaker in years in which the IMF's liquidity is relatively high: Then, past IMF participation is a much weaker predictor of present participation.

I describe this finding in more detail before I defend the identifying assumption. On the one hand, the finding supports the view that the IMF has a regular clientele that is routinely supplied (Bird et al. 2004; Reinhart and Trebesch 2016). A measure of past IMF participation – *IMFprobability*, defined as the fraction of years the country has been under an IMF program since the start of the observation period – robustly predicts present IMF programs. On the other hand, the finding also shows that the Fund is more likely to grant loans to countries beyond its more regular clientele when it has abundant liquid resources. This is in line with previous research emphasizing that bureaucratic incentives inside the Fund have contributed to the expansion of the IMF's scope of activity since the 1970s (Barnett and Finnemore 2004; Dreher and Lang 2019; Reinhart and Trebesch 2016). In times of high IMF liquidity, IMF staff have both the financial means and an increased incentive to more actively look for additional clients. There is a bureaucratic incentive to ensure the IMF's relevance by promoting participation in its programs. This incentive is particularly strong when the IMF's financial resources are relatively little used.¹⁰ This can explain why in years with high IMF liquidity the link between a country's history of IMF participation and its likelihood of present program participation is substantially weaker than in years with low IMF liquidity.¹¹

The identification strategy is thus based on a difference-in-differences logic. Differences in IMF liquidity lead to differences in the link between past IMF participation and the likelihood of receiving an IMF program. This relationship is captured by regressing the endogenous treatment variable (*IMFprogram*) on an interacted instrumental variable in the first stage of a 2SLS-regression:

$$IMFprogram_{i,t} = \alpha_1 (IMFprobability_{i,t} \times IMFliquidity_t) + \alpha_2 IMFprobability_{i,t} + \mathbf{X}'_{i,t} \alpha_3 + \delta_i + \tau_t + u_{i,t}$$

⁹ See Knack (2000) for the related concept of "aid dependence."

¹⁰ Personal conversations with IMF staff (Washington DC, November 2017) support this. Staff suggested that colleagues whose countries are under IMF programs become more important within the organization. They also suggested that the IMF's re-designing and re-labelling of lending facilities in recent high-liquidity years is an attempt to make programs more attractive for new potential program countries.

¹¹ Note that this does not imply that past IMF participation will not predict present IMF participation in high liquidity years, it would just be a less strong predictor in these years.

Here, *IMFprogram* is a binary variable indicating that country *i* was under an IMF program for at least 5 months in year *t* (Dreher 2006, updated). *IMFprobability* measures the country's history of participating in IMF programs and is defined as the fraction of years the country has been under a program between 1973 and year *t*. *IMFliquidity* is the natural logarithm of the IMF's liquidity ratio, defined as the amount of liquid IMF resources divided by liquid IMF liabilities.¹² *X* is a vector of control variables that are described below. δ_i and τ_t stand for full sets of country and year fixed effects.

Figure 1 visualizes the result of this first-stage regression.¹³ It shows that in years with higher IMF liquidity the probability of past IMF participation is a substantially weaker (even if still positive and significant) predictor of IMF programs. In these years, the Fund is more generous¹⁴ and implements more programs for countries beyond its more regular clientele than when liquidity is lower. This pattern is exploited for identification.

The key feature of this approach is that only the isolated interaction effect is used as a source of exogenous variation (Bartik 1991; Nunn and Qian 2014). The constituent terms of the interaction are controlled for in both stages of the 2SLS-regression.¹⁵ As in other approaches that are based on a difference-in-differences logic, threats to the identifying assumption can therefore only result from a specific pattern: Even if there was endogeneity between the IMF's liquidity and inequality, the exclusion restriction would only be violated if the unobserved variables driving this endogeneity were affecting inequality differently in countries with different levels of IMF participation history (for details Nizalova and Murtazashvili 2016).

To demonstrate why this is unlikely, Fig. 2 plots the temporal variation of the IMF's liquidity along with inequality trends in countries with low and high IMF probability. The main sources of the variation in IMF liquidity are the IMF Quota Reviews.¹⁶ The Articles of Agreement (Art. III, 2a) require the Board of Governors to review the amount of financial resources members commit to the Fund ("quotas") once every 5 years. In the observation period, these reviews led to liquidity increases in all but three cases. Once the quota increase is decided, members commit more resources, hence causing a jump in the Fund's liquid resources. In Fig. 2 these jumps can be seen, for instance, in the late 1970s, early 1980s and late 1990s when member countries executed their respective payments of the 7th, 8th, and 11th General Review of Quotas. As the timings of the quota reviews follow this institutional rule and are thus predetermined, the timing of these spikes is thus plausibly exogenous to inequality trends and related economic trends in individual countries. To support this empirically, Figs. 7–10 in Appendix G show that global economic cycles (global growth, global crises) are independent of and not correlated with IMF liquidity. More importantly, even if this was the case, such a correlation would bias the result only if it was dependent on a country's *IMFprobability*. This is why the dashed lines in Fig. 2 also show inequality trends in low- and high-

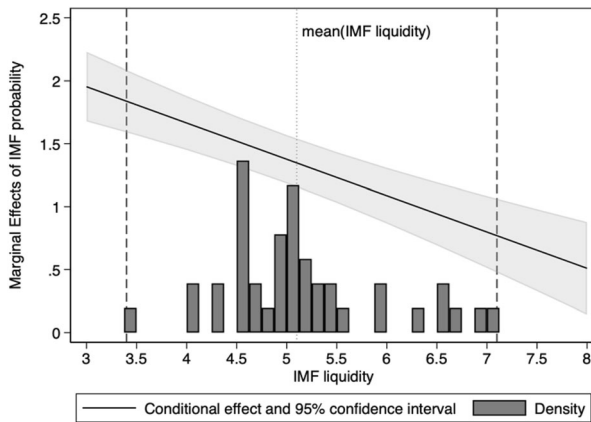
¹² For further details on this variable and on all others see below and Appendix A.

¹³ Detailed results, including tests of instrument relevance, are reported in the Section 4.

¹⁴ The liquidity ratio, which is not included in the regressions because of multicollinearity with year fixed effects, is positively correlated with the yearly count of program countries ($r = .3$).

¹⁵ Note that the year fixed effects control for the level effect of *IMFliquidity*.

¹⁶ A second and less important source of variation is the fact that in some years, individual, extraordinarily large transactions affect liquidity liabilities. In the Section 4.6, I show that this is unproblematic and that the results hold when this variation is excluded.



Note: Marginal effect plot of the first stage. Indicates marginal effects of *IMFprobability* on *IMFprogram* for different levels of *IMFliquidity*. Based on specification 1 in Table 1 (see below). The histogram plots the density of *IMFliquidity* over time.

Fig. 1 Visualized effect of the IV

probability countries. These trends are close to parallel and none of them is correlated with a trend in IMF liquidity.¹⁷

In sum, it is unlikely that there are unobserved variables that affect a potential correlation between the IMF’s liquidity and income inequality conditional on how regularly a country has received IMF programs in the past. The fact that all regressions include two-way fixed effects and hold for varying vectors of control variables further reduces this likelihood. Furthermore, several robustness tests, which are designed to challenge the identifying assumption, fail to produce different results.

3.3 Empirical model and data

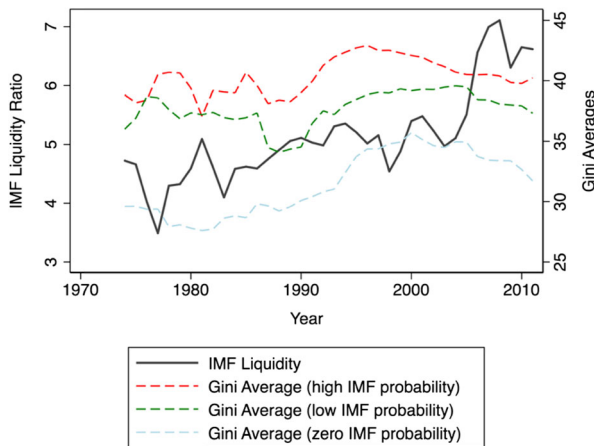
Based on this strategy designed to isolate quasi-exogenous variation in IMF programs, the second stage of the 2SLS panel regressions is specified as follows:

$$Inequality_{i,t} = \beta IMFprogram_{i,t-1} + \gamma IMFprobability_{i,t-1} + \mathbf{X}'_{i,t-1} \mu + \delta_i + \tau_t + u_{i,t}$$

In the baseline, I follow the related literature on IMF program effects and lag the variable by 1 year. To look at longer-term effects, I introduce different lags in additional regressions.

In the baseline, the dependent variable *Inequality* is the Gini coefficient of net income taken from the Standardized World Income Inequality Database (SWIID). The SWIID combines source data from multiple databases and, in contrast to other datasets like All The Ginis (ATG), standardizes them to ensure comparability across countries and over time. As the SWIID is widely used in related research (Acemoglu

¹⁷ This is relevant because Christian and Barrett (2017) show that the identifying assumption of such approaches can be violated if these trends are non-parallel and some are correlated with trends in the time-varying component of the interacted IV. For further clarification, Figure 5 in Appendix G illustrates a fabricated scenario in which the identifying assumption could be violated. Furthermore, Figures 7–10 in Appendix G show that the IMF’s liquidity is not correlated with global economic cycles as measured by global GDP growth or the global number of banking crises.



Note: The figure plots the temporal variation of the IMF's liquidity. The dashed lines show the year-specific crosscountry averages of Gini for sets of countries with zero, above-median, and below-median values of IMF probability.

Fig. 2 The IMF's liquidity ratio

et al. 2015; Dorsch and Maarek 2019; Oberdabernig 2013), I follow this literature in choosing the SWIID in the baseline, but show that the results are robustness to using ATG (Appendix G).¹⁸

Going beyond the Gini coefficient, I additionally use data on absolute income for all ten deciles of a country's income distribution from the Global Consumption and Income Project (GCIP). This allows determining whether inequality changes because of absolute income losses at one end or income gains at the other end of the income distribution.

A lagged vector of covariates consisting of two variable sets is added to the regressions.¹⁹ The first comprises the standard covariates of inequality: *GDP* per capita and its square to control for the country's level of economic development including a potential non-linear relationship à la Kuznets (1955) as well as *Education*, measured by average years of schooling, *Trade (% GDP)*, *Life Expectancy* and *Regime Type*.²⁰ The second set of covariates includes variables that the literature identified as key determinants of IMF programs: *Current Account Balance (% GDP)*, *Investments (% GDP)*, *GDP Growth*, *UNGA Voting*, and an indicator for the presence of a systemic *Banking Crisis*.²¹

To enhance the plausibility of the exclusion restriction I additionally add the two interactions "*Global Number of Banking Crises x IMFprobability*" and "*Global GDP Growth x IMFprobability*" as controls. This accounts for the potential concern that global cycles of growth and crises could influence both the IMF's liquidity ratio and inequality differently in countries with different IMF participation histories (see

¹⁸ This robustness test is relevant as the SWIID's imputation approach is sometimes criticized (Jenkins 2015). In contrast to the SWIID, the ATG database does not impute any missing values. The use of data on income inequality for a large panel of countries comes with an inescapable trade-off between data quality and data coverage. While the use of the SWIID prioritizes data coverage, the use of the ATG prioritizes data quality.

¹⁹ For descriptive statistics, definitions, and sources of all variables see Appendix A.

²⁰ Regime Type is based on the Polity IV index and codes countries with a score of 6 or higher as democracies (Marshall et al. 2011).

²¹ A robustness test in Appendix G adds *Debt (% GDP)* as an additional control variable.

Appendix G for details on this point; this appendix also reports additional empirical exercises that further address this potential concern). As current levels of inequality are heavily dependent on previous levels I follow the standard in the literature and include the lagged dependent variable.²² The annual data cover the 1973–2013 period and a maximum of 155 countries.

4 Results

4.1 First-stage results

The results reported in panel B of Table 1 demonstrate that the instrument is relevant. In the first-stage the IV enters with a negative coefficient that is statistically significant at the 1 % level. Jointly interpreted with the positive coefficient on *IMFprobability* this reflects the relationship described and illustrated in Fig. 1 above: *IMFliquidity* reduces the positive association between *IMFprobability* and *IMFprogram*. Underidentification is rejected at the 0.1% level and the Kleibergen-Paap F-statistics comfortably surpass conventional levels of weak identification tests.²³

These results are robust across specifications without covariates²⁴ (column 1), with standard covariates of IMF programs (column 2), and with standard covariates of inequality (column 3). It holds for all results reported in this paper that adding control variables does not substantially affect the coefficients of interest. This supports the argument that the identification strategy is able to isolate quasi-exogenous variation. The results of alternative first-stage specifications designed to challenge this argument are reported in the Section 4.6 and in Appendix G.

4.2 Second-stage results: IMF programs increase inequality

The baseline results of the second stage are reported in panel A of Table 1. They show that IMF programs, on average, increase income inequality. Across the three specifications with and without control variables, the coefficient is statistically significant ($p_{(1)} = 0.030$; $p_{(2)} = 0.010$, $p_{(3)} = 0.018$) and substantial in size. Participating in an IMF program increases the country's Gini coefficient of net income in the subsequent year by a little more than one point.

The magnitude of this effect is equivalent to an increase in the Gini coefficient by 34 to 51% of a within-country standard deviation. As inequality is slow to change, increases of this size within 1 year are relatively rare events (9% of all observations in the sample). Since differences in the Gini coefficient are difficult to interpret directly, an elaboration on a method proposed by Blackburn (1989) yields a more intuitive assessment of the effect size; on average, the change in inequality induced by receiving

²² See Dorsch and Maarek (2019) and Oberdabemig (2013). As $T > 20$, a potential Nickell bias is negligible (Beck and Katz 2011). A Fisher-type augmented Dickey-Fuller unit-root test rejects that *Inequality* has a unit root. The results are robust to excluding the lagged dependent variable.

²³ Most commonly used are the Staiger-Stock threshold of 10 and the most conservative Stock-Yogo critical value of 16.38, which tolerates a maximum 2SLS size distortion of 10%.

²⁴ *IMFprobability*, the lagged dependent variable, and the fixed effects are always controlled for.

Table 1 IMF programs and income inequality

	(1)	(2)	(3)
	Panel A: Second Stage		
IMF program _{t-1}	1.130** (0.521)	1.319** (0.515)	1.338** (0.565)
	Panel B: First Stage		
IMF liquidity x IMF probability _{t-1}	-0.276*** (0.052)	-0.311*** (0.059)	-0.367*** (0.069)
Inequality Controls	No	Yes	Yes
IMF Controls	No	No	Yes
Observations	3766	2985	2573
K-P underidentification test (p)	0.000	0.000	0.000
K-P weak identification (F)	27.699	27.422	28.330

Note: 2SLS regressions. Dependent variable is the *Gini* coefficient of net income. All regressions include country fixed effects, year fixed effects, the lagged dependent variable, and *IMFprobability*. Standard errors, clustered at the country level, in parentheses

Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$

See Appendix C for the full regression output

an IMF program in the previous year is equivalent to a transfer of four to 5% of the poorer half's mean income to the richer half (Appendix B).

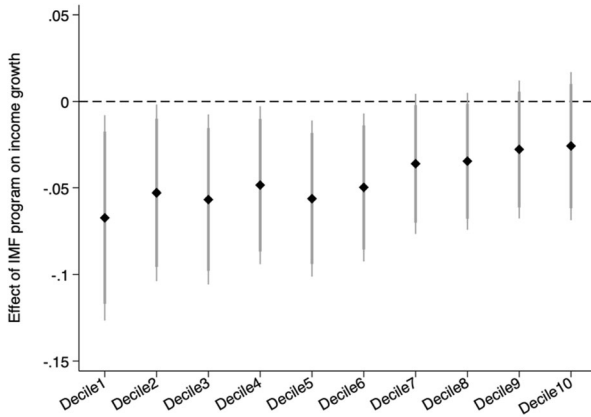
4.3 Decile-specific effects and persistence

The next set of regressions aims to determine whether these increases in inequality result from income gains for the rich or from income losses for the poor. For this purpose, I use country-year-decile-specific income data from the GCIP to calculate annual growth rates of income for each decile of a country's income distribution. These growth rates are then used as dependent variables in specifications that are otherwise identical to those reported in Table 1. The results are plotted in Fig. 3 (and reported in Appendix D).

For the bottom deciles, point estimates are statistically significant and indicate negative income growth effects of IMF programs of about five percentage points. For the top deciles the point estimates are not statistically significant at conventional levels. The throughout negative coefficients support previous studies that find negative average growth effects (Barro and Lee 2005; Dreher 2006; Przeworski and Vreeland 2000) of IMF programs and challenge those that find positive growth effects (Bas and Stone 2014). While confidence intervals are too large to compare the size of the income losses across deciles, these results allow drawing one key conclusion: Under IMF programs inequality rises because of falling incomes for the poor and not because of rising incomes for the rich.

Then, I examine longer term effects. Figure 4 and Table 2 report the effects of a year under an IMF program for different levels of lags. Consistent with the expectation that these effects need some time to fully materialize, the positive effect is statistically significant for the 5 years following a year under a program and strongest and most significant after 3 years. After 6 years the effect is no longer significantly different from zero.

These estimated lagged effects are based on regressions that lag the treatment variable *IMF program* by one to 6 years. As IMF programs usually last more than



Note: The figure plots coefficients of IMFprogram in regressions that are based on specification 1 in Table 1, where the dependent variable is substituted by the income growth rate of income deciles 1–10. 95% and 90% confidence bands in grey. See Appendix D for the full regression output

Fig. 3 Decile-specific effects

1 year – in my sample the average program length is 4 years – these lagged effects of program years are estimated based on programs that are either ongoing or that already ended. In Appendix E, I thus examine ongoing IMF programs separately and find that their estimated lagged effects are somewhat larger than the ones that include completed programs. An alternative way to analyze the pattern over time is to look at the start of an IMF program and its lagged effects. I also examine this in Appendix E and find that estimated lagged effects of program starts are very similar to lagged effects of program years. This suggests that much of the effect is driven by the early program period.

4.4 Heterogeneity

Next, mechanisms are analyzed. A first step examines a heterogeneous effect that supports the theoretical argument that IMF programs constrain government responsiveness to domestic preferences. A second step examines the role of IMF

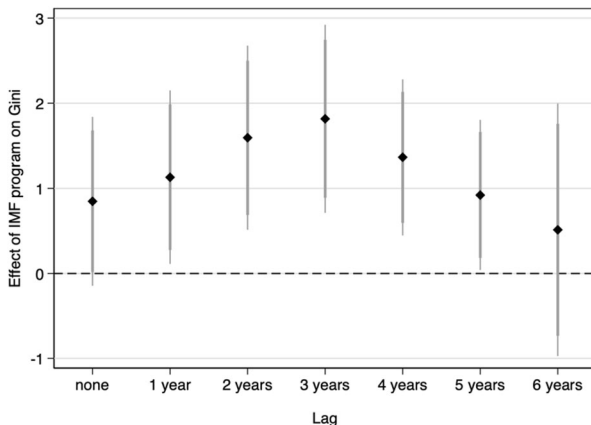


Fig. 4 Long-term effects

Table 2 Long-term effects

	t	t-1	t-2	t-3	t-4	t-5	t-6
IMF program	0.847* (0.506)	1.130** (0.521)	1.593*** (0.552)	1.816*** (0.564)	1.363*** (0.468)	0.920** (0.450)	0.511 (0.758)
Observations	3766	3766	3726	3685	3643	3598	3556

Note: Coefficients for different lags of *IMF program* from regressions that are otherwise identical to specification 1 in Table 1. Standard errors, clustered at the country level, in parentheses. The figure (Fig. 4) plots these coefficients along with 95% confidence intervals

Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$

conditionality for the effect. Appendix H differentiates between concessional and non-concessional programs.

The theoretical discussion above suggests that IMF programs can increase inequality because the policy priorities of IMF decision-makers can diverge from the preferences of governments, which are likely to be more responsive to the distributional preferences of their domestic audience. An extension of this argument is that the effect of an IMF program on a country will then depend on how responsive the government of this country is in the counterfactual absence of an IMF program (see also Nooruddin and Simmons 2006). At a highly stylized level, it is arguably fair to say that democratic governments are, on average, more responsive to the preferences of their citizens, including the relatively poor.²⁵ This is supported by the fact that democracies exhibit larger public sectors, higher levels of social spending and other policies that benefit the relatively poor (Huber et al. 2008; Jensen and Skaaning 2015; Rodrik 1999). An IMF program in a democracy can cut and reverse these policies by constraining the government's responsiveness to these preferences for a limited period, thereby leading to an increase in inequality. In a non-democracy, however, an IMF program can constrain pre-existing government responsiveness to a lesser degree because responsiveness is already lower. Accordingly, there are, on average, fewer pro-poor policies that IMF programs can cut or reverse and inequality often is already higher. In the baseline sample, the average Gini coefficient is 36 in democracies and 40 in non-democracies.

To test this extension of the argument, democracies and non-democracies are considered separately in Table 3.²⁶ The results show that the main effect is driven by democratic program countries. In democracies, IMF programs substantially increase

²⁵ Of course, government responsiveness also strongly differs among countries with the same regime type. There are relatively responsive autocracies and relatively non-responsive democracies (e.g., Geddes et al. 2014). For this argument, however, it is only relevant that democracies and non-democracies differ on average (see also Knack and Keefer 2007).

²⁶ In columns 1–2 and 5–6 the sample is split on both states, in columns 3–4 and 7–8 the fitted values of the variable of interest calculated by means of the entire sample are used. The latter is a valid strategy to the extent that there is no systematic difference of the IV's effect on IMF program between democracies and non-democracies. Theoretically, there is no obvious reason why this should be the case. Empirically, the first-stage regressions for the split samples show that the coefficients of the IV are similar in both samples and only in column 5 do they not reach statistical significance at the 10%-level. This suggests that splitting the sample only on the second stage is also valid. Standard errors in these regressions are cluster bootstrapped to account for two-stage estimation.

inequality (columns 1–4). The coefficients range from 1.8 to 2.3 and are, thus, larger compared to the full sample.²⁷ The effect is robust to whether or not control variables are included and whether fitted values from the full or only the democratic sample are used. The instrument maintains its relevance despite the smaller sample size in columns 1 and 2. As soon as only nondemocracies are considered, the effect disappears (columns 5–8). Here, the estimated coefficients are close to zero and far from statistically significant at conventional levels.²⁸ In line with theoretical expectations, the distributive effects of IMF programs imply a more substantial divergence from the counterfactual policies in democracies.

4.5 Conditionality

Next, I further extend the core analysis by providing evidence on the role that IMF conditionality plays for the link between IMF programs and increasing inequality. Not all IMF programs are the same. Research has repeatedly highlighted important differences in the design of conditionality and challenged the claim that the IMF applies identical ‘cookie-cutter’ programs (Stone 2008). Since the above discussion suggests that IMF conditions are a key mechanism driving the effect, a natural expectation is that more extensive conditionality in IMF programs will be associated with larger increases in inequality. At the same time, not all IMF conditions have a distributional dimension. The theoretical considerations suggest that conditions with potentially inequality-increasing effects include the sectors *social spending*, *trade and financial liberalization*, and *labor-market reforms*. The empirical analysis will thus also differentiate between IMF conditions in different sectors.

For this analysis, the sample is restricted to country-years in which an IMF program begins, following the approach by Rickard and Caraway (2019) to circumvent the selection-into-program problem. Informed by the results of the main analysis I then regress the change in the Gini coefficient over the subsequent three-year-period on several measures of conditionality at the time of an IMF program start:

$$\text{Inequality}_{i,t+3} - \text{Inequality}_{i,t} = \beta \text{IMFconditions}_{i,t} + \mathbf{X}'_{i,t} \mu + \tau_t + \varepsilon_{i,t}$$

Initially, *IMFconditions* indicates the ‘scope of conditionality’ defined as the number of policy areas that conditions cover (in the spirit of Dreher et al. 2015). In alternative specifications, this variable is substituted by a set of binary variables indicating whether any condition addressed a given policy area. Appendix A provides a description of these policy areas. Appendix F describes the empirical approach and the data, which is based on the MONA database and Andone and Scheubel (2017), in more detail.

The results of this analysis, reported in Table 4, suggest that inequality increases more during IMF programs with more extensive conditionality than during programs with fewer conditions (columns 1–2). Second, when examining specific policy areas

²⁷ In accordance with the results for long-term effects *IMFprogram* is lagged by 3 years in this table. The substance of the results does not depend on this choice.

²⁸ In column 5, the IV is not strong enough to rule out weak instrument bias. In column 6 the F-statistic exceeds the Stock-Yogo critical value of 6.66 that tolerates 2SLS size distortions of 20%.

Table 3 Democracies and non-democracies

	Democracies			Non-Democracies				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Second Stage								
IMF Program _{t-3}	1.901** (0.739)	1.901*** (0.721)	2.271** (0.895)	1.677** (0.700)	-0.057 (2.260)	-0.228 (0.741)	-0.031 (1.328)	-0.247 (1.032)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Sample split	1st & 2nd stage	1st & 2nd stage	2nd stage	2nd stage	1st & 2nd stage	1st & 2nd stage	2nd stage	2nd stage
Obs.	2094	1708	3687; 2094	2632; 1708	1317	860	3687; 1317	2632; 860
Panel B: First Stage								
IV	-0.315*** (0.077)	-0.396*** (0.089)	-0.264*** (0.056)	-0.444*** (0.077)	-0.142 (0.125)	-0.487*** (0.164)	-0.264*** (0.056)	-0.444*** (0.077)
K.-P. underid. p	0.001	0.000	0.000	0.000	0.256	0.020	0.000	0.000
K.-P. weak id. F	16.958	19.951	22.292	32.916	1.286	8.866	22.292	32.916

Note: 2SLS regressions. Dependent variable is the *Gini* coefficient of net income. All regressions include country fixed effects, year fixed effects, the lagged dependent variable, and *IMFprobability*. In columns 1–2 and 5–6 standard errors (in parentheses) are clustered at the country level; in the remaining regressions standard errors are cluster bootstrapped. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$

(columns 3–4), the results are consistent with the theoretical considerations on the ‘public spending’ and ‘labor market reforms’ channels discussed above: Conditionality addressing the social/pension sector and the labor market of the private sector are associated with increasing inequality. In contrast, there is no evidence for the expected association between IMF conditions targeting trade or capital account policies and increasing inequality. In sum, these results support conditionality as a plausible channel for the main effect (see also Forster et al. 2019) and suggests a role for both IMF-mandated labor market reforms and cuts in social spending.

Several additional heterogeneity analyses in Appendix H (pages 33–38 of the supporting information) further support this interpretation. By modifying the baseline IV specification, they show that the baseline effect is driven a) by *non-concessional IMF programs*, which typically demand more substantial policy reforms than concessional programs, and b) by *programs with more binding conditions*. In contrast, they provide no support for loan size as a mechanism (Tables 20–22 in Appendix H).

4.6 Robustness

The following section summarizes additional tests that examine the robustness of these results. They are presented in more detail in Appendix G (pages 14–32 of the supporting information). First, concerns regarding the exclusion restriction are addressed. The results are robust to using only the IMF’s liquid resources as the time-varying component of the IV and to excluding observations with large purchases and repurchases of IMF credit (Table 12). Table 13 shows that controlling for interactions of global economic cycles with

Table 4 IMF conditionality

	(1)	(2)	(3)	(4)
Scope of Conditionality	0.154** (0.076)	0.163** (0.073)		
Social Sector (incl. Pensions)			1.435*** (0.493)	1.727*** (0.577)
Trade and Financial Liberalization			0.424 (0.465)	0.133 (0.381)
Labor Market (private sector)			2.247*** (0.717)	2.614*** (0.761)
Labor market (public sector)			−0.974 (0.676)	−1.032 (0.661)
Year FE	Yes	Yes	Yes	Yes
Inequality Controls	Yes	Yes	Yes	Yes
IMF Controls	No	Yes	No	Yes
Period	1993–2013	1993–2013	1993–2013	1993–2013
Observations	296	273	296	273
R-squared	0.099	0.218	0.137	0.262

Note: OLS regressions in the sample of observations with active IMF programs. Dependent variable is the Gini coefficient of net income. Standard errors, clustered at the country level, in parentheses

Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$

IMFprobability does not affect the results and that substituting the IV with these interactions does not produce significant first-stage effects. This supports the claim that the IV does not pick up global economic cycles. To further increase the plausibility of the first-stage effect, I then randomize the temporal order of all *IMFliquidity* values for 1000 placebo regressions. The resulting IV coefficients are, as expected, normally distributed around zero and *all* have smaller *t*-statistics than does the coefficient estimated based on the real temporal order. Substituting the time-varying probability by a time-invariant probability that is absorbed by country fixed effects also does not affect the results (Table 14). The next table shows that standard OLS-FE specifications with control variables produce a null finding, while simple OLS regressions yield a statistically significant positive association (Table 15). Subsequent tests based on Altonji et al.'s (2005) method show that selection-on-unobservables relative to selection-on-observables would have to be more than three times as large and go in the opposite direction if the true reduced-form effect was in fact zero (Table 15).

Next, to show that the IV strategy is not dependent on including the period after the global financial crisis (GFC), I remove the post-2008 sample in Table 16. While *F*-statistics naturally decrease in this smaller sample, they stay above 10 and the IV maintains its relevance.

Then, to relate the results to previous studies aiming to estimate a causal IMF effect, I substitute the IV with UNGA voting similarity to the United States (Table 17). While results go in the same direction, the effect size that this approach identifies is doubtful (140% of a within-country standard deviation). Under the assumption that this study's IV is excludable, this finding combined with the fact that UNGA voting enters with a significantly positive sign as a control in the baseline (see Appendix C) suggest that UNGA voting is linked to inequality through more channels than just IMF programs. This violates the exclusion restriction and biases the coefficient upwards.²⁹

In Table 18, I modify the set of control variables and add *Debt (% GDP)*. The results are robust. Then, I modify the dependent variables: When using the Gini coefficient of market income as an alternative outcome variable, results are very similar and slightly more significant than in the baseline (Table 19, columns 1–3). Additionally, I employ ATG data as an alternative to the SWIID (Table 19, columns 4–6). Even though this dramatically reduces the sample size, the results are again robust.

5 Conclusions

According to the results presented in this article, IMF programs increase income inequality within countries. The effect is largest 3 years after a program year and observable for about 5 years. An analysis of decile-specific income data shows that the effect is due to decreasing absolute incomes for the poor, while, on average, there are no significant income gains for any income decile. An analysis of IMF conditionality suggests that IMF-mandated austerity measures and labor-market reforms are among the channels.

For the IMF, these results highlight an unintended consequence of its loan programs. According to its former Managing Director, Christine Lagarde, “reducing excessive inequality is not just morally and politically correct, but it is good economics.”³⁰ The

²⁹ Furthermore, *F*-statistics for UNGA-voting as IV are below critical thresholds.

³⁰ On Twitter, June 17, 2015, <https://twitter.com/lagarde/status/611261555372621824>.

finding that IMF programs, thus, do not seem to conform with Lagarde's view of 'good economics' is echoed in the IMF's 2019 *Review of Program Design and Conditionality*, which notes a "limited focus on the quality of social spending, and on social protection and inequality" (p. 24). The fact that the same document states that "[m]any stakeholders emphasized that program design should be more attentive to the potential negative impacts of conditionality on [...] inequality" (p. 5) could indicate an emerging willingness inside the IMF to tackle this issue more seriously.

Future research on the IMF, on global governance, and on the drivers of income inequality can draw on this article. First, the proposed identification strategy can be useful for scholars investigating the effects of IMF programs more broadly. Several conceptual arguments and robustness tests suggest that the probability that the identifying assumption is violated is low. Nevertheless, as for most empirical strategies based on non-experimental data, violations of the identifying assumption cannot be ruled out with certainty. More research that challenges this and other strategies used to isolate the effects of the IMF is thus needed to advance our understanding of how international organizations shape the global economy.

Second, the results of this paper support the view that official financial assistance can have unintended adverse implications for governance in recipient countries. While the previous literature emphasized adverse effects of aid on democratic institutions (Knack 2000, 2004), this article suggests that the conditions international organizations attach to aid can undermine the functioning of existing democratic institutions. The lack of democratic governance at the global level presents a challenge for democratic governance at the domestic level. Future research on this topic could further our understanding of what a more democratic form of global governance could look like.

Third, the article adds to the growing literature that stresses the role of changing policies and institutions as determinants of inequality. While their contribution to current trends of rising inequality across many countries is well-established, it remains an open question as to why so many countries modify their national policies and institutions in a way that inequality increases. This article's results suggest that *international* policies and institutions can play a significant role in this regard. Examining the underlying mechanisms linked to how global governance interacts with political processes at the national level is a promising area for future research.

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