

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 conditionality is more extensive. These results suggest that IMF programs can constrain government responsiveness to domestic distributional preferences.

Keywords: International Monetary Fund (IMF), Inequality

JEL codes: F53, O19

1 Introduction

Over the course of the last decades, income inequality has been rising in many countries. While multiple factors have contributed to this development, there is now a broad consensus that changing national policies explain a substantial part of it (OECD 2011; World Bank 2016; World Inequality Lab 2017). Quite naturally, this prompts the question as to why so many countries changed their policies in a way that inequality increased. While the literature has often searched for answers by examining the pressures that economic globalization exerts on policies that ensure a more equal 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, 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 (e.g., Reinsberg et al. 2018). 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, Sturm, and Vreeland 2015; Nelson 2014; Stone 2008), the design of IMF programs is likely 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 governments 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). The existing approaches in the literature that addresses this problem either control only for observables or rely on problematic instrumental variables (IVs) that are likely to violate the exclusion restriction. 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.¹

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, the article contributes to the literatures on the IMF's effects (e.g., Dreher and Walter 2010; Kentikelenis, Stubbs, and King 2016; Reinsberg et al. 2018; Vreeland 2007a) and on IMF decision-making (e.g., Copelovitch 2010; Dreher, Sturm, and Vreeland 2009; Schneider and Tobin 2020; Stone 2008). It offers a combination of these literatures by arguing that the specific structure of IMF decision-making has direct implications for the IMF's effects. From a methodological perspective, the article speaks to research on the political economy of international organizations (e.g., Dreher and Lang 2019; Vreeland 2019) by proposing a new approach for estimating the effects of international organizations. More generally, it speaks to the literature on the unintended consequences of official financial assistance (e.g., Easterly 2005; Knack 2000) and its interactions with domestic democracy (e.g.,

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, Kaplan, and Wong 2019; Nelson and Wallace 2017; Schneider and Tobin 2020; Stubbs et al. 2018).

Gartzke and Naoi 2011; Knack 2004; Nelson and Wallace 2017). Lastly, it adds a ‘global governance’ perspective to the growing literature on the causes behind the current trend of rising inequality.

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 the Appendices. 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, 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 then runs via national parliaments, governments, their representatives in the IMF’s executive board, and ends with the IMF’s staff. Based on the literature on IMF decision-making, 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 recent reviews of this voluminous literature see Dreher and Lang 2019 and Vreeland 2019).

The delegates of these governments not only have the largest formal voting power in the Executive Board, but also does much of their considerable impact on IMF policies run through so-called “informal governance” (Stone 2008). The literature shows that 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 2010a) interests. The governments of the countries that receive most IMF programs, on the other hand, tend to lack both significant formal voting power and substantial informal channels of influence. For instance, most typical program countries do not have individual representatives in the Board as minor IMF shareholders are grouped and represented by joint Directors – an institutional arrangement that weakens their 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). 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 and deregulating their labor markets (Woods 2006).² For the IMF bureaucracy, the gradual expansion of the scope of IMF conditionality into policy areas like labor-market regulation has often been linked to the bureaucratic incentive to expand the organization's mission (Barnett and Finnemore 2004; Dreher and Lang 2019; Kentikelenis, Stubbs, and King 2016; Reinhart and Trebesch 2016). Furthermore, scholars have also identified a tendency among IMF staff to favor market-liberal ideas over

² In a sense, the IMF is at the center of what Krasner's (1985) called "structural conflict."

government intervention in market processes and provided evidence that policies stipulating reduced public spending as well as trade and financial liberalization are associated to these ideological preferences (Barnett and Finnemore 1999; Chwieroth 2007a; Nelson 2014).

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, 600) “there is almost always some limit on public debt or government spending.” According to Kentikelenis, Stubbs, and King (2016), more than 70 percent of IMF programs include conditions on trade and financial liberalization and about 50 percent set labor-market-related 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 (e.g., Rickard and Caraway 2018; 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, 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, Stubbs, and King 2015; Nooruddin and Simmons 2006; Rickard and Caraway

2018), and less-regulated labor markets (Blanton, Blanton, and Peksen 2015; Caraway, Rickard, and Anner 2012). 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 program 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. (Furceri and Loungani 2018; de Haan and Sturm 2017; 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 lead to increased 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). But for any study that aims to identify the effects of IMF programs, the central challenge 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, 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.³

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

³ For details on problems of selection models without exclusion restrictions, see Puhani (2000).

the United States (or other G5 countries) in the UN General Assembly (UNGA) has become the ‘standard instrument’ for IMF programs.⁴ However, as the other IVs used in this literature, this measure is not clearly excludable to macroeconomic outcomes at the country-level.⁵ It rests on the assumption that IMF programs are the only channel through which a country’s UNGA voting behavior is linked to economic outcomes in the same country. But it is likely that a government’s foreign policy preferences articulated in UNGA voting are related to a government’s preferences in domestic policy, which in turn are clearly linked to economic outcomes.⁶ 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, Strezhnev, and Voeten 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.⁷

⁴ Barro and Lee (2005) first proposed this IV.

⁵ 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).

⁶ For theory, see Moravcsik (1997); for empirical evidence, see Mattes et al. (2015).

⁷ 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.

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. This pattern is often attributed to “recidivism” and bureaucratic incentives of IMF staff (Bird, Hussain, and Joyce 2004; Dreher and Lang 2019).⁸ In this paper, I detect a specific heterogeneity in this relationship and exploit it for a new identification strategy. I find that the link between past IMF participation and present IMF participation crucially depends on the IMF’s liquidity. In years in which IMF liquidity is relatively low, IMF programs indeed 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.

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, Hussain, and Joyce 2004; Reinhart and Trebesch 2016). On the other hand, it 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

⁸ See Knack (2000) for the related concept of “aid dependence.”

relatively little used.⁹ This can explain why in years with high IMF liquidity the link between a country's history of IMF participation and its likelihood of present program participation is substantially weaker than in years with low IMF liquidity.

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:

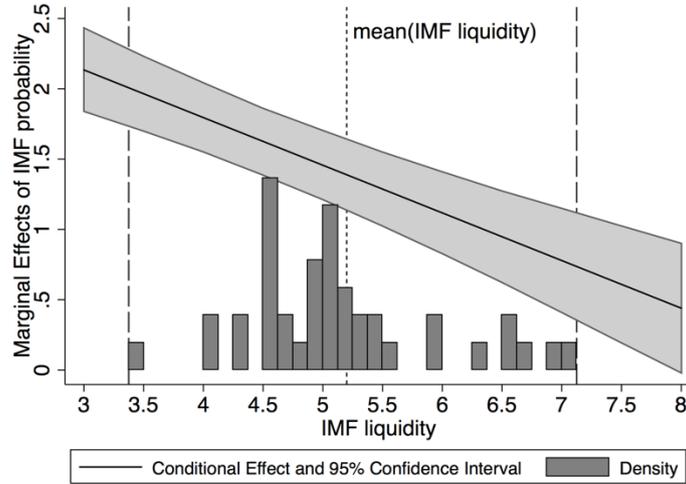
$$\begin{aligned} IMFprogram_{i,t} = & \alpha_1(IMFprobability_{i,t} \times IMFliquidity_t) \\ & + \alpha_2 IMFprobability_{i,t} + \mathbf{X}'_{i,t} \alpha_3 + \delta_i + \tau_t + u_{i,t} \end{aligned}$$

IMFprogram is a binary variable indicating that country *i* was under an IMF program for at least five months in year *t* (Dreher 2006b, 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.¹⁰ \mathbf{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.

⁹ 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.

¹⁰ 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 *IMFlquidity*. Based on specification 1 in Table 1 (see below).

Figure 1 visualizes the result of this first-stage regression.¹¹ It shows that in years with higher IMF liquidity the probability of past IMF participation is a substantially weaker predictor of IMF programs. In these years, the Fund is more generous¹² and implements more programs for countries beyond its more regular clientele. 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 (Nunn and Qian 2014). The constituent terms of the interaction are controlled for in both stages of the 2SLS-regression and not assumed to be exogenous.¹³ As in other approaches that are based on a difference-in-differences logic, threats to the identifying assumption can therefore only result from a specific pattern: Even if there was endogeneity between the IMF’s liquidity and inequality, the exclusion restriction would only be violated if the unobserved variables driving this endogeneity were affecting inequality differently in

¹¹ Detailed results, including tests of instrument relevance, are reported in the results section.

¹² 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 ($r = .3$).

¹³ Note that the year fixed effects control for the level effect of *IMFlquidity*.

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.¹⁴ 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. 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, it is unlikely that they are linked to inequality trends in individual countries. But more importantly, even if this was the case, such an unobserved link would bias the result only if it was dependent on a country's *IMFprobability*. This is why the dashed lines in the graph 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.¹⁵

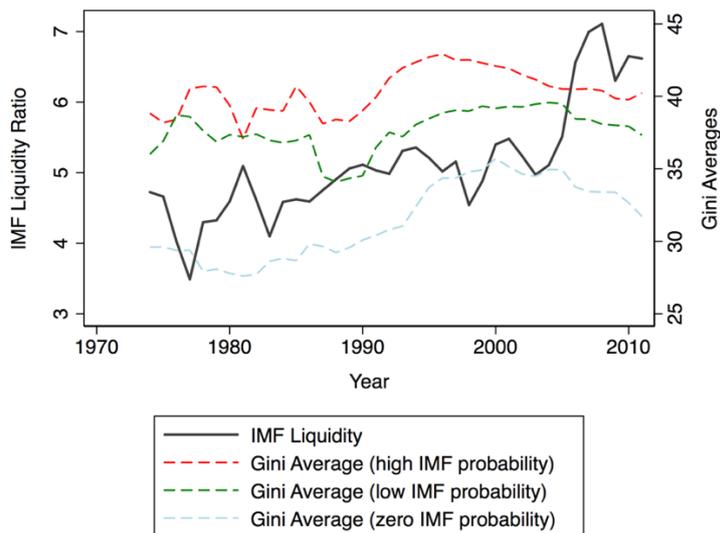
In sum, it is unlikely that there are unobserved variables that affect a potential correlation between the IMF's liquidity and income inequality conditional on how regularly a country has received IMF programs in the past. The fact that all regressions include two-way fixed effects

¹⁴ 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.

¹⁵ For further clarification, Figure 4 in Appendix G illustrates a fabricated scenario in which the identifying assumption could be violated. Furthermore, Figures 6 and 7 in Appendix G show that the IMF's liquidity is not correlated with global economic cycles as measured by global GDP growth or the global number of banking crises.

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.

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*.

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:

$$\begin{aligned}
 Inequality_{i,t} &= \beta \widehat{IMFprogram}_{i,t-1} \\
 &+ \gamma IMFprobability_{i,t-1} + \mathbf{X}'_{i,t-1} \mu + \delta_i + \tau_t + u_{i,t}
 \end{aligned}$$

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).

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.¹⁶ 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*. The second set of covariates includes variables that the literature identified as key determinants of IMF programs: *Current Account Balance (% GDP)*, *Investments (% GDP)*, *GDP Growth*, *UNGA Voting*, and an indicator for the presence of a systemic *Banking Crisis*.

To enhance the plausibility of the exclusion restriction I additionally add as controls the interaction of the global total of both *Banking Crises* and *Global GDP Growth* with *IMFprobability*, to show that global cycles of growth and crises do not influence both the IMF's liquidity ratio and inequality differently in countries with different IMF participation histories. As current levels of inequality are heavily dependent on previous levels I follow the standard in the literature and include the lagged dependent variable.¹⁷ The annual data cover the 1973-2013 period and a maximum of 155 countries.

¹⁶ For descriptive statistics, definitions, and sources see Appendix A.

¹⁷ Dorsch and Maarek 2018; 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.

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 percent level and the Kleibergen-Paap F-statistics comfortably surpass conventional levels of weak identification tests.¹⁸

These results are robust across specifications without covariates¹⁹ (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.

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 ($p_{(1)}=0.030$; $p_{(2)}=0.010$,

¹⁸ 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.

¹⁹ *IMFprobability*, the lagged dependent variable, and the fixed effects are always controlled for.

$p_{(3)}=0.018$) 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.

Table 1 – IMF Programs and Income Inequality

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

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

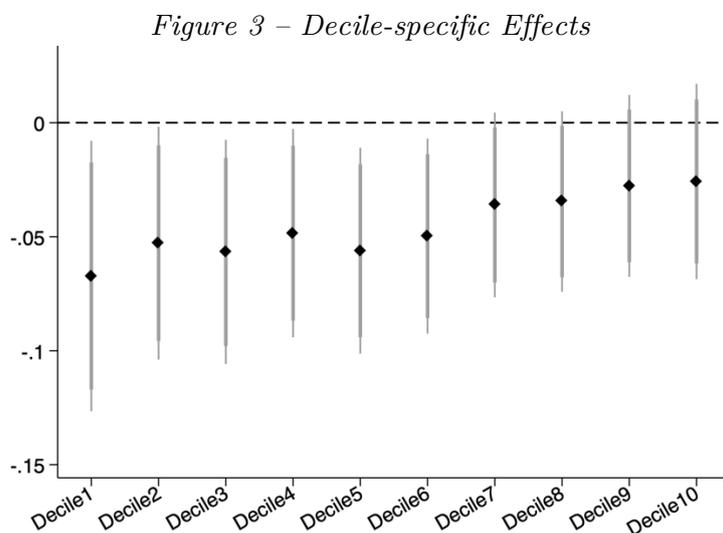
Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$

See Appendix C for the full regression output.

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).



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 IMF programs 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. While results are based on the baseline specification without controls they are very similar when these are added (Appendix E).

Figure 4 – Long Term Effects

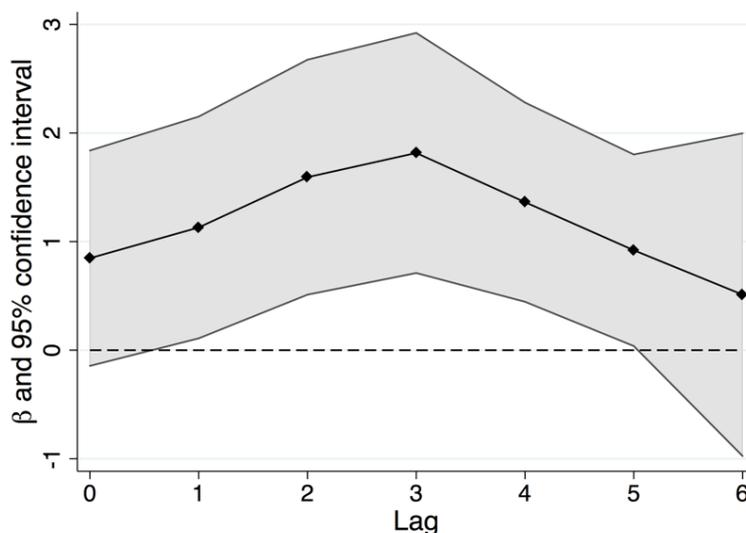


Table 2 – Long Term Effects

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

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

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

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.

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. 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.²⁰ 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 (e.g., Huber, Mustillo, and Stephens 2008; Jensen and Skaaning 2015, Rodrik 1998). An IMF program in a democracy can cut and reverse these policies by constraining the government's responsiveness to these preferences for a limited period, thereby leading to an increase in inequality. In a non-democracy, however, an IMF program can constrain pre-existing government responsiveness to a lesser degree because responsiveness is already lower. Accordingly, there are, on average, fewer pro-poor policies that IMF programs can cut or reverse and inequality often is already higher. In the baseline sample the average Gini is 36 in democracies and 40 in non-democracies.

²⁰ 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).

To test this extension of the argument, democracies and non-democracies are considered separately in Table 3.²¹

Table 3 – Democracies and Nondemocracies

	Democracies				Non-Democracies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Second Stage								
IMF	1.901**	2.063**	2.271***	1.829**	-0.057	-0.363	-0.031	-0.407
Program _{t-3}	(0.739)	(0.810)	(0.861)	(0.824)	(2.260)	(0.787)	(1.462)	(1.141)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Sample split	1 st & 2 nd stage	1 st & 2 nd stage	2 nd stage	2 nd stage	1 st & 2 nd stage	1 st & 2 nd stage	2 nd stage	2 nd stage
Obs.	2094	1743	3766; 2094	2526; 1743	1317	878	3766; 1317	2526; 878
Panel B: First Stage								
IV	-0.315***	-0.367***	-0.276***	-0.356***	-0.142	-0.464***	-0.276***	-0.356***
	(0.077)	(0.088)	(0.052)	(0.067)	(0.125)	(0.168)	(0.052)	(0.067)
K.-P. underid. p	0.001	0.001	0.000	0.000	0.256	0.026	0.000	0.000
K.-P. weak id. F	16.958	17.545	27.699	24.121	1.286	7.647	27.699	24.121

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

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.²² The effect is robust

²¹ 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.

²² 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.

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 not statistically significant at conventional levels.²³ In line with theoretical expectations, the distributive effects of IMF programs imply a more substantial divergence from the counterfactual policies in democracies.

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.

²³ 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%.

For this analysis, the sample is restricted to country-years in which an IMF program begins, following the approach by Rickard and Caraway (2018) 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:

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

Initially, *IMFconditions* indicates the ‘scope of conditionality’ defined as the number of policy areas that conditions cover (in the spirit of Dreher, 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 and suggests a role for both IMF-mandated labor market reforms and cuts in social spending.²⁴

Table 4 – IMF Conditionality

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

Robustness

The following section summarizes additional tests that examine the robustness of these results. They are presented in more detail in Appendix G (pages 12-25). First, concerns regarding the exclusion restriction are addressed. The results are robust 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 10). To 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 the coefficient estimated based on the real temporal order. Substituting

²⁴ In a recent paper, Forster et al. (2019) focus on how different IMF conditions affect inequality.

the time-varying probability by a time-invariant probability that is absorbed by country fixed effects also does not affect the results (Table 11). 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 12).

Second, to relate the results to previous studies aiming to estimate a causal IMF effect, I substitute the IV with UNGA voting (Table 13, columns 1-3). While results point 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 (Appendix D) 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.²⁵

Third, I modify the variables of interest. While the baseline definition of IMF programs follows the literature's standard, for robustness I use Barro and Lee's (2005) definition and only consider Stand-By Arrangements and the Extended Fund Facility and find very similar results (Table 13, columns 4-6). 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 14, columns 1-3). Additionally, I employ ATG data as an alternative to the SWIID (Table 14, columns 4-6). Even though this dramatically reduces the sample size, the results are again robust.

²⁵ Furthermore, F-statistics for UNGA-voting as IV are below critical thresholds.

5 Conclusions

According to the results presented in this article, IMF loan programs increase inequality of incomes 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.”²⁶ 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) most recent *Review of Program Design and Conditionality*, which notes a “limited focus on the quality of social spending, and on social protection and inequality” (p.24). The fact that the same document states that “[m]any stakeholders emphasized that program design should be more attentive to the potential negative impacts of conditionality on [...] inequality” (p. 5) could indicate an emerging willingness inside the IMF to tackle this issue more seriously.

Future research on the IMF 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

²⁶ On Twitter, June 17, 2015, <https://twitter.com/lagarde/status/611261555372621824>.

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 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	38.08	9.23	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	1	Indicator 1 if IMF program in place for at least 5 months in year t, (Dreher 2006).
IMF liquidity (ln)	5.42	0.75	4.1	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	1	$\frac{\sum_{T=1973}^t I(\text{IMFprogram}_{it} = 1)}{t-1973}$ Own calculation based on (Dreher 2006).
GDP per capita (ln)	8.21	1.61	4.92	11.38	Gross domestic product per capita in constant 2005 USD (World Bank 2016)
Education	7.56	2.85	0.89	13.18	Average years of schooling, linear interpolation of data for five-year periods (Barro and Lee 2013)
Trade	77.09	51.26	12.01	439.66	Trade (% GDP) (World Bank 2016)
Life Expectancy	68.71	9.61	27.08	82.93	Life expectancy at birth in years (World Bank 2016)
Democracy	0.66	0.47	0	1	Indicator 1 if Polity IV index is 6 or higher (Marshall, Jaggers, and Gurr 2011)
Current account balance	-2.1	6.53	-47.21	26.77	Balance on current account (% GDP) (IMF 2016).
Investments	23.11	7.14	-2.42	74.82	Gross capital formation (% of GDP) (World Bank 2016).
GDP growth	3.63	4.45	-50.25	35.22	GDP growth (annual %) (World Bank 2016).
Banking crisis	0.11	0.31	0	1	Indicator 1 if systemic banking crisis in year t in country i, (Laeven and Valencia 2012).
UNGA voting	0.14	0.91	-2.14	3.01	Ideal point of voting behavior in the UNGA (Bailey, Strezhnev, and Voeten 2017).
Global GDP growth	3.03	1.51	-2.08	6.98	Growth of global GDP; own calculations based on World Bank (2016).

Banking crises	14.54	10.12	0	30	Global total of Banking Crisis in year t, based on Laeven and Valencia (2012)
Liquid resources (ln)	11.3	0.66	9.84	12.96	IMF liquid resources (see LQR)
Gross Gini	45.36	7.12	20.25	71.13	Gini coefficient of market income according to the SWIID version 5.0 (Solt 2016)
Gini (ATG)	39.63	9.91	20	69.8	Gini coefficient (Gini _{all}) according to the ATG Dataset (Milanovic 2014)

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 9 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

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

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

$$(P \times 0.5) + (P \times \frac{1-S}{S} \times 0.5) = M$$
$$P = 2MS$$

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)
IMFliquidity	-0.276***	-0.308***	-0.356***
× IMFprobability	(0.052)	(0.063)	(0.067)
IMFprobability	2.760***	2.673***	3.172***
	(0.282)	(0.315)	(0.286)
Gini	0.003	-0.001	-0.003
	(0.003)	(0.004)	(0.005)
GDP per capita (ln)		-0.177	-0.135
		(0.280)	(0.331)
GDP per capita Squared (ln)		-0.002	-0.007
		(0.017)	(0.020)
Education		-0.057**	-0.058**
		(0.025)	(0.028)
Trade		-0.000	-0.001
		(0.001)	(0.001)
Life Expectancy		0.008	0.010*
		(0.005)	(0.006)
Regime Type		-0.005	0.004
		(0.049)	(0.053)
Current Account Balance			0.002
			(0.003)
Investments			-0.006**
			(0.003)
GDP Growth			0.002
			(0.002)
Banking Crisis			0.080**
			(0.038)
UNGA Voting			0.102***
			(0.034)
Global GDP Growth ×			0.002
IMFprobability			(0.027)
Banking Crises			0.006
× IMFprobability			(0.004)
Observations	3766	3010	2625
K.-P. underid. LM	18.452	16.045	18.973
K.-P. underid. p	0.000	0.000	0.000
K.-P. weak id. F	27.699	24.121	28.441

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** (0.521)	1.435*** (0.550)	1.288** (0.573)
IMFprobability _{t-1}	-1.844** (0.841)	-1.901** (0.881)	-2.591** (1.087)
Gini _{t-1}	0.916*** (0.011)	0.915*** (0.013)	0.911*** (0.014)
GDP per capita (ln) _{t-1}		2.224** (0.932)	2.728*** (0.868)
GDP per capita squared (ln) _{t-1}		-0.077 (0.051)	-0.100** (0.050)
Education _{t-1}		-0.066 (0.079)	-0.056 (0.091)
Trade _{t-1}		-0.001 (0.002)	0.001 (0.003)
Life Expectancy _{t-1}		-0.025 (0.019)	-0.016 (0.023)
Regime Type _{t-1}		0.020 (0.110)	-0.044 (0.127)
Current Account Balance _{t-1}			0.003 (0.009)
Investments _{t-1}			0.013 (0.009)
GDP growth _{t-1}			-0.018** (0.008)
Banking Crisis _{t-1}			-0.235* (0.134)
UNGA Voting _{t-1}			0.231* (0.132)
Global GDP Growth × IMFprobability _{t-1}			0.122** (0.050)
Banking Crises × IMFprobability _{t-1}			-0.003 (0.012)
Observations	3766	3010	2625
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

Dep. Var.:										
Income growth rate for decile:	1	2	3	4	5	6	7	8	9	10
IMF program	-0.067** (0.030)	-0.053** (0.026)	-0.057** (0.025)	-0.048** (0.023)	-0.056** (0.023)	-0.050** (0.022)	-0.036* (0.021)	-0.035* (0.020)	-0.028 (0.020)	-0.026 (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: Full Table on Long-Term Effects

Table 8 – Long-Term Effects

Lag:	(1)	(2)	(3)
t	0.847* (0.506) [3766]	1.417*** (0.543) [3010]	1.525** (0.604) [2625]
t-1	1.130** (0.521) [3766]	1.435*** (0.550) [3010]	1.288** (0.572) [2625]
t-2	1.593*** (0.552) [3726]	1.633*** (0.561) [2977]	1.330** (0.702) [2625]
t-3	1.816*** (0.564) [3685]	1.773*** (0.554) [2943]	1.303** (0.664) [2625]
t-4	1.363*** (0.468) [3643]	1.683*** (0.543) [2909]	1.234** (0.632) [2625]
t-5	0.920** (0.450) [3598]	1.401** (0.605) [2871]	0.962** (0.675) [2625]
t-6	0.511 (0.758) [3556]	0.914 (0.911) [2834]	0.547 (0.907) [2625]

Note: The table reports β -coefficients for different lags of the variable IMFprogram in specifications (1)-(3), which are otherwise identical to the regressions in Tables 1 and 2. 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 in 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.²⁷ Second, I code binary variables indicating whether any condition addressed one of nine policy areas.²⁸ 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 (2018) 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 (2018) 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.

²⁷ This approach follows Dreher, Sturm, and Vreeland (2015).

²⁸ See Appendix A for a description of these policy areas.

Contrary to the predictions regarding the ‘liberalization’ channel, however, conditions addressing trade policy or the financial sector are not significantly associated with rising 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 9 – IMF Conditionality:

	(1)	(2)	(3)	(4)
Scope of Conditionality	0.154** (0.076)	0.163** (0.073)		
Social Sector (incl. Pensions)			1.435*** (0.493)	1.727*** (0.577)
Labor Market (private sector)			2.247*** (0.717)	2.614*** (0.761)
Trade and Financial Liberalization			0.424 (0.465)	0.133 (0.381)
Labor market (public sector)			-0.974 (0.676)	-1.032 (0.661)
SOE reform			0.095 (0.494)	0.022 (0.519)
Foreign Exchange Systems			0.586 (0.390)	0.519 (0.426)
Central Bank			0.529 (0.588)	0.622 (0.625)
Financial Sector			0.250 (0.520)	0.459 (0.460)
Government			0.069 (0.857)	0.128 (0.838)
Residual Category			-0.321 (0.453)	-0.089 (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

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.²⁹ These vary when economically large members obtain and repay loans that are large relative to total IMF resources (“purchase” and “repurchase” in IMF jargon).³⁰ In Figure 1 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.³¹ As the first three columns in Table 10 show, the results do not differ substantially. To address these concerns in the most cautious

²⁹ 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.

³⁰ 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%.

³¹ 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.

way possible, I also run regressions using only liquid resources as the time-variant factor of the IV. 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 10. 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 trends

In addition, I provide more detail on trends in 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 4 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.

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. 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 11). 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

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

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 12). 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 selection bias the OLS coefficients suffer from. 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*.

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 13, 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 reported in Table 1 (see also Appendix D, p. SI-5) provide empirical evidence for the violation of the exclusion restriction of UNGA voting: In the full baseline specification, 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.³²

Alternative treatment variable

I then modify the treatment variable. The paper so far followed the conventional practice of the literature on IMF program effects by jointly considering Stand-By Arrangements (SBA), the Extended Fund Facility (EFF), the Structural Adjustment Facility (SAF) and the Poverty Reduction and Growth Facility (PRGF) (e.g., Oberdabernig 2013). Barro and Lee (2005, 1248), however, argue that only SBA and EFF programs should be considered while the others “should be viewed more as foreign aid, rather than lending or adjustment programs”. In Table 13 (columns 4-6) I follow their approach and find that the results hold when only SBA and EFF programs are considered.

Alternative dependent variables

Regarding the dependent variable, I first substitute the Gini coefficient of net income by that of gross income (*Gross Gini*) (Table 14, 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 Income Inequality Database (WIID), on which the SWIID builds, over the SWIID.³³ The WIID, however, offers

³² 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.

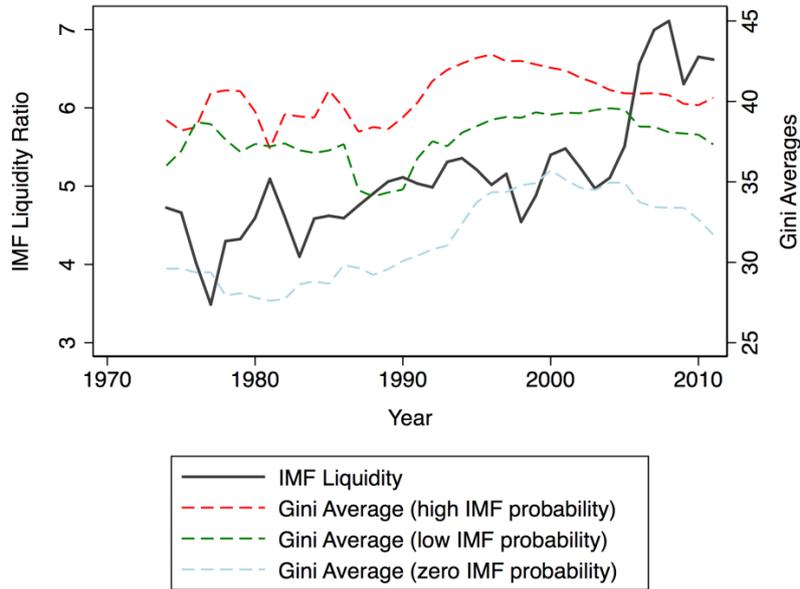
³³ Jenkins (2015) concerns, however, relate to an older version of the SWIID and Solt (2015) is able to overcome many of these concerns. The reader is referred to the entire special issue of the *Journal of Economic Inequality* (December 2015, Volume 13, Issue 4) for details on this debate.

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 *Gini_{all}* 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 14 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.

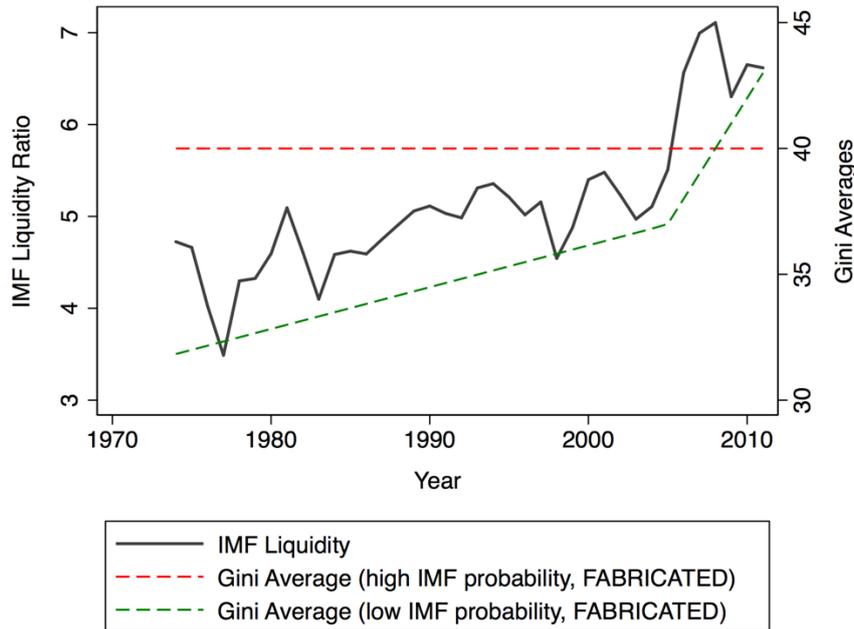
I conclude that the results are robust to these modifications.

Figure 4 – Spurious Correlations Between Inequality and IMF Liquidity?

Panel A: Actual Trends

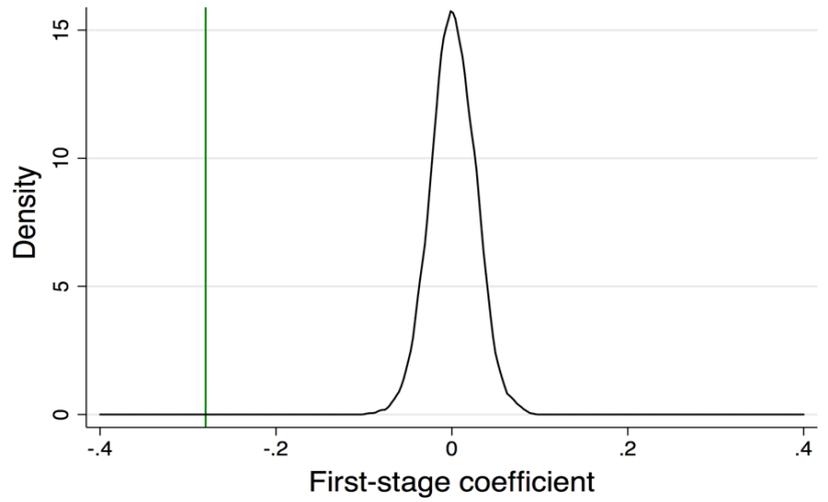


Panel B: Problematic Trends



Note: The figure plots the variation of *IMFlquidity* 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 *IMFlquidity*. Panel B illustrates with fabricated data how potentially problematic trends would look like (see p. SI-12). In this example, *IMFlquidity* is correlated with the long-term trend of *Gini* in low-probability countries, but not with the trend in high-probability countries.

Figure 5 – 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 *IMFlquidity*. The horizontal line shows the first-stage coefficient based on the actual order of the values.

Figure 6 – IMF Liquidity and Global GDP Growth

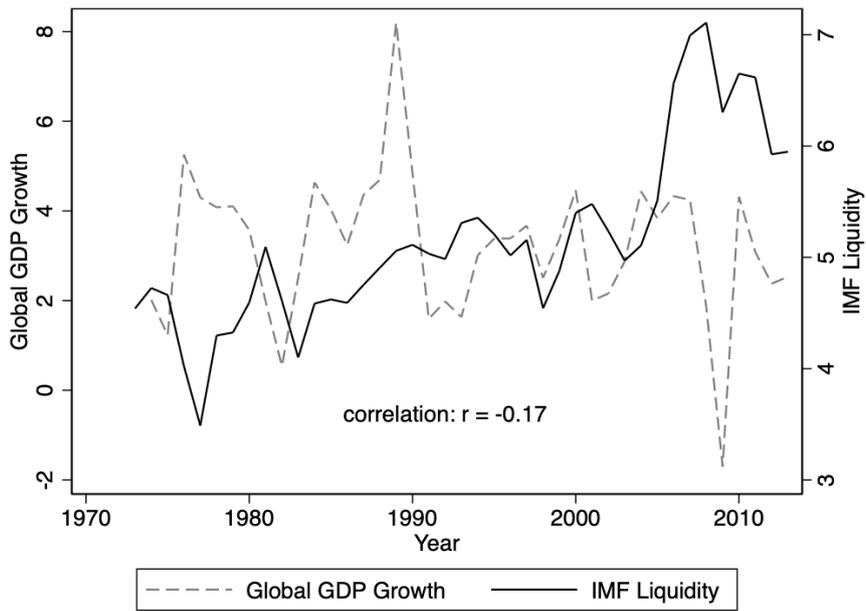


Figure 7 – IMF Liquidity and Global Crises

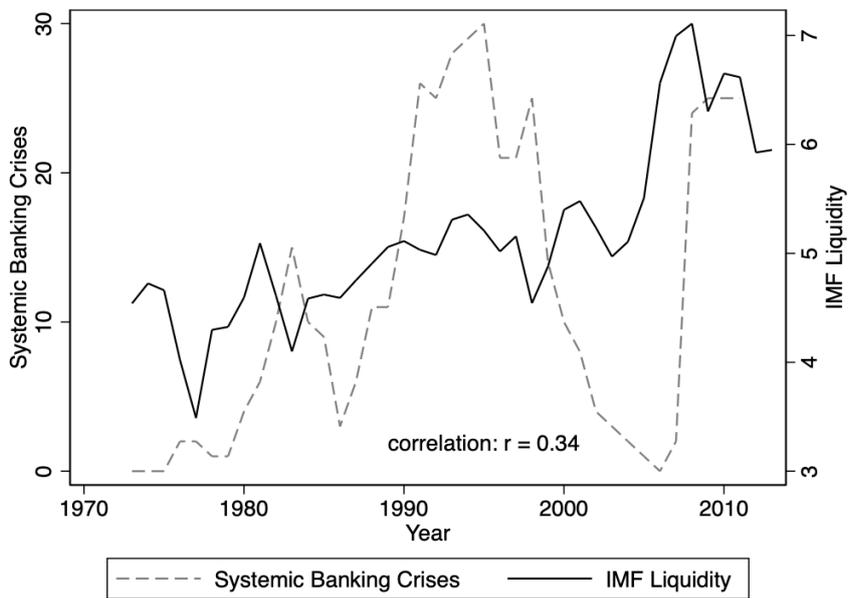


Table 10 – Robustness: Challenging the Liquidity Variable

	Excluding large IMF transactions			IV with liquid resources		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second Stage						
IMF Program _{t-1}	1.244**	1.727***	1.403**	1.172*	2.037**	1.351**
	(0.546)	(0.610)	(0.568)	(0.668)	(0.970)	(0.615)
Panel B: First Stage						
IV _{t-1}	-0.277***	-0.299***	-0.357***	-0.168***	-0.177***	-0.387***
	(0.053)	(0.064)	(0.069)	(0.043)	(0.056)	(0.087)
K.-P. underid. LM	17.480	14.320	17.423	12.652	9.325	18.762
K.-P. underid. p	0.000	0.000	0.000	0.000	0.002	0.000
K.-P. weak id. F	27.240	21.671	26.727	15.599	10.058	19.818
Inequality Controls (t-1)	No	Yes	Yes	No	Yes	Yes
IMF Controls (t-1)	No	No	Yes	No	No	Yes
N	3654	2901	2536	3766	3010	2625
Adjusted R ²	0.878	0.840	0.853	0.879	0.823	0.855

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 11 – Robustness: Challenging the Probability Variable

	(1)	(2)	(3)
Panel A: Second Stage			
IMF program _{t-1}	1.901*	1.691**	1.634**
	(1.145)	(0.746)	(0.788)
Panel B: First Stage			
IMF liquidity x	-0.173**	-0.271***	-0.300***
IMFprobability(constant) _{t-1}	(0.071)	(0.072)	(0.075)
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 12 – 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*** (0.903)	0.651** (0.270)	0.016 (0.071)	0.082 (0.073)	0.122 (0.076)			
IV _{t-1}						-0.312** (0.142)	-0.442*** (0.168)	-0.459** (0.201)
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	3012	2627	3768	3012	2627
Adjusted R ²	0.057	0.120	0.898	0.885	0.884	0.898	0.885	0.884

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 13 – Robustness: Alternative IV, Alternative Treatment

	UNGA voting as IV			Only SBA & EFF Programs		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second Stage						
IMF Program _{t-1}	4.644*** (1.773)	4.258** (2.002)	4.259* (2.247)			
SBA/EFF Program _{t-1}				0.762*** (0.286)	0.805*** (0.312)	0.722* (0.389)
Panel B: First Stage						
UNGA voting _{t-1}	0.061*** (0.023)	0.073** (0.033)	0.075** (0.034)			
IV _{t-1}				-0.558*** (0.059)	-0.576*** (0.068)	-0.531*** (0.071)
K.-P. underid. LM	6.084	2.546	3.005	18.574	19.987	19.963
K.-P. underid. p	0.014	0.111	0.083	0.000	0.000	0.000
K.-P. weak id. F	7.139	4.999	4.961	89.310	72.677	56.272
Inequality Controls	No	Yes	Yes	No	Yes	Yes
IMF Controls	No	No	Yes	No	No	Yes
N	3520	3001	2682	3766	3010	2625
Adjusted R ²	0.626	0.635	0.620	0.890	0.875	0.874

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 14 – 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*** (0.561)	1.544*** (0.579)	1.222** (0.575)	1.220 (1.155)	1.974** (0.927)	2.043** (1.014)
Panel B: First Stage						
IV _{t-1}	-0.276*** (0.053)	-0.314*** (0.063)	-0.363*** (0.067)	-0.557*** (0.113)	-0.699*** (0.130)	-0.624*** (0.135)
K.-P. underid. LM	18.804	16.732	19.608	12.093	12.857	10.483
K.-P. underid. p	0.000	0.000	0.000	0.001	0.000	0.001
K.-P. weak id. F	27.637	24.864	29.173	24.112	28.779	21.447
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	3009	2624	928	814	758
Adjusted R ²	0.870	0.862	0.859	0.493	0.512	0.491

Note: Dependent variables *Gross Gini* (columns 1-3) and *Gini_{all}* (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

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