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IMF, Structural Adjustment, and Poverty:

A Cross-National Difference-in-Differences

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IMF, Structural Adjustment, and Poverty: A Cross-National Difference-in-Differences Analysis, 1980-2018

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Abstract

The International Monetary Fund (IMF) has been one of the world's most powerful international organizations in setting the parameters for economic reforms in the developing world. In this study, using data from 1980-2018 from 57 countries, we test competing hypotheses surrounding the impact of the IMF's lending programs on poverty incidence in participant countries. Departing from the prevailing practice of relying on instrumental variables, we employ a novel difference-in-differences approach that ensures clean comparisons between "treatment" and "control" units based on their program participation histories. Besides providing a quantitative estimate of the average program effect, we evaluate whether the IMF's alleged anti-poverty focus in recent decades has made any difference. We find that IMF program participation leads to large increases (3.6-5.7 percentage points) in the proportion of a country's population living under the $3.65/day and $6.85/day international poverty lines (2017 PPP) and the country-specific Societal Poverty Line. We also find that the poverty reduction measures incorporated by the IMF into its programs have not been effective in mitigating the poverty-increasing program effects. Overall, our findings show that IMF programs have been detrimental to the welfare of vulnerable populations in participant countries.

INTRODUCTION

The International Monetary Fund (IMF) has been one of the world's most powerful international organizations in setting the parameters for economic reforms in the developing world (Halliday and Carruthers 2007; Kentikelenis and Seabrooke 2017; Kentikelenis, Thomas H. Stubbs, and King 2016). Critics have argued that the IMF has been a vector of neoliberal politics by promoting policy reforms intended to lessen the role of the welfare state and enhance the role of markets in national economies (Babb and Kentikelenis 2018). Scholars and activists have highlighted the adverse social consequences of IMF-mandated reforms that impose austerity on participant countries, which disproportionately impacts the poor and the vulnerable (Stubbs et al. 2022, 2023). In contrast to its reputation, the IMF claims that—beyond ensuring necessary international macroeconomic stability through its role as global "lender of last resort"—it has embraced an anti-poverty focus in its lending programs in the last two decades (Stubbs et al. 2022). It also claims that it is a champion of the Sustainable Development Goals set by 193 countries in 2015 (Annett and Lane 2018; Benedek et al. 2021). Has the IMF lived up to its promise about the new mandates that it has taken upon itself? Or does it embody "organized hypocrisy", where the underlying practices have still been the same (Kentikelenis and Stubbs 2023)?

The impact of IMF lending programs on development and poverty has drawn significant academic attention. While the IMF has argued that its macroeconomic policies are pro-poor in the long run and that it has reformed the designs of its lending programs to protect the poor from short-term adverse impacts, critics of the IMF have highlighted the harms of its lending programs and maintained skepticisms about the sincerity in the IMF's social protection and poverty reduction efforts. Some argue that the adverse consequences for the poor are unintended consequences that need to be addressed with swift organizational reforms by the IMF, while others, drawing on dependency and world-systems theories, argue that the negative impacts are the result of structural hierarchies between countries and international organizations designed to reproduce those hierarchies. Questions about the extent to which IMF programs have increased poverty in participant countries and the mechanisms by which they would do so form an important empirical basis for this debate. Past studies have employed a variety of different methods and yielded mixed results. Among quantitative studies, the concern for the presence of unobservable confounders has led researchers to adopt a variety of instrumental-variable approaches to identifying causal effects. The use of instrumental variables has now become the prevailing practice when researchers seek to evaluate the impact of IMF programs (Kentikelenis and Stubbs 2023; Stubbs et al. 2020).

The present study aims to provide a robust quantitative estimate of the impact of IMF programs on poverty while addressing the methodological limitations of previous research. We employ a novel statistical approach that departs from the prevailing practice of using instrumental variables, which we view as unreliable, and focuses instead on making clean and credible comparisons through a difference-in-differences research design. By analyzing distinct episodes of IMF intervention with explicitly selected treatment and control groups, we derive quantitative estimates of the average effect of IMF interventions on poverty in participating countries. We also evaluate whether the impact has been mitigated by the purportedly poverty-mitigating components of IMF programs in recent years.

IMF, STRUCTURAL ADJUSTMENT, AND POVERTY

The IMF and the World Bank both originated from the 1944 Bretton Woods Conference as part of an effort to finance the rebuilding of Europe after WWII and put in place a system of global financial and monetary governance to prevent another global depression (Stiglitz 2002; Vreeland 2006). The initial mandate of the IMF was to stabilize the Bretton Woods system of pegged exchange rates by overseeing the exchange rates of member governments and providing conditional financial resources temporarily to members to correct for balance of payments maladjustments (Babb and Kentikelenis 2018). Over the years, the IMF has changed markedly. After the collapse of the Bretton Woods system and the global move towards floating exchange rates after 1973, only the second aspect of the original mandate survives. For the first three decades after its founding, the IMF practiced policy-conditional emergency loans that required austerity measures such as reductions in fiscal deficit, but it retained a neutral stance about the relative role of states and markets in national economies (Babb and Kentikelenis 2018). In the 1980s, in the context of the rise of neoliberal conservative governments in the US and the UK and the Third World debt crisis, the IMF began to extend beyond its original mandate and use conditionality to continue to promote austerity (which they refer to as "stabilization") but also to restructure economic institutions along "market fundamentalist" or neoliberal lines (Babb and Kentikelenis 2018). These reforms — broadly known as the "Washington Consensus" — are based on free market ideology and push for policies such as privatization, trade and financial liberalization, and economic deregulation (Babb and Kentikelenis 2018).

The IMF has faced criticisms from civil society groups and academics for failing in its mission to promote global stability, damaging the economic prospects of countries that turned to it for assistance, and harming the life chances of the populations of those countries, especially those who are already poor (Stiglitz 2002). It is argued that conditionalities such as rapid trade liberalization and tight monetary policies can lead to the destruction of vulnerable industries and mass unemployment in the absence of social protection, forcing workers and farmers into poverty (Stiglitz 2002). Capital market liberalization and the lifting of capital controls can lead to influxes of money into and out of the country, which can also lead to increased volatility and instability in the country's economy (Nosrati et al. 2024; Stiglitz 2002).

In contrast, the Fund insists that since the late 1970s, it has sought to promote economic stability in the long run, "which is necessary for alleviating poverty"(International Monetary Fund 1998). Moreover, it claims that its mission has evolved to promote "high quality growth" which "is accompanied by policies that attempt to reduce poverty and improve the equality of opportunity" (International Monetary Fund 1998). Some scholars support the IMF's position by arguing that IMF programs may potentially alleviate poverty by providing funding in cases of severe illiquidity and helping avoid more severe reductions in government expenditures and pro-poor spending (Bird, Qayum, and Rowlands 2021). Proponents of the Washington Consensus argue that trade and capital market liberalization that the IMF prescribes would lead to a reduction in poverty due to increases in wages of unskilled labor in developing countries that have a comparative advantage in labor (Rudra and Tobin 2017; Winters and Martuscelli 2014). They also argue that trade openness and global integration promoted by the IMF would allow developing countries to import capital and modern technology to reap the "advantages of backwardness" to converge to wealthy countries (Sachs and Warner 1995). Others hypothesize that IMF programs may have no direct effect on poverty as they mainly target highly visible macroeconomic indicators and the formal sector and not the informal sector from which the poor mainly derive their income (Easterly 2001).

In contrast to analyses centered around the nation-state, dependency and world systems theories take a systemic perspective on the relationship between globalization and poverty and emphasize hierarchy as a structural feature of the capitalist world-system (Chase-Dunn 1998). In this framework, intergovernmental organizations like the IMF are central actors that perpetuate the dependency of "peripheral" (developing) nations on the "core" (the US and the West), and the continuing underdevelopment of the periphery is the result of the reproduction of core/periphery differences in state strength (Chase-Dunn 1998; Reinsberg et al. 2019; Shandra et al. 2004). The difference in state strength and the attendant dependency relations are reproduced and deepened by the operations of international financial institutions like the IMF, as the practice of conditionality gives core nations—the creditors—the ability to shape the political economies of developing countries in their own interests (Kentikelenis, Thomas H Stubbs, and King 2016; Reinsberg et al. 2019). From this perspective, the IMF would give priority to the agendas of the West such as debt servicing and trade and capital liberalization by developing countries, allowing them access to cheap labor and resources, over other goals such as social protection and poverty reduction (Chossudovsky 1997).

The two theoretical camps sketched out above can be designated as broadly neoliberal on the one hand, and dependency or world-systems theories on the other. Their sharply contrasting accounts of the IMF's policy repertoire yield two competing hypotheses surrounding the causal link between structural adjustment and poverty:

- ❖ Neoliberal hypothesis: IMF programs promote global macroeconomic stability and financial integration through its fiscal and external sector reforms; in addition, the IMF's "pro-poor" policy efforts (see below) serve to protect society's most vulnerable from short-term shocks. In tandem, these reforms have positive effects on economic development and hence on poverty. In short: IMF programs reduce poverty.

- ❖ Dependency/world-systems hypothesis: IMF programs represent the confluence of (a) durably institutionalized core-periphery power relations that shape global policy arrangements in favour of the core's economic interests, and (b) an ideological framing of such power relations in terms of neoliberal globalization. IMF programs are thus designed to ensure both continued debt servicing and access to cheap labour and resources at any cost. They thus reproduce core-periphery inequalities at the expense of durable investments in social protection in the global South. In short: IMF programs cause poverty.

Existing evidence

The two aforementioned theoretical accounts offer starkly contrasting frameworks for understanding the IMF and its policy interventions. But what does the existing empirical evidence show? Findings from past empirical studies of the relationship between IMF programs and poverty and poverty-related indicators have varied. While most quantitative studies found that IMF programs led to increased poverty in developing countries, some found the opposite. Garuda (2000) estimated the effects of 58 IMF programs between 1975-91 on Gini coefficients and the income of the poorest quintile and found evidence of a significant deterioration in income distribution and the incomes of the poor in IMF program countries relative to non-program countries. Focusing on the labor share of income, Pastor (1987) and Vreeland (2002)found that IMF programs had negative distribution consequences. Oberdabernig (2013) found adverse short-run effects of IMF programs on poverty (headcount ratio and gap) and inequality (Gini indices) for the whole sample of 86 low- and middle-income countries between

1982-2009, while the sign of the estimated effect reversed for the 2000-2009 subsample. Biglaiser and McGauvran (2022) found that IMF loan arrangements contributed to more people getting trapped in the poverty cycle and identified trade and exchange conditions, labor conditions, privatization, revenue and tax conditions, and institutional reforms as the IMF-mandated reforms that were leading to increased poverty. Stubbs and colleagues (2022) found that stricter IMF-mandated austerity was associated with higher poverty headcounts and poverty gaps. Easterly (2001) found that the growth elasticity of poverty declines by around two points for every additional IMF or World Bank adjustment loan per year, leading economic growth to be less pro-poor. Studies also found negative effects of IMF programs on poverty-related health indicators and on social and health spending, which would indirectly impact poverty. Stuckler and colleagues (2008), Maynard, Shircliff, and Testivo (2012), and Nosrati and colleagues (2022) found that IMF programs led to increased prevalence and mortality from tuberculosis (TB) and respiratory diseases. Kentikelenis and colleagues (2015) and Stubbs and colleagues (2017) found that IMF programs led to reduced public spending on health. Forster and colleagues (2020) found that IMF programs led to increased neonatal mortality and reduced health system access and identified labor market reforms as key drivers for the adverse consequences. In contrast, Bird and colleagues (2021) found that IMF programs did not significantly increase poverty and income inequality. Hajro and Joyce (2009) also found no significant direct impact of IMF programs on poverty measured by infant mortality and Human Development Index (HDI). Clements and colleagues (2013) at the IMF found that education and health spending rose during IMF programs at a faster pace than in developing countries as a whole and that IMF programs were associated with increases in the share of government spending in education and health. While results from quantitative studies have been, overall, damaging to the IMF's position, we would like to highlight that we believe there are important methodological issues in these studies, which we will discuss below.

Findings from qualitative and descriptive case studies have also been mixed. Some show that the structural adjustments policies pushed for by IMF's lending programs have worsened inequality and poverty in the participating countries; others show that the programs have no impact on poverty or the impact was not long-lasting. In Jamaica, Handa and King (2003) found short-term jumps in both inequality and poverty when IMF-mandated financial liberalization occurred, but the removal of agricultural price and exchange controls led to growth in domestic agriculture and the exporting sectors, which contributed to a full recovery. However, Handa and King (2003)found that the liberalization of Jamaica's exchange rate in 1991 and the devaluation that followed led to food price inflation and, consequently, a decline in children's weight, especially among children in urban areas. Behrman and Deolalikar (1991), also studying the case of Jamaica but in an earlier period, found no evidence of deterioration in health, nutrition, and education during the IMF-mandated macroeconomic adjustments in the 1980s. Brekk and Hicks (1991)found that structural adjustment policies had short-term negative effects and long-term positive effects on real wages of the poor in the case of Malawi between 1987-1990. In Pakistan, Kemal (2001) and Jamal (2003) found an increase in Gini coefficient and poverty incidence in the decade after the initialization of the World Bank/IMF structural adjustment program in 1988. Konadu-Agyemang (2000) found uneven impact of IMF structural adjustment programs in Ghana, leading to improvements in macroeconomic performance at the cost of widening socioeconomic and geospatial disparities and the suffering of the rural poor whose access to education, health, and other services was severely curtailed by cuts to public spending. In Zambia, Chansa, Nkandu, and Banda (2009) found that IMF macroeconomic programs that prioritized stabilization over public spending severely limited Zambia's ability to control HIV/AIDS and TB by reducing government health expenditure as share of GDP, leading to a reliance on donor funding for health and personnel deficiency in the health sector. Examining the impact of IMF structural adjustment policies in Zambia, Chansa, Nkandu, and Banda (2009) observed that IMF-initiated civil service downsizing and trade liberalization increased unemployment and worsened the country's poverty levels.

In response to criticisms that IMF conditionalities had detrimental impacts on populations by widening inequalities and pushing millions into poverty, the IMF claims that it has incorporated social concerns and concerns for the poor into its programs as "social conditionality"(Strauss-Kahn 2010). It claims that ever since it replaced its Enhanced Structural Adjustment Facility (ESAF) with the Poverty Reduction and Growth Facility (PRGF) and introduced the Poverty Reduction Strategy Papers (PRSP) for low-income countries, it has embraced an anti-poverty focus for its work in low-income countries to ensure that its lending programs are pro-poor (IMF Staff 2001). The PRSP is a document detailing the country's plans to reduce poverty required by the IMF for low-income countries to borrow from the IMF or World Bank. The process of formulating the PRSP has been criticized as donor-led, ignoring the political realities of the countries involved, and as ineffective without a mechanism of enforcing the poverty reduction plans, which could be providing only political cover for domestic elites pushing for policies that are harmful for the poor (Dijkstra 2011). The IMF's external communications have consistently reinforced expectations of the IMF to aid poverty reduction and safeguard social protection in developing countries (IMF Staff 2001; Wojnilower 2017; Wojnilower and Monasterski 2017). For example, in the aftermath of the global financial crisis in 2008-2009, the IMF claimed that it "placed considerable emphasis on strengthening social protection for the vulnerable" and “supported countries’ effort in finding practical solutions” to do so (Gottselig 2009). However, studies have casted doubt on the sincerity of these claims, showing that the IMF’s practice has not followed its rhetoric (Grabel 2011; Kentikelenis, Thomas H Stubbs, et al. 2016; Ortiz et al. 2010; Van Waeyenberge, Bargawi, and McKinley 2013; Weisbrot et al. 2009). Despite its claims to the contrary, Kentikelenis, Stubbs, and King (2016) found that the IMF had continued to advocate labor market liberalization, public sector employment reduction, and reductions in government spending in their programs. Moreover, when the IMF incorporated social spending floors that were meant to be “pro-poor”, these failed to be implemented half of the time due to a lack of state capacity or a lack of interest in the borrowing countries (Kentikelenis and Stubbs 2023).

In this study, we test the hypothesis that IMF lending programs have increased poverty incidence in participant countries. In addition, we test whether the IMF's alleged anti-poverty focus and the “social conditions” included in its programs in recent decades have made any difference.

DATA AND METHODS

In this section, we first describe the data. We then discuss methodological limitations of past studies that employ instrumental variables and conventional two-way fixed effects regressions. We highlight the salient issues of unclean comparison and post-treatment bias when evaluating the impact of IMF programs, which have received little to no attention from past studies. We then describe our research design and empirical strategy, which explicitly addresses these issues.

Data and variables

Our main dependent variables are shares of a country's population living below three different international poverty lines defined by the World Bank — \$2.15, \$3.65, and \$6.85 expressed in 2017 Purchasing Parity Power (PPP) prices — and the share of population below the Societal Poverty Line (SPL), which is defined as the larger of \$2.15 or the sum of \$1.15 and 50% of the country's median daily consumption in 2017 PPP prices. The SPL is designed as a country-specific measure of poverty that complements the international poverty lines, which do not vary across countries. For these variables, we use the consumption-based estimates from the World Bank's Poverty and Inequality Platform (PIP). These monetary measures of poverty suffer from several limitations. The use of PPPs in inter-country poverty comparison has been criticized because the measure of average prices constructed in PPPs involves a wide variety of commodities that are irrelevant to absolute poverty assessment (Deaton 2010; Reddy and Pogge 2010). It has also been shown that the international poverty lines are not good proxies of multidimensional poverty (Burchi, Rippin, and Montenegro 2018). As the poverty lines focus on income and consumption of households, they do not measure important aspects of poverty such as the quality of and access to government services, which could be negatively impacted by IMF programs. Despite the limitations, these "dollar-a-day" measures nevertheless continue to carry significant weight in policy discussions globally and influence strategies and decisions by international organizations such as the World Bank and IMF. In this article, we center our analysis around these measures and leave the rigorous investigation of the impacts of IMF programs on other measures of poverty for future research. In terms of data quality, measurement errors in the poverty headcount would not bias our estimate as long as they are not correlated with participation in IMF programs. However, if IMF programs lead to a hollowing out of state capacity and deterioration in bureaucratic quality of governments in participant countries

(Reinsberg et al. 2019) and drive a greater share of the population into informal settlements and the informal sector, it could potentially lead National Statistical Offices (NSOs) to more severely undercount the poor (Lucci, Bhatkal, and Khan 2018), resulting in downward biases in our estimates.

Data for countries' participation in IMF programs and program conditions are taken from the IMF Monitor database. To control for a country's macroeconomic condition that may simultaneously influence its decision to participate in an IMF program and its poverty headcount, our set of control variables include GDP growth rate and a binary indicator of banking crisis occurrence. To control for a country's circumstances in terms of political regime and stability, we include the *polity2* index that measures a country's political regime (democratic versus authoritarian) and a binary indicator for armed conflict taking place in the country. See Table A.1 for data sources.

Instrumental-Variable Analyses of IMF Program Impact

One key challenge to evaluating the impact of IMF programs is the issue of selection bias. Countries could be participating in an IMF program in response to an economic crisis or changes in domestic political circumstances. To avoid confusing the effects of such forces with those of IMF programs, existing studies often control for observable characteristics such as GDP per capita, presence of banking crisis, holdings of foreign reserves, and measures of democracy in their regression- or matching-based methods. Conditioning on these observed confounders would help block non-causal associations between program participation and the outcome, and, under strong assumptions, the remaining association may be interpreted as causal effects (Felton and Stewart 2022). However, there may also be *unobservable* confounders that may simultaneously lead countries to participate in IMF programs and increase poverty. Unobservable confounders that are time-invariant or country-invariant can be controlled for by using year- and country-fixed effects, but there may be unobservable confounders that are time-varying *and* country-varying. One such candidate suggested by the literature is the "political willingness" of the country's government to implement reforms (Vreeland 2003). Concerns about confounding due to time- and country-varying unobservable factors have motivated researchers to turn to the use of instrumental variables, an approach pervasive in economics and increasingly used in political science and sociology that has become the prevailing practice in the literature evaluating the impact of IMF programs (Biglaiser and McGauvran 2022; Clements et al. 2013; Easterly 2001; Forster et al. 2020; Nosrati et al. 2022; Oberdabernig 2013; Stubbs et al. 2017, 2020, 2022; Vreeland 2003).

The validity of an instrumental-variable (IV) approach relies on identifying a variable ('instrument') that satisfies the following conditions (Felton and Stewart 2022)

1. (Conditional) unconfoundedness: sharing no common causes with the treatment or outcome.
2. The relevance criterion: being highly correlated with the treatment variable.
3. The exclusion restriction: having *no other channel* to the outcome than through the treatment variable.
4. Monotonicity: increases the treatment value monotonically (through its effect on compliers).

Only the relevance criterion is verifiable with empirical data (Angrist and Pischke 2008; Hernan and Robins 2023). For the remaining criteria that cannot be empirically verified, researchers need to make a case for why one may reasonably assume them to hold based on subject-matter knowledge (Hernan and Robins 2023). With a valid instrument, researchers can identify the Local Average Treatment Effect (LATE) or, in other words, average treatment effect among *compliers* — the subpopulation for whom the instrument encourages treatment uptake — by making use of only unconfounded variation in treatment assignment caused by the instrument (Angrist and Pischke 2008). Note that even if one has a valid instrument that satisfies the assumptions required, whether or not the complier subpopulation is necessarily a population of interest to the researcher is a separate question.

Recent reviews of IV approaches have revealed how fragile IVs are to small violations of the strong assumptions that they depend on (Felton and Stewart 2022; Lal et al. 2024). These reviews conducted mass replications of published IV papers in political science and sociology and found that IV estimates were often much larger in magnitude than OLS-based selection-on-observables estimates (Felton and Stewart 2022; Lal et al. 2024). This finding is concerning, given that researchers are typically motivated to use an IV approach when they suspect that the OLS estimates correcting only for observable confounders would be biased *away* from zero due to the presence of unobservable confounders (Felton and Stewart 2022). The systematically larger IV estimates (relative to OLS) could be a result of weak instruments (in terms of correlation with the endogenous treatment variable) amplifying the biases caused by violations of the exclusion restriction (Lal et al. 2024). Furthermore, when the effect of the proposed instrument on treatment is small relative to the variance of the outcome, the IV estimator's variance will be high (Baiocchi, Cheng, and Small 2014). Given that achieving statistical significance with a high-variance estimator requires unusually large estimates (Gelman and Carlin 2014), *p*-hacking and selective reporting of estimates from non-experimental IVs can lead to a systematic exaggeration of effect sizes in published studies (Brodeur et al. 2016; Felton and Stewart 2022). These issues call for researchers to exercise caution when considering an IV approach and evaluating the credibility of IV estimates.

Good instruments are hard to come by. In the case of studying the impact of IMF programs, devising a valid instrument is extremely challenging if not infeasible, given the complex process and heterogeneous circumstances that lead countries to participate in IMF programs. The IMF does not assign countries into its programs experimentally, and what drives a country to participate in an IMF program often also influences the country's outcomes through other channels (Vreeland 2006). Past studies of IMF programs used political economic variables such as a countries' voting patterns at the UN General Assembly (UNGA) as instruments for their participation in IMF programs (Barro and Lee 2005; Clements et al. 2013; Dreher 2006; Stubbs et al. 2022). Doubts have been cast on whether the LATE identified using such an instrument is representative of all IMF programs (Dreher, Eichenauer, and Gehring 2018). Furthermore, these instruments have also faced criticisms over possible violations of the exclusion restriction, since the variables used are likely correlated with countries' outcomes through channels other than participating in IMF programs (Lang 2021). For example, it is likely that, besides influencing participation in IMF programs, a government's foreign policy preferences (as reflected in UNGA voting patterns) are linked to economic outcomes through other channels such as its domestic policy preferences; hence, a country's UNGA voting would not satisfy the exclusion restriction required for a valid instrument (Lang 2021).

Currently, the "state-of-the-art" instrument that prevails in the literature is the compound instrument proposed by Lang (2021) and Stubbs and colleagues (2020) which takes the form of the interaction between a measure of within-country mean exposure to IMF programs and a measure of the IMF's budget constraint. According to the proponents of the compound instrument, the unconfounded variation in treatment assignment caused by the compound instrument comes from the idea that in times of high liquidity, the IMF has both the financial means and the bureaucratic incentive to more actively look for additional clients beyond long-standing program participants, breaking the ties between past propensity to participate with current participation (Lang 2021). Whether the IMF and countries behave as theorized has not been rigorously investigated empirically, and it is unclear whether countries encouraged to participate in IMF programs in this way would share a similar experience with the typical countries that participate in IMF programs. Furthermore, we show later that the compound instrument also performs poorly in terms of predicting IMF program participation. In our main analysis, we avoid using instruments that are likely of poor quality and focus on accounting for selection due to observable factors and controlling for time-invariant and country-invariant factors with fixed effects, while addressing other issues that have so far received no attention from the literature — that of *unclean comparisons* and *post-treatment biases*.

Differences-in-Differences and Two-Way Fixed Effects Regressions

In the absence of a valid instrument, we employ a difference-in-differences (DID) approach that attempts to deal with the issue of unobservable confounders only through controlling for unit- and time-fixed effects. This is a common estimation approach to evaluating the impact of policy interventions when using data with a time dimension. Typically, this approach leverages variation (e.g. across states or countries) in the adoption and in timing of adoption of an intervention ("treatment") and compares the differences between "treated" and "control" units over time to infer causal effects (Baker, Larcker, and Wang 2022). The identification of causal effects depends on the assumption that the counterfactual outcomes of the treated units would be parallel in trend with those of control units. Recent advances in econometric theory suggest that in staggered treatment adoption settings — when units get treated at different points in time — the standard two-way fixed effects (TWFE) regressions used in implementing DID often do not provide valid estimates of the causal effect due to the fact that already-treated units are effectively used as control units in comparison with newly treated units (Baker et al. 2022; Goodman-Bacon 2021). When treatment effects can vary over time (e.g. phasing in with a lag), TWFE estimates can even reverse the sign of the treatment effect, even when treatment is randomly assigned (Baker et al. 2022).

When studying the effects of IMF programs, this is further complicated by the fact that countries enter and exit programs — sometimes at high frequency — and the fact that programs can have enduring effects that last beyond the years in which they were active. Reform measures required by IMF conditionality might not be implemented immediately, and the lengthier programs that the IMF introduced since the mid-1980s often targeted structural changes amounting to an overhaul of a country's policy arrangements (Kentikelenis and Stubbs 2023). For example, suppose country *A* entered an IMF program for the first time in 1995 and exited the program in 1998. If the program required the country's government to privatize its state-owned enterprises, country *A* may not re-nationalize its state-owned enterprises immediately after the program ended. Hence, even though there was not an active program, observations from country *A* after 1998 would be ill-suited as control observations to be compared with treated observations. Therefore, there are good reasons to believe that a standard TWFE regression using a reversible binary indicator of current program participation would not provide valid causal estimates in this setting.

Several approaches have been proposed in the econometric literature to address the issue of unclean comparisons in TWFE regressions (Baker et al. 2022; Callaway and Sant'Anna 2021; Sun and Abraham 2021). In this article, our research design employs the "stacked regression" approach to difference-in-differences (Cengiz et al. 2019) and the "local projections" approach to event study (Dube et al. 2023) to overcome these issues (see details below). Our research design follows a strict "clean control" criterion of using only not-yet-treated and never-treated units as control units. Moreover, to address the potentially long-lasting effects of IMF programs, we assume treatment to be an "absorbed" state, where countries do not "forget" that they have experienced an IMF program and they remain treated even after there is no longer an active program.

Post-Treatment Bias

Another issue that has received little attention from studies of IMF programs is the presence of *post-treatment bias*, which may arise when conditioning on variables that could be affected by the treatment (Acharya, Blackwell, and Sen 2016; Biglaiser and McGauvran 2022; Montgomery, Nyhan, and Torres 2018). To illustrate how bias can arise, we use an example provided by Acharya, Blackwell, and Sen (2016). Suppose that there is no causal effect of having a car accident (the "treatment") on the probability of having cancer (the outcome of interest), if we condition on individuals being a patient in the hospital (a likely consequence of having a car accident) when examining the relationship between having car accidents and having cancer, then we would find a negative correlation between car accidents and cancer (Acharya et al. 2016). If one is admitted to the hospital and one has not had a car accident, then one is likely to be admitted due to other reasons such as having cancer. This, however, does not mean that car accidents have a cancer-fighting effect. Hence, the correlation does not have a causal interpretation.

Post-treatment bias can pose a serious obstacle to identifying causal relationships. In the case of estimating the impact of IMF programs, which can have extensive effects on a country's political economy, covariates such as GDP growth rate and incidence of armed conflict can plausibly be consequences of IMF programs. In this study, we address the issue of post-treatment bias by employing the "local projections" approach to difference-in-differences event study proposed by Dube et al. (2023). This approach enables us to control only for pretreatment dynamics in covariates and outcome when estimating the dynamic path of the average treatment effect of IMF programs.

Research Design

Our research design includes two complementary approaches to estimating the impact of IMF programs. First, we adopt the "stacked regression" approach to difference-in-differences proposed by Cengiz and colleagues (2019) to evaluate the average impact of IMF programs on participating countries' poverty headcount ratios. Second, we implement the "local projections" approach to difference-in-differences event study proposed by Dube et al. (2023) to examine how program effect evolves over time. In both approaches, we focus on a window around the years in which countries entered an IMF program for the first time since 1980. To account for the enduring effects of IMF programs beyond program years, we assume program participation to be an absorbed state, where after a country enters a program for the first time, it stays treated regardless of whether there is still an active program.

A calendar year where one or more countries enter an IMF program for the first time since 1980 is considered an "event". When implementing the stacked regression approach, we build event-specific country-year panel datasets that center around the year of the event. Each event-specific panel includes observations from the treated cohort (i.e. countries that enter IMF programs in the year of the event) as well as observations from not-yet- and never-participating countries that serve as "clean controls" within the event window (Baker et al. 2022; Cengiz et al. 2019). We generate an event-specific identifying variable and then stack the event-specific panels into a single dataset (hereafter as the "stacked dataset"). This procedure is illustrated graphically in Figure 1 and described in detail below. We then estimate a model with *event-specific* year- and country-fixed effects on the stacked dataset. This approach ensures that the treatment group and control group within each panel contain only countries that had no prior IMF program experience before the event, thus avoiding the problem of unclean comparisons. Furthermore, focusing on first-time entries, our approach does not rely on variations in program participation from repeated IMF program entries and exits to identify program effects. Repeat borrowers may face fewer conditions in later arrangements with the IMF because they already had to implement extensive conditionality in previous programs (Kentikelenis and Stubbs 2023). Hence, if we consider repeated entries alongside first-time entries, it could significantly understate the impact of IMF programs.

For each event-specific panel, we define the event window to include observations between three years prior to and five years after the event year. For each event-specific panel, observations within this window are assigned to the treatment group if they belong to countries entering an IMF program for the first time in the event year and to the control group if they belong to countries that either never participated in an IMF program throughout the entire study period or entered their first program at least five years after the event year. The stacked dataset is the aggregation of these event-specific panels. The following regression is then estimated on the stacked dataset:

$$Y_{its} = (Treat_{is} \times Post_{ts})\beta + X_{its}\delta + \mu_{is} + \gamma_{ts} + \varepsilon_{its} \quad (1)$$

Index *i* indicates country, *t* indicates year, and *s* indicates the event. *Y* is the poverty headcount ratio. β is the key quantity of interest as an estimate of the average impact of IMF programs on poverty incidence in participating countries. *Treat* indicates if the country is assigned to the treatment group with respect to event s. *Post* indicates if the year is after the event year s. μ and γ are country- and year-fixed effects, respectively, that are specific to event s. ε is a stochastic error term.

To test whether the IMF's incorporation of an anti-poverty mandate after 1999 and the subsequent inclusion of social protection conditions in IMF programs have made any difference, we add into our stacked regression specification a variable that counts the cumulative number of social protection and poverty conditions to which the country has been subjected. To assess the relationship between IMF program stringency and poverty, we replace the interaction of *Treat* and *Post* in Eq. (1) with variables that count the cumulative number of IMF (binding) conditions that country *i* has been subjected to by year *t*. Binding conditions are the types of conditions to which the IMF attaches greater importance, requiring official waivers for loan disbursement if they are fulfilled.

Besides this "static" stacked regression specification, we also employ the local projections approach to event study proposed by Dube and colleagues (2023) to estimate how the treatment effect evolves over time. This approach emphasizes the clean control criterion in the same spirit as the stacked regression model and has several important additional features such as flexibility in the definitions of the treatment and control groups and allowing us to control only for pretreatment values of the control variables, addressing the concern of post-treatment bias discussed above. Similar to the stacked regression model above, we focus on the first instances that countries entered IMF programs since 1980 and assume that participation in an IMF program is an absorbed state and irreversible. To implement this approach, we begin with a country-year panel dataset spanning 1980 and 2019 with our set of time- and country-varying control variables X_{it} and an indicator D_{it} indicating whether country *i* has ever been under an IMF program by year *t*. For each time horizon h = -*3*, 0, 1,..., 5, we estimate the following specification:

$$Y_{i,t+h} - Y_{i,t-1} = \beta^h \Delta D_{it} + \eta^h \Delta y_{i,t-1} + \sum^{3}{}_{p=0} \Delta X_{i,t-p} \Gamma^h{}_p + \mu^h{}_t + \varepsilon^h{}_{it} \qquad (2)$$

on a restricted sample that includes only (1) treatment group observations where $\Delta D_{it} = D_{it} - D_{i,t-1} = 1$ and (2) clean control group observations where $D_{i,t+h} = 0$. In other words, in each year *t*, the treated units are countries that enter IMF programs for the first time since 1980 at time *t*. The clean control units are countries that have never participated in any IMF programs by time *t*+h. $\Delta y_{i,t-1}$ is the difference in outcome between time *t-1* and *t-2*. $\Delta X_{i,t-p}$ is the difference in values of the control variables between *t-p* and *t-p-1*. μ^h is year-fixed effects and captures the global trends in outcome between *t*+h and *t-1*. β^h is the parameter of interest as the estimator of the cumulative effect of IMF programs on poverty h years after entering the program, controlling for pretreatment dynamics in outcome and control variables. The estimates for each h are collected and visualized in a single event study plot to show the dynamic path of the cumulative impact of IMF programs on poverty. We inspect differences between treatment and control group countries at the time horizon h = -3 to check for pre-existing trend differences that may precede the IMF intervention.

To test the robustness of our finding, we estimate the stacked regression models using alternative control variables, alternative definitions of the treatment group, and a wider event window with 5 pre-treatment periods and 8 post-treatment periods. Furthermore, we use Mahalanobis distance matching based on 3-year averages in the pre-treatment period of outcome and control variables to prune control observations used in our local projections event study estimation and benchmark the differences in trends between IMF participating countries and matched control countries at the pre-treatment average poverty levels. The aim of this matching exercise is to find a set of clean control observations that better match the treated observations in terms of pretreatment outcome and control variables to reduce model dependence (King and Nielsen 2019).

RESULTS

Stacked Difference-in-Differences

Using a stacked dataset built from stacking observations within an event window (3 pre-treatment years and 6 post-treatment years) from carefully selected treatment and "clean" control groups, we estimate the effect of participating in IMF programs on poverty headcounts. Table 1 reports estimates from Eq. (1), showing that participating in an IMF program leads to substantial increases in the proportion of the population living under poverty. Specifically, columns 2-4 in Table 1 show that, on average, IMF program participation leads to a 5.3 (95% CI [0.008, 0.098]) percentage point increase in the proportion of population living under $3.65/day (2017 PPP), a 5.5 (95% CI [0.012, 0.099]) percentage point increase in the proportion of population living under $6.85/day (2017 PPP), and a 3.9 (95% CI [0.008, 0.07]) percentage point in the proportion of population living under the societal poverty line. Adding the cumulative number of IMF poverty reduction conditions into our model does not significantly alter the estimates (Table 2). Our results suggest that the poverty reduction measures that the IMF has incorporated into its programs have not been effective relative to the poverty-increasing effects of other aspects of its lending programs. In Table 3, we replace the indicator for program entry with variables that count the number of (binding) conditions that the treated country has been subjected to by a given year. Results suggest that more stringent programs (with more conditions attached) led to larger increases in poverty incidence.

As robustness checks, we estimate our model in Eq. (1) using alternative sets of control variables, alternative definitions of the treatment group and control group when building the stacked dataset, and an alternative event window with longer pre- and post-treatment periods. Table A.2 shows that using alternative sets of control variables does not significantly alter our main finding. In Table A.3, we report results from estimating our model in Eq. (1) on a stacked panel dataset that uses a relaxed definition of the treatment group where, besides when they are entering an IMF program for the first time, countries also get assigned to the treatment group when they are re-entering another IMF program after not being under a program for at least 8 years prior. The estimated effects are slightly smaller than estimates in Table 1, but they remain sizable. Finally, we widen the event window used in building the stacked dataset to instead have 5 pre-treatment and 8 post-treatment periods. This results in even larger estimates (4.7 to 8.3 percentage points) of the effects of IMF programs on poverty headcounts, as shown in Table A.4.

Local Projection Event Study

Figures 2-5 plot the local projection event study estimates from Eq. (2). The dashed vertical line divides the pre-treatment periods on the left and post-treatment periods on the right. There are no substantial pre-existing trend differences ("pre-trends") between IMF program participants and control countries. Following entry into IMF programs, poverty incidence in program-participating countries, in particular the proportions of population living under $3.65 a day and $6.85 a day, significantly deteriorated relative to control countries to, respectively, 4 and 6 percentage points higher than what they would have been. As a robustness check, we use Mahalanobis-distance matching based on pre-treatment averages in outcome and control variables to prune control observations, and we benchmark the differences between the treatment and control observations at their pre-treatment average poverty levels rather than their poverty levels at relative time -1. The resulting event study estimates (Figures A.1-4) also suggest that countries that participated in IMF programs saw their poverty levels significantly worsen relative to control countries similar to them before entering the programs.

Standard Two-Way Fixed-Effects and Instrumental Variable

In contrast to our results, when we estimate a standard two-way fixed effects (TWFE) model that uses a reversible binary indicator for a country's current program participation status (1 when under a program; 0 when there is no active program) as the "treatment", controlling for country- and year-fixed effects, we find much smaller and statistically insignificant estimates (Table A.5). Hence, if researchers overlook the issues of unclean comparison and enduring treatment effects beyond treatment years and estimate the effect of IMF programs on poverty with a standard two-way fixed-effects regression, they would reach a markedly different conclusion than ours.

Using the same reversible indicator of current program participation status as the treatment variable, we employ one of the state-of-the-art compound instrumental variables (IV) derived from the interaction between the country-specific average exposure to IMF programs over the sample period and the number of countries with an IMF program in a given year as proxy for the IMF's annual budget constraint (Forster et al. 2020; Lee and Woo 2021; Stubbs et al. 2020; Vreeland 2003). Figure A.5 plots the residualized instrument and treatment variables after partialling out the covariates, showing a weak relationship between the two. In other words, the proposed instrument is a weak predictor of IMF program participation. This is confirmed by the F-statistics reported in Table A.6, which are far smaller than the conventional threshold of 10 (Stock, Wright, and Yogo 2002) and the higher threshold of 104.7 suggested by Lee et al. (2022) for the relevance criterion.[1]

As discussed previously, we believe that these state-of-the-art compound instruments are also problematic on conceptual grounds. Without a deeper understanding of how the compound instruments relate to countries' participation in IMF programs, we are unable to assess the validity and generalizability of estimates from using such instruments. Though statistically insignificant, the two-stage least-squares (2SLS) estimates reported in Table A.7, are substantially larger than the standard TWFE estimates in Table A.5 and the estimates from our preferred models. With imprecision, the IV estimates suggest that IMF programs lead to over 30 percentage-point increases in proportion of population under the $2.15 and the societal poverty lines. When the common rationale for IV approaches is to correct for any upward-bias of OLS

[1] Besides two-stage least squares estimation, a common approach to addressing the issue of selection due to unobservable factors in the literature on IMF programs is to use some variant of the Heckman selection model to take account of the qualities that make countries prone to participating in IMF programs (Oberdabernig 2013; Stubbs et al. 2022; Vreeland 2003). As such approaches also depend on having a valid instrument, they face similar issues mentioned in our discussion of IV approaches such as weak instruments.

from unobservable confounders, that the IV estimates are much larger than OLS estimates should cast further doubts on the validity of the compound instrument. While the observational nature of our main analysis implies that we cannot rule out the presence of unobservable or omitted variables confounding the relationship between IMF programs and poverty that we observe in our main analysis, we believe that our research design presents a more reliable approach than existing IV approaches to evaluating the impact of IMF programs.

DISCUSSION

This study revisits a long-standing debate on the relationship between IMF lending programs and poverty. Specifically, taking a novel methodological approach in our statistical analysis, we test the competing hypotheses surrounding the causal effect of IMF lending programs on poverty incidence in participant countries and whether the supposedly pro-poor "social conditions" included in its programs in recent decades made any difference. We depart from past work on IMF programs in our assumption that participating in IMF programs may have enduring effects beyond programs' active years; that is, we assume that program participation is an absorbed state that is irreversible. Benefitting from advances in the methodological literature, our focus on first-time entries and our use of the stacked regression and the local projections approaches to difference-in-differences address important methodological issues in previous studies related to (1) unclean comparison (i.e. using observations from countries previously exposed to IMF programs as control observations to be compared with newly treated observations) and (2) post-treatment bias that may arise when evaluating the impact of something as wide-reaching as IMF programs.

Our findings provide overwhelming support for the dependency/world-systems approach hypothesis: they consistently show that IMF programs lead to substantial increases in poverty, particularly in the shares of the population living under the international poverty lines at $3.65 and $6.85 (2017 PPP) a day and the societal poverty line. Our estimates also suggest that the poverty reduction measures incorporated in IMF programs in more recent decades have not made any significant difference to programs' overall poverty-increasing effects. While the observational nature of our analysis means that we cannot rule out the existence of unobserved or omitted confounders, our substantive finding — that IMF programs increase poverty — is robust across alternative model specifications and alternative definitions of the treatment group and the event window. In the absence of an instrumental variable that credibly satisfies the strong assumptions required for identification, we present a reliable approach to evaluating the impact of IMF programs that focuses on generating clean comparisons between treatment and control countries based on their histories of (non)exposure to IMF programs and their political and economic circumstances preceding program entry.

We note two limitations to our study. Our results are based on monetary measures of poverty that do not take into account the impact of IMF programs on other dimensions of poverty, such as access to and quality of government services, health, and education. Moreover, if poverty statistics suffer a decline in quality as a result of IMF programs, leading to a further undercounting of the poor, it would lead to a downward bias in our estimates. In other words, these limitations suggest that our results are likely to underestimate the impact of IMF programs on poverty. Nevertheless, our results provide a robust confirmation of previous studies that found significant adverse effects of IMF programs on poverty. Our results also call into question the efficacy of anti-poverty measures that the IMF adopted in recent decades. The evidence presented in this article demonstrates that, overall, IMF programs have been detrimental to the welfare of vulnerable populations in participant countries, and our findings call for IMF lending programs to be reformed to prevent further harm.

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Tables

Table 1. Stacked Regression Estimates

	Proportion of Population Below Poverty Lines (2017 PPP)			
	$2.15	$3.65	$6.85	SPL
IMF Program	0.043	0.053*	0.055*	0.039*
	(0.022)	(0.023)	(0.022)	(0.015)
GDP Per Capita Growth Rate	-0.106***	-0.242***	-0.249***	-0.102***
	(0.017)	(0.059)	(0.065)	(0.018)
Banking Crisis	-0.016	-0.018	-0.018	-0.015
	(0.012)	(0.013)	(0.014)	(0.009)
Polity 2 Index	-0.001	-0.002	-0.001	-0.001
	(0.001)	(0.002)	(0.002)	(0.001)
Armed Conflict	0.014	0.033	0.042	0.018
	(0.011)	(0.020)	(0.033)	(0.007)
Adjusted R-squared (full model)	0.965	0.959	0.942	0.966
Observations	2947	2947	2938	2947
RMSE	0.048	0.061	0.062	0.032
Number of Countries	57	57	56	57

*p < .05; **p < .01; p < .001

Note: The outcome variable is the proportion of population in the country living under a given international poverty line. Each model includes event-specific country- and year-fixed effects. Standard errors are robust and clustered at the country level. The treatment variable *IMF Program* is the interaction of the indicator of being in the treatment group (i.e. entering a program for the first time in the event year) and the indicator of post-treatment years.

Table 2. Stacked Regression Estimates, Controlling for Poverty Conditions

	Proportion of Population Below Poverty Lines (2017 PPP)			
	$2.15	$3.65	$6.85	SPL
IMF Program	0.041	0.047*	0.049*	0.037*
	(0.022)	(0.022)	(0.021)	(0.015)
Cumulative Binding Poverty Reduction Conditions	0.036	0.081*	0.099**	0.034*
	(0.023)	(0.027)	(0.022)	(0.015)
GDP Per Capita Growth Rate	-0.093***	-0.222***	-0.231**	-0.093***
	(0.015)	(0.06)	(0.071)	(0.018)
Banking Crisis	-0.015	-0.018	-0.016	-0.015
	(0.012)	(0.012)	(0.014)	(0.009)
Polity 2 Index	-0.000	-0.002	-0.001	-0.000
	(0.001)	(0.002)	(0.002)	(0.001)
Armed Conflict	0.009	0.025	0.039	0.015*
	(0.009)	(0.019)	(0.035)	(0.007)
Adjusted R-squared (full model)	0.968	0.962	0.945	0.968
Observations	2904	2904	2895	2904
RMSE	0.047	0.058	0.061	0.033
Number of Countries	57	57	56	57

*p < .05; **p < .01; p < .001

Note: The outcome variable is the proportion of population in the country living under a given international poverty line. Each model includes event-specific country- and year-fixed effects. Standard errors are robust and clustered at the country level. The treatment variable *IMF Program* is the interaction of the indicator of being in the treatment group (i.e. entering a program for the first time in the event year) and the indicator of post-treatment years. Also included in the models is the cumulative number of binding conditions related to poverty reduction.

Table 3. Stacked Regression Estimates, By Total Conditions

	Proportion of Population Below Poverty Lines (2017 PPP)			
	$3.65	$3.65	$6.85	$6.85
Cumulative Conditions	0.00036*		0.00045**	
	(0.00014)		(0.00016)	
Cumulative Binding Conditions		0.00051*		0.00066**
		(0.00019)		(0.00023)
GDP Per Capita Growth Rate	-0.22536***	-0.22540***	-0.23570**	-0.023645**
	(0.05950)	(0.05976)	(0.07071)	(0.07063)
Banking Crisis	-0.007	-0.00753	-0.00398	-0.00444
	(0.01246)	(0.01249)	(0.01284)	(0.01292)
Polity 2 Index	-0.00149	-0.00136	-0.0011	-0.00097
	(0.00207)	(0.00205)	(0.00224)	(0.00221)
Armed Conflict	0.02623	0.02637	0.04015	0.04028
	(0.01821)	(0.01821)	(0.03374)	(0.03367)
Adjusted R-squared (full model)	0.96168	0.96166	0.94525	0.94540
Observations	2904	2904	2895	2895
RMSE	0.05838	0.05840	0.06043	0.06035
Number of Countries	57	57	56	56

*p < .05; **p < .01; p < .001

Note: The outcome variable is the proportion of population in the country living under a given international poverty line. Each model includes event-specific country- and year-fixed effects. Standard errors are robust and clustered at the country level. The treatment variable is the cumulative number of (binding) conditions that the country has been subjected to.

Figures

Figure 1. Illustration of the Stacked Dataset

Note: This figure illustrates the procedure for building the stacked dataset. We use Armenia, Greece, and Finland to provide an illustrative example. Armenia entered its first IMF program in 1995. Greece entered its first IMF program in 2010. Finland never participated in an IMF program. The upper panel represents a panel dataset built for the event where countries entered their first IMF programs in 1995, and the lower panel is for the event in 2010. Each color-coded block represents the time series data for a given country (with start and end years as marked) that is included in the event-specific panel. Each cell within a block represents a year of data. Countries assigned to the treatment group for a given event are shown in blue, and control group countries are shown in red. The event window is defined to include observations from three years prior and five years after the event year. Event-specific panels such as these two are bound into a single dataset to form the stacked dataset.

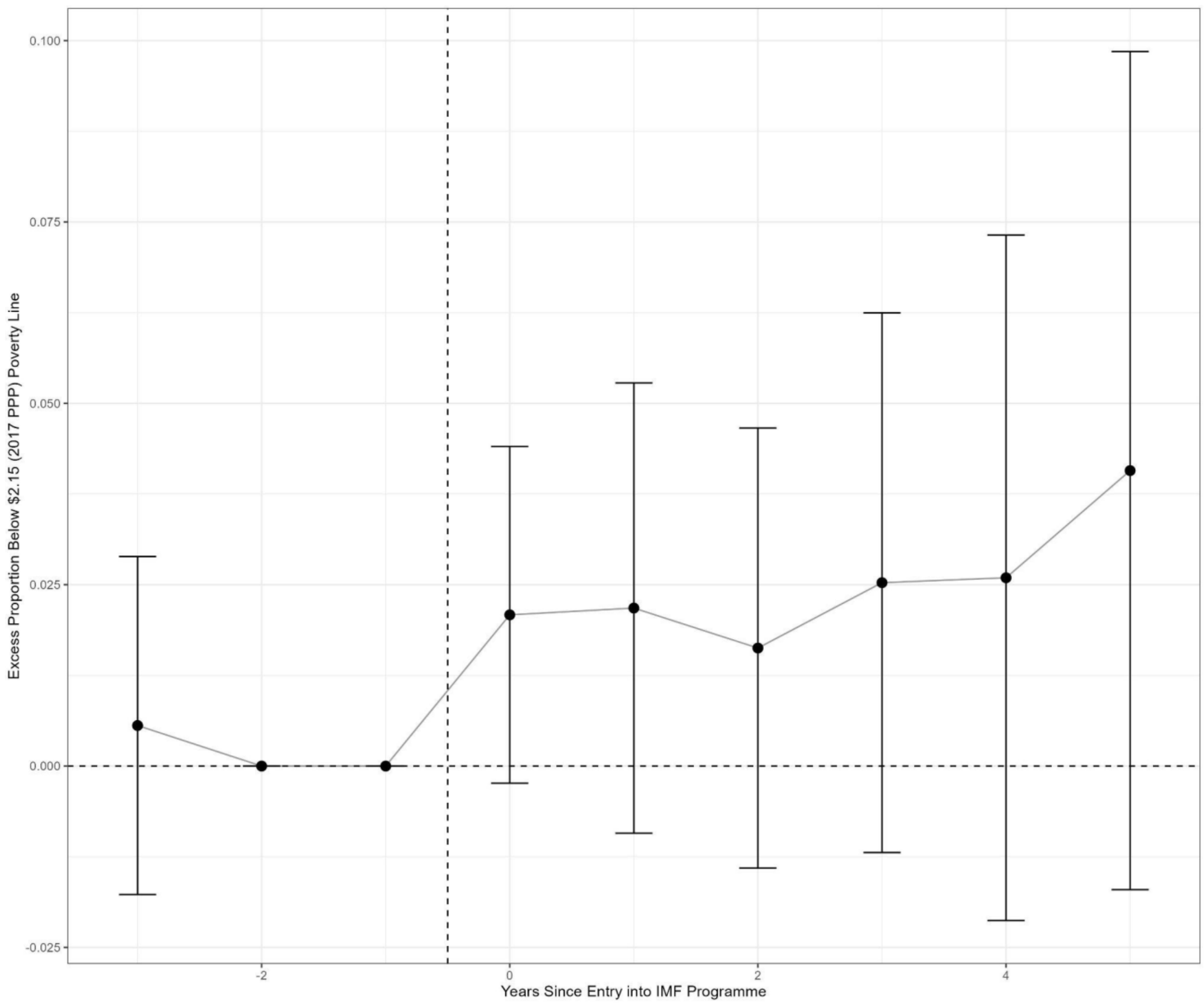

Figure 2. Local Projection Event Study (Headcount ratio $2.15)

Note: The figure plots our estimates (vertical axis) of how the impact of IMF programs on poverty incidence changes over time. Time (years) relative to IMF program entry runs on the horizontal axis. The outcome variable here is the proportion of population in the country living under the international poverty line of $2.15 (2017 PPP) a day. Each model includes year-fixed effects. Standard errors are robust and clustered at the country level. Error bars represent 95% confidence intervals.

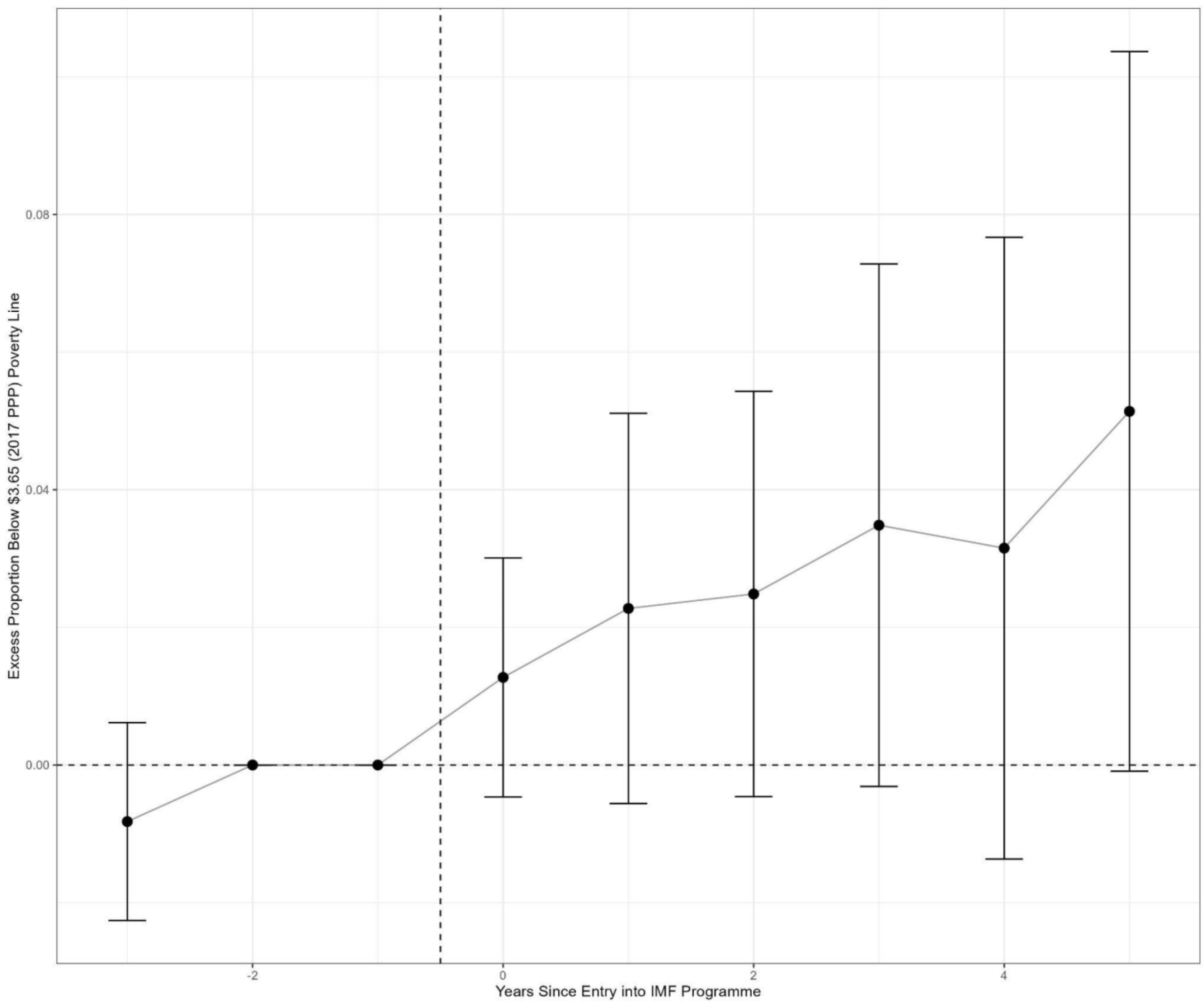

Figure 3. Local Projection Event Study (Headcount ratio $3.65)

Note: The figure plots our estimates (vertical axis) of how the impact of IMF programs on poverty incidence changes over time. Time (years) relative to IMF program entry runs on the horizontal axis. The outcome variable here is the proportion of population in the country living under the international poverty line of $3.65 (2017 PPP) a day. Each model includes year-fixed effects. Standard errors are robust and clustered at the country level. Error bars represent 95% confidence intervals.

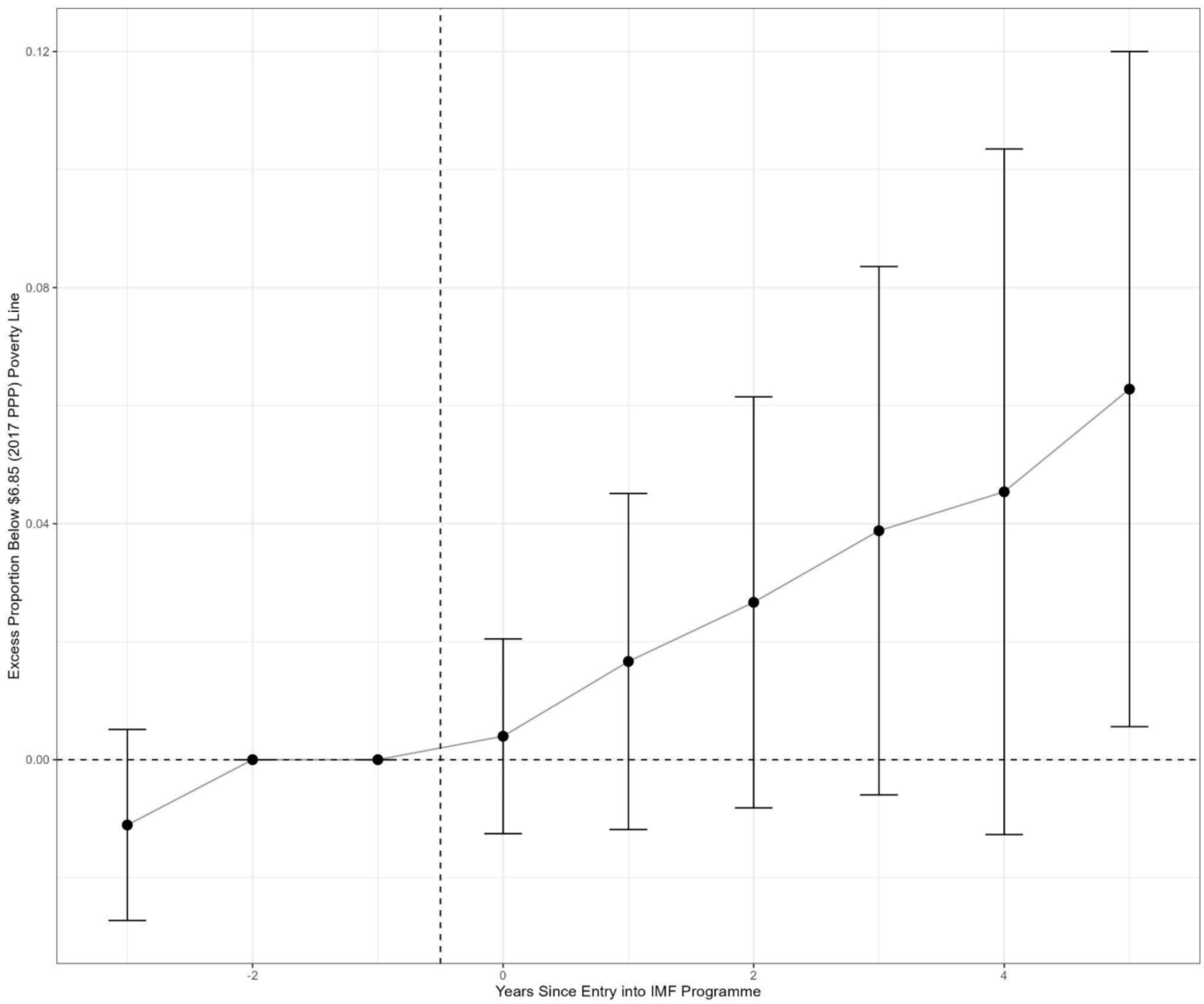

Figure 4. Local Projection Event Study (Headcount ratio $6.85)

Note: The figure plots our estimates (vertical axis) of how the impact of IMF programs on poverty incidence changes over time. Time (years) relative to IMF program entry runs on the horizontal axis. The outcome variable here is the proportion of population in the country living under the international poverty line of $6.85 (2017 PPP) a day. Each model includes year-fixed effects. Standard errors are robust and clustered at the country level. Error bars represent 95% confidence intervals.

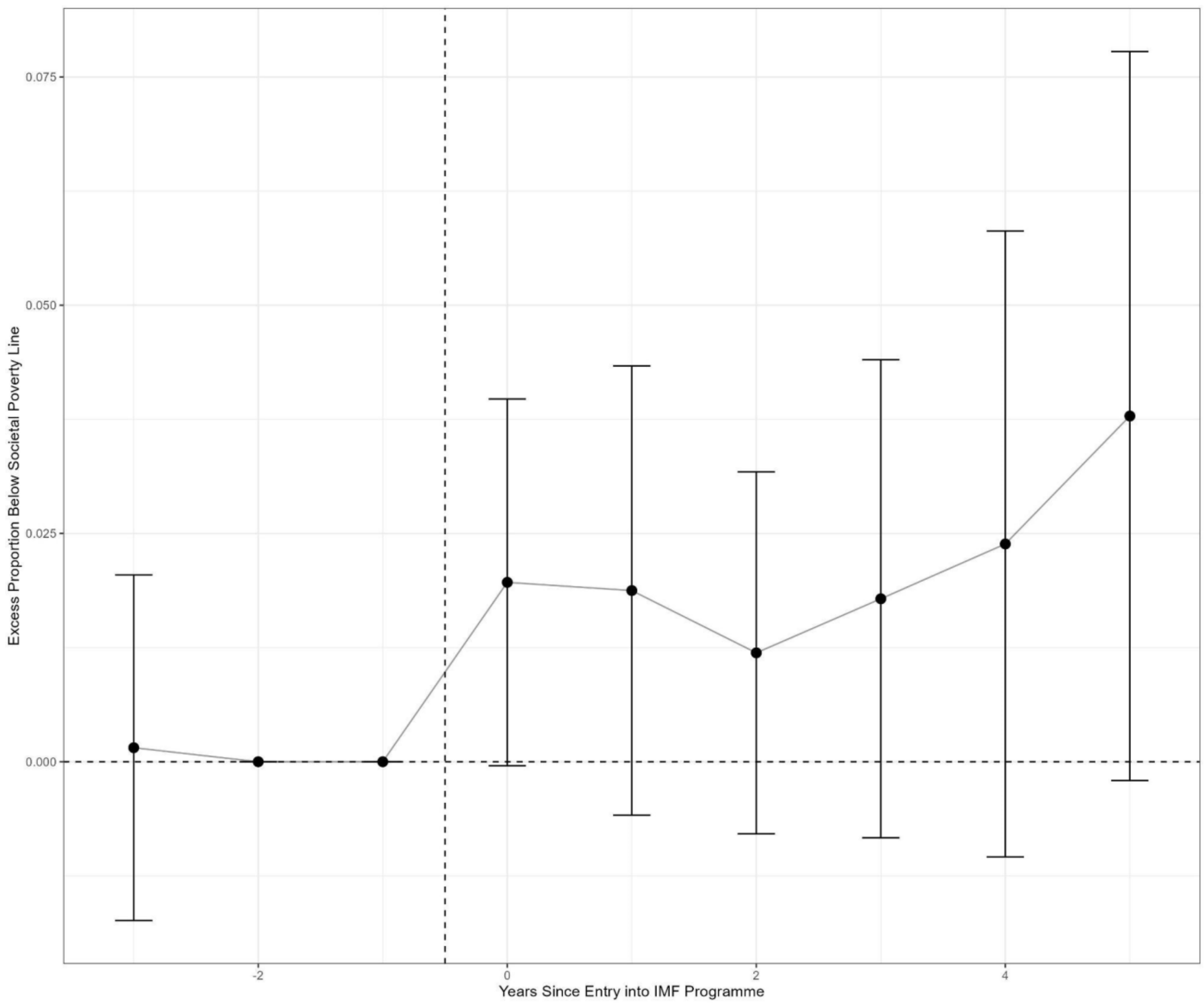

Figure 5. Local Projection Event Study (Societal Poverty Line)

Note: The figure plots our estimates (vertical axis) of how the impact of IMF programs on poverty incidence changes over time. Time (years) relative to IMF program entry runs on the horizontal axis. The outcome variable here is the proportion of population in the country living under the societal poverty line. Each model includes year-fixed effects. Standard errors are robust and clustered at the country level. Error bars represent 95% confidence intervals.

Appendix A. Data Source and Robustness Tests

Table A.1. Data Source

Variable	Source
Headcount ratios under \$2.15, \$3.65, \$6.85, and the Societal Poverty Line (2017 PPP)	Poverty and Inequality Platform (World Bank 2024)
IMF Program Participation and Number of Conditions	Authors' construction using data from Kentikelenis and Stubbs (2023)
GDP per capita growth rate	Authors' construction using GDP per capita data from the World Development Indicators database (World Bank 2023)
Banking Crisis	Laeven and Valencia (2018)
Total Reserves Minus Gold	International Financial Statistics (International Monetary Fund 2024)
Polity 2 Index	Polity5 Annual Time-Series, 1946-2018, Center for Systemic Peace (Marshall and Gurr 2020)
Coup d'etat indicator	Coups d'etat 1950 to Present Dataset by Powell and Thyne (2011)

Armed Conflict	UCDP/PRIO Armed Conflict Dataset (Davies, Pettersson, and Öberg 2023; Gleditsch et al. 2002)

Note: Each observation from these datasets is at the country-year level. These datasets were merged into a single panel dataset and then used to construct the stacked dataset used in our main analysis.

Table A.2. Stacked Regression Estimates, Alternative Set of Control Variables

	Proportion of Population Below Poverty Lines (2017 PPP)			
	SPL	$6.85	SPL	$6.85
IMF Program	0.039*	0.054*	0.036*	0.046*
	(0.018)	(0.021)	(0.017)	(0.022)
GDP Per Capita Growth Rate	-0.093***	-0.234***		
	(0.014)	(0.061)		
Total Reserves Minus Gold	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Banking Crisis			-0.000	0.017
			(0.014)	(0.015)
Polity 2 Index	-0.001	-0.002	-0.001	-0.002
	(0.001)	(0.002)	(0.001)	(0.002)
Coup d'etat	0.022	0.067	0.032	0.091
	(0.021)	(0.056)	(0.025)	(0.070)
Adjusted R-squared (full model)	0.971	0.950	0.969	0.934
Observations	2173	2173	2295	2295
RMSE	0.032	0.057	0.033	0.066
Number of Countries	51	51	52	52

*p < .05; **p < .01; p < .001

Note: The outcome variable is the proportion of population in the country living under a given international poverty line. Each model includes event-specific country- and year-fixed effects. Standard errors are robust and clustered at the country level. The treatment variable *IMF Program* is the interaction of the indicator of being in the treatment group (i.e. entering a program for the first time in the event year) and the indicator of post-treatment years.

Table A.3. Stacked Regression Estimates, Relaxed Definition of Treatment Group

	Proportion of Population Below Poverty Lines (2017 PPP)			
	$2.15	$3.65	$6.85	SPL
IMF Program	0.033	0.044*	0.040*	0.028*
	(0.018)	(0.018)	(0.017)	(0.013)
GDP Per Capita Growth Rate	-0.094***	-0.214**	-0.220**	-0.091***
	(0.024)	(0.067)	(0.072)	(0.022)
Banking Crisis	-0.014	-0.016	-0.014	-0.013
	(0.012)	(0.012)	(0.013)	(0.009)
Polity 2 Index	-0.001	-0.003	-0.002	-0.001
	(0.001)	(0.002)	(0.002)	(0.001)
Armed Conflict	0.016	0.033	0.039	0.019*
	(0.011)	(0.020)	(0.029)	(0.008)
Adjusted R-squared (full model)	0.962	0.956	0.946	0.964
Observations	3493	3493	3484	3493
RMSE	0.050	0.063	0.062	0.035
Number of Countries	69	69	68	69

*p < .05; **p < .01; p < .001

Note: The outcome variable is the proportion of population in the country living under a given international poverty line. Each model includes event-specific country- and year-fixed effects. Standard errors are robust and clustered at the country level. The treatment variable *IMF Program* is the interaction of the indicator of being in the treatment group and the indicator of post-treatment years. The stacked dataset used here adopts a more relaxed definition of the treatment group by allowing past program participants that haven't been exposed to IMF programs for at least 8 years to be assigned to the treatment group if they participate again.

Table A.4 Stacked Regression Estimates, Wider Event Window

	Proportion of Population Below Poverty Lines (2017 PPP)			
	$2.15	$3.65	$6.85	SPL
IMF Program	0.055	0.075*	0.084**	0.047*
	(0.031)	(0.033)	(0.031)	(0.022)
GDP Per Capita Growth Rate	-0.066	-0.189*	-0.194*	-0.068*
	(0.034)	(0.077)	(0.078)	(0.031)
Banking Crisis	-0.019	-0.026	-0.026	-0.021
	(0.018)	(0.015)	(0.017)	(0.013)
Polity 2 Index	-0.003	-0.005*	-0.004	-0.002
	(0.002)	(0.002)	(0.003)	(0.001)
Armed Conflict	0.030	0.060	0.064	0.030*
	(0.021)	(0.036)	(0.043)	(0.015)
Adjusted R-squared (full model)	0.930	0.926	0.914	0.931
Observations	3739	3739	3728	3739
RMSE	0.065	0.078	0.075	0.046
Number of Countries	57	57	57	57

*p < .05; **p < .01; p < .001

Note: The outcome variable is the proportion of population in the country living under a given international poverty line. Each model includes event-specific country- and year-fixed effects. Standard errors are robust and clustered at the country level. The treatment variable *IMF Program* is the interaction of the indicator of being in the treatment group (i.e. entering a program for the first time in the event year) and the indicator of post-treatment years. For the stacked dataset used in these models, we define the event window to be wider, with 5 pre-treatment periods and 8 post-treatment periods.

Table A.5. Standard Two-Way Fixed-Effects with Reversible Treatment

	Proportion of Population Below Poverty Lines (2017 PPP)			
	$2.15	$3.65	$6.85	SPL
Current IMF Program Participation	-0.001	0.015	0.021*	-0.001
	(0.008)	(0.008)	(0.010)	(0.006)
GDP Per Capita Growth Rate	-0.098	-0.092	-0.049	-0.086*
	(0.051)	(0.058)	(0.060)	(0.035)
Banking Crisis	-0.017	-0.026**	-0.028*	-0.011
	(0.010)	(0.009)	(0.014)	(0.008)
Polity 2 Index	0.001	0.002	0.003*	0.001
	(0.002)	(0.002)	(0.002)	(0.001)
Armed Conflict	0.036*	0.039*	0.015	0.020
	(0.016)	(0.015)	(0.013)	(0.011)
Adjusted R-squared (full model)	0.856	0.894	0.875	0.858
Observations	3162	3162	3150	3162
RMSE	0.104	0.102	0.096	0.073
Number of Countries	104	104	104	104

*p < .05; **p < .01; p < .001

Note: The outcome variable is the proportion of population in the country living under a given international poverty line. Instead of using a stacked dataset, these models are estimated on more conventional country-year panel dataset. Each model includes country- and year-fixed effects. Standard errors are robust and clustered at the country level. The treatment variable *Current IMF Program Participation* is an indicator of whether the country is currently under an active IMF program or not.

Table A.6 Instrumental Variable First Stage Regression

	Current IMF Program Participation
ZPRO	0.010**
	(0.003)
GDP Per Capita Growth Rate	-0.094
	(0.129)
Banking Crisis	0.161***
	(0.039)
Polity 2 Index	0.009*
	(0.004)
Armed Conflict	0.001
	(0.042)
Adjusted R-squared (full model)	0.400
Observations	5586
RMSE	0.370
Number of Countries	163
Effective F-statistic	0.9989
Robust F-statistic	4.0801

*p < 0.05; **p<0.01; p<0.001

Note: The outcome variable is an indicator of whether the country is currently under an active IMF program or not. The instrumental variable *ZPRO* is the interaction between the country's average IMF program exposure over the study period with each year's total number of countries under an IMF program. Each model includes country- and year-fixed effects. Standard errors are robust and clustered at the country level.

Table A.7. Two-Stage Least Square Estimates with Compound Instrument

	Proportion of Population Below Poverty Lines (2017 PPP)			
	$2.15	$3.65	$6.85	SPL
Current IMF Program Participation	0.370	0.193	-0.215	0.324
	(0.558)	(0.471)	(0.531)	(0.426)
GDP Per Capita Growth Rate	-0.192	-0.137	0.012	-0.167
	(0.154)	(0.126)	(0.177)	(0.123)
Banking Crisis	-0.074	-0.054	0.008	-0.061
	(0.085)	(0.072)	(0.085)	(0.066)
Polity 2 Index	-0.005	-0.001	0.007	-0.004
	(0.010)	(0.008)	(0.009)	(0.007)
Armed Conflict	0.036	0.039*	0.015	0.020
	(0.024)	(0.017)	(0.018)	(0.020)
Adjusted R-squared (full model)	0.555	0.842	0.751	0.400
Observations	3162	3162	3159	3162
RMSE	0.182	0.124	0.135	0.150
Number of Countries	104	104	104	104

*p < .05; **p < .01; p < .001

Note: The outcome variable is the proportion of population in the country living under a given international poverty line. These models are estimated on a country-year panel dataset. Each model includes country- and year-fixed effects. Standard errors are robust and clustered at the country level. The treatment variable *Current IMF Program Participation* is an indicator of whether the country is currently under an active IMF program or not. Program participation is instrumented with the interaction between the country’s average IMF program exposure over the study period with each year’s total number of countries under an IMF program.

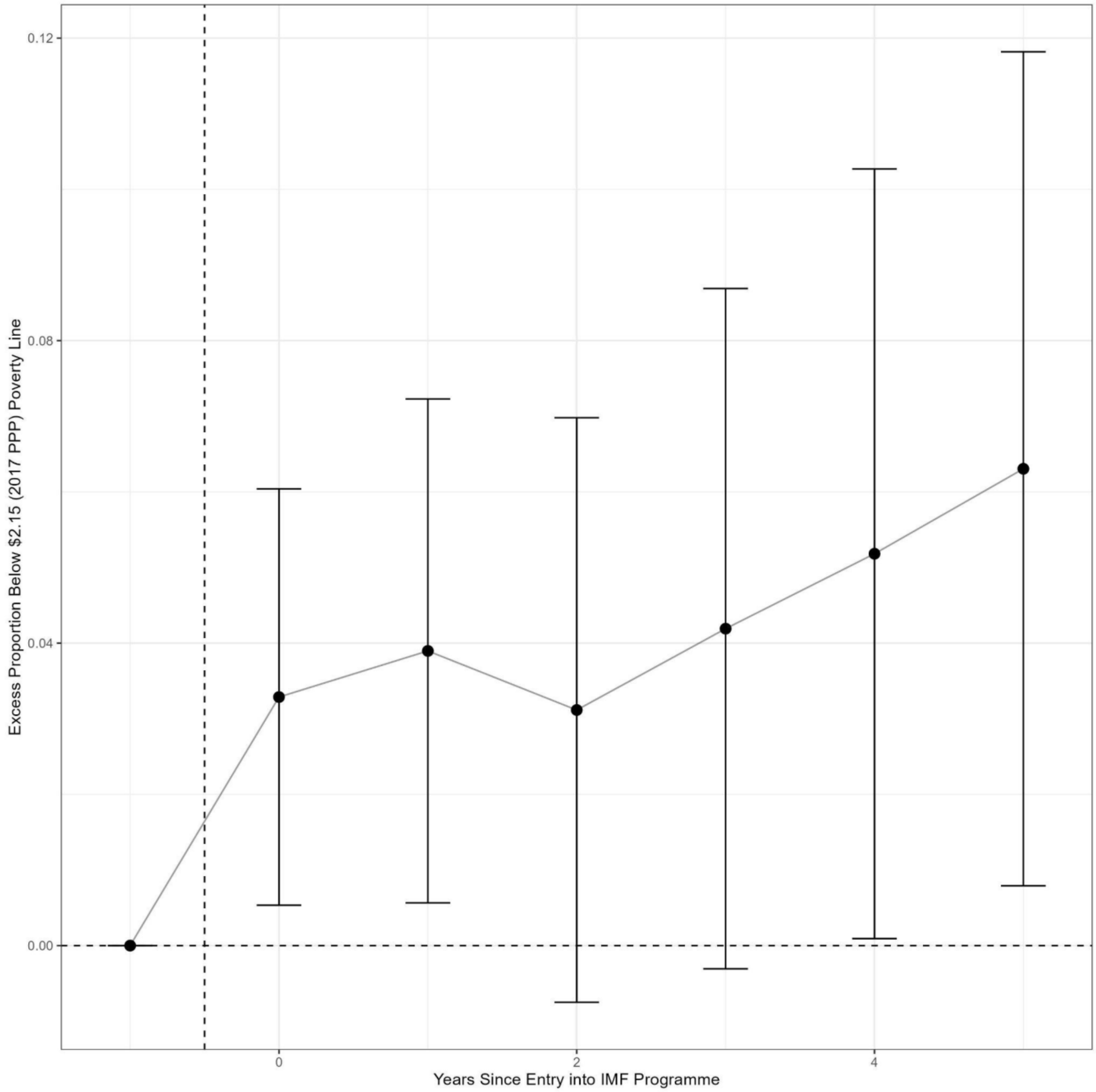

Figure A.1 Local Projection Event Study, Matched Sample (Headcount ratio $2.15)

Note: The figure plots our estimates (vertical axis) of how the impact of IMF programs on poverty incidence changes over time. Time (years) relative to IMF program entry runs on the horizontal axis. The pre-treatment benchmark (left of the vertical dashed line) is set to the 3-year average before the event. The outcome variable here is the proportion of population in the country living under the international poverty line of $2.15 (2017 PPP) a day. Each model includes year-fixed effects. Standard errors are robust and clustered at the country level. Error bars represent 95% confidence intervals.

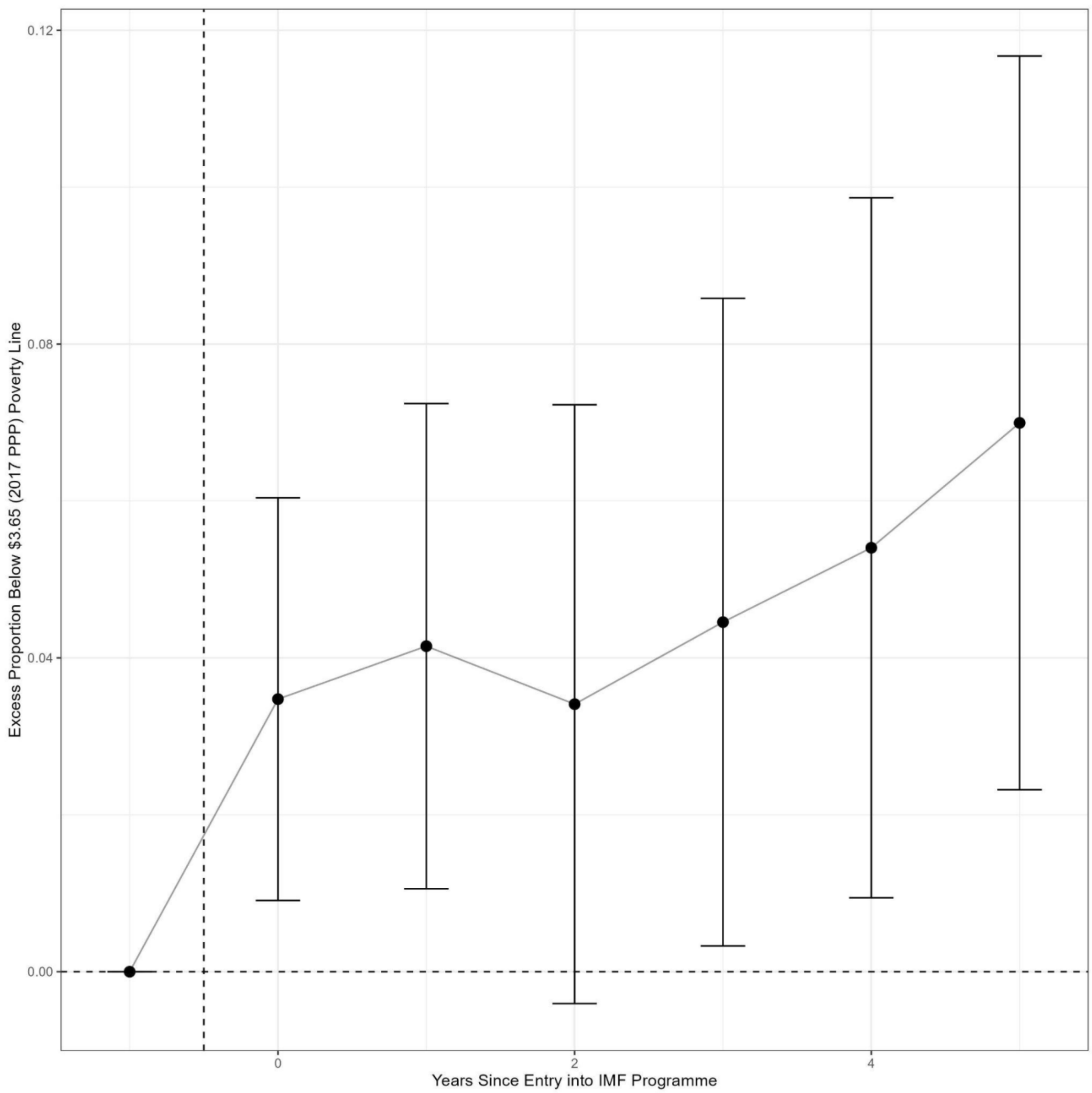

Figure A.2 Local Projection Event Study, Matched Sample (Headcount ratio $3.65)

Note: The figure plots our estimates (vertical axis) of how the impact of IMF programs on poverty incidence changes over time. Time (years) relative to IMF program entry runs on the horizontal axis. The pre-treatment benchmark (left of the vertical dashed line) is set to the 3-year average before the event. The outcome variable here is the proportion of population in the country living under the international poverty line of $3.65 (2017 PPP) a day. Each model includes year-fixed effects. Standard errors are robust and clustered at the country level. Error bars represent 95% confidence intervals.

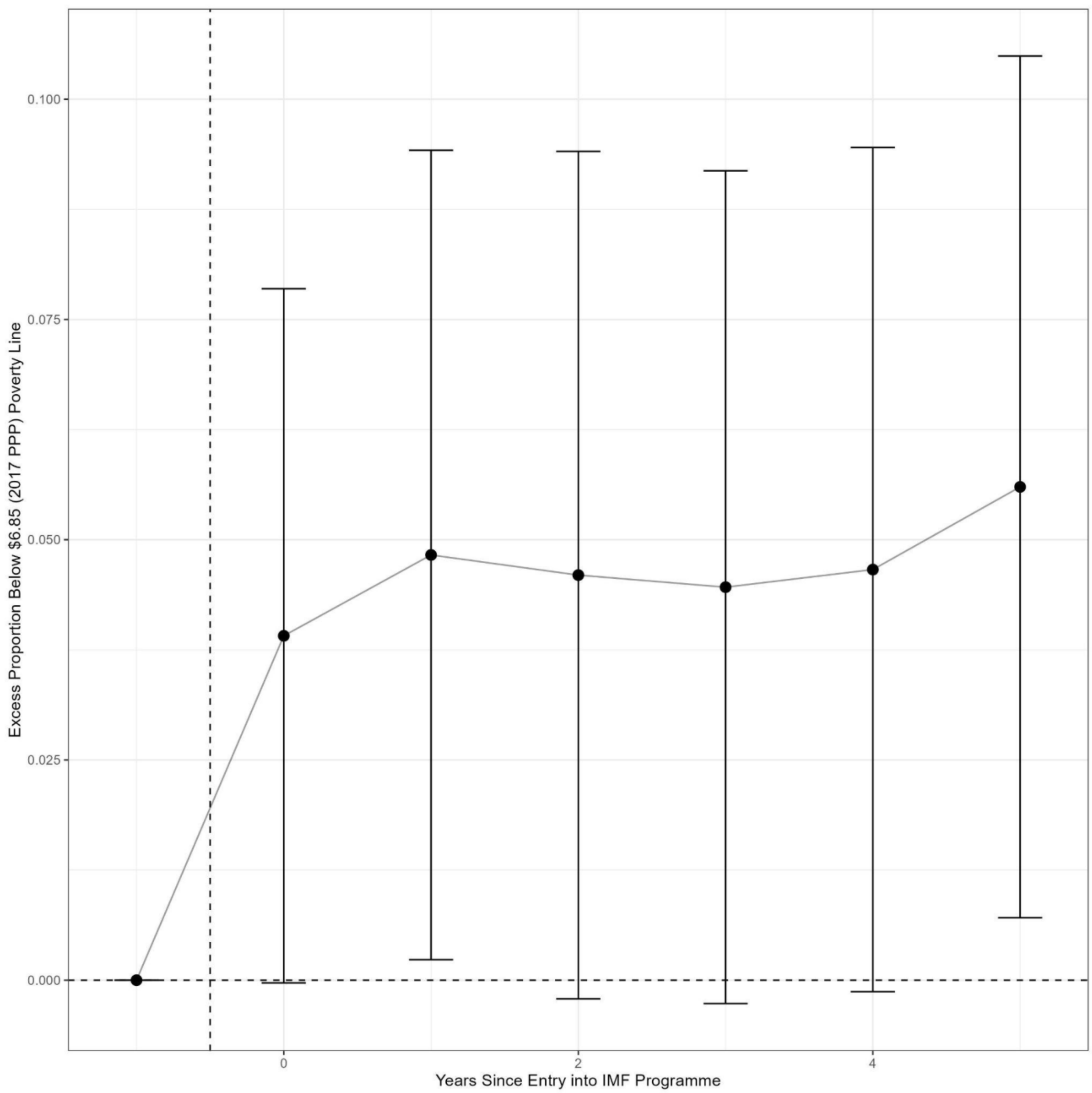

Figure A.3 Local Projection Event Study, Matched Sample (Headcount ratio $6.85)

Note: The figure plots our estimates (vertical axis) of how the impact of IMF programs on poverty incidence changes over time. Time (years) relative to IMF program entry runs on the horizontal axis. The pre-treatment benchmark (left of the vertical dashed line) is set to the 3-year average before the event. The outcome variable here is the proportion of population in the country living under the international poverty line of $6.85 (2017 PPP) a day. Each model includes year-fixed effects. Standard errors are robust and clustered at the country level. Error bars represent 95% confidence intervals.

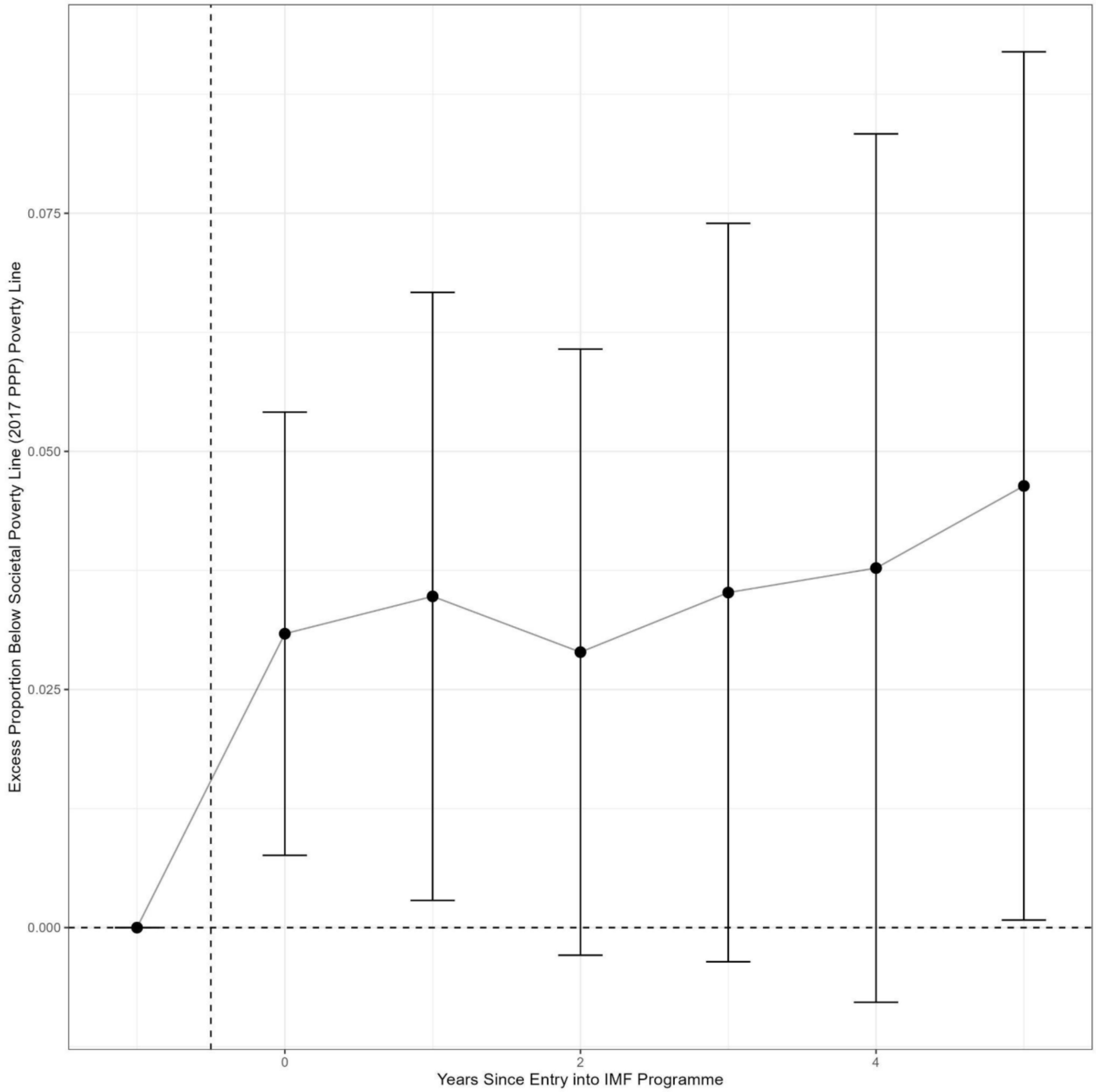

Figure A.4 Local Projection Event Study, Matched Sample (Societal Poverty Line)

Note: The figure plots our estimates (vertical axis) of how the impact of IMF programs on poverty incidence changes over time. Time (years) relative to IMF program entry runs on the horizontal axis. The pre-treatment benchmark (left of the vertical dashed line) is set to the 3-year average before the event. The outcome variable here is the proportion of population in the country living under the societal poverty line. Each model includes year-fixed effects. Standard errors are robust and clustered at the country level. Error bars represent 95% confidence intervals.

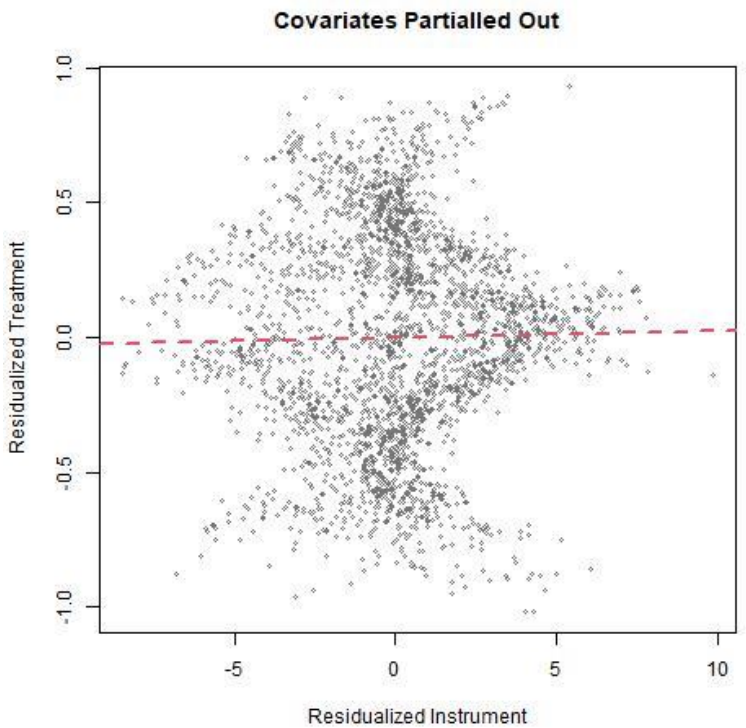

Figure A.5 Visualizing the First Stage Using the Compound Instrument

Note: The figure visualizes the first stage relationship by plotting the residualized treatment (current IMF program participation status) against the residualized instrument (the interaction between the country's average IMF program exposure over the study period with each year's total number of countries under an IMF program) after partialling out the covariates (GDP per capita growth rate, banking crisis indicator, polity2 index, and armed conflict indicator) and fixed effects. A linear regression line (dashed line) regressing the residualized treatment on residualized instrument is plotted.