APPENDIX (NOT INTENDED FOR PRINT)

Supporting Information for

The Economics of the Democratic Deficit:

The Effect of IMF Programs on Inequality

SUPPORTING INFORMATION

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Appendix A: Variables

Table 5 – Descriptive Statistics and Data Sources

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

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

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

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

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

Appendix B: Interpreting Differences in Gini Coefficients

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

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

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

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

$$P = 2MS$$

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

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

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

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

Appendix C: Baseline: Full Regression Output

Table 6 – Baseline, First Stage

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

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

Table 7 – Baseline, Second Stage

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

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

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

Appendix D: Decile-specific Effects: Full Regression Output

Table 8 – Decile-Specific Effects

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

Note: 2SLS regressions. Dependent variable is the income growth rate of deciles 1-10. All regressions include country fixed-effects, year fixed-effects, the lagged dependent variable, and IMFprobability. Standard errors, clustered at the country level, in parentheses. Significance levels: * p<.10, ** p<.05, *** p<.01

Appendix E: Long-Term Effects: Alternative Specifications

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

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

Table 9 – Long-Term Effects with Alternative Treatment Variables

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

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

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

Table 10 – Long-Term Effects with Control Variables

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

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

Appendix F: IMF Conditionality

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

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

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

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

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

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

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

Table 11 – IMF Conditionality:

	(1)	(2)	(3)	(4)
Scope of Conditionality	0.154**	0.163**		
	(0.076)	(0.073)		
Social Sector (incl. Pensions)			1.435***	1.727***
			(0.493)	(0.577)
Labor Market (private sector)			2.247***	2.614***
			(0.717)	(0.761)
Trade and Financial Liberalization			0.424	0.133
			(0.465)	(0.381)
Labor market (public sector)			-0.974	-1.032
			(0.676)	(0.661)
SOE reform			0.095	0.022
			(0.494)	(0.519)
Foreign Exchange Systems			0.586	0.519
			(0.390)	(0.426)
Central Bank			0.529	0.622
			(0.588)	(0.625)
Financial Sector			0.250	0.459
			(0.520)	(0.460)
Government			0.069	0.128
			(0.857)	(0.838)
Residual Category			-0.321	-0.089
			(0.453)	(0.371)
Year FE	Yes	Yes	Yes	Yes
Controls (Inequality)	Yes	Yes	Yes	Yes
Controls (IMF)	No	Yes	No	Yes
Observations	296	273	296	273
R-squared	0.099	0.218	0.137	0.262

Note: OLS regressions in the sample of observations with active IMF programs. Dependent variable is the Gini coefficient of net income. Standard errors, clustered at the country level, in parentheses.
*Significance levels: * p<.10, ** p<.05, *** p<.01*

Appendix G: Robustness

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

Challenging the identification I: IMF liquidity

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

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

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

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

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

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

Challenging the identification II: Heterogeneous and correlated trends

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

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

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

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

Challenging the identification III: Randomization

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

Challenging the identification IV: IMF probability

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

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

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

Challenging the identification V: Selection on observables vs. unobservables

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

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

Challenging the identification VI: Excluding the post-GFC period

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

Alternative instrumental variable

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

An additional control variable: debt

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

Alternative dependent variables

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

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

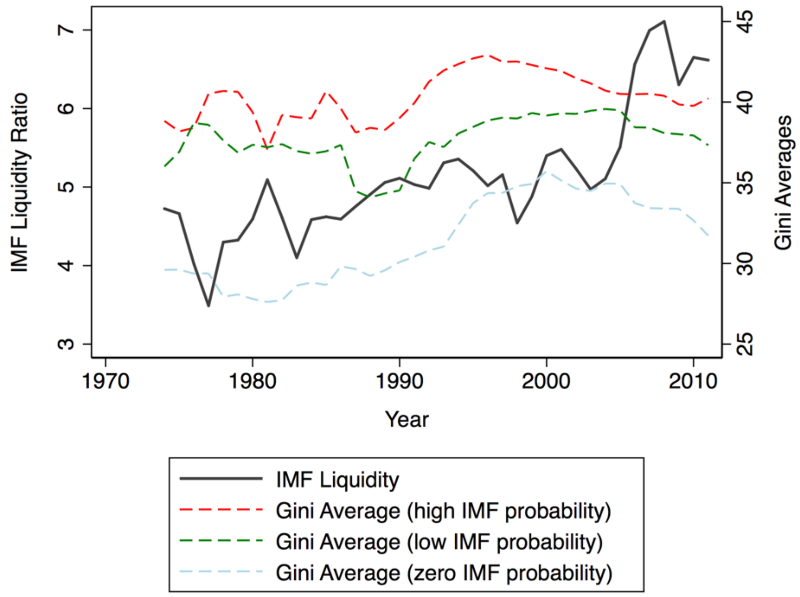
Income Inequality Database (WIID), on which the SWIID builds, over the SWIID.[7] The WIID, however, offers multiple Gini coefficients for many country-year observations. Since there is no commonly accepted procedure for choosing the respective values, the use of the WIID for regression analyses necessitates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

I conclude that the results are robust to these modifications.

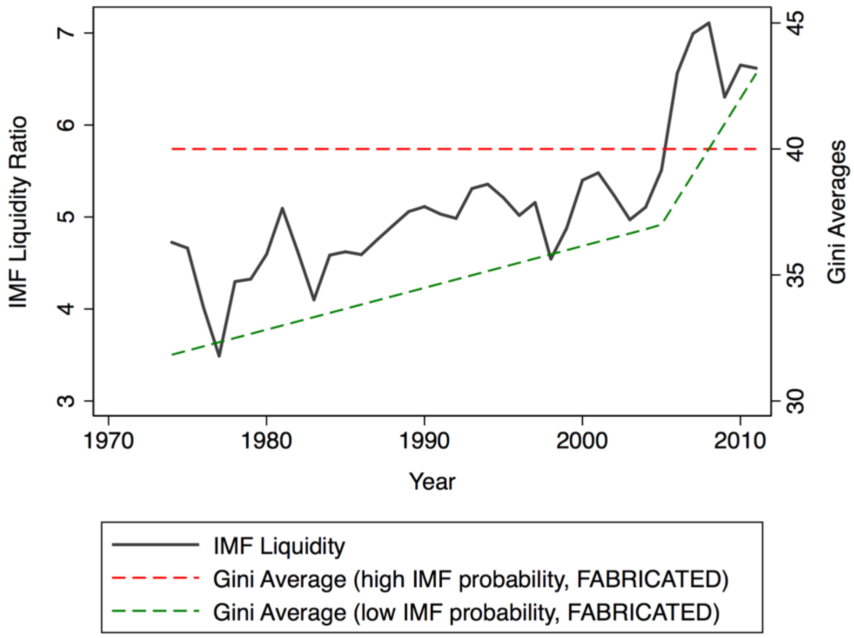
[7] Jenkins (2015) concerns, however, relate to an older version of the SWIID and Solt (2015) is able to overcome many of these concerns. The reader is referred to the entire special issue of the *Journal of Economic Inequality* (December 2015, Volume 13, Issue 4) for details on this debate.

Figure 5 – Spurious Correlations Between Inequality and IMF Liquidity?

Panel A: Actual Trends

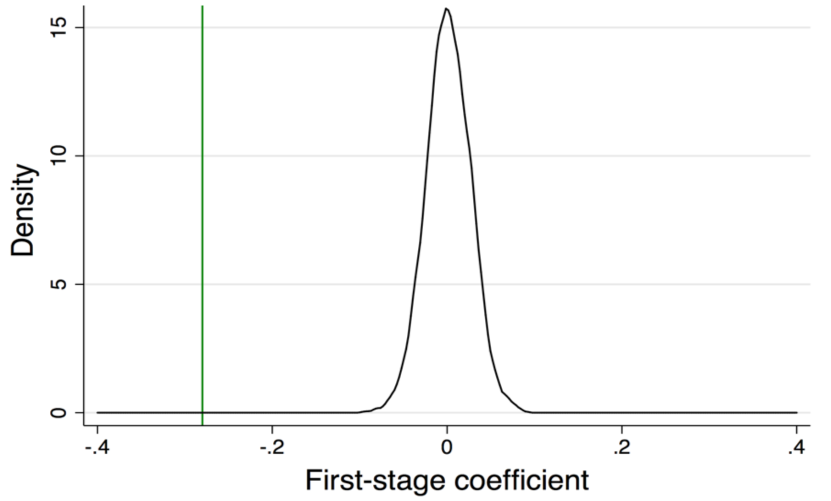


Panel B: Problematic Trends



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

Figure 6 – Randomizing IMF liquidity



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

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

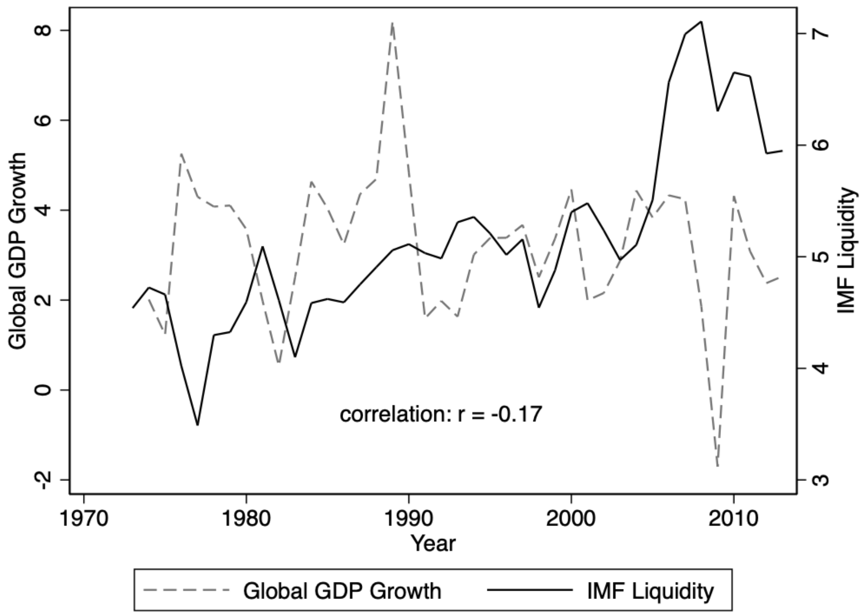


Figure 8 –IMF Liquidity and Global Crises: Variation over Time

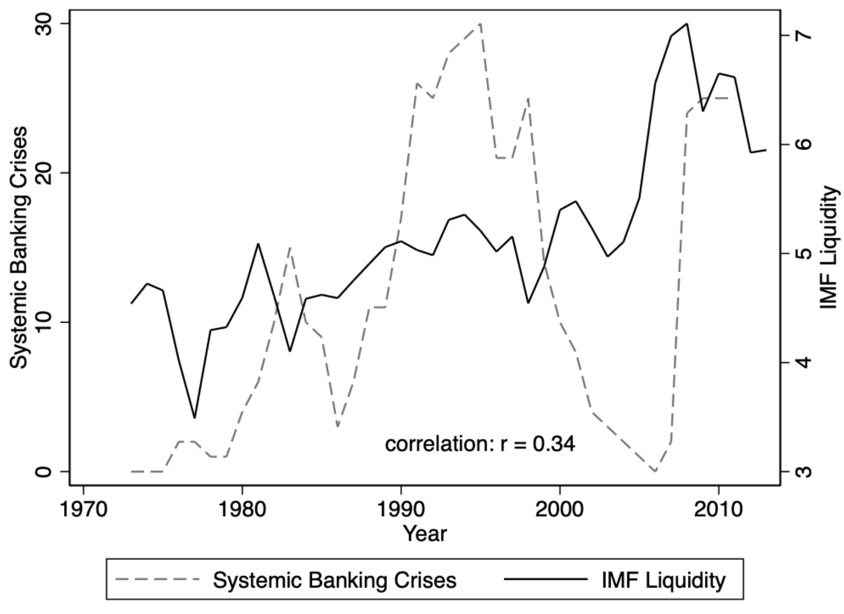


Figure 9 – IMF Liquidity and Global GDP Growth: Correlation

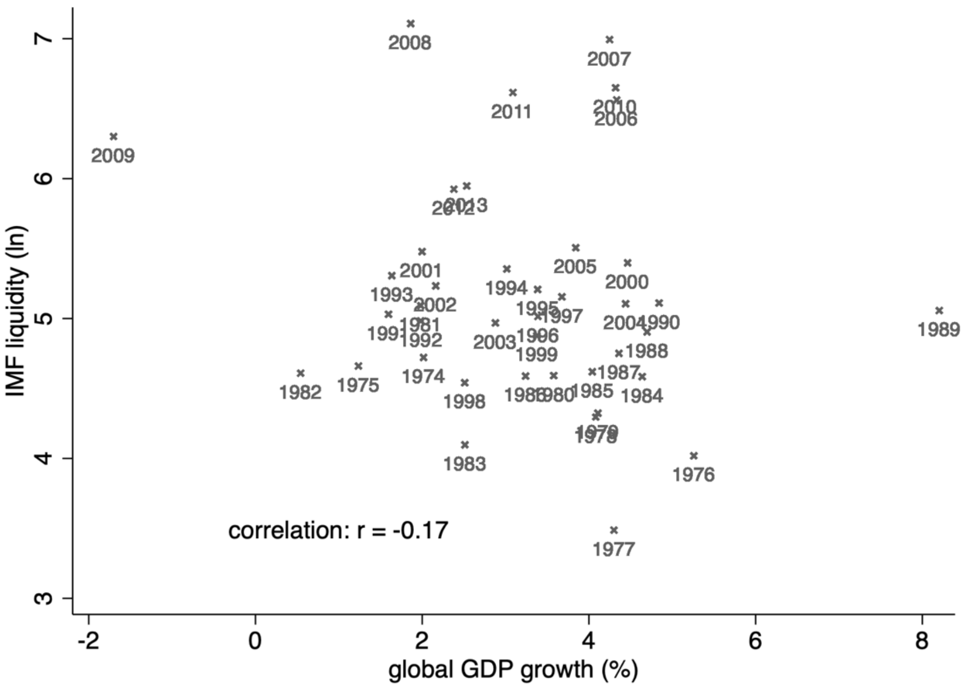


Figure 10 – IMF Liquidity and Global GDP Growth: Correlation

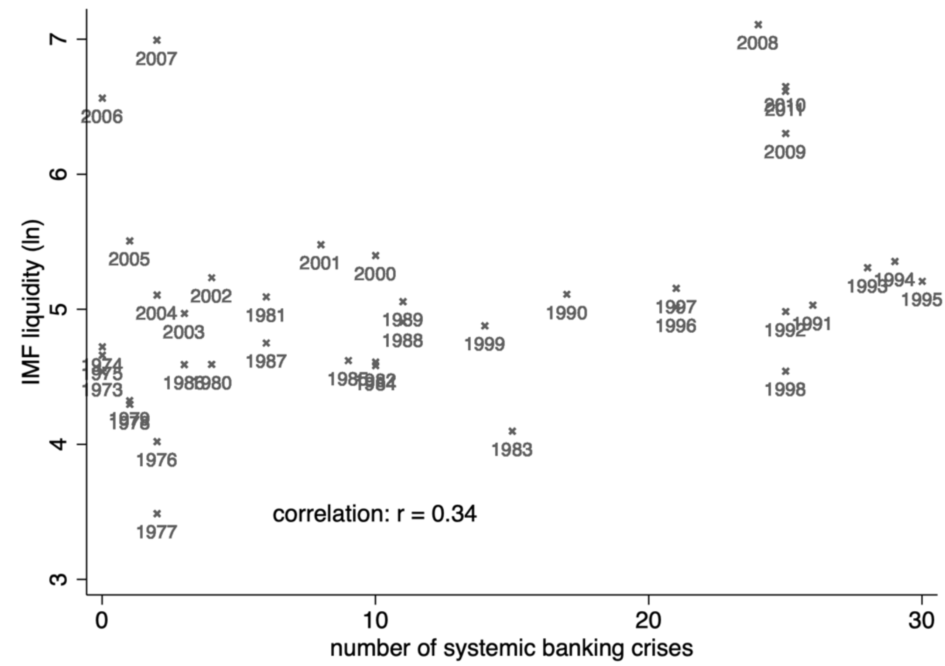


Table 12 – Robustness: Challenging the Liquidity Variable I

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

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

Table 13 – Robustness: Challenging the Liquidity Variable II

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

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

Table 14 – Robustness: Challenging the Probability Variable

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

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

Table 15 – Robustness: Selection on Unobservables

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

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

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

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

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

Table 17 – Robustness: UNGA voting as IV

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

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

Table 18: Controlling for Debt

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

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

Table 19 – Robustness: Alternative Inequality Data

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

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

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

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

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

Conditionality

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

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

Financing

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

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

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

Concessional programs vs. non-concessional programs

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

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

An additional plausibility check

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

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

Table 20 – Conditionality or Financing?

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

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

Table 21: Concessional and Non-concessional IMF programs

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

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

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

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

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

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