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

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Abstract: This study investigates the distributional effects of international organizations within their member countries. It addresses the issue empirically by examining the causal effect of International Monetary Fund (IMF) programs on income inequality. Introducing a new instrumental variable for IMF programs, I exploit time variation in the IMF's liquidity and cross-sectional variation in a country's probability of having a lending arrangement with the IMF. Using panel data for 155 countries over the 1973–2013 period, the results show that IMF programs substantially increase income inequality in democracies, while having no such effect in non-democracies. The size of this effect on democracies is smaller the more democratized the IMF's decision-making processes are. These results are consistent with the theory that powerful, 'democratically deficient' international organizations that interfere in domestic politics are capable of restricting the responsiveness of democratic governments to the preferences of their citizens.

Keywords: International Organizations, International Monetary Fund (IMF), Income Inequality, Democracy

JEL codes: F33, F53, O15, O19

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1. Introduction

In recent decades, international organizations (IOs) have become powerful political actors. In the increasingly globalized world, these specialized multilateral institutions often can address the growing amount of cross-border interdependencies more effectively than states individually (Keohane 1984). To fulfill their tasks, IOs have over time extended their authority and their intrusiveness into the national societies of their member states (Barnett and Finnemore 2004).

This trend has engendered criticism. A major concern is the lack of democratic control over the activities of international organizations. Many scholars argue that IOs suffer from a 'democratic deficit' as their accountability to the citizens they affect is weak (e.g., Nye 2001). As a consequence, "[i]nternational organizations are widely believed to undermine domestic democracy" (Keohane, Macedo, and Moravcsik 2009, 1). When they exert political influence on their member states IOs limit the role played by domestic politics and in democracies thus reduce the control citizens can exercise on the institutions governing them.

Arguably, this deficit is particularly problematic if IOs have distributional effects. As their influence can then create winners and losers, the absence of democratic control means that citizens lack a mechanism to guide and constrain (re)distributional policies according to their preferences (Gartzke and Naoi 2011). Since empirical research on the distributional effects of IOs is scarce[1], this paper, first, aims to augment the literature with causal evidence on their impact on income inequality. Second, I investigate whether the effect I find can indeed be explained by the idea that IOs 'undermine democracy'.

The theoretical argument I develop is based on the widely-held view that democracies tend to exhibit lower levels of income inequality than non-democracies, because their governments are more responsive to the interests of poorer segments of society, who benefit from a relatively egalitarian income distribution (Meltzer and Richard 1981). I derive the hypothesis that democratically deficient IOs that are powerful enough to affect economic outcomes in member states make inequality rise due to their relative lack of such responsiveness and accountability. If the argument holds, the effect, I argue, should be observable only in democracies: only there can the interference of democratically deficient IOs in national policy-making limit the functioning of domestic accountability mechanisms. Further examining this channel, I expect the efforts to 'democratize' IO decision-making processes, which scholars have noted in recent years, to mitigate this effect (Grigorescu 2015).

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[1] In a review of the literature, Armingeon (2010, 314) concludes that "there is surprisingly little systematic research on the interaction between IOs and national welfare policies." Exceptions include Vreeland (2002) and Oberdabernig (2013).

In the empirical part of the paper I focus on the loan programs administered by the IMF to test these hypotheses. The IMF is often considered "the most powerful international institution in history" (Stone 2002, 1). It has vast financial resources at its disposal and its loan arrangements can make use of conditionality as a potent instrument to directly affect economic policies in program countries. In addition to these theoretical considerations, the fact that almost all developing countries have received IMF loans since the early 1970s underlines the empirical importance of investigating the effects of IMF programs. Furthermore, as IMF programs indicate the direct influence of an IO on a given country for a well-defined period of time, from a methodological perspective they provide an ideal setting for analyzing the hypotheses with international panel data.

The methodological challenge, however, is to establish causality since IMF programs are clearly not randomly assigned. Extant approaches addressing this problem rely on instrumental variables (IVs) that are likely to be related to the outcome through channels other than IMF programs and thus violate the exclusion restriction. To fill this gap, I employ a novel identification strategy for IMF programs inspired by recent methodological innovations (Nunn and Qian 2014; Werker, Ahmed, and Cohen 2009). I exploit exogenous variation over time in the IMF's liquidity and interact this variable with a country's probability of participating in IMF programs, thereby introducing variation across countries. When controlling for the levels of the interacted variables, this interaction term is excludable to country-specific variables such as income inequality and thus allows me to determine the causal effect of IMF programs. As the exclusion restriction of this new IV holds not only for inequality but also for other economic and political outcomes on the country-level, the methodological section of this paper can be considered as an attempt to provide the literature with a tool to investigate the causal effects of IMF programs at large.

Foreshadowing the main results, I find strong statistical evidence that supports the hypotheses. IMF programs are shown to increase inequality in democracies but have no such effect in non-democracies. In democracies, the effect is statistically significant, robust to a battery of additional tests and substantial in magnitude. On average, the rise in inequality induced by IMF programs is equivalent to a lump-sum transfer of four to eight percent of the poorer half's mean income by each person in the poorer half to each person in the richer half. In light of this evidence, the study also adds to the literature on IMF effects that stresses the importance of interaction effects with program countries' political systems (Caraway, Rickard, and Anner 2012). More generally, it is a contribution to the growing literature on the causes behind the continuing trend of rising inequality within many countries.[2]

[2] For an overview on the causes and consequences of inequality see Dabla-Norris et al (2015). See Piketty (2014) for the contribution that recently sparked a surge of interest in this topic.

The remainder of this paper is structured as follows: The ensuing section presents the theory and derives empirically testable hypotheses. A method to systematically examine these is developed in section 3. Subsequently, the results of this approach are presented in section 4 before their scholarly and political implications are discussed in a concluding section.

2. Theory and Hypotheses

The 'Democratic Deficit' and Inequality

According to a prominent argument, international organizations suffer from a 'democratic deficit' as the decision-making processes in IOs do not satisfy basic democratic standards.[3] In the literature "[t]he main problem […] of the democratic deficit is generally understood to be the relative lack of accountability of IOs to the individuals whose lives they directly affect" (Grigorescu 2013, 177). While scholars have discussed many aspects of democratically deficient decision-making in IOs, two dimensions of this 'relative lack of accountability' receive particular attention.

Some scholars emphasize the relative autonomy of IOs and their bureaucracies. In their seminal work, Barnett and Finnemore (2004) argue that IOs require a certain degree of independence from national public interests in order to effectively carry out their assigned tasks. Their research as well as many subsequent studies have demonstrated that the bureaucracies of IOs indeed enjoy a significant amount of discretion (Copelovitch 2010; Hawkins et al. 2006; Stone 2011). Such autonomy, however, naturally limits the citizens' opportunities to influence decision-making in IOs. Global governance, according to Dahl (1994), thus faces a "democratic dilemma" – a trade-off between "system effectiveness" and "citizen participation."

Other scholars consider the control powerful governments exert over IOs as more problematic: "The problem is not a lack of accountability as much as the fact that the principal lines of accountability run to powerful states" (Grant and Keohane 2005, 37). Indeed, empirical evidence for the disproportional influence of economically large countries on IOs abounds; the United States and other G7 governments, for instance, have repeatedly been shown to influence decision-making in international financial institutions (e.g., Kilby 2009; Thacker 1999; Vreeland and Dreher 2014). Governments of countries that usually receive financial assistance from these institutions, however, often have a very limited influence on their general political orientation and the influence of these countries' citizens is even lower.

[3] The term was coined by Marquand (1979) to criticize a lack of democracy in the EU but is now widely used and applied to various IOs (e.g., Nye 2001).

Whether IOs are relatively autonomous or largely controlled by the executives of their most powerful members – to the extent that IOs are indeed 'democratically deficient' and "less responsive to the wishes of voters" (Vaubel 2004, 319) than democratic states, their interference with domestic decision-making processes presents a problem for citizens in democracies. Democracies ensure the responsiveness of their governments to the preferences of their citizens by enabling citizens to hold their governments accountable for their actions (Dahl 1971). Yet if powerful IOs temporarily override national political processes and limit the government's autonomy (Nooruddin and Simmons 2006) they weaken democratic accountability mechanisms and undermine this responsiveness. I argue that this may have distributional implications.

According to the standard arguments in the literature, democratic governments promote a more egalitarian distribution of income across society than non-democratic governments.[4] Meltzer and Richard's (1981) prominent model builds on the right-skew of the income distribution to show that the decisive voters earn less than mean income. As democratic governments respond to their interests for redistribution from the rich to the poor, inequality decreases. Broadened access to political power is thus closely linked to broadened access to economic resources (see also Acemoglu and Robinson 2005; Boix 2003). A related line of argument is based on the idea of "political survival" (Bueno de Mesquita et al. 2003): To stay in power, non-democratic leaders need to satisfy only a limited "winning coalition" of actors with the power to keep them in office and, consequently, provide them with targeted private goods to ensure their support. Democratic leaders, however, are accountable to and dependent on a larger base of supporters (i.e., voters) and, therefore, tend to provide public goods. As these are mostly funded through progressive taxation and often contribute to mitigating wage differentials, increased public goods provision tends to have distributional implications. While not all empirical studies find the effect, the vast majority of the theoretical literature suggests there is substantial empirical support for these arguments.[5]

In the terminology of the 'political survival'-framework, democratically deficient IOs may weaken the functioning of domestic democracy because the public of many countries is not part of the 'winning coalition' of the decision-makers in IOs. Neither IO bureaucrats nor the national executives of the most powerful members need their support for political survival. Ordinary citizens, thus, have fewer opportunities to enforce their demands in IOs than they do within autonomous democracies. Instead of being accountable to these citizens, decision-makers in IOs

[4] For a recent literature review see Acemoglu et al. (2015).

[5] Empirical support for the link between democracy and lower inequality is reported in, e.g., Blaydes and Kayser (2011), Reuveny and Li (2003), Rodrik (1999). Statistically insignificant or non-robust results for this relationship are, e.g., reported in Acemoglu et al. (2015), Mulligan, Gil, and Sala-i-Martin (2004), Scheve and Stasavage (2012). The empirical link between democracy and higher public goods provision is well established (e.g., Avelino, Brown, and Hunter 2005; Jensen and Skaaning 2015).

will tend to satisfy the interests of those 'winning coalitions' that can actually hold them to account. Provided that trade-offs between these interests and income equality exist (which I show is the case more often than not below), IOs will tend to increase inequality when they exert influence on democracies. In non-democracies, on the other hand, no such effect should be observable because the responsiveness of the governing institutions to citizens is weak irrespective of IO influence. If democratically deficient IOs exert influence on non-democracies the domestic decision-making processes they override are already democratically deficient. In other words, democracy – and its tendency towards income equality – cannot be undermined. If the argument holds, inequality should not be affected.

Hypotheses

To make this general conjecture empirically testable, I specify my hypotheses in the following. As argued in the introduction, among the IOs that focus on economic issues and have the necessary resources and policy instruments, the prime candidate for the analysis for theoretical, empirical and methodological reasons is the IMF.[6] What is more, the IMF has been explicitly criticized for "undermin[ing] the democratic process by imposing policies" (Stiglitz 2000) and in the literature there is ample evidence indicating that the two previously discussed dimensions of the democratic deficit apply directly to the IMF:

On the one hand, scholars emphasize the substantial autonomy of the IMF's bureaucracy. The IMF itself argues that its staff aims to achieve the Fund's main policy goals. Conditionality in IMF programs will therefore "establish adequate safeguards for the temporary use of the general resources of the Fund" (IMF Articles of Agreement, Article V, 3a) and thus aim to ensure loan repayment. Moreover, the Fund has often underlined that economic growth and price stability are additional primary objectives of its programs (IMF 2016a; Polak 1991). In line with public choice theory, IMF staff, however, also face bureaucratic incentives, making the maximization of power and budget key determinants of the IMF's bureaucratic decision-making (Vaubel 1986). Several studies have observed such behavior within the IMF and argue that its officials push for longer programs, larger loans and more far-reaching conditionality than what is economically optimal (Barnett and Finnemore 2004; Copelovitch 2010; Vaubel 1996). Additionally, staff preferences are also determined by the IMF's organizational culture (Chwieroth 2013). Studies have tracked IMF decision-making to a "neoliberal" ideational culture prevalent among IMF officials resulting from their educational background (Chwieroth 2007). The finding that program countries with policy-makers whose beliefs are closer to this ideational culture receive favorable

[6] The argument, however, could also be applied to other IOs in future empirical research (World Bank, EU, WTO, Regional Development Banks etc.).

treatment from the Fund supports this argument (Nelson 2014). According to this line of research, IMF decision-making is biased towards "neoliberal", market-based responses to economic problems while Fund officials advocating for government intervention in market processes and outcomes are underrepresented (Chorev and Babb 2009; Chwieroth 2007, 2010; Stiglitz 2002).

On the other hand, research demonstrates that the IMF's most powerful member states exert considerable influence on its decision-making. This is evidenced by countries receiving favorable treatment from the Fund if they are politically close to the United States, geopolitically important, members of the United Nations Security Council (UNSC), or cast votes in line with the Fund's major shareholders in the United Nations General Assembly (UNGA) and in the UNSC (Dreher and Jensen 2007; Reynaud and Vauday 2009; Stone 2008; Thacker 1999; Vreeland and Dreher 2014). Countries that are indebted to U.S. commercial banks also receive benefits (Broz and Hawes 2006; Copelovitch 2010; Gould 2003). The IMF's decision-making, thus, at least partly reflects the financial and (geo-)political interests of its most powerful member governments.

Both Copelovitch (2010) and Stone (2008) provide syntheses of these two arguments. They argue that the staff's influence is conditional on the major shareholders' interest in intervening in IMF decision-making. They empirically show that the staff's impact is most significant when there is "agency slack" because of heterogeneity in the major shareholders' interests (Copelovitch 2010) and when countries are of no particular political importance to the United States (Stone 2008, 2011).[7]

In sum, for the two major actors within the IMF's decision-making structure, limiting or reducing inequality is not a high-ranking policy goal. Instead, it is obvious that the policy preferences resulting from the staff's main policy goals, bureaucratic incentives and ideational culture, may stand in contrast to the public's distributional preferences: Far-reaching macroeconomic and structural policy conditions with a focus on debt repayment, growth and price stability combined with an inclination toward free-market liberal policies may very well come at the cost of increasing income inequality. And to the extent that geopolitical and financial interests of the major shareholders are also reflected in the design of conditionality in IMF programs, the aim of avoiding adverse distributional effects is further deprioritized. In fact, foreign aid and World Bank projects have been found to be less effective for developmental goals when they are politically motivated (Dreher, Eichenauer, and Gehring 2016; Kilby 2013, 2015).

[7] While the program country certainly also has an impact on the policies implemented under IMF programs (e.g., Caraway, Rickard, and Anner 2012), for the hypothesis only the deviation from the country's decision-making in the absence of IMF intervention, the counterfactual, is relevant.

Conditionality in IMF programs with potentially adverse distributional consequences includes, for instance, conditions that aim to reduce financial losses for commercial banks from G7 countries. Gould (2003) has found that their preferences are reflected in "bank-friendly conditions" that give priority to debt repayment, requiring resources that might otherwise be allocated to areas such as social spending. More direct distributional consequences can result from the conditions found in many IMF arrangements that demand cuts in public sector employment and wages (Nooruddin and Vreeland 2010; Rickard and Caraway 2014). Furthermore, most IMF programs include conditionality calling for the privatization or restructuring of state-owned enterprises, which in many cases has led to mass layoffs. Conditions on trade and financial liberalization are also common and may increase inequality, e.g., through adverse employment effects on previously protected sectors.[8] Furthermore, the fact that many IMF arrangements request reductions in pensions, employment protection, government expenditure and, minimum wages also can have adverse distributional effects (Kentikelenis, Stubbs, and King 2016).[9]

All in all, these considerations suggest that, due to the policy preferences that shape them, IMF programs can come at the cost of increasing income inequality. Hence, for the empirical analysis the following hypotheses are proposed:

H_1: IMF programs cause higher income inequality within countries.

H_2: IMF programs cause higher income inequality in democracies.

H_3: IMF programs do not cause higher income inequality in non-democracies.

While H_1 is the general hypothesis relevant primarily from an empirical perspective, H_2 and H_3 aim to shed light on the proposed theoretical mechanism: In democracies, IMF programs can weaken existing accountability mechanisms; in non-democracies such mechanisms are weak irrespective of IMF programs being in place.

To further investigate the 'democratic deficit' as a mechanism, I test two additional hypotheses. So far I have ignored the possibility that the decision-making processes of IOs in general, and of the IMF in particular, could have changed over time. Recent research calls the assumption of stable decision-making processes into question. Confronted with pressures for legitimization, international organizations have reacted by engaging in efforts to enhance accountability mechanisms with citizens in order to move closer to the democratic ideal (Grigorescu 2015).

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8 In the literature, there is no consensus on the direction of the distributional effects of trade liberalization. For a review see Goldberg and Pavcnik (2007).

9 See Dreher (2009) for a review of the literature on IMF conditionality.

One of these efforts is to increasingly grant non-governmental transnational actors (TNAs) access to IOs' decision-making processes (Steffek, Kissling, and Nanz 2008). An index developed by Tallberg et al. (2014) measures the degree of such access and provides systematic evidence for an "opening up" of most IOs towards TNAs largely since the 1990s. For the IMF the index shows an increase in TNA access beginning in 1998. To the extent that "[n]on-governmental organizations can democratize IGOs by expanding participation and increasing accountability" (Vabulas 2013, p.194), I expect this 'democratization' of IOs to mitigate their hypothesized adverse distributional effects. To be sure it is disputed to what extent TNAs actually make IOs more democratic (Agné, Dellmuth, and Tallberg 2015). Also, more inclusive decision-making processes at the IO level cannot fully remove the concern that intrusive IOs can weaken the functioning of domestic democracy. However, non-governmental organizations often play important roles in lobbying for policies addressing the interests of the poor by giving a political voice to groups that are easy to neglect (Gerring, Thacker, and Alfaro 2012). If they thereby strengthen accountability mechanisms between poorer segments of society and IOs, the latter should become more responsive to these demands and more sensitive to adverse distributional effects:

> H_4: The effect of IMF programs on income inequality in democracies decreases with increasing TNA access to IMF decision-making.

Another source of variation that can be exploited to make the theoretical explanation more plausible is the difference in IMF lending facilities under which programs are designed. In 1999, the Fund established the Poverty Reduction and Growth Facility (PRGF).[10] According to the IMF (2001), "foremost among them [the distinctive features of the new facility] is broad public participation and increased national ownership." In combination with the explicit focus on poverty reduction, the aims to let country authorities lead the process and involve civil society in the program design are an attempt to avoid extensive interference in domestic political systems and strengthen accountability. To the extent that PRGF programs, as a consequence, override the domestic democratic system to a lesser extent, the theoretical considerations suggest the following hypothesis:

> H_5: The effect of IMF programs on income inequality in democracies is lower in PRGF programs.

The subsequent chapter presents the empirical strategy to test these hypotheses.

[10] In 2010, the "Extended Credit Facility" replaced the PRGF.

3. Method and Data

Endogeneity

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 income levels while IMF programs where in place (Klein 2008; Peet 2009; Stiglitz 2002). An illustrative example is the case of Argentina, which was under one of the economically largest and longest IMF programs of all times. Democratic since 1983, Argentina received financial assistance from the Fund for almost the entire 1983–2004 period. From the beginning to the end of these two decades the country's Gini coefficient rose from 38 to 45. Especially during the mass protests at the turn of the millennium many blamed reforms with origins in IMF conditions implemented by Carlos Menem's government for this trend, as well as widespread poverty and unemployment. The IMF had demanded and supported policies such as the privatization of state-owned enterprises leading to mass layoffs, fiscal austerity that resulted in cuts in public wages and pensions, and during the 1998-2002 recession opposed social programs for the poor and government plans such as increasing teachers' salaries (Klein 2008; Paddock 2002; Rodrik 2003). When the program ended after Argentina's last purchase of IMF resources in 2004, inequality started to decline and in 2013 the Gini coefficient reached 38 again.

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

The case of Argentina illustrates that the central challenge for any study investigating the causal effects of IMF programs on economic outcomes is nonrandom selection (Przeworkski and Vreeland 2000). The national economic and political conditions that drive selection into IMF programs are likely related to the determinants of inequality levels and other economic and political outcomes. As IMF programs and inequality could thus be correlated in the absence of a causal effect, regression coefficients could be severely biased without a valid identification strategy. Problematically, not all of the potentially confounding variables are observable. While many key variables that explain IMF programs[11] suffer from missing data, the more limiting

[11] For an overview see Steinwand and Stone (2008).

problem is that many relevant conditions are intrinsically difficult, if not impossible, to measure. Vreeland (2002) lists "political will" as an example: Governments that favor IMF programs, e.g., due to a political preference for austerity policies, might also be more likely to implement policies leading to more inequality, irrespective of the presence of an IMF program. The lack of measurement of such variables as "political will" would thus bias the coefficient.[12]

In theory, 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 2007, 82). So far one strand of this research has, for this reason, either limited itself to correct for selection on observables (e.g., Doyle 2012; Hartzell, Hoddie, and Bauer 2010), or additionally controlled for selection on unobservables by means of Heckman-style selection models without exclusion restrictions (e.g., Mukherjee and Singer 2010; Nooruddin and Simmons 2006; Przeworkski and Vreeland 2000). The former 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.[13]

The other strand of research has incorporated exclusion restrictions in Heckman-, bivariate probit-, or two-stage least squares (2SLS) models (e.g., Atoyan and Conway 2006; Barro and Lee 2005; Dreher and Gassebner 2012). In these studies, one variable, first proposed as an IV by Barro and Lee (2005), has become the 'standard instrument' for IMF programs: the share of votes cast in line with the US or G7 countries in the UNGA. However, as the other IVs used in this literature,[14] this 'standard instrument' is not clearly excludable to macroeconomic outcomes. It rests on the assumption that the only channel through which UNGA voting behavior affects macroeconomic outcomes in a country is the presence of an IMF program. But it is likely that a government's preferences in foreign policy articulated in UNGA roll-call votes are related to a

[12] Another example is political favoritism. As discussed above, there is increasing evidence that many countries receive IMF programs when this is in the interest of the IMF's most powerful shareholders (Dreher, Sturm, and Vreeland 2009). Many of the various political, economic, geostrategic, and ideological factors that determine these members' preferences are hardly measurable but might be correlated with inequality.

[13] For further details regarding problems related to Heckman-models without exclusion restrictions see Puhani (2000).

[14] Alternatively, 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) and US aid (Eichengreen, Gupta, and Mody 2008) have been used. But the assumption that these country-specific macroeconomic variables do not affect the respective dependent country-specific macroeconomic variable 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. The country's share of IMF staff (Barro and Lee 2005) is not excludable to the country's economic and political power. A proposed alternative is to use the number of countries under an IMF program or the number of past IMF program years (Atoyan and Conway 2006; Oberdabernig 2013). However the former may be correlated with global economic crises, while the latter may capture country-specific characteristics such as weak economic governance.

government's preferences in domestic policy, which in turn are clearly linked to macroeconomic outcomes (for a theory along these lines see Moravcsik 1997; for a recent empirical confirmation see, e.g., Mattes et al. 2015). To paraphrase Moravcsik, I argue that identification strategies should 'take preferences seriously' – especially since the authors of the most widely used UNGA voting data suggest that their data "can be interpreted as states' positions towards the U.S.-led liberal order" (Bailey, Strezhnev, and Voeten 2015). The assumption that this political position is unrelated to domestic policies and the state of the domestic economy is not plausible. Hence, a new instrument is needed.[15]

Identification Strategy

My identification strategy exploits exogenous variation in the IMF's liquidity of financial resources. I apply a recent methodological innovation (Nunn and Qian 2014; Werker, Ahmed, and Cohen 2009)[16] and interact this time-variant variable with a country-variant variable indicating the country's probability of receiving an IMF program. The resulting interaction term varies over time and across countries and, after controlling for the levels of the two variables, introduces exogenous variation to the extent that the isolated interaction effect is excludable from alternative channels. Thus, even if there was endogeneity between the time-variant level variable and the outcome, the exclusion restriction would only be violated if the unobserved variables driving this endogeneity were correlated with the country-specific probability (for econometric details see Bun and Harrison 2014; Esarey 2015; Nizalova and Murtazashvili 2016).

Specifically, I use the natural logarithm of the IMF's liquidity ratio (LQR_t) – defined as the amount of liquid IMF resources divided by its liquid liabilities[17] – and interact it with the fraction of years country i has been under an IMF program between 1973 and year t ($IMFprob_{it}$).

$$IV_{it} = \ln(LQR_t) \times IMFprob_{it} = \ln(LQR_t) \times \frac{\sum_{T=1973}^{t} \mathrm{I}(IMFprogram_{iT} = 1)}{t - 1973}$$

In the first-stage equation $IMFprogram_{it}$ is regressed on this interaction term and on all second-stage variables. While year fixed effects control for the level effect of the liquidity ratio, I also control for $IMFprob_{it}$ in both stages. The identification can therefore be interpreted as a difference-in-difference approach: After controlling for the levels, the IV's coefficient indicates how the IMF's liquidity affects the likelihood of receiving an IMF program in year t differently in countries with different participation probabilities.

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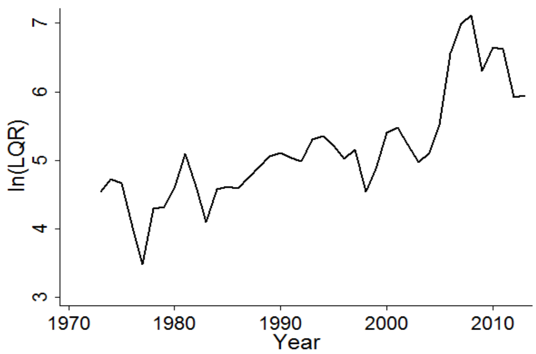
[15] Of the four existing studies on the IMF's distributional effects Pastor (1987) conducts before-and-after comparisons, Garuda (2000) controls only for selection on observables, Vreeland (2002) addresses selection on unobservables without an exclusion restriction and Oberdabernig (2013) relies on the excludability of UNGA voting.
[16] Dreher and Langlotz (2015) present a modified application.
[17] For further details on this variable and on all others see Appendix A1.

I expect this coefficient to be negative for the following reason: Multiple studies show that "recidivism" is a prime determinant of IMF programs. Countries that have a history of frequently participating in IMF programs are much more likely to do so again. The IMF, thus, has a regular clientele that is routinely supplied (e.g., Bird, Hussain, and Joyce 2004). In years, however, in which the Fund has abundant liquid resources, it has the means to extend its clientele. As Barnett and Finnemore (2004) demonstrate, the organizational incentive to do so is an important explanation of the IMF's expansion since the 1970s. Arguably, the higher its liquidity, the more generous the Fund can be. In times of high liquidity ratios, the Fund can thus grant loans to countries that would otherwise be less likely to receive IMF programs. This would be captured by a negative coefficient.

Figure 1



Whereas this 'relevance' of the instrument can and will be tested empirically in section 4, its 'excludability' is untestable and must be theoretically defended. Figure 1 shows the variation of ln(LQR_t) over time. The main sources of this variation are the IMF's Quota Reviews.[18] The Articles of Agreement (Article III, Section 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 quota increases in all but three cases (IMF 2016b). In Figure 1 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 timing of the quota reviews follows the mentioned institutional rule and is thus exogenously given, it is very unlikely that they are linked to intra-state income inequality through unobserved channels. Even if this was the case, the correlation between such unobserved variables and the outcome would bias the result only if it was dependent on a country's probability of participating in IMF programs. In other words, a sceptic would have to find unobserved variables that affect the impact of the IMF's liquidity ratio on income inequality

[18] Liquid resources additionally vary when the IMF adjusts the basket of currencies it considers "usable." The usability status, however, is very stable over time, changes mostly for small economies and therefore has a minor effect on the amount of liquid resources.

conditional on how regularly the country has received IMF programs in the past – after controlling for country and year fixed effects and a large vector of control variables. It is unlikely that such variables exist.

Some readers, however, 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.[19] These vary when economically large members obtain and repay loans that are large relative to total IMF resources ("purchase" and "repurchase" in IMF jargon).[20] In the figure 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% of total IMF quotas, any concern regarding excludability would relate to very few observations. Second, the timing of such transactions is 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 *IMFprob* because of the difference-in-difference 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.[21] To address these concerns in the most cautious way possible, I also run regressions using only liquid resources as the time-variant factor of the IV. This variable is, by construction, not determined by the Fund's liquid liabilities.

Econometric Model and Data

Armed with this excludable instrument, I estimate 2SLS panel regressions to identify the causal effect of IMF programs on income inequality:

1st stage:

$$IMFprogram_{it-1} = \alpha \ln(LQR_{t-1}) \times IMFprob_{it-1} + \delta_1 \, IMFprob_{it-1} + \eta_1 \, Gini_{it-1} + \pi_1 X'_{it-1} + \xi_i + \varrho_t + u_{it}$$

2nd stage:

$$Gini_{it} = \beta \, \widehat{IMFprogram}_{it-1} + \delta_2 \, IMFprob_{it-1} + \eta_2 \, Gini_{it-1} + \pi_2 X'_{it-1} + \xi_i + \varrho_t + \varepsilon_{it}$$

---

19 The liquidity ratio's (ln) correlation with liquid resources (ln) is $r = .83$, while with liquid liabilities (ln) it is $r = .23$.

20 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%.

21 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. See Appendix A5 for all robustness tests.

The annual time-series cross-sectional data cover the 1973-2013 period and a maximum of 155 countries. As not all data are available for all countries and years, the panel is unbalanced and the number of observations depends on the explanatory variables used.

The dependent variable *Gini* is the Gini-coefficient of net income taken from the Standardized World Income Inequality Database (SWIID) (Solt 2014). The SWIID combines source data from multiple inequality databases and, in contrast to other panel datasets like All The Ginis (ATG) (Milanovic 2014) or the World Income Inequality Database, standardizes them – using the Luxemburg Income Study as the baseline – to make the data comparable across countries and over time. Because of this standardization and its comprehensive coverage (n = 4,631) the SWIID is widely used in related research based on panel data (Acemoglu et al. 2015; Oberdabernig 2013; Ostry, Berg, and Tsangarides 2014). I follow this literature in the choice of this database, but also run robustness tests with ATG data.

The explanatory variable of interest, *IMFprogram*, is a dummy that equals 1 if country *i* was under an IMF program for at least five months in year *t* (definition based on Dreher 2006). In the baseline I follow the related literature on the effects of IMF programs and lag the variable by one year (e.g., Nooruddin and Simmons 2006). To look at longer-term effects I introduce different lags in additional regressions. It may take time for the effect to operate because of lagged consequences of economic reforms and it is relevant to see whether and when potential changes in inequality are undone after IMF programs.

Furthermore, to account for unobserved country-specific characteristics and time-specific trends, I include country and year fixed effects (ξ and ρ). As current levels of inequality are heavily dependent on previous levels it is standard to also include the lagged dependent variable (LDV) (Acemoglu et al. 2015).[22] In addition, I include a lagged vector of covariates consisting of two variable sets.[23] The first comprises the standard covariates of inequality: *GDP/Capita* and its square to control for the country's level of economic development including a potential non-linear relationship à la Kuznets (1955), *Education* measured as 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* measured as state ideal points, and an indicator variable for the presence of a systemic *Banking Crisis*. I additionally interact the global total of both *Banking Crises* and *Global GDP Growth* with *IMFprob* to enhance the exclusion restriction's plausibility by demonstrating that global economic developments do not influence both the

[22] As in all regressions T > 20, a potential Nickell bias (Nickell 1981) is negligible (Beck and Katz 2009). A Fisher-type augmented Dickey-Fuller unit-root test rejects the hypothesis that *Gini* has a unit root.

[23] For descriptive statistics, definitions and sources see Appendix A1.

IMF's liquidity ratio and inequality differently in countries with different levels of IMF participation probabilities.

4. Results

First-Stage Estimates and Relevance of the Instrument

I begin by testing the relevance of the instrument. Table 1 shows the first-stage estimates of the 2SLS regressions with different sets of control variables, whose coefficients are omitted to reduce clutter (see Appendix A3 for the full table). The results demonstrate that the instrument is relevant. They show a highly significant, negative correlation between the IV and the presence of an IMF program. The Kleibergen-Paap LM statistic rejects the null hypothesis that the equation is underidentified at the 0.1% level. The cluster-robust Kleibergen-Paap F-statistics easily surpass conventional levels of weak identification tests, such as Staiger and Stock's (1997) threshold of ten as well as Stock and Yogo's (2005) most conservative critical value of 16.38 (tolerating a maximum 2SLS size distortion of 10%).

These results are robust across different specifications. In column 1, only the levels of the interaction term, the LDV ($Gini_{t-1}$), as well as country and year fixed effects are controlled for. Under the assumption that the IV is excludable conditional on these variables, this specification without additional control variables already yields an unbiased coefficient of interest in the second stage. Nevertheless, in columns 2 and 3 I successively add the two sets of covariates described above. None of them significantly alters the relevant coefficient, its significance, the F-statistic or the underidentification test statistics.

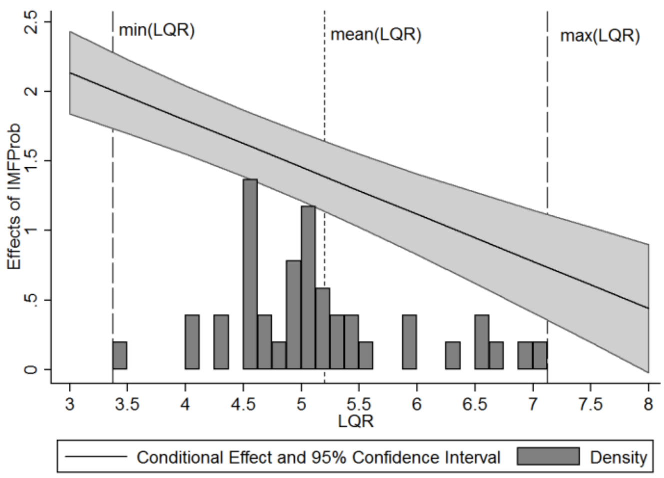
Table 1 – First Stage Regressions

	(1)	(2)	(3)
LQR × IMFprob	-0.276***	-0.308***	-0.356***
	(0.052)	(0.063)	(0.067)
IMFprob	2.760***	2.673***	3.172***
	(0.282)	(0.315)	(0.286)
LDV	0.003	-0.001	-0.003
	(0.003)	(0.004)	(0.005)
Inequality controls	No	Yes	Yes
IMF controls	No	No	Yes
Observations	3766	3010	2625
Kleibergen-Paap (K.-P.) underidentification test LM-statistic	18.452	16.045	18.973
K.-P. underidentification test p-value	0.000	0.000	0.000
K.-P. weak identification test F-statistic	27.699	24.121	28.441

Notes: Dependent variable *IMFprogram*. All regressions include country and year fixed effects, *IMFprob* and the lagged dependent variable. Standard errors, robust to clustering at the country level, in parentheses.
Significance levels: * p<.10, ** p<.05, *** p<.01

As previous studies found, countries that participated in IMF programs in the past are significantly more likely to participate in such programs in the present ("recidivism") (Bird, Hussain, and Joyce 2004). A new finding, however, is that this effect is dependent on the Fund's liquidity. As the negative sign of the interaction term's coefficient indicates and Figure 2 illustrates, in years with higher liquidity ratios the probability of past IMF participation is a weaker, but still significant, predictor of IMF programs. The Fund is, thus, not only more generous in years with higher liquidity ratios[24] but in these years it also implements more programs for countries beyond its more regular clientele. In sum, the IV is plausibly excludable to inequality levels in specific countries, proves to be highly relevant, and allows an intuitive interpretation of its linkage with the presence of IMF programs.

Figure 2 - Visualized Effect of the IV in IMF Programs



Main Results

Table 2 presents the results of the second-stage of the 2SLS regressions. Specifications 1-3 correspond to those reported in Table 1.[25] In line with H_1, the results show that IMF programs significantly increase income inequality. Across all specifications the coefficient is statistically significant at least at the 5% level (1% in specification 2) and substantial in size. Having an IMF program in year *t* on average increases the country's Gini coefficient of net income in year *t+1* by at least 1.1 points. This result is robust both to different sets of control variables and to different samples, which vary because of missing data for the added controls.

[24] The liquidity ratio – which is not included in the regressions because of perfect multicollinearity with year fixed effects – is positively correlated with the number of countries under IMF programs in a given year (r = .3).

[25] See Appendix A4 for the full table including the coefficients of the covariates.

To assess the magnitude of the effect, note that it is equivalent to an increase in the Gini coefficient by at least 34% and up to 51% of a within-country standard deviation. As inequality is slow to change, increases in inequality of this size within one year are rare events (8.6% of all observations in the sample). While this indicates a substantial effect size, differences in the Gini coefficient are difficult to interpret directly. Therefore, I expand on a method proposed by Blackburn (1989) to quantify the size of the effect in a more intuitive way. According to Blackburn's metric, an increase in the Gini by 1.1 (1.3) points is equivalent to a lump-sum transfer of 2.2 (2.6) percent of the country's mean income from the bottom half to the upper half. To view this from the perspective of an individual belonging to a country's poorer half, consider that in the sample's average country those below the median earn approximately 25% of the total national income (World Bank 2016). Hence, on average the change in inequality induced by a year under an IMF program is equivalent to a transfer of four to five percent of the poorer half's mean income by each person in the poorer half to each person in the richer half (see Appendix A2).

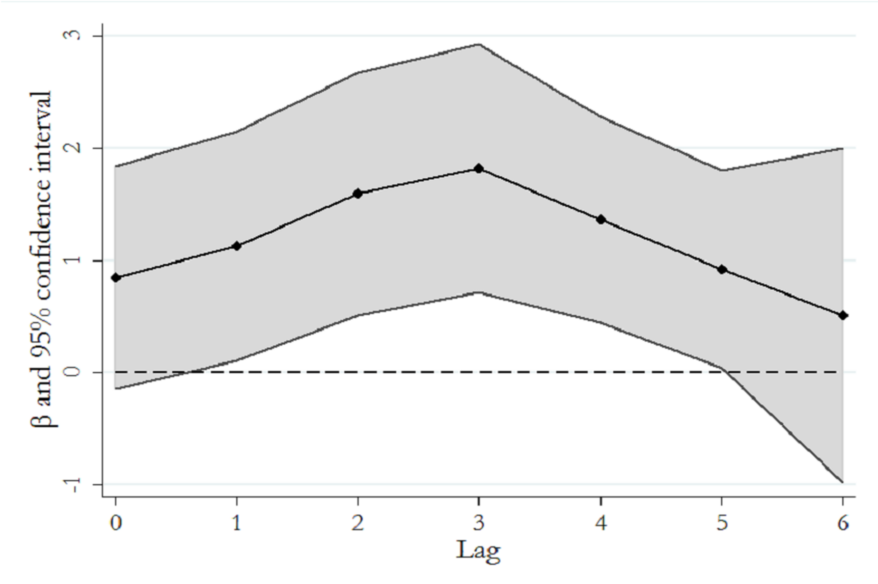
Table 2 – Main Results

	(1)	(2)	(3)
IMF Program$_{t-1}$	1.130**	1.435***	1.288**
	(0.521)	(0.550)	(0.573)
IMFprob$_{t-1}$	-1.844**	-1.901**	-2.591**
	(0.841)	(0.881)	(1.087)
Gini$_{t-1}$	0.916***	0.915***	0.911***
	(0.011)	(0.013)	(0.014)
Inequality controls (t-1)	No	Yes	Yes
IMF controls (t-1)	No	No	Yes
Observations	3766	3010	2625
Adjusted R^2	0.880	0.853	0.858

Notes: Dependent variable *Gini*. Second-stage regressions corresponding to Table 1.

Next, I test how IMF programs affect inequality in the longer term. Figure 3 illustrates and Table 3 reports the estimates of the coefficient of interest for the baseline specification (1) with different levels of lags. It indicates that the effect is statistically significant during all of the following five years and strongest and most significant after three years. After six years the effect is no longer significantly different from zero. Results are very similar when adding the control variables (see Appendix A5).

Figure 3 and Table 3 – Long Term Effects



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

Note: Reported are β-coefficients and standard errors for different lags of the variable *IMFprogram*. Specifications otherwise identical to specification 1 in Tables 1 and 2.

Heterogeneous Effects

To shed light on the underlying mechanisms I split the sample into democracies and non-democracies.[26] Note first the descriptive statistics in Table 4. They show the average of *Gini* depending on whether the observation is a democracy and on whether an IMF program was in place and reports t-tests comparing the respective means. As expected, the Gini is significantly higher in non-democracies and in countries under an IMF program. It is furthermore interesting and in line with the hypotheses that the large and highly significant difference in inequality between democracies and non-democracies entirely disappears when only countries under IMF programs are compared.

Table 3 – Conditional Means of Gini

E(Gini)	not under IMF program	under IMF program	t-tests
Democracies	34.52	41.22	t = 14.37; p < 0.001
Non-democracies	39.61	41.05	t = 3.28; p = 0.001
t-tests	t = 12.95; p < 0.001	t = -0.39; p = 0.68	

[26] The definition of democracy follows the Polity IV index and treats observations with a Polity score of 6 and higher as democracies (Marshall, Jaggers, and Gurr 2011).

As these descriptive statistics are obviously inadequate to isolate the IMF's causal effect, Table 5 presents the 2SLS regression results with the sample split into democracies and non-democracies to test H_2 and H_3. In columns 1-2 and 5-6 it is split on both stages, in columns 3-4 and 7-8 the fitted values of the variable of interest calculated by means of the entire sample are used.[27]

Table 4 – Sample Split

	Democracies				Non-Democracies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IMF Program$_{t-3}$	1.901**	2.063**	2.271***	1.829**	-0.057	-0.363	-0.031	-0.407
	(0.739)	(0.810)	(0.861)	(0.824)	(2.260)	(0.787)	(1.462)	(1.141)
IV in 1st stage	-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)
controls	No	Yes	No	Yes	No	Yes	No	Yes
sample split	1st & 2nd stage	1st & 2nd stage	2nd stage	2nd stage	1st & 2nd stage	1st & 2nd stage	2nd stage	2nd stage
N	2094	1743	3766/2094	2526/1743	1317	878	3766/1317	2526/878
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: Specifications as in Table 1 and 2; in columns 1-2 and 5-6 standard errors as before; in the remaining regressions standard errors are cluster bootstrapped.

Columns 1-4 show that IMF programs increase inequality in democracies.[28] The effect is robust to whether or not control variables are included and whether fitted values from the full or only the democratic sample are used. The Kleibergen-Paap tests show that the instrument maintains its relevance despite the smaller sample size in columns 1 and 2. The point estimates, which are statistically significant across all specifications, range from 1.8 to 2.3 and are, thus, larger compared to the full sample. In terms of within country standard deviations in democracies IMF programs increase inequality by 75 % to 95 % of a standard deviation. Again applying the metric based on Blackburn (1989), this is equivalent to a transfer of about eight percent of the average poor person's income to the average rich person. As soon as only non-democracies are

[27] 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 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.

[28] In accordance with the results for long-term effects reported in Table 3, in this and in the following table, *IMFprogram* is lagged by three years. The substance of the results, however, does not depend on this choice.

considered (columns 5-8) the effect entirely disappears. The coefficients are close to zero and not statistically significant at conventional levels.[29]

In sum, the inequality-increasing effect of IMF programs seems to be entirely driven by the democratic sample. In line with the hypotheses IMF programs appear to weaken the inclination of democratic governments to more egalitarian income distributions. The size of the effect is equivalent to cutting the average difference of approximately four Gini points between democracies and autocracies in half.

Further examining the plausibility of this channel, I test H_4 by including the interaction term *IMF×TNA* (= *IMFprogram* × *TNAaccess*) as an additional regressor (Table 6 columns 1-2). To estimate its coefficient I employ the IV estimator proposed by Bun and Harrison (2014).[30] As expected, the interaction term enters with a significantly negative coefficient. As the TNA-index is only an approximate measure of how 'democratically deficient' IMF decision-making processes are in different years, the size of the effect should be interpreted with caution. The direction of the effect, however, can be interpreted and supports the expectation that the 'opening up' of the IMF towards societal actors makes the organization more sensitive to the distributional effects of its activities.

In columns 3-6 I test H_5 by separately examining the effects of PRGF programs and other IMF programs in democracies. In line with H_5 there is a substantial difference. The coefficient for PRGF programs is small and not statistically significant at conventional levels. If all other IMF programs are considered, however, the effect on inequality is stronger than in the baseline regressions. As discussed above, the IMF's emphasis on public participation, national ownership and poverty reduction in its PRGF programs can explain why these kinds of lending arrangements have no significant adverse distributional effects. In sum, the empirical tests of hypotheses H_4 and H_5 suggest that variation in the decision-making processes that lead to the design of IMF programs explain variation in the programs' effects on inequality. The more inclusive and democratized these processes are, the smaller are the adverse distributional consequences.

[29] Note that in column 5, the specification without control variables and with the sample split on both stages, the IV in the small non-democratic sample is not strong enough to reliably rule out weak instrument bias. Columns 6-8, however, show that the coefficient of interest remains insignificant when the IV's relevance is increased by adding control variables or by using fitted values from the full sample. In column 6 the underidentification hypothesis can be rejected at the 5% level and the F-statistic surpasses Stock and Yogo's (2005) critical values of 6.66 tolerating a 2SLS size distortion of 20%.

[30] Bun and Harrison's (2014) "IV3" estimator adds the IV multiplied by *TNAaccess* as well as *IMF×TNA* to the set of instruments for *IMFprogram* while treating *IMF×TNA* as exogenous. This identification is valid under the plausible assumptions that TNA access to the IMF is exogenous to inequality levels and that the degree of endogeneity of IMF programs and inequality does not depend on TNA access: $E(IMF \times Gini \mid TNA) = E(IMF \times Gini)$.

Table 5 – TNA Access and PRGF Programs

	(1)	(2)	(3)	(4)	(5)	(6)
IMF Program$_{t-3}$	0.717**	0.985**				
	(0.285)	(0.472)				
IMF×TNA$_{t-3}$	-2.799***	-3.824**				
	(1.075)	(1.688)				
PRGF program$_{t-3}$			0.490	0.483		
			(0.961)	(1.120)		
Non-PRGF program$_{t-3}$					1.841***	2.089**
					(0.698)	(0.893)
Controls	No	Yes	No	Yes	No	Yes
N	2094	1743	2094	1743	2094	1743
Adj.R^2	0.838	0.829	0.841	0.834	0.782	0.759
K.-P. underid. LM	37.968	21.039	5.625	5.204	10.827	8.506
K.-P. underid. p	0.000	0.000	0.018	0.023	0.001	0.004
K.-P. weak id. F	77.962	32.011	14.900	14.443	19.953	12.307

Notes: Specifications as before; only democracies are considered. For columns 1-2 see text and footnote 29.

Robustness

I run a series of additional test to confirm the robustness of these results.[31] First, I address concerns regarding the exclusion restriction. Most importantly, the results are robust to excluding the country-year observations with large purchases and repurchases of IMF credit as well as to using only the IMF's liquid resources as the time-varying component of the IV (Table R1). Furthermore, substituting the time-varying probability (*IMFprob*) by a time-constant probability that is multicollinear with country fixed effects also does not affect the results (Table R2). Additional tests à la Altonji et al. (2005) 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 effect of the IV on the outcome was in fact zero (Table R3). The same table also reports OLS and reduced form estimates of the baseline specification for comparison.

Second, to make the regressions comparable to previous studies I substitute my IV with UNGA voting (Table R4, columns 1-3). The results are again similar. The latter variable, however, is less relevant, with F-statistics below critical thresholds in two out of three specifications, and the coefficients of interest are larger. As the estimated inequality-increasing effect in these regressions is equivalent to almost 140% of a within-country standard deviation within one year the plausibility of the effect size is somewhat doubtful. Under the assumption that the IV employed in this paper is excludable, this finding and the fact that UNGA voting enters with a significantly positive sign as a control in the baseline regression (see Appendix A4) suggest the following

[31] See Appendix A8 for the tables and a more detailed description of the tests.

explanation: UNGA voting is linked to inequality through more channels than just IMF programs. This violates the exclusion restriction and hence biases the coefficient upwards.

Third, I modify the main variables of interest. While my baseline definition of IMF programs follows the literature's standard, in a robustness test I use Barro and Lee's (2005) definition and only consider Stand-By Arrangements (SBA) and the Extended Fund Facility (EFF). The results are similar (Table R4, columns 4-6). When using the Gini coefficient of market income, the regressions again yield very similar results, suggesting that IMF programs redistribute net income via changes in gross income rather than by means of changes in taxes and transfers (Table R5, columns 1-3). Additionally, I employ ATG data as an alternative to the SWIID (Table R5, columns 4-6).[32] Even though the use of this dataset dramatically reduces the sample size, the results are again robust.

5. Conclusions

Do international organizations have distributional effects within their member countries? According to the evidence presented here, the loan programs of the IMF – one of the most powerful IOs – on average lead to redistribution of income from the poor to the rich in participating countries. The analysis suggests that this effect is causal and economically significant. It is observable only in democracies, where IMF programs can restrict the domestic governments' responsiveness to their citizens' preferences, and weakens when the 'democratic-deficit' in IMF decision-making is mitigated.

For the IMF – whose Managing Director, Christine Lagarde, recently claimed that "reducing excessive inequality […] is not just morally and politically correct, but it is good economics" (IMF 2015) – the main result highlights an unintended consequence of its lending arrangements. It may encourage the Fund to revise its policy advice and conditionality with regards to their distributional implications. Interestingly, the identified heterogeneous effects that shed light on the underlying channels give hope that such revisions can indeed make a difference. They indicate that the Fund's relative lack of accountability to affected societies is likely to drive the adverse distributional consequences found in the data. When the IMF, however, gives greater autonomy to the program country's national political process, as is the case in PRGF programs, and allows societal actors to have more influence over decision-making processes, these effects are mitigated. For internal Fund policies these results suggest that reforms aiming at so-called

[32] For a criticism of the SWIID's approach see Jenkins (2015). For a defense see Solt (2015).

"country ownership" and "participatory processes," are – if implemented as articulated – likely to reduce adverse distributional consequences (IMF 2014).[33]

For international organizations more generally, the main result clearly indicates the political power they can possess. Their activities not only affect public goods provision and general welfare, but can also have a distributional dimension. From a normative perspective, such distributional power already points to the need for more effective accountability mechanisms between IOs and affected societies to ensure that the allocation of gains and losses is under democratic control. The empirical analysis, by revealing that the effect of IMF programs varies depending on the regime type, supports the view that such democratic control over IOs is weak. Apparently, the interference of a powerful IO in national democratic systems can undermine extant domestic accountability mechanisms. As a consequence, policy outcomes may deviate from those produced in functioning democracies. To counteract this unintended effect, 'democratizing' IOs themselves can help. The study shows that enhanced democratic accountability mechanisms in IOs, apart from being normatively desirable, can also produce better policy results.

Future research both on IMF program effects and on the drivers of income inequality can draw lessons from this study. First, the proposed identification strategy can be useful for scholars investigating the causal effects of IMF programs more broadly. The instrumental variable plausibly fulfills the exclusion restriction with regards to a range of additional political and economic outcomes. Second, the results add to the growing literature that stresses the role of policies and institutions as determinants of inequality (Alvaredo et al. 2013; Dabla-Norris et al. 2015; OECD 2011). While their contribution to current trends of rising inequality across countries is well-established in this literature, it remains an open question as to why so many countries modified their national policies and institutions in a way that increased inequality. This study's results suggest that *inter*-national policies and institutions played a significant role.

[33] The 2014 revision of the Fund's 2002 Conditionality Guidelines – after previous revisions in 2005, 2008, 2010 – for the first time states that staff should accommodate "distributional targets" of country authorities where possible. Future research based on post-2014 data could examine whether this affected the design of IMF programs and their distributional effects. In general, further investigating the processes that help to prevent adverse effects of IMF programs seems to be a promising area for future study.

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Appendix

Appendix A1: Variables

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 2014)
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)
LQR (ln)	5.42	0.75	4.1	7.11	IMF liquidity ratio = 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
IMFprob	0.25	0.25	0	1	$\frac{\sum_{T=1973}^{t} I(IMFprogram_{it} = 1)}{t-1973}$ 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 (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 World Economic Outlook)
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 2015)
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. Own calculations based on (Laeven and Valencia 2012)
TNA index	0.14	0.14	0	0.32	Index of TNA access to the IMF (Tallberg et al. 2014)
PRGF program	0.15	0.36	0	1	Indicator 1 if IMF program under the PRGF in place for at least 5 months in year t (Dreher 2006)
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 2014)
Gini (ATG)	39.63	9.91	20	69.8	Gini coefficient (Giniall) according to the ATG Dataset (Milanovic 2014)

Note: The sample of the full specification (Table 2, column 3) was used for calculating the values in this table.

Appendix A2: 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

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

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

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

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

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

Appendix A3: Full Table 1 (Baseline - First Stage)

	(1)	(2)	(3)
LQR × IMFprob	-0.276***	-0.308***	-0.356***
	(0.052)	(0.063)	(0.067)
IMFprob	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/Capita (ln)		-0.177	-0.135
		(0.280)	(0.331)
GDP/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 × IMFprob			0.002
			(0.027)
Banking Crises × IMFprob			0.006
			(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*. All regressions include country fixed effects and year fixed effects. Standard errors, robust to clustering at the country level, in parentheses.
Significance levels: * $p<.10$, ** $p<.05$, *** $p<.01$

Appendix A4: Full Table 2 (Baseline - Second Stage)

	(1)	(2)	(3)
IMF Program$_{t-1}$	1.130**	1.435***	1.288**
	(0.521)	(0.550)	(0.573)
IMFprob$_{t-1}$	-1.844**	-1.901**	-2.591**
	(0.841)	(0.881)	(1.087)
Gini$_{t-1}$	0.916***	0.915***	0.911***
	(0.011)	(0.013)	(0.014)
GDP/Capita (ln)$_{t-1}$		2.224**	2.728***
		(0.932)	(0.868)
GDP/capita squared (ln)$_{t-1}$		-0.077	-0.100**
		(0.051)	(0.050)
Education$_{t-1}$		-0.066	-0.056
		(0.079)	(0.091)
Trade$_{t-1}$		-0.001	0.001
		(0.002)	(0.003)
Life Expectancy$_{t-1}$		-0.025	-0.016
		(0.019)	(0.023)
Regime Type$_{t-1}$		0.020	-0.044
		(0.110)	(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 × IMFprob$_{t-1}$			0.122**
			(0.050)
Banking Crises × IMFprob$_{t-1}$			-0.003
			(0.012)
Observations	3766	3010	2625
Adjusted R^2	0.880	0.853	0.858

Note: Second-stage regressions corresponding to Table 1. Dependent variable *Gini*.

Appendix A5: Full Table on 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 A6: Robustness

This section describes in more detail the robustness tests summarized in the results section. As discussed in section 3, some readers might be concerned that the large purchases and repurchases of IMF resources that affect the Fund's liquid liabilities could be correlated with inequality levels through channels other than IMF programs even after conditioning on the IMF participation probability. I therefore exclude the country-years with the 100 largest flows from and to the IMF.[34] As the first three columns in Table R1 show, the results do not differ substantially. An even more cautious approach is presented in the remaining three columns. In these regressions I substitute the ratio between liquid resources and liquid liabilities (the liquidity ratio) by liquid resources only. 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. 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.

Another modification concerns the second factor of the interacted instrument (Table R2). Like Nunn and Qian (2014) as well as Dreher and Langlotz (2015) I also report results employing an IV based on a country-specific probability that does not vary over time substituting $IMFprob_{it}$ by $IMFprob\_const_i$, which is given by

$$IMFprob\_const_i = \frac{\sum_{T=1973}^{2013} I(IMFprogram_{iT} = 1)}{41}$$

I thereby make the probability perfectly 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.

In the next table I report OLS and reduced form estimates (Table R3). 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

[34] I also ran regressions in which I excluded 50 and 200 observations. The results, which are available upon request, do not change substantially.

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, which I claim is causal, was 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.

To compare the results to studies using the standard instrument for IMF programs, I substitute the IV with UNGA voting behavior *ceteris paribus* (Table R4, columns 1-3). These regressions estimate IMF programs to cause rises in inequality of approximately four Gini points, comparable to Oberdabernig (2013), who uses the same IV. Considering that the estimated coefficients are equivalent to a change of up to 140% 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 even fall below Stock and Yogo's (2005) lowest critical value of 5.53 that tolerates a 2SLS size distortion of 25%. 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 my IV strategy identifies the true causal effect of IMF programs, the baseline regressions reported in Table 2 (Appendix A4) even provide empirical evidence for the violation of the exclusion restriction of UNGA voting: In the full baseline specification voting similarity with the U.S. in the UNGA is correlated 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.[35]

As a last step, I modify the main variables of interest. Regarding the independent 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 R5 (columns 4-6) I follow their approach and find that the results hold when only SBA and EFF programs are considered.

[35] 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.

Regarding the dependent variable, I first substitute the Gini coefficient of net income by that of gross income (*Gross Gini*), which is also taken from the SWIID. The fact that the results are very similar, indicates 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 cuts in public salaries and pensions or by rising unemployment after privatizations. 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 (e.g., Acemoglu et al. 2015; Oberdabernig 2013; Ostry, Berg, and Tsangarides 2014) 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.[36] The WIID, however, offers multiple Gini coefficients for many country-year observations. Since there is no commonly accepted procedure for choosing the respective values, the use of the WIID for regression analyses necessitates quite arbitrary decisions. This is presumably also why the SWIID is used much more frequently than the WIID.[37] 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. The author himself advises caution when using the resulting variable *Giniall* in regressions as the concepts underlying the calculation of the Gini coefficients are based on income and consumption, net and gross, as well as household and individual levels. Unfortunately, too few observations remain if the sample is restricted to one concept. Nevertheless, to address this issue I control for dummy variables that indicate the respective concepts interacted with country fixed effects. Columns 4-6 in Table R5 report the results. Note that, compared to the baseline, the sample size is severely limited. Nevertheless the coefficient of interest is statistically significant in the specifications that include control variables and even somewhat larger than in the baseline.

I conclude that the results are robust to these modifications.

[36] The 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.

[37] On Google Scholar the SWIID has 732 citations, while the WIID has 10 (May 20, 2016, search term "world income inequality database").

Table R1

	Excluding large (re)purchases			IV with liquid resources		
	(1)	(2)	(3)	(4)	(5)	(6)
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)
IV (1st stage)	-0.277***	-0.299***	-0.357***	-0.168***	-0.177***	-0.387***
	(0.053)	(0.064)	(0.069)	(0.043)	(0.056)	(0.087)
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
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

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

Table R2

	(1)	(2)	(3)
IMF Program$_{t-1}$	1.901*	1.691**	1.634**
	(1.145)	(0.746)	(0.788)
IMFprob_const x LQR	-0.173**	-0.271***	-0.300***
(IV in 1st stage)	(0.071)	(0.072)	(0.075)
Inequality Controls (t-1)	No	Yes	Yes
IMF Controls (t-1)	No	No	Yes
N	3766	3010	2625
Adjusted R^2	0.851	0.838	0.841
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

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

Table R3

	OLS	OLS-FE	OLS (Baseline)			Reduced Form (Baseline)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IMF Program$_{t-1}$	5.113***	0.651**	0.016	0.082	0.122			
	(0.903)	(0.270)	(0.071)	(0.073)	(0.076)			
IV (1st stage)						-0.312**	-0.442***	-0.459**
						(0.142)	(0.168)	(0.201)
Selection Ratio $\beta_8 / (\beta_8 - \beta_6)$							3.12	
Country & Year fixed effects	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 R4

	UNGA voting as IV			Only SBA & EFF Programs		
	(1)	(2)	(3)	(4)	(5)	(6)
IMF Program$_{t-1}$	4.644***	4.258**	4.259*			
	(1.773)	(2.002)	(2.247)			
UNGA voting (IV in 1st stage)	0.061***	0.073**	0.075**			
	(0.023)	(0.033)	(0.034)			
SBA/EFF Program$_{t-1}$				0.762***	0.805***	0.722*
				(0.286)	(0.312)	(0.389)
IV (1st stage)				-0.558***	-0.576***	-0.531***
				(0.059)	(0.068)	(0.071)
Inequality Controls (t-1)	No	Yes	Yes	No	Yes	Yes
IMF Controls (t-1)	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
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

Note: Dependent variable *Gini.* All regressions include country fixed effects and year fixed effects as well as the lagged dependent variable. In columns 4-6 only SBA and EFF programs are used to calculate the variable *IMFprob*, 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 R5

	Gross Gini (SWIID)			ATG Data		
	(1)	(2)	(3)	(4)	(5)	(6)
IMF Program$_{t-1}$	1.666***	1.544***	1.222**	1.220	1.974**	2.043**
	(0.561)	(0.579)	(0.575)	(1.155)	(0.927)	(1.014)
IV (1st stage)	-0.276***	-0.314***	-0.363***	-0.557***	-0.699***	-0.624***
	(0.053)	(0.063)	(0.067)	(0.113)	(0.130)	(0.135)
Inequality Controls (t-1)	No	Yes	Yes	No	Yes	Yes
IMF Controls (t-1)	No	No	Yes	No	No	Yes
ATG Controls (t-1)	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
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

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