JUNIOR INDEPENDENT WORK

Understanding the IMF's Impact on Income Inequality: Institutional Change or Business as Usual?

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I pledge my honor that this paper represents my own work in accordance with University regulations.

> "Structural adjustments? That was before my time. I have no idea what it is. We don't do that any more. No, seriously, you have to realise that we have changed the way in which we offer our financial support."
>
> *Christine Lagarde, Former IMF Managing Director*[1]

I Introduction

As Managing Director Lagarde's words demonstrate, the IMF has made significant efforts to distance itself from the controversial record of austerity and structural adjustment programs. Despite the univocal rhetoric, however, whether the Fund has truly transformed its practices and their impacts remains an open question. In settling this debate, it is worthwhile to ask: has the IMF's effect on income inequality in developing countries changed in recent years?

The IMF's ability to influence, guide, and direct domestic policy decisions in countries around the world stands unparalleled among international actors (Stone, 2002; Babb, 2005). As such, the main tool it uses to affect such policy change—the practice of placing conditions on its loans, known as 'conditionality'—has been the subject of controversy for decades. Central to the debate on conditionality are questions over the degree to which IMF policy prescriptions benefit or harm the poor in borrowing countries. Scholars have sought to understand the effect of IMF program participation on GDP growth (Przeworski and Vreeland, 2002; Barro and Lee 2005; Dreher, 2006), inflation (Bird 2001), health and education spending (Clements, Gupta, and Nozaki, 2013), poverty (Hajro and Joyce, 2009), and a range of other economic outcomes in their efforts to answer this question.

Simultaneously, politicians, political parties, civil society groups, academics, and policy makers (including some at the IMF) have increasingly highlighted the economic and social importance of income inequality and its rise. As scholars have turned their attention to the effects

[1] IMF, "Transcript of the International Monetary and Financial Committee (IMFC) Press Briefing," IMF.org, April 12, 2014, https://www.imf.org/en/News/Articles/2015/09/28/04/54/tr041214b.

of IMF program participation and conditionality on income inequality, they have generally found that IMF programs exacerbate income inequality primarily due to the regressive impacts of austerity and liberalization.

Studying whether and the degree to which the IMF's effect on income inequality has changed over time can potentially provide insight into the "fundamental transformation" theory of IMF self-reform espoused by Lagarde and others both inside and outside the Fund (Grabel, 2011). The theory contests that changes in rhetoric and practice which the Fund has undertaken over the past two decades—driven by a combination of self-learning, development of academic insight, and public pressure from IMF critics—constitute a transformation significant enough to alter Fund program impacts. These changes, which followed the Asian Financial Crisis of the late 90s and increased criticism of IMF lending programs, include the replacement of the concessional Enhanced Structural Adjustment Facility (ESAF) with the Poverty Reduction and Growth Facility (PRGF) in 1999. In 2001, Managing Director Horst Köhler initiated a new policy to streamline conditionality and reduce its intrusiveness (Köhler, 2001). Later that same year, the IMF established the Independent Evaluation Office to assess and recommend solutions for adverse impacts of Fund programs (Weaver, 2010). Since this shift to a warmer, more progressive IMF began, the Fund has increased its rhetoric and policies around protecting social spending, reducing income and gender inequalities, promoting environmental sustainability, fighting corruption, and an array of other popular causes (Grabel, 2011).

If we observe a less regressive effect from more recent programs, we can interpret this as evidence of change in the ways the IMF designs and enforces agreements. Failing to observe such a change may support the business as usual hypothesis that practice has largely stayed the same (Kentikelenis, Stubbs, and King, 2016).

To test the hypothesis of period-dependent effects and overcome statistical problems of endogenous selection into IMF programs I employ an instrument variable 2SLS approach recently pioneered by Lang (2016) on a panel data sample of 137 low and medium income countries over a sample period from 1980 to 2014. I test for the presence of regime change following the Asian Financial Crisis and IMF reforms. Results estimate that on average, IMF programs help redistribute income one year after program years across the sample period. The data also suggest that programs were less regressive following the Asian Financial Crisis, but the difference is not statistically significant in all model specifications.

II Literature Review

In the first cross-national study on the IMF's distributional effects, Garuda (2000) uses propensity matching based on observable variables to find that IMF programs exacerbate income inequality overall, but that countries with more severe imbalances prior to participation experience significant increases in income inequality after participation while countries with mild imbalances *ex ante* see reductions in income inequality following participation.

Yet countries which enter IMF agreements likely differ systematically from countries which don't along unobservables such as "political will" to enact policies similar to those the Fund would be expected to prescribe (Vreeland, 2002). The endogeneity of program participation and the number of conditions a program receives is the primary methodological obstacle for analysis of IMF programs. To correct for selection bias, Vreeland (2002) uses Heckman propensity scores for program participation to predict participation in his regression. He finds significant negative effects of participation on labor's share of income. Oberdabernig (2013) employs Bayesian model averaging on 90 different models calculated with Heckman estimators from a number of different instruments and dozens of potential controls to correct for

endogenous participation and model uncertainty. She finds that Fund programs increase income inequality over her entire 1982-2009 sample, but decrease inequality over the 2000-2009 subsample. Although Heckman estimators are often employed to measure impacts of Fund programs, they cannot be easily calculated when the dependent variable of interest is continuous rather than binary. This complicates efforts to disentangle the effect of conditionality and conditions themselves from other components of IMF program participation (technical advice, signaling to potential investors, and loan tranches themselves). Differentiating between conditionality and these other components of IMF program participation allows further insight into the mechanism through which IMF programs affect inequality. Additionally, Heckman estimators preclude fixed effects from the regression due to multicollinearity (Stubbs, 2018).

As an alternative to Heckman estimators, system GMM models which include lagged outcome variables in the regression equation have been used in some studies of IMF program effects. Yet these models rely on a number of assumptions, including that "throughout the study period, [countries] sampled are not too far from steady states, in the sense that deviations from long-run means are not systematically related to fixed effects" (Roodman, 2009). This assumption is unlikely to hold in the context of IMF interventions (Stubbs *et al.*, 2018).

In light of the adverse selection problem and limitations to both Heckman estimators and GMM models, IMF scholars have sought to use instrument variables in 2SLS regressions to isolate IMF program treatment effects, but have struggled to find IVs which satisfy both exogeneity and relevance requirements (Vreeland, 2006). Barro and Lee (2005) were the first to propose using the share of votes cast in alignment with the US and other G7 nations in the UN General Assembly as the standard instrument due to the empirical link between such voting patterns and IMF loan offers. Yet others have recognized two main problems with this approach.

First, models using UNGA votes as instruments identify from geo-politically motivated IMF loans which may differ systematically from more routine programs, thus jeopardizing external validity. Second, the instrument likely fails the exclusion restriction due to the correlation between UNGA voting patterns and domestic politics. For example, countries who vote with the US in the UNGA may be more interested in privatizing SOEs independent of any IMF influence. Given that such domestic politics likely shape the outcome variable of interest, UNGA voting patterns may correlate with the error term (Lang, 2016).

To overcome this shortcoming Lang (2016) developed a new instrument variable which exploits changes in the IMF's liquidity ratio as an exogenous factor influencing whether a country enters an IMF program while not directly affecting income inequality nor other potential unobserved determinants directly. Lang finds that over a 1973-2013 sample period, IMF programs exacerbate income inequality in democracies while having no significant effect in non-democracies. More recently, Forster *et al.* (2019) proxy for the Fund's budget constraint with the number of countries under an IMF program, and instrument for both program participation and the degree of conditionality imposed on a program. They find that Fund programs, when excluding the effect of conditionality, reduced income inequality, but regressive effects of condirions rendered programs net regressive over a 1980-2014 sample period.

This study builds on the work of Lang as well as Forster *et al.* to correct for endogengeity of program participation and conditionality in understanding the IMF's distributional effects. In addition to testing instrument relevance, I use Lang and Forster's alternative instrument specifications to over-identify the model and test for joint instrument exogeneity. In addition to this econometric innovation, I perform Chow tests to test for the presence of a regime change in

the way IMF conditions affect inequality following the Asian Financial Crisis and related reforms.

III Data

I constructed a panel dataset for 134 low and medium income countries over a 1980-2014 sample period. Restricting the study to low and medium income countries reflects the understanding that the determinants of income inequality differ in low and high income countries (Dabla-Norris *et al.*, 2015). This total also excludes microstates with populations less than 200,000 in 1997—the midpoint of my analysis. 1980 represents the first year in which reliable data is available for IMF program conditionality. See **Appendix A** for a full list of countries.

Income inequality is measured by Gini coefficients as reported in the Standardized World Income Inequality Database (SWIID) and calculated based on post-tax and transfer disposable income. For robustness tests, I use shares of income going to the bottom and top quintiles from the World Development Index (WDI), but this data is much less complete, does not measure income post-tax and transfer in a standardized way, and is therefore discounted in cross-national studies of income distributions.

The independent variable of interest—the IMF Program—will have two components: First, a dummy variable for IMF program participation within a given year. Second, a sum of the number of conditions on a country for a given year to measure severity of conditionality and differentiate the effects of conditions from other channels through which participation can impact income inequality (the loan itself, technical advice, stability signaling, etc.). This variable can be measured in a number of ways including the sum of all conditions, or the sum of only conditions which must be met to maintain loan disbursement—what the literature has termed "binding

conditions".[2] In the absence of compelling reasons to prefer one measure of conditionality over the other, I perform my main analysis on all conditions and test for robustness by restricting to only binding conditions and correcting for incomplete implementation of these conditions.

The main flaw of this approach lies in the heterogeneity of conditions themselves. Conditions cover a wide range of policy areas from budgetary directives, to privatization of state-owned enterprises and anti-corruption measures. They further vary along dimensions of depth or intrusiveness ranging from the removal of capital controls to the commission of an exploratory study, the criteria on which they are evaluated, the degree to which they affect loan disbursement, the extent to which they are fulfilled by borrowing countries, and their interaction with other conditions. **Appendix B** provides a few examples of different types and content of conditions. The diversity of conditions suggests that the sum of conditions is an imperfect measure of conditionality. Nevertheless, the existing literature agrees that these measures are useful proxies for the intrusiveness of conditionality (Ivanova *et al.* 2001; Bulir and Moon 2004; Copelovitch 2010; Dreher and Jensen 2007; Dreher, Sturm, and Vreeland 2009).

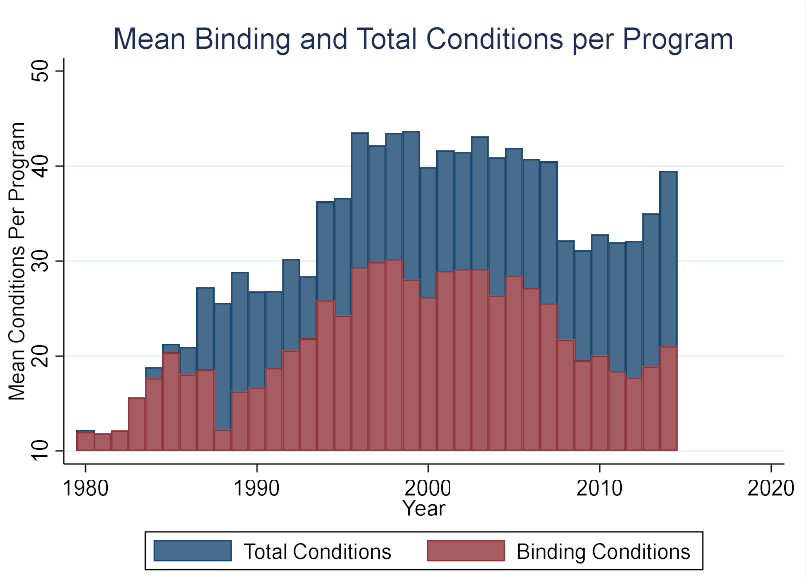
Given limitations in the data, a lack of available instruments, and the relatively small magnitude of observed effects, it is difficult to classify and discern the effects of different types of conditions. To provide illustrative evidence of the evolution of condition packages over time, **Table 1** tabulates the number of conditions in 9 policy areas by decade based on data from Kentikelenis et *al.* (2016). In addition, **Figure 1** graphs the average number of all conditions and binding conditions per program by year. There is an apparent reversal of the trend towards higher numbers of conditions in the 21st century, but conditionality may be on the rise toward the end of the sample period.

[2] From a technical standpoint, Prior Actions, Quantitative Performance Criteria, and Structural Performance Criteria are considered binding while Indicative Benchmarks and Structural Benchmarks are considered non-binding.

Table 1: Number of conditions per policy areas

Policy Area	1980-1989	1990-1999	2000-2014
External debt conditions Debt management and external arrears.	1238	4071	5596
Financial sector, monetary policy, and Central Bank issues Financial institution regulation, financial SOE privatization, treasury bills, interest rates, Central Bank regulation, money supply, and domestic credit.	1069	2847	3618
Fiscal Policy Expenditure administration, fiscal transparency, audits, budget preparation, domestic arears, and fiscal balance.	196	1225	2949
External Sector (trade and exchange system) Foreign reserves, trade liberalization, exchange rate policy, capital account liberalization, and foreign direct investment.	255	1332	1624
Revenue issues Customs administration, tax policy, tax administration, and audits of private enterprises.	6	512	794
Privatization and State-owned enterprise reforms and pricing Non-financial SOE privatization (incl. liquidation and bankruptcy proceedings), SOE restructuring, subsidies, price liberalization, audits, marketing boards, and corporatization and rationalization.	25	543	572
Labor issues Wage and employment limits, pensions, and social security institutions.	3	242	377
Institutional reforms Judicial system reforms, anti-corruption measures, competition enhancement, private sector development, devolution, sectoral policies, social policies (excl. poverty reduction policies), price increases for food, water, public transport, or other basic needs goods, land registries, granting of property rights, environmental regulations and access to commons.	5	197	183
Poverty-reduction policies Poverty Reduction Strategy Paper development, increases in social sector spending, and implementation of social safety nets.	0	18	67
Total	2797	10987	15780

Figure 1



Box 1[3]

The IMF lends under a number of facilities, but the most important distinction is between concessional and non-concessional arrangements.

Concessional loans provide funds to low-income countries at below-market interest rates (0.0-0.5%) with repayment starting 5.5-10 years after initial disbursement. Before 1999, concessional loans were given under the **Structural Adjustment Facility (SAF)** and **Enhanced Structural Adjustment Facility (ESAF)**. In 1999, the Fund replaced the ESAF with the **Poverty Reduction and Growth Facility (PRGF)**.

Non-concessional loans, since the 1980s, have largely taken two forms: The **Stand-By Agreement (SBA)** which lasts 1-2 years and provides short-term relief for acute crises and the **Extended Fund Facility (EFF)** which lasts 3-4 years and is designed to address long-term or structural imbalances.

I code three categorical variables for a Concessional, SBA, and EFF loan types respectively.

In addition to heterogeneity of the conditions themselves, different types of IMF programs respond to different crises, offer different degrees and forms of assistance, are financed through different mechanisms, and vary in the number and type of conditions applied to them. The most prevalent program types are described in **Box 1** above. **Table 2** breaks down the number of each type of program by decade and I differentiate by program type in some specifications. Data on loans and conditions come from IMF Monitor which the most recent scholarship had identified as a more complete and accurate source of data for IMF programs and conditions than the IMF's own MONA database (Kentikelenis, Stubbs, and King, 2016). In addition, this source enables me to select alternative measures of conditionality by classifying conditions as binding or non-binding, and including information on whether the IMF issued a waiver for a given condition.

[3] Alexander E. Kentikelenis, Thomas H. Stubbs, and Lawrence P. King, "IMF Conditionality and Development Policy Space, 1985–2014," *Review of International Political Economy* 23, no. 4 (July 3, 2016): 543–82, https://doi.org/10.1080/09692290.2016.1174953.

Table 2 Number of program years by type of arrangement

Arrangement	1980-1989	1990-1999	2000-2014
Concessional Structural Adjustment Facility (SAF); Enhanced Structural Adjustment Facility (ESAF); Poverty Reduction and Growth Facility (PRGF).	83	297	444
Enhanced Fund Facility (EFF)	66	93	48
Stand-By Agreement (SBA)	288	230	187
Total	392	620	679

Note: See **Box 1** for further descriptions

Extensive literature exists on relevant and robust determinants of income inequality. In my analysis I use a battery of control variables including those commonly used in studies of the IMF's impact on income inequality. All variables are described in **Appendix C** and summary stats are provided in **Appendix D**. Control variables are divided into three groups: instrument variables, economic controls, and political controls. To instrument IMF participation, I follow Lang's approach and interact the IMF liquidity ratio with the propensity of a country to enter into a program for a given year—measured by the country's average number of years in a program between the beginning of the sample period and the year of interest. Similarly, to instrument conditionality, I interact the IMF liquidity ratio with the propensity of a country to accept conditions in a given year—measured by the country's average number of conditions up to that point in the sample. Alternative instruments come from the Forster *et al.* approach to interact the total number of countries in an IMF program in a given year with these same propensities to enter IMF programs and accept conditions. Discussions of the instrument's theoretical strength and exogeneity are contained in Section IV.

Economic controls include macroeconomic variables that predict both inequality and program participation. The natural logarithm of per capita GDP as well as inflation, and unemployment are all thought to correlate with higher levels of inequality (Gourdon, 2007; Oberdabernig, 2013). Trade and FDI as percentages of GDP are thought to reduce income

inequality (Jaumotte *et al.*, 2013). Education measured in mean years of schooling and life expectancy at birth measure levels of human development and are thought to predict lower levels of inequality (Lang, 2016). I also include the percentage of the population living in urban areas, annual GDP growth rates, government expenditure as a percentage of GDP, total reserves measured in months of imports, the current account balance as a percentage of GDP, the Chinn Ito index of financial openness, and an indicator for the presence of a systemic banking crisis which serve as additional economic controls for inequality and predicting program participation (Forster *et al.*, 2019).

Political controls for income inequality and program participation include a democracy index, a measure of the executive branch's political orientation with more left-leaning governments receiving higher index values, and indicators for the occurrence of legislative and executive elections in a given year (Lang, 2016). Democracies and left-leaning governments are thought to be less tolerant of income inequality. Elections may affect whether countries participate and the degree of conditionality the IMF imposes. All control variables and summary statistics are provided in **Appendices C and D** respectively.

IV Methodology

For reasons discussed above, this study builds off the recent developments in instrument variable design to overcome the challenges of endogenous selection into IMF programs and conditionality. To do so, I run a two-stage least square (2SLS) regression with country and year fixed effects of Gini coefficients on my dependent variables of IMF program participation and conditionality. After testing for instrument relevance and exogeneity in the full sample, I test for regime changes in the way conditions affect income inequality around 2001.

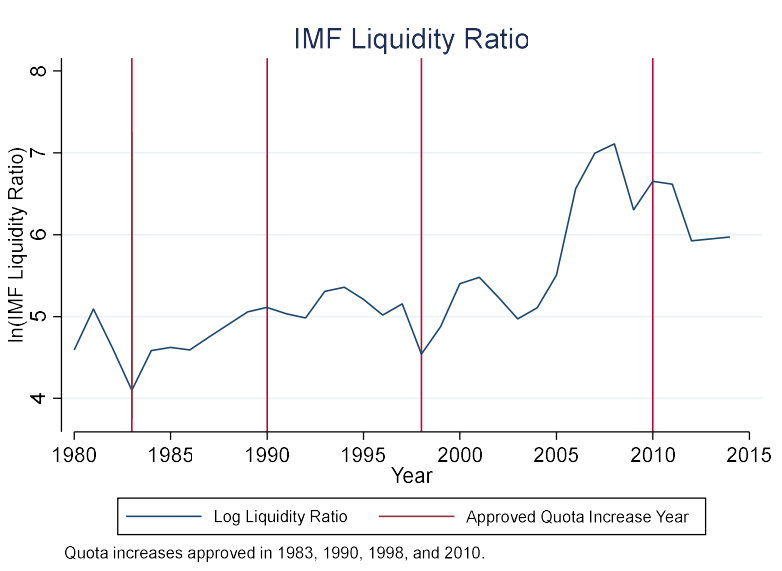
Country and year fixed effects can absorb some unobservables that influence country participation, conditionality, and inequality such as a country's long-term political orientation or global economic events, but time-variant country-specific variables such as 'political will' to enact liberalization reforms remain uncaptured by these models and may correlate with both program participation and levels of income inequality (Vreeland 2002). In addition to omitted variable bias, scholars have identified potential endogeneity bias from the fact that domestic political realities shape the conditions the IMF prescribes. Researchers have found that the Fund imposes fewer conditions on more politically constrained regimes such as democracies and election-facing executives (Rickard and Caraway, 2014). Extending this logic, others have argued that higher levels of income inequality *ex ante*, reduce a government's ability to enact liberalization reforms and therefore correlate with less severe conditionality from the Fund (Forster *et al.*, 2019). Such a systematic relationship between high levels of income inequality and lower levels of conditionality would bias our coefficient estimates for the true effect of conditions downward. Finally, measurement error in the explanatory variables could contribute to downward-biasing attenuation bias.

Valid instruments to correct for these sources of bias should both predict the explanatory variables of program participation and conditionality (the relevance condition) and not correlate with income inequality except through our explanatory variables (the exclusion assumption). Instrumenting with the IMF's liquidity ratio offers to satisfy both requirements. First, observers of the IMF have found that countries are less likely to receive loans and must accept higher levels of conditions when the Fund faces more serious budget constraints (Lang, 2016). As more assets become available to lend the Fund is more likely to approve programs and do so with

fewer conditions attached. This dynamic reflects changes in bargaining power between IMF officials and country representatives when negotiating IMF programs.

Lang argues that the exogeneity of the liquidity ratio stems from the fact that changes in the ratio correlate primarily to IMF Quota Reviews which occur periodically. At these meetings members agree on contributions to their quotas and often approve increases in IMF resources. The evolution of the liquidity ratio is presented in **Figure 2** with vertical lines corresponding to years in which the Fund approved additional quotas. Clearly, the schedule of these meetings does not derive from movements in Gini coefficients and the meetings themselves do not directly affect income inequality. Lang dismisses the ability of large loans and their repayment to seriously affect the liquidity ratio relative to quota reviews, but careful inspection of the liquidity ratio shows that its movements are not wholly predicted by quota increases. For instance, the large increase in liquidity in the mid-2000s correlates with Turkey and Brazil's repayments of large loans while many countries in Asia curtailed IMF borrowing in the aftermath of the Asian Financial Crisis (Vreeland, 2007).

Figure 2



Despite these concerns, a number of factors support the exogeneity of a liquidity ratio-derived instrument. First, most loan repayments do not dramatically affect the liquidity ratio. Second, of the large loan repayments which do move the liquidity ratio, these are agreed upon years in advance rendering their influence on the liquidity ratio virtually exogenous. Third, While the conclusion of a large IMF program in country A may have distributional consequences in country A, we should only be concerned with the liquidity ratio's exogeneity if the repayment affected inequality in other countries, say countries B, C, and D, through a channel other than the fact that these countries are now more likely to enter an IMF program. Finally, the specific design of the instruments discussed below interacts the liquidity ratio with a country's propensity to enter IMF programs and accept conditions. The unobserved mechanism between the liquidity ratio and inequality would have to be conditional on these country and time-specific propensities, country fixed effects, year fixed effects, and the vector of control variables. Beyond the theoretical defensibility of the instrument's exogeneity, over-identification tests discussed in section V provide statistical evidence for the theory.

Following Lang's methodology, I construct my primary instrument for program participation by interacting the Fund's liquidity ratio with a country's propensity to participate in an IMF program in a given year. To construct an instrument for conditionality I interact the liquidity ratio with a country's propensity to accept conditions in a given year. These instruments are represented by equations 1 and 2 below:

$$IV1_{i,t} = \ln(LiqRatio_t) \times IMFprob_{i,t} = \ln(LQR_t) \times \frac{\sum_{T=1980}^{t}(Program_i)}{t-1979} \quad (1)$$

$$IV2_{i,t} = \ln(LiqRatio_t) \times CONDprop_{i,t} = \ln(LQR_t) \times \frac{\sum_{T=1980}^{t}(TotalConditions_i)}{t-1979} \quad (2)$$

where *i* denotes country *i*, *t* a given year, Program_i is an indicator for a program in country *i*, and TotalConditions_i is the number of conditions applied to a given country. The advantage to such an approach is that it interacts a time-variant variable with a country-variant variable indicating the country's propensity to receive an IMF program and accept conditions. The resulting interaction term varies over time and across countries, therefore introducing exogenous variation (Werker, Ahmed, and Cohen, 2009; Nunn and Qian, 2014).

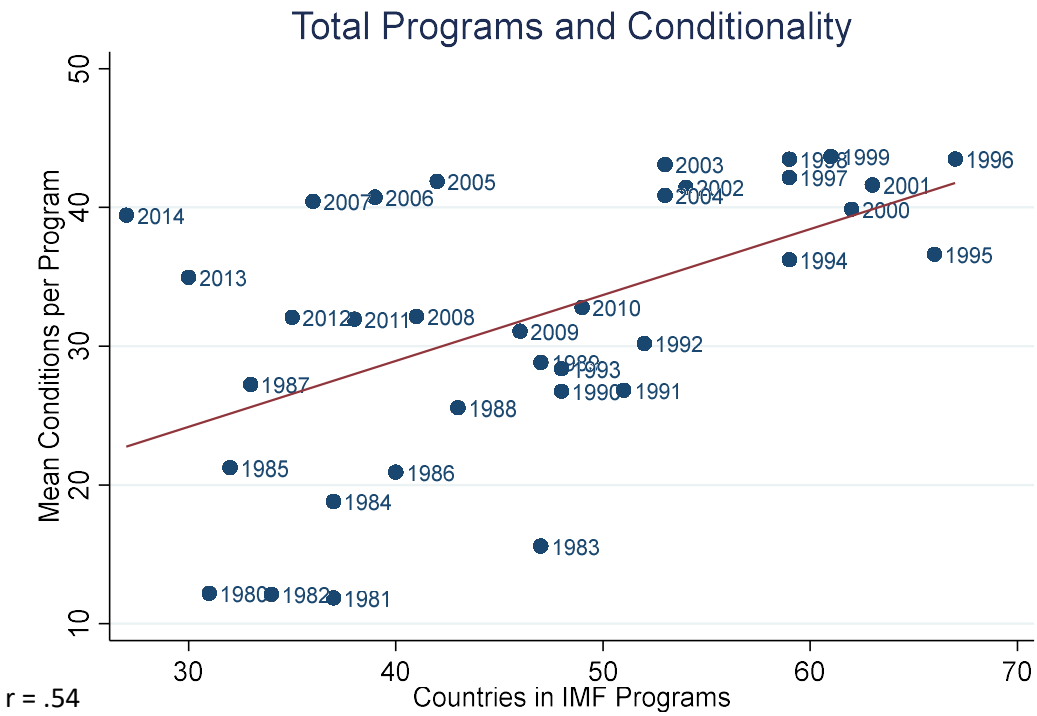
As an alternative to Lang's instrumentation, Forster *et al.* use the total number of countries in IMF programs in a given year to proxy for the IMF's liquidity based on the logic that more countries participating in programs strains resources at the Fund and lowers the liquidity ratio. **Figure 3** plots the average number of conditions per program against the total number of participating countries to illustrate the instrumented relationship. The correlation of 0.54 suggests this relationship can be exploited to create alternative instruments for participation and conditionality, as illustrated in equations 3 and 4 below.

$$IV3_{i,\,t} = \ln(LiqRatio_t) \times IMFprob_{i,\,t} = \text{TotalPrograms}_t \times \frac{\sum_{T=1980}^{t}(Program_i)}{t - 1979} \tag{3}$$

$$IV4_{i,\,t} = \ln(LiqRatio_t) \times CONDprop_{i,\,t} = \text{TotalPrograms}_t \times \frac{\sum_{T=1980}^{t}(TotalConditions_i)}{t - 1979} \tag{4}$$

Including both sets of instruments affords the opportunity to over-identify the model and thereby test instrument exogeneity.

Figure 3



Using these sets of instruments, I run two first-stage regressions and test for instrument strength using the Kleibergen-Paap Wald F-statistic. By the rule of thumb, F-statistics above 10 indicate adequately relevant instruments. The first-stage regressions are described by equations 5 and 6 below. Equation 7 presents the second stage main regression.

$$\widehat{Program}_{i,\,t} = \pi_0 + \boldsymbol{\pi}'_1\mathbf{IV}_{i,\,t} + \boldsymbol{\pi}'_2\mathbf{X}_{i,\,t} + \mu_i + \upsilon_t \quad (5)$$

$$\widehat{TotalConditions}_{i,\,t} = \delta_0 + \boldsymbol{\delta}'_1\mathbf{IV}_{i,\,t} + \boldsymbol{\delta}'_2\mathbf{X}_{i,\,t} + \mu_i + \upsilon_t \quad (6)$$

$$\text{Gini}_{i,\,t} = \beta_0 + \beta_1\widehat{Program}_{i,\,t-1} + \beta_2\widehat{TotalConditions}_{i,\,t-1} + \boldsymbol{\beta}'_3\mathbf{X}_{i,\,t-1} + \mu_i + \upsilon_t + \varepsilon_{i,\,t} \quad (7)$$

Where for country, *i*, in year, *t*, π_0 and δ_0 are constants, $\mathbf{IV}_{i,\,t}$ represents the matrix of instruments, with $\boldsymbol{\pi}'_1$ and $\boldsymbol{\delta}'_1$ representing their coefficients. $\mathbf{X}_{i,\,t}$ is the matrix of control variables from the main regression, described in section III above as well as **Appendix C**. μ_i captures time-invariant

country fixed effects, and υ_t captures country-invariant year fixed effects. Assuming that most of an IMF program's effect on the income distribution is not instantaneous, I lag all explanatory variables in the main regression by one year. In this equation, β_1 is the coefficient of the endogenetiy-corrected IMF program participation and β_2 is the endogeneity-corrected coefficient of conditionality. With these regressions, I can test for over-identification to verify the exogeneity of the instruments and confirm their validity for this analysis.

To test the hypothesis of institutional change at the IMF, I perform Chow tests of regime change in and around 2001 and compare the effect of conditionality across the two regimes. A statistically significant coefficient on the interaction between the measure for conditionality and the post-period indicator would provide evidence of changing distributional effects of conditionality over time. In addition to these tests, I repeat my analysis with alternative measures of conditionality and I test the distributional effects of the three main types of IMF programs described above.

V Results

Table 3 reports the correlation table between Gini coefficients and the explanatory variables of interest. The negative correlation between Gini coefficients and conditions corroborates the theory that countries with higher levels of inequality receive fewer conditions under IMF programs. This correlation becomes stronger when we look at only observations with an active program.

Table 3
Correlation Matrix: full sample

Variable	Gini	Program	Total Conditions
Gini	1.0000		
Program	-0.0422	1.0000	
Total Conditions	-0.0732	0.8083	1.0000

Correlation Matrix: Program = 1

Variable	Gini	Total Conditions
Gini	1.0000	
Total Conditions	-0.1143	1.0000

Advancing to multivariate analysis, **Table 4** presents results of the basic OLS regression with heteroscedasticity robust, clustered standard errors, but without correcting for endogeneity. specification 1 includes only basic economic control variables. In specifications 2 and 3, respectively, I add the indicator for an IMF program and then the total number of conditions as explanatory variables. specification 4 includes additional economic and political controls. I then perform a Hausman test for fixed vs. random effects which confirms the appropriateness of fixed effects with a p-value of 0.002 and specification 5 utilizes fixed and time effects. From these specifications we can see the most consistently significant control variables are per capita GDP growth, which correlates with higher levels of income inequality as well as education and life expectancy which correspond to lower levels of inequality. Once additional controls are added, we see that unemployment exacerbates inequality while a government's left leaningness correlates with lower levels of inequality as expected.

Turning to the explanatory variables of interest, the effect of an IMF program distinct from the effect of conditions remains statistically insignificant across all 5 models. Conditions first contribute to lower levels of inequality, but have no statistically significant effect in the

fixed effects specifications. As a reasonability check on these findings before proceeding further, specification 6 includes indicators for the three types of IMF programs described in **Box 1** above. This specification shows that conditions exacerbate inequality at the 5% level and raises the within-unit R^2 value. Furthermore, we can see that separate from the effects of conditions, Concessional IMF programs reduce income inequality, SBA agreements are neutral in their distributional impact, and Extended Fund Facility programs are regressive. This affirms the reasonableness of results obtained thus far.

The magnitude of these results may appear small, but Gini coefficients are measured on a 0 to 100 scale. Across the entire sample, Gini values range from a minimum of 19.8 to a maximum of 66.5, meaning that movements occur within a 47-point band. At the sample mean Gini value of 42.8, a 1-point change in the Gini represents a 2.3% proportional change—a relatively large movement for the single year between the lagged program variables and observed Gini coefficients. In this study, even small effects are economically significant.

Table 4: Baseline Model

Dependent Variable	Gini Coefficient					
Specification	(1)	(2)	(3)	(4)	(5)	(6)
L.Program		-1.016	0.361	1.055	-0.0185	0.599**
		(0.790)	(0.971)	(0.994)	(0.188)	(0.261)
L.Total Conditions			-0.0370**	-0.0419**	0.00194	0.00409
			(0.0167)	(0.0168)	(0.00327)	(0.00300)
L.Concessional						-0.995***
						(0.285)
L.SBA						-0.578**
						(0.282)
L.EFF						-0.274
						(0.334)
L.ln(Per Capita GDP)	2.485**	2.275**	2.192**	1.593	3.410***	3.489***
	(1.020)	(1.013)	(1.014)	(1.217)	(1.100)	(1.081)
L.Education	-0.947***	-0.926***	-0.907***	-1.268***	0.0255	0.0352
	(0.347)	(0.346)	(0.343)	(0.344)	(0.202)	(0.199)
L.Trade	-0.00839	-0.00954	-0.00961	0.00212	-0.00306	-0.00370
	(0.0163)	(0.0166)	(0.0165)	(0.0172)	(0.00518)	(0.00517)
L.FDI	-0.0529	-0.0493	-0.0485	-0.0464	-0.0160*	-0.0144

	(0.0449)	(0.0445)	(0.0438)	(0.0453)	(0.00867)	(0.00901)
L.Inflation	-0.000240	-0.000314	-0.000295	-0.00281	-0.000278	-0.000310
	(0.00109)	(0.00109)	(0.00108)	(0.00288)	(0.000528)	(0.000562)
L.Unemployment	0.199	0.207	0.213	0.318**	0.0882*	0.0844*
	(0.134)	(0.137)	(0.137)	(0.133)	(0.0471)	(0.0459)
L.Life Expectancy	-0.273**	-0.270**	-0.269**	-0.287***	0.0209	0.00619
	(0.106)	(0.105)	(0.105)	(0.107)	(0.0558)	(0.0542)
L.Urban				-0.0210	0.0367	0.0300
				(0.0520)	(0.0540)	(0.0541)
L.GDP Growth				0.0354	-0.0353***	-0.0372***
				(0.0567)	(0.0122)	(0.0120)
L.Gov. Expenditure				-0.222	-0.0300	-0.0292
				(0.141)	(0.0344)	(0.0347)
L.Reserves				0.249	0.00415	0.00643
				(0.169)	(0.0525)	(0.0529)
L.CAB				0.0149	-0.0387***	-0.0394***
				(0.0566)	(0.0124)	(0.0125)
L.Financial Openness				2.669*	-0.00338	-0.00352
				(1.558)	(0.555)	(0.552)
L.Sys. Bank Crisis				-1.644**	-0.0225	-0.0399
				(0.766)	(0.165)	(0.155)
L.Democracy Index				0.766***	0.0621	0.0766
				(0.235)	(0.0717)	(0.0699)
L.Gov. Orientation				0.683*	-0.227**	-0.223**
				(0.407)	(0.108)	(0.105)
L.Leg. Election				-0.894**	-0.0602	-0.0570
				(0.420)	(0.0460)	(0.0463)
L.Exe. Election				1.283*	0.0538	0.0437
				(0.761)	(0.0570)	(0.0555)
Constant	46.20***	47.92***	48.34***	50.94***	13.80	14.34
	(4.628)	(5.004)	(4.964)	(6.798)	(8.820)	(8.788)
Country Fixed Effects	No	No	No	No	Yes	Yes
Year Fixed Effects	No	No	No	No	Yes	Yes
Observations	1,914	1,914	1,914	1,497	1,497	1,497
R-squared	0.208	0.212	0.216	0.342	0.263	0.275

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5 presents results from first-stage regressions on the four instrument variables discussed in Section IV. With F-statistics 22.4 and 41.9, we can have confidence in the relevance of the instruments for predicting program participation and total conditions respectively. In the test for over-identification on the second-stage model with all four instruments, a calculated p-value of 0.49 suggests that we cannot reject the null of all exogenous instruments at standard

confidence levels. Given these statistical results, we have sufficient evidence for the relevance and exogeneity of our instruments.

Table 5: First Stage Regressions and Instrument Strength

VARIABLES	Program	Total Conditions
IV1	0.227***	9.477***
	(0.0714)	(3.608)
IV2	-0.00747***	-0.437***
	(0.00178)	(0.101)
IV3	-0.00595	-1.694***
	(0.00878)	(0.379)
IV4	0.000926***	0.0904***
	(0.000220)	(0.00970)
Control Vector	Yes	Yes
Country Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
F-Statistic	22.37	41.90
Over-identification Test p-value	0.49	0.49
Observations	1,497	1,497
R-squared	0.212	0.268

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Having established the strength and exogeneity of the instruments, **Table 6** presents results of second-stage regressions. Specification 7 regresses Gini coefficients on the instrumented program participation, and total conditions variables along with economic and political controls, country fixed effects, and year fixed effects. According to this model, program participation, absent any effect from conditionality, lowers Gini coefficients by 1.89 points. Our measure of conditions itself is statistically significant at the 10% level, suggesting that each additional condition increases Gini coefficients by 0.016. To interpret this coefficient, it is important to consider that IMF programs often have dozens of conditions—an average of 33.2 conditions per program across the entire sample period. As such, we could say that an average IMF program's net effect on the income distribution would be a decrease of 1.35 points on the Gini scale. This effect remains statistically significant at the 1% level.

Table 6: Endogeneity-Corrected Models

Dependent Variable	Gini Coefficient		
Specification	(7) Total Conditions	(8) Total Binding Conditions	(9) Implementation-Corrected Total Binding Conditions
L.Program	-1.887***	-2.517***	-2.073***
	(0.533)	(0.568)	(0.451)
L.Total Conditions	0.0162*	0.0574***	0.0565***
	(0.00870)	(0.0131)	(0.0121)
L.ln(Per Capita GDP)	3.291***	3.624***	3.862***
	(0.326)	(0.338)	(0.355)
L.Education	0.0750	0.0824	0.0162
	(0.0965)	(0.100)	(0.0997)
L.Trade	-0.00769***	-0.00738***	-0.0102***
	(0.00270)	(0.00285)	(0.00302)
L.FDI	-0.0117	-0.0114	0.00129
	(0.00831)	(0.00864)	(0.0115)
L.Inflation	-0.000226	-0.000520	-0.000529
	(0.000630)	(0.000658)	(0.000540)
L.Unemployment	0.116***	0.115***	0.0805***
	(0.0204)	(0.0214)	(0.0207)
L.Life Expectancy	0.0213	-0.00373	0.00741
	(0.0260)	(0.0275)	(0.0281)
L.Urban	0.0543***	0.0453**	0.0645***
	(0.0169)	(0.0176)	(0.0198)
L.GDP Growth	-0.0380***	-0.0360***	-0.0250**
	(0.0110)	(0.0115)	(0.0109)
L.Gov. Expenditure	-0.00908	-0.00663	0.0224
	(0.0182)	(0.0188)	(0.0174)
L.Reserves	-0.0198	-0.0256	0.0223
	(0.0210)	(0.0220)	(0.0220)
L.CAB	-0.0309***	-0.0324***	-0.0441***
	(0.00810)	(0.00844)	(0.00829)
L.Financial Openness	-0.112	-0.107	-0.304
	(0.222)	(0.231)	(0.221)
L.Sys. Bank Crisis	0.153	0.266	0.230
	(0.234)	(0.247)	(0.203)
L.Democracy Index	0.0836**	0.0867**	0.0300
	(0.0379)	(0.0396)	(0.0353)
L.Gov. Orientation	-0.284***	-0.281***	-0.137**
	(0.0519)	(0.0535)	(0.0537)
L.Leg. Election	-0.0102	0.0606	0.0342
	(0.0958)	(0.101)	(0.0933)
L.Exe. Election	0.0372	-0.00275	-0.0221
	(0.119)	(0.124)	(0.119)
Constant	8.852***	0.341***	6.508*
	(3.231)	(0.0241)	(3.473)
Country Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Program F-Statistic	22.37	10.82	17.31
Total Conditions F-Statistic	41.90	11.63	17.80
Observations	1,497	1,497	1,156

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The results suggest that IMF programs on average redistribute post-tax and transfer income toward the bottom half of the income distribution one year after program participation. This stands in contrast to a relatively large body of endogenetiy-corrected studies on the IMF's distributional effects which find net regressive effects (Vreeland, 2002; Oberdabernig, 2013; Lang 2016; Forster *et al.*, 2019). To test the robustness of these results, specification 8 uses the total number of binding conditions as the measure of conditionality and redefines the instrument variables appropriately. With an average of 22.5 binding conditions per program, this specifaction suggests that on average an IMF program decreases Gini coefficients by 1.22 points. As an additional robustness check, I use a measure of binding conditions which discounts conditions for which a country received an implementation waiver from the IMF and redefines the instrument variables appropriately in specification 9. This measure provides a clearer picture of implementation-corrected level of conditionality, but the data for this measure is unavailable from 2009-2014 which reduces the sample size considerably. With a mean of 21.9 non-waived conditions per program, the results of specification 9 show that an average IMF program would decrease a country's Gini coefficient by 0.78 points. In all three specifications, tests for instrument over-identification cannot reject exogeneity. In short, results are relatively consistent across all three specifications.

As additional robustness checks, **Appendix E** includes regression results for the share of income controlled by the top and bottom income quintiles as the outcome variables of interest. These models encounter more severe missing data problems and results are statistically insignificant. Finally, **Appendix F** shows results from models where I redefine the explanatory variables of interest to capture program type in an effort to test whether programs differ in their

distributional effects. In these models, the instruments are weak and invalid, jeopardizing any attempt to make meaningful inference.

Turning to the final question of analysis, I test to see if the distributional effects of IMF programs have changed since the Asian Financial Crisis and its aftermath. 2001 provides a reasonable break point for splitting the sample because by this time, the political backlash against the IMF in Asia had peaked and the IMF instituted a number of reforms in response (Vreeland, 2007). **Table 7** presents regression results incorporating an interaction between the total conditions variable and the post-period indicator. In addition, to preserve the model's instrumentation, I interact the post-2001 period indicator with IV2 and IV4 as well. As in Table 6, I use three alternative specifications for the conditionality variable: total conditions, total binding conditions, and binding conditions discounting waived conditions.

Chow tests indicate statistically significant break points in 2001 for all three specifications. In the total binding and non-binding conditions specification, specification 10, the interaction between the total conditions and the post period indicator is negative, but insignificant. This coefficient becomes statistically significant when we redefine the conditionality measure as only binding conditions or implementation-corrected conditions, providing some evidence that IMF conditions have become less regressive since 2001. If we consider the distributional impact of an average IMF program in both periods, programs before 2001 had an average of 31 conditions and those after 2001 had 37.4 conditions on average. This would suggest that the average IMF program reduced Gini coefficients by 1.25 points before 2001 and 1.48 points after 2001 once we consider the average number of conditions per program.

For the binding conditions measure in specification 11, programs before 2001 had 21.9 binding conditions on average and lowered Gini coefficients by 1.09 while programs after 2001

with an average of 23.8 binding conditions per program lowered Gini coefficients by 1.63. Instrument strength declines significantly in the implementation-adjusted model and over-identification tests reject the joint exogeneity of all instruments. Alternative break points of 2000, and 2002 yield similar results. Coefficients in specification 12, where I use the implementation-adjusted measure of binding conditions, resemble those in specifications 10 and 11 reasonably well. But I hesitate to make any inference from this model due to the relatively weak instruments and its failure to pass the over-identification test.

Table 7: Test for Regime Change (2001)

Dependent Variable	Gini Coefficient		
VARIABLES	(10) Total Conditions	(11) Total Binding Conditions	(12) Implementation-Corrected Total Binding Conditions
L.Program	-1.783***	-2.523***	-2.115***
	(0.528)	(0.570)	(0.448)
L.Total Conditions	0.0173**	0.0655***	0.0640***
	(0.00877)	(0.0136)	(0.0122)
L.Total Cond × Post 2001	-0.00918	-0.0279**	-0.0278***
	(0.00693)	(0.0118)	(0.0101)
L.Post 2001	-5.492***	-5.509***	-3.753***
	(0.777)	(0.817)	(0.686)
L.ln(Per Capita GDP)	3.282***	3.599***	3.902***
	(0.326)	(0.344)	(0.359)
L.Education	0.0741	0.0927	0.0215
	(0.0968)	(0.103)	(0.102)
L.Trade	-0.00830***	-0.00901***	-0.0122***
	(0.00277)	(0.00301)	(0.00315)
L.FDI	-0.0101	-0.00722	0.00987
	(0.00844)	(0.00902)	(0.0121)
L.Inflation	-0.000269	-0.000633	-0.000664
	(0.000632)	(0.000672)	(0.000552)
L.Unemployment	0.115***	0.113***	0.0733***
	(0.0204)	(0.0217)	(0.0210)
L.Life Expectancy	0.0282	0.00933	0.0209
	(0.0266)	(0.0286)	(0.0294)
L.Urban	0.0514***	0.0407**	0.0619***
	(0.0170)	(0.0179)	(0.0200)
L. GDP growth	-0.0369***	-0.0333***	-0.0182
	(0.0111)	(0.0117)	(0.0113)
L.Gov. Expenditure	-0.00760	0.00136	0.0333*
	(0.0183)	(0.0196)	(0.0182)
L.Reserves	-0.0182	-0.0259	0.0253
	(0.0211)	(0.0224)	(0.0223)
L.CAB	-0.0296***	-0.0287***	-0.0413***

	(0.00822)	(0.00879)	(0.00850)
L.Financial Openness	-0.119	-0.111	-0.327
	(0.223)	(0.236)	(0.225)
L.Sys. Bank Crisis	0.161	0.314	0.252
	(0.235)	(0.253)	(0.207)
L.Democracy Index	0.0877**	0.0977**	0.0414
	(0.0382)	(0.0407)	(0.0361)
L.Gov. Orientation	-0.285***	-0.293***	-0.146***
	(0.0520)	(0.0548)	(0.0547)
L.Leg. Election	-0.00304	0.0808	0.0550
	(0.0963)	(0.104)	(0.0953)
L.Exe. Election	0.0261	-0.0314	-0.0213
	(0.120)	(0.127)	(0.121)
Constant	14.14***	13.29***	9.136***
	(2.876)	(3.023)	(3.214)
Country Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Program F-Statistic	42.22	14.45	17.06
Total Conditions F-Statistic	30.19	24.11	10.25
Over-identification Test	Pass	Pass	Fail
Chow Test	Structural Break	Structural Break	Structural Break
Observations	1,497	1,497	1,156

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A number of limitations of the above results merit discussion. First, the panel data for many variables of interest to this study suffer from serious missing data problems. For example, even Gini coefficients are missing for 40% of country years. If missing data differs systematically from observed data, this could introduce bias to coefficient estimates. Additionally, this study lacks data on the mechanism through the IMF funds particular programs. Not all programs are funded by member quotas and the IMF has a dense system of bilateral and multilateral arrangements to borrow from member countries to bolster its lending capacity. Such arrangements do not appear in the liquidity ratio because they are not considered liquid assets. Thus, their presence may complicate the relationship between the liquidity ratio and lending practices the current instruments rely on. Finally, even though the liquidity ratio-derived instruments seem to be relevant and defensibly exogenous, they do a poor job at predicting

whether a country enters a particular type of IMF program and thus do not help us understand different effects of different types of programs.

VI Conclusion:

This study verified the relevance of the IMF's liquidity ratio to use when constructing instrument variables for IMF program participation and the application of conditions. Additionally, I found that instruments constructed from a proxy of the IMF's budget constraint—the total number of countries under a program—can be used as viable alternative instruments in line with Forster *et al.*. When we combine both sets of instruments, we can confirm instrument endogeneity.

Beyond methodological progress, results show significant redistributive effects of IMF programs, standing at odds with the existing scholarly consensus on the IMF's distributional impacts. If nothing else, this suggests that earlier authors' results may be contingent on sample design and control variable specifications and that further research is needed to understand the IMF's distributional effects with more precision. Consistent with the existing literature, I found that conditions themselves increase income inequality while other effects of IMF programs tend to ameliorate inequality within one year. Secondly, the significant reduction of conditionality's regressive effects after 2001 supports theories of institutional change at the Fund and the way it designs programs since the Asian Financial crisis and its aftermath. The reduced regressive effect could be due to lower incorporation of austerity conditions over time, the inclusion of health and education spending floor conditions, or the increase of other poverty-reduction conditions. These areas merit further investigation.

As we enter into a period of global economic undcertainty and fragility, IMF lending is likely on track to expand significantly. Our understanding of how the Fund's budget constraint

influencing lending practices suggests that with more countries under IMF programs, we can expect more conditions on this lending. In the face of these trends, this study suggests that researchers should seek to understand how conditionality and its effects have or have not changed over time. In doing so, further work should be done to compare results obtained through IV approaches with other endogenous participation correcting approaches such as GMM models.

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Appendix A: Countries

Afghanistan	Cuba	Korea, North	Peru
Albania	Czech Republic	Korea, South	Philippines
Algeria	Cote d'Ivoire	Kyrgyz Republic	Poland
Angola	Djibouti	Lao PDR	Puerto Rico
Argentina	Dominican Republic	Latvia	Russian Federation
Armenia	Ecuador	Lebanon	Rwanda
Azerbaijan	Egypt	Lesotho	Saudi Arabia
Bahrain	El Salvador	Liberia	Senegal
Bangladesh	Equatorial New Guniea	Libya	Sierra Leonne
Barbados	Eritrea	Lithuania	Slovak
Belarus	Estonia	Madagascar	Solomon
Belize	Eswatini	Malawi	Somalia
Benin	Ethiopia	Malaysia	South
Bhutan	Fiji	Maldives	Sri
Bolivia	Gabon	Mali	Sudan
Bosnia	Gambia, The	Mauritania	Suriname
Botswana	Georgia	Mauritius	Syrian
Brazil	Ghana	Mexico	Tajikistan
Bulgaria	Guatemala	Moldova	Tanzania
Burkina Faso	Guinea	Mongolia	Thailand
Burundi	Guinea-Bissau	Morocco	Togo
Cabo Verde	Guyana	Mozambique	Trinidad
Cambodia	Haiti	Myanmar	Tunisia
Cameroon	Honduras	Namibia	Turkey
Central African Rep.	Hungary	Nepal	Turkmenistan
Chad	India	Nicaragua	Uganda
Chile	Indonesia	Niger	Ukraine
China	Iran	Nigeria	Uruguay
Colombia	Iraq	North Macedonia	Uzbekistan
Comoros	Jamaica	Oman	Venezuela
Congo	Jordan	Pakistan	Vietnam
Costa Rica	Kazakhstan	Panama	Yemen,
Croatia	Kenya	Papua New Guinea	Zambia
		Paraguay	Zimbabwe

Appendix B: Examples of Conditions

Condition Type	Policy Area	Content	Country	Year	Source
Prior Action	External Sector	Adopt in the Council of Ministers an overall strategy for the liberalization over the medium term of the cotton sector	Chad	1999	IMF, 1999
Quantitative Performance Criterion	External Debt	Maintain a deficit ceiling on new public debt of 23.5 billion USD	Turkey	2001	IMF, 2001
Structural Performance Criterion	Financial Sector	Merge four state banks into the newly established Bank Mandiri. Loan decisions and treasury management of these banks are to be placed under centralized control	Indonesia	1998	IMF, 1998
Indicative Target/ Benchmark	Fiscal Policy	Maintain ceiling on state budget primary spending of 55.8 billion Euros	Greece	2012	IMF, 2012
Structural Benchmark	Labor Issues	Finalize a review of public sector employment and remuneration that serves to inform policy reform	Jamaica	2013	IMF, 2013

Note: Condition types considered binding indicated in red.

Appendix C: Variable Names, Descriptions, and Sources

Name	Description	Source
Gini	Gini coefficient of disposable income on 0 to 100 scale	SWIID
TopQuinI	Income share, top quintile	WDI
BottomQuinI	Income share, bottom quintile	WDI
Program	Binary indicating presence of a program in a given year	Kentikelenis et al. (2016)
TotalCond	Count of total number of all conditions placed on a country in a given year	Kentikelenis et al. (2016)
TotalCond2	Count of total binding conditions placed on a country in a given year	Kentikelenis et al. (2016)
TotalCond3	Implementation-corrected count of conditions placed on a country in a given year which discounts TotalCond2 by the number of waivers a country received from the IMF for its binding conditions	Kentikelenis et al. (2016)
Concesssional	Binary indicating concessional program (either SAF, ESAF, or RPGF)	Kentikelenis et al. (2016)
SBA	Binary indicating presence of SBA agreement	Kentikelenis et al. (2016)
EFF	Binary indicating presence of EFF agreement	Kentikelenis et al. (2016)
TotalProgram	Number of Countries participating in IMF program in a given year	Kentikelenis et al. (2016)

IMFLiq_ln	IMF liquidity ratio measured as the amount of liquid IMF assets divided by its liquid liabilities	Lang (2016)
IMFprob	Approximated probability of participating in an IMF program in year t. Measured as average number of years under IMF program between 1980 and year t.	Own calculation based on Lang (2016)
Condprop	Approximated propensity for a condition to be placed on a given country in year t. Measured as average number of conditions between 1980 and year t.	Own calculation based on Forster et al. (2019)
Cond2prop	Approximated propensity for a binding condition to be placed on a given country in year t. Measured as average number of binding conditions between 1980 and year t.	Own calculation based on Forster et al. (2019)
Cond3prop	Approximated propensity for an implementation-corrected binding condition to be placed on a given country in year t. Measured as average number of implemented binding conditions between 1980 and year t.	Own calculation based on Forster et al. (2019)
PerCapitaGDP_ln	Natural logarithm of per capita GDP	WDI
Education	Education measured in mean years of schooling	Quality of Governance Database
Trade	Measured as total imports and exports as share of GDP	WDI
FDI	Measured as share of GDP	WDI
Inflation	Annual inflation rate	WDI
Unemployment	Unemployment rate as percentage of total labor force	WDI
LifeExpectancy	Life expectancy in years	WDI
Urban	Percentage of total population living in urban areas	WDI
growthGDP	annual percentage GDP growth	WDI
GovExp	General government final consumption expenditure as share of GDP	WDI
Reserves	Total reserves in months of imports	WDI
CAB	Current account balance as share of GDP	WDI
FinOpenness	Financial openness as measured by Chinn-Ito Financial Openness Index	Chinn and Ito
UNGAvoting	Ideal point of voting behavior in the UNGA	Bailey, Strezhnev, and Voeten (2017)

SystemicBank	Binary indicating presence of systemic banking crisis	Laeven and Valencia
DemocracyIndex	Index level of democracy measured as Freedom House score divided by Imputed Polity Score	Quality of Governance Database
GovOr	Government political orientation measured as (1) Right; (2) Center; (3) Left	Cruz et al. (2016)
LegEle	Binary indicating whether a legislative election was held in a given year	Cruz et al. (2016)
ExeEle	Binary indicating whether an executive election was held in a given year	Cruz et al. (2016)

Appendix D: Summary Statistics

VARIABLES	N	Mean	Std. Dev	Min	Max
Gini	2,721	41.84	7.820	19.84	66.46
TopQuinI	977	48.53	7.911	32.40	71
BottomQuinI	977	5.795	2.226	0.800	11.40
TotalCond	4,415	12.16	19.77	0	148
TotalCond2	4,415	8.257	14.07	0	124
TotalCond3	3,611	8.462	13.96	0	114
Program	4,690	0.345	0.475	0	1
Concessional	4,690	0.176	0.381	0	1
SBA	4,690	0.150	0.357	0	1
EFF	4,690	0.0441	0.205	0	1
TotalProgram	4,690	46.26	11.07	27	67
IMFLiq_ln	4,690	5.351	0.741	4.100	7.109
IMFprob	4,690	0.323	0.298	0	1
Condprop	4,690	8.494	8.577	0	40.72
Cond2prop	4,690	6.251	6.272	0	30
Cond3prop	4,690	5.906	5.944	0	28.5
PerCapitaGDP_ln	4,171	7.678	1.170	4.898	10.51
Education	3,223	6.191	2.992	0.045	13.16
Trade	3,913	74.57	39.01	0.021	375.4
FDI	3,964	3.103	6.397	-55.23	161.8
Inflation	4,160	50.08	456.8	-31.57	15,444
Unemployment	3,519	8.410	6.592	0.180	37.94
LifeExpectancy	4,690	63.38	9.566	26.17	81.72
Urban	4,690	46.65	21.24	4.339	94.94
growthGDP	4,161	3.900	7.089	-64.05	150.0

GovExp	3,734	14.94	6.205	0.911	69.54
Reserves	3,381	4.296	4.459	0.0022	79.24
CAB	3,603	-3.798	9.613	-148.0	62.30
FinOpenness	3,501	0.427	0.317	0.060	1
SystemicBank	4,690	0.024	0.153	0	1
DemocracyIndex	4,267	4.933	3.101	0	10
GovOr	4,628	1.121	1.305	0	3
LegEle	4,630	0.185	0.389	0	1
ExeEle	4,630	0.110	0.313	0	1

See **Appendix C** for descriptions of all variables

Appendix E: Endogeneity-Corrected Top and Bottom Income Quintile Regressions

VARIABLES	Income Share for Top Quintile	Income Share for Bottom Quintile
L.Program	0.238	-0.208
	(0.841)	(0.231)
L.ln(Per Capita GDP)	2.765***	-0.343
	(0.895)	(0.246)
L.Education	-0.115	-0.0717
	(0.237)	(0.0650)
L.Trade	-0.00154	-0.00241
	(0.00726)	(0.00199)
L.FDI	-0.0165	0.0123**
	(0.0224)	(0.00614)
L.Inflation	-0.000580	0.000843*
	(0.00162)	(0.000444)
L.Unemployment	0.122**	-0.0468***
	(0.0512)	(0.0141)
L.Life Expectancy	0.0994	-0.0374
	(0.0838)	(0.0230)
L.Urban	0.177***	-0.0330**
	(0.0480)	(0.0132)
L.GDP Growth	-0.00352	-0.00708
	(0.0296)	(0.00813)
L.Gov. Expenditure	0.0935*	-0.0368***
	(0.0520)	(0.0143)
L.Reserves	0.0448	-0.0324*
	(0.0626)	(0.0172)
L.CAB	-0.0386	0.00586
	(0.0236)	(0.00648)
L.Financial Openness	0.828	0.142
	(0.578)	(0.159)
L.Sys. Bank Crisis	0.0320	0.259*
	(0.564)	(0.155)
L.Democracy Index	0.163	-0.0109
	(0.107)	(0.0294)
L.Gov. Orientation	-0.150	-0.0138
	(0.122)	(0.0336)
L.Leg. Election	-0.0320	0.0448

	(0.235)	(0.0646)
L.Exe. Election	-0.0896	0.0129
	(0.272)	(0.0746)
Constant	2.079	15.90***
	(9.866)	(2.709)
Country Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Program F-Statistic	15.72	15.72
Observations	710	710

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix F: Endogeneity-Corrected Model by Program Type

Dependent Variable	Gini Coefficient		
Specification	Concessional	EFF	SBA
Program Type			
L.Concessional	-4.109*		
	(2.422)		
L.EFF		-7.698***	
		(2.880)	
L.SBA			-1.900*
			(1.040)
L.Total Conditions × Program Type	0.0553	0.0477	-0.000892
	(0.0347)	(0.0333)	(0.0331)
L.ln(Per Capita GDP)	3.616***	3.452***	3.254***
	(0.347)	(0.466)	(0.343)
L.Education	0.0801	0.0491	-0.00369
	(0.101)	(0.130)	(0.104)
L.Trade	-0.00692**	-0.00997**	-0.00503*
	(0.00331)	(0.00409)	(0.00258)
L.FDI	-0.00680	-0.00800	-0.0174**
	(0.00978)	(0.0118)	(0.00857)
L.Inflation	-0.000319	-0.000973	0.000390
	(0.000629)	(0.000893)	(0.000763)
L.Unemployment	0.0931***	0.153***	0.118***
	(0.0193)	(0.0346)	(0.0213)
L.Life Expectancy	-0.0312	0.0875**	0.0367
	(0.0404)	(0.0422)	(0.0292)
L.Urban	0.0233	0.0727***	0.0603***
	(0.0182)	(0.0255)	(0.0188)
L.GDP Growth	-0.0323***	-0.0441***	-0.0511***
	(0.0110)	(0.0166)	(0.0127)
L.Gov. Expenditure	-0.0109	0.0304	-0.0115
	(0.0209)	(0.0318)	(0.0186)
L.Reserves	-0.0145	0.00665	-0.0144
	(0.0228)	(0.0272)	(0.0214)
L.CAB	-0.0320***	-0.0506***	-0.0358***
	(0.00867)	(0.0116)	(0.00820)
L.Financial Openness	-0.0334	-0.0371	-0.214
	(0.219)	(0.301)	(0.239)

L.Sys. Bank Crisis	-0.0203	0.0613	0.230
	(0.228)	(0.313)	(0.250)
L.Democracy Index	0.0881**	0.0319	0.111***
	(0.0401)	(0.0526)	(0.0409)
L.Gov. Orientation	-0.190***	-0.216***	-0.302***
	(0.0539)	(0.0681)	(0.0581)
L.Leg. Election	-0.0126	-0.0526	-0.0327
	(0.0979)	(0.131)	(0.0976)
L.Exe. Election	0.0164	0.00986	0.0386
	(0.120)	(0.163)	(0.128)
Constant	16.06***	7.763*	13.23***
	(3.151)	(4.523)	(2.930)
Country Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Program F-Statistic	7.17	2.79	6.77
Total Conditions F-Statistic	9.47	3.03	4.52
Observations	1,497	1,497	1,497

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: The interaction between Total Conditions and the Program Type ensures that any measured effect of conditionality on Gini coefficients is linked to the particular type of program each specification isolates and not conditions from other types of programs.