The Economics of the Democratic Deficit: The Effect of IMF Programs on Inequality

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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 five 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

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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 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 five 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, Sturm, and Vreeland 2009; Schneider and Tobin 2020;

[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).

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

IMF decision-making

International organizations like the IMF can be considered as sets of "nested principal-agent relationships" (Nielson 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 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.

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 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

[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.

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

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 Buira 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, Stubbs, and King (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. 2018). Through the threat to withhold loan disbursements, IMF conditionality rises costs for domestic political actors, e.g., parliaments, to block reforms under a 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, de Gortari, and Itskhoki 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, Manning, and Smith 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

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, Hoddie, and Bauer 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

[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, Cruz, and Graham 2012), exchange rates (Clements, Gupta, and Nozaki 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).

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]

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 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 (Chwieroth 2007a; Dreher and Lang 2019; Nelson 2014).

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[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.

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

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

[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.

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:

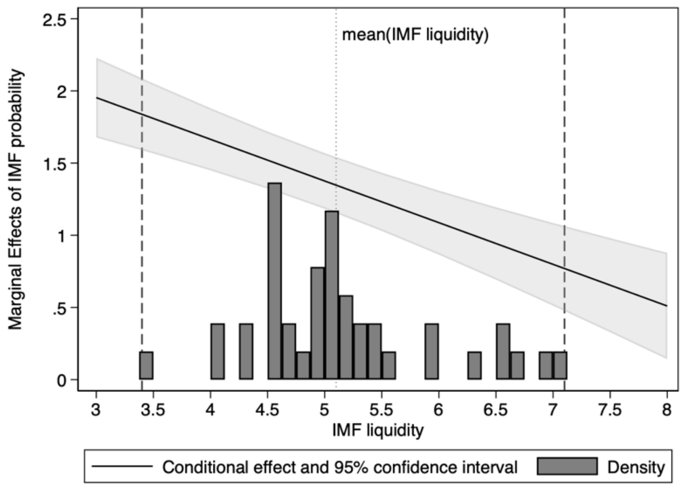
$$IMFprogram_{i,t} = \alpha_1(IMFprobability_{i,t} \times IMFliquidity_t)$$
$$+ \alpha_2 IMFprobability_{i,t} + \boldsymbol{X'}_{i,t}\alpha_3 + \delta_i + \tau_t + u_{i,t}$$

Here, *IMFprogram* is a binary variable indicating that country i was under an IMF program for at least five 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] $\boldsymbol{X}$ 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.

[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.

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

Figure 1 – Visualized Effect of the IV



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.

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 generous[14] 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

[13] Detailed results, including tests of instrument relevance, are reported in the results section.

[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*.

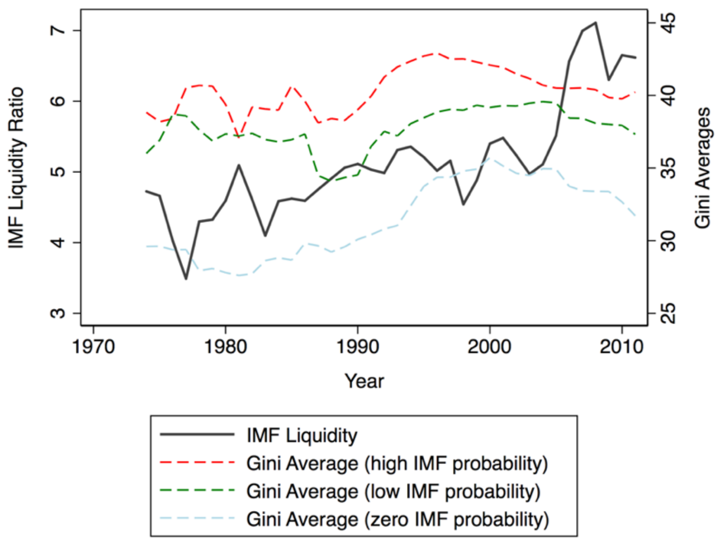
variables driving this endogeneity were affecting inequality differently in countries with different levels of IMF participation history (for details Nizalova and Murtazashvili 2016).

To demonstrate why this is unlikely, Figure 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 five 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 Figure 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, Figures 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 Figure 2 also show inequality trends in low- and high-probability countries. These trends are close to parallel and none of them is correlated with a trend in IMF liquidity.[17]

[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 robustness section, I show that this is unproblematic and that the results hold when this variation is excluded.

[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

Figure 2 – The IMF's Liquidity Ratio



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

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.

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:

$$Inequality_{i,t} = \beta \; IMF\widehat{program}_{i,t-1}$$

$$+\gamma \; IMFprobability_{i,t-1} + \boldsymbol{X'}_{i,t-1}\,\mu + \; \delta_i \; + \; \tau_t \; + \; u_{i,t}$$

is not correlated with global economic cycles as measured by global GDP growth or the global number of banking crises.

In the baseline, I follow the related literature on IMF program effects and lag the variable by one 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 et al. 2015; Dorsch and Maarek 2018; 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 (%*

[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).

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 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

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 one percent level. Jointly interpreted with the positive coefficient on *IMFprobability* this reflects the relationship described and illustrated in Figure 1 above: *IMFliquidity* reduces the positive association between *IMFprobability* and *IMFprogram*. Underidentification is rejected at the 0.1

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

[22] See Dorsch and Maarek (2018) and Oberdabernig (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.

percent level and the Kleibergen-Paap F-statistics comfortably surpass conventional levels of weak identification tests.[23]

These results are robust across specifications without covariates[24] (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 robustness section and in Appendix G.

Table 1 – IMF Programs and Income Inequality

	(1)	(2)	(3)
		Panel A: Second Stage	
IMF program$_{t-1}$	1.130**	1.319**	1.338**
	(0.521)	(0.515)	(0.565)
		Panel B: First Stage	
IMF liquidity x IMF probability$_{t-1}$	-0.276***	-0.311***	-0.367***
	(0.052)	(0.059)	(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.

[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 percent.

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

Second-Stage Results: IMF Programs Increase Inequality

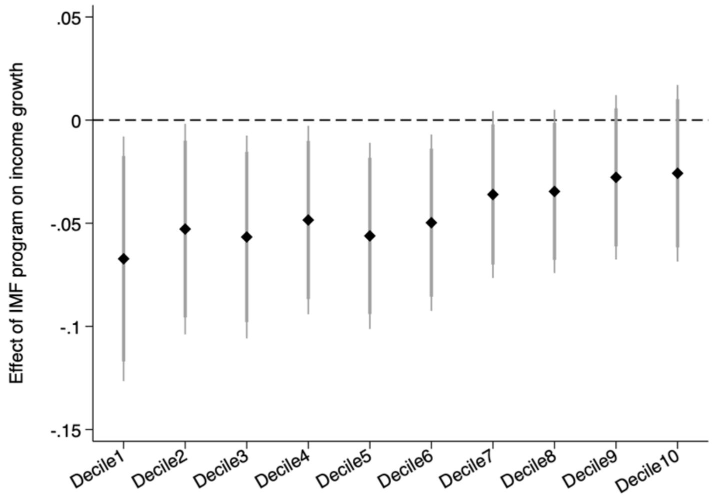
The baseline results of the second stage are reported in panel A of Table 1. They show that IMF programs, on average, increase income inequality. Across the three specifications with and without control variables, the coefficient is statistically significant ($p_{(1)}$=0.030; $p_{(2)}$=0.010, $p_{(3)}$=0.018) 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 percent of a within-country standard deviation. As inequality is slow to change, increases of this size within one year are relatively rare events (9 percent 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 an IMF program in the previous year is equivalent to a transfer of four to five percent of the poorer half's mean income to the richer half (Appendix B).

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 Figure 3 (and reported in Appendix D).

Figure 3 – Decile-specific Effects



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.

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 five years following a year under a program and strongest and most significant after three years. After six years the effect is no longer significantly different from zero.

Figure 4 – Long Term Effects

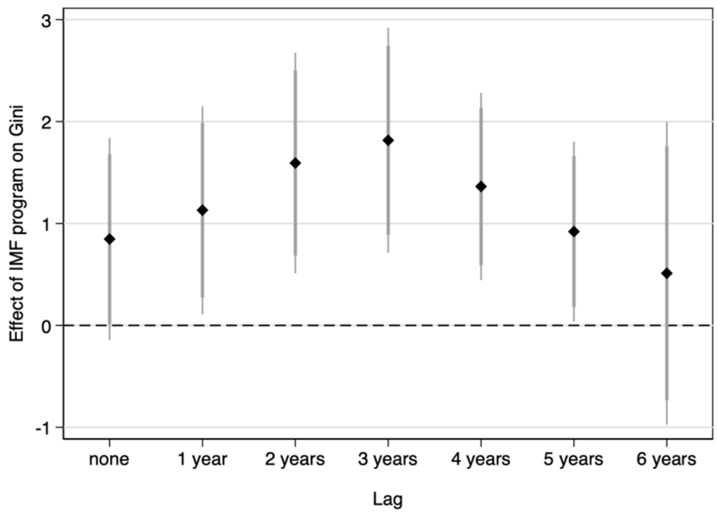


Table 2 – Long Term Effects

	t	*t-1*	*t-2*	*t-3*	*t-4*	*t-5*	*t-6*
	0.847*	1.130**	1.593***	1.816***	1.363***	0.920**	0.511
IMF program	(0.506)	(0.521)	(0.552)	(0.564)	(0.468)	(0.450)	(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 plots these coefficients along with 95 percent confidence intervals.
Significance levels: * p<.10, ** p<.05, *** p<.01

These estimated lagged effects are based on regressions that lag the treatment variable *IMF program* by one to six years. As IMF programs usually last more than one year – in my sample the average program length is four 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.

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 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-

[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, Wright, and Frantz 2014). For this argument, however, it is only relevant that democracies and non-democracies differ on average (see also Knack and Keefer 2007).

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 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.

[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 IMFprogram 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.

[27] In accordance with the results for long-term effects *IMFprogram* is lagged by three 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 Nondemocracies

	Democracies				Non-Democracies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Second Stage							
IMF	1.901**	1.901***	2.271**	1.677**	-0.057	-0.228	-0.031	-0.247
Program$_{t-3}$	(0.739)	(0.721)	(0.895)	(0.700)	(2.260)	(0.741)	(1.328)	(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***	-0.396***	-0.264***	-0.444***	-0.142	-0.487***	-0.264***	-0.444***
	(0.077)	(0.089)	(0.056)	(0.077)	(0.125)	(0.164)	(0.056)	(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

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:

$$Inequality_{i,t+3} - Inequality_{i,t} = \beta\ IMFconditions_{i,t} + \boldsymbol{X'}_{i,t}\,\mu + \tau_t + \varepsilon_{i,t}$$

Initially, *IMFconditions* indicates the 'scope of conditionality' defined as the number of policy areas that conditions cover (in the spirit of Dreher, Sturm, and Vreeland 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 (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.

Table 4 – IMF Conditionality

	(1)	(2)	(3)	(4)
Scope of Conditionality	0.154**	0.163**		
	(0.076)	(0.073)		
Social Sector (incl. Pensions)			1.435***	1.727***
			(0.493)	(0.577)
Trade and Financial Liberalization			0.424	0.133
			(0.465)	(0.381)
Labor Market (private sector)			2.247***	2.614***
			(0.717)	(0.761)
Labor market (public sector)			-0.974	-1.032
			(0.676)	(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

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).

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 *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 percent 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 three years after a program year and observable for about five 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 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

[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.

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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APPENDIX (NOT INTENDED FOR PRINT)

Supporting Information for

The Economics of the Democratic Deficit:

The Effect of IMF Programs on Inequality

SUPPORTING INFORMATION

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Appendix A: Variables

Table 5 – Descriptive Statistics and Data Sources

Variable	Mean	SD	Min	Max	Description and Source
Gini	37.90	9.14	17.96	68.16	Gini coefficient of net income according to the SWIID version 5.0 (Solt 2016).
IMF program	0.32	0.47	0.00	1.00	Indicator 1 if IMF program in place for at least 5 months in year t, (Dreher 2006).
IMF liquidity (ln)	5.42	0.75	4.10	7.11	IMF liquidity ratio, equals liquid resources (usable currencies plus Special Drawing Rights contributed) divided by liquid liabilities (total of members' reserve tranche positions plus outstanding IMF borrowing from members); own calculation based on data from the IMF's Annual Reports 1973-2013 and the IMF's International Financial Statistics
IMF probability	0.25	0.25	0.00	1.00	$\frac{\sum_{T=1973}^{t} I(IMFprogram_{it}=1)}{t-1973}$ Own calculation based on (Dreher 2006).
GDP per capita (ln)	8.54	1.54	5.31	11.61	Gross domestic product per capita in constant 2005 USD (World Bank 2016)
Education	7.57	2.87	0.89	13.18	Average years of schooling, linear interpolation of data for five-year periods (Barro and Lee 2013)
Trade	75.99	50.69	12.01	439.66	Trade (% GDP) (World Bank 2016)
Life Expectancy	68.75	9.55	27.08	82.93	Life expectancy at birth in years (World Bank 2016)
Democracy	0.66	0.47	0.00	1.00	Indicator 1 if Polity IV index is 6 or higher (Marshall, Jaggers, and Gurr 2011)
Current account balance	-1.96	6.27	-47.21	26.77	Balance on current account (% GDP) (IMF 2016).
Investments	23.10	6.75	-2.42	61.47	Gross capital formation (% of GDP) (World Bank 2016).
GDP growth	3.64	4.40	-50.25	35.22	GDP growth (annual %) (World Bank 2016).
Banking crisis	0.11	0.31	0.00	1.00	Indicator 1 if systemic banking crisis in year t in country i, (Laeven and Valencia 2012).
UNGA voting	0.15	0.91	-2.14	3.01	Ideal point of voting behavior in the UNGA (Bailey, Strezhnev, and Voeten 2017).
Global GDP growth	3.18	1.59	-1.70	8.20	Growth of global GDP; own calculations based on World Bank (2016).
Banking crises	14.51	10.11	0.00	30.00	Global total of Banking Crisis in year t, based on Laeven and Valencia (2012)

Liquid resources (ln)	11.30	0.67	9.84	12.96	IMF liquid resources (see LQR)
Gross Gini	45.27	7.02	20.25	71.13	Gini coefficient of market income according to the SWIID version 5.0 (Solt 2016)
Gini (ATG)	39.42	9.88	20.00	69.80	Gini coefficient (Giniall) according to the ATG Dataset (Milanovic 2014)
Debt (% GDP)	60.67	43.00	0.00	624.64	Debt over GDP from the IMF's historical public debt database (IMF 2020)
IMF program, large loan-to-GDP ratio (above median)	0.17	0.37	0.00	1.00	Same as *IMF program* but set to zero for IMF programs with loan-to-GDP ratios below the median. Data on loan sizes from IMF (2018)
IMF program, small loan-to-GDP ratio (below median)	0.15	0.36	0.00	1.00	Same as *IMF program* but set to zero for IMF programs with loan-to-GDP ratios above the median. Data on loan sizes from IMF (2018)
IMF program, many conditions (above median)	0.19	0.39	0.00	1.00	Same as *IMF program* but set to zero for IMF programs with number of binding applicable conditions ratios below the median. Data on conditions from Kentikelenis et al. (2016)
IMF program, few conditions (below median)	0.13	0.34	0.00	1.00	Same as *IMF program* but set to zero for IMF programs with number of binding applicable conditions ratios above the median. Data on conditions from Kentikelenis et al. (2016)
IMF program (non-concessional)	0.17	0.38	0.00	1.00	Same as *IMF program* but only includes programs organized under SBA and EFF facilities
IMF program (concessional)	0.16	0.36	0.00	1.00	Same as *IMF program* but only includes programs organized under ESAF and PRGF facilities

Note: The sample of the full specification (Table 1, column 3) was used for calculating the values in this table.

Conditionality Variable	Mean	SD	Min	Max	Description
Scope of Conditionality	5.46	2.26	0	9	Number of policy areas covered by IMF Conditionality
Foreign Exchange Systems	0.24	0.43	0	1	IMF condition addressing foreign exchange systems and restrictions (current and capital)
Trade / Financial Liberalization	0.44	0.50	0	1	IMF condition addressing international trade policy and financial liberalization
Central Bank	0.13	0.33	0	1	IMF condition addressing the central bank
Financial Sector	0.78	0.42	0	1	IMF condition addressing the financial sector
Government	0.84	0.36	0	1	IMF condition addressing the general government
Labor Market (public sector)	0.08	0.28	0	1	IMF condition addressing the civil service, public employment and wages
Social Sector (incl. Pensions)	0.10	0.30	0	1	IMF condition addressing pensions and other social sector reforms
SOE reform	0.77	0.42	0	1	IMF condition addressing reforms of public enterprises in the non-financial sector
Labor Market (private sector)	0.03	0.17	0	1	IMF condition addressing labor market reforms in the private sector
Residual Category	0.63	0.48	0	1	IMF condition addressing other structural reforms

Note: The sample of the specifications 1 and 3 in Table 11 was used for calculating the values in this table. Source: Andone and Scheubel (2017).

Appendix B: Interpreting Differences in Gini Coefficients

Following Blackburn (1989), a change in the Gini coefficient ($G \in [0, 100]$) by ΔG points is equivalent to a lump-sum transfer of L from all those below the median to all those above the median, given by

$$L = \frac{2\Delta G}{100} \times M\text{, where M is the country's mean income.}$$

Knowing M and the poorer half's share of total income S, the mean income of the poorer half P is given by

$$(P \times 0.5) + (P \times \frac{1\text{-}S}{S} \times 0.5) = M$$

$$P = 2MS$$

The lump-sum transfer relative to the poorer half's mean income is, hence, given by:

$$\frac{L}{P} = \frac{\Delta G}{100} \times \frac{1}{S}$$

The sample average for S is S = 0.25 (data from the World Bank's World Development Indicators).

Example: According to Blackburn's metric, an increase in the Gini by 1 point is equivalent to a lump-sum transfer of 2 percent of the country's mean income from the bottom half to the upper half. To view this from the perspective of the average individual belonging to a country's poorer half, consider that in the sample's average country those below the median earn approximately 25 percent of the total national income. Hence, such a change in inequality is equivalent to a transfer of 4 percent of the poorer half's mean income to the richer half.

Appendix C: Baseline: Full Regression Output

Table 6 – Baseline, First Stage

	(1)	(2)	(3)
IMFliquiditiy	-0.276***	-0.311***	-0.367***
× IMFprobability	(0.052)	(0.059)	(0.069)
IMFprobability	2.760***	2.691***	3.209***
	(0.282)	(0.296)	(0.290)
Gini	0.003	-0.001	-0.004
	(0.003)	(0.004)	(0.005)
GDP per capita (ln)		-0.107	0.010
		(0.293)	(0.361)
GDP per capita squared (ln)		-0.005	-0.016
		(0.018)	(0.022)
Education		-0.056**	-0.054*
		(0.025)	(0.028)
Trade		-0.000	-0.000
		(0.001)	(0.001)
Life Expectancy		0.006	0.008
		(0.005)	(0.006)
Regime Type		-0.009	-0.005
		(0.047)	(0.053)
Current Account Balance			0.002
			(0.003)
Investments			-0.006**
			(0.003)
GDP Growth			0.003
			(0.002)
Banking Crisis			0.089**
			(0.038)
UNGA Voting			0.105***
			(0.035)
Global GDP Growth			0.006
× IMFprobability			(0.027)
Banking Crises			0.006
× IMFprobability			(0.005)
Observations	3766	2985	2573
K.-P. underid. LM	18.452	17.265	19.397
K.-P. underid. p	0.000	0.000	0.000
K.-P. weak id. F	27.699	27.422	28.330

Notes: Dependent variable *IMFprogram*. First-stage regressions of Table 1. All regressions include country fixed effects and year fixed effects. Standard errors, robust to clustering at the country level, in parentheses. Significance levels: * p<.10, ** p<.05, *** p<.01

Table 7 – Baseline, Second Stage

	(1)	(2)	(3)
IMF program$_{t-1}$	1.130**	1.319**	1.338**
	(0.521)	(0.515)	(0.565)
IMFprobability$_{t-1}$	-1.844**	-1.732**	-2.472**
	(0.841)	(0.846)	(1.070)
Gini$_{t-1}$	0.916***	0.916***	0.910***
	(0.011)	(0.013)	(0.014)
GDP per capita (ln)$_{t-1}$		2.442***	3.089***
		(0.944)	(0.884)
GDP per capita squared (ln)$_{t-1}$		-0.090*	-0.114**
		(0.050)	(0.052)
Education$_{t-1}$		-0.060	-0.048
		(0.078)	(0.092)
Trade$_{t-1}$		-0.001	0.001
		(0.003)	(0.003)
Life Expectancy$_{t-1}$		-0.036**	-0.030
		(0.017)	(0.022)
Regime Type$_{t-1}$		0.060	-0.030
		(0.107)	(0.131)
Current Account Balance$_{t-1}$			0.006
			(0.009)
Investments$_{t-1}$			0.011
			(0.010)
GDP growth$_{t-1}$			-0.017**
			(0.008)
Banking Crisis $_{t-1}$			-0.238*
			(0.139)
UNGA Voting$_{t-1}$			0.227*
			(0.135)
Global GDP Growth × IMFprobability$_{t-1}$			0.109**
			(0.050)
Banking Crises × IMFprobability$_{t-1}$			-0.002
			(0.012)
Observations	3766	2985	2573
Adjusted R^2	0.880	0.853	0.858

Notes: Dependent variable Gini. Second-stage regressions of Table 1. All regressions include country and year fixed effects. Standard errors, robust to clustering at the country level, in parentheses. Significance levels: * p<.10, ** p<.05, *** p<.01

As regards the coefficients of the control variables, the lagged dependent variable is, unsurprisingly, highly significant as inequality is a highly time-persistent phenomenon (Dorsch and Maarek 2018). The coefficient on *IMFprobability* cannot be interpreted in isolation. This variable captures the variation that the predicted values of *IMFprogram*, which themselves include variation of *IMFprobability*, do not already capture. The purpose of controlling for *IMFprobability* is to make sure that this possibly endogenous part of the variation in predicted values is controlled for and netted out (see also Nunn and Qian 2014). GDP per capita is associated with higher inequality levels, while there is some weak evidence for the Kuznets curve hypothesis: Albeit consistently negative, the coefficient on the squared term is only significant in specification 3. As in previous studies, education is associated negatively with income inequality, even though the effect is not statistically significant in this sample. Systemic banking crises are also associated with decreasing inequality. As capital is usually distributed more unequally than income, the reduction of income from capital during such crises could explain this finding (see also Piketty 2014).

Appendix D: Decile-specific Effects: Full Regression Output

Table 8 – Decile-Specific Effects

Dep. Var.: Income growth rate for decile:	1	2	3	4	5	6	7	8	9	10
IMF program	-0.067**	-0.053**	-0.057**	-0.048**	-0.056**	-0.050**	-0.036*	-0.035*	-0.028	-0.026
	(0.030)	(0.026)	(0.025)	(0.023)	(0.023)	(0.022)	(0.021)	(0.020)	(0.020)	(0.022)
Observations	5899	5902	5903	5902	5904	5906	5907	5904	5906	5902
K-P underid. (p)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
K-P weak id. (F)	35.921	36.093	35.182	35.175	36.329	36.433	36.457	36.733	36.224	36.497

Note: 2SLS regressions. Dependent variable is the income growth rate of deciles 1-10. 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

Appendix E: Long-Term Effects: Alternative Specifications

As discussed in the main text, the estimated lagged effects in the baseline are based on regressions that lag the treatment variable *IMF program* by one to six years. They thus estimate the lagged effects of a year under an IMF program. As IMF programs typically last several years these lagged effects are estimated from programs that are either ongoing or that already ended. The first column of Table 9 thus examines ongoing IMF programs separately. In these regressions, the treatment variable IMF program ongoing is coded like *IMF program* but additionally requires the program to be still ongoing in year *t* to be set to 1; in other words, the variable is set to 0 if the program ended between year *t-x* and year *t*. In these regressions the estimated lagged effects are similar but somewhat larger than in the baseline. This makes sense because these effects are estimated only based on observations where the influence of the IMF is still ongoing and has not yet ended.

An alternative way to analyze the pattern over time is to look at lagged effects of the start of an IMF program. This is what the second column in Table 9 does. Here, the treatment variable *IMF agreement* indicates years in which an agreement on the start of an IMF program was reached. The results show that that the estimated lagged effects of program starts are very similar to lagged effects of program years. A possible interpretation of these results is that much of the effect is driven by the early program period.

Table 9 – Long-Term Effects with Alternative Treatment Variables

Treatment Variable:	IMF program ongoing	IMF agreement
Lag:		
t	0.847*	1.120*
	(0.506)	(0.670)
t-1	1.148**	1.504**
	(0.540)	(0.673)
t-2	1.678***	1.936***
	(0.634)	(0.642)
t-3	2.215***	1.874***
	(0.793)	(0.503)
t-4	2.020***	1.225***
	(0.717)	(0.388)
t-5	1.460**	0.740**
	(0.686)	(0.338)
t-6	0.596	0.292
	(0.860)	(0.422)

Note: Coefficients for different lags of different treatment variables, each from a separate regression.
The lags of the binary treatment variable *IMF program ongoing* are coded as the lags of *IMF program* but additionally require the program to be still ongoing in year *t* to be set to 1.
The binary treatment variable *IMF agreement* indicates years in which the country agreed with the IMF on the start of a new IMF program.
The specifications are otherwise identical to those reported in Table 2 in the main text.
Standard errors, clustered at the country level, in parentheses.
Significance levels: * p<.10, ** p<.05, *** p<.01

To show that the results of lagged effects are robust to the inclusion of the control variables, Table 10 adds the same sets of control variables as in the baseline regressions to these specifications. The estimates are barely affected by the inclusion of control variables.

Table 10 – Long-Term Effects with Control Variables

Lag:	(1)	(2)	(3)
t	0.847*	1.312***	1.660***
	(0.506)	(0.508)	(0.629)
t-1	1.130**	1.319**	1.338**
	(0.521)	(0.515)	(0.565)
t-2	1.593***	1.483***	1.312**
	(0.552)	(0.516)	(0.604)
t-3	1.816***	1.614***	1.313**
	(0.564)	(0.506)	(0.531)
t-4	1.363***	1.623***	1.315***
	(0.468)	(0.507)	(0.498)
t-5	0.920**	1.506**	1.096**
	(0.450)	(0.609)	(0.521)
t-6	0.511	1.125	0.735
	(0.758)	(1.017)	(1.003)

Note: The table reports β-coefficients for different lags of the variable IMFprogram in specifications (1)-(3), which are otherwise identical to the specifications in Tables 1 and 2. Each coefficient is from a separate regression. Standard errors in parentheses; number of observations in square brackets.

Appendix F: IMF Conditionality

As an extension to the paper's core analysis I examine evidence on the role of IMF conditionality for the link between IMF programs and increasing inequality. I use data extracted from the IMF's Monitoring of Fund Arrangements (MONA) database with an algorithm developed by Andone and Scheubel (2017) in order to create an annualized and harmonized dataset from both the archived (1993-2002) and the current (2002-2013) MONA data. First, I code the variable *Scope of Conditionality* defined as the number of policy areas that conditionality covers.[1] Second, I code binary variables indicating whether any condition addressed one of nine policy areas.[2] For the analysis, I restrict the sample to country-years for which the MONA database indicates the start of an IMF program. Informed by the results of the main analysis I then regress the change in Gini over the subsequent three-year-period on the conditionality variables at the time of the IMF program start. This sample restriction follows the approach by Rickard and Caraway (2019) to circumvent the selection-into-program problem. However, it allows inferences only for countries under IMF programs and provides correlational evidence only. Like Rickard and Caraway (2019) I was unable to find a relevant and excludable instrument for IMF conditions. To nevertheless mitigate the selection-into-conditions problem, I add the same set of control variables as before.

The results show that inequality increases significantly more during IMF programs with more extensive conditionality than during programs with fewer conditions. When examining specific policy areas, it becomes apparent that conditions targeting the labor market or the social and pension sector are associated with rising inequality. In program countries in which IMF conditions address the labor market the Gini rises by almost three points more than in countries whose programs do not cover this policy area. During IMF programs in which conditionality addresses the social and pension sector, income inequality in the subsequent three-year period rises, on average, by about two Gini points more than otherwise.

While these results cannot provide causal evidence, they are consistent with the idea that conditionality is a plausible channel for the main effect. They are also consistent with the theoretical considerations on 'social spending' and 'labor market reforms' discussed above. Contrary to the predictions regarding the 'liberalization' channel, however, conditions addressing trade policy or the financial sector are not significantly associated with rising

[1] This approach follows Dreher, Sturm, and Vreeland (2015).

[2] See Appendix A for a description of these policy areas.

inequality. Even though the point estimate is positive, it is not statistically significant on conventional levels in this sample.

While these results support the main theoretical argument, a word of caution regarding their interpretation is in order. First, the data on conditionality is limited to a much shorter time period than the data used for the main analysis. Second, its structure does not allow a direct test of all channels discussed above, as the disaggregation by policy areas in the MONA database is not in line with the scholarly literature's theoretical considerations on determinants of inequality. While social spending and labor market reforms can be captured, the effects of more general spending cuts and capital account liberalization cannot be isolated. Third, the information that is included only provides the policy area and not the exact content of the condition. It does neither cover its stringency nor the extent of compliance. Fourth, while restricting the sample to IMF program countries circumvents the selection-into-program problem, potential endogeneity bias resulting from selection-into-conditions cannot be ruled out. For these reasons this evidence should be considered as suggestive and correlational rather than as definitive and causal. While this study's focus is on causally identifying the aggregate effect, future research should zero in on the underlying channels.

Table 11 – IMF Conditionality:

	(1)	(2)	(3)	(4)
Scope of Conditionality	0.154**	0.163**		
	(0.076)	(0.073)		
Social Sector (incl. Pensions)			1.435***	1.727***
			(0.493)	(0.577)
Labor Market (private sector)			2.247***	2.614***
			(0.717)	(0.761)
Trade and Financial Liberalization			0.424	0.133
			(0.465)	(0.381)
Labor market (public sector)			-0.974	-1.032
			(0.676)	(0.661)
SOE reform			0.095	0.022
			(0.494)	(0.519)
Foreign Exchange Systems			0.586	0.519
			(0.390)	(0.426)
Central Bank			0.529	0.622
			(0.588)	(0.625)
Financial Sector			0.250	0.459
			(0.520)	(0.460)
Government			0.069	0.128
			(0.857)	(0.838)
Residual Category			-0.321	-0.089
			(0.453)	(0.371)
Year FE	Yes	Yes	Yes	Yes
Controls (Inequality)	Yes	Yes	Yes	Yes
Controls (IMF)	No	Yes	No	Yes
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*

Appendix G: Robustness

This section describes the robustness tests summarized in the results section in more detail.

Challenging the identification I: IMF liquidity

First, I address concerns regarding the exclusion restriction. Some readers might worry that the denominator of the liquidity ratio, i.e., the amount of the Fund's liquid liabilities, threatens the excludability of the instrument. While most variation in the liquidity ratio is induced by the changing amount of liquid resources, to a significantly lesser extent it also depends on the liquid liabilities.[3] These vary when economically large members obtain and repay loans that are large relative to total IMF resources ("purchase" and "repurchase" in IMF jargon).[4] In Figure 2 this is visible, for instance, in the mid-2000s when Brazil and Turkey repaid extraordinarily large loans. In general, I argue that this does not undermine the excludability of the IV: First, the vast majority of these flows are not sizable enough to significantly affect the liquidity ratio. As in most cases the amount of resources transferred is significantly less than 1 percent of total IMF quotas, any concern regarding excludability would relate to very few observations. Second, the timing of such transactions is usually agreed upon years in advance. Given also that explanatory variables are lagged, it is unlikely that the schedule of large transactions developed with economically large countries is correlated with future levels of inequality in specific countries. Third, even if there was a correlation it would have to be conditional on *IMFprobability* because of the difference-in-differences style model the interacted IV estimates.

Nevertheless, to be cautious I run a robustness test in which I exclude the 100 observations that exhibit the largest flows from and to the IMF.[5] As the first three columns in Table 12 show, the results do not differ substantially. To address these concerns in the most cautious way possible, I also run regressions using only liquid resources as the time-variant factor of the IV.

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[3] The logged liquidity ratio's correlation with logged liquid resources is $r = 0.83$, while with logged liquid liabilities it is $r = 0.23$. In addition, minor changes in liquid liabilities can result from changes in IMF borrowing. A last source of variation is the fact that liquid resources additionally vary when the IMF adjusts the basket of currencies it considers "usable." The usability status, however, is highly stable over time, changes mostly for small economies and therefore has a very minor effect on the amount of liquid resources.

[4] The liquid liabilities' second source of variation is the Fund's borrowing from its members. While total borrowing by the Fund is zero in many years, its average share of the liquid liabilities is approximately 15%.

[5] This leaves only observations with a (re)purchase to total quota ratio of less than 0.57% (0.37%) in the sample. Regressions with 50 and 200 excluded observations produce virtually the same results.

This variable is, by construction, not determined by the Fund's liquid liabilities. By refraining from dividing the variable by liquid liabilities, I only exploit variation in liquid resources, whose only substantial source of variation is the exogenous timing of quota reviews. These results are presented in the last three columns of Table 12. While the instrument's relevance naturally decreases because some valuable variation is lost, it is still strong enough to confirm the robustness of the result to this alternative specification.

Challenging the identification II: Heterogeneous and correlated trends

In addition, I provide more detail on trends of inequality in sets of countries with different levels of *IMFprobability* (see the discussion of Figure 2 in the main text). As background information, note that Christian and Barrett (2017) show that the findings by Nunn and Qian (2014) could be driven by a spurious correlation between the time-varying constituent term of their interacted IV and a particular time trend in their outcome variable for a set of countries with a specific level of their probability measure. This is why Figure 5 again plots year-specific cross-country averages of *Gini* for countries with different levels of *IMFprobability* over time (Panel A, same as Figure 2 in the main text) and contrasts these with fabricated trends that would be problematic (Panel B). As described in the main text there is no evidence for trends that could threaten the exclusion restriction. Instead, the Gini trends seem to be parallel across these groups and substantially different as compared to the *IMFliquidity* time series. As Christian and Barrett (2017) show, a problem in Nunn and Qian (2014) arises from the fact that the time series of the time-varying constituent term of their interacted IV is remarkably similar to a simple (inverse-U shaped) trend and does not vary strongly from one period to the next. As *IMFliquidity* exhibits no obvious similarity to any such simple trend and is subject to several idiosyncratic shocks, it is much less likely to be correlated with a similar trend in the outcome variable. Panel B then shows how potentially problematic trends in inequality would look like: Countries with different levels of *IMFprobability* would exhibit different trends in inequality and for one of these groups this trend would follow the *IMFliquidity* trend while for the other group it would not. Such heterogeneous trends ("difference-in-differences") would constitute a threat to the identifying assumption. In the actual data, however, there is no evidence for such heterogeneous trends.

To further examine whether unobserved trends drive the results, I test whether *IMFliquidity* is correlated with global macroeconomic conditions that could affect national inequality levels through an interaction with (variables that are correlated with) *IMFprobability.* Relevant macroeconomic conditions are variables that indicate increased borrower demand for IMF programs like global growth slumps or the number of global financial crises. These could drive the first stage effect, if they are correlated with *IMFliquidity.* To examine this, Figures 7 and 8 plot the time-variation of *IMFliquidity* along with annual rates of global GDP growth and with the global total of systemic banking crises. None of the two time series exhibit a similar time trend as *IMFliquidity*. Figure 9 and 10 then directly examine the correlations by plotting scatter plots and by reporting the Pearson's correlation coefficients. There is no visual correlation and the correlation coefficients are small. For *IMFliquidity* and annual rates of global GDP growth the correlation coefficient is r_1 = -0.17; for *IMFliquidity* and *the* global total of systemic banking crises it is r_2 = 0.34.

Next, I further examine this in a regression framework in Table 13. Column 1 replicates the full baseline specification (Table 1, column 3), while column 2 removes the two interactions *Global GDP growth* × *IMFprobability* and *Number of banking crises* × *IMFprobability*, which are included in the full baseline specification (see p. 19). Comparing the two specifications shows that the inclusion of these two interactions neither affects the first-stage coefficient of the *IV* (= *IMFliquidity* × *IMFprobability*) nor the second-stage coefficient of *IMF program*. This shows that the IV based on *IMFliquidity* does not pick up the variation of these two measures of global macroeconomic conditions. Columns 3 and 4 of Table 13 take this one step further and use these interactions (*Global GDP growth* × *IMFprobability* and *Number of banking crises* × *IMFprobability*, respectively) as the excluded instruments. As the two regressions show, the two interactions do not enter with statistically significant signs in the first stage and produce Kleibergen-Paap F-statistics that are below 2. If global macroeconomic conditions were driving the associations we would see significant results here.

In sum, there is no evidence that the IV approach based on *IMFliquidity* picks up yearly variation in global macroeconomic conditions.

Challenging the identification III: Randomization

To further increase the confidence that the first stage does not pick up an artefact, I run placebo regressions in which I randomize the values of *IMFliquidity*. I run 1000 iterations of such regressions, which are based on a randomized order of the actual values of *IMFliquidity*, and find that the resulting IV coefficients are normally distributed around zero (Figure 6). The coefficient's t-statistics are all smaller than in the first-stage regression based on the actual values of *IMFliquidity*. None of the 1000 coefficients that emerge from the randomization is as distant from zero as the coefficient estimated based on the original data. This increases confidence in the mechanism driving the first-stage and suggests that it is unlikely that in the first stage the IV picks up an artefact.

Challenging the identification IV: IMF probability

Another modification concerns the second factor of the interacted instrument (Table 14). Like Nunn and Qian (2014) I also report results employing an IV based on a country-specific probability that does not vary over time, substituting $IMFprobability_{it}$ by $IMFprobability(constant)_i$, which is given by

$$IMFprobability(constant)_i = \frac{\sum_{T=1973}^{2013} \mathrm{I}(\mathrm{IMFprogram}_{iT} = 1)}{41}$$

I thereby make the probability multicollinear with the country fixed effects. While I am more convinced by the time-varying probability because it avoids using future realizations to explain the present, the results are robust to this modification.

Challenging the identification V: Selection on observables vs. unobservables

In the next table I report OLS and reduced form estimates (Table 15). First, I run OLS and OLS-fixed effect (FE) models (columns 1-2) and then calculate the OLS estimates for the baseline model, i.e., I do not instrument for IMF programs, ceteris paribus (columns 3-5). As the results show, IMF programs are correlated with higher inequality in OLS and OLS-FE regressions without control variables but there is no correlation when endogeneity is only insufficiently addressed in OLS-FE models with different sets of control variables. Together with the statistically significant effect found in the 2SLS regressions these results suggest that the proposed IV is able to eliminate the (negative) selection bias the OLS coefficients suffer from. In other words, a standard OLS-FE model with standard control variables would not be able to find the positive effect that the IV strategy is able to identify.

In columns 6-8 I report the results of reduced form regressions of the baseline specifications. They show that the IV has a statistically significant effect on inequality. This relationship is not significantly affected when a large vector of control variables is added to the regression. Following Altonji, Elder, and Taber (2005) this enhances the plausibility of the exclusion restriction: The comparison of the β-coefficients of the models with and without these covariates (6 vs. 8) shows that the so-called "selection ratio" is 3.12. This means that if the effect were in reality driven by unobserved variables, this selection on unobservables would have to be *more than three times* as large as the selection on observed variables, and it would have to go in the *opposite direction*.

Challenging the identification VI: Excluding the post-GFC period

Given that there was a strong increase in liquidity after the global financial crisis (GFC), one might be concerned that the utility of the instrument depends on including the period after the GFC. To test this, the regressions reported in Table 16 exclude the post-2008 period. The results show that in this restricted sample, the instrument maintains its relevance, the first-stage coefficient of the IV is similar in size as compared to the full sample and the Kleibergen-Paap F-statistics stay above 10.

Alternative instrumental variable

To compare the results to studies using the current standard instrument for IMF programs, I substitute the IV with UNGA voting behavior ceteris paribus (Table 17, columns 1-3). These regressions estimate IMF programs to cause rises in inequality of approximately four Gini points. First, these regressions support the main result. Second, however, considering that the estimated coefficients are equivalent to a change of up to 140 percent of a within-country standard deviation, this effect is strikingly large. One reason why these coefficients may be biased is that the instrument is not relevant enough; in specifications 2 and 3 the Kleibergen-Paap F-statistics fall below Stock and Yogo's (2005) lowest critical value of 5.53 that tolerates a 2SLS size distortion of 25 percent. A second reason could be that the instrument is not excludable. As argued above, plausible alternative channels are governments' political and ideological preferences. Under the assumption that the IV strategy applied in this paper identifies the causal effect of IMF programs, the baseline regressions provide empirical evidence for the violation of the exclusion restriction of UNGA voting: In the full baseline specification (see Table 7 in Appendix C for the full regression output), voting similarity with the United States in the UNGA is associated with higher levels of inequality when controlling for the causal effect of IMF programs. This finding suggests that UNGA voting is linked positively to inequality through channels other than IMF programs and is, thus, an invalid instrumental variable when the outcome of interest is inequality.[6]

An additional control variable: debt

The two baseline sets of control variables ("IMF controls" and "inequality controls") were selected based on findings of the previous literatures on the determinants of IMF programs and inequality. While neither of these literatures points to a particularly important role for debt (e.g., Moser and Sturm 2011, Dorsch and Maarek 2018), it stands to reason that debt is a relevant control variable in this setting. Countries with more debt could be more likely to seek assistance from the IMF and, at the same time, could be more likely to implement austerity reforms that increase inequality. While the IV strategy should remove any potential bias resulting from this hypothesized link, it would be reassuring to find that results hold when debt is controlled for. This is why Table 18 replicates the three baseline regressions but additionally controls for the country's debt-to-GDP ratio. This variable is taken from the IMF's historical public debt database (IMF 2020). The results hold.

Alternative dependent variables

Regarding the dependent variable, I first substitute the Gini coefficient of net income by that of gross income (*Gross Gini*) (Table 19, columns 1-3). The fact that the results are very similar, could indicate that IMF programs affect inequality mainly by leading to changes in the distribution of wages in contrast to affecting the extent of redistribution. This could, for instance, be driven by labor market reforms such as minimum wage reductions, cuts in pensions or by rising short-term unemployment after privatizations. An important caveat of these findings, however, is that the differences between market and net inequality that the SWIID indicates are not reliable for all countries (Solt 2016, 1274-5). Future research could investigate the exact channels in more detail. As a final robustness test, I change the inequality dataset. Until here I followed the related literature (Dorsch and Maarek 2018; Oberdabernig 2013) in choosing the SWIID as the source for panel data on Gini coefficients. Jenkins (2015) however, voices concerns about the SWIID's methodology and recommends the World

[6] As inequality is clearly linked to other economic conditions, analyses of IMF program effects on other economic outcomes are likely to suffer from the same problem when UNGA voting is used as an IV.

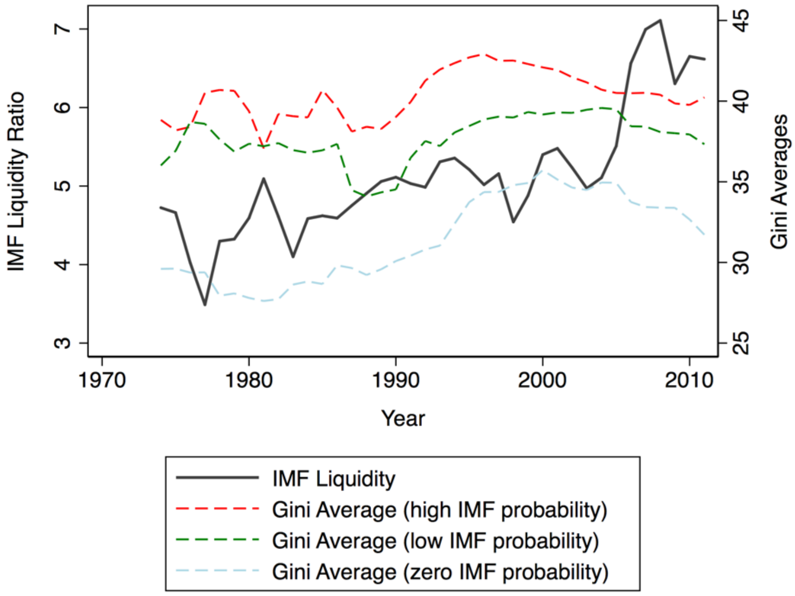
Income Inequality Database (WIID), on which the SWIID builds, over the SWIID.[7] The WIID, however, offers multiple Gini coefficients for many country-year observations. Since there is no commonly accepted procedure for choosing the respective values, the use of the WIID for regression analyses necessitates highly arbitrary decisions. This is presumably also why the SWIID is used much more frequently than the WIID. An alternative is offered by Milanovic (2014), who derives the final Gini value if multiple observations exist through "choice by precedence." While this approach makes sure that in each case the observation of the highest possible quality is chosen, it combines data from nine different sources with different methodologies without further standardization. Milanovic himself advises caution when using the resulting variable *Giniall* in regressions as the concepts underlying the calculation of the Gini coefficients are based on income and consumption, net and gross, as well as household and individual levels. Unfortunately, too few observations remain if the sample is restricted to one concept. Nevertheless, to address this issue I control for dummy variables that indicate the respective concepts interacted with country fixed effects. Columns 4-6 in Table 19 report the results. Note that, compared to the baseline, the sample size is severely limited. Nevertheless, the coefficient of interest is still consistently positive and statistically significant in the specifications that include control variables.

I conclude that the results are robust to these modifications.

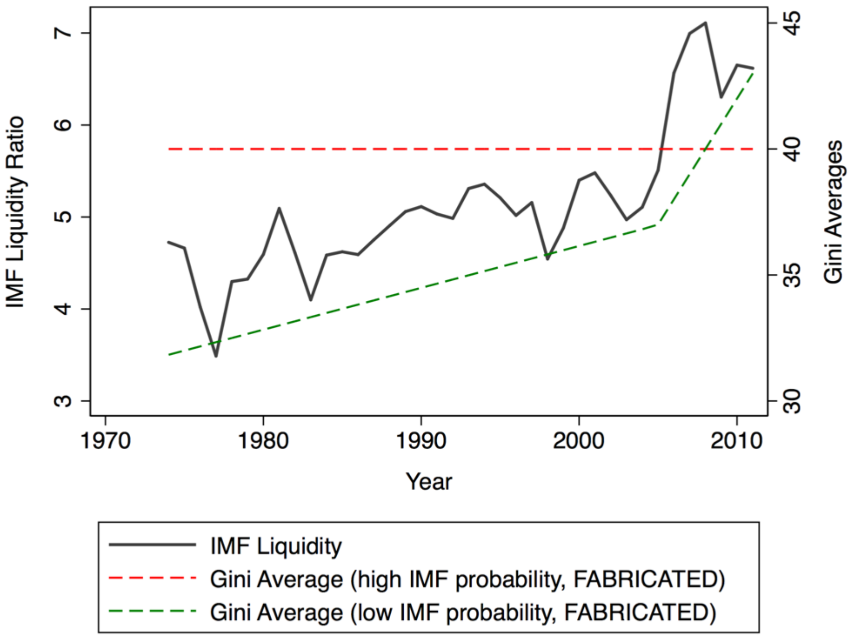
[7] Jenkins (2015) concerns, however, relate to an older version of the SWIID and Solt (2015) is able to overcome many of these concerns. The reader is referred to the entire special issue of the *Journal of Economic Inequality* (December 2015, Volume 13, Issue 4) for details on this debate.

Figure 5 – Spurious Correlations Between Inequality and IMF Liquidity?

Panel A: Actual Trends

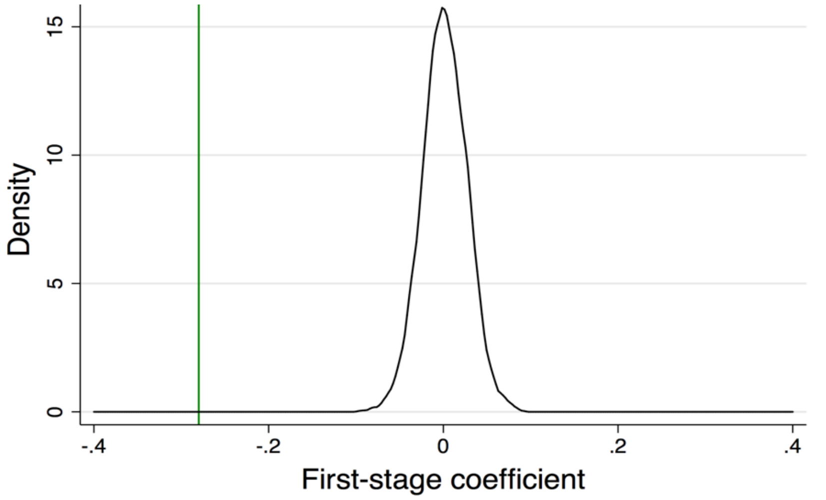


Panel B: Problematic Trends



Note: The figure plots the variation of *IMFliquidity* over time. The dashed lines plot the year-specific cross-country averages of *Gini* for sets of countries with above-median and below-median levels of *IMFprobability*. In Panel A, where the actual data is used, it becomes visible that time trends in *Gini* are very similar for both groups and that none of them follows the trend in *IMFliquidity*. Panel B illustrates with fabricated data how potentially problematic trends would look like (see p. SI-12). In this example, *IMFliquidity* is correlated with the long-term trend of *Gini* in low-probability countries, but not with the trend in high-probability countries.

Figure 6 – Randomizing IMF liquidity



Note: The graph plots the density distribution of 1,000 first-stage coefficients that are estimated when running 1,000 first-stage regressions based on a randomized order of the values of *IMFliquidity*. The horizontal line shows the first-stage coefficient based on the actual order of the values.

Figure 7 –IMF Liquidity and Global GDP Growth: Variation over Time

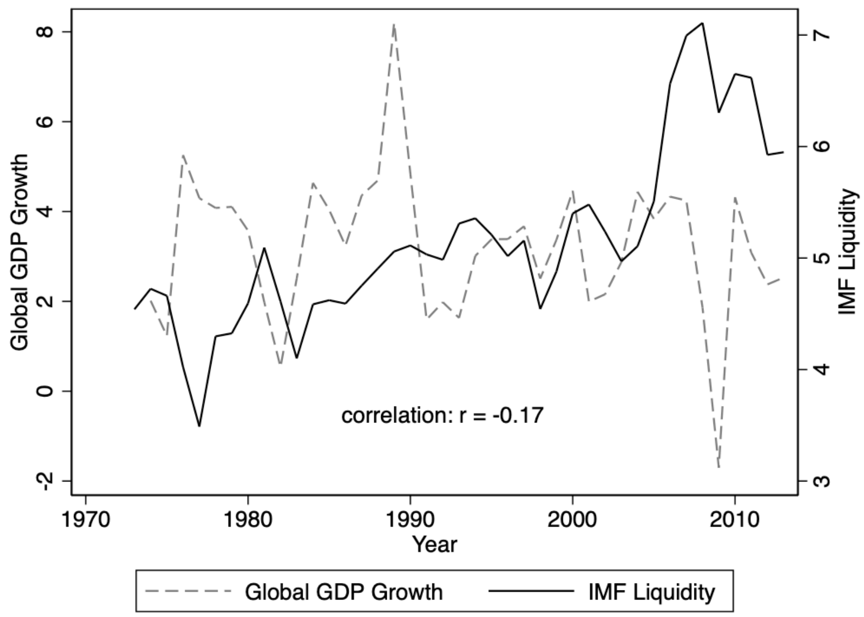


Figure 8 –IMF Liquidity and Global Crises: Variation over Time

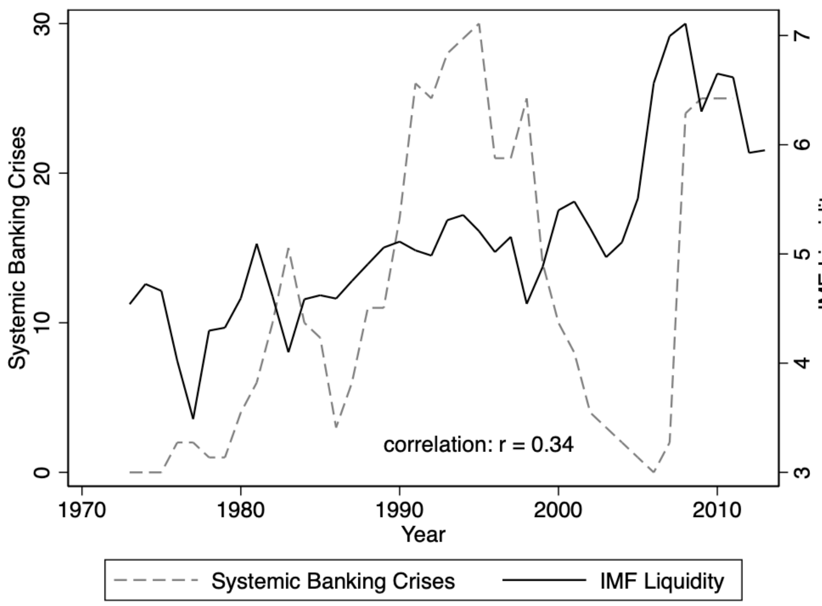


Figure 9 – IMF Liquidity and Global GDP Growth: Correlation

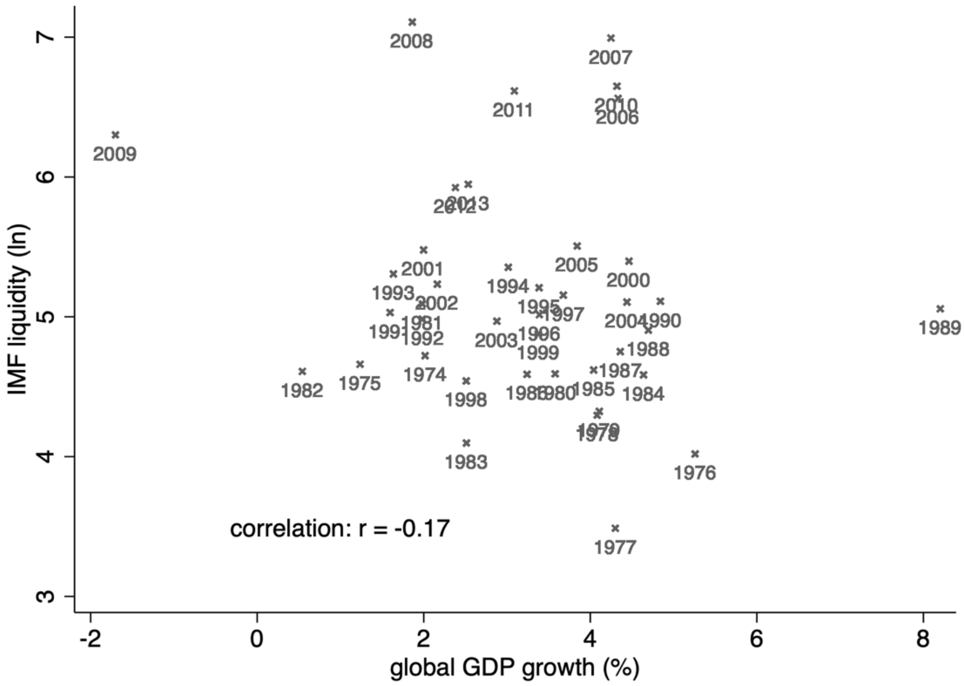


Figure 10 – IMF Liquidity and Global GDP Growth: Correlation

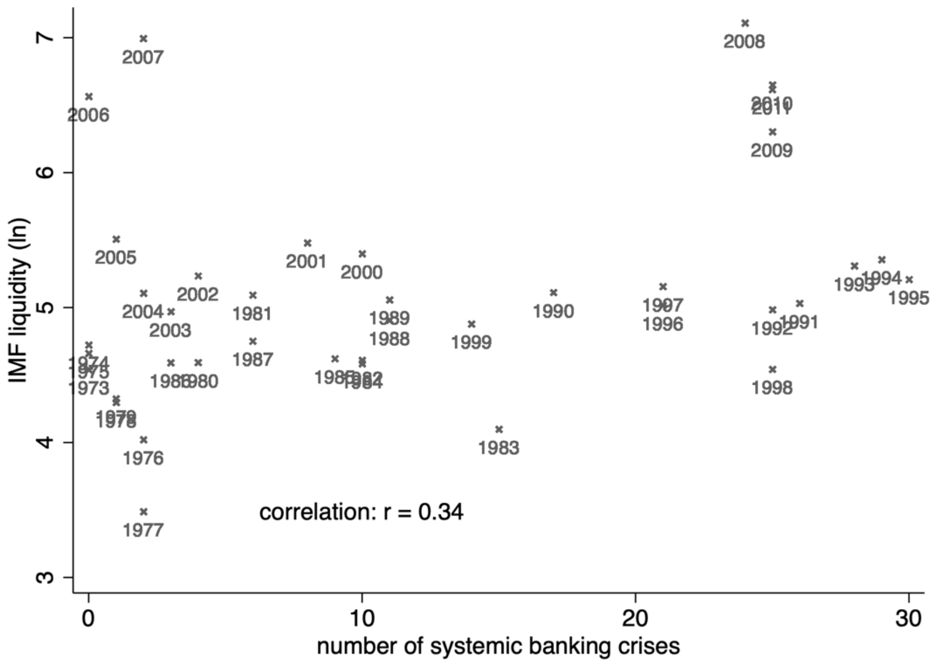


Table 12 – Robustness: Challenging the Liquidity Variable I

	Excluding large IMF transactions			IV with liquid resources		
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Second Stage			
IMF Program$_{t-1}$	1.222**	1.571***	1.472**	1.172*	1.551*	1.407**
	(0.561)	(0.579)	(0.595)	(0.668)	(0.869)	(0.622)
			Panel B: First Stage			
IV$_{t-1}$	-0.270***	-0.297***	-0.351***	-0.168***	-0.180***	-0.393***
	(0.050)	(0.058)	(0.066)	(0.043)	(0.049)	(0.093)
K.-P. underid. LM	18.058	16.123	19.212	12.652	11.078	18.637
K.-P. underid. p	0.000	0.000	0.000	0.000	0.001	0.000
K.-P. weak id. F	28.485	26.360	28.581	15.599	13.556	18.013
Inequality Controls (t-1)	No	Yes	Yes	No	Yes	Yes
IMF Controls (t-1)	No	No	Yes	No	No	Yes
N	3622	2844	2456	3766	2985	2573
Adjusted R^2	0.874	0.848	0.852	0.879	0.849	0.854

Note: Dependent variable *Gini*. All regressions control for *IMFprobability*, country fixed effects, year fixed effects, and the lagged dependent variable. Standard errors, robust to clustering at the country level, in parentheses. Significance levels: * p<.10, ** p<.05, *** p<.01

Table 13 – Robustness: Challenging the Liquidity Variable II

	(1)	(2)	(3)	(4)
			First stage:	
IMF liquidity	-0.367***	-0.366***		
x IMF probability	(0.069)	(0.069)		
Global GDP growth	0.006		-0.005	
x IMF probability	(0.027)		(0.023)	
Number of banking crises	0.006			0.006
x IMF probability	(0.005)			(0.004)
Excluded IV in first stage	IMF liquidity x IMF probability	IMF liquidity x IMF probability	Global GDP growth x IMF probability	Number of banking crises x IMF probability
			Second stage:	
IMF program	1.338**	1.359**	-23.083	-0.172
	(0.565)	(0.566)	(110.349)	(2.159)
Controls	Yes	Yes	Yes	Yes
Observations	2725	2725	2725	2725
K-P underidentification (p-value)	0.000	0.000	0.773	0.217
K-P weak identification (F-statistic)	27.426	26.863	0.083	1.574

Note: 2SLS regressions. Dependent variable is Gini (t+1). All regressions include country fixed-effects, year fixed-effects, and the lagged dependent variable. Standard errors, clustered at the country level, in parentheses. Significance levels: * p<.10, ** p<.05, *** p<.01

Table 14 – Robustness: Challenging the Probability Variable

	(1)	(2)	(3)
	Panel A: Second Stage		
IMF program$_{t-1}$	1.901*	1.557**	1.567**
	(1.145)	(0.680)	(0.706)
	Panel B: First Stage		
IMF liquidity x IMFprobability(constant)$_{t-1}$	-0.173**	-0.287***	-0.331***
	(0.071)	(0.072)	(0.076)
K.-P. underid. LM	4.877	10.832	12.933
K.-P. underid. p	0.027	0.001	0.000
K.-P. weak id. F	5.876	14.241	15.906
Inequality Controls	No	Yes	Yes
IMF Controls	No	No	Yes
N	3766	3010	2625
Adjusted R^2	0.851	0.838	0.841

Note: Dependent variable *Gini*. All regressions control for country fixed effects, year fixed effects, and the lagged dependent variable. Note that *IMFprobability(constant)* does not need to be controlled for because it is fully absorbed by country fixed effects. Standard errors, robust to clustering at the country level, are in parentheses. Significance levels: * p<.10, ** p<.05, *** p<.01

Table 15 – Robustness: Selection on Unobservables

	OLS	OLS-FE	OLS (Baseline)			OLS Reduced Form (Baseline)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IMF Program$_{t-1}$	5.113***	0.651**	0.016	0.063	0.108			
	(0.903)	(0.270)	(0.071)	(0.073)	(0.076)			
IV$_{t-1}$						-0.312**	-0.410**	-0.491**
						(0.142)	(0.161)	(0.204)
Selection Ratio $\beta_8 / (\beta_8 - \beta_6)$							3.12	
Country & Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LDV & IMFprob$_{t-1}$	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Inequality Controls (t-1)	No	No	No	Yes	Yes	No	Yes	Yes
IMF Controls (t-1)	No	No	No	No	Yes	No	No	Yes
N	3963	3963	3768	2987	2575	3768	2987	2575
Adjusted R^2	0.057	0.120	0.898	0.886	0.885	0.898	0.886	0.885

Note: Dependent variable Gini. Standard errors, robust to clustering at the country level, are in parentheses. Significance levels: * p<.10, ** p<.05, *** p<.01

Table 16 – Robustness: Instrument relevance without post-GFC period

	(1)	(2)	(3)
IMF liquidity x IMF probability$_{t-1}$	-0.214***	-0.250***	-0.284***
	(0.053)	(0.065)	(0.082)
IMF probability$_t$	2.530***	2.464***	2.907***
	(0.298)	(0.327)	(0.328)
Inequality Controls	No	Yes	Yes
IMF Controls	No	No	Yes
Sample	excl. post-2008	excl. post-2008	excl. post-2008
Observations	3376	2724	2319
K-P underidentification test (p)	0.000	0.001	0.001
K-P weak identification (F)	16.444	14.863	12.061

Note: First-stage of 2SLS regressions. Dependent variable in the first stage is IMF program.
The sample excludes the period after the global financial crisis (GFC) of 2008.
All regressions include country fixed-effects, year fixed-effects, and the lagged dependent variable (Gini). Standard errors, clustered at the country level, in parentheses.
Significance levels: * p<.10, ** p<.05, *** p<.01

Table 17 – Robustness: UNGA voting as IV

	(1)	(2)	(3)
		Panel A: Second Stage	
IMF Program$_{t-1}$	4.644***	3.921**	3.939**
	(1.773)	(1.962)	(1.984)
SBA/EFF Program$_{t-1}$			
		Panel B: First Stage	
UNGA voting$_{t-1}$	0.061***	0.075**	0.087**
	(0.023)	(0.037)	(0.037)
IV$_{t-1}$			
K.-P. underid. LM	6.084	2.211	3.362
K.-P. underid. p	0.014	0.137	0.067
K.-P. weak id. F	7.139	4.180	5.547
Inequality Controls	No	Yes	Yes
IMF Controls	No	No	Yes
N	3520	2910	2573
Adjusted R^2	0.626	0.671	0.658

Note: Dependent variable *Gini*. All regressions include, country fixed effects, year fixed effects and the lagged dependent variable. In columns 4-6 only SBA and EFF programs are used to calculate the variable *IMFprobability*, which the regressions also control for. Standard errors, robust to clustering at the country level, are in parentheses. Significance levels: * p<.10, ** p<.05, *** p<.01

Table 18: Controlling for Debt

	(1)	(2)	(3)
First stage:			
IMF liquidity x IMF probability	-0.263***	-0.287***	-0.342***
	(0.052)	(0.059)	(0.070)
Debt (% GDP)	0.000	0.001**	0.001***
	(0.000)	(0.000)	(0.000)
Second stage:			
IMF program	1.203**	1.245**	1.187**
	(0.586)	(0.551)	(0.585)
Debt (% GDP)	-0.001	-0.000	0.000
	(0.001)	(0.002)	(0.002)
Inequality controls	No	Yes	Yes
IMF controls	No	No	Yes
Observations	3738	2970	2558
K-P underid. (p)	0.000	0.000	0.000
K-P weak id. (F)	25.934	23.629	23.924

Note: 2SLS regressions. Dependent variable is Gini. All regressions include country fixed-effects, year fixed-effects, and the lagged dependent variable. Standard errors, clustered at the country level, in parentheses. Significance levels: * p<.10, ** p<.05, *** p<.01

Table 19 – Robustness: Alternative Inequality Data

	Gross Gini (SWIID)			ATG Data		
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Second Stage			
IMF Program$_{t-1}$	1.666***	1.395**	1.278**	1.220	2.038**	2.072**
	(0.561)	(0.544)	(0.566)	(1.155)	(0.912)	(1.046)
			Panel B: First Stage			
IV$_{t-1}$	-0.276***	-0.316***	-0.373***	-0.557***	-0.664***	-0.647***
	(0.053)	(0.060)	(0.069)	(0.113)	(0.133)	(0.137)
K.-P. underid. LM	18.804	17.952	19.987	12.093	12.702	10.889
K.-P. underid. p	0.000	0.000	0.000	0.001	0.000	0.001
K.-P. weak id. F	27.637	28.099	28.928	24.112	25.099	22.151
Inequality Controls	No	Yes	Yes	No	Yes	Yes
IMF Controls	No	No	Yes	No	No	Yes
ATG Controls	No	No	No	Yes	Yes	Yes
N	3765	2984	2572	928	812	736
Adjusted R^2	0.870	0.867	0.858	0.493	0.511	0.484

Note: Dependent variables *Gross Gini* (columns 1-3) and *Giniall* (columns 4-6). All regressions control for *IMFprobability*, country fixed effects, year fixed effects, and the lagged dependent variable. Standard errors, robust to clustering at the country level, in parentheses.
Significance levels: * p<.10, ** p<.05, *** p<.01

Appendix H: Heterogeneity II: Conditionality, Loan Size, Concessional Loans

In the main text, I examine the heterogeneous effect of IMF programs depending on the extent and design of conditionality by making comparisons within the set of IMF programs. This appendix implements an alternative approach. It applies the baseline IV setup but uses alternative treatment variables that take the scope of conditionality into account. In two additional exercises it also disaggregates IMF programs by their loan size and their degree of concessionality.

On a cautionary note, it should be noted that the IV strategy is not ideally suited to analyze such disaggregations. The IV strategy builds on a quasi-exogenous source of variation in a country's probability to receive an IMF program. It does not have a theory that links this source of variation to the scope of conditionality or to loan size as these are selection processes that are somewhat different to the "selection into programs." It is thus not clear whether in such regressions the first stage will be strong enough and whether the IV strategy solves the problems of "selection into conditionality" and "selection into loan size." While these results should be interpreted with caution, the reader might still be interested to see whether these auxiliary results are in line with the interpretation of the main results.

Conditionality

Specifically, I take panel data on the extent of IMF conditionality and use a variable that indicates the number of binding IMF conditions that are applicable for a given country in a given year (Kentikelenis et al. 2016). Based on this measure, I code two binary variables indicating observations with active IMF programs that are above the median of conditions ("many conditions") and below the median ("few conditions"). I then use these alternative binary indicators for IMF programs in a new set of regressions. These regressions are specified as in the baseline except that the variable *IMFprogram* is substituted by the alternative binary indicators. This implies that the variable *IMFprobability* is also adjusted and calculated based on the respective alternative binary indicators of *IMFprogram*.

The results are presented in column 1 and 2 of Table 20. The regression based on "IMF program, many conditions" (column 1) produces a positive and statistically significant coefficient, suggesting that the positive baseline effect is driven by IMF programs with many conditions, in line with the main results. The regression in column 2 is based on "IMF program, few conditions." In this specification, the first stage is too weak to produce meaningful results. The first-stage Kleibergen-Paap F-statistic is close to 1 leading to a highly imprecise second-stage coefficient that cannot be interpreted in a meaningful way. While we thus cannot infer whether IMF programs with few conditions affect inequality, we can cautiously interpret these results as further evidence for the hypothesis that conditionality is a mechanism for the main effect.

Financing

Beyond conditionality, an alternative mechanism for the main effect could be the amount of money provided to the recipient country (the "financing" mechanism). To test this alternative mechanism, I collect additional data on IMF loan size (IMF 2018). I then use these data to calculate loan-to-GDP ratios for all countries under IMF programs and code two binary variables indicating those observations with a loan-to-GDP ratio above the median ("large loan") and below the median ("small loan"), analogous to the approach for conditionality.

The results are presented in column 3 and 4 of Table 20. Column 3 focuses on IMF programs with "large loans." Based on a sufficiently strong first stage, this regression produces an insignificant coefficient, suggesting that the baseline effect is not driven by IMF programs with large loans. This is further supported by column 4 which produces a statistically significant, positive coefficient for IMF programs with smaller loans. Taken together, these two regressions provide evidence against the hypothesis that IMF programs with large loans are behind the main effect.

In sum, these additional analyses lend further support to the hypothesis that the main effect is due to the "conditionality mechanism" while they provide no support for the idea that it is due to a "financing mechanism."

Concessional programs vs. non-concessional programs

An alternative way to examine heterogeneous effects of IMF programs is to differentiate between concessional and non-concessional IMF programs. Non-concessional loans, which in the observation period were primarily organized under the SBA ("Stand-By Arrangement") and the EFF ("Extended Fund Facility") facilities are usually short-term loan programs that react to urgent economic crises and often include strong policy conditions. Concessional loans, on the other hand, which in the observation period were organized under the IMF's lending facilities PRGF ("Poverty Reduction and Growth Fund") and SAF ("Structural Adjustment Facility") are more long-term forms of financial assistance or insurance and typically include fewer policy conditions (Barro and Lee 2005; Oberdabernig 2013).

The theoretical considerations in the main text suggest that the effect should be primarily driven by programs with particularly stringent conditionality and thus by non-concessional IMF programs rather than by concessional ones. Table 21 implements this differentiation by separately examining the effects of non-concessional programs (SBA or EFF) and concessional programs (PRGF or SAF). The results show that non-concessional IMF programs increase inequality (columns 1-3) while concessional ones do not (columns 4-6).

An additional plausibility check

Note that in Table 21 the *IMFprobability* variable is based only on the types of IMF programs that are examined in the respective specification (concessional vs. non-concessional). This different definition of *IMFprobability* can be used for a plausibility check of the first-stage effect: The *IMFprobability* variable based on concessional programs should be less likely to predict non-concessional programs than the *IMFprobability* variable based on non-concessional programs; and vice versa.[8] I implement this plausibility check in Table 22. It is reassuring for the identification strategy that these regressions yield the expected pattern: Across all six specifications the first-stage coefficients in Table 22 are closer to zero and less precisely estimated and the Kleibergen-Paap F-statistics, which test instrument relevance, are substantially smaller than in Table 21.

[8] I thank an anonymous reviewer for this idea.

Table 20 – Conditionality or Financing?

	(1)	(2)	(3)	(4)
IMF program, many conditions (above median)	1.456*			
	(0.863)			
IMF program, few conditions (below median)		-10.395		
		(10.550)		
IMF program, large loan-to-GDP ratio (above median)			0.114	
			(0.519)	
IMF program, small loan-to-GDP ratio (below median)				2.491**
				(1.064)
Controls	Yes	Yes	Yes	Yes
Observations	2573	2573	2573	2573
Adjusted R-squared	0.852	-0.344	0.879	0.810
K-P underidentification (p-value)	0.000	0.291	0.000	0.007
K-P weak identification (F-statistic)	21.672	1.138	37.749	12.220

Note: 2SLS regressions. Dependent variable: Gini. All regressions include country fixed-effects and year fixed-effects as well as the lagged dependent variable. Standard errors, robust to heteroskedasticity and correlation at the country level, are in parentheses.
Significance levels: * p<.10, ** p<.05, *** p<.01

Table 21: Concessional and Non-concessional IMF programs

	(1)	(2)	(3)	(4)	(5)	(6)
First Stage:						
IMF probability (non-concessional) x IMF liquidity	-0.558***	-0.579***	-0.542***			
	(0.059)	(0.065)	(0.072)			
IMF probability (non- concessional)	4.198***	4.170***	4.424***			
	(0.313)	(0.353)	(0.333)			
IMF probability (concessional) x IMF liquidity				-0.719***	-0.685***	-0.672***
				(0.115)	(0.123)	(0.121)
IMF probability (concessional)				5.709***	5.417***	5.179***
				(0.670)	(0.723)	(0.687)
Second Stage:						
IMF program (non-concessional)	0.762***	0.779***	0.821**			
	(0.286)	(0.282)	(0.380)			
IMF program (concessional)				-0.138	0.077	0.318
				(0.620)	(0.717)	(0.738)
Inequality Controls	No	Yes	Yes	No	Yes	Yes
IMF Controls	No	No	Yes	No	No	Yes
Observations	3766	2985	2573	3766	2985	2573
Adjusted R-squared	0.890	0.876	0.874	0.894	0.881	0.878
K-P underidentification (p-value)	0.000	0.000	0.000	0.000	0.000	0.000
K-P weak identification (F-statistic)	89.310	78.186	56.246	39.239	31.187	30.924

Note: 2SLS regressions. Dependent variable: Gini. All regressions include country fixed-effects and year fixed-effects as well as the lagged dependent variable. Standard errors, robust to heteroskedasticity and correlation at the country level, are in parentheses.
Significance levels: * p<.10, ** p<.05, *** p<.01

Table 22: Concessional and Non-concessional IMF programs: First-stage plausibility check

	(1)	(2)	(3)	(4)	(5)	(6)
First Stage:						
IMF probability (concessional) x IMF liquidity	0.412***	0.390***	0.356***			
	(0.094)	(0.108)	(0.105)			
IMF probability (concessional)	-2.878***	-3.041***	-2.864***			
	(0.688)	(0.766)	(0.688)			
IMF probability (non-concessional) x IMF liquidity				0.040	0.054	0.058
				(0.036)	(0.041)	(0.049)
IMF probability (non- concessional)				-0.295	-0.398*	-0.906***
				(0.219)	(0.226)	(0.322)
Second Stage:						
IMF program (non-concessional)	0.241	-0.136	-0.599			
	(1.085)	(1.258)	(1.398)			
IMF program (concessional)				-10.553	-8.346	-7.678
				(9.578)	(6.323)	(6.383)
Inequality Controls	No	Yes	Yes	No	Yes	Yes
IMF Controls	No	No	Yes	No	No	Yes
Observations	3766	2985	2573	3766	2985	2573
Adjusted R-squared	0.893	0.880	0.872	0.371	0.456	0.500
K-P underidentification (p-value)	0.000	0.000	0.000	0.248	0.162	0.223
K-P weak identification (F-statistic)	19.425	13.127	11.575	1.242	1.766	1.419

Note: 2SLS regressions. Dependent variable: Gini. All regressions include country fixed-effects and year fixed-effects as well as the lagged dependent variable. Standard errors, robust to heteroskedasticity and correlation at the country level, are in parentheses.
Significance levels: * p<.10, ** p<.05, *** p<.01

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