ONLINE APPENDIX:

Evaluating the effects of IMF conditionality: An extension of quantitative approaches and an empirical application to public education spending

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Appendix A. Monte Carlo simulation

In this appendix, we present simulation results to show that our proposed solution for endogeneity bias performs as least as well as conventional approaches employed by research on the effectiveness of IMF programs.

We posit the following data generation process:

$$\underbrace{y^{*\prime}}_{1xJ} = \underbrace{\theta'}_{1xJ} + \underbrace{\varepsilon'}_{1xJ} \tag{1}$$

$$\underbrace{\theta'}_{1xJ} = \underbrace{y'}_{1xJ} \underbrace{\Delta}_{JxJ} + \underbrace{x'}_{1xK} \underbrace{B}_{KxJ} \tag{2}$$

$$E[\varepsilon|x] = E[\varepsilon|x_{.1}...x_{.T}] = 0 \tag{3}$$

$$\underbrace{\varepsilon}_{Jx1} \sim \mathcal{N}(\underbrace{0}_{Jx1}, \underbrace{\Sigma}_{JxJ}) \tag{4}$$

$$y = g(y^*) = [g_1(y^*), g_2(y^*), g_3(y^*)]' \tag{5}$$

Therein, $y^{*\prime}$ is the stacked vector of outcome variables (for $J = 3$ equations in the system), with y_1^* denoting education expenditure, y_2^* the latent process for being under an IMF program, and y_3^* the number of IMF conditions. Hence, g_1 and g_3 are linear, while g_2 is non-linear.

θ' refers to the linear index, and Δ is a coefficient matrix representing the effects of the potentially endogenous variables in the system. B collects the coefficients of the exogenous predictors. We assume strict exogeneity and that the errors are identically distributed (but not necessarily independent) (Roodman 2011, 168). In addition, we assume a multivariate error structure that allows for arbitrary cross-equation correlation. Hence, the matrix Σ (with typical entry $\Sigma_{ij} = \sigma_{ij}$) is one on all diagonal entries and has three unique cross-equation correlation parameters, $\sigma_{ij} = \sigma_{ji}$ for $i \neq j$.

The above system of equations has two key features. First, the number of observations can vary by equation. This also implies that different link functions may be applicable in each equation. While the outcome equation is linear, the auxiliary equations have a probit-type link. Second, the above system is potentially simultaneous as long as we do not impose more structure on the cell entries of the matrix Δ (with typical entry $\Delta_{ij} = \delta_{ij}$).

In the ideal case, in which the IMF treatments are exogenous, the matrix Δ would have all entries zero except Δ_{12} and Δ_{13}, which are the parameters of interest capturing the effects of the IMF treatments. However, in reality, we cannot readily assume IMF treatments are exogenous, which implies non-zero entries in the latter columns of the matrix Δ. Nonetheless, we may find instrumental variables for the IMF treatments. By using instrumental variables, we can ensure that the outcome variable does not appear as a right-hand side variable in the treatment equations. In other words, using valid instruments yields a ‘recursive system’ for which the likelihood function can be readily evaluated and parameters of interest obtained through maximum likelihood estimation. The Stata package ***cmp*** estimates such systems consistently under fairly mild assumptions (Roodman 2011).

We deploy our instrumental variables in two auxiliary equations. Their linear indices read as follows (now with subscripts for an individual observation):

$$\theta_{2,it} = \gamma_2 q_{it} + \underbrace{[x']_{it}}_{1xK} \underbrace{B_2}_{Kx1} \tag{6}$$

$$\theta_{3,it} = \gamma_3 z_{it} + \underbrace{[x']_{it}}_{1xK} \underbrace{B_3}_{Kx1} \tag{7}$$

The first terms in each equation, q_{it} and z_{it}, are the compound instruments that interact a time-invariant variable ($\frac{1}{T}\sum_t^T q_{it}$ and $\frac{1}{T}\sum_t^T z_{it}$) with a time-varying variable ($\frac{1}{N}\sum_t^T q_{it}$ and $\frac{1}{N}\sum_t^T z_{it}$), where N is the number of cross-section units and T is the number of time periods. In short, one may also write $q_{it} = \bar{q}_i\bar{q}_t$ and $z_{it} = \bar{z}_i\bar{z}_t$. The design matrix entails included instruments and two-way fixed effects (suppressed here for notational convenience).

To illustrate the setup of our Monte Carlo simulation, we specify the individual equations along with parameter values below:

$$y_{1it}^* = b_{10} + b_{11}x_{1it} + \nu_i + \varepsilon_{1it} \tag{8}$$

$$y_{1it} = \begin{cases} 1, \text{ if } y_{1it}^* > 0 \\ 0, \text{ else} \end{cases} \tag{9}$$

$$y_{2it} = \begin{cases} b_{20} + b_{21}x_{2it} + \varphi_i + \varepsilon_{2it}, & \text{if } y_{1it} = 1 \\ 0, & \text{else} \end{cases} \tag{10}$$

$$y_{3it} = b_{30} + b_{31}y_{1it} + b_{32}y_{2it} + b_{33}x_{3it} + \alpha_i + \varepsilon_{3it} \tag{11}$$

$$\begin{pmatrix} \varepsilon_{1it} \\ \varepsilon_{2it} \\ \varepsilon_{3it} \end{pmatrix} \sim \mathcal{N}\left[\mathbf{0}, \begin{pmatrix} 1 & \sigma_{12} & \sigma_{13} \\ \sigma_{12} & 1 & \sigma_{23} \\ \sigma_{13} & \sigma_{23} & 1 \end{pmatrix}\right] \tag{12}$$

The following table summarizes the true parameters (suppressing fixed parameters for fixed effects in each equation):

Equation 1	Equation 2	Equation 3	Global parameters
$b_{10} = 0.5$	$b_{20} = -0.5$	$b_{30} = 1.0$	$\sigma_{12} =?$
$b_{11} = 1.0$	$b_{21} = 2.0$	$b_{31} = 1.5$	$\sigma_{13} =?$
		$b_{32} = -0.5$	$\sigma_{23} =?$
		$b_{33} = -3.0$	$\rho_1=?$
			$\rho_2 =?$

These parameter values reflect previous expectations regarding the influence of IMF interventions on socio-economic outcomes. For example, in this case, country participation in IMF programs has a positive effect on education expenditure, while each additional condition has the opposite effect that is three times smaller in absolute size. Obviously, these choices are arbitrary, but they are not consequential for our simulation results.

In the following, we perform 500 simulations. Each simulation draws a sample of 1,000 observations, $N = 50$ countries and $T = 20$ years. We draw all predictors from a standard normal distribution. Errors are assumed to follow a multivariate standard normal distribution. While the equation-specific parameters for key predictors and the fixed effects are fixed, the global parameters will vary across a range of scenarios, which allows us to study the performance of different estimators.

We examine seven scenarios. The first five scenarios reflect perfectly the model structure but vary the global parameters:

Scenario 1: Correctly specified model, mild cross-equation correlation ($\sigma_{12} = \sigma_{13} = \sigma_{23} = 0.2$), and moderately strong instruments ($\rho_1 = \rho_2 = 0.27$).

Scenario 2: We assume only a weak instrument for IMF programs is available ($\rho_1 = 0.01$), otherwise as in Scenario 1.

Scenario 3: We assume only a weak instrument for conditionality is available ($\rho_2 = 0.01$), otherwise as in Scenario 1.

Scenario 4: Weak instrument for both IMF treatments ($\rho_1 = \rho_2 = 0.01$), otherwise as in Scenario 1.

Scenario 5: Correctly specified model, high cross-equation correlation ($\sigma_{12} = 0.5$, $\sigma_{13} = 0.6$, $\sigma_{23} = 0.7$), and moderately strong instruments.

In two additional scenarios, we allow for endogeneity due to omitted variables. While estimates will necessarily be biased under these circumstances, we are interested in whether this bias remains within reasonable bounds. We study two cases.

Scenario 6: An omitted variable causes both IMF programs and the outcome variable (true parameter fixed at one in both equations). All other parameters are as in the first scenario.

Scenario 7: An omitted variable causes both IMF conditions and the outcome variable (again with true parameter being one in both equations). All other parameters are as in the first scenario.

To assess the performance of estimators, we calculate three quantities of interest (Bell and Jones 2015). First and foremost is the bias, defined as the mean of the ratios of the true parameter and the estimated one. Hence, $Bias(\beta_1) = 1$ implies that the estimator yields the correct value of β_1 on average. Second, we compute the root mean squared error (RMSE), which assesses bias and efficiency; lower values of RMSE indicate better estimates. Third, optimism (O) evaluates how the standard errors compare to the true sampling variability of the simulations; values above one suggest that the estimator is overconfident in its estimates, while values below one imply overly conservative estimates. Bell and Jones (2015) use these quantities to compare the performance of different panel estimators. For a general introduction to Monte Carlo simulation, see e.g., Jackman (2009).

We compare performance across six estimators. The first is the above-discussed MLE estimator (CFA/IV/MLE). As a probit-type model, the first stage does not include fixed effects. The second is a double-IV approach, which allows us to include fixed effects also in the IMF program equation (IV/IV/MLE). The third is a two-step estimator implementing the control function approach for unobserved selection into IMF programs but without correction for endogeneity of IMF conditionality (CFA/-/OLS). The fourth uses IV to instrument for IMF programs without correcting for endogenous conditionality (IV/-/OLS). The fifth is tantamount to a two-stage least squares estimator that takes into account endogeneity of IMF conditionality but not endogeneity of IMF programs (-/IV/OLS). The last is a simple OLS regression that assumes all covariates are exogenous (-/-/OLS).

Table A1: **Scenario 1**.

	Bias(b_{31})	RMSE(b_{31})	O(b_{31})	Bias(b_{32})	RMSE(b_{32})	O(b_{32})
-/-/OLS	0.863	0.248	0.958	1.069	0.037	0.987
-/IV/OLS	0.863	0.025	0.987	1.023	0.083	0.996
IV/-/OLS	1.041	0.325	1.052	1.069	0.037	1.017
CFA/-/OLS	1.043	0.032	1.046	1.069	0.037	0.989
IV/IV/MLE	1.043	0.328	1.052	1.023	0.083	0.999
CFA/IV/MLE	1.055	0.335	1.103	1.024	0.083	0.999

Notes: Performance indicators are Bias (1 is best, <1 overestimates the true effect), RMSE (0 is best), and (O)ptimism (1 is best, <1 underestimates the true variance).

Table A1 shows the results for scenario 1. As this scenario involves a rather mild cross-equation correlation, all estimators yield acceptable results. CFA/IV/MLE and IV/IV/MLE have the smallest bias. Both underestimate the true effect of IMF programs slightly, and the effect of conditionality only marginally. The double-IV estimator is slightly better, having minimal bias for IMF program (-4.3%) and conditionality (-2.3%) and the smaller RMSE and smaller optimism. The good performance of these two estimators is due to the relatively strong instruments. A joint F-test confirms this ($F > 100$). Turning to other alternatives, the bias for conditionality is higher in the CFA approach and in the IV approach (-7%), which both instrument for IMF programs only. Instrumenting for conditionality alone minimizes the respective bias but overstates the true effect of IMF programs (+16%). Plain OLS performs worst here as it is subject to both biases.

Table A2: **Scenario 2**.

	Bias(b_{31})	RMSE(b_{31})	O(b_{31})	Bias(b_{32})	RMSE(b_{32})	O(b_{32})
-/-/OLS	0.865	0.245	0.986	1.066	0.036	1.015
-/IV/OLS	0.865	0.246	1.014	1.036	0.087	1.040
IV/-/OLS	1.445	2.053	0.509	1.066	0.036	1.043
CFA/-/OLS	0.978	5.560	0.474	1.066	0.036	1.014
IV/IV/MLE	0.586	2.390	0.670	1.036	0.087	1.037
CFA/IV/MLE	1.161	0.684	0.951	1.036	0.087	1.037

Notes: Performance indicators are Bias (1 is best, <1 overestimates the true effect), RMSE (0 is best), and (O)ptimism (1 is best, <1 underestimates the true variance).

Table A2 presents the results for scenario 2, in which the instrument for IMF programs is weak. The joint F-test in models in which both instruments are included does not indicate a weak instrument ($F > 58$), but the F-test on the IMF dummy instrument does ($F = 3.3$). Overall, CFA/IV/MLE is the preferred estimator. It understates the effect of the IMF dummy (-16%) but estimates the effect of conditionality with the smallest bias (+3.6%). Comparable performance is achieved only by the CFA estimator, which has a smaller bias on the IMF dummy (+2.2%) but a larger bias on the conditionality regressor (-6.6%). To our surprise, the linear estimators perform poorly. Double-IV has an unacceptable bias on the IMF dummy (+71%) as does the single-IV estimator (-44%). In the presence of a weak instrument for the IMF dummy, IV estimation on this variable is not advisable. In contrast, plain OLS and a conditionality-instrumented estimator have still acceptable performance (+16%).

Table A3: **Scenario 3**.

	Bias(b_{31})	RMSE(b_{31})	O(b_{31})	Bias(b_{32})	RMSE(b_{32})	O(b_{32})
-/-/OLS	0.865	0.245	0.985	1.066	0.036	1.016
-/IV/OLS	0.845	0.246	1.011	0.890	0.466	0.509
IV/-/OLS	1.042	0.320	1.045	1.066	0.036	1.042
CFA/-/OLS	1.038	0.318	1.031	1.066	0.036	1.014
IV/IV/MLE	1.046	0.324	1.045	0.284	0.657	0.551
CFA/IV/MLE	1.116	0.342	1.115	3.497	0.587	0.528

Notes: Performance indicators are Bias (1 is best, <1 overestimates the true effect), RMSE (0 is best), and (O)ptimism (1 is best, <1 underestimates the true variance).

Table A3 shows the results for scenario 3, with a poor instrument for conditionality. In this scenario, IV/IV/MLE and CFA/IV/MLE perform poorly, as the bias is more than 300% in both cases. However, the bias is concentrated in the conditionality coefficient, while the program-related bias is contained. Clearly, this is due to the weak conditionality instrument ($F = 3.4$). The standard CFA estimator performs best, as it is more slightly robust than the standard IV estimator in the IMF program coefficient and equally good in the conditionality coefficient. Plain OLS and IV estimation only for conditionality follow the ranks, given their larger bias in the first variable.

Table A4: **Scenario 4**.

	Bias(b_{31})	RMSE(b_{31})	O(b_{31})	Bias(b_{32})	RMSE(b_{32})	O(b_{32})
-/-/OLS	0.868	0.238	0.910	1.069	0.037	1.028
-/IV/OLS	0.868	0.238	0.935	1.122	0.500	0.456
IV/-/OLS	1.126	1.906	0.529	1.069	0.037	1.059
CFA/-/OLS	0.631	16.548	1.171	1.069	0.037	1.030
IV/IV/MLE	1.465	2.218	0.386	-0.024	0.596	0.509
CFA/IV/MLE	1.170	0.699	0.951	-0.280	0.617	0.459

Notes: Performance indicators are Bias (1 is best, <1 overestimates the true effect), RMSE (0 is best), and (O)ptimism (1 is best, <1 underestimates the true variance).

Table A4 on scenario 4 is the toughest scenario thus far, with two weak instruments. The joint F-test is below the conventional threshold of ten ($F = 7$), with individual F-tests being worse ($F = 3.5$). In this case, it is clearly advisable to use a simple estimator. For example, plain OLS overstates the IMF effect by 15% and understates the effect of conditionality by 7%. Instrumenting for conditionality in this context simply increases the bias in this variable but leaves the IMF dummy bias unchanged. Instrumenting the IMF dummy instead reduces its absolute bias to 13%. The CFA estimator performs less well (+59%), but again there is no contagion on the conditionality effect. Double IV performs worst in terms of bias in both variables. CFA/IV/MLE performs better -- suggesting that it is more robust against misspecification than double IV—but its bias is not acceptable either. The latter two estimators even fail to recover the correct sign of the conditionality coefficient. These results raise concerns about estimations with extremely weak instruments (Young 2018).

Table A5: **Scenario 5**.

	Bias(b_{31})	RMSE(b_{31})	O(b_{31})	Bias(b_{32})	RMSE(b_{32})	O(b_{32})
-/-/OLS	0.726	0.571	0.969	1.205	0.086	0.974
-/IV/OLS	0.726	0.571	0.997	1.024	0.079	0.988
IV/-/OLS	1.041	0.307	1.001	1.203	0.085	1.004
CFA/-/OLS	1.036	0.304	1.029	1.203	0.085	0.977
IV/IV/MLE	1.041	0.309	1.008	1.024	0.079	0.991
CFA/IV/MLE	1.024	0.297	1.044	1.024	0.079	0.991

Notes: Performance indicators are Bias (1 is best, <1 overestimates the true effect), RMSE (0 is best), and (O)ptimism (1 is best, <1 underestimates the true variance).

Assuming moderately strong instruments again, we now study a scenario with high cross-equation correlation, for instance due to unobserved third variables affecting the outcomes (Scenario 5). In this scenario, CFA/IV/MLE clearly performs best, given that is has been developed for precisely these circumstances. Its bias in the IMF dummy and conditionality regressor is minimal (-2.4%), and its RMSE is lowest. IV/IV/MLE yields acceptable results, too, with a slightly higher bias on the IMF dummy (-4%). Approaches that do not instrument for conditionality are prone to bias (-20%), while failure to instrument for endogenous IMF programs gives rise to an even higher bias (+38%). Instruments are strong, given the joint F-statistic ($F > 105$).

Table A6: **Scenario 6**.

	Bias(b_{31})	RMSE(b_{31})	O(b_{31})	Bias(b_{32})	RMSE(b_{32})	O(b_{32})
-/-/OLS	0.352	2.752	1.011	1.044	0.037	0.977
-/IV/OLS	0.353	2.752	1.037	1.165	0.153	1.020
IV/-/OLS	0.030	1.251	0.845	1.043	0.037	1.005
CFA/-/OLS	0.257	1.230	1.213	1.043	0.037	0.978
IV/IV/MLE	0.618	1.298	0.841	1.164	0.152	1.016
CFA/IV/MLE	1.020	0.555	1.370	1.159	0.149	1.009

Notes: Performance indicators are Bias (1 is best, <1 overestimates the true effect), RMSE (0 is best), and (O)ptimism (1 is best, <1 underestimates the true variance).

In the final two scenarios, we explicitly introduce bias relating to non-included omitted variable causing one of the IMF treatment and the outcome variable. Table A6 shows the results for Scenario 6, in which a third factor causes both IMF programs and the outcome of interest. Again, we find CFA/IV/MLE minimizes the bias in both treatment effects, with a negligible bias in the IMF variable (-2%) and an acceptable bias in the conditionality effect (-16%). These results are remarkable given the poor performance of the alternatives. Double-IV overstates the IMF program effect (+62%) and understates the second effect (-16.4%). Simpler estimators instrumenting for IMF programs yield a small bias on the conditionality coefficient but have unacceptable biases in the IMF program coefficient (thirty times the true value in the IV case). The same conclusion holds for plain OLS, which misses the IMF program effect by 284%. Instrumenting for conditionality alone also is no option (despite $F = 17.4$), as the IMF program effect is contaminated. The good performance of CFA/IV/MLE is partly due to strong instruments, as shown by the high joint F-statistic ($F > 72$).

Table A7: **Scenario 7**.

	Bias(b_{31})	RMSE(b_{31})	O(b_{31})	Bias(b_{32})	RMSE(b_{32})	O(b_{32})
-/-/OLS	0.812	0.377	0.927	8.145	0.423	0.891
-/IV/OLS	0.810	0.381	0.954	1.141	0.222	1.096
IV/-/OLS	-0.705	0.615	0.961	8.224	0.423	0.916
CFA/-/OLS	1.127	0.618	0.955	8.234	0.423	0.891
IV/IV/MLE	0.979	0.614	0.957	1.141	0.047	1.099
CFA/IV/MLE	0.872	0.662	1.020	1.141	0.222	1.099

Notes: Performance indicators are Bias (1 is best, <1 overestimates the true effect), RMSE (0 is best), and (O)ptimism (1 is best, <1 underestimates the true variance).

Finally, Scenario 7 in Table A7 shows the effect of introducing unobserved correlation between IMF conditionality and the outcome variable. A similar picture emerges: the two MLE estimators yield acceptable results, but the linear alternatives ought to be discarded. Double IV is the best estimator, with minimal bias in the IMF program effect (2%) and a moderate bias in the conditionality effect (14%). This second bias is the same as the respective one for CFA/IV/MLE, which has a larger bias in the IMF program effect (14%). Instruments are strong (joint $F > 79$). A somewhat worse alternative is not to instrument for IMF programs and use an IV estimator, whereas not instrumenting for conditionality yields estimates that are more than eight times smaller than the true effect. Despite high individual F-statistics in these vases (between $F = 33$ and $F = 52$), the biases are considerable.

Overall, the results show that the choice of estimator depends on the global parameters of the estimation problem. If instruments are moderately strong, the MLE estimators are the preferable ones. If instruments are weak, simpler methods are advisable. Throughout all scenarios, the CFA approach is more robust than IV estimation, as it hinges on fewer assumptions. The ideal case for CFA/IV/MLE arises when equations highly correlate with each other, but strong instruments are available, and omitted-variable bias is a potential issue. While these conditions would need to be assessed individually for each application, we suspect that they cover the widest range of possible scenarios using real-world data. Our recommendation thus is to use CFA/IV/MLE, while IV/IV/MLE is an attractive option when the researcher wishes to include fixed effects in all stages (and especially in the first stage).

Works cited:

Bell, A., & Jones, K. (2015). Explaining fixed effects: Random effects modeling of time-series cross-sectional and panel data. *Political Science Research and Methods, 3*, 133-153.

Jackman, S. (2009). *Bayesian Analysis for the Social Sciences*. Chichester: John Wiley & Sons Ltd.

Roodman, D. (2011). Fitting fully observed recursive mixed-process models with Cmp. *Stata Journal*, *11*(2), 159–206.

Young, A. (2018). Consistency without inference: Instrumental variables in practical application. *Working Paper*. London School of Economics.

Appendix B. Creating the IMF conditionality dataset

To create the conditionality dataset, we extracted relevant information from loan agreements. When requesting a loan from the IMF, countries send a letter to its management setting out the amount and duration of the loan, main objectives, and associated conditionality. These documents—drafted by country policymakers in collaboration with IMF staff—are known as Letters of Intent with attached Memoranda of Economic and Financial Policies, and are reviewed and updated in regular intervals. For example, a program that is reviewed five times over its duration is linked to six Letters of Intent and Memoranda of Economic and Financial Policies: one for the original approval and then one for each review. This set of documents forms our data, and we extracted the raw text of all conditions, including the number of times conditions were applicable per year, relevant for quantitative conditions which commonly apply on a quarterly basis. Replication of coding was performed in various stages to ensure inter-coder reliability. Where uncertainties arose, they were discussed and resolved by consensus. In all cases requiring a coding decision, we opted for the most cautious approach—that is, one that would understate conditionality.

The IMF's conditions can be either quantitative or structural. The former take the form of quantitative targets that countries have to meet and often maintain throughout the program period. Structural conditions concern a wider range of reforms in the domestic economy and afford governments less flexibility. Building on the quantitative–structural divide, the IMF formally distinguishes five types of conditions, which are indicative of the relative weight it attaches to their implementation. These five types can be further grouped into binding conditions (those that the IMF places most weight on) and non-binding conditions (less weight attached and can relatively easily be modified as the program progresses). Binding conditions directly determine scheduled disbursements of loans and must be implemented for the program to continue; whereas non-binding conditions serve as markers for broader progress assessment and non-implementation does not automatically suspend the loan. Between 1990 and 2014 we yielded a total of 50,266 conditions (33,153 binding and 17,113 non-binding).

After the conditions were extracted, the next stage of the coding process entailed classifying them into mutually exclusive policy areas, building on practices adopted by the IMF's Independent Evaluation Office (IEO 2007), the IMF Monitoring of Fund Arrangements database and academic research, and taking into account the potential for miscoding. Policy areas are summarized in Table B1 below. The process was conducted independently by two researchers and then compared. Discrepancies were discussed and resolved by consensus. Occasionally, conditions did not neatly fit in a policy area. First, some conditions included content that was in substantively different policy areas. For example, the text for a condition stipulated the "reduction in the maximum import tariff rate to 35 percent, together with an increase in the GST [general sales tax] rate to at least 12 percent". This was subsequently split into two conditions: one on trade issues and another on tax policy. Second, we classified conditions under the 'main' policy area in the majority of instances of ambiguity. Common examples are budget-related conditions, like "submit budget law to Parliament for approval, including limits on government wage bill." In this instance, despite containing measures directly affecting labor, we classified this condition under the expenditure issues policy area.

Third, where ambiguous conditions contained reforms in ‘neighboring’ policy areas, we opted to merge entire policy areas. The main examples of such merging are the categories ‘financial sector, monetary policy, and Central Bank issues.’’

Table B1. Number of conditions by policy area, 1990-2014

	Total conditions	Binding conditions	Non-binding conditions
External debt issues *Debt management and external arrears.*	13421	12025	1396
Financial sector, monetary policy, and Central Bank issues *Financial institution regulation, financial SOE privatization, treasury bills, interest rates, Central Bank regulation, money supply, and domestic credit.*	12323	7939	4384
Expenditure issues *Expenditure administration, fiscal transparency, audits, budget preparation, domestic arrears, and fiscal balance.*	8872	5327	3545
External sector (trade and exchange system) *Foreign reserves, trade liberalization, exchange rate policy, capital account liberalization, and foreign direct investment.*	4400	3676	724
Revenue issues *Customs administration, tax policy, tax administration, and audits of private enterprises.*	4092	1574	2518
State-owned enterprise reform and pricing *SOE restructuring, subsidies, price liberalization, audits, marketing boards, and corporatization and rationalization.*	2074	913	1161
Labor issues (public and private sector) *Wage and employment limits, pensions, and social security institutions.*	1936	737	1199
Institutional reforms *Judicial system reforms, anti-corruption measures, competition enhancement, private sector development, devolution, sectoral policies, social policies (excl. poverty reduction policies), price increases for food, water, public transport, or other basic needs goods, land registries, granting of property rights, environmental regulations and access to commons.*	1321	475	846
Privatizations *Non-financial SOE privatization (incl. liquidation and bankruptcy proceedings).*	1002	397	605
Poverty reduction policies *Poverty Reduction Strategy Paper development, increases in social sector spending, and implementation of social safety nets.*	825	90	735
Total	50266	33153	17113

Works cited:

IEO. (2007). *Structural Conditionality in IMF-Supported Programs*. Washington, DC.

Appendix C. Descriptions and sources of variables used in empirical analysis

Variable	Description	Source
Government education spending	Government expenditure on education as a share of gross domestic product (%)	World Bank (2016)
IMF all conditions	Total count of binding conditions in IMF program	Kentikelenis, Stubbs, and King (2016)
IMF expenditure conditions	Count of binding expenditure conditions in IMF programs, which include those related to expenditure administration, fiscal transparency, audits, budget preparation, domestic arrears, and fiscal balance	Kentikelenis, Stubbs, and King (2016)
IMF revenue conditions	Count of binding revenue conditions in IMF programs, which include those related to customs administration, tax policy, tax administration, and audits of private enterprises	Kentikelenis, Stubbs, and King (2016)
IMF participation	Dummy variables: = 1 if IMF program active for 5 or more months in a year, 0 otherwise	Authors' calculations
GDP per capita	Gross domestic product per capita in 2005 USD (logged)	World Bank (2016)
Urbanization	Urban population as a share of total population (%)	World Bank (2016)
Dependency ratio	Population aged under 15 as a share of working-age population (%)	World Bank (2016)
Democracy	Average of Freedom House and Imputed Polity measures of democracy, transformed to a scale of 0 to 10	Teorell, Dahlberg, Holmberg, Rothstein, Khomenko, and Svensson (2016)
Government balance	Difference of general government revenue and general government total expenditure as a share of gross domestic product (%)	IMF (2016)
Trade	Sum of exports and imports of goods and services measured as a share of gross domestic product (%)	World Bank (2016)
IMF liquidity ratio	IMF liquid resources divided by liquid liabilities (logged)	Lang (2016)
Education commitments of ODA	Education commitments of official development assistance in 2011 USD (logged)	OECD (2014)
Role-equivalent IMF countries	Sum of country-years that role-equivalent countries spent under IMF participation in the past three years	Authors' calculations
UNGA voting alignment	Voting similarity index with US on a scale ranging from 0 to 1, where 1 is perfect similarity and 0 is perfect difference	Voeten, Strezhnev, and Bailey (2016)
UNSC temporary membership	Dummy variables: = 1 if country is a temporary member of UNSC, 0 otherwise	Dreher, Sturm, and Vreeland (2009)

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Appendix D. Summary statistics of the variables used in main empirical analysis

Variable	Observations	Mean	Standard deviation	Minimum	Maximum
Government education spending	1307	4.409	2.152	0.999	19.258
IMF all conditions	1307	10.611	16.027	0.000	124.000
IMF expenditure conditions	1307	1.767	3.312	0.000	21.000
IMF revenue conditions	1307	0.607	1.662	0.000	13.000
IMF participation	1307	0.415	0.493	0.000	1.000
GDP per capita	1307	7.442	1.173	4.947	9.685
Urbanization	1307	48.184	20.689	6.455	94.612
Dependency ratio young	1307	57.785	22.350	19.392	107.211
Democracy	1307	6.151	2.819	0.000	10.000
Government balance	1307	-2.293	5.358	-31.226	44.901
Trade	1307	81.389	39.639	16.439	399.987
IMF liquidity ratio	1307	5.683	0.766	4.543	7.109

Appendix E. First-stage models for effect of IMF conditionality on government education spending

Model specification	1. Controls only	2. All conditions and controls	3. All conditions, exogenous controls	4. All conditions and controls	5. All conditions, exogenous controls	6. All conditions and controls
Identification strategy	OLS	OLS	Conditionality IV, Participation IV	Conditionality IV, Participation IV	Conditionality IV, Participation CFA	Conditionality IV, Participation CFA
	Dependent variable: IMF conditions, 1990-2014					
Conditionality compound	.	.	-0.4548***	-0.4510***	-0.3556***	-0.3542***
	.	.	[0.0784]	[0.0752]	[0.0657]	[0.0628]
Log(GDP per capita)	.	.	-19.6498***	-21.3877***	-15.1602***	-17.0945***
	.	.	[4.0411]	[4.0940]	[2.8350]	[3.0708]
Urbanisation	.	.	0.2150	0.1882	0.2524	0.2342
	.	.	[0.1999]	[0.2112]	[0.1777]	[0.1825]
Dependency ratio	.	.	0.0242	0.0361	0.0472	0.0542
	.	.	[0.1362]	[0.1275]	[0.0960]	[0.0905]
Democracy	.	.	.	0.7634*	.	0.1546
	.	.	.	[0.4029]	.	[0.2744]
Government balance (lagged)	.	.	.	0.3564***	.	0.3310***
	.	.	.	[0.1207]	.	[0.1105]
Trade (lagged)	.	.	.	0.0393	.	0.0321
	.	.	.	[0.0268]	.	[0.0224]
Constant	.	.	150.5320***	156.9048***	107.4382***	116.3145***
	.	.	[32.4923]	[32.5674]	[22.3446]	[23.1911]
Country fixed effects	.	.	Yes	Yes	Yes	Yes
Year fixed effects	.	.	Yes	Yes	Yes	Yes
	Dependent variable: IMF participation, 1990-2014					
Participation compound	.	.	-0.2328***	-0.2367***	0.5876***	0.5728***
	.	.	[0.0600]	[0.0581]	[0.0463]	[0.0473]
Log(GDP per capita)	.	.	-0.4769***	-0.5040***	-0.3791***	-0.4401***

	.	.	[0.1449]	[0.1359]	[0.0905]	[0.1024]
Urbanisation	.	.	0.0016	0.0000	0.0078**	0.0092**
	.	.	[0.0083]	[0.0079]	[0.0035]	[0.0040]
Dependency ratio	.	.	-0.0010	-0.0006	-0.0124***	-0.0115***
	.	.	[0.0052]	[0.0050]	[0.0036]	[0.0035]
Democracy	.	.	.	0.0535***	.	0.0383**
	.	.	.	[0.0163]	.	[0.0193]
Government balance (lagged)	.	.	.	0.0056	.	0.0054
	.	.	.	[0.0036]	.	[0.0101]
Trade (lagged)	.	.	.	0.0010	.	0.0012
	.	.	.	[0.0010]	.	[0.0015]
Constant	.	.	4.0462***	4.0636***	2.3299***	2.3471***
	.	.	[1.1258]	[1.0697]	[0.7807]	[0.7836]
Country fixed effects	.	.	Yes	Yes	No	No
Year fixed effects	.		Yes	Yes	Yes	Yes
Number of observations	.	.	1307	1307	1307	1307
Number of countries	.	.	134	134	134	134

Notes: Cluster-robust standard errors in brackets. * p<0.10, ** p<0.05, *** p<0.01.

Appendix F. First-stage models for effect of IMF conditionality on government education spending, disaggregated conditions

Model specification	7. Structural vs quantitative conditions	8. Structural vs quantitative conditions	9. Expenditure vs other conditions	10. Expenditure vs other conditions	11. Revenue vs other conditions	12. Revenue vs other conditions
Identification strategy	Conditionality A IV, Conditionality B IV, Participation IV	Conditionality A IV, Conditionality B IV, Participation CFA	Conditionality A IV, Conditionality B IV, Participation IV	Conditionality A IV, Conditionality B IV, Participation CFA	Conditionality A IV, Conditionality B IV, Participation IV	Conditionality A IV, Conditionality B IV, Participation CFA
	Dependent variable: IMF conditions A, 1990-2014					
Conditionality A compound	-0.7994***	-0.7816***	-0.2951***	-0.2544***	-0.4558***	-0.4340***
	[0.1076]	[0.1090]	[0.0692]	[0.0616]	[0.1477]	[0.1455]
Log(GDP per capita)	-4.2878**	-3.7144**	-3.9244***	-3.1242***	-1.6278***	-1.3346***
	[1.7346]	[1.6377]	[0.9642]	[0.8861]	[0.5558]	[0.4875]
Urbanisation	-0.0589	-0.0595	-0.0031	0.0026	0.0220	0.0246
	[0.0691]	[0.0676]	[0.0476]	[0.0463]	[0.0244]	[0.0244]
Dependency ratio	0.0230	0.0287	-0.0156	-0.0099	0.0052	0.0081
	[0.0371]	[0.0355]	[0.0267]	[0.0238]	[0.0159]	[0.0155]
Democracy	0.1054	0.0391	0.0623	-0.0298	-0.0350	-0.0684
	[0.1053]	[0.0961]	[0.0842]	[0.0737]	[0.0457]	[0.0422]
Government balance (lagged)	0.0608	0.0580	0.0535*	0.0492*	0.0351	0.0336
	[0.0457]	[0.0455]	[0.0280]	[0.0274]	[0.0225]	[0.0221]
Trade (lagged)	0.0053	0.0049	-0.0008	-0.0021	-0.0033	-0.0038
	[0.0069]	[0.0070]	[0.0070]	[0.0061]	[0.0043]	[0.0042]
Constant	32.8282**	27.8771**	26.3071***	19.4196***	14.0443***	11.3383***
	[12.7575]	[12.0273]	[7.5817]	[6.5981]	[4.1721]	[3.7867]
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
	Dependent variable: IMF conditions B, 1990-2014					
Conditionality B compound	-0.3331***	-0.2512***	-0.4315***	-0.3522***	-0.3732***	-0.3044***

	[0.0584]	[0.0512]	[0.0839]	[0.0782]	[0.0691]	[0.0590]
Log(GDP per capita)	-16.3983***	-12.0168***	-17.7299***	-13.9344***	-20.4214***	-16.1055***
	[2.7520]	[2.1500]	[3.7466]	[2.8816]	[3.9301]	[3.0273]
Urbanisation	0.2478	0.2485	0.1999	0.2306	0.1796	0.2197
	[0.1748]	[0.1527]	[0.1886]	[0.1627]	[0.2041]	[0.1752]
Dependency ratio	0.0068	0.0361	0.0354	0.0576	0.0036	0.0329
	[0.1058]	[0.0722]	[0.1111]	[0.0790]	[0.1206]	[0.0844]
Democracy	0.6790**	0.1652	0.6569*	0.154	0.7758**	0.2187
	[0.3240]	[0.2127]	[0.3547]	[0.2435]	[0.3815]	[0.2624]
Government balance (lagged)	0.2899***	0.2668***	0.3065***	0.2842***	0.3195***	0.2964***
	[0.0949]	[0.0870]	[0.1035]	[0.0952]	[0.1143]	[0.1053]
Trade (lagged)	0.0317	0.0281	0.0413*	0.0358*	0.0441*	0.0377*
	[0.0243]	[0.0204]	[0.0224]	[0.0193]	[0.0253]	[0.0207]
Constant	118.1641***	79.8436***	132.9463***	97.3035***	146.8721***	107.1137***
	[22.3631]	[15.2241]	[28.8702]	[21.1141]	[31.0730]	[22.3781]
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
	Dependent variable: IMF participation, 1990-2014					
Participation compound	-0.2319***	0.5585***	-0.2170***	0.5713***	-0.2103***	0.5703***
	[0.0521]	[0.0484]	[0.0569]	[0.0477]	[0.0553]	[0.0469]
Log(GDP per capita)	-0.5048***	-0.3965***	-0.5038***	-0.4356***	-0.5055***	-0.4294***
	[0.1358]	[0.0933]	[0.1360]	[0.1041]	[0.1357]	[0.1019]
Urbanisation	-0.0001	0.0089**	-0.0001	0.0089**	-0.0001	0.0092**
	[0.0079]	[0.0037]	[0.0078]	[0.0041]	[0.0078]	[0.0040]
Dependency ratio	-0.0007	-0.0090***	-0.0009	-0.0114***	-0.0011	-0.0109***
	[0.0049]	[0.0033]	[0.0050]	[0.0036]	[0.0050]	[0.0036]
Democracy	0.0536***	0.0420**	0.0528***	0.0378*	0.0535***	0.0390**
	[0.0163]	[0.0195]	[0.0164]	[0.0195]	[0.0162]	[0.0193]
Government balance (lagged)	0.0056	0.0050	0.0056	0.0056	0.0055	0.0055

	[0.0036]	[0.0100]	[0.0036]	[0.0102]	[0.0036]	[0.0099]
Trade (lagged)	0.0010	0.0006	0.0010	0.0011	0.0010	0.0012
	[0.0010]	[0.0015]	[0.0010]	[0.0015]	[0.0010]	[0.0015]
Constant	4.0716***	1.8648***	4.0716***	2.3385***	4.0862***	2.2295***
	[1.0690]	[0.6703]	[1.0690]	[0.7919]	[1.0678]	[0.7862]
Country fixed effects	Yes	No	Yes	No	Yes	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1307	1307	1307	1307	1307	1307
Number of countries	134	134	134	134	134	134

Notes: Cluster-robust standard errors in brackets. * p<0.10, ** p<0.05, *** p<0.01.

Appendix G. First-stage models for robustness checks for effect of IMF conditionality on government education spending

Model specification	13. Implementation-corrected conditions	14. Implementation-discounted conditions	15. All conditions, non-binding included	16. All conditions	17. All conditions	18. All conditions	19. All conditions	20. All conditions	21. All conditions
Identification strategy	Conditionality IV, Participation IV	Conditionality IV, Participation IV	Conditionality IV, Participation IV	Conditionality IV, Participation IV	Conditionality IV, Participation CFA	Conditionality IV, Participation IV	Conditionality IV, Participation IV (UNGA)	Conditionality IV, Participation IV (UNSC)	Conditionality IV (UNGA & UNSC), Participation IV
	Dependent variable: IMF conditions, 1990-2014								
Conditionality compound	-0.3617***	-0.2948***	-0.3953***	-0.3293***	-0.2361***	-0.4511***	-0.3264***	-0.3121***	.
	[0.0873]	[0.0811]	[0.0868]	[0.0824]	[0.0808]	[0.0772]	[0.0579]	[0.0564]	.
UNGA voting alignment	.	.	.	.	.	.	.	.	-13.8040
	.	.	.	.	.	.	.	.	[8.5227]
UNSC temporary member	.	.	.	.	.	.	.	.	-0.0034
	.	.	.	.	.	.	.	.	[0.4537]
Log(GDP per capita)	-12.7939**	-14.2407**	-33.3493***	-19.4474***	-16.0095***	-21.3142***	-22.3701***	-23.2938***	-25.9330***
	[6.0415]	[5.6678]	[6.2474]	[4.7766]	[4.3302]	[4.0482]	[4.1041]	[4.2028]	[4.6013]
Urbanisation	0.2868	0.2196	0.3610	0.0939	0.1749	0.1832	0.2591	0.1957	0.3150
	[0.3647]	[0.3377]	[0.2841]	[0.2447]	[0.2062]	[0.2160]	[0.2209]	[0.2233]	[0.2770]
Dependency ratio	0.0829	0.0362	0.0664	-0.0054	0.0399	0.0323	0.0047	-0.0237	-0.0988
	[0.1671]	[0.1561]	[0.1840]	[0.1553]	[0.1215]	[0.1330]	[0.1209]	[0.1254]	[0.1328]
Democracy	0.9413*	0.7277*	1.3852**	0.6732	-0.0114	0.7644*	0.6308	0.7137*	0.6352
	[0.5240]	[0.4084]	[0.6208]	[0.4881]	[0.3518]	[0.4018]	[0.4049]	[0.4081]	[0.4635]
Government balance (lagged)	0.3701***	0.3357**	0.5007***	0.2812**	0.2490**	0.3563***	0.3307***	0.3712***	0.3520***

	[0.1305]	[0.1307]	[0.1832]	[0.1187]	[0.1111]	[0.1186]	[0.1218]	[0.1208]	[0.1286]
Trade (lagged)	0.0666**	0.0476	0.0303	0.0477	0.0347	0.0397	0.0343	0.0394	0.0390
	[0.0337]	[0.0314]	[0.0387]	[0.0300]	[0.0243]	[0.0268]	[0.0282]	[0.0275]	[0.0326]
Log(Education commitments of ODA)	.	.	.	0.8784	0.8460*	.	.	.	.
	.	.	.	[0.5529]	[0.4466]	.	.	.	.
Role-equivalent IMF countries	.	.	.	.	.	-0.0126	.	.	.
	.	.	.	.	.	[0.0700]	.	.	.
Constant	144.2493***	155.0121***	241.5021***	125.2897***	89.9409***	157.1547***	161.8877***	168.4115***	183.4670***
	[51.7152]	[47.6404]	[48.5038]	[35.1741]	[28.6199]	[32.9031]	[32.7030]	[33.9051]	[37.7652]
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Dependent variable: IMF participation, 1990-2014								
Participation compound	-0.2660***	-0.2386***	-0.2272***	-0.1918***	0.5827***	-0.2161***	.	.	-0.0905***
	[0.0627]	[0.0634]	[0.0616]	[0.0647]	[0.0504]	[0.0656]	.	.	[0.0252]
UNGA voting alignment	.	.	.	.	.	.	0.5347**	0.0109	.
	.	.	.	.	.	.	[0.2710]	[0.0381]	.
UNSC temporary member	.	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.	.
Log(GDP per capita)	-0.1240	-0.1291	-0.5024***	-0.4456***	-0.3300***	-0.5168***	-0.5142***	-0.5307***	-0.5156***
	[0.1863]	[0.1866]	[0.1357]	[0.1459]	[0.1069]	[0.1345]	[0.1366]	[0.1381]	[0.1370]
Urbanisation	0.0194	0.0194	-0.0001	-0.0055	0.0044	0.0011	-0.0011	-0.0013	-0.0010
	[0.0127]	[0.0127]	[0.0078]	[0.0100]	[0.0036]	[0.0080]	[0.0079]	[0.0079]	[0.0078]
Dependency ratio	0.0003	-0.0001	-0.0009	-0.0015	-0.0099***	-0.0001	-0.0058	-0.0054	-0.0038
	[0.0052]	[0.0052]	[0.0050]	[0.0061]	[0.0038]	[0.0052]	[0.0046]	[0.0046]	[0.0048]
Democracy	0.0616***	0.0612***	0.0524***	0.0571***	0.0411*	0.0531***	0.0477***	0.0509***	0.0487***
	[0.0200]	[0.0200]	[0.0163]	[0.0177]	[0.0217]	[0.0164]	[0.0161]	[0.0164]	[0.0160]

Government balance (lagged)	0.0076**	0.0075**	0.0055	0.0043	-0.0027	0.0059	0.0050	0.0049	0.0050
	[0.0037]	[0.0036]	[0.0036]	[0.0038]	[0.0112]	[0.0037]	[0.0036]	[0.0036]	[0.0037]
Trade (lagged)	0.0005	0.0005	0.0010	0.0016	0.0006	0.0010	0.0010	0.0010	0.0010
	[0.0011]	[0.0011]	[0.0010]	[0.0011]	[0.0016]	[0.0010]	[0.0010]	[0.0010]	[0.0010]
Log(Education commitments of ODA)	.	.	.	0.0166	-0.0025	.	.	.	.
	.	.	.	[0.0174]	[0.0307]	.	.	.	.
Role-equivalent IMF countries	.	.	.	.	.	0.0023	.	.	.
	.	.	.	.	.	[0.0024]	.	.	.
Constant	1.7750	1.7189	4.0748***	3.3554***	1.6490	4.0030***	4.1992***	4.3887***	4.2829***
	[1.5518]	[1.5456]	[1.0695]	[1.2053]	[1.0109]	[1.0904]	[1.0503]	[1.0763]	[1.0745]
Country fixed effects	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	968	968	1307	1040	1040	1307	1285	1287	1265
Number of countries	129	129	134	120	120	134	132	131	129

Notes: Cluster-robust standard errors in brackets. * p<0.10, ** p<0.05, *** p<0.01.

Appendix H. Implementation-discounted conditionality indicators

There are three ways of addressing issues of implementation—one direct approach and two indirect approaches—each with its own set of limitations (Arpac et al., 2008).

The simplest and most direct way of correcting for implementation entails subtracting waived conditions from the total conditions applicable. Only binding conditions have such data available, where non-implementation necessitates a waiver to be granted by the Executive Board (EB). However, this method does not capture the case where an IMF program is off-track and reviews are not concluded (i.e. it does not even reach the EB for review and waiver requests). Subtracting waivers from total conditions may also be an inappropriate inflection of the burden a condition carries given that waivers reflect not only non-implementation of a condition but also partial or delayed implementation.

There are also two indirect approaches that overcome some of the shortcomings of the direct method, such as the inability to track implementation when a program does not make it to review. First, an indirect way to estimate implementation entails examining the proportion of the loan disbursed as a proxy for program interruptions. Countries can only receive the agreed-upon loan tranches from the IMF insofar as they implement the associated conditionality. Failure to do so leads to countries being unable to draw subsequent loan tranches. Therefore, the burden of conditionality can be discounted by the proportion of the loan actually disbursed (Killick, 1995; Papi et al. 2015). Two main problems exist with this strategy: countries can opt to borrow less despite meeting all the conditionality attached to their program, and—more importantly—there is a more accurate and direct way to estimate interruptions, which we adopt instead.

The second approach to correcting for implementation entails an assessment of whether or not a program was interrupted, before discounting conditions during the interruption period. Program interruptions can be directly measured by examining borrowing countries' failure to complete reviews. An interruption can be temporary, lasting a few months, or permanent. They are measured as the time lag between the initially agreed-upon review dates and the actual review dates (Mecagni, 1999). However, a limitation of this approach is that it tells us little about what actually caused the review delay. While program interruptions most often occur as a result of failing to meet conditions, they can also be due to events that are extraneous to conditionality, such as administrative delays or changes in political leadership. Given that this scenario is uncommon and no better alternatives exist, we adopt this strategy.

Following the approach adopted by IMF staff analyses (Mecagni 1999, Ivanova et al. 2006, Nsouli et al. 2006), we coded temporary interruptions—a deviation from program implementation that is subsequently corrected (i.e. the country gets back on-track with the program)—and permanent interruptions. An interruption is formally defined as a program review for a Stand-By Arrangement delayed by more than 90 days or not completed at all; or a program review for an Extended Fund Facility, Enhanced Structural Adjustment Facility, Structural Adjustment Facility, or Poverty Reduction and Growth Facility program delayed by more than 180 days or not completed at all. The exception to this rule is programs that are cancelled and replaced with another, in which case non-completed reviews are not counted as interruptions. A permanent interruption occurs if the program does not resume (i.e. there is no subsequent review after the interruption event).

Because our unit of analysis is the country–year rather than country–program, a further transformation on interruptions is required. We discount each condition in a given year within the relevant arrangement by a coefficient determined by the number of quarters interrupted in a given year within the relevant arrangement. An interruption is coded from the quarter where the program review was original scheduled up until, but not including, the quarter where the next review actually occurred. For a permanent interruption, all quarters following the interruption are coded as interrupted. Conditions in interrupted quarters for a given arrangement are discounted by the following coefficients: 1 for no interruptions; 0.75 if one quarter is interrupted; 0.50 if two; 0.25 if three; and 0 if four.

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