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Poverty, Inequality, and the International Monetary Fund: How Austerity Hurts the Poor and Widens Inequality

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Abstract: Among the drivers of socio-economic development, this article focuses on an important yet insufficiently understood international-level determinant: the spread of austerity policies to the developing world by the International Monetary Fund (IMF). In offering loans to developing countries in exchange for policy reforms, the IMF typically sets the fiscal parameters within which development occurs. Using an original dataset of IMF-mandated austerity targets, we examine how policy reforms prescribed in IMF programs affect inequality and poverty. Our empirical analyses span a panel of up to 79 countries for the period 2002–2018. Using instrumentation techniques, we control for the possibility that these relationships are driven by the IMF imposing harsher austerity measures precisely in countries with more problematic economies. Our findings show that stricter austerity is associated with greater income inequality for up to two years, and that this effect is driven by concentrating income to the top 10% of earners while all other deciles lose out. We also find that stricter austerity is associated with higher poverty headcounts and poverty gaps. Taken together, our findings suggest that the IMF neglects the multiple ways its own policy advice contributed to social inequity in the developing world.

Keywords: poverty, inequality, International Monetary Fund, austerity

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1 Introduction

The ongoing COVID-19 crisis has thrust millions into poverty and exacerbated already wide inequalities around the world. To assist countries in dealing with the fallout of the pandemic, global economic governance organizations have sprung into action. Both the International Monetary Fund (IMF) and the World Bank have approved a large number of new programs—albeit far below estimated need (Stubbs et al. 2021)—and are expected to scale up their lending over 2021. In particular, the IMF—the world’s guardian of balanced budgets and debt service—has even given a green light to low- and middle-income countries opening the public coffers in order to handle the economic fallout of the pandemic. In April 2020, IMF Managing Director Kristalina Georgieva encouragingly told developing countries to “spend as much as you can, but keep the receipts” (IMF 2020b). At the end of the year, she clarified that these emergency-spending receipts “cannot be stacked in a drawer and forgotten. They should be tracked, publicized, and audited” (IMF 2020c). These comments point to growing fears of a coming austerity shock: the IMF has already advised countries to restart fiscal consolidation in 2021 (Gallagher 2020; Munevar 2020), and—through its loan-for-reform programs—it can ensure that this takes place. A rapid, radical, and premature return to austerity could further worsen poverty and inequality.

Given the centrality of the IMF in guiding economic recovery in developing countries, it is worth revisiting its record on poverty and inequality: how have they been impacted by IMF lending programs and their mandated policy reforms? This has been a topic of sustained controversy. In recent years, the IMF has styled itself as a champion of meeting Sustainable Development Goals pertaining to reducing poverty and inequalities (IMF 2020a). This self-promoted profile builds on the organization’s reputation following the publication of high-impact research on the determinants and consequences of inequality over the 2010s (Dabla-Norris et al. 2015; Fabrizio et al. 2015, 2017; IMF 2014; Ostry and Berg 2014), and even a volte-face on the merits of pursuing neoliberal reforms: as senior officials from the IMF’s research department summarized, “instead of delivering growth, some neoliberal policies have increased inequality, in turn jeopardizing durable expansion” (Ostry, Loungani, and Furceri 2016, p. 38).

This rebranding of the IMF as an inequality-combating champion was a sharp departure from the reputation of the organization across developing countries. Both scholars and civil society have long highlighted the adverse social consequences of IMF-mandated reforms (e.g. Cornia, Jolly, and Stewart 1987; Pastor 1987). Invariably, this work pointed to IMF-mandated austerity as a key culprit. Under its tutelage, countries had to institute drastic reductions in public spending, which directly and disproportionately impacted the poor and the vulnerable.

We revisit these controversies, and innovate by examining the impact of the scale of mandated fiscal consolidation in IMF programs on poverty and inequality using a panel of up to 79 countries between 2002 and 2018. Leveraging novel data on IMF fiscal conditionality (Ray and Gallagher 2020), we find that stricter austerity targets are associated with increases in income inequality for up to two years. Further analyses reveal that this effect is driven by the concentration of income into the top 10% of earners, while all other income deciles lose out. In addition, we find that stricter austerity targets are associated with increases in both the share of the population living in poverty and the average distance the poor are from the poverty line. By using Heckman estimation techniques throughout, we also account for the possibility that these relationships are driven by the IMF imposing harsher austerity measures precisely in countries prone to socio-economic turmoil. Therefore, our results can be interpreted as causal.

The article is structured as follows. First, we describe recent debates on the social consequences of IMF programs, and explore the mechanisms via which austerity can exert influence on inequality and poverty. Second, we describe the data employed and our adopted identification strategy. Third, we present the results of our quantitative analysis. In the final section, we contextualize these findings and identify some limitations, policy implications, and directions for future research.

2 Poverty, Inequality, and the Role of the IMF

The controversies surrounding the impact of IMF programs on poverty and inequality have persisted over time. A key role in these debates is accorded to the role of fiscal consolidation policies, more simply known as “austerity.” This refers to measures to secure debt service and reduce budget imbalances—commonly achieved through a mix of cuts in public spending and increases in taxation. While the IMF accepts that these policies require tough choices by politicians, it considers them nonetheless essential. As former IMF Managing Director Dominique Strauss-Khan explained, “countries only need IMF resources when they are ‘sick’—when they face serious balance of payments problems requiring policy adjustment. If you go to the doctor with a liver problem, the doctor will treat you, yes, but will also insist that you stop drinking. So policy conditions are necessary” (Atkinson 2009).

The IMF claims vulnerable populations are sheltered from austerity via “measures to increase spending on, and improve the targeting of, social safety net programs” (IMF 2015). These predominantly take the form of social spending minima on health and education, a cornerstone in the IMF’s purported attention to

the social consequences of austerity (Clegg 2014). Indeed, recent studies by IMF staff find their programs are associated with increases in social spending (Clements, Gupta, and Nozaki 2013), and that social spending floors are “helpful in ensuring adequate allocations for poverty ... in the short term in an environment of tight budgetary position” (Gupta, Schena, and Yousefi 2020, p. 6351).

However, the track record of IMF-mandated austerity has not lived up to its promise. Social spending floors are only implemented around half the time and have not protected social spending from austerity measures (Stubbs and Kentikelenis 2018). In addition, most scholarship on the impact of IMF programs on poverty and inequality—summarized in Table 1—reveals adverse effects that persist over the medium term. Most recently, Lang (2021) documented causally that increases in inequality due to IMF programs result both from relative and absolute losses of income by the poor. Only one study finds no effects, and—in some specifications—inequality and poverty-reducing effects for IMF interventions (Bird, Qayum, and Rowlands 2020). However, they use propensity score matching methods, which—among other issues—are not able to account for selection bias due to unobservable factors like political will (Bas and Stone 2014; Stubbs et al. 2020; Vreeland 2003).

How does the purported impact of austerity measures on poverty and inequality manifest? The mechanisms can be direct or indirect. Direct pathways refer to effects on individuals’ incomes and livelihoods. Stark reductions in government spending lead to contractions in economic activity, which have follow-on implications for employment levels and salaries. This debate has been raging for years within and outside the IMF in relation to the so-called “fiscal multipliers”—that is, estimates of changes in government spending or tax revenues on the level of GDP (Batini et al. 2014). A persistent criticism has been that—depending on the method used—multipliers are sometimes estimated as being too low (Blanchard and Leigh 2014). As a result, IMF projections can show that austerity measures are unlikely to have adverse effects on economic activity. The implications of wrong estimates can be devastating. For instance, in the case of IMF lending to Greece in the early 2010s, the organization severely miscalculated the effect of government spending cuts on the economy, plunging the country into a deep recession (Wyplosz and Sgherri 2016). Further, austerity measures are also accompanied by cuts in the number and wages of public sector personnel—another direct effect on the disposable income of those holding such jobs.

Increased taxes also impact levels of economic activity and individual income and wealth. IMF programs are associated with increases in value-added taxes (Reinsberg, Stubbs, and Kentikelenis 2020), which place a greater burden on poorer households. Instead, opting for value-added taxes may mean that alternative forms of taxation—like income and corporate taxes—are not pursued; this

Table 1: Empirical evidence on the relationship between IMF programs, poverty and inequality.

	Span	Countries	Sample composition	Method	Dependent variable	Results: IMF programs associated with ...
Pastor (1987)	1965–1981	18	Latin American countries	Yearly absolute and relative comparisons	Labor share of income	Absolute and relative reductions in labor share of income
Garuda (2000)	1975–1991	39	Low- and middle-income countries	Propensity score estimation	Gini coefficient and income of the poorest quintile	Adverse effects on poverty and inequality
Vreeland (2002)	1961–1993	110	All countries	Heckman-corrected regression	Labor share of income from manufacturing	Reductions in labor share of income
Easterly (2003)	1980–1998	65	Low- and middle-income countries	Ordinary and two-stage least-squares regression	Poverty spells	Poor benefit less from economic expansions during a program compared to economic expansions without a program, but they are also hurt less by contractions
Oberdabernig (2013)	1982–2009	86	Low- and middle-income countries	Treatment effect regressions and model averaging	Various poverty indicators and inequality indices	Adverse short-run effects on poverty and inequality, while for a 2000–09 subsample the results are reversed for inequality
Forster et al. (2019)	1980–2014	135	Low- and middle-income countries	Two-stage least-squares regression	Gini coefficient of disposable income	Increases in inequality after an IMF program, and these effects persist in the medium term
Bird, Qayum, and Rowlands (2020)	1990–2015	48	Low- and middle-income countries	Propensity score estimation	Various poverty indicators and inequality indices	No significant association with poverty and inequality
Lang (2021)	1973–2013	155	Low- and middle-income countries	Two-stage least-squares regression	Gini coefficient of net income	Increases in inequality for up to five years

favours business interests and can contribute to improved economic fortunes of the wealthy (Bird and Gendron 2007; Emran and Stiglitz 2005; Stewart 2016; Stiglitz 2010).

Turning to indirect mechanisms, these pertain to the impact of austerity on the availability of social protection policies, which can help cushion shocks to livelihoods. Closures of social services and reductions of staff, and the discontinuation of or cuts in social assistance programs can all lead to social groups having inadequate support at a time of heightened need (Stubbs et al. 2017). While these policies certainly affect the poor, individuals higher up on the income distribution are not immune. For instance, changes to social assistance programs might not be relevant to poor people in the informal sector, but they will impact the ability of those with lost formal sector jobs to maintain their livelihoods.

3 Data and Methods

3.1 Variables

We investigate the effects of IMF austerity on several inequality and poverty measures for countries between 2002 and 2018, controlling for known confounders. Data sources and summary statistics for all variables are reported in the Web Appendix (Table A1). As discussed earlier, our expectation is that IMF programs that require a greater fiscal adjustment and include more conditions will result in greater increases in poverty and inequality. Previous studies model such fiscal adjustment as homogenous, using either an IMF program participation dummy or a count of the number of fiscal conditions. Allowing for such effect heterogeneity, we employ two IMF measures to isolate the effect of austerity.

For our main explanatory variable, we use a new dataset on IMF fiscal conditionality measuring the intensity of fiscal adjustment required by countries participating in IMF programs (Ray and Gallagher 2020). The IMF fiscal adjustment indicator measures implied changes in the fiscal balance incorporated in so-called “Quantitative Performance Criteria” (QPC) on headline fiscal deficit targets. QPCs are binding, such that failure to implement them results in suspension of the program (Kentikelenis, Stubbs, and King 2016).¹ For each QPC, the fiscal target is measured as a share of the borrower’s gross domestic product (GDP) and then compared to the baseline level from the calendar year prior to the signing of the

¹ The IMF frequently revises QPC targets, so the dataset records the fiscal balance target value that actually applied on the assessment date. The dataset also omits fiscal balance targets that were granted a waiver.

program. The IMF fiscal adjustment indicator is then calculated as the cumulative, annualized change in government fiscal balances between the baseline and the target, expressed in percentage points of GDP per year increase. An increase in a surplus or decrease in a deficit is shown as a positive value (i.e. more austerity), whereas a decrease in the surplus or increase in the deficit is shown as a negative value (i.e. less austerity).

A limitation of the dataset is that it *only* captures binding fiscal targets set for end-December, thereby omitting fiscal adjustment in country-years where binding targets are set for end-March, end-June, or end-September but *not* for end-December. This is an unavoidable consequence of measuring the intensity of fiscal adjustment as a share of the borrowers' annual GDP, as there are no mid-year comparison points for annualizing. As a result, our sample size is limited and further steps to account for measurement error are necessary. In addition, the dataset does not include QPCs that are one step removed from the fiscal balance, such as limits on credit to the government. These forms of measurement error necessitate additional steps to our identification strategy, described further below. In total, the dataset contains a maximum of 355 observations across 79 countries between 2002 and 2018.² To ensure results are not unduly impacted by outliers, we exclude nine observations that are more than three standard deviations from the mean.³

We also include a measure for the total number of IMF conditions in a given country-year, based on a newly updated version of the IMF Monitor's conditionality dataset (Kentikelenis, Stubbs, and King 2016). We only count binding conditions, following established procedures in this field of study (Stubbs et al. 2017). If jointly included with the fiscal conditionality measure, the coefficient estimate will capture all remaining aspects of conditionality. It therefore allows us to empirically isolate the effect of fiscal adjustment from other IMF-mandated policy reforms.

Our main measure of inequality is the Gini coefficient of disposable (post-tax, post-transfer) income reported by the Standardized World Income Inequality Database (SWIID) (Solt 2020). SWIID is among the most widely used in studies on inequality in developing countries (e.g. Afesorghor and Mahadevan 2016; Bergh

² While the dataset includes some observations for the IMF fiscal adjustment indicator in 2001, we begin from 2002 because it is the first year of complete coverage. Total observations in regression analyses are fewer than the value reported here due to missing data on dependent and control variables.

³ Outliers are as follows: Armenia in 2009 (−0.0965), Antigua and Barbuda in 2010 (0.1680) and 2011 (0.1780), Burkina Faso in 2010 (−0.1116), Croatia in 2004 (−0.0930), Iraq in 2005 (0.2880) and 2006 (0.1557), Iceland in 2009 (−0.0954), and the Maldives in 2009 (−0.1111). In robustness checks, we add outliers to the analyses.

and Nilsson 2010; Dorsch and Maarek 2019; Forster et al. 2019; Kerrissey 2015; Oberdabernig 2013; Pleninger and Sturm 2020), and provides more extensive coverage than other established sources (e.g. Deininger and Squire 1998; Milanovic 2019). A drawback to using the SWIID is that it relies on imputations to fill in missing data points, rather than using observed data points only. Jenkins (2015) in particular questions the underlying assumptions used to derive the estimates and calls for more transparency in the imputation process. More recent versions of the dataset have partially addressed these criticisms and provide additional information on how imputations are derived (Solt 2020).

In addition to using the Gini coefficient, we take advantage of underutilized data on income decile shares from the Global Consumption and Income Project (GCIP), in order to locate where changes in the income distribution occur. The income decile share is defined as the proportion of a country's total income held by a particular income decile in a given year. For example, for South Africa in 2015, decile one—the sum of incomes of the bottom 10% of population—had 0.75% of the country's total income, decile five had 3.33%, and decile 10 had 54.57%. GCIP data combine several sources to generate extensive time-series cross-sectional income data for all 10 deciles (Lahoti, Jayadev, and Reddy 2016). As with the SWIID, this dataset also relies on imputations to derive estimates.

Following previous research on inequality, we include a set of control variables for economic, political, and demographic factors that could plausibly affect the income Gini or decile shares: the natural logarithm of GDP per capita and its quadratic, because inequality is expected to rise in early stages of economic development and then decline in later stages (Afesorgbor and Mahadevan 2016; Dreher and Gaston 2008), following an inverted “U” shape in relation to increases in GDP per capita that the inclusion of the squared term allows us to model (Kuznets 1955); average years of schooling, since more people with higher education implies that a larger share of the population will enjoy a wage premium (Bergh and Nilsson 2010; Jaumotte, Lall, and Papageorgiou 2013; Meschi and Vivarelli 2009; Woo et al. 2013); trade openness measured as exports plus imports as a share of GDP, because countries may weaken labor market policies and lower taxes—thereby reducing resources for social programs—in a race-to-the-bottom to improve global competitiveness (Dreher and Gaston 2008; Meschi and Vivarelli 2009); life expectancy, which could be either positively or negatively associated with inequality as previous studies find mixed results (Forster et al. 2019; Lang 2021; Oberdabernig 2013); and levels of democracy, since democratic governments are more inclined to help lower and middle classes with progressive taxes, minimum-wage laws, price subsidies, and public works provision, thereby having more equitable income distributions (Afesorgbor and Mahadevan 2016; Dreher and

Gaston 2008; Reuveny and Li 2003).⁴ As current levels of inequality are heavily dependent on previous levels, we also include a lagged dependent variable (Lang 2021; Meschi and Vivarelli 2009; Oberdabernig 2013; Plening and Sturm 2020); the econometric rationale for doing so is to mitigate serial error correlation beyond the computation of clustered standard errors (Beck and Katz 2011). In addition, the inclusion of country fixed effects account for time-invariant country-level characteristics, and year fixed effects control for common external shocks across all countries.⁵

For poverty, our main dependent variables are headcount ratios at various dollar-a-day values as a share of the population: the well-established \$1.90 and \$3.20 indicators from the World Bank's (2020) World Development Indicators dataset; and the \$1.44, \$1.86, and \$2.50 measures from GCIP. The latter offer the advantage of greater data coverage, but are derived from imputations. We also use World Bank data on the poverty gap—how far, on average, the poor are from the poverty line—at \$1.90 and \$3.20 a day.

We include a standard set of controls in the analyses on poverty: GDP per capita (logged) and GDP growth, because better economic circumstances are expected to lift people out of poverty (Adams 2004; Easterly and Fischer 2001; Oberdabernig 2013; Ravallion and Chen 1997); the income Gini and its interaction with GDP growth, as inequality exercises downward pressure on the extent to which growth benefits the poor (Beck, Demirgüç-Kunt, and Levine 2007; Dabla-Norris et al. 2015; Easterly 2003; Mosley, Hudson, and Verschoor 2004), since growth in a high-inequality context is less likely to benefit the poor than in a low-inequality context; and a corruption perception index, since corruption can reduce the share of government spending that reaches the poor (Hajro and Joyce 2009; Mosley, Hudson, and Verschoor 2004). We also include country and year fixed effects. Unlike inequality, poverty rates are not heavily path dependent, so there is no need for a lagged dependent variable.⁶ Omitted from our list of controls is

4 Additional control variables for inequality analyses are included in robustness checks in Section 4.

5 We are aware that the inclusion of a lagged dependent variable in the presence of fixed effects can produce biased estimates (Nickell 1981). And in our multiple-equation setup, we are unable to use the bias-corrected Anderson-Hsiao estimator for unbalanced dynamic panel data (Bruno 2005). Nevertheless, this bias concentrates in the lagged dependent variable coefficient, which is not of substantive interest to us; and since our data covers up to 16 years, any bias is likely to be negligible (Beck and Katz 2011; Nunn and Qian 2014). Regardless, findings are generally robust to excluding the lagged dependent variable for the income Gini and deciles, though at lower levels of statistical significance. These results are available in the replication code.

6 A cursory examination of the data shows poverty is considerably less path dependent than income inequality. Specifically, the within-country standard deviation for the poverty headcount ratio at \$1.90 is 6.887, whereas the respective statistic for the income Gini is 1.594, despite both variables being measured on a 0–100% scale.

government social spending (Mosley, Hudson, and Verschoor 2004), as it is a key channel by which IMF fiscal adjustment is hypothesized to influence poverty; by including it, we would block this pathway, giving rise to post-treatment bias (Angrist and Pischke 2008).⁷

3.2 Estimation Techniques

We estimate inequality and poverty equations separately with Ordinary Least-Squares (OLS) regression, set-out formally as follows:

$$\text{INQ}_{it} = \alpha_1 \text{INQ}_{it-1} + \alpha_2 \text{IMFADJ}_{it-1} + \alpha_3 \text{IMFCOND}_{it-1} + \alpha_4 X_{it-1} + \mu_i + \delta_t + u_{it} \quad (1)$$

$$\text{POV}_{it} = \beta_1 \text{IMFADJ}_{it} + \beta_2 \text{IMFCOND}_{it} + \beta_3 Y_{it} + \mu_i + \delta_t + v_{it} \quad (2)$$

where INQ and POV are the respective measures of inequality and poverty, i is the country, t is the year, IMFADJ is the IMF fiscal adjustment indicator, IMFCOND is the number of IMF conditions, μ_i is a set of country dummies and δ_t a set of year dummies, X and Y are vectors of control variables for inequality and poverty, α and β are the vectors of coefficients, and u and v the error terms. For inequality models, a lagged dependent variable is included and all other variables are lagged one year, following previous studies (Lang 2021; Meschi and Vivarelli 2009; Oberdabernig 2013; Pleninger and Sturm 2020); we also test on deeper lag structures for explanatory variables in additional regressions—at $t-2$ and $t-3$ —as research shows some effects unfold only after a substantive period of time has elapsed (Lang 2021).

With regard to identification strategy, a key issue we face is that our IMF fiscal adjustment indicator does not capture targets set outside of end-December or that have fiscal implications that are one step removed from the budget balance. To account for this form of measurement error, we adopt two interlinked strategies: first, we restrict analyses only to country-years with a fiscal deficit condition in end-December; and, second, we perform a Heckman (1979) correction to account for non-random assignment into the sample.

Restricting analyses to observations with fiscal adjustment in end-December means that we capture a conditioned effect, or average treatment effect on the treated (ATET). This strategy is well-established in research investigating the effects of IMF conditionality at large, where analyses are confined to country-years featuring IMF program participation (e.g. Beazer and Woo 2016; Casper 2017; Rickard and Caraway 2019). In such an econometric setting, we can then make

⁷ Additional control variables for poverty analyses—including government social expenditures—are included in robustness checks in Section 4.

claims about the kind of socio-economic outcomes that a country under an IMF program *with a different level of fiscal adjustment* might experience. However, a shortcoming of this approach is that results can *only* be interpreted within the context of country-years requiring fiscal adjustment.

We also face methodological challenges to identifying the ATET of IMF fiscal adjustment due to non-random assignment into the sample. There exist multiple sources of potential bias. On the one hand, measurement error on the adjustment variable would introduce bias if it were systematically correlated with the outcome, although we find this possibility unlikely. For example, if countries that are more likely to experience higher (or lower) poverty or inequality are also more likely to have headline fiscal deficit conditions for either March, June, or September, and not for December, then our sample would be biased toward a subset of stronger (or weaker) performers. Similarly, if the IMF is more (or less) likely to assign headline fiscal conditions to poor performers, instead allocated conditions with indirect fiscal implications, then we would again have a biased sample of stronger (or weaker) performers. On the other hand, the circumstances of countries receiving more severe IMF fiscal adjustment may be systematically different from those receiving more lenient adjustment, and these underlying differences may in turn affect inequality or poverty. While we can—and do—control for known observable factors, it may be that some of them are unknown or inherently unobservable, such as a country's political will to implement adjustment (Vreeland 2003). Failure to account for factors that codetermine fiscal adjustment and inequality or poverty would result in a biased estimate of the effect of fiscal adjustment.

To deal with these endogeneity challenges, we employ a standard Heckman two-step correction. This technique is suitable when the outcome equation is limited to observations *only* where the country has selected into the treatment (Stubbs et al. 2020), in our case a headline condition on fiscal adjustment for end-December. It corrects for endogeneity bias by treating non-random assignment of countries into the treatment as an omitted variable problem (Heckman 1979; Wooldridge 2010). In effect, the omitted variable is a catch-all term that captures the qualities that make the entity prone to selection into the treatment, including on unobservable variables. The approach entails initially estimating a probit model to predict a country's selection into the sample of observations with IMF fiscal adjustment values:

$$\Pr(|\text{IMFADJ}|_{it} > 0) = F(\gamma_1 W_{it} + \gamma_2 Z_{it} + \delta_t + \epsilon_{it}) \quad (3)$$

where IMFADJ denotes the absolute value of our IMF fiscal adjustment indicator, i is the country, t is the year, $F(\dots)$ is the cumulative distribution function, W is a vector of control variables from the outcome equation for either inequality or

poverty, Z is an excludable instrument that influences selection into IMF fiscal adjustment but not inequality or poverty, δ_t is a set of year dummies, γ_1 and γ_2 are the respective vectors of coefficients on the controls, and ε is the error term.⁸ We are unable to introduce country dummies due to the well-known incidental parameter bias found in limited dependent variable models (Greene 2004).

We then compute the inverse-Mills ratio or hazard, λ , for each observation in the sample:

$$\hat{\lambda}_{it} = \frac{\varphi((W_{it} + Z_{it})\hat{\psi})}{\Phi((W_{it} + Z_{it})\hat{\psi})} \quad (4)$$

where φ denotes the standard normal density function, Φ the standard normal cumulative distribution function, and $\hat{\lambda}$ is an estimated value taken from Equation (3). The inverse-Mills ratio is then added to our set of controls for inequality and poverty in Equations (1) and (2).

For selection into IMF fiscal adjustment, we incorporate two excludable instruments based on insights from the established literature on IMF program participation. A valid instrument ought to explain whether or not a country has an IMF fiscal adjustment condition (the relevance criterion), but must not be correlated with income inequality or poverty except through fiscal adjustment (the exclusion criterion).

First, we use United Nations General Assembly (UNGA) voting distance with the United States. This instrument is among the most widely adopted in IMF effects studies (Barro and Lee 2005; Steinwand and Stone 2008; Stubbs et al. 2020; Thacker 1999). The argument for its relevance in our context is that, all else equal, countries that vote similarly to the US are less likely to contain a fiscal adjustment condition. Seminal research by Strom Thacker (1999) underpins this claim. His study showed that shifting UN voting pattern alignment toward the US increases a country's chances of receiving a loan from the IMF, reasoning that the US government pressures the IMF to approve loans on favorable terms to politically friendly countries. The potential for UNGA voting similarity to be used as an instrument in IMF effects research was then realized in a pioneering study by Barro and Lee (2005) on the impact of IMF program participation on economic growth. To fulfill the exclusion criterion in our context, UNGA voting similarity with the US must also not affect poverty or inequality except via IMF fiscal adjustment. While this claim is inherently untestable, it is a plausible assumption. However, doubts have been cast on whether the Local Average Treatment Effect (LATE) of the

⁸ For inequality, explanatory variables for IMF fiscal adjustment enter lagged one year (at $t-1$), whereas for poverty they enter contemporaneously (at t), as is consistent with respective lag structures in the outcome equations.

instrument is representative of all IMF programs, not just politically motivated ones (Dreher, Eichenauer, and Gehring 2018).⁹ For this reason, we incorporate a second instrument.

Drawing on recent methodological innovations in political science, we construct a compound instrument for selection into IMF fiscal adjustment (Forster et al. 2019; Lang 2021; Nunn and Qian 2014; Reinsberg et al. 2019; Stubbs et al. 2020). This entails interacting the mean country-specific fiscal adjustment indicator—an endogenous variable—with the IMF's budget constraint—a plausibly exogenous variable. The mean country-specific fiscal adjustment is calculated by summing all IMF fiscal adjustment values for a given country, then dividing it by the total number of adjustment observations. Its value is therefore constant across years within any given country, but different for each country. The IMF's budget constraint is approximated by the natural log of its liquidity ratio (Lang 2021). Its value is different for each year but is constant across countries in any given year. This econometric strategy of interacting an endogenous variable (i.e. country-specific exposure to IMF fiscal adjustment) with an exogenous one (i.e. IMF liquidity ratio) to form an exogenous compound instrument is supported by analytical proofs (Bun and Harrison 2019; Nizalova and Murtazashvili 2016).

In terms of instrument relevance of the IMF compound instrument, the cross-sectional average of fiscal adjustment approximates the general propensity of a country to obtain a specific amount of adjustment in any given year. Furthermore, research shows that, on average, the IMF increases the stringency of conditionality when country demand for loans is strong, and reduces stringency when country demand for loans is weak (Chapman et al. 2017; Dreher and Vaubel 2004). The rationale for this relationship is that as the IMF assists more countries, resource scarcity—measured here as IMF liquidity—prompts them to give harsher adjustment to any given country as a safeguard measure for loan repayments (Dreher and Vaubel 2004; Vreeland 2003). The inverse also holds: the IMF is more generous with its loans when it has high liquidity in order to maximize revenues from interest payments and maintain a position of global power (Babb and Buira 2005; Barnett and Finnemore 2004; Dreher and Vaubel 2004). This implies less fiscal adjustment in times of resource abundance as the IMF tries to entice borrowers into programs.

In terms of instrument excludability, our explanation follows a similar logic as Lang's (2021) IMF participation instrument vis-à-vis the exogenous variation of the budget constraint. The instrument fulfills the exclusion criterion because country-specific changes in fiscal adjustment that deviate from its long-run average are

9 Results are robust to excluding the UNGA United States voting distance instrument.

brought about only by decisions of the IMF that do not pertain to any given country. Some of these decisions include the introduction of social spending floors in the late-1990s or the conditionality streamlining initiative of the early-2000s (IMF 2001). One might also be concerned about potential direct effects of the general propensity of a country to obtain a specific amount of fiscal adjustment in any given year on the outcome of interest. We control for this effect through the inclusion of country fixed effects in the outcome equations. There could also be a question on the excludability of the liquidity variable insofar as wealthy member countries can replenish IMF resources in response to a greater number of countries participating in programs, which would diminish the Fund's risk aversion such that the organization is willing to agree to more lenient adjustment when bargaining a new program with a recipient country (Dreher and Vaubel 2004). This logic is flawed because the financial resources that members commit to the IMF's General Resource Account are predetermined via five-yearly quota reviews; and fluctuations in voluntary contributions to the IMF's concessionary lending budget are accounted for by the inclusion of year fixed effects in outcome equations. The instrument is therefore excludable to the extent that variables correlated with IMF fiscal adjustment do not affect inequality differently in low- versus high-exposure countries, conditional on controls.

Using a compound instrument is akin to a continuous difference-in-difference design—the impact of fiscal adjustment on income inequality and poverty is compared between country-years with high and low-exposure—and, as such, must fulfill additional assumptions concerning parallel and non-overlapping trends (Christian 2017), assessed in the Web Appendix (Figures A1 and A2).¹⁰

In addition to our two instruments, we include a standard set of economic and political determinants specific to selection into IMF programs: current account

10 Non-parallel trends across groups with different exposure to the country-varying component of the interacted instrument can introduce statistical bias. In our case, trends over time in the average IMF fiscal adjustment—our “exposure” variable—should be similar across groups of countries with above-mean exposure and groups of countries with below-mean exposure. In addition, statistical bias would also be introduced if there was a non-linear trend in the time-varying component of the interacted instrument that is similar to the respective trends in the exposure and outcome variables in the high-exposure group of countries. In our application, we would be concerned about similarly shaped non-linear trends in IMF liquidity, the average IMF fiscal adjustment, and our outcomes of interest if these trends only occur among high-exposure countries but not low-exposure countries. In Figures A1 and A2, we find in all outcomes that both exposure groups are similar in terms of their trending patterns. There is also no trend similarity between IMF liquidity and the average fiscal adjustment, or between IMF liquidity and any of the outcomes, among countries exposed to above-average fiscal adjustment.

balance as a share of GDP, foreign reserves in months of imports, and binary variables for legislative and executive elections.¹¹

Likewise, the number of conditions may be endogenous as selection into conditionality is not random (Stone 2008; Stubbs et al. 2020). With regard to inequality and poverty, the IMF may be more lenient towards poorer performing countries not necessarily because of distributional concerns *per se*, but to maintain social and political stability (Forster et al. 2019). Such systematic differences between countries that receive more conditions and those that receive fewer would mean the uncorrected estimates of IMF coefficients underestimate the true effect. The established approach to account for endogeneity of conditionality is to adopt an instrumental variable design and then use maximum likelihood estimation over a system of three simultaneous equations—conditionality, participation, and outcome equations (Stubbs et al. 2020). However, because analyses are restricted to non-zero values of fiscal adjustment, we lack the requisite number of observations for model convergence using MLE. Results on the effect of the number of conditions should therefore be interpreted with care, since our method is unable to purge the coefficients of bias—a common limitation in studies on conditionality effects (e.g. Rickard and Caraway 2019; Woo 2013). Nevertheless, it is plausible that some of the same qualities that make a country prone to selection into IMF fiscal adjustment also impact the number of conditions, incorporated in the inverse-Mills ratio.

4 Results

4.1 Inequality

In Table 2, we present the results of our quantitative analyses on the Gini coefficient of disposable income on six variants of our model. Models 1 and 2 examine the impact of our explanatory variables on the income Gini coefficient after one year. In Model 1, we initially exclude the variable measuring the total number of conditions. Focusing on the outcome equation, IMF fiscal adjustment exhibits a positive and statistically significant relationship at $p < 0.01$, meaning deeper fiscal consolidation is associated with more inequality. Results on control variables that reach standard thresholds of statistical significance follow the expected effect direction established by previous literature on inequality: the lagged dependent variable and trade lead to increases in the income Gini, while more years of

¹¹ Alternative control variables for the selection equation are included in robustness checks in Section 4.

schooling result in declines. Once adding a variable measuring the total number of IMF conditions in Model 2, findings for IMF fiscal adjustment and control variables remain substantively unchanged. On average, a country on an IMF program requiring an annual fiscal adjustment of 10 percentage points can expect the income Gini to increase by 0.26 points after one year, all other factors held constant. However, the IMF conditions variable does not reach standard thresholds of statistical significance. In the selection equation for both models, our excludable instruments are statistically significant, indicating that they are relevant instruments: positive for the IMF compound variable and negative for UNGA United States voting distance, as is consistent with expectations. Results on other statistically significant variables are positive for GDP per capita and life expectancy; and negative for GDP per capita squared, economic growth, foreign exchange reserves, and trade as a share of GDP. We also test on deeper lag structures for explanatory variables in additional regressions—at $t-2$ and $t-3$ —as research shows some effects unfold only after a substantive period of time has elapsed (Lang 2021).

Table 2: Effect of IMF fiscal adjustment on Gini coefficient of disposable income.

	(1) t-1	(2) t-1	(3) t-2	(4) t-2	(5) t-3	(6) t-3
Outcome equation						
L.IMF fiscal adjustment	2.4792 ^c [0.9598]	2.5686 ^c [0.9689]	1.5341 ^a [0.8965]	1.8158 ^b [0.8914]	1.5764 [1.3834]	1.5042 [1.3777]
L.Number of conditions	– –	0.0010 [0.0016]	– –	0.0032 ^b [0.0014]	– –	–0.0007 [0.0019]
L.Income Gini	0.9154 ^d [0.0227]	0.9173 ^d [0.0229]	0.9300 ^d [0.0205]	0.9349 ^d [0.0202]	0.9119 ^d [0.0297]	0.9110 ^d [0.0295]
L.GDP per capita (log)	0.5533 [1.5550]	0.4280 [1.5663]	3.1580 ^b [1.3783]	2.6833 ^a [1.3714]	4.8104 ^b [1.8684]	4.9181 ^c [1.8794]
L.GDP per capita^2 (log)	–0.0433 [0.0964]	–0.0344 [0.0974]	–0.1887 ^b [0.0844]	–0.1549 ^a [0.0843]	–0.2457 ^b [0.1139]	–0.2535 ^b [0.1152]
L.Years of schooling	–0.1569 ^b [0.0750]	–0.1518 ^b [0.0753]	–0.0818 [0.0707]	–0.0607 [0.0702]	0.0569 [0.1233]	0.0511 [0.1234]
L.Trade	0.0025 ^a [0.0013]	0.0025 ^a [0.0013]	0.0004 [0.0012]	0.0004 [0.0012]	0.0040 ^b [0.0018]	0.0040 ^b [0.0018]
L.Life expectancy	–0.0448 [0.0279]	–0.0456 [0.0278]	–0.0576 ^b [0.0260]	–0.0610 ^b [0.0256]	–0.0489 [0.0394]	–0.0480 [0.0390]
L.Democracy	0.0164 [0.0399]	0.0162 [0.0399]	–0.0486 [0.0360]	–0.0484 [0.0354]	–0.1342 ^c [0.0519]	–0.1342 ^c [0.0514]
Inverse-Mills ratio	–0.0919 [0.0934]	–0.0927 [0.0933]	–0.1189 [0.0874]	–0.1273 [0.0867]	–0.3552 ^b [0.1647]	–0.3513 ^b [0.1635]
Constant	6.4438 [6.5082]	6.7776 [6.5229]	–4.8723 [5.9346]	–3.5016 [5.8641]	–14.8690 ^a [8.2166]	–15.1852 ^a [8.1906]

Table 2: (continued)

	(1) t-1	(2) t-1	(3) t-2	(4) t-2	(5) t-3	(6) t-3
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Selection equation						
L.Income Gini	0.0008 [0.0085]	0.0008 [0.0085]	0.0023 [0.0088]	0.0023 [0.0088]	0.0035 [0.0092]	0.0035 [0.0092]
L.GDP per capita (log)	2.4912 ^d [0.5444]	2.4912 ^d [0.5444]	2.4280 ^d [0.5489]	2.4280 ^d [0.5489]	2.2437 ^d [0.5614]	2.2437 ^d [0.5614]
L.GDP per capita ² (log)	-0.1801 ^d [0.0324]	-0.1801 ^d [0.0324]	-0.1738 ^d [0.0327]	-0.1738 ^d [0.0327]	-0.1625 ^d [0.0334]	-0.1625 ^d [0.0334]
L.Years of schooling	0.0237 [0.0278]	0.0237 [0.0278]	0.0140 [0.0286]	0.0140 [0.0286]	0.0051 [0.0300]	0.0051 [0.0300]
L.Trade	-0.0024 ^a [0.0014]	-0.0024 ^a [0.0014]	-0.0025 ^a [0.0014]	-0.0025 ^a [0.0014]	-0.0028 ^b [0.0014]	-0.0028 ^b [0.0014]
L.Life expectancy	0.0204 ^b [0.0095]	0.0204 ^b [0.0095]	0.0189 ^b [0.0095]	0.0189 ^b [0.0095]	0.0188 ^a [0.0097]	0.0188 ^a [0.0097]
L.Democracy	0.0047 [0.0260]	0.0047 [0.0260]	-0.0006 [0.0268]	-0.0006 [0.0268]	0.0093 [0.0282]	0.0093 [0.0282]
L.Growth	-0.0376 ^c [0.0130]	-0.0376 ^c [0.0130]	-0.0375 ^c [0.0130]	-0.0375 ^c [0.0130]	-0.0368 ^c [0.0133]	-0.0368 ^c [0.0133]
L.Current account	-0.0073 [0.0074]	-0.0073 [0.0074]	-0.0075 [0.0077]	-0.0075 [0.0077]	-0.0038 [0.0081]	-0.0038 [0.0081]
L.Reserves	-0.0542 ^c [0.0198]	-0.0542 ^c [0.0198]	-0.0561 ^c [0.0203]	-0.0561 ^c [0.0203]	-0.0523 ^b [0.0208]	-0.0523 ^b [0.0208]
L.Legislative election	-0.0911 [0.1183]	-0.0911 [0.1183]	-0.0963 [0.1212]	-0.0963 [0.1212]	-0.0367 [0.1256]	-0.0367 [0.1256]
L.Executive election	0.1605 [0.1413]	0.1605 [0.1413]	0.1400 [0.1457]	0.1400 [0.1457]	0.1049 [0.1528]	0.1049 [0.1528]
L.UNGA US distance	-0.4347 ^d [0.0989]	-0.4347 ^d [0.0989]	-0.4337 ^d [0.1014]	-0.4337 ^d [0.1014]	-0.4377 ^d [0.1067]	-0.4377 ^d [0.1067]
L.IMF compound	4.8417 ^b [1.9211]	4.8417 ^b [1.9211]	4.1273 ^b [1.9837]	4.1273 ^b [1.9837]	3.3686 [2.0760]	3.3686 [2.0760]
Constant	-9.1640 ^d [1.9165]	-9.1640 ^d [1.9165]	-8.8968 ^d [1.9289]	-8.8968 ^d [1.9289]	-8.1895 ^d [1.9607]	-8.1895 ^d [1.9607]
Country fixed effects	No	No	No	No	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Diagnostics						
N	1666	1666	1591	1591	1479	1479
N selected	172	172	164	164	151	151

Standard errors in brackets. ^a $p < 0.10$, ^b $p < 0.05$, ^c $p < 0.01$, ^d $p < 0.001$.

Next, we examine the effect of IMF fiscal adjustment after two years in Models 3 and 4. We find a weaker effect than that experienced after one year, and with less statistical certainty, at $p < 0.10$ when excluding the conditions variable and $p < 0.05$ when including it. Notably, the number of conditions now also yields a statistically significant positive effect, in line with our theoretical expectations, although the size of the effect is decidedly marginal: each additional condition increases the income Gini by 0.003 points. On average, an IMF fiscal adjustment of 10 percentage points will result in an increase to income Gini of 0.18 points after two years, all else held constant. Among control variables, the lagged dependent variable and GDP per capita exerts a positive effect; GDP per capita squared and life expectancy exert a negative effect; and all other variables do not reach standard thresholds of significance. In selection equations, both our excludable instruments are strong and findings on controls remain consistent with the previous two models.

For Models 5 and 6, we find that after three years neither IMF fiscal adjustment nor the total number of conditions is associated with increases in the income Gini at standard thresholds of significance, although the effect direction for fiscal adjustment is comparable to that observed in the two year lag models. This finding diverges from a recent study showing that IMF intervention increases inequality for up to five years (Lang 2021), likely due to our more nuanced measure of IMF intervention, which captures the extent of fiscal adjustment required rather than a simple dummy variable for IMF program participation. Using this measure also restricts us to fewer observations—those country-years with IMF fiscal adjustment conditions—so our effect is measured with less precision (i.e. larger standard errors). Results on control variables are comparable to those in previous models, with the exception of democracy and the inverse-Mills ratio which carry a statistically significant negative effect. In selection equations, the IMF compound instrument is no longer relevant. All else is substantively unchanged.

Next, we investigate the effect of IMF fiscal adjustment on inequality by focusing on income decile shares for all 10 deciles of countries' income distributions, allowing us to unpack where in the income distribution losses and gains are accrued. In Figure 1, we summarize information from 10 separate regression models by plotting the effect of IMF fiscal adjustment for each income decile share, conditional on the covariates. Full results are available in Web Appendix (Table A2). We find a statistically significant negative effect of fiscal adjustment on income decile shares one to eight. The magnitude of the effect expands incrementally from decile one (IMF adjustment coefficient of -0.014) to decile eight (coef., -0.032). While the fiscal adjustment coefficient for income decile share nine is also comparable (coef., -0.029), it does not reach standard thresholds of statistical significance. For income decile share 10, the effect of the IMF adjustment

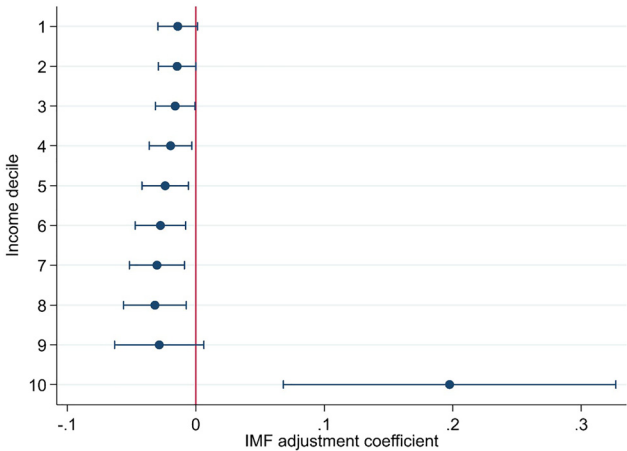


Figure 1: IMF fiscal adjustment effect on share of income decile. Error bars are 95% confidence intervals.

coefficient turns positive and is large relative to the other deciles (coef., 0.198). These results indicate IMF fiscal adjustment targets fostered inequality by concentrating income to the top 10% of earners. While all other deciles lose out (with the possible exception of decile nine), the biggest losses are accrued by middle class earners, plausibly a product of wage, employment, and pension cuts for civil servants.

We perform a series of robustness checks presented in the Web Appendix. To begin with, we check that findings on the income Gini after one year hold using four sets of alternative controls (Table A3). First, based on the study by Forster et al. (2019), we include GDP per capita (logged), trade, democracy, the Penn World Table human capital index, net inflows of foreign direct investment as a share of GDP, inflation, and unemployment as a share of total labor force (all lagged one year); we also include a lagged dependent variable, but exclude the measure of government orientation because our sample size is reduced by 45% due to missing data. In this context, IMF fiscal adjustment maintains its statistically significant positive effect at $p < 0.05$, although the effect size is attenuated (10%pt. adjustment leads to a 0.19 income Gini increase). Second, following Meschi and Vivarelli (2009), we control for the lagged dependent variable, trade, lagged trade, human capital, lagged human capital, inflation, GDP per capita, and GDP per capita squared. Again, the effect for IMF fiscal adjustment holds at $p < 0.05$ and is of comparable size (10%pt. adjustment leads to a 0.22 income Gini increase). Third, we use Dreher and Gaston’s (2008) model specification of a lagged dependent

variable, the KOF globalization index, GDP per capita, GDP per capita squared, and democracy. Results remain consistent ($p < 0.05$, 10%pt. adjustment leads to a 0.17 income Gini increase). Fourth, we use our preferred specification but without the squared term of GDP per capita. Results are substantively unchanged ($p < 0.05$, 10%pt. adjustment leads to a 0.24 income Gini increase).

We then test the effect of IMF fiscal adjustment on the income Gini after one year using five variations of our selection model (Table A4): a stripped version, incorporating outcome controls and the UNGA United States voting distance instrument only; Forster et al.'s (2019) specification, which includes both a second-lag of an IMF program participation dummy and the compound instrument, but excludes the UNGA United States voting distance instrument; a revised version of the Forster model, with the second-lag of an IMF program participation dummy and the UNGA United States distance instrument but without the IMF compound instrument; another revised version of the Forster model that excludes the IMF program participation dummy; and a final version of our preferred specification that excludes the lagged dependent variable. Changes to variables included in the selection model do not substantively alter our results. Finally, we re-run our analyses on the income Gini after one year and on the 10 income decile shares with IMF fiscal adjustment outliers included (Table A5). Our results remain robust.

4.2 Poverty

We investigate the impact of IMF fiscal adjustment on various poverty indicators in Table 3. Only the outcome equation is presented; full results are available in the Web Appendix (Table A6). For Models 7 and 8, the dependent variables are the World Bank measures of the poverty headcount ratio at \$1.90 and \$3.20 a day respectively. On the \$1.90 a day measure, IMF fiscal adjustment shows a statistically significant positive effect at $p < 0.10$, but does not reach standard thresholds of significance on the \$3.20 measure day. We detect no effect for the number of IMF conditions on both models. Among statistically significant control variables, GDP per capita displays a negative effect and the income Gini a positive effect, as consistent with expectations. It may be that coefficient estimates are measured with low precision (i.e. high standard errors) because there are relatively few observations ($n_{selected} = 130$).

In Models 9, 10, and 11, we use poverty headcount measures available from GCIP, at \$1.44, \$1.86, and \$2.50 a day respectively, giving us 54 more observations than the World Bank measures per model. We find a statistically significant positive association between IMF fiscal adjustment and all three measures of poverty, and statistically significant control variables fall in their expected direction. The

Table 3: Effect of IMF fiscal adjustment on various poverty indicators.

	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Poverty head- count \$1.90	Poverty head- count \$3.20	Poverty head- count \$1.44	Poverty head- count \$1.86	Poverty head- count \$2.50	Poverty gap \$1.90	Poverty gap \$3.20
IMF fiscal adjustment	30.6207 ^a [16.1628]	42.1695 ^a [26.8615]	27.0144 ^b [11.3399]	40.5987 ^c [14.9030]	50.5262 ^c [18.6020]	6.3759 [8.5301]	20.5683 ^a [12.1151]
Number of conditions	-0.0023 [0.0265]	0.0628 [0.0439]	-0.0210 [0.0179]	0.0111 [0.0237]	0.0852 ^c [0.0292]	-0.0105 [0.0141]	0.0086 [0.0199]
GDP per capita (log)	-8.0782 ^b [3.2551]	-26.6576 ^d [5.4858]	-4.6548 ^a [2.4274]	-9.7593 ^c [3.2160]	-27.7945 ^d [3.9500]	-0.1226 [1.6806]	-7.4370 ^c [2.4452]
GDP growth	-0.5504 [0.4766]	-0.7304 [0.8500]	-0.5308 ^a [0.3206]	-0.6918 [0.4482]	-0.3193 [0.4912]	-0.2329 [0.2207]	-0.3999 [0.3613]
Income Gini	1.1713 ^d [0.3299]	1.3890 ^b [0.5590]	0.3086 [0.2347]	0.9045 ^c [0.3115]	2.0774 ^d [0.3813]	0.5482 ^c [0.1688]	0.8660 ^d [0.2480]
Growth ^b Income Gini	0.0082 [0.0120]	0.0118 [0.0213]	0.0097 [0.0082]	0.0111 [0.0114]	0.0050 [0.0127]	0.0045 [0.0056]	0.0062 [0.0091]
Corruption	0.0177 [0.0593]	-0.0148 [0.1027]	0.0370 [0.0425]	0.0790 [0.0576]	0.0551 [0.0674]	0.0098 [0.0291]	0.0095 [0.0447]
Inverse-Mills ratio	3.1296 [1.9617]	6.5335 ^a [3.4523]	2.9324 ^b [1.4122]	4.7483 ^b [1.9424]	3.3845 [2.2063]	0.5770 [0.9351]	2.4469 ^a [1.4840]
Constant	20.1175 [25.6881]	170.4532 ^d [43.4399]	24.8525 [20.1885]	40.4017 [26.7406]	146.3666 ^d [32.8617]	-19.2743 [13.1882]	28.5072 [19.3068]
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1782	1782	1836	1836	1836	1782	1782
N selected	130	130	184	184	184	130	130

Standard errors in brackets. ^a $p < 0.10$, ^b $p < 0.05$, ^c $p < 0.01$, ^d $p < 0.001$. Selection models reported in Appendix.

effect of IMF fiscal adjustment is weakest on the \$1.44 measure, where a 10 percentage point adjustment would, on average, raise the poverty headcount ratio by 2.70 percentage points ($p < 0.05$); for the \$1.86 measure, the same adjustment would result in a 4.06 percentage point increase ($p < 0.01$); and for the \$2.50 measure, it would lead to an upsurge of 5.05 percentage points. The latter also yields a positive relationship with the number conditions at $p < 0.01$, with each additional condition increasing the share of people in poverty at the \$2.50 a day mark by 0.085 percentage points.

In Figure 2, we illustrate graphically how Model 11 would predict changes to the poverty headcount ratio at \$2.50 a day, varying IMF fiscal adjustment and averaging the remaining covariates in the sample. For example, fixing IMF fiscal adjustment at 5 percentage points, our model would predict a poverty headcount at 27.35% of the population, compared to 24.83% with no adjustment.

Next, we consider the impact of IMF fiscal adjustment on the World Bank's poverty gap measures at \$1.90 and \$3.20 a day in Models 12 and 13. A statistically significant positive relationship is detected only for the \$3.20 a day measure, and results on controls are consistent with poverty headcount models.

We then conduct additional tests to establish robustness of results in the Web Appendix. We initially re-estimate the model for the poverty headcount ratio at \$2.50 using alternative control variables (Table A7): first, we replicate Mosley et al.'s (2004) specification by removing growth and its interaction term with income Gini, and add a control for government social spending as a share of GDP;

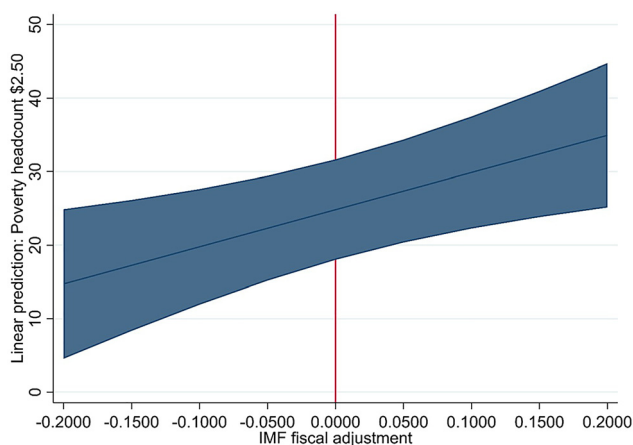


Figure 2: Predictive margins of effect of IMF fiscal adjustment on poverty.

Effect of IMF fiscal adjustment on poverty headcount ratio at \$2.50 a day (constant 2005 PPP \$), with 95% confidence intervals. Predictive margins based on Model 11 (Table 3).

second, we add growth and its interaction with income Gini to the previous model; third, we use our original model but without the interaction term; fourth, we use our original model but without income Gini or its interaction with growth; fifth, following Beck et al.'s (2007), we add controls for years of schooling, inflation, trade, population growth, and the dependency ratio, but remove the income Gini; sixth, we then add the income Gini to the previous model; seventh, we add natural resource rents as a share of GDP and democracy to the previous model, since economies dominated by resource-based commodities are associated with growing poverty, whereas democratic governments are more likely to implement pro-poor policy (Nissanke and Thorbecke 2006; Reuveny and Li 2003); eighth, we then remove the income Gini from the previous model; ninth, we strip the previous model of remaining plausibly endogenous controls, namely growth and trade; tenth, we use our original model but without growth or its interaction term. Results on IMF fiscal adjustment are robust across all models, whereas findings on the number of conditions hold in five of 10 models (and approach statistical significance in the other five).

Following this, we re-run analyses on our original set of controls but using the same four variations of our selection equations described in robustness checks for inequality (Table A8). Results are substantively unchanged. Lastly, findings are robust to the inclusion of IMF fiscal adjustment outliers for all poverty headcount measures from GCIP, but not for headcount and gap measures from the World Bank (Table A9).

5 Discussion and Conclusions

This article incorporated new data on the intensity of fiscal adjustment to examine the effects of IMF austerity on poverty and inequality. In so doing, we overcame limitations of earlier studies that treat the extent of fiscal consolidation required from programs as homogenous. We used a dataset of up to 79 countries observed in the period from 2002 to 2018 and deployed an appropriate econometric strategy to find that greater austerity leads to greater income inequality and higher poverty. Probing this relationship further, we found that the effect on inequality is exerted for up to two years, and is driven by concentrating income into the top 10% of earners. For poverty, the effect is apparent across multiple poverty headcount measures—on the World Bank's \$1.44 and GCIP's \$1.86, \$1.90, and \$2.50 measures—and one of our two poverty gap measures—at \$3.20. Confidence in our findings was bolstered by the fact that our results were consistent across a range of different models estimated in robustness checks. These findings call into question the

flattering results of the IMF's own studies on the impact of its programs on vulnerable populations (Gupta, Schena, and Yousefi 2020).¹²

Before discussing the implications of these findings, we note three limitations. First, a potential problem was missing data for the IMF fiscal adjustment measure, which only captured headline fiscal targets set for end-December. To mitigate this concern, we restricted analyses to countries with a fiscal deficit condition and performed econometric corrections to account for non-random assignment into the sample. Our results can therefore only be understood relative to other countries undergoing IMF fiscal adjustment. Second, while we employed the best available methods to address potential endogeneity of IMF fiscal adjustment, due to computational constraints we were unable to apply a correction to potential endogeneity on the number of conditions—a common limitation in studies on conditionality effects (e.g. Rickard and Caraway 2019; Woo 2013). Third, inequality data was imputed rather than observed. We cannot discount the possibility that the imputation process introduced measurement error, and have no way to verify it because observed data is mostly unavailable for developing countries.

Such limitations are not to be downplayed, yet the advances made in this article over previous studies together allow us to corroborate the early and all but overwhelming evidence on the impact of IMF on poverty and inequality. At this writing in the midst of the COVID-19 economic crisis, the IMF may be engaging in more country programs than during any other period in its history. The IMF has repeatedly said that the external shock from the COVID-19 crisis was an external one that is not a function of domestic policy. The Fund has underscored the need to protect the poor and vulnerable, and its pronouncements have been interpreted as “officially burying” austerity (Giles 2020). The evidence from this article strongly affirms that fiscal austerity will not help protect the vulnerable.

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¹² Gupta, Schena, and Yousefi (2020) study contains serious methodological shortcomings. They use a cross-sectional version of the autoregressive distributed lag specification (CS-ARDL), where the outcome (i.e. social spending) is conditioned on past levels of the outcome and on past levels of the treatment (i.e. IMF intervention), but with no additional control variables. While this kind of identification strategy is appropriate if there is no other variable that affects *both* treatment and outcome, in their context there are many such additional factors, including economic growth, trade, or democracy (Stubbs et al. 2020). The estimates they obtain are therefore biased and unreliable.

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