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Summary

Background

In 2015, 193 nations committed to UN Sustainable Development Goal 3·2, which aims to reduce child mortality rates by 2030. Despite progress, challenges remain, especially in low- and middle-income countries. International Monetary Fund (IMF) lending programmes may influence these outcomes through structural adjustments which prioritise macroeconomic stability over public spending. In this study, we examine the impact of IMF interventions on neonatal mortality, addressing concerns about existing analyses with a novel statistical approach.

Methods

We used neonatal mortality data from 1985-2018 across 106 countries, controlling for economic, political, and social factors. Our analysis employed a "stacked regression" approach, focusing on first-time entries into IMF programmes to avoid biases from repeated programme entries and exits. We also assessed dose-response relationships between IMF programme stringency and neonatal mortality. Finally, we explored possible causal pathways and conducted tests of robustness using alternative models and definitions.

Findings First-time participation in an IMF programme was associated with a 5·8% increase in neonatal mortality rates, a finding that was robust across different model specifications and control group definitions. We also found a dose-response relationship, with each additional IMF condition increasing neonatal mortality by 0·032% on average. IMF programmes were linked to declines in GDP per capita, government expenditure, and healthcare workforce size, suggesting these as possible causal pathways. Our findings are consistent with earlier studies on both child mortality outcomes and possible mechanisms linking them to IMF programmes, which we summarise in the online appendix.

Interpretation IMF policies may hinder progress toward the UN's child mortality goals, indicating an urgent need to reconsider the features of these programmes.

Keywords: International Monetary Fund, structural adjustment, mortality, child health

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RESEARCH IN CONTEXT

Evidence before this study

The International Monetary Fund (IMF) provides financial support to countries in economic distress, requiring them to implement bundled packages of policy reforms. There have been longstanding concerns about the impact of these IMF programmes on health outcomes, including on neonatal mortality. We conducted a literature review of articles published from January 1, 2000, to June 1, 2023, across multiple databases including MEDLINE, Scopus, and Web of Science, using search terms related to IMF programme participation (IMF OR International Monetary Fund OR structural adjustment program OR SAP) and health outcomes (health OR health outcomes OR mortality OR neonatal mortality OR infant mortality OR child mortality). In a secondary search, the reference lists of all studies identified in our primary search were also reviewed. We selected only quantitative analyses attempting to draw causal inferences about the impact of programme participation on health outcomes, published in peer-reviewed journals. Findings of these earlier studies show connections between IMF programmes and negative health outcomes, including increased neonatal, infant and under-five mortality rates, higher maternal mortality rates and higher tuberculosis and suicide mortality rates, along with higher child malnutrition and tuberculosis prevalence, increased disability-adjusted life years, and decreased child vaccination rates. We found only one study showing an indirect link between programmes and *reduced* child mortality, through economic growth. Despite the consistency of these findings, establishing causation remains challenging due to the problem of confounding and selection bias. Many studies have attempted to use fixed-effects panel regressions and instrumental variable approaches to deal with this, but concerns remain about unobserved time-varying confounders, instrument validity, and staggered policy adoption.

Added value of this study

This study investigates the impact of IMF programme participation on neonatal mortality rates per 1,000 live births in 106 countries from 1985–2018, controlling for macroeconomic factors, political conditions, and female education levels. We depart from conventional analyses through our use of a stacked regression approach that focuses on a window of years surrounding initial IMF programme entry. This approach assumes that joining an IMF programme may exert enduring effects beyond the programme's active years. It ensures that only countries without any prior IMF programme experience—clean controls—are part of the sample. We find that first-time participation in an IMF programme is associated with a 5.8% increase in the neonatal mortality rate, a finding that is robust to a variety of alternative specifications. Further, we found a dose-response relationship: the greater the number of IMF conditions, the higher the neonatal mortality rate. Exploring potential mechanisms, we find that IMF programmes are associated with lower per capita GDP, decreased government spending, and a diminished healthcare workforce. Finally, our review of quantitative and qualitative studies on the mechanisms through which IMF programmes can lead to adverse health outcomes adds support to our finding.

Implications of all the available evidence

The finding of the present study that IMF programme participation is associated with increased neonatal mortality rates joins an extensive body of research linking IMF programmes with negative health outcomes. To safeguard population health, IMF programmes require reforms to ensure that such adverse effects do not continue to occur in participating nations.

Introduction

In 2015, 193 nations embraced Target 3·2 of the UN Sustainable Development Goals, which seeks to halt preventable deaths of newborns and children under the age of five by 2030. Specifically, these nations pledged to reduce neonatal mortality rates to 12 or fewer per 1,000 live births and under-five mortality rates to 25 or fewer per 1,000 live births. Despite concerted global health efforts in recent years to curb child mortality worldwide, the path to SDG 3.2 remains challenging, particularly for low- and middle-income regions.[1–3] Of particular concern is that, while strides have been made in reducing infant mortality, the decline in neonatal mortality has been notably slower.[2]

Neonatal mortality rates have a wide range of upstream institutional determinants, including local state capacities and health infrastructures. Influential international financial organisations like the International Monetary Fund (IMF) are uniquely positioned to shape these institutional determinants through their conditional lending programmes.[4] Countries across the world that are experiencing economic turmoil may turn to the IMF to secure financial support. However, fresh credit is typically issued on the condition that national governments agree to a wide variety of policy reforms, bundled into so-called structural adjustment programmes. The goal of these reforms is to ensure macroeconomic stability through reduced public spending and other related policy interventions.

Civil society groups, as well as a growing body of academic literature, have posited that structural adjustment programmes, by prioritising debt repayment over long-term public investments and by promoting free-market economic policies, may adversely affect population health outcomes. Three broad channels have been identified as central to this process.[5] First, the IMF-mandated pursuit of macroeconomic targets via extensive spending cuts and institutional reforms can directly affect healthcare infrastructures, including the quality of provision, the extent of coverage, and the day-to-day functioning of individual care units. Second, health systems can be indirectly affected by interventions in non-health-related policy domains, such as currency devaluations that increase the price of imported medicines and equipment. Third, structural adjustment can act upon the social determinants of health, including (un)employment, poverty, and reduced education expenditures. (See Table 1 for all such mechanisms.) In Appendix Table 1 Panel A we review 18 qualitative case studies on the impact of IMF programmes operating through these mechanisms, finding that 15 of them found detrimental effects of IMF programme participation, one found positive effects and two found mixed effects.

Turning to research on health outcomes, the extant quantitative literature on the effects of IMF lending programmes generally demonstrates strong adverse associations with a variety of health outcomes (see Appendix Table 2 for a review of 13 quantitative studies). However, compelling causal evidence remains scarce. The heterogeneous nature of IMF interventions across countries and time periods coupled with the heterogeneous nature of the countries themselves (with respect to 'pre-treatment' characteristics) render standard statistical models inadequate. Many existing approaches have relied on two-way fixed-effects panel regressions. However, recent advances in statistics cast doubts on these results, as explained below. Some of these analyses have also relied on instrumental variables to account for unobserved confounding variables. An instrument is a variable that influences the outcome only through its impact on the treatment variable and no other channel; in this case, a conditioning force that makes IMF intervention more likely and has no other channel of association with a country's contemporary distress. Instruments used in prior studies have proven theoretically problematic or yielded implausible results (see Appendix Table 3 for a discussion of this approach and a review of such studies).

The purpose of this paper is to address these gaps by using a novel approach. We analyse many distinct episodes of IMF intervention with explicitly selected control groups to derive credible quantitative effect estimates across 106 countries between 1985 and 2018 (in our main specification).

Methods

Data and variables

The outcome variable for our main specification is log-transformed neonatal deaths per 1,000 live births observed across 106 countries between 1985 and 2018. Neonatal mortality data are drawn from the UN Inter-agency Group for Child Mortality Estimation. Data for countries' participation in IMF programmes and programme conditionalities are taken from the IMF Monitor database. To control for macroeconomic conditions that might simultaneously influence a country's IMF participation and neonatal mortality rate, our economic covariates include log-transformed GDP per capita and a binary banking crisis indicator. To control for political conditions, our covariates include the *polity2* regime type index and a binary indicator of armed conflict incidence. Finally, we also control for the average years of formal education in a country's female population, which is an important predictor of child mortality. (See Appendix Table 4 for data sources.)

Statistical analysis

To assess the impact of IMF programme participation (the 'treatment'), a common approach in policy evaluation is to estimate two-way fixed effects (TWFE) models that isolate variation within units over time while controlling for aggregate time trends. However, recent studies in econometric theory suggest that TWFE estimates with staggered treatment timing often do not produce valid causal estimates due to the fact that already-treated units are used as effective control units compared with newly treated units, which introduces invalid comparisons in the presence of treatment effect heterogeneity and any lagged effects.[6] When treatment effects can change over time, for example, phasing in with a lag, TWFE estimates can even reverse the sign of the true treatment effect. When evaluating the impact of IMF programmes, this is further complicated by the fact that countries enter and exit programmes — sometimes at high frequency — and the fact that programmes can have long-lasting effects on participating countries beyond the years that they were under a programme. To circumvent these issues and the biases that they may entail, we implement a 'stacked regression' approach to difference-in-differences that focuses on a window around years in which countries entered an IMF programme *for the first time* since 1985, when structural adjustment programmes and conditionalities became prevalent. We define each calendar year where countries entered IMF programmes for the first time as an 'event'. For each event, we create a data set that centres around the event year and includes observations of the treated cohort and observations from not-yet and never-treated countries that serve as ‘clean controls.’[6,7] These event-specific data sets are stacked together into one. We then estimate a two-way fixed-effects model on the stacked dataset.

This approach ensures that we have only countries with no previous experiences with IMF programmes in the treatment and control groups that are being compared. It also has the virtue of not using observations where a country repeatedly entered and exited programmes to identify programme effects. In contrast to other approaches that use a reversible binary IMF participation status variable as the treatment variable, our approach views entering an IMF programme as an event that could potentially have lasting impacts beyond the years in which the country was under a programme. For each event, we define the event window to include observations between three years prior to and five years after the event year. Examining the three years preceding an IMF intervention permits accounting for trends that may precede or even engender the intervention and not mistakenly ascribing the trend to the intervention. Only events between 1988 and 2013 are included in the stacked data set to ensure that there are three years before and five years after each event year in the event-specific data sets. Observations within this window are assigned to the treatment group if they belong to countries entering an IMF programme for the first time during the event year and to the control group (as ‘clean’ controls) if they belong to countries that never entered an IMF programme throughout the study period or entered their first programme at least five years after the event year. Each intervention event thus has a panel consisting of a treated country or countries and an event-specific set of control countries. The entire dataset is the aggregation of these intervention-event panels. The following two-way fixed effects 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 log-transformed neonatal mortality rate. β is the key quantity of interest. *Treat* indicates if the country is in the treatment group with respect to event *s*. *Post* indicates if the year is after the event year s. μ and γ are event-specific country-fixed effects and year-fixed effects, respectively. ε_{its} is a stochastic error term. Besides this 'static' specification, we also use the following dynamic specification to estimate how the treatment effect changes over time:

$$Y_{its} = \sum^{5}{}_{j=-3, j\neq -1}(Treat_{is} \times 1\{t-s=j\})\beta_j + X_{its}\delta + \mu_{is} + \gamma_{ts} + \varepsilon_{its} \quad (2)$$

Index *j* indicates time relative to event s and β_j is the coefficient on the interaction of treatment group status and binary indicator of time relative to event s and represents treatment effect at relative time *j*. Following common practice for avoiding multicollinearity, we exclude the relative period -1 from the specification and normalise β_{-1} to zero.[8] We then plot the estimates of interest in an event study plot to visualise the estimated dynamic path of the treatment effect and to check if there are pre-existing differences in trends ("pre-trends") between treatment and control groups by testing the null hypothesis that β_j = 0 where *j* < 0.

To assess dose-response relationships between the stringency of IMF programmes and neonatal mortality, we extend the analysis to an alternative measure of IMF participation that takes the value of the cumulative number of conditions that the country has been subjected to since the year of entry. We argue that this provides us with a measure of the stringency of the programme and hence of the intrusiveness of the event. Finally, to probe the hypothesised mechanisms relating countries' participation in IMF programmes to population health, we conduct an analysis of potential causal pathways, including economic growth, government expenditures, and the size of the health workforce. Finally, we test the robustness of our finding by using alternative model specifications and alternative definitions of the treatment and control groups. The latter is done by relaxing the treatment group definition to include instances of entry into IMF programmes subsequent to the first as events if there are at least 8 untreated years in between and tightening the definition of the control group to exclude observations from countries in North America and Western and Northern Europe from acting as control group observations. All statistical analyses were done using R (version 4.3.0) with the *estimatr* package.

Role of the funding source

The funders of the study had no role in study design, data collection, data analysis, data interpretation, or writing of the report.

Results

Our baseline model (Table 2) indicates that participating in an IMF programme for the first time is associated on average with a 5·8% increase in the neonatal mortality rate. In contrast to the stacked regression model, a conventional two-way fixed-effects model with a reversible binary indicator for country's programme participation status, controlling for country- and year-fixed effects, yields a much smaller estimate that is statistically insignificant (Appendix Table 5). Figure 1 shows the estimated dynamic path of the treatment effect from the dynamic stacked regression model. To put our estimates in more concrete terms, we use Algeria as an example. Algeria in 1988 was on the eve of entering an IMF programme for the first time in our data set and it was near the mean of treated countries on the eve of entry in terms of population size (23·9 million), births (0·8 million), and neonatal mortality rate (24·3 per 1,000 live births). Between 1988 and 1994, Algeria's neonatal mortality declined from 24·3 to 21·6 per 1,000 live births (an 11·1% decline). If the effect of Algeria entering the programme is close to average, based on the estimated dynamic path of the treatment effect (Figure 1 and Model 1 in Appendix Table 6), neonatal mortality rates in Algeria would have fallen further to 20·5 per 1,000 live births by 1994 had it not entered the programme in 1989. Based on annual births data from Algeria, this would amount to 4,308 preventable neonatal deaths between 1989 and 1994. That the confidence intervals for the pre-event periods do not cover 0 in Figure 1 suggests that there may be pre-existing trend differences between the treatment and control countries. This is not surprising, as we would not expect countries entering IMF programmes to have improving population health. However, these pre-existing differences are no longer significant after we take account of heterogeneity in trends of neonatal mortality rate across regions (see Appendix Figure 1) and control for region-specific time trends instead of an aggregate time trend (Appendix Figure 2).

According to our baseline model using the cumulative number of IMF conditions as a measure of IMF programme stringency, each additional condition mandated by the IMF increases the country's neonatal mortality rate by 0·032% on average (Model 1 in Table 3). The mean cumulative number of conditions that treated countries carry 5 years after the year of entry is 177, which amounts to a 5·7% higher neonatal mortality rate 5 years after the year of entry. At the maximum cumulative number of conditions 5 years after the year of entry — 496 — this amounts to a 15·9% higher neonatal mortality rate. This suggests that countries' experiences differed with the stringency of the IMF programme; the more stringent programmes were, the more they worsened neonatal health. When we consider only the cumulative number of binding conditions — types of conditions to which the IMF attaches greater importance, requiring official waivers for loan disbursement if they are missed — we find that each additional binding condition increases neonatal mortality rates by 0·042% (Model 2 in Table 3). In our data set, the mean cumulative number of binding conditions that treated countries carry five years after the year of entry is 121, and the maximum is 388. These figures correspond to neonatal mortality rates that are 5·1% and 16·3% higher, respectively.

To explore possible pathways linking IMF programmes to neonatal mortality, we estimate a series of models that use a variety of outcome variables that may mediate the relationship. As shown in Table 4, we find that IMF programmes are associated significantly with a 6·9% drop in GDP per capita, a 1·8 percentage point drop in government expenditure as a percentage of GDP, a 10% drop in physicians per 1,000, and a 9% drop in nurses and midwives per 1,000. We view these only as indicative results about mechanisms that require more rigorous investigation in future research.

Results from using alternative specifications of the baseline model that include no covariates, all covariates except log GDP per capita, and alternative measures of political and macroeconomic conditions are broadly similar to the estimates from the baseline model (Appendix Table 7). The slightly larger estimate from removing log GDP per capita from the set of covariates suggests that the total effect on neonatal mortality, which includes the indirect effect through economic growth, could be larger. We also replaced both country-fixed effects with region-fixed effects and year-fixed effects with region-specific year-fixed effects to retain meaningful variation across countries within a region and to account for differences in trends in neonatal mortality across regions (Appendix Figure 3). In Appendix Figure 3, where we learn by comparing countries to their close neighbours within the same region, we find substantially larger coefficients compared to our baseline estimates. As shown in Appendix Table 8, the coefficient is 4·6 times as large as the coefficient from the baseline model when region-fixed effects and region-specific fixed effects are used. We note that such comparisons of close neighbours are standard in comparative social science.[9] Relaxing the treatment group definition or tightening the control group definition used in constructing the stacked data set leads to varying sizes in our estimates, but the substantive finding that IMF programmes are associated with significantly higher neonatal mortality remains the same (Appendix Tables 9 and 10). We also employed the local projections approach to event study proposed by Dube and colleagues[10] to control for pre-treatment dynamics of the covariates to avoid post-treatment bias that might arise from conditioning on variables that are consequences of treatment (See Online Appendix discussion of Local Projection Approach to Difference-in-Differences). The results were similar to our baseline event study estimates, with no significant pre-treatment differences between treatment and control groups (Appendix Figure 4). In sum, our main finding is robust to alternative model specifications and controls as well as alternative definitions of the treatment and control groups.

Discussion

Although global neonatal mortality has declined in the last three decades, our findings suggest that IMF structural adjustment programmes negatively impacted the progress of participating countries. Our statistical analysis shows that IMF programmes— for an average participating country — result in a 5·8% higher neonatal mortality rate per 1,000 live births than would have otherwise occurred over five years after entry. Our analysis of potential pathways suggests that the aggregate effect may have been mediated by how IMF programmes negatively impact economic development, government spending, and the size of the healthcare workforce. We have also demonstrated a dose-response relationship: more conditions result in greater neonatal mortality. Finally, these quantitative findings are consonant with our review of case studies. Taken together, these findings strongly suggest a causal relationship.

Our main finding confirms that of earlier quantitative studies on the relationship between IMF programmes and population health while benefiting from advances in the methodological literature to generate cleaner and more reliable comparisons between treatment and control units. Furthermore, compared to earlier studies that used a reversible binary indicator of current IMF programme participation as the treatment variable, our approach takes into account the potential irreversibility of structural changes brought about by IMF programmes. The observational nature of our analysis means that we cannot rule out the existence of unobserved confounders biasing our estimates. Nevertheless, our substantive finding — that IMF programmes are associated with increases in neonatal mortality — is robust across various alternative model specifications and definitions of treatment and control groups. We consider our results as providing an important empirical basis for future research that investigates the impact of IMF programmes on other health metrics as well as on the underlying mechanisms, including research on the particular IMF conditions most responsible for these deleterious effects. Our findings indicate that IMF programmes may impede the UN's goal of reducing global neonatal mortality to below 12 per 1,000 live births by 2030. We therefore conclude that structural adjustment programmes, as currently designed and implemented by the IMF, must be scrutinized and potentially reformed to prevent further harm to population health in participating countries.

Disclosure of AI

AI technology was used to improve readability in a small number of locations in the article. The use of AI was in strict accordance with journal guidelines.

Declarations of interests

We declare no competing interests.

Acknowledgments

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TABLES AND FIGURES

Table 1: Mechanisms through which IMF programmes may impact neonatal mortality

	Specific Mechanism	**Description**
Direct effects on health systems	Healthcare Financing	Under IMF programmes, governments may reduce public health expenditure or shift towards private financing, leading to fluctuations in the volume and quality of healthcare services. Reduced public investment in healthcare could limit access to essential services for newborns, potentially increasing neonatal mortality rates.
	Healthcare Workforce	IMF programmes can affect the public sector healthcare workforce through redundancies, hiring freezes, or wage cuts. The use of 'wage bill ceilings' further exacerbates this problem by setting a maximum limit on government spending on wages, making it difficult to hire or retain healthcare workers. This can lead to a 'medical brain drain,' where skilled healthcare professionals migrate in search of better opportunities. The reduced workforce and expertise could have adverse effects on neonatal care quality and availability.
	Dimensions of Health Coverage	IMF programs can introduce revenue-raising and cost-cutting measures that directly affect various dimensions of health coverage, including the introduction of user fees and co-payments for healthcare services and medicines. While initially aimed at generating additional resources and improving efficiency, these fees have been found to reduce access for the poor and incur high administrative costs. Such barriers to healthcare can disproportionately affect vulnerable populations, including newborns, by limiting access to essential neonatal care services and medications.
	Health System Decentralization	Structural adjustment programmes can lead to health system decentralization by transferring fiscal and operational responsibilities to subnational levels. While intended to generate short-term savings and localize decision-making, this can result in inadequate coordination and budget execution problems in the health system. Local authorities may lack the technical capacities and may

		divert funds to other uses, impacting the availability and quality of neonatal care.
	Public-Private Mix in Health Service Provision	Deregulation under IMF programmes can increase the role of the private sector in health service provision, possibly expanding service for those able to afford it, but often resulting in reduced public health care provision and/or outsourcing to NGOs. The NGOs may fill these gaps but often with coordination issues and a limited range of services. Such disruptions can adversely affect vulnerable populations, including newborns, by limiting access to comprehensive neonatal care.
	Priority Spending Floors	To mitigate the negative impacts of structural adjustments on health, some IMF programmes incorporate priority spending floors, stipulating minimum expenditures on social spending. However, these are often not adequately integrated into programme designs and are usually secondary to economic targets. They are consequently inconsistently observed, which could lead to underinvestment in essential neonatal care services.
Indirect effects on health systems	Currency Devaluation	IMF programmes have promoted currency devaluation to improve external competitiveness, leading to higher import prices which can limit access to essential medicines and equipment.
	Trade and Capital Account Liberalization	The removal of tariffs reduces trade tax revenues, potentially weakening the fiscal basis for healthcare. These measures also make economies vulnerable to global fluctuations and can impede stable healthcare financing. However, if economic benefits materialize over time, this can boost public revenues that could be invested in healthcare.
	Privatization	Privatization policies aim to raise funds for governments but can lead to the loss of reliable public revenue sources in the long-term, undermining health

		policy. Also, some state-owned enterprises provide health benefits that may be withdrawn post-privatization, affecting access to healthcare.
	Aid Flows and Donor Interventions	Increased aid flows and donor interventions could potentially offset the adverse effects of structural adjustment on health systems. However, studies have found that aid often substitutes for rather than complements government spending on health. While IMF programmes may catalyze certain types of aid, this does not consistently extend to health aid.
Effects on social determinants of health	Unemployment, Poverty, Inequality	Structural adjustment programmes can lead to reduced incomes and increased unemployment, contributing to health disparities. Policies like privatization and regressive taxation reduce affordability of healthcare for poor households, with long-term implications, including on neonatal mortality.
	Education	Education is a key social determinant of health with long-term impacts on individuals' knowledge and social mobility. Structural adjustment policies, particularly those advocating user fees for primary education or workforce reduction in education, impede educational attainment, especially for women and the poor. This can reduce the protective effect of parents' education on child health.
	Environment and Environmental Policies	Structural adjustments impact environmental policies, leading to degradation and health risks. Policies favoring large agricultural interests can also affects urbanization patterns, increasing vulnerability to environmental and health hazards.
	Cultural Norms, Societal Values, and Social Cohesion	Structural adjustment policies impact cultural norms and social cohesion, often eroding community bonds and increasing individualism. This can lead to increased psychosocial stress, affecting a range of health outcomes. However, such policies can also prompt societal responses that foster resilience, such as community initiatives or social movements, which may provide resources to mitigate stress.

Note: Table constructed based on Kentikelenis (2017).[5] See Appendix Table 1 for a review of qualitative and quantitative studies on mechanisms through which IMF programmes affect health outcomes.

Table 2: Baseline stacked difference-in-differences model

	Neonatal Mortality Rate
IMF Programme	0.058*
	[0.021;0.096]
Log GDP per Capita	-0.068
	[-0.141;0.011]
Banking Crisis	-0.000
	[-0.014;0.014]
Polity	0.002
	[-0.002;0.006]
Armed Conflict	-0.008
	[-0.041;0.028]
Mean Years of Female Education	-0.072*
	[-0.122; -0.019]
Adjusted R^2	0.996
Observations	9256
RMSE	0.063
Number of Clusters	106

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. Model included event-specific country- and year-fixed effects. The model outcome variable was log-transformed neonatal mortality rate. Coefficients and confidence intervals have been back-transformed and subtracted by 1 to be interpreted as percentage change in neonatal mortality rate associated with an increase of 1 in each predictor.

Table 3: Cumulative number of conditions

	Neonatal Mortality Rate	
	[95% CI]	
	Model 1	Model 2
Cumulative Number of Total Conditions	0.00032*	..
	[0.00010; 0.00055]	..
Cumulative Number of Binding Conditions	..	0.00042*
	..	[0.00011; 0.00074]
Log GDP per Capita	-0.06296	-0.06325
	[-0.13958; 0.02048]	[-0.14002; 0.02037]
Banking Crisis	0.00206	0.00190
	[-0.01180; 0.01612]	[-0.01196; 0.01596]
Polity	0.00190	0.00207
	[-0.00204; 0.00586]	[-0.00184; 0.00598]
Armed Conflict	-0.00623	-0.00640
	[-0.04082; 0.02960]	[-0.04100; 0.02944]
Mean Years of Female Education	-0.07072*	-0.07106*
	[-0.12117; -0.01738]	[-0.12147; -0.01775]
Adjusted R^2	0.99585	0.99584
Observations	9223	9223
RMSE	0.06271	0.06275
Number of Clusters	106	106

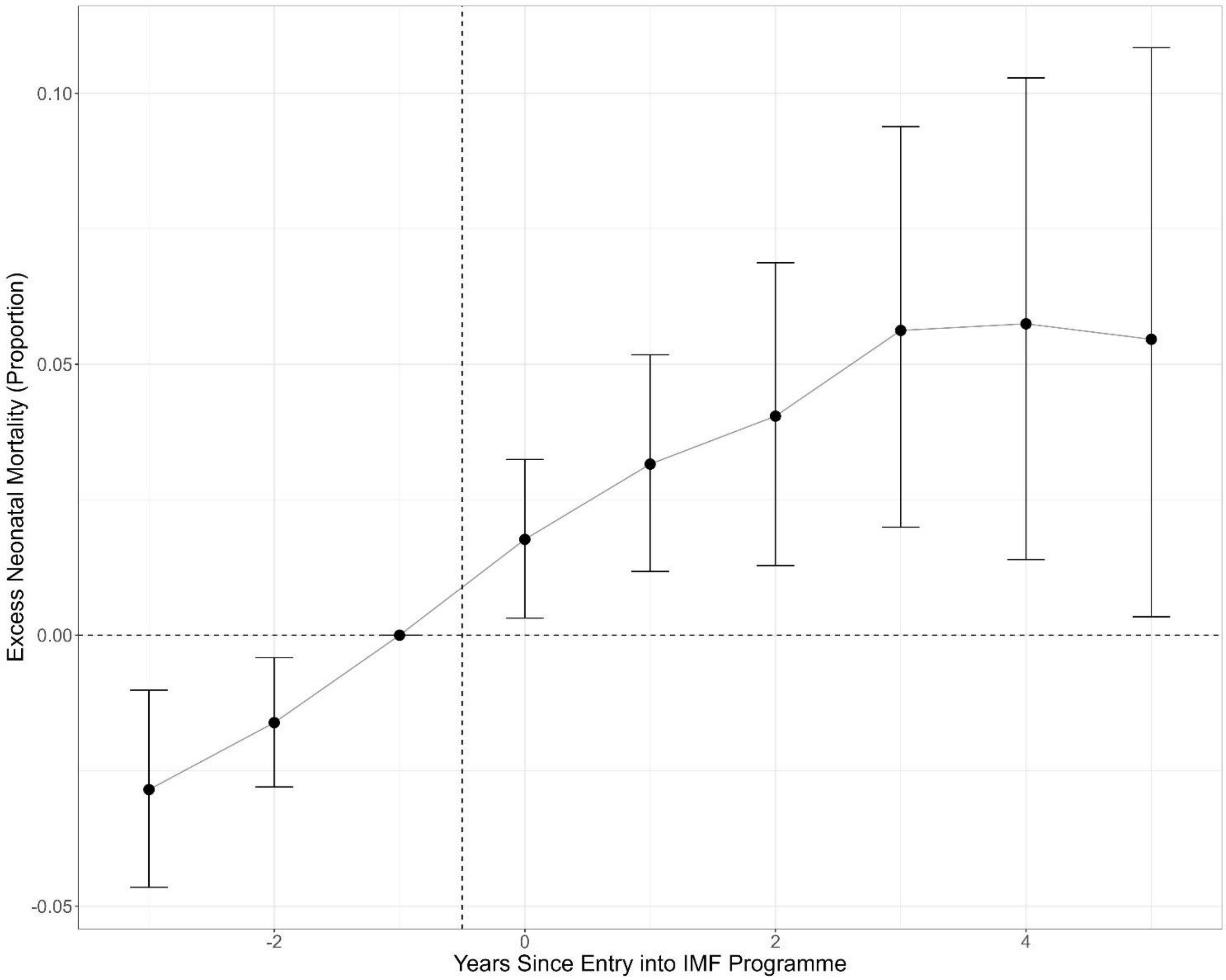
Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. Model included event-specific country- and year-fixed effects. The model outcome variable was log-transformed neonatal mortality rate. Coefficients and CIs have been back-transformed and subtracted by 1 to be interpreted as percentage change in neonatal mortality rate associated with an increase of 1 in each predictor. Column 1 shows that each additional IMF condition is associated with a 0.032 percent increase in neonatal mortality, and column 2 shows that each additional binding IMF condition is associated with a 0.042 percent increase in neonatal mortality.

Table 4: Pathway analysis

	GDP	Govt. Exp.	Govt. Hlth. Exp.	Phys.	Nu. & Mw.
IMF Programme	-0.069*	-1.779*	-0.089	-0.100*	-0.090*
	[-0.121; -0.015]	[-3.234; -0.324]	[-0.262; 0.126]	[-0.168; -0.027]	[-0.158; -0.017]
Adjusted R^2	0.996	0.891	0.990	0.951	0.975
Observations	13282	10669	6200	7989	4870
RMSE	0.092	3.611	0.181	0.228	0.122
Number of Clusters	131	113	92	134	106

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. All models included event-specific country-fixed effects and year-fixed effects. The outcome variable in column 1 was log-transformed GDP per capita. The outcome variable in column 2 was government expenditure (% of GDP). The outcome variable in column 3 was log-transformed government health expenditure per capita. The outcome variable in column 4 was log-transformed physicians per 1,000 population. The outcome variable in column 5 was log-transformed nurses and midwives per 1,000 population. The coefficients and confidence intervals for columns 1 and 3-5 have been back-transformed and subtracted by 1 to be interpreted as percentage change in the outcome variable associated with participating in an IMF programme. For example, column 1 shows that IMF programme participation is associated with a 6.9 percent reduction in GDP per capita. Meanwhile, column 2 coefficient should be interpreted as a 1.78 percentage point reduction in government expenditure as a percentage of GDP.

Figure 1: Stacked difference-in-differences event study



ONLINE APPENDIX

The International Monetary Fund and Neonatal Mortality Rates, 1985-2018

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Additional Pathways

We extend our analysis of pathways using the baseline stacked difference-in-differences model to three additional possible pathways linking IMF programmes to neonatal mortality: log dentists per 10,000 (an indicator of health systems resources), labour force participation rate (an indicator of macroeconomic conditions), and the ratio of top 10 percent and bottom 50 percent income shares (an indicator of income inequality). As shown in Appendix Table 11, we do not find significant effects of IMF programmes on these three outcomes.

Robustness Tests

We assess the robustness of our estimates first by altering the model specification and then by altering the definitions of the treatment group and the control group used in constructing the stacked data set.

Alternative Model Specifications

To test the robustness of our estimates, we modify our model specifications by (1) modifying the covariates included, (2) replacing year-fixed effects with region-specific year-fixed effects, and (3) replacing country-fixed effects with region-fixed effects and year-fixed effects with region-specific year-fixed effects. The motivation behind the latter two changes is twofold. First, we may expect countries in the same geographical region to have more similarities with one another, and comparing countries in the same region retains substantively meaningful between-unit variation that is eliminated by country-level fixed effects. Second, as shown in Appendix Figure 1, trends in neonatal mortality rate varied substantially across regions between 1985 and 2019, so it may be more appropriate to allow year-effects to vary by regions than to control for a global aggregate time trend.

Alternative Definitions of the Treatment Group and the Control Group

To test for robustness of our results, we estimate the dynamic models on different stacked data sets constructed using modified definitions of the treatment group and the control group:

(1) We relax our definition of the treatment group to allow countries that have not participated in any IMF programmes for at least 8 years to join another treatment group for a later event-specific data set. In other words, we expand the definition of an "event" when building the stacked data set to include both instances where countries entered an IMF programme for the first time *and* where countries entered an IMF programme after at least 8 years of not

being under a programme. Appendix Table 9 reports the event study estimates after adopting this more relaxed definition of the treatment group.

(2) We tighten our definition of the control group to exclude countries from Northern and Western Europe and North America, which may be different in unobservable ways from IMF programme-participating countries that might cause biases in our estimates. As shown in Appendix Table 10, event study estimates after removing these countries remain close to those in Appendix Table 6.

Extended Methods

Instrumental Variables

A causal interpretation of our main estimates depends on the absence of omitted or unobservable confounders that influence both countries' participation in IMF programmes and their health outcomes. There may be unobservable time-varying factors, such as political will,[1] that make countries that participate in IMF programmes differ meaningfully from those that do not. This concern for selection on unobservable confounders into IMF programmes has motivated the instrumental variable (IV) approach implemented in studies such as Barro and Lee,[2] Nelson and Wallace,[3] Lang,[4] and Stubbs and colleagues.[5] We provide illustrative examples of studies using an instrument to evaluate IMF programmes in Appendix Table 3, along with the common issues that such an approach faces. The validity of an instrumental-variable approach relies on identifying a variable ('instrument') that satisfies the conditions of (1) being highly correlated with the treatment variable ("relevance criterion") and (2) having no other channel to the outcome than through the treatment variable ("exclusion restriction"). With a valid instrument, researchers can identify the causal effect of treatment by using exogenous variation in treatment assignment caused by the instrument, much like in a randomised experiment setting. Instruments proposed in past studies have faced criticisms over breaches of the exclusion restriction. For example, one instrument widely used in this literature is a country's voting similarity with the U.S. or G7 in the UN General Assembly (UNGA). This may be problematic, as it is likely that a government's foreign policy preferences as expressed in its UNGA voting behaviour are related to domestic policies and factors other than those related to IMF programmes that might impact the domestic economy[4] and population health, which would mean a violation of the exclusion restriction. Besides voting patterns at the UNGA, other political economy variables used in past research as instruments for IMF participation are also subject to this violation.[5]

To circumvent this issue, an instrument recently popularised by Lang[4] and Stubbs and colleagues[5] takes the form of the interaction between a measure of within-country mean exposure to IMF programmes and a measure of the IMF's budget constraint. Proponents of this instrument argue that the instrument satisfies the relevance criterion because when facing liquidity concerns, the IMF signs fewer loan agreements and imposes more stringent loan conditions. Moreover, they argue that the exclusion criterion holds because the Fund's budget constraint is determined independently of a given country. Both criteria are essential for the validity of the instrument. We adopt the compound instrument that is derived from the interaction between the country-specific average exposure to IMF programmes over the sample period (mIMFPROG) and the number of countries with an IMF programme in a given year (mIMFBUDG), which proxies the Fund's annual budget constraint.[1] Suppose the data generating process is the following:

$$Y_{it} = \beta IMFPROG_{it} + X_{it}\delta + \mu_i + \gamma_t + \varepsilon_{it} \quad (1)$$

Y is log-transformed neonatal mortality. *IMFPROG* is a reversible binary indicator of whether a country is under an IMF programme in a given year. *X* is our baseline set of time-varying covariates. μ and γ are country- and year-fixed effects. We estimate the two-stage regression model with the following first stage:

$$IMFPROG_{it} = \pi\,(mIMFPROG_i \times mIMFBUDG_t) + X_{it}\psi + \alpha_i + \kappa_t + \nu_{it} \quad (2)$$

And the second stage, with the fitted values from equation (2):

$$Y_{it} = \beta IMF\widehat{PRO}G_{it} + X_{it}\delta + \mu_i + \gamma_t + \varepsilon_{it} \quad (3)$$

Appendix Tables 12 and 13 report the first stage and two-stage least squares (2SLS) estimates when no time-varying covariates are included in the model and when we add the covariates back in. First stage results indicate that the correlation between the compound instrument and IMF participation is relatively weak, which could lead to badly biased estimates.[6] Two-stage least square (2SLS) without controlling for any covariates (Model 1 in Appendix Table 13) suggests that participation in an IMF programme is associated with a 44.8% higher neonatal mortality rate. When the baseline set of covariates are included, the estimate becomes indistinguishable from 0 at conventional significance levels, but remains sizable at a 30.3% higher neonatal mortality rate (Model 2 in Appendix Table 13). Reduced-form estimates are reported in Appendix Table 14.

Our analysis of potential pathways using this approach finds a large estimate of a 47.4% reduction in GDP per capita associated with participating in IMF programmes (Appendix Table 15) and an 8.5 percentage point increase in labour force participation rate (Appendix Table 16). We believe these estimates to be implausibly large and inconsistent with the case-study evidence. Moreover, we believe the instrument is problematic on conceptual grounds. Is the instrument capturing the experience of countries that would not normally be under IMF programmes but are somehow induced into accepting conditionality by an eager IMF that is flush with cash, or is the instrument capturing countries that would normally be under IMF conditionality, but due to a shortage of funds forced to accept more heavy-handed conditions by a more cash-strapped and desperate IMF? Moreover, does the process by which countries enter IMF programmes match the process theorised by proponents of the compound instrument? Without a deeper understanding of how the compound instrument relates to countries' participation in IMF programmes, we are unable to determine if the results from using this type of instrument are valid and generalisable. We recommend avoiding this IV approach unless a valid and much more intuitive instrument is identified.

Two-Way Fixed-Effects Models

If we neglect the issues posed by the biases of two-way fixed effects regressions in settings where there are likely heterogeneous treatment effects[7] and if we believe that a binary indicator of an active IMF programme in a country in a given year is a good measure of whether a country is currently experiencing the effects of participating in IMF programmes, we could run a two-way fixed effects regression that, as the treatment variable, uses a binary indicator of IMF participation that reverts back to 0 when programmes are no longer active, controlling for country- and year-fixed effects and the set of covariates used in our baseline model. Our results attempting this are shown in Appendix Table 5 and would suggest that IMF programmes have no significant effect on countries' neonatal mortality rates. Recent statistical advances regarding two-way fixed effects regressions and the likely lasting impacts of IMF programmes beyond their active years call into question the validity of this approach.

Local Projection Approach to Difference-in-Differences Event Study

Besides the stacked regression approach proposed by Cengiz and colleagues,[8] there have been several difference-in-differences estimators proposed to address the biases of two-way fixed-effects regression.[9–11] In particular, Dube and colleagues[11] propose a "local projection" approach to difference-in-differences event studies (LP-DiD) that employs the "clean control" condition in the spirit of stacked difference-in-differences regressions and has important features such as flexibility in the definitions of the treatment and control groups and the ability to control only for pretreatment values of the control variables.

Given that some of our covariates (GDP per capita for instance) may themselves be consequences of IMF programmes, controlling for them may introduce post-treatment bias.[12] To illustrate how bias can arise in this setting, we use an example from Acharya and colleagues.[12] Suppose that there is no causal relationship between treatment ("car accident") and outcome ("having cancer"), if we condition on being a patient in a hospital (post-treatment after a car accident) when estimating the effect of having a car accident on the probability of having cancer, we would find a strong negative correlation between the two, since if one is in the hospital and one has not had a car accident, one is likely to be admitted for other reasons such as cancer. This, however, does not mean that car accidents have a cancer-fighting effect, and the estimate we find does not have a causal interpretation. To address this concern in our setting, we implement the local projection approach to study the impact of IMF programmes on neonatal mortality and make use of its feature of controlling only for pretreatment dynamics of our covariates. As in our baseline model, we focus on the first instances where countries entered an IMF programme since 1985 and consider a binary irreversible treatment variable D that indicates whether a given country has entered its first IMF programme or not. Following Dube and colleagues,[11] for each time horizon h = -3, …, 5, we restrict the panel data set that we started with to only observations that are either newly treated (ΔD_{it} = 1) or will not have been treated before time $t + h$ ($D_{i,t+h}$ = 0), then we estimate the following LP-DiD specification on each restricted data set:

$$y_{i,t+h} - y_{i,t-1} = \beta^h \Delta D_{it} + \Sigma_{p=0}^{3} \Delta X_{i,t-p} {\Gamma^h}_p + {\delta^h}_t + {e^h}_{it} \quad (4)$$

β is the parameter of interest. $\Delta X_{i,t-p}$ is the difference between the set of control variables at time t - p and the preceding period. δ is year-fixed effects. The estimates are plotted as an event study plot with time horizon on the horizontal axis in Appendix Figure 4, which suggests that, controlling for dynamics in the covariates in the 4 years prior to entry, entering an IMF programme significantly worsens a country's neonatal mortality rate. The size of the estimates is near our estimates from Figure 1, adding further confidence to our main finding.

Appendix Table 1: Prior Studies of IMF Participation and Mechanisms Affecting Health Outcomes

Panel A: Qualitative Case Studies

Study	Country/Region and Period	Method	Key Results	Implicated Mechanisms
Chapman, 2004[13]	Mozambique, Manica Province (1993-1995; 1998)	Key informant interviews and reproductive health questionnaires with a sample of 83 women of reproductive age (15 to 49 years old) during pregnancy and after birth, using life histories of a subset of 15 women from the pregnancy case study group, and community-level focus group sessions, observations of health care practitioners.	Devastating cuts in health spending in compliance with IMF conditions alongside other social changes have led pregnant women to shun the formal healthcare system.	Financing Structure of Health Care; Cultural Norms, Societal Values, and Social Cohesion
Donato et al., 2021[14]	Portugal (1996-2015)	Compared medicine utilization data from national outpatient drug use inventory with other relevant indicators over 5-year intervals for a period of 20 years.	Following MOU signing with European Commission, ECB and IMF, a rise in out-of-pocket and austerity measures may have contributed to decreased access to medicines in Portugal, although other factors were involved.	Financing Structure of Health Care; Dimensions of Health Coverage
Farmer, 2001[15]	Haiti (1990s)	Ethnographic field work	Documents how social inequalities, worsened by structural adjustment programmes, shape the distribution of infectious diseases such as tuberculosis and AIDS	Unemployment, Poverty, Inequality
Farmer, 2009[16]	Rwanda (2005-2008)	Personal account (author is an anthropologist and doctor, who served as a physician in Rwanda during this period)	Discusses the long-term negative effects of the violence/genocide of the 1990s which he attributes, in part, to inequalities worsened by IMF structural adjustment programmes.	Unemployment, Poverty, Inequality
Foley, 2009[17]	Senegal (1996-2006)	Twenty months of field research in 1996-1999, 2002, 2005, 2006, with focus groups, interviews, participant observation and document reviews.	Two decades of IMF-backed structural adjustment reforms discussed with other factors as contributing to increasing poverty and weakened public health care provision.	Financing Structure of Health Care; Unemployment, Poverty, Inequality
Kentikelenis et al., 2016[18]	131 countries (1985-2014)	Analysis of IMF conditions	Contrary to IMF claims of having reduced conditions and placed greater emphasis on social protection, the authors show a rise in conditions following the global financial crisis, continued 'liberalizing' reforms that are detrimental to social protection and weak commitment to 'priority spending floors' for social spending.	Financing Structure of Healthcare, Priority Spending Floors

Lundy, 1996[19]	Jamaica (1992)	Interviews with 56 health care professionals and 16 senior government health and environment officials; participant observation to assess health care facilities/services.	Uses available statistics from the 1980s to show that structural adjustment was associated with decreased government spending on health and reduction in the size of the health care workforce, including in community health aides used for primary health care in poor rural communities. Also, food prices increased following currency devaluation and elimination of food subsidies to comply with the IMF SAP; malnutrition subsequently rose. Most interviewed participants described a decline in public health care services and standards following from SAPs, with an exodus of medical workers, increasing wait times, drug and equipment shortages, declining facilities and access to utilities.	Financing Structure of Health Care; Healthcare Workforce; Dimensions of Health Coverage; Currency Devaluation; Unemployment, Poverty, Inequality
Moore et al., 2022[20]	Uganda (1980-2019)	Observations, interviews (N = 43), focus group discussions (N = 24), and life history interviews (N = 26) in 6 south-central Ugandan communities in 2019; archival records.	Structural adjustment programmes with the WB, IMF and US government, starting in the 1980s, repealed the monopoly over the coffee trade held by cooperative unions since 1969. The collapse of this sector led to the displacement of women, with many forced into 'transactional' sexual relations, leading to the rapid spread of HIV.	Macroeconomic Reforms; Unemployment, Poverty, Inequality
Morais Nunes et al., 2019[21]	Portugal (2011-2016)	Archival, descriptive statistics; econometric analyses of productivity	Following structural adjustments made in accordance with Portugal's agreement with the Troika, the NHS workforce was reduced, copayments have increased while access to NHS services has decreased. However, structural reforms in the health care sector had positive effects in terms of drugs administration and consumption, on the one hand, and secondary care expenditures reduction, on the other hand.	Financing Structure of Health Care; Healthcare Workforce; Dimensions of Health Coverage
Munala et al., 2021[22]	Kenya (Not stated)	28 interviews with care providers from 8 Post Rape Care centres in Nairobi.	IMF/WB lending policies have led to "significant under-resourcing of the national response to sexual violence and the health system more broadly."	Financing Structure of Health Care; Unemployment, Poverty, Inequality
Petrou, 2021[23]	Cyprus (2019-2020)	Case study, reviewing policy implementation	As one of its structural reforms agreed to in its agreement with the Troika, Cyprus has created a new National Healthcare System, with initial evidence of increased efficiency and equity.	Financing Structure of Health Care; Healthcare Workforce; Public-Private Mix in Health Service Provision
Pfeiffer, 2003[24]	Mozambique (1994-1995)	Qualitative study of livelihood dynamics, comparing 50 households with poorly growing children with 50 households with children showing healthy growth.	Implementation of WB and IMF SAPs increased the vulnerability of families on cash incomes and overall economic insecurity by removing subsidized purchase of rural produce by the	Unemployment, Poverty, Inequality; Cultural Norms, Societal Values, and Social Cohesion

			government, eliminating women's cooperatives, and privatizing land. Women’s subordinated position in the external cash economy undermines their intrahousehold bargaining power, to the detriment of their ability to provide childcare.	
Pfeiffer, 2003[25]	Mozambique, Manica Province (1993-1995; 1998)	Three years of participant observation	Declining salaries within the national health system and the proliferation of NGOs following implementation of SAPs has fragmented the health sector and created an internal brain drain from the public sector to NGOs, which pay higher salaries.	Financing Structure of Health Care; Public-Private Mix in Health Service Provision
Pfeiffer, 2004[26]	Mozambique (1993-2003)	Fieldwork across 1993-2003 period.	Argues that privatization and free markets following structural adjustment has driven the displacement of community outreach and education programmes by 'social marketing' conducted through commercial advertising and private sales. The use of social marketing to promote condom use in the context of the HIV crisis conflicts with anti-condom messages from Christian churches, endangering goal of increasing condom use.	Financing Structure of Healthcare; Public-Private Mix in Health Service Provision
Stubbs et al., 2017[27]	16 West African countries (1995-2014)	Qualitative analysis of IMF conditionalities	IMF policy reforms reduce fiscal space for investment in health, limit staff expansion of doctors and nurses, and lead to budget execution challenges in health systems	Financing Structure of Health Care; Healthcare Workforce
Thow et al., 2021[28]	Ghana (1957-)	Qualitative (29 interviews with policy actors; document review)	Examines long term consequences of the WB and IMF-backed shift to a liberal economic approach in the 1980s, showing how it has hindered the integration of nutrition into food policy. Places strong emphasis on the role of export-led agriculture and policy makers unwilling to "undertake strong intervention in food policy."	Financing Structure of Health Care; Dimensions of Health Coverage
Van Der Geest et al., 2000[29]	Zambia (1996-2000)	Twenty-five focus group discussions, 35 interviews and observation at 2 rural and 2 urban health centres.	Documents widespread public dissatisfaction with introduction of user fees, undertaken while Zambia was undergoing IMF structural adjustment, due to perception that the fees were not accompanied by improvement in quality of	Financing Structure of Health Care; Dimensions of Health Coverage

			care and particularly not by better availability of medical drugs.	
Vavrus, 2005[30]	Tanzania (1996-2003)	Ethnographic study in a Chagga community in Tanzania, focusing on secondary school students	Reviews broad impact of IMF and WB SAPs in Tanzania, from an early sharp decline in public sector spending and employment to long-term improvement in the economy. Private healthcare system now provides better care to those able to afford it; however, public system has deteriorated. A further effect is that access to secondary school declined due to the imposition of fees and removal of subsidized food prices; the study argues that this has led young women to engage in high-risk sex in return for resources to attend school.	Financing Structure of Healthcare; Public-Private Mix in Health Service Provision; Unemployment, Poverty, Inequality

Panel B: Quantitative Analyses

Study	Country/Region and Period	Method	Key Results	Implicated Mechanisms
Antonakakis and Collins, 2014[31]	Greece (1968-2011)	Regression (IVs = austerity measures (gov't expenditures as percent of GDP); DV = realised suicidality)	Fiscal austerity, imposed after agreement with IMF, EC and the ECB, led to significant increase in overall suicide rates in Greece, especially for males and people aged 45-89.	Unemployment, Poverty, Inequality; Cultural Norms, Societal Values, and Social Cohesion
Assaad and Arntz, 2005[32]	Egypt (1988-1998)	Descriptive statistics, regression, comparing data from 1988 and 1998 household surveys	SAPs, agreed to in return for loans from the IMF and other international financial organizations, led to dwindling employment opportunities in the public sector, and women faced barriers, partially due to geographical immobility, in accessing non-governmental employment, widening gender-based inequality.	Unemployment, Poverty, Inequality
Buchmann, 1996[33]	58 countries (1975-1987)	Regression analysis (IV = IMF programme participation, debt levels; DVs = Indicators of female education)	IMF SAPs negatively affect female educational enrolment.	Education

Clements et al., 2013[34]	140 countries (1985-2009)	Instrumental variable regression (IV = IMF participation; Instrumental variables = international reserves; exchange rate, index of exchange rate classification; DV = public spending on education and health.)	A 5-year IMF-supported programme is linked to a 0.75% of GDP rise in education spending and a 1% increase in health spending.	Financing Structure of Health Care
Daoud et al., 2017[35]	67 countries (2000)	Regression analysis (IV = IMF participation; DV = child health)	IMF programmes reduce the protective effects of parental education on child health, especially in rural areas.	Education
Dreher, 2006[36]	98 countries (1970-2000)	Instrumental variable regression (IV = IMF programme participation; DV = Average five-year GDP growth rate; Instrumental variable = alignment of UNGA votes with G7 countries)	IMF programmes reduce growth rates; this is mitigated by compliance with conditionality, but overall impact remains negative.	Unemployment, Poverty, Inequality
Goulas and Zervoyianni, 2016[37]	30 developing and transition countries (1991-2008)	Regression analysis (IV = IMF programme participation; DV = suicide rate)	The study reveals that involvement in IMF programmes is associated with increased suicide mortality, with being more prominent among males. The overall rise in suicide mortality due to IMF participation ranges from 4 to 14 percentage points. The highest suicide rate is observed within the age group of 45 to 64, with an increase of 18 percentage points.	Cultural Norms, Societal Values, and Social Cohesion
Handa and King, 2003[38]	Jamaica (1989-1996)	Synthetic cohort analysis, using national micro survey data to measure 'weight for height' while controlling other variables	Steep currency devaluation following Jamaica's 1991 exchange liberalization as part of its IMF SAP led to spike of food price inflation and a subsequent decline in children's weight.	Currency Devaluation; Unemployment, Poverty, Inequality
Huber et al., 2008[39]	Latin America (1970-2000)	Prais-Winsten regression (IV = IMF programme participation; DV = Health spending as share of GDP)	IMF programme participation was associated with an increase in health spending.	Financing Structure of Health Care
IEO, 2003[40]	All developing countries (1985-2000)	ARIMA model (IV = IMF programme participation; DV = Health spending as share of GDP, share of total government spending, share of per capita government spending)	Decline in spending in democracies; increase in non-democracies.	Financing Structure of Health Care

Kentikelenis, Stubbs and King, 2015[41]	63 low-income countries (1985-2009)	OLS Regression (IV = IMF programme participation; DV = Gov't health expenditures as share of GDP, share of total and discretionary government spending and per capita spending)	Increase in spending in Sub-Saharan African LICs; decrease elsewhere.	Financing Structure of Health Care
Morais Nunes et al., 2019[21]	Portugal (2011-2016)	Archival, descriptive statistics; econometric analyses of productivity	Following structural adjustments made in accordance with Portugal's agreement with the Troika, the NHS workforce was reduced, co-payments were increased and access to NHS services has decreased. However, there were positive effects in terms of drugs administration and consumption, and secondary care expenditures reduction.	Financing Structure of Health Care; Healthcare Workforce; Dimensions of Health Coverage
Nooruddin and Simmons, 2006[42]	107 countries (1980-2000)	Regression analysis (IV's = IMF programme participation; Regime type; DV = share of total expenditures allocated to health and education)	IMF programmes reduce the role of domestic politics: democracies tend to allocate larger budget shares to public services, but this difference disappears under IMF programmes (non-democracies allocate more; democracies less).	Financing Structure of Health Care
Noy, 2011[43]	18 OECD and 23 Latin American/ Caribbean Countries (1980s-19990s)	Systematic comparison of social spending in the OECD and Latin America and the Caribbean with regression analyses (IV's = various, including WB or IMF SAP; DV = social spending as percent of GDP)	In Latin America and the Caribbean, "the presence of international financial institutions powerfully pattern health and social spending: decreasing spending on welfare and social security and increasing health spending."	Financing Structure of Health Care
Oberdabernig, 2013[44]	86 low- and middle-income countries (1982-2009)	Treatment Effects Regression Analysis (IV = IMF programme participation; DV = various measures of poverty and inequality)	Adverse short-run effects of IMF programmes on poverty and inequality over the entire period, but reversed results for the 2000-2009 period. Evidence also suggests that short-run effects might disappear in the long run.	Unemployment, Poverty, Inequality
Pongou et al., 2006[45]	Cameroon (1991-1998)	Regression analysiswith pooled cross-sectional data from two Demographic and Health Surveys conducted in 1991 and 1998 to identify determinants of increased malnutrition (IV's = changes in household economic status, maternal health seeking behavior; DV = prevalence of malnutrition)	IMF SAP and economic crises were associated with declining economic status of households and reduced access to health care, causing an increase in malnutrition that was largest among low-SES children.	Macroeconomic reforms; Unemployment, Poverty, Inequality

Reeves et al., 2014[46]	27 European countries (1995-2011)	Regression analyses to identify determinants of changes in health expenditures following the Great Recession (IV = various, including IMF programme participation; DV = changes in government health expenditures)	"IMF borrowers were significantly more likely to reduce healthcare budgets than non-IMF borrowers."	Macroeconomic reforms; Financing Structure of Health Care
Shandra et al., 2008[47]	50 poor countries (1990-2000)	Lagged dependent variable panel regression models (IV = SAP conditionality index; DV = water pollution)	WB or IMF debt, structural adjustment, and industrial exports increase water pollution	Environment and Environmental Policies
Shandra, Shandra, and London, 2012[48]	30 countries in Sub-Saharan Africa (1990-2005)	Two-way Fixed Effects Regression (IV = IMF Loans; DV = Infant mortality)	IMF SAPs increase infant mortality via higher HIV prevalence, decreased access to improved water/sanitation sources, lower female educational attainment, and different macroeconomic features.	Unemployment, Poverty, Inequality; Education; Environment and Environmental Policies
Stubbs et al., 2017[27]	16 West African countries (1995-2014)	Cross-national FE models (IV = IMF policy reforms; DV = Gov't health spending per capita, 1995-2012); Heckman (inverse Mills ratio)	Each additional binding IMF reform reduces per capita government health expenditure by 0.25%.	Financing Structure of Health Care; Healthcare Workforce
Stubbs and Kentikelenis, 2018[49]	59 low-income countries (1988-2014)	Instrumental variable regression (IV = IMF programme participation; DV = education spending (n = 48, 1988-2014) and health spending (n = 59, 1995-2014; Instruments: total number of countries under IMF programmes (IMF liquidity constraint) and UNGA voting similarity with US)	IMF programme participation has no significant effect on education but is associated with decreased health spending.	Financing Structure of Health Care

Stubbs et al., 2016[50]	136 IMF Programme Countries (1986-2009)	Regression analysis (IV = IMF Participation; DV = Aid catalysis	Investigating the IMF claim that its programmes encourage aid by signalling policy credibility, the study finds that "IMF programmes catalyse aid on aggregate, but the evidence varies across different types of aid. Aid catalysis is stronger and more robust in sectors linked to the IMF's core competency areas, namely debt-related relief and general budget support, but weaker and less robust for infrastructure, production, multisector, and humanitarian aid, and non-existent for health and education."	Aid Flows and Donor Interventions; Aid Flows and Donor Interventions
Stubbs et al., 2017[27]	16 West African countries (1995-2014)	Cross-national FE models (IV = IMF policy reforms; DV = Gov't health spending per capita, 1995-2012); Heckman (inverse Mills ratio)	Each additional binding IMF reform reduces per capita government health expenditure by 0.25%.	Financing Structure of Health Care
Stuckler et al., 2011[51]	IMF borrower countries vs. non-borrower countries (1996-2006)	Regression analysis to determine whether being under an IMF programme determines diversion of donor aid flows away from health care. (IV = Donor Aid Flows; DV = Health Spending; separate models for IMF-borrowing countries and non-borrowing countries)	For each $1 of development assistance for health, non-IMF countries add about $0.45 to the health system whereas IMF borrowers add less than $0.01.	Aid Flows and Donor Interventions
Vreeland, 2002[52]	110 countries (1961-1993)	Regression with 'dynamic version of the Heckman selection model' (IV = IMF programme participation; DV = labor share of income from manufacturing)	"IMF programmes have negative distributional consequences… [T]he change in capital share of income…is large enough to increase the income of capital, despite lower growth rates."	Unemployment, Poverty, Inequality

Appendix Table 2: Quantitative Studies on IMF Structural Adjustment Programme Participation and Health Outcomes

Study	Sample and Period	Method	Key Results
Shandra et al., 2004[53]	59 low- and middle-income countries (1980-1997)	Panel regression (IV = IMF programme participation; DV = Infant mortality rate (IMR)	IMF programme participation increases infant mortality and does so more strongly at lower levels of democracy.
Pongou et al., 2006[45]	Cameroon (children) (1991, 1998)	Multivariate regression (IV = household economic status, maternal health-seeking behaviour; DV = child malnutrition)	Economic crisis and impact of IMF structural adjustment programmes during the 1990s led to increased child malnutrition, largest among children of low SES.
Stuckler et al., 2008[54]	21 Post-communist E. Europe & former Soviet Union countries (1989-2003)	Multivariate regression (IV = IMF programme participation; DV = Tuberculosis mortality	IMF programme participation was associated with increased tuberculosis incidence, prevalence, and mortality.
Hajro & Joyce, 2009[55]	82 low and middle-income countries (1985-2000)	IV = IMF programme participation; DV = Infant mortality rate (IMR)	IMF concessional loans increased the impact of growth on lowering infant mortality; no effect for concessional loans.
Shandra et al., 2010[56]	65 low- and middle-income countries (1990-2000)	Lagged dependent variable panel regression (IV = Structural adjustment programme; DV = maternal mortality rate (MMR)	SAP participation led to higher maternal mortality rates.

Maynard et al., 2012[57]	74 low- and middle-income countries (1990-2005)	Longitudinal panel regressions (IV = IMF programme participation; DV = prevalence of Tuberculosis	Increased prevalence of tuberculosis, due to reduced governmental capacity to provide health services.
Shandra, Shandra, and London , 2012[48]	30 countries in Sub-Saharan Africa (1990-2005)	Two-way FE regression (IV = IMF programme participation; DV = Infant mortality)	SAPs increase infant mortality via higher HIV prevalence, decreased access to improved water/sanitation sources, lower female educational attainment, and different macroeconomic features.
Hoddie & Hartzell, 2014[58]	220 countries (1999)	Regression (IV = IMF programme participation; DV = Disability-adjusted life years (DALYs)	SAPs increase the exposure of populations to conditions that elevate disability and death rates, and detrimental effects persist over the longer term.
Pandolfelli, Shandra and Tyagi, 2014[59]	37 African nations (1990-2005)	Regression with Heckman model (IV = IMF SAP participation; DV = Maternal mortality)	Sub-Saharan African nations with SAPs had higher levels of maternal mortality than those not under such programmes.
Goulas & Zervoyianni, 2016[37]	30 low- and middle-income countries (1991-2008)	Regression (IV = IMF programme participation; DV = Suicide mortality rate	There was a positive causal relationship between IMF programme participation and suicide mortality, strongest for males and individuals aged 45-64.
Daoud et al., 2017[35]	67 low- and middle-income countries (circa 2000)	DV = Child malnutrition prevalence	Increase in child malnutrition, interacting with parental education (reducing protective effect of latter)

Daoud and Reinsberg, 2019[60]	128 low- and middle-income countries (1995-2014)	Instrumental Variable Regression (IV = IMF programme participation; DV = Child vaccination coverage and U5MR; Instrument = interaction between country-specific average number of conditions and their global average for the same year)	IMF policy conditions on public-sector employment result in reductions in child vaccination coverage.
Forster et al., 2020[61]	137 countries low and middle-income countries. (1980-2014)	Instrumental Variable Regression (IV =IMF SAP participation; number of conditions; DV = health system access, neonatal mortality; Instrument = Interaction of number of countries under IMF programmes and country-specific probability of participation)	Structural adjustment reforms lower health system access and increase neonatal mortality, with labor market reforms driving these effects.

Appendix Table 3: Instrumental Variable Approaches to Evaluating IMF Programmes

Class of Instrumental Variables	Examples of Instruments Used	Issues
Economic and (geo)political determinants of IMF programme participation	- U.S. military assistance to country over 1980-99 (Easterly, 2005[62]) - time spent under IMF programmes during 1970-1979 (Easterly 2005[62]) - country's debt-to-GDP ratio (Dreher and Gassebner, 2012[63]) - international reserves in months of imports lagged by one period (Clements et al., 2013[34]) - log of the bilateral exchange rate to the U.S. dollar lagged by one period (Clements et al., 2013[34]) - exchange rate classification index lagged by one period (Clements et al., 2013[34]) - fraction of country's votes in the UN General Assembly in agreement with the average of theG7 (Dreher, 2006[36]) - fraction of country's votes in the UN General Assembly in agreement with the U.S. and European countries (Barro and Lee, 2005[2]) - country's IMF quota and staff share (Barro and Lee, 2005[2]) - average short-term interest rates in the Northern financial centres (the U.S., UK, Germany, France, Japan, and Switzerland) weighted by country's external debt profile (Nelson and Wallace, 2017[3])	- It is unclear which set of countries were induced by the instrument to be more (less) likely to be exposed to IMF programmes and conditions. The estimated effect could be non-generalisable. - Some of these instruments may be only weakly correlated with the IMF variables. - Some of these instruments may plausibly affect outcomes of interest outside the IMF channel, which is a breach of the exclusion restriction required to identify causal effects.
Interaction of a measure of country's propensity of being exposed to IMF programmes and conditions and a measure of IMF's budget constraint	- Interaction of the log of IMF liquidity ratio and a time-varying measure of country's propensity to be exposed to IMF programmes (Lang, 2021[4]; Nelson and Wallace, 2017[3]; Stubbs et al., 2020[5]) - Interaction of the number of countries with an IMF programme in a given year (as a proxy of IMF's budget constraint) and the fraction of the entire sample period in which the country was under a programme (Forster et al., 2019[64]; Forster et al., 2020[61]; Lee and Woo, 2021[65]) - Interaction of the number of countries with an IMF programme in a given year and the average number of a specific type of conditions that the country received during the entire sample period (Daoud and Reinsberg 2019[60]; Forster et al., 2019[64]; Forster et al., 2020[61]; Lee and Woo, 2021[65], Reinsberg et al.2019[66], Reinsberg et al., 2019[67]; Reinsberg et al., 2020[68]) - Interaction of the log of IMF liquidity ratio and the average number of a specific type of conditions that the country received during the entire sample period (Biglaiser and McGauvran 2022[69]; Reinsberg et al. 2022; Stubbs et al., 2020[5])	- It is unclear which set of countries were induced by the instrument to be more (less) likely to be exposed to IMF programmes and conditions. It is also unclear whether the relationship between the instruments and the IMF variables is monotonic, e.g. always inducing countries to be either more or less likely to enter into programmes. The estimated effect could be non-generalisable. - This class of instruments can yield inconsistent and implausible results, such as IMF programmes being associated with a 47.4% reduction in GDP per capita and an 8.5 percentage point increase in labor force participation (Tables 14 and 15).

Appendix Table 4: Data Sources

Variable	Description	Source
Mortality rate, neonatal	Number of deaths within the first 28 days of life, expressed per 1,000 live births	IGME Accessed Nov. 2022
IMF participation status	Binary indicator variable for whether an IMF programme has been active for at least five months in a given year	Kentikelenis and Stubbs, 2023[70]
Total Conditions	Number of conditions in a given year	Kentikelenis and Stubbs, 2023[70]
Total Binding Conditions	Number of binding conditions in a given year	Authors' calculation using Kentikelenis and Stubbs, 2023[70]
GDP per capita	GDP per capita (constant 2015 US$) [NY.GDP.PCAP.KD]	WDI Accessed Sept. 2022
Banking crisis	Binary indicator variable for whether the country is undergoing a systemic banking crisis in a given year	Created by authors from the database on systemic banking crises by Laeven and Valencia, 2018[71]
Polity2 Index	Revised Combined Polity Score	Polity5 Annual Time-Series, 1946-2018, Center for Systemic Peace
Armed Conflict	Binary indicator variable for whether an armed conflict that caused at least 25 battle-related deaths takes place in the country in a given year	Created by authors from the UCDP/PRIO Armed Conflict Dataset version 23.1
Mean Years of Female Education	Mean years of school of females aged 25 to 29	Global Educational Attainment Distributions 1970-2030, GHDx
Total Reserves Minus Gold	International Liquidity, Total Reserves excluding Gold, US Dollars [RAXG_USD]	IFS Accessed Nov. 2022
Democracy	Egalitarian democracy index	The QoG Institute Accessed Aug. 2022
Coup d'etat	Binary indicator variable for whether a coup d'etat takes place in the country in a given year	Created by authors from Coups d'etat 1950 to Present dataset by Powell and Clayton, 2011[72]

Government Expenditure as Percentage of GDP	General government final consumption expenditure (% of GDP) [NE.CON.GOVT.ZS]	WDI Accessed Sept. 2022
Government Health Expenditure per Capita	Domestic general government health expenditure per capita (current US$) [SH.XPD.GHED.PC.CD]	WDI Accessed Sept. 2022
Physicians per 1,000	Physicians (per 1,000 people) [SH.MED.PHYS.ZS]	WDI Accessed Sept. 2022
Nurses & Midwives per 1,000	Nurses and midwives (per 1,000 people) [SH.MED.NUMW.P3]	WDI Accessed Sept. 2022
Dentists per 10,000	Dentists (per 10,000 people) [GHO indicator ID: 5323]	GHO Accessed July 2023
Labor Force Participation Rate	Labor force participation rate, total (% of total population ages 15+) (modeled ILO estimate) [SL.TLF.CACT.ZS]	WDI Accessed Sept. 2022
Top 10/Bottom 50	Ratio of top 10% and bottom 50% income shares	WID Accessed June 2023

Note: Data for all variables are collected at the country-year level. IGME: (UN) Inter-agency Group for Child Mortality Estimation. WDI: (World Bank) World Development Indicators. IFS: (IMF) International Financial Statistics. GHO: (WHO) Global Health Observatory. GHDx: (IHME) Global Health Data Exchange. WID: World Inequality Database.

Appendix Table 5: Two-Way Fixed-Effects Model with Reversible Binary Indicator for IMF Programme Participation as Treatment

	Neonatal Mortality
	Model 1
Active IMF Programme	0.005
	[−0.019; 0.029]
Log GDP per Capita	−0.198*
	[−0.317; −0.060]
Banking Crisis	−0.024
	[−0.051; 0.004]
Polity	0.009*
	[0.004; 0.014]
Armed Conflict	0.034
	[−0.002; 0.071]
Mean Years of Female Education	−0.021
	[−0.062; 0.021]
Adjusted R^2 (full model)	0.976
Observations	4961
RMSE	0.158
N Clusters	163

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. Model included country- and year-fixed effects. The model outcome variable was log-transformed neonatal mortality rate. The treatment variable was a binary indicator of whether the country is under an active IMF program in a given year. Coefficients and confidence intervals have been back-transformed and subtracted by 1 to be interpreted as percentage change in neonatal mortality rate associated with an increase of 1 in each predictor.

Appendix Table 6: Alternative Specifications of Stacked Difference-in-Differences Event Study

	Neonatal Mortality		
	Model 1	Model 2	Model 3
Treat x Event Time -3	-0.028^*	-0.029	0.232
	$[-0.047; -0.010]$	$[-0.059; 0.003]$	$[-0.073; 0.637]$
Treat x Event Time -2	-0.016^*	-0.007	0.205
	$[-0.028; -0.004]$	$[-0.026; 0.012]$	$[-0.074; 0.568]$
Treat x Event Time 0	0.018^*	0.020	0.251
	$[0.003; 0.033]$	$[-0.002; 0.043]$	$[-0.037; 0.625]$
Treat x Event Time 1	0.032^*	0.041^*	0.214
	$[0.012; 0.052]$	$[0.002; 0.081]$	$[-0.039; 0.533]$
Treat x Event Time 2	0.040^*	0.056^*	0.267^*
	$[0.013; 0.069]$	$[0.004; 0.110]$	$[0.014; 0.584]$
Treat x Event Time 3	0.056^*	0.081^*	0.281^*
	$[0.020; 0.094]$	$[0.015; 0.151]$	$[0.017; 0.613]$
Treat x Event Time 4	0.057^*	0.087^*	0.278^*
	$[0.013; 0.103]$	$[0.012; 0.169]$	$[0.018; 0.605]$
Treat x Event Time 5	0.055^*	0.089^*	0.303^*
	$[0.003; 0.109]$	$[0.003; 0.182]$	$[0.032; 0.647]$
Log GDP per Capita	-0.067	-0.061	-0.289^*
	$[-0.140; 0.012]$	$[-0.196; 0.097]$	$[-0.404; -0.153]$
Banking Crisis	0.000	0.002	-0.106
	$[-0.014; 0.015]$	$[-0.026; 0.031]$	$[-0.213; 0.017]$
Polity	0.002	-0.001	-0.012
	$[-0.002; 0.006]$	$[-0.007; 0.004]$	$[-0.032; 0.008]$
Armed Conflict	-0.007	-0.027	0.154
	$[-0.042; 0.028]$	$[-0.098; 0.050]$	$[-0.181; 0.625]$
Mean Years of Female Education	-0.072^*	-0.030	-0.078^*
	$[-0.122; -0.019]$	$[-0.122; 0.071]$	$[-0.131; -0.023]$
Country-Fixed Effects	Yes	Yes	No
Year-Fixed Effects	Yes	No	No
Region-Fixed Effects	No	No	Yes
Region x Year-Fixed Effects	No	Yes	Yes
Adjusted R^2 (full model)	0.996	0.996	0.904
Observations	9256	9256	9256
RMSE	0.063	0.062	0.302
N Clusters	106	106	106

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. The model outcome variable was log-transformed neonatal mortality rate. Coefficients and confidence intervals have been back-transformed and subtracted by 1 to be interpreted as percentage change in neonatal mortality rate associated with an increase of 1 in each predictor. Estimates from Model 1 are visualised in Figure 1. Model 2 replaced year-fixed effects in Model 1 with region-specific year-fixed effects. Model 3 replaced country-fixed effects in Model 2 with region-fixed effects. Event time -1 is omitted to avoid multi-collinearity.

Appendix Table 7: Stacked Difference-in-Differences Controlling for Alternative Sets of Covariates

	Neonatal Mortality		
	Model 1	Model 2	Model 3
IMF Programme	0.051*	0.063*	0.039*
	[0.011; 0.091]	[0.025; 0.103]	[0.001; 0.079]
Banking Crisis		0.003	
		[−0.010; 0.016]	
Polity		0.001	
		[−0.003; 0.005]	
Armed Conflict		0.000	
		[−0.030; 0.031]	
Mean Years of Female Education		−0.066*	−0.083*
		[−0.111; −0.018]	[−0.131; −0.032]
Log GDP per Capita			−0.058
			[−0.143; 0.036]
Total Reserves Minus Gold			−0.000*
			[−0.000; −0.000]
Democracy			0.118
			[−0.073; 0.348]
Coup d'etat			−0.014
			[−0.049; 0.023]
Adjusted R^2 (full model)	0.995	0.996	0.995
Observations	15109	10019	9353
RMSE	0.070	0.065	0.063
N Clusters	137	110	109

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. All models included event-specific country- and year-fixed effects. The model outcome variable was log-transformed neonatal mortality rate. Coefficients and confidence intervals have been back-transformed and subtracted by 1 to be interpreted as percentage change in neonatal mortality rate associated with an increase of 1 in each predictor.

Appendix Table 8: Alternative Specifications of Stacked Difference-in-Differences

	Neonatal Mortality		
	Model 1	Model 2	Model 3
IMF Programme	0.058*	0.074*	0.265*
	[0.021; 0.096]	[0.007; 0.144]	[0.008; 0.587]
Log GDP per Capita	−0.068	−0.061	−0.291*
	[−0.141; 0.011]	[−0.195; 0.096]	[−0.404; −0.156]
Banking Crisis	−0.000	0.000	−0.104
	[−0.014; 0.014]	[−0.028; 0.029]	[−0.211; 0.017]
Polity	0.002	−0.001	−0.012
	[−0.002; 0.006]	[−0.006; 0.004]	[−0.032; 0.008]
Armed Conflict	−0.008	−0.027	0.153
	[−0.042; 0.028]	[−0.098; 0.049]	[−0.181; 0.623]
Mean Years of Female Education	−0.072*	−0.033	−0.078*
	[−0.122; −0.019]	[−0.124; 0.068]	[−0.131; −0.022]
Adjusted R^2 (full model)	0.996	0.996	0.904
Observations	9256	9256	9256
RMSE	0.063	0.062	0.302
N Clusters	106	106	106

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. The model outcome variable was log-transformed neonatal mortality rate. Coefficients and confidence intervals have been back-transformed and subtracted by 1 to be interpreted as percentage change in neonatal mortality rate associated with an increase of 1 in each predictor. Model 1 is the baseline model reported in Table 2. Model 2 replaced year-fixed effects in Model 1 with region-specific year-fixed effects. Model 3 replaced country-fixed effects in Model 2 with region-fixed effects.

Appendix Table 9: Stacked Difference-in-Differences Event Study with Relaxed Treatment Group Definition

	Neonatal Mortality		
	Model 1	Model 2	Model 3
Treat x Event Time -3	−0.015	−0.025*	0.294
	[−0.031; 0.003]	[−0.049; −0.000]	[−0.005; 0.682]
Treat x Event Time -2	−0.009	−0.008	0.282
	[−0.019; 0.001]	[−0.023; 0.007]	[−0.005; 0.652]
Treat x Event Time 0	0.010	0.017	0.308*
	[−0.001; 0.022]	[−0.002; 0.037]	[0.014; 0.687]
Treat x Event Time 1	0.018*	0.036*	0.286*
	[0.001; 0.036]	[0.005; 0.068]	[0.018; 0.624]
Treat x Event Time 2	0.024	0.052*	0.317*
	[−0.000; 0.049]	[0.009; 0.096]	[0.052; 0.650]
Treat x Event Time 3	0.035*	0.075*	0.320*
	[0.003; 0.068]	[0.020; 0.133]	[0.052; 0.657]
Treat x Event Time 4	0.036	0.082*	0.321*
	[−0.002; 0.076]	[0.019; 0.148]	[0.055; 0.655]
Treat x Event Time 5	0.033	0.083*	0.334*
	[−0.011; 0.080]	[0.012; 0.159]	[0.057; 0.682]
Log GDP per Capita	−0.074	−0.071	−0.289*
	[−0.144; 0.003]	[−0.200; 0.079]	[−0.402; −0.155]
Banking Crisis	−0.002	−0.002	−0.100
	[−0.016; 0.012]	[−0.034; 0.031]	[−0.210; 0.024]
Polity	0.001	−0.002	−0.010
	[−0.002; 0.005]	[−0.006; 0.003]	[−0.029; 0.010]
Armed Conflict	−0.007	−0.023	0.155
	[−0.038; 0.026]	[−0.086; 0.044]	[−0.182; 0.630]
Mean Years of Female Education	−0.067*	−0.022	−0.081*
	[−0.115; −0.017]	[−0.110; 0.075]	[−0.132; −0.027]
Country-Fixed Effects	Yes	Yes	No
Year-Fixed Effects	Yes	No	No
Region-Fixed Effects	No	No	Yes
Region x Year-Fixed Effects	No	Yes	Yes
Adjusted R^2 (full model)	0.996	0.996	0.899
Observations	10729	10729	10729
RMSE	0.063	0.061	0.310
N Clusters	122	122	122

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. The model outcome variable was log-transformed neonatal mortality rate. Coefficients and confidence intervals have been back-transformed and subtracted by 1 to be interpreted as percentage change in neonatal mortality rate associated with an increase of 1 in each predictor. Model 1 was the baseline dynamic model with event-specific country- and year-fixed effects. Model 2 replaced year-fixed effects in Model 1 with region-specific year-fixed effects. Model 3 replaced country-fixed effects in Model 2 with region-fixed effects. Event time -1 is omitted to avoid multi-collinearity. Compared to Table 7, the construction of the stacked data set used here adopted a more relaxed definition of the treatment group that allowed countries to be included in the treatment group for multiple events as long as they remained untreated for at least 8 years before each event subsequent to the first.

Appendix Table 10: Stacked Difference-in-Differences Event Study with Stricter Control Group Definition

	Neonatal Mortality		
	Model 1	Model 2	Model 3
Treat x Event Time -3	-0.027^*	-0.030	0.233
	$[-0.049; -0.004]$	$[-0.067; 0.008]$	$[-0.117; 0.720]$
Treat x Event Time -2	-0.015^*	-0.007	0.209
	$[-0.029; -0.001]$	$[-0.030; 0.015]$	$[-0.108; 0.638]$
Treat x Event Time 0	0.017^*	0.020	0.259
	$[0.000; 0.034]$	$[-0.006; 0.047]$	$[-0.063; 0.691]$
Treat x Event Time 1	0.029^*	0.038	0.212
	$[0.005; 0.054]$	$[-0.004; 0.082]$	$[-0.067; 0.574]$
Treat x Event Time 2	0.037^*	0.052	0.265
	$[0.003; 0.073]$	$[-0.004; 0.113]$	$[-0.014; 0.623]$
Treat x Event Time 3	0.053^*	0.069	0.268
	$[0.007; 0.100]$	$[-0.001; 0.143]$	$[-0.018; 0.636]$
Treat x Event Time 4	0.054	0.083^*	0.264
	$[-0.001; 0.112]$	$[0.001; 0.172]$	$[-0.017; 0.625]$
Treat x Event Time 5	0.052	0.091	0.298
	$[-0.013; 0.120]$	$[-0.004; 0.195]$	$[-0.002; 0.689]$
Log GDP per Capita	-0.072	-0.063	-0.298^*
	$[-0.149; 0.012]$	$[-0.209; 0.110]$	$[-0.426; -0.140]$
Banking Crisis	0.000	-0.007	-0.144
	$[-0.021; 0.022]$	$[-0.066; 0.055]$	$[-0.292; 0.036]$
Polity	0.001	-0.001	-0.011
	$[-0.003; 0.006]$	$[-0.007; 0.004]$	$[-0.032; 0.009]$
Armed Conflict	-0.007	-0.026	0.138
	$[-0.041; 0.029]$	$[-0.101; 0.056]$	$[-0.206; 0.631]$
Mean Years of Female Education	-0.073^*	-0.036	-0.083^*
	$[-0.138; -0.003]$	$[-0.141; 0.082]$	$[-0.142; -0.019]$
Country-Fixed Effects	Yes	Yes	No
Year-Fixed Effects	Yes	No	No
Region-Fixed Effects	No	No	Yes
Region x Year-Fixed Effects	No	Yes	Yes
Adjusted R^2 (full model)	0.995	0.995	0.878
Observations	6571	6571	6571
RMSE	0.070	0.071	0.337
N Clusters	92	92	92

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. The model outcome variable was log-transformed neonatal mortality rate. Coefficients and confidence intervals have been back-transformed and subtracted by 1 to be interpreted as percentage change in neonatal mortality rate associated with an increase of 1 in each predictor. Model 1 was the baseline dynamic model with event-specific country- and year-fixed effects. Model 2 replaced year-fixed effects in Model 1 with region-specific year-fixed effects. Model 3 replaced country-fixed effects in Model 2 with region-fixed effects. Event time -1 is omitted to avoid multi-collinearity. Compared to Table 7, the construction of the stacked data set used here adopted a stricter definition of the control group that excluded countries from North America and Western and Northern Europe.

Appendix Table 11: Stacked Difference-in-Differences and Additional Pathways

	Dentist	LFP	Top 10/Bottom 50
IMF Programme	-0.048	-1.377	0.113
	$[-0.127; 0.039]$	$[-3.621; 0.868]$	$[-0.032; 0.257]$
Adjusted R^2 (full model)	0.978	0.887	0.986
Observations	6104	7416	11889
RMSE	0.154	3.098	0.288
N Clusters	111	127	116

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. All models included event-specific country-fixed effects and year-fixed effects. The outcome variable in column 1 was log-transformed dentist per 10,000 population. The outcome variable in column 2 was labor force participation rate (%). The outcome variable in column 3 was the ratio of top 10 percent income share and bottom 50 percent income share. The coefficient and confidence interval for column 1 have been back-transformed and subtracted by 1 to be interpreted as percentage change in dentist per 10,000 associated with participating in an IMF programme. Column 1 shows that IMF programme participation is associated with a 4.8 percent reduction in dentist per 10,000. Meanwhile, column 2 coefficient should be interpreted as a 1.38 percentage point reduction in labor force participation rate. These estimates, however, are statistically indistinguishable from 0.

Appendix Table 12: IV First Stage Results

	IMF Programme	
	Model 1	Model 2
mIMFPROG x mIMFBUDG	0.011*	0.009*
	[0.006; 0.016]	[0.004; 0.015]
Log GDP per Capita		−0.230*
		[−0.326; −0.134]
Banking Crisis		0.148*
		[0.071; 0.226]
Polity		0.009*
		[0.001; 0.016]
Armed Conflict		−0.027
		[−0.114; 0.060]
Mean Years of Female Education		0.003
		[−0.029; 0.035]
F-statistic (proj. model)	19.222	8.041
Adjusted R^2 (full model)	0.413	0.433
Observations	6509	5016
RMSE	0.352	0.360
N Clusters	193	163

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. All models included country-fixed effects and year-fixed effects.

Appendix Table 13: IV 2SLS Results

	Neonatal Mortality	
	Model 1	Model 2
IMF Programme	0.448*	0.303
	[0.011; 1.076]	[−0.101; 0.886]
Log GDP per Capita		−0.147
		[−0.302; 0.043]
Banking Crisis		−0.061*
		[−0.115; −0.003]
Polity		0.007*
		[0.000; 0.014]
Armed Conflict		0.042
		[−0.003; 0.088]
Mean Years of Female Education		−0.023
		[−0.064; 0.020]
Adjusted R^2 (full model)	0.948	0.967
Observations	6376	4961
RMSE	0.224	0.183
N Clusters	193	163

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. All models included country-fixed effects and year-fixed effects. The model outcome variable was log-transformed neonatal mortality rate. Coefficients and confidence intervals have been back-transformed and subtracted by 1 to be interpreted as percentage change in neonatal mortality rate associated with an increase of 1 in each predictor.

Appendix Table 14: IV Reduced-Form Results

	Neonatal Mortality	
	Model 1	Model 2
mIMFPROG x mIMFBUDG	0.004*	0.003
	[0.000; 0.008]	[−0.001; 0.006]
Log GDP per Capita		−0.195*
		[−0.312; −0.059]
Banking Crisis		−0.024
		[−0.052; 0.006]
Polity		0.009*
		[0.004; 0.015]
Armed Conflict		0.033
		[−0.002; 0.071]
Mean Years of Female Education		−0.021
		[−0.061; 0.021]
Adjusted R^2 (full model)	0.962	0.976
Observations	6583	4976
RMSE	0.191	0.158
N Clusters	193	163

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. All models included country-fixed effects and year-fixed effects. The model outcome variable was log-transformed neonatal mortality rate. Coefficients and confidence intervals have been back-transformed and subtracted by 1 to be interpreted as percentage change in neonatal mortality rate associated with an increase of 1 in each predictor.

Appendix Table 15: IV Pathway Analysis

	GDP	Govt. Exp. (% of GDP)	Govt. Hlth. Exp.	Phys.	Nu. & Mw.
IMF Programme	-0.474^{*}	-2.637	-0.451	0.092	0.091
	$[-0.654; -0.199]$	$[-9.402; 4.128]$	$[-0.729; 0.114]$	$[-0.311; 0.731]$	$[-0.395; 0.967]$
Adjusted R^2 (full model)	0.960	0.776	0.973	0.950	0.941
Observations	6133	5205	3734	3313	2405
RMSE	0.290	3.839	0.348	0.315	0.270
N Clusters	191	172	190	192	191

Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. All models included country-fixed effects and year-fixed effects. The outcome variable in column 1 was log-transformed GDP per capita. The outcome variable in column 2 was government expenditure (% of GDP). The outcome variable in column 3 was log-transformed government health expenditure per capita. The outcome variable in column 4 was log-transformed physicians per 1,000 population. The outcome variable in column 5 was log-transformed nurses and midwives per 1,000 population. The coefficients and confidence intervals for columns 1 and 3-5 have been back-transformed and subtracted by 1 to be interpreted as percentage change in the outcome variable associated with participating in an IMF programme.

Appendix Table 16: IV Additional Pathways

	Dentist	LFP	Top 10/Bottom 50
IMF Programme	−0.328	8.529*	−0.178
	[−0.694; 0.475]	[1.398; 15.659]	[−1.809; 1.452]
Adjusted R^2 (full model)	0.910	0.600	0.860
Observations	2473	3308	5783
RMSE	0.480	6.195	0.820
N Clusters	191	190	172

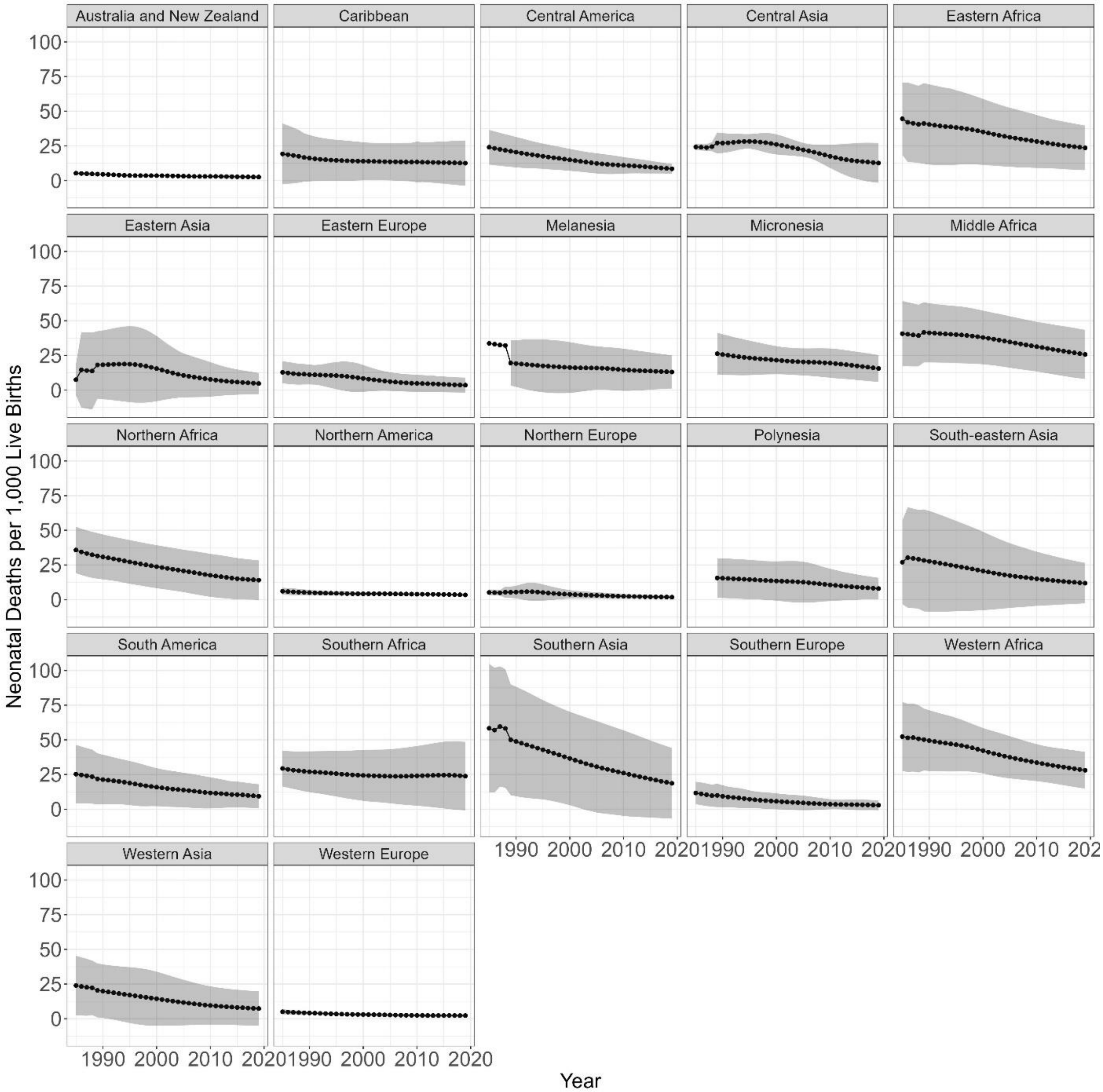
Note: * indicates that 0 falls outside the 95% confidence interval reported in brackets. Standard errors used to construct confidence intervals are robust and clustered at the country level. All models included country-fixed effects and year-fixed effects. The outcome variable in column 1 was log-transformed dentist per 10,000 population. The outcome variable in column 2 was labor force participation rate (%). The outcome variable in column 3 was the ratio of top 10 percent income share and bottom 50 percent income share. The coefficient and confidence interval for column 1 have been back-transformed and subtracted by 1 to be interpreted as percentage change in dentist per 10,000 associated with participating in an IMF programme.

Appendix Table 17: Summary Statistics

Variable	N (Country-Year)	Mean	Std. Dev.	Min	Max
Neonatal Deaths per 1,000 live births	6645	19.1	15.2	0.8	78.9
GDP per capita	6206	11514.8	18281.3	167.2	181709.3
Banking crisis	6825	0.06	0.2	0.0	1.0
Polity	5308	2.6	6.9	-10.0	10.0
Armed Conflict	6825	0.2	0.4	0.0	1.0
Mean Years of Female Education	6545	8.6	4.0	0.3	15.7

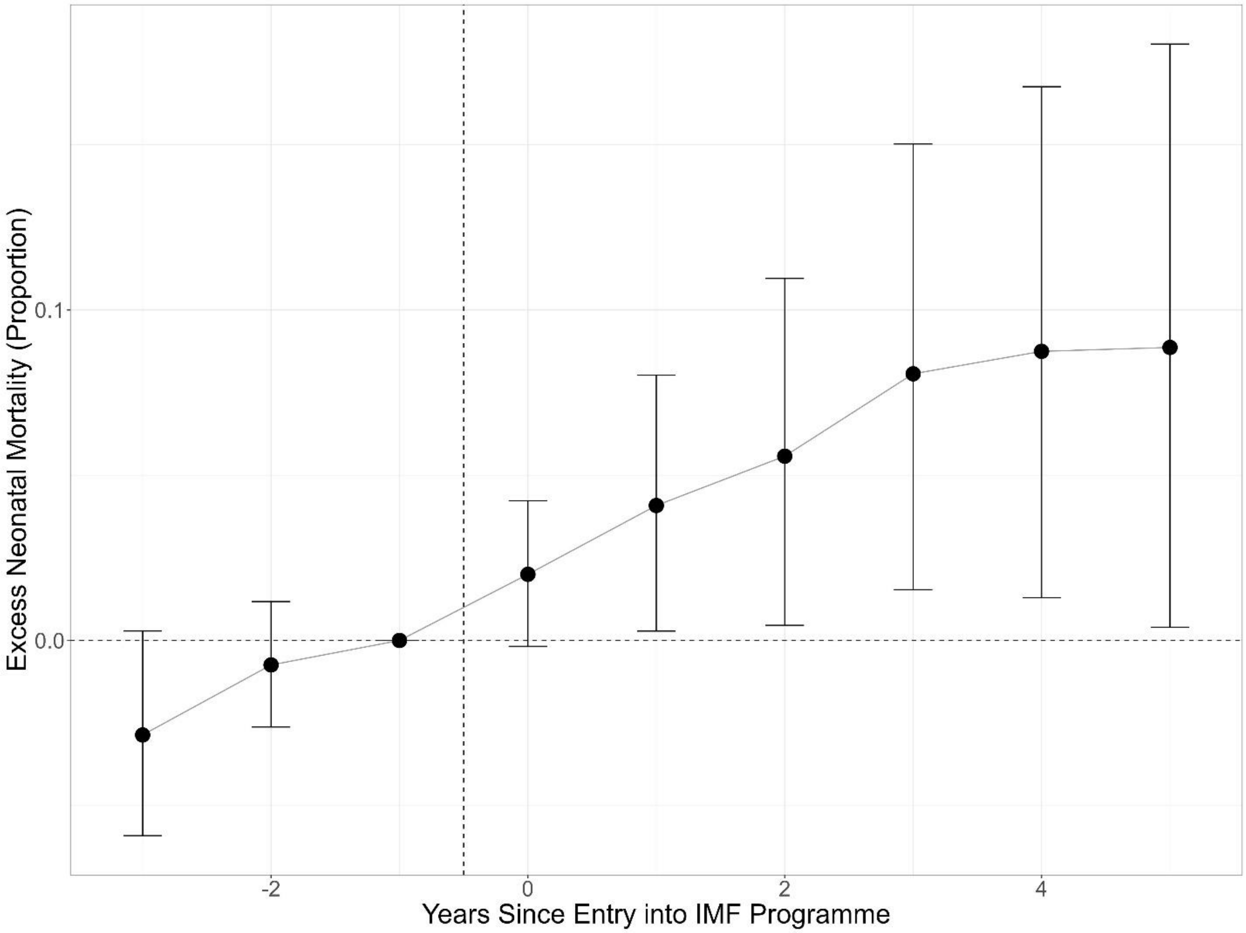
Note: See Appendix Table 4 for data sources.

Appendix Figure 1: Neonatal Mortality Rate by Region

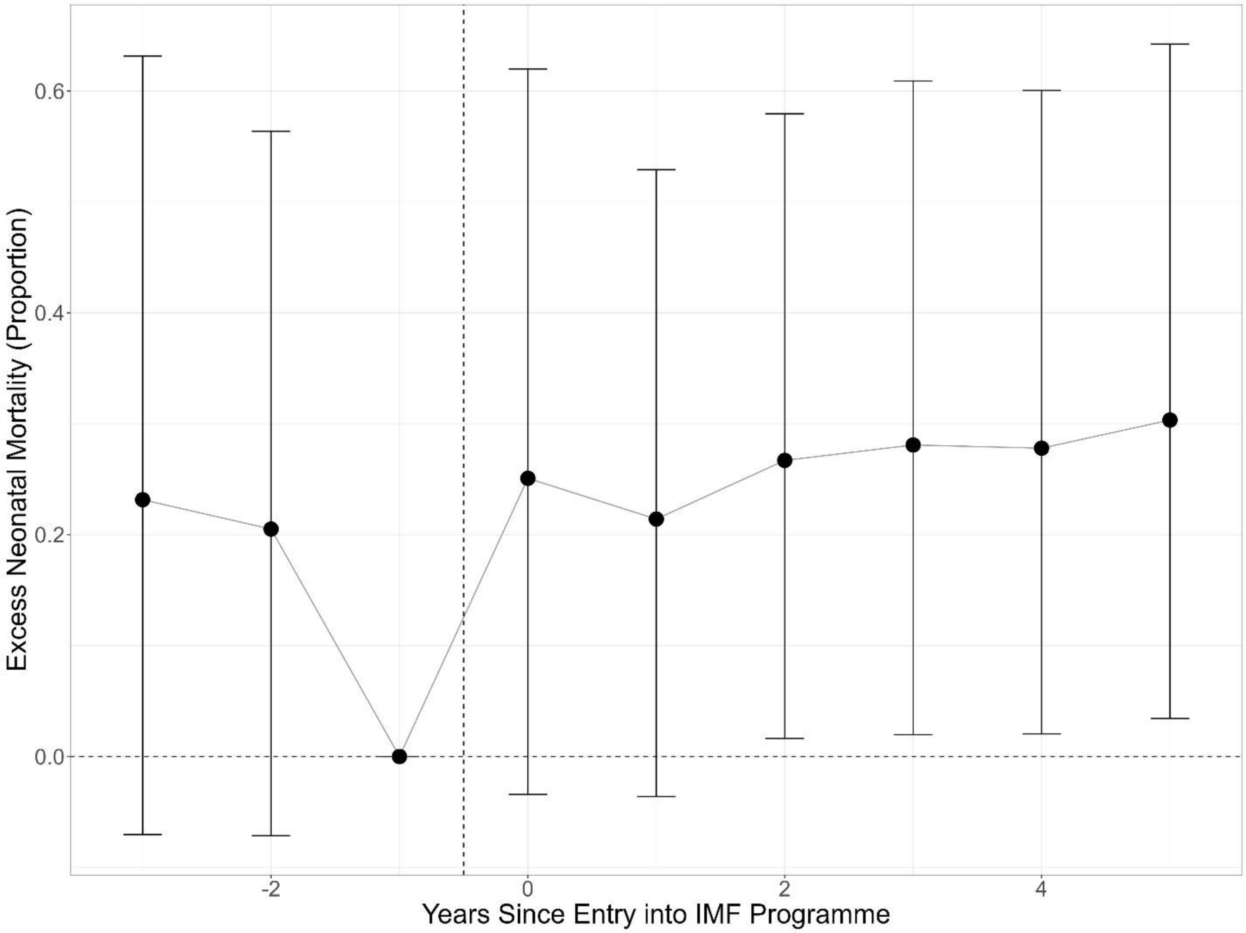


Note: Each point represents the region's mean neonatal mortality rate in a given year. Shaded area shows 1.96 SD from the regional mean. Regions are defined using UN Statistical Division's subregion classification.

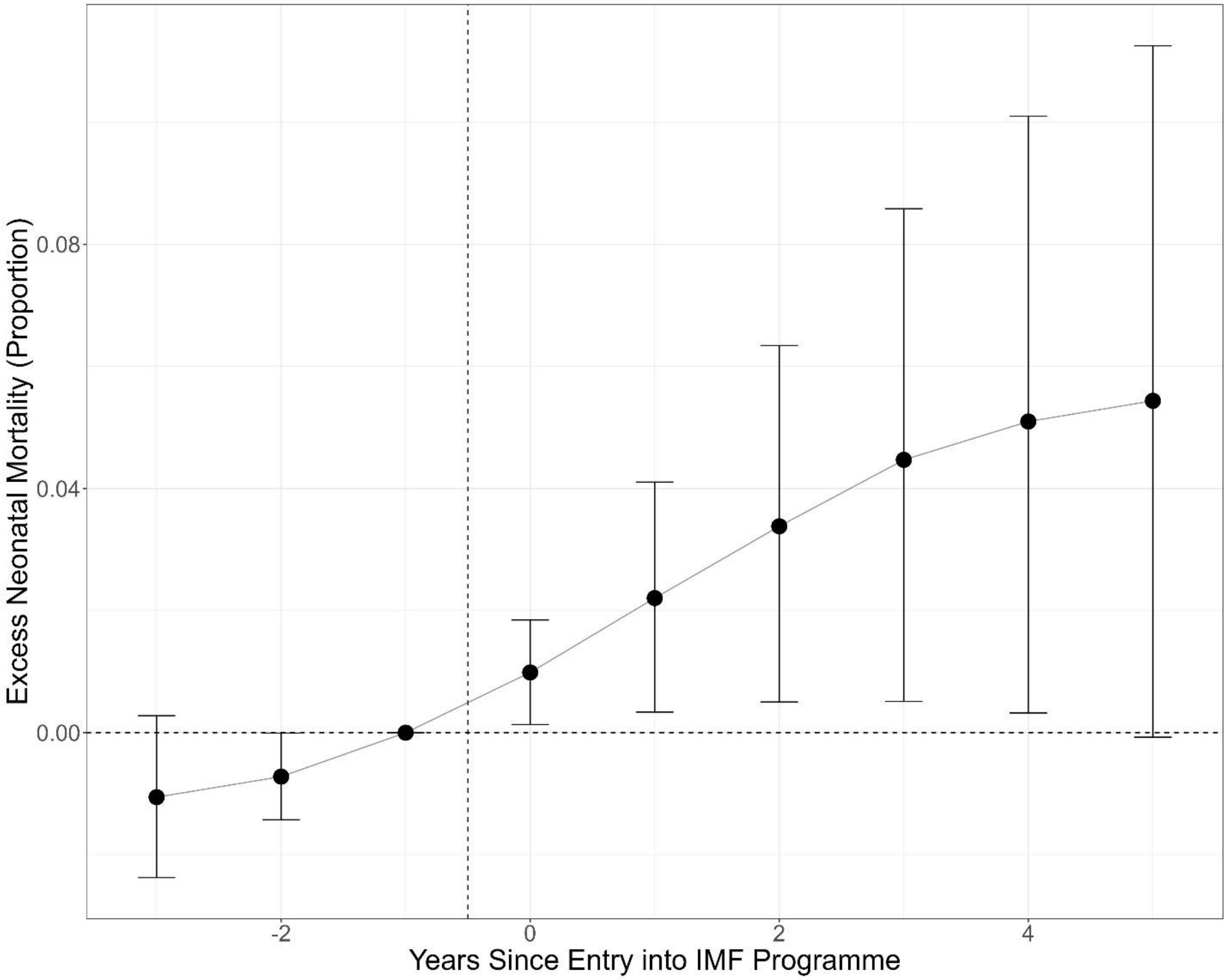
Appendix Figure 2: Stacked Difference-in-Difference Event Study with Country-Fixed Effects and Region-Specific Year-Fixed Effects



Appendix Figure 3: Stacked Difference-in-Differences Event Study with Region-Fixed Effects and Region-Specific Year-Fixed Effects



Appendix Figure 4: Local Projection Difference-in-Differences



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