Effects of IMF-Supported Programs on Gender Inequality[1]

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ABSTRACT: Macroeconomic volatility often entails policies that may disproportionately impact vulnerable populations. Some economies address weak economic fundamentals with IMF-supported reform programs, which have been shown to impact gender inequality. To accurately evaluate program effects, however, requires well-matched counterfactuals: How would gender disparities have evolved in the country in the absence of the program? We analyze 125 IMF-supported programs (1994-2022) using custom-tailored control groups that match each IMF-supported program country's gender/economic trends, including fiscal spending, debt, and reserves. For most countries, we cannot reject the null hypothesis that IMF programs have no effect on gender inequality, as the observed gender outcomes were not statistically significantly different from those in controls with similar fiscal spending, debt, and reserves trajectories but no programs.

JEL Classification Numbers: *O19, O11, O5, J1*

Keywords: IMF-supported programs, Gender Inequality, Synthetic Control Methods

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We thank Chie Aoyagi, Jocelyn Boussard, Diego Calderon, Qiaoe Chen, Martin Cihak, Stefania Fabrizio, Eurydice Fotopoulou, Rishi Goyal, Deeksha Kale, Yazan Al-Karablieh, Glen Kwende, Marina Tavares Mendes, Rasmané Ouedraogo, Andrea Schaechter, Cindy Xu, Audrey Mensah Yiadom, and Genet Zinabou for comments and suggestions. We also thank the editor and three referees for helpful comments.

I) Introduction

Macroeconomic volatility impacts all segments of society, especially vulnerable populations. Economic recovery trajectories often include fiscal and monetary consolidations that are designed to stabilize the macroeconomic environment. The effects of such policies can increase gender inequality when consolidations lead to economic slowdowns and reductions in expenditures that disproportionately affect women and girls.[2] Some countries that experience substantial economic volatility also adopt IMF-supported reform programs to mitigate the economic impact and aid recovery efforts. Several studies have associated IMF-supported programs with negative gender outcomes (see Table 1). To accurately determine how IMF-supported programs affect gender, we need to compare them against well-matched counterfactuals. Counterfactuals provide a vital baseline, showing the trajectory gender disparities would have taken if the programs had not been implemented. Without this baseline, it is difficult to separate the program's contributions to gender inequality from effects produced by other factors. Failing to account for other factors, such as economic shocks or standard recovery policies, could lead to their impact being mistakenly attributed to the IMF-supported program.

While counterfactuals are never observed, they can be constructed. The novel feature of this paper is that we apply Synthetic Control Methods (SCM) to construct custom-tailored counterfactuals for each individual country that adopted an IMF-supported program. SCM emerged as "arguably the most important innovation in the policy evaluation literature in the last 15 years" (Athey and Imbens, 2017). Each synthetic control group closely matches the trend of each program country's gender inequality and economic characteristics in the years prior to program adoption. The quality of SCM's counterfactual fit for each program country allows for precise estimation of the effects of IMF-supported programs on gender.[3] We can then assess the

[2] The UN Office of the High Commissioner for Human Rights (2013) links economic consolidations with effects on health, food, water, work, social security, and education. United Nations (2015) and IMF (2024b) outline that macroeconomic policies, in general, are rarely gender neutral (Evans and others 2024, Flamini and others 2023). Business cycles and economic volatility can also affect gender inequality (either directly, see e.g., Gomes, 2024, Bluedorn et al., 2021, Thioune et al. 2024, and An et al. 2022, or indirectly, e.g., via fertility, see Madsen et al. 2020 and Madsen et al. 2018 or via wages, see Le et al. 2013).

[3] The methodology has been previously applied to the evaluation of IMF-supported program impacts in the context of GDP growth (see Kuruc, 2022; Newiak and Willems, 2017), to IMF membership and reserve accumulation (Lipscy and Lee, 2019), and to government ownership of IMF reforms (Konstantinidis and Reinsberg, 2019). A rich literature examines IMF program effects in contexts other than gender see, Andresen and Sturm (2024), Chletsos and Sintos

evolution of gender inequality for each IMF-supported program country by comparing its observed performance against the meticulously constructed counterfactual scenarios.

Our program and gender data cover the years 1990-2023 and a universe of 480 possible programs. Given the SCM design and data requirements, we are left with up to 125 IMF programs (depending on the gender indicator) that can be analyzed with proper counterfactuals across eight different gender indicators yielding 615 different country estimates of IMF-supported program effects. The gender inequality indicators we examine include (a) female labor force participation, (b) the ratio of female to male labor force participation, (c) adolescent fertility, (d) maternal mortality, (e) gender parity indices for primary/secondary/tertiary education in countries that adopted programs, and (f) the UN's Human Development Report's Gender Inequality Index (GII).[4]

Across all programs and all gender indicators, we find that for between 86 and 90 percent of program countries, we cannot reject the null hypothesis that IMF programs have no effect on gender inequality, as gender outcomes were not statistically significantly different from those observed in controls that feature similar fiscal spending, debt, and reserve trajectories but no programs. Interestingly, for the remaining program countries that do exhibit statistically significant results, roughly an equal number of countries indicate positive/negative gender impacts. The finding that gender impacts can be positive or negative is important as it augments previous findings that are based on effects that have been averaged over at times large numbers of countries (see Kern et al. 2024; and Beazer and Woo 2015, who previously examined this heterogeneity). Our findings are robust and consistent across all (i) gender inequality indicators, (ii) program years, (iii) countries, (iv) country-income levels, and (v) IMF program types. Note that the methodology does not allow us to identify how specific IMF reforms affect gender inequality, only whether

(2023), Boockmann and Dreher (2003), Dreher 2004), Dreher (2009), Dreher, Marchesi, and Vreeland (2008), Dreher and Sturm (2012), Dreher, Sturm and Vreeland (2009), Dreher, Sturm and Vreeland (2015), Fratzscher and Reynaud (2011), Kern, Nosrati, Reinsberg, and Sevinc (2023), Kern, Reinsberg, and Rau-Göhring (2019), Kern, Reinsberg, Shea (2024), and Reinsberg, Kern, Rau-Göhring (2021), Nelson and Wallace (2017), Voeten (2014), Lang (2021), Busse and Vogel (2024) and Krahnke (2023). Alternative approaches to employing the best possible matching control groups through causal identification are covered by Stubbs et al. (2020) and reviewed in our methodology section below.

[4] Aside from maternal mortality, adolescent fertility, tertiary education, and female labor force participation, the index also includes the percentage of seats in a country's parliament held by women (zero is perfect equality). For robustness, we also examined other gender inequality indices, such as the UN's Gender Development Index and the IMF's gender inequality index; results are qualitatively identical but cover fewer years.

countries with IMF-supported programs experience different degrees of gender inequality compared to the counterfactual.

Table 1 summarizes that previous studies of IMF-supported program effects on gender relied primarily on two methodological approaches: case studies without control groups or "yes/no program" dummy variables that pool countries with and without IMF programs. Case studies without counterfactuals cannot identify a treatment effect associated with IMF programs. Pools of yes/no country dummies, which distinguish between “program” and "no-program" countries, may include countries with fundamentally different economic conditions. The control pool also cannot provide tailored controls for any single IMF-supported program country. The estimated average effects of the pool of program countries may also average out individual countries’ positive and negative effects. When the broad set of “no-program” countries does not include countries facing very similar dire economic conditions as those of IMF-supported program countries, the control group exhibits a greater likelihood of better economic and gender outcomes. In this case, the effects of negative economic shocks on gender may be incorrectly attributed to IMF-supported programs because the counterfactual is not a good fit.

To illustrate the issue, consider a scenario in which adverse economic events necessitate fiscal and monetary tightening to prevent hyperinflation or default. These policies likely have harmful effects on the population at large and on gender inequality in particular. In the absence of proper control groups that exhibit similar economic and gender trajectories to program countries, studies may have mistakenly attributed the negative effects of adverse economic conditions on gender inequality to program conditions.

Alternatively, consider a scenario where the effects on gender inequality are evaluated with a custom-tailored control group for *each* IMF-supported program country. The custom-tailored control group did not adopt an IMF-supported program; however, it can be shown to have experienced very similar gender and economic conditions to those of the country of interest prior to adopting its IMF-supported program. Now, we have a counterfactual that allows us to isolate the effect of IMF-supported programs on gender outcomes without conflating the effects with those produced by economic conditions. The accuracy of the estimated program effects on gender inequality thus depends crucially on the quality of the control group that constitutes the counterfactual benchmark for each program country. Here, it is important that our control groups

can mirror the nonlinear gender trajectories in the years preceding the adoption of the IMF-supported program, exhibiting similar trajectories of gender inequality, external debt, government consumption, and foreign exchange reserves.

Table 1.
Literature Review on the Impact of IMF-Supported Programs on Gender Inequality

Study	Gender Indicator	Sample Size	Control Group	Program Effect
ActionAid (2022)	Public budgets, social spending	Case studies	None	Neg. effects discussed
ActionAid (2023)	Health, education expenditures, fem. wage bills	10 countries, case studies	None	Neg. effects discussed
Bradshaw et al. (1993)	Under-five mortality rate	Up to 64 countries, 1987	None	Some -, sig. specifications
Buchmann (1996)	Fem. sec. enrol.; fem. Econ. activity, mat. mort. fertility.	58 program countries, 1975-1987.	None	Some -, sig. specifications
Detraz and Peksen (2016)	Women's economic rights	115 countries, 1981-2004	IMF-supported program dummy	Some -, sig. specifications
Donald and Lusiani (2017)	Female labor force participation	Case studies	None	Neg. effects discussed
Forster et al. (2020)	Neonatal mortality, health system access	137 countries, 1980-2014	IMF-supported program dummy	Some -, sig. specifications
Forsythe et al. (2000)	UN Gender Development Index	129 countries, 1970-1992	Structural adj. index	Some +, -, sig. specifications
Cagatay and Ozler (1995)	Female Labor Force Participation	165 countries, 1985 & 1990	IMF/WB-supported program dummy	Some +, -, sig. specifications
Heinzel et al. (2024)	Women in ministerial positions	141 countries, 1980-2018	IMF-supported program dummy	Some +, -, sig. specifications
Kamal (2022)	Female unemployment, maternal mortality, sec. enrol. gender inequality index	181 countries, 1993-2018	IMF-supported program/cond. dummies	Some -, sig. specifications
Karamessini and Rubery (2014)	Gender employment gap	GRC, HUN, ISL, IRL, ITA, PRT, ES, UK; 2008-2012	None	Neg. effects discussed
Kern, Reinsberg, and Lee (2024)	Gender unemployment gap, female/male labor force participation	128 countries, 1992-2018	IMF-supported program dummy	Some -, sig. specifications
Pandolfelli et al. (2014)	Maternal mortality	37 countries, 1990-2005	IMF-supported program dummy	Some -, sig. specifications
Reinsberg et al. (2024)	Female public sector employment/wage ratio;	95 countries, 2000-2018	IMF-supported program dummy	Some -, sig. specifications
Shandra et al. (2010)	Maternal mortality	65 countries, 1990-2005	Structural adj. dummy	Some -, sig. specifications
Karamessini (2014)	Female employment, public sector employment/wage ratio;	GRC	None	Neg. effects discussed

A theoretical discussion of the mechanisms underlying gendered effects is helpful in anchoring the approaches of the previous literature (Table 1) and in interpreting our results below. Broadly speaking, there are two approaches. One approach suggests that IMF reforms cause gender inequality, implying that IMF-supported reforms exert a greater impact on gender than reforms in countries with similar economic characteristics but no IMF-supported reforms. On the other hand,

a second approach suggests that IMF-supported loans and reforms allow for additional fiscal space and perhaps faster economic recoveries to cushion negative effects on gender.

The specific mechanisms of gendered effects of IMF-supported programs relate to a) fiscal spending cuts, b) privatization of public services, c) labor market reforms, d) trade liberalizations, and e) currency devaluations/inflation. Floro (1995) first noted that public spending cuts often reduce social services such as healthcare, education, and childcare, which forces women to compensate through unpaid labor. Standing (1999) first suggested that privatizations cause public sector job losses, disproportionately affecting women who are overrepresented in public sector roles (e.g., teaching, nursing). Razavi (2007) also notes that such privatizations can be associated with increased costs charged by newly privatized companies in the utilities, education, and healthcare sectors, which disproportionately affect low-income women. In addition, Seguino (2000) and Braunstein (2012) argue that IMF-supported labor market reforms and deregulations may push women at disproportionately greater rates into informal jobs with lower wages, lacking social protections or collective bargaining. Çağatay (2001) suggests that trade liberalizations may produce disproportionately greater shocks in women-dominated sectors (e.g., textiles, agriculture). On the other hand, the promotion of export-oriented growth strategies endorsed by IMF-supported programs may create manufacturing jobs that can favor female employment (see Çağatay & Özler, 1995). Finally, currency devaluation and inflation increase prices for imported goods (e.g., food, medicine), which primarily strain women's budgeting roles (see Sparr, 1994).

In arguing that these effects negatively impact women, the authors assume that these effects are greater in IMF-supported program countries than in countries with similar economic conditions but no IMF-supported programs. While the above effects are usually thought to exert negative effects on gender inequality, the IMF has argued that, by the same reasoning, financial support, economic reforms, and social spending floors in IMF-supported programs may provide fiscal space that can reduce negative gender outcomes or even improve the gender impacts discussed above through faster economic recoveries (IMF, 2022; and IMF, 2019).

The paper is organized as follows: The next two sections introduce the methodology and data, followed by an example of a specific program country and its synthetic control group to highlight the mechanics of our approach. The following sections present results for all countries along with robustness checks. The final section concludes.

II) IMF-Supported Program Evaluation and Control Group Methodology

II.1) Synthetic Control Group Motivation

The credibility of statistical methodologies that employ control groups to proxy counterfactuals hinges on the extent to which these control groups accurately reflect the evolution of gender and macroeconomic variables in program countries during the years preceding the adoption of the IMF-supported program. The better the match of gender and economic characteristics between program countries and their custom-tailored counterfactuals, the greater the precision of the estimated gender impact for each program country. While a substantial body of literature has examined the impact of IMF-supported programs on gender inequality, this study is the first to assess that impact using Synthetic Control Methodology (SCM). SCM generates precise counterfactuals for each program country. By comparing gender inequality in countries experiencing economic conditions with and without programs, SCM enables us to accurately estimate the economic and statistical significance of "gender impact gaps," where the gender impact gap is the difference between the control group's and the program country's gender outcomes after an IMF-supported program was adopted.

Approaches to counterfactuals in policy evaluation include regressions with program dummies, difference-in-difference estimation, propensity score matching, and Synthetic Control Methods (SCM). Among these, only SCM accommodates time-varying matching and effects as part of the methodology. For each program country, SCM constructs a custom-tailored control group consisting of non-program countries whose economic and gender performance closely matches that of the program country in the years preceding the adoption of the IMF-supported program. We report the quality of the match for each country below. The better the program country's and the control group's nonlinear data match in the years before the IMF-supported program was adopted, the greater the accuracy of the estimated gender effect after the program has been implemented.

Below, we sketch fundamental differences between the various approaches to control groups, focusing only on those most relevant to our analysis. Table 1 highlighted that most previous studies used program dummies to identify gender effects. Counterfactuals based on pooled program dummies may not, however, provide well-tailored control groups for any one country that adopts an IMF-supported program. The approach establishes average effects that are

matched with averaged effects of the control group. Averaged effects may, however, wash out positive and negative effects at the individual country level. Also, since most non-program countries do not experience economic conditions that match the reserve, debt, or gender trajectories of IMF program countries, the program dummy approach to control groups may also conflate effects such as weak economic conditions (low reserves, high debt, and government consumption) with the effects of IMF-supported programs. In SCM, the control group matches the trajectory of key variables for program countries in the years prior to program adoption. This establishes the notion that the control group experiences similar economic conditions during the matching phase.

Difference-in-Difference (DiD) estimation requires the assumption that program countries and control groups experience parallel trends in the year prior to the program adoption (see, for instance, Abadie 2005; Angrist and Pischke 2009). Our data reveal, however, that trends in gender inequality diverge substantially and nonlinearly, even among program countries. This highlights not only that the assumption of parallel trends is violated in our data but also that it is important to base our evaluation of program effects on individual countries as the unit of interest instead of aggregates or averages. Nonlinear DiD does address nonlinear trends, but the approach requires the knowledge of researchers to specify the interactions that drive the observed trend variations (Wooldridge, 2023). When DiD methodology relies on researcher-selected control groups, it does so without an explicit metric to assess their validity. If DiD methodology or researcher-specified interactions do not accurately match the counterfactuals' trends to program countries, the accuracy of post-IMF-supported program adoption estimates is compromised.

Matching estimators use observables to correlate gender outcomes and economic conditions in countries with and without IMF-supported programs. Average gender outcomes are then compared for pools of program countries and averaged outcomes of the control group. Kellogg and others (2021) note that the applicability of matching methods is limited when trends and characteristics of gender inequality and economic conditions in the pre-IMF-supported program years differ across countries, especially due to unobservable factors. This is certainly the case across the 29 years and 125 programs in our study. Kellogg et al. (2021) suggest that such differences become particularly salient in comparative case studies covering large aggregates, such as states or countries. In those cases, the averaged program impacts may also not be representative of the diverse individual experiences of program countries that we document below.

II.2) Synthetic Control Methodology

SCM gained widespread application in policy evaluation due to the high quality of the control groups being constructed and the interpretable and transparent nature of its approach. Below, we sketch the standard SCM setup in a moderately edited summary of surveys by Wiltshire (2024) and Abadie (2021).[5] SCM constructs a unique, tailored control group for *each* country that adopted an IMF-supported program (commonly referred to as an "intervention" or "treatment"). Gender inequality is observed four years pre- and post-program.[6] SCM then constructs a counterfactual, a synthetic control group from a weighted average of "donor pool" countries that minimizes the difference in gender and economic characteristics in the years prior to the adoption of the IMF-supported program. In our application, donor pools include approximately 80 countries, with a maximum of 104 countries. The year 1994 is an outlier on the low side, with only 29 countries. This is certainly sufficient for inference and significance, as previous SCM studies had similarly sized donor pools.

The ability of SCM to provide individual and synthetically tailored control groups for each IMF program country allows it to account for heterogeneous responses to both observed and unobserved factors in the years preceding IMF-supported program adoption. Each SCM control group is constructed to match the observed (nonlinear) trend in gender inequality in each program country while controlling for other important economic characteristics, specifically reserves coverage (reserves in months of imports) and additional variables relating to debt and fiscal expenditures discussed below. The intuition is that only countries that are alike on both unobservable and observable factors follow a similar trajectory in the years prior to the adoption of the IMF-supported program. We report the quality of the match in the years prior to the adoption of the IMF-supported program in columns (4) to (7) in Tables A.1.1 to A.8.2.

After establishing the high quality of the match between the program country and its counterfactual, SCM allows for a comparison between gender inequality trajectories in countries

[5] Synthetic control methods were developed by Abadie and Gardeazabal (2003); Abadie, Diamond, and Hainmueller (2010, 2015). Extensions include Cavallo and others (2013); Dube and Zipperer (2015); Acemoglu and others (2016); Kreif and others (2016); Galiani and Quistorff (2017); Abadie and L'Hour (2021); Abadie (2021); Ben-Michael et al., (2021); Peri et al., (2022); Powell (2023); and Wiltshire (2023).

[6] We experiment with longer pre- and post-program windows below, results are qualitatively identical as reported in the robustness section below. However, the number of programs that can be examined is reduced.

with and without programs. Gender impact gaps arise when the impact of a program on gender inequality differs significantly from that of the control group (for instance, via conditionality such as changes in workplace policies, a floor on social protection, or an elimination of fuel subsidies).[7] SCM provides p-values to assess the statistical significance of gender impact gaps in the years after the IMF-supported program has been adopted.

Formally, with data for $j = 1, 2, \ldots, J+1$ countries, we assume country $j = 1$, is the "treated" country that adopted an IMF-supported program. In our baseline case, we observe countries for nine years, four years prior to the program, and $T_0 = 5$ is the program year. The post-program adoption period lasts five years, as it includes the year in which the program was adopted as well as four subsequent years. To rule out the possibility that previous programs contaminate the program's effect on gender inequality, we examine only cases where countries did not receive a program in the four years prior to T_0.[8]

The "donor pool" consists of a collection of countries, $j = 2, \ldots, J+1$ that did not adopt an IMF-supported program in the four years before and after T_0. These requirements, along with the need to balance the panel for each program and donor country, eliminate a substantial number of programs from our sample (see the next section on data). For each country at time t, we observe an outcome, a gender inequality indicator, Y_{jt}. The specific indicators we examine are discussed in the data section below. We also observe a set of k predictors of the outcome, $X_{1j}, \ldots, X_{kj}$. The $k \times 1$ vector X_1 contains predictors for the program country under observation, and the $k \times J$ matrix, $X_0 = [X_2 \cdots X_{J+1}]$ collects the values of these predictors for countries in the donor pool. Using SCM terminology, we define Y_{jt} as the potential outcome with {*N*}*o* intervention and with {*I*}ntervention, respectively; in our application, the "intervention" is the adoption of a program. For each period t, we can then estimate the '*gender gap*,' τ, between the program country and its control group

[7] The assessment of the impact of specific conditionality is outside the scope of the paper; our goal is to assess the effects of IMF-supported programs in this paper. The same is true regarding the timing of conditionality and loan tranche disbursements, which differ from program to program.

[8] Some IMF-supported programs are scheduled to be administered in sequence. If there is a one-year program at time t, and another one-year program is to begin at $t+1$ (scheduled at t), the estimated effect from t to $t+4$ includes the effect of both programs. Note also that programs may feature different disbursement schedules and different conditionalities to be imposed at different times across the program window. There is no uniform way to introduce such variation in any IMF program evaluation approach since each program is custom-tailored, and the true common denominator across programs is only the program start. Some programs may thus feature greater gender impacts earlier (if financially/programmatically frontloaded) rather than later (if programs are backloaded).

$$\tau_{1t} = Y_{1t}^{I} - Y_{1t}^{N} \quad (1)$$

Equation (1) allows for the nonlinear evolution of gender inequality and economic conditions in the program country and the control group. Before the program is adopted, the observed gender gap serves as a litmus test for how well the control group aligns with the program country's characteristics.

It is often difficult to find a well-matched counterfactual based on either a single country or an averaged effect derived from a pool of countries. Instead, SCM constructs synthetic control groups by assigning weights to countries chosen from the donor pool. These weights, represented by a $J \times 1$ vector, $W = (w_2, \ldots, w_{J+1})$, form a weighted average to match the characteristics of the program country before the program is adopted. Given these weights, the estimated gender outcomes for the control group are $\hat{Y}_{it}^{N}$, or

$$\hat{Y}_{it}^{N} = \sum_{j=2}^{J+1} w_j Y_{jt} \; . \quad (2)$$

Weights for control group countries are chosen to minimize the differences between the program country and the donor pool in the years prior to program adoption. SCM specifies a set of weights $V = (v_2, \ldots, v_k)$ that reflect the relative importance assigned to the k predictors to minimize the discrepancy between the program country's predictors, X_1, and the control group's predictors $X_0 W$. Abadie and Gardeazabal (2003) choose predictor weights, V, such that the weights of the countries in the control group, $W(V)$, minimize the discrepancy between the outcomes for the IMF-supported program and the control group's outcomes in the years prior to program adoption.[9] This is achieved by minimizing the mean squared prediction error (MSPE)

$$\text{MSPE} = \sum_{t \epsilon T_0} \left(Y_{1t} - w_2 \boldsymbol{V} Y_{2t} \; - \cdots - w_{j+1} \boldsymbol{V} Y_{j+1t} \right)^2 \quad (3)$$

where w^* indicates the MSPE minimizing weights, and the estimated gender gap in the years prior to program adoption, $t < T_0$, is given by

[9] Abadie (2021) notes that the minimization of equation (3) subject to the weight constraints may select a solution with countries that are very different from the program country in the space of the predictors, even when an alternative solution exists based only on countries with predictor values similar to those of the program country. This may lead to interpolation bias and the possibility of many solutions to the minimization problem. Whenever program countries under observation are indicated to produce a non-unique weighting vector for the synthetic control group, we exclude the program from our study.

$$\hat{\tau}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt} \ . \quad (4)$$

With the gender gap for each year prior to program adoption in hand, it is possible to use equation (4) to obtain the "*gender impact gap"* for each program country, *j = 1,* which is simply the estimated gender gap after a program has been adopted, t ≥ T_0; the null hypothesis is that $\hat{\tau}_{1t} = 0$. Here, it is important to recall that the control group matches countries based on the evolution of gender inequality and key macroeconomic indicators in the years prior to program adoption. To produce relevant counterfactuals, SCM then constructs a synthetic control group for each program country such that the two exhibit similar trajectories of gender equality and macroeconomic difficulties. Only when the control group's trajectory matches the program country's can we hope to isolate a gender impact once the program is adopted at T_0.

At this point, it is useful to summarize the fundamental differences in the construction of counterfactuals for SCM and other approaches. DiD, Matching, and Dummy approaches rely on researcher-specified control groups, typically comprising countries without programs. SCM's control group for each individual IMF program country is constructed based on a data match established from a weighted set of countries contained in a donor pool. In SCM, researchers specify the dimensions along which IMF program countries and their control groups are matched (e.g., similar gender inequality, reserves, government expenditures, debt, etc.). In alternative approaches, researchers may refine these control groups directly by either selecting countries with no programs or selecting countries with researcher-identified "economic crises." However, these methods do not explicitly evaluate how well the counterfactual aligns with the gender and economic conditions of any program country prior to the adoption of the IMF program. Instead, these approaches assume an adequate fit or, at the very least, the best possible fit based on researcher-selected observables. However, when counterfactuals do not align with the program country's actual trajectory of gender and economic conditions in the years preceding program adoption, they provide an imprecise benchmark for assessing the impact of the IMF-supported program after it was implemented.

In contrast, the SCM employs a data-driven approach to matching the gender and economic trajectory of each program country with its own custom-tailored counterfactual, the synthetic control group. The selection of the synthetic control group is thus not based on researchers' subjective assessment of trajectory similarities. Rather, it relies on actual concordance in the

trajectories of key variables of interest, which include observables and unobservables. The quality of the match is then considered indicative of the counterfactual's reliability and, by extension, the credibility of the estimated performance gap in the years after the IMF-supported program has been adopted. This fundamental distinction in methodologies highlights the SCM's potential to provide more robust and empirically grounded counterfactuals for analyzing the impacts of programs.

II.2) Sources of Bias, Bias-Correction, and Inference

II.2.1) Sources of Bias

Sources of potential bias in all approaches to policy evaluation, including SCM, include interpolation, extrapolation, and regression to the mean bias. Some biases are associated with overfitting, where an excellent pre-program fit is mistaken for a robust representation of the counterfactual. This occurs when SCM, in an attempt to match the program country's pre-program data, incorporates noise and irrelevant details from the donor pool. Such a counterfactual is then unreliable in predicting post-program outcomes. Below, we clarify sources of potential biases and discuss diagnostics that have been developed to allow researchers to identify and correct for biases.

Interpolation is central to SCM's construction of synthetic counterfactuals using weighted averages of donor countries. Interpolation bias arises when synthetic program countries do not provide reliable counterfactuals for the years following the program. There are three main sources of interpolation bias; some are associated with overfitting: (i) too many donors/predictors, (ii) matching to noise, and (iii) convex hull bias.

When donor pools are excessively large, interpolation bias and overfitting may arise since the likelihood of spurious matches with models that overfit noise increased (see Abadie, 2021). This is because larger donor pools increase the probability that a combination of countries, purely by chance, matches the outcomes of a program country in the pre-intervention years. This type of bias manifests when the optimization algorithm distributes very small, non-zero weights across many donor units in constructing the synthetic control. Our donor pools (in benchmark and robustness samples) are of similar size as donor pools in the two seminal applications of SCM in the literature (California's Tobacco Tax study by Abadie et al., 2010 and the German Unification study by Abadie et al., 2015). Also, our synthetic control groups never include many donor units with very small, non-zero weights. Note that such instances did occur for our excluded programs

whose weighting matrices were non-unique; these matches are routinely excluded from analysis in SCM. Our synthetic control groups are parsimonious; in our largest sample (female labor force participation), the average synthetic control group consists of about 4 countries with a maximum of 7 and the average donor weight across synthetic control groups is 33%.

A similar bias can arise when too many predictors are admitted that are not fundamental drivers of the gender and economic characteristics of IMF program countries. In this case, a good fit in the pre-program years may again indicate only well-matched noise. Similarly, when all pre-intervention outcomes are included in the matching vector, overfitting is also likely because the resulting weights prioritize a good fit on the pre-program gender outcomes over other covariates. Our predictor pool is parsimonious and relies only on the most basic variables the IMF uses to identify adverse economic conditions for IMF-supported programs (reserves, debt, and government consumption). We do not include all pre-intervention outcomes in the matching vector, as suggested by Kaul et al., (2022), which explains the slightly worse match in the t-4 period in the Annex Tables below.

Matching to noise or "fitting the error term" may also be a source of bias in SCM, one that may also be associated with overfitting. Seeking to find good matches for program countries' pre-program outcome trajectories, SCM attempts to fit both systematic components and idiosyncratic errors. As SCM attempts to fit idiosyncratic errors, especially in bias-correction, it is also fitting the error term, which may increase the likelihood of overfitting. If the flexibility of the model (e.g., large donor pools, many pre-program years) allows it, SCM can "chase the noise" in the pre-program years, to produce synthetic control groups that performs poorly as a counterfactual in the post-program years (see e.g., Kellogg et al., 2021 and Ben-Michael et al., 2021). While bias-correction doesn't directly involve fitting the error term of the outcome variable, the method does focus on adjusting the synthetic control's pre-program fit to account for matching imperfections in the pre-program years. Bias-correction (discussed in detail below) is implemented using outcome modeling, exactly in the same fashion as it is applied for propensity score matching estimators. We report bias-corrected results in our robustness section below. Extending the pre-program matching period to include additional years allows SCM to place greater weight on systematic effects, and we report robustness results for 6 to 12-year pre-program periods in the robustness section below. Note, however, that in our application, systematic shocks occur, by definition, close to the program year.

Another source of interpolation bias may arise when the program countries' characteristics lie outside the convex hull of countries in the donor pool. In this case, the pre-program fit is low, and the synthetic control cannot accurately represent the counterfactual in the post-program years (Kellogg et al., 2021). With bias-correction, the quality of the fit prior to program adoption is excellent throughout, with near-zero effect gaps in the pre-program years. Without bias-correction, there are instances when SCM cannot produce quality synthetic control groups; programs are then excluded when the gaps between program country and control group exceed 10% of the gender indicator in the final pre-program matching period. While this matching quality exclusion threshold affects the number of programs we can include in the analysis, we experimented with larger and smaller thresholds to find that our results are not robust.

Extrapolation bias may occur in SCM when the weighted average of the control countries does not accurately represent the counterfactual outcome of the program country after the program is adopted. The above-discussed interpolation biases may thus drive extrapolation bias, also known as regression to the mean bias (see Illenberger et al., 2020). This bias may occur when countries that selected into the synthetic program country exhibit particularly high or low pre-program outcomes in the pre-program years and then subsequently naturally regress back toward their means, regardless of whether they had a program or not. This may create spurious effects in the post-program years in the absence of a program. This type of bias is unlikely to exist in our application since pre-program years are generally characterized by nonlinear trajectories, as the nature of IMF-supported programs involves a collapse of reserves and sharp, nonlinear changes in debt and government spending immediately before program adoption that must be matched by synthetic control groups.

II.2.2) Bias-Correction

When bias exists, the estimated gender impact gap after the program has been adopted may be contaminated. Researchers developed several methods and diagnostics to address and correct for these biases in synthetic control settings. SCM bias-correction, introduced by Abadie and L'Hour (2021) and Ben-Michael et al. (2021), is known as Augmented Synthetic Control Methods (ASCM). It is analogous to bias-correction methods proposed for matching estimators (see Abadie and Imbens 2011). Note that while bias-correction may not necessarily improve the fit of the outcome variable, it does address discrepancies in the linear combinations of predictors used to

establish the outcome variable for program countries and controls. ASCM "de-biases" the original SCM estimate using an estimated bias. ASCM relies on the outcome model to correct for mismatches and controls pre-program fit while minimizing extrapolation bias from the convex hull by combining the strengths of SCM (matching pre-program outcome trends) with regression-based adjustments (accounting for remaining imbalances).

We implement Wiltshire's (2025) bias-correction that is based on Ben-Michael et al.'s (2021) approach in our robustness section. We prefer results with bias-correction because they provide the best pre-program fit, and hence, one might argue the best estimation of gender effects. However, to address concerns regarding overfitting, we report non-bias-corrected results in the main section and relegate bias-corrected results to the robustness section. ASCM also produces bias-corrected p-values to assess the significance of the observed gender impact gap (see Abadie, Diamond, and Hainmueller 2015). For each program country, *i*, the vector $\widehat{W}^i$ is obtained from the estimation on the uncorrected outcomes, Y_{jt}, to minimize the root mean square prediction error, *R*,

$$R = \left(\frac{1}{T_0}\sum_{t=1}^{T_0}\left(Y_{1t} - \sum_{j=2}^{J+1} w_{1j}^{*} Y_{jt}\right)^2\right)^{1/2}.$$

Let $\hat{\mu}_{0t}^{1}(x)$ be a sample regression function for program country 1 predicting Y_{It} given $X_I = x$, estimated by first regressing Y_t on all predictor variables. Then $\tilde{Y}_{1t} = Y_{1t} - \hat{\mu}_{0t}^{1}(X_1)$ and for donors, $\tilde{\hat{Y}}_{1t}^{N} = \sum_{j=2}^{J+1} \widehat{w}_j^1 \left(Y_{jt} - \hat{\mu}_{0t}^{1}(X_j)\right)$ can be calculated using the bias-corrected gender impact gap, $\hat{\tau}_{BC1t} = \tilde{Y}_{1t} - \tilde{\hat{Y}}_{1t}^{N}$, or

$$\hat{\tau}_{BC1t} = \left(Y_{1t} - \sum_{j=2}^{J+1} w_{1j}^{*} Y_{jt}\right) - \sum_{j=2}^{J+1} w_{1j}^{*} \left(\hat{\mu}_{0t}^{1}(x_1) - \hat{\mu}_{0t}^{1}(x_j)\right), \qquad (4')$$

The first term on the right-hand side of (4') is the synthetic control estimator in (4), and the second term is a regression adjustment correcting for discrepancies between the predictor values for the program country and the predictor values that contribute to the synthetic control. We can also interpret (4') as a synthetic control estimator applied to regression residuals. Equation (4') nests the "classic" synthetic control estimator without bias-correction in (4) when the estimated bias from predictor variable discrepancies is zero. Note that bias-correction affects only the accuracy with which the synthetic control matches a program country's gender and economic indicator trajectory in the years prior to the adoption of the IMF program. Since bias-correction focuses on the prediction error, it may be seen as overfitting and we report bias-corrected results only as

robustness results below to confirm that bias correction does not change our results dramatically. Ultimately, results with and without bias correction are qualitatively identical (see Table 2a and Table 2b).

II.2.3) Inference and Overfitting Diagnostics

Our goal is not only to quantify whether a gender impact gap exists but also to determine whether gaps are statistically significant. Large-sample inference is generally inappropriate for SCM since donor pools are usually small. Following Wiltshire (2024) and Abadie, Diamond, and Hainmueller (2010, 2015), we employ an SCM inferential procedure based on permutations or "in-space placebo tests." In these tests, treatment status is permuted for *untreated* countries in the donor pool. Each donor pool country, $j \neq 1$, is then assigned an "as-if treatment" status, and SCM is then applied to this country to produce an "as-if treated" or in-space placebo gender impact gap (4'). This as-if treatment permutation across all untreated countries yields a permutation distribution of bias-corrected gender impact gap estimates for placebos.

As in traditional statistical inference, a quantitative comparison between the distribution of placebo effects and the program effect is operationalized using p-values. These p-values, based on classical randomization inference, are constructed by calculating the fraction of placebo gaps that are greater than or equal to the effect estimated for the program country (Rosenbaum 2005). The gender impact gap observed for the program country is considered significantly different when its magnitude is extreme relative to the permutation distribution. Operationally, p-values are based on the ratio of the root mean square prediction error in the years before and after program adoption,

$$r_j = \frac{R_j(T_0+1,T)}{R_j(1,T_0)} \tag{5}$$

This ratio measures the quality of the synthetic control fit in the pre-and post-treatment periods. The p-value for the inferential procedure is then based on the permutation distribution

$$p = \frac{1}{j+1}\sum_{j=1}^{j+1} I_+\left(r_j - r_1\right), \tag{6}$$

where I_+ is an indicator function that returns one for nonnegative arguments and zero otherwise. Confidence intervals are constructed as described by Firpo and Possebom (2018) and Arkhangelsky et al. (2021).[10]

Excellent pre-treatment fit is desirable and indeed a key feature of SCM because that is how the method establishes that the synthetic control is a plausible counterfactual. The fit of our SCM matches (see Figure 1 as an example) looks very similar to Figures 2 in two previous, seminal SCM applications to the California Tobacco Tax (Abadie, 2010) and German Unification (Abadie, 2015). All three Figures exhibit excellent fits for the pre-program years, which is exactly what SCM's data-driven approach is designed to achieve. At the same time, an excellent pre-program fit may raise suspicions of overfitting. How can we distinguish between overfitting and excellent matches? The placebo tests described above have been developed to examine whether pre-program fit reflects stable underlying relationships or overfitted models that merely capture noise. The placebo test asks: "If the treatment had occurred for other countries in the donor pool, would we see similar effects as for the program country?" By performing such "false" treatment exercises, it can be assessed how likely the observed gender gap is to have occurred by chance or how likely it is the result of SCM overfitting the pre-program periods.

Evidence for overfitting exists when the placebo permutations exhibit systematically similar post-program effects in terms of size and magnitude as the program country. Such a pattern would suggest that SCM is overfitting the pre-treatment data in a way that leads to spurious post-treatment effects, or that there is simply a common underlying trend that SCM captures for countries, regardless of treatment (Ben-Michael et al., 2021). In our application, placebo tests all present as diverse distributions of effects that do not systematically replicate the program country's effect in terms of size and sign. We observe rich distributions of results over permuted countries; see Figure 2 as a representative example, where the visual representations of placebo permutations for the program country and placebos display unsystematic differences in the placebo post-program years. Figure 2 is representative of the permutations we observed for all our countries (the 615 individual Figures are available upon request). In summary, placebo tests are crucial for inference and for establishing the credibility of SCM results. They act as a form of falsification

[10] The estimation is operationalized using Wiltshire's (2024) Stata package that leverages the synthetic control method Stata package provided by Abadie, Diamond, and Hainmueller (2011).

test, helping to rule out the possibility that an observed treatment effect is merely a result of chance, model misspecification, or, most relevant to this discussion, overfitting to pre-treatment data. If the model is overfitting, the "effects" seen in placebo tests would falsely mimic the program country, thereby diminishing confidence in the main finding.

II.3) Data

Data are primarily from the World Bank's World Development Indicator database (WDI 2024). Annex B provides a detailed description of each variable, along with the associated WDI codes. Data span from 1990 to 2023, the most recent year available at the time of writing. Given the methodological constraints of SCM, this allows us to examine programs from 1994 to 2022 and to report gender effects from 1994 to 2023. The list of programs is derived from the IMF's Commitment Database (IMF 2024d), with cross-checks to the IMF's MONA database (IMF 2024c).[11] One could argue that only structural adjustment programs should be included since much of the previous literature focused on these types of IMF and World Bank programs and their effects on gender. However, other IMF-supported program types may also impact gender inequality and have been associated with gender impacts in previous studies. At the outset of our study, we remained agnostic about which program might produce a gender impact—hence we include all programs with ex-post conditionality. As demonstrated in the robustness section below, program-type does not affect our conclusions.

From the global dataset, we created an individual dataset for each program country. Our analysis includes eight different indicators of gender inequality to capture gender inequality of outcomes and opportunity: the ratio of female to male labor force participation, female labor force participation, adolescent fertility, maternal mortality, and gender gaps in primary, secondary, and tertiary education. We also consider the UN's (United Nations 2025) gender inequality index as a composite measure of gender inequality.

Data availability and study design significantly influence the size of our sample. We have two main criteria for inclusion: (1) for program countries to be included, a country must not have

[11] We cast the widest possible net and exclude only programs without ex-post conditionality (Rapid Credit Facility, RCF; Rapid Financing Instrument, RFI; Flexible Credit Lines, FCL; Short-term Liquidity Line, SLL; Flexible Credit Line, FCL). The program sample consists of roughly equal shares of General Resource Account (GRA) and Poverty Reduction and Growth Trust (PRGT) programs. Below we show that the specific type of IMF programs is not relevant to our results; effects of IMF-supported programs are qualitatively similar across IMF program types.

adopted another program in the four years prior to the current program. This ensures that during the pre-program years, when a control group is selected, the outcome variable is not influenced by any other conditionality. (2) For each year *t*, we create a donor pool of countries from which we select a control group, as described in the methodology section. This donor pool consists of all countries that did not receive a program during the entire eight-year period (four years before and four years after the adoption of a program). Additionally, we exclude high-income countries (as classified by the World Bank) from the donor pool when a country that adopted a program was not a high-income country. SCM also requires a balanced panel for all included countries. These data and design requirements reduce our sample from 480 program countries to at most 125 (for the case of female labor force participation).[12]

In our baseline analysis, we examine the largest number of programs that could be included in the analysis. To achieve this, we choose as predictors the lagged gender inequality indicator as well as reserves coverage (reserves in months of imports) as an indicator. For robustness, we extend the set of predictors to include not only the lagged gender indicator and reserve coverage (reserves in months of imports), but also government consumption, and reserves as a percent of external debt. This robustness check yields a smaller sample and qualitatively similar results, as shown below.

Finally, we examine alternative approaches to donor pool selection, focusing only on countries that are preselected as struggling with key economic conditions. The previous literature focused on countries that were identified as "crisis" countries in seminal papers by Rogoff and Reinhart (2009), Laeven and Valencia (2018), Schularick and Taylor (2012), Romer and Romer (2017), and Nguyen et al. (2022). The "crisis" databases reveal surprisingly little overlap in their country-year observations that are identified as "economic crises." This is easily understood, given that different researchers stipulate different criteria and thresholds for "crises." In our baseline, we follow the strict SCM approach, where the control group for program country "x" is simply

[12] Of the 480 programs, 199 were eliminated due to missing data (balanced panel); 113 programs were dropped due to the requirement that no other IMF program could occur within the matching window; 32 programs were excluded due to non-unique weighting matrices or insufficiently matching donor pools; and 11 programs were removed due to our matching quality exclusion restriction. Balance test diagnostics show no statistically significant differences for Reserve Coverage (our benchmark runs) and Reserves as Percent of External Debt between our sample and the programs that could not be included (61% after all due to missing data). For Government Consumption as Percent of GDP, which is one of the robustness variables, the excluded programs have a statistically significant 1.6 percent greater mean.

"synthetic x," which best mirrors country "x" outcome trajectory in the years prior to IMF program adoption, following its nonlinear trends in terms of gender, reserves, debt, and government consumption. An alternative is to introduce researcher priors as to how "economic crises" are defined. In our robustness section, we thus also examine results when the donor pool is restricted to countries without IMF programs that were identified as "crisis" countries in prominent "economic crisis" databases. Results are qualitatively identical.

II.4) The Mechanics of SCM: An Application to a Specific IMF-Supported Program

To illustrate the mechanics of SCM, we provide a step-by-step example relevant to our study. We apply SCM to the Dominican Republic's 2003 program with the IMF and to a single indicator of gender inequality, female labor force participation. We provide visuals that highlight the SCM approach and the associated changes in female labor force participation in the Dominican Republic and its synthetic control group over time. The numeric estimates for each year are provided in row 53 of Annex Table A.3.1.

In 2003, the Dominican Republic negotiated a $600 million "Stand-By Arrangement" with the IMF, resulting in an immediate disbursement of $120 million. The arrangement followed economic hardship that developed over the previous years (see IMF, 2003). The years prior to IMF program adoption were marked by a sharp deterioration in public finances (doubling the public debt-to-GDP ratio), a contraction of GDP growth (from 5.9 percent in 1999 to -1.3 percent in 2003), and an 80 percent currency depreciation of the Dominican Peso (from DOP 15.83 per USD in 1999 to DOP 29.38 per USD in 2003), alongside substantial capital outflows—the central bank lost half of its foreign currency reserves.

The program required fiscal consolidation through "a raise [in] the fiscal primary surplus over the next three years to a level that would allow a gradual reduction in the public debt ratio." This was to be achieved through "a broader reform of the tax system and spending." In addition, the program necessitated monetary consolidation so that "monetary restraint will help contain pressures on the currency and inflation." Spending floors were also included, with the government "making additional efforts to reduce poverty, with improvements in the efficiency and targeting of social programs. They have sought to avoid cuts in social programs" (IMF, 2003).

The case of the Dominican Republic is representative of the difficult economic conditions that countries in our sample faced that adopted IMF-supported programs. The core elements of the

recovery plan, fiscal/monetary consolidations and social spending floors are common measures observed in countries with low reserves, high debt, and government consumption. They aim to curb inflation and control government debt while avoiding severe social outcomes. Figure 2 shows Female Labor Force Participation Rates in the Dominican Republic (bold) and its synthetic control group (dashed) selected from 39 donor pool countries—those that featured similar income classifications and a balanced panel for all variables across the SCM period of investigation. SCM selects a subset of countries from the donor pool to construct a "Synthetic Dominican Republic" by minimizing the discrepancy between the Dominican Republic's outcomes and the control group's outcomes in the years before program adoption, as laid out in equation (3) earlier. In the case of the Dominican Republic's program with the IMF, the SCM assigns four countries the following weights: Mauritius (0.591), Belize (0.298), Saint Vincent and the Grenadines (0.097), and Algeria (0.014).

Note that none of the countries that comprise the synthetic control actually experienced an "economic crisis," as measured by Reinhart and Rogoff's (2009) "crisis" indicator dataset. The synthetic control group did, however, trigger rule-of-thumb indicators that the IMF uses to identify countries with weak economic fundamentals: Reserves in Months of Imports fell below 2 months of imports for several years. Such balance of payments issues are similar to those observed in the Dominican Republic at the time, which struggled with Reserves in Months of Imports of around two months over much of the event horizon.

Our primary interest in this paper is, however, gender inequality. In our baseline case, we include lagged Female Labor Force Participation and Reserve Coverage in the set of predictors. The pre-program match in Figure 1 illustrates how well the synthetic control fits the (nonlinear) evolution of female labor force participation in the Dominican Republic in the years before program adoption. This reflects the quality of the control for the counterfactual, which we also observe in similar plots for other countries.

Figure 1 shows that SCM constructs a well-fitting synthetic control for the Dominican Republic's non-linear pre-IMF-program female labor force participation. The quality of the match provides confidence in the accuracy of the estimated gender impact gap after the program was adopted. Note that the Dominican Republic's female labor force participation dropped slightly at the start of the program but recovered after 2004. The gap to the synthetic control group increased,

however, since the synthetic control group experienced an improvement in the Female Labor Force Participation Rate.

Figure 1.
Female Labor Force Participation Rate: Dominican Republic and its Control Group

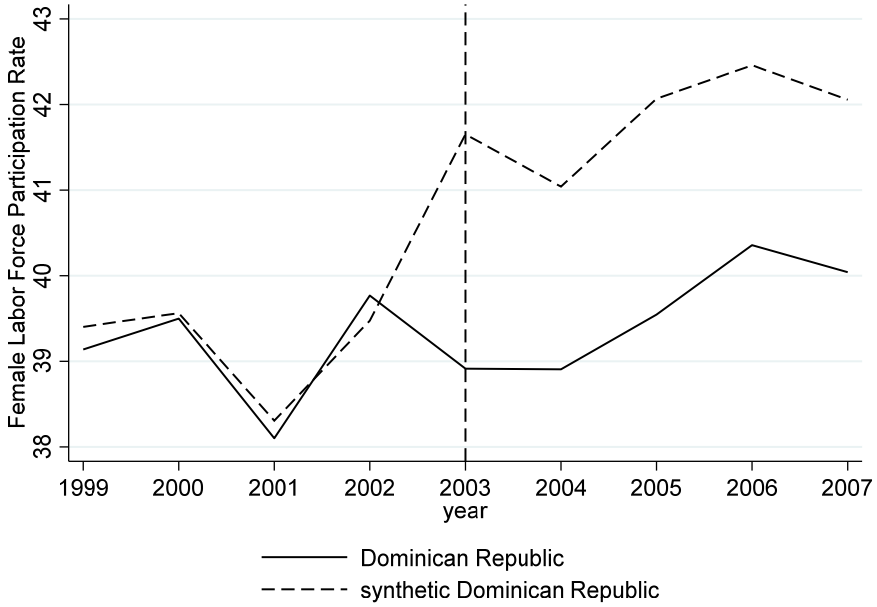
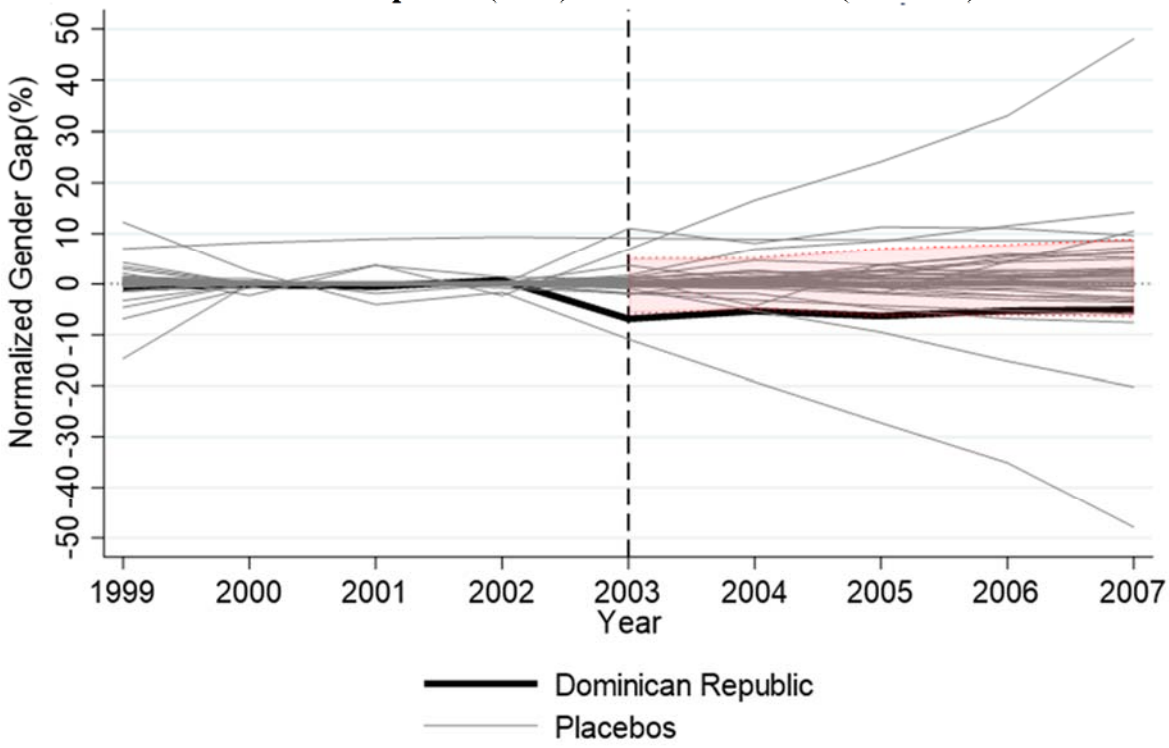


Figure 1 shows raw levels, but our focus is on the normalized gender gap below. The normalized gap represents the percentage difference between the Dominican Republic's indicator and the 'Synthetic Dominican Republic' indicator, normalized by the program country's indicator level in the last pre-program year. This ensures that all program effects are measured as deviations from a common baseline, and percent gaps are comparable across the countries in our Annex Tables A.1.1 to A.8.2.

Figure 2 shows this gender impact gap for the Dominican Republic as a solid black line, while the light gray gender impact gaps are for the as-if treated placebo permutations of the donor pool, along with the 90% confidence interval. Visual inspection reveals that the Dominican Republic's experience was unusual (as lying outside the 90% confidence band) in 2003, the year the program was adopted and 2004. This is also indicated in A.3.1 row 48. Figure 2 highlights that, in the particular case of the Dominican Republic's 2003 IMF-supported program, the effect of the program on gender is negative, although the *level* of the Dominican Republic's female labor force participation recovers after the initial drop in the program year. This is because the program country improved at a slower rate than the control group.

Figure 2
Normalized Gender Impact Gap: Female Labor Force Participation Rate, Dominican Republic (Bold) versus Donor Pool (Placebos)



The negative effects and their sizes, especially in 2003 and 2004, are thus not only a function of the Dominican Republic's own evolution of Female Labor Force Participation, but also because its recovery does not match or catch up to the evolution of Female Labor Force Participation in the control group. These insights emphasize the importance of well-matched control groups when evaluating the impact of programs on gender. The point is not (only) to identify a direct positive or negative gender effect in program countries; rather, the analysis must compare the evolution of gender inequality after program adoption to that of the control group.

III) The Impact of IMF-Supported Programs on Eight Gender Inequality Indicators

III.1) Benchmark and Predictor Robustness

Below, we present the SCM estimates of the gender impact gap between program countries and their synthetic control groups. As illustrated in the example of the Dominican Republic in the previous section, this impact gap may be positive or negative, and the sign of the impact may not necessarily align with the absolute level effect reported for each gender inequality measure. This

is because the paper's goal is to identify the effects of gender on IMF-supported programs implemented in the aftermath of substantial economic volatility. Below, we provide estimates of the economic and statistical significance of the net effect of program conditionality on gender inequality outcomes, as discussed in the methodology section.

We chose eight prominent gender inequality indicators that have been used extensively in the literature and that are common ingredients in the various gender indices that have been constructed over various time periods by different authors and organizations.[13] These indicators are the adolescent fertility rate, the ratio of female to male labor force participation, the Female Labor Force Participation Rate, the maternal mortality rate, primary enrollment gender parity index (GPI; the ratio of the female to male net enrollment rate for primary enrollment), the secondary enrollment GPI, the tertiary enrollment GPI, and the UNDP's GII. Predictors for these indicators that are used to establish the control groups include lagged values of the indicator and reserve coverage in our base case. In a more extensive robustness analysis, we also include government consumption and external debt. However, the additional data requirements of the robustness section reduce the number of programs we can include because either program countries or donor pools lack data.[14]

Table 2 summarizes the results for all our gender inequality indicators for our baseline and robustness cases, which is based on the non-bias-corrected Tables for all countries' gender indicators in Annex A. Table 2 focuses on the summary results for each gender inequality indicator and highlights the positive and negative effects of programs as well as the statistical significance of the effects. The findings are striking and remarkably consistent across different gender indicators. First, the most important finding is that effects of IMF-supported programs on gender are largely not statistically significant for the vast majority of countries. Between 80 and 94 percent of programs show a statistically *in*significant evolution of the gender impact gap after a program

[13] See UNDP's Gender Development Index (GDI), the Gender Inequality Index (GII), the Global Gender Gap Index (GGGI) of the World Economic Forum (WEF 2024), the Social Institutions and Gender Index (SIGI) of the Organization for Economic Cooperation and Development (OECD 2014), the Women's Economic Opportunity Index (WEOI) of the Economist Intelligence Unit (EIU 2010), the Gender Equity Index (GEI) from Social Watch (Social Watch 2012), the Gender Gap Measure Index (GGM) from Klasen and Schüler (2011), and the IMF's Gender Inequality Index (GII) from Stotsky et al. (2016).

[14] Note that the data constraints in the robustness section at times reduce the size of the donor pool. This lack of data may also force SCM to choose slightly different synthetic control groups for a given program country. Differences in results as compared to the benchmark are thus potentially due to both additional controls and smaller donor pools.

was adopted. Formally, we cannot reject the null hypothesis that IMF programs have no effect on gender inequality since gender outcomes were not statistically significantly different from those observed in controls that feature similar fiscal spending, debt, and reserves trajectories but no programs. Or, the magnitude of the program country's change in gender inequality is not extreme relative to the permutation distribution. This result suggests that, although positive or negative impacts on gender can be observed after program adoption, and while these impacts may be sizable, they are generally not statistically different from the effects produced by synthetic control groups without IMF-supported programs.

Table 2
Effect Result Summary by Gender Indicator

	Economic Predictors Used to Match Control Groups							
	Predictor: Reserve Coverage Only				Predictor Robustness: Reserve Coverage, Gov't Consumption, Reserves as Percent of External Debt			
	Programs	Positive Significant	Negative Significant	Not Significant	Programs	Positive Significant	Negative Significant	Not Significant
Adolescent Fertility Rate (-)	115	6	8	101 (88%)	82	6	5	71 (87%)
Female/Male Labor Force Participation (+)	122	10	7	105 (86%)	84	3	3	78 (93%)
Female Labor Force Participation (+)	125	10	9	106 (85%)	88	6	6	76 (86%)
Maternal Mortality Rate (-)	75	6	9	60 (80%)	50	4	4	42 (84%)
Primary Enrollment GPI (+)	52	4	3	45 (87%)	31	0	0	31 (100%)
Secondary Enrollment GPI (+)	23	1	1	21 (91%)	10	0	1	9 (90%)
Tertiary Enrollment GPI (+)	18	1	0	17 (94%)	13	0	1	12 (92%)
Gender Inequality Index (-)	85	6	5	74 (87%)	63	2	3	58 (86%)
Sum	615	44 (7%)	42 (7%)	529 (86%)	421	21 (5%)	23 (5%)	377 (90%)

Notes: Reserve Coverage = Reserves in Months of Imports. GPI = Gender Parity Index (ratio of number of girls to number of boys enrolled). The maternal mortality rate is the number of deaths per 100,000 live births caused by pregnancy or termination. The adolescent fertility rate is the number of births per 1,000 women aged 15–19 years. Significant gender impact gaps are indicated in the Table when at least 1 of the four post-program years shows a significant effect at the 10 percent level. Sign directions are determined by the last significant effect in the sequence, from t to t+4, with priority given to the latest horizon (t+4). If no significant effects are found, the classification is 'not significant,' which applies to most cases. (+) next to the indicators denotes that a positive significant impact means improved gender outcomes, while (-) indicates that a negative significant effect is equivalent to improved gender outcomes.

Second, among those countries that exhibit statistically significant results for a given gender indicator, effects are positive and negative in roughly equal numbers. This implies that, even in the few cases where significant IMF gender impacts are observed, there is no systematic positive or negative impact. Note also that the Tables of the individual results in the Annex

highlight that even when significant, effects never surpass the 5 percent significance threshold; no program effect in any time period is statistically significant at the 1 percent level. The majority of results thus do not find systematic support in the data for hypotheses that suggest program funding softens economic adjustments to produce better gender outcomes than the control group, or more detrimental outcomes as feared by critics of IMF-supported program reforms.

Third, the individual Tables underlying Table 2 provided in Annex A indicate the match's quality in the nonlinear gender trajectories between each program country and its individual control group in the years before program adoption. As another robustness exercise, we also employed bias-correction (Ben-Michael et al., 2021) to reduce remaining outcome deviations between program countries and their synthetic control groups in the matching periods. Table 2b highlights the summary of bias-corrected results, which as qualitatively identical to our non-bias-corrected findings. The number of programs that can be included in the bias-corrected results increases since we have fewer non-unique weighting matrices and fewer exclusions due to noisy pre-program matching.

Table 2b
Bias-Corrected Effect Result Summary by Gender Indicator

	Economic Predictors Used to Match Control Groups							
	Reserve Coverage Only				Reserve Coverage, Gov't Consumption, Reserves as Percent of External Debt			
	Programs	Positive Significant	Negative Significant	Not Significant	Programs	Positive Significant	Negative Significant	Not Significant
Adolescent Fertility Rate (-)	139	9	5	125 (90%)	110	5	5	100 (91%)
Female/Male Labor Force Participation (+)	125	7	8	110 (88%)	94	3	6	85 (90%)
Female Labor Force Participation (+)	136	9	10	116 (85%)	100	5	7	88 (88%)
Maternal Mortality Rate (-)	84	4	5	75 (89%)	62	4	2	56 (90%)
Primary Enrollment GPI (+)	60	2	6	52 (87%)	34	1	4	29 (85%)
Secondary Enrollment GPI (+)	26	6	2	18 (69%)	13	1	1	11 (85%)
Tertiary Enrollment GPI (+)	27	0	1	26 (96%)	19	0	1	18 (95%)
Gender Inequality Index (-)	87	5	7	75 (86%)	66	3	2	61 (92%)
Sum	684	42 (6%)	44 (6%)	597 (87%)	498	22 (4%)	28 (6%)	448 (90%)

Notes: See Table 2

Fourth, inspection of the Annex Tables reveals that we do not find evidence that programs produced better gender outcomes post-1999, when social spending floors became standard,

including in programs under the IMF's Poverty Reduction and Growth Trust. This may be because such social spending floors were generally not designed with a particular gender lens until recently (see IMF 2018 and IMF 2024a). Unfortunately, programs after 2022, when the IMF's Board approved the institution's first strategy to mainstream gender into its core work (IMF 2022), could not be included in the universe of program cases we could examine due to data constraints.

III.2 Effects of IMF Loan Types on Gender Indicators

The corollary to our findings above is that specific *types* of IMF-supported programs also do not indicate systematic impacts on gender outcomes. Our sample contains all five main IMF Lending Facilities, and Table 3 shows that there is no discernible pattern where any one type of Facility produces systematically statistically significant impacts on gender indicators. Qualitatively replicating our results in Table 2, Table 3 shows that all program types exhibit greater than 80% insignificant results. Just like in Table 2, among those results that are significant, about equal numbers report positive and negative effects.

Table 3
Effect Result Summary by Loan Type, All Gender Indicators

IMF Loan Type	Predictor: Reserve Coverage			
	Programs	Positive Significant	Negative Significant	Not Significant
Exogenous Shocks Facility	15	0	1	14 (93%)
Extended Credit Facility	270	19	10	241 (89%)
Extended Fund Facility	99	2	9	88 (89%)
Precautionary and Liquidity Line	12	1	1	10 (83%)
Stand-By Arrangement / Stand-By Credit Facility	218	22	21	175 (80%)
Structural Adjustment Facility	1	0	0	1 (100%)
Sum	615	44 (7%)	42 (7%)	529 (86%)

Note: When multiple programs were present, only the first program was included in the count.

III.3 Averaged Effects of IMF-Supported Programs on Gender Indicators

We believe the individual country results in Annex Tables A.1.1 – A.8.2 are crucial to our understanding of how well-matched control groups identify a diversity of effects of IMF programs on gender across countries. However, in most cases, we cannot reject the null hypothesis that IMF programs have no effect on gender inequality since gender outcomes were not statistically significantly different from those observed in controls that feature similar fiscal spending, debt,

and reserves trajectories but no programs. Ben-Michael et al. (2022) point out that, in program evaluation approaches, averaging individual effects can lead to poor fit for the average and possible bias. However, the previous literature focuses mainly on averaged results obtained from average effects in pooled samples or on averages calculated via staggered treatments (except for matching approaches). Wiltshire (2023) argues that when treatment adoption is averaged or staggered, two-way fixed effects estimators for both difference-in-differences (static DiD) and event-study (dynamic DiD) research designs may yield biased estimates due to heterogeneous treatment effects across cohorts or event years. Ben-Michael et al. (2022) highlight that pooled SCM, by contrast, can achieve a nearly perfect fit for the average treated unit. Nevertheless, the approach suffers from standard pooled estimation issues where the averaged fit yields substantially worse country-specific fits, leading to poor estimates of country-level treatment effects. This may introduce bias for the average effect if the data-generating process varies over time. This is why it is of such great interest for us to also examine individual results.

To compare our results to these previous approaches, we can also average our individual results produced by SCM methodology to generate averaged treatment effects. These averaged effects are provided in Figures 4.a to 4.h. Each Figure plots the individual results (from the Annex Tables) in gray along with the averaged effects in bold. The Figures also report SCM confidence intervals developed by Arkhangelsky et al. (2021) and implemented by Wiltshire (2023) in his staggered treatment SCM.[15] Figures 4.a to 4.h show individual country results in light gray and the average over all results in bold red. The Figures highlight the quality of the match between the program country and its control group before the program is adopted and summarize the diversity of positive and negative impacts documented in Tables A.1.1 – A.8.2.

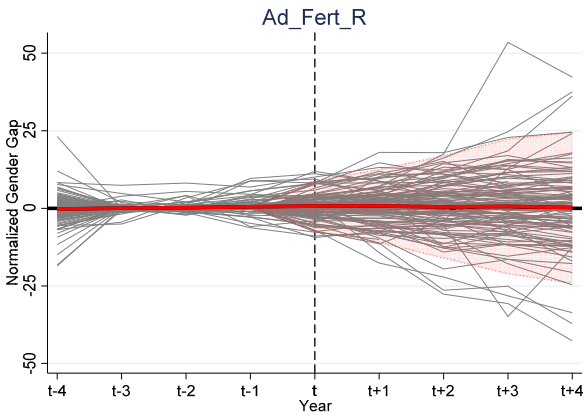
The averaged match is excellent in the pre-program years, most estimates of the averaged IMF-supported program effect across gender indicators are near zero; at the 99% significance level, we lack sufficient evidence to reject the null hypothesis that the averaged treatment effects are zero. This implies that, for the full sample of IMF-supported program countries, we cannot reject the hypothesis that the post-program changes in gender inequality are similar to those in the control groups. These control groups consisted of countries that did not adopt a program but

[15] SCM for staggered treatment timing was originally developed by Cavallo et al. (2013), Dube and Zipperer (2015), Acemoglu et al. (2016), Kreif et al. (2016), Galiani and Quistorff (2017), Abadie and L'Hour (2021), Abadie (2021), Ben-Michael et al. (2022), Peri, Rury, and Wiltshire (2024), Powell (2023), and Wiltshire (2023).

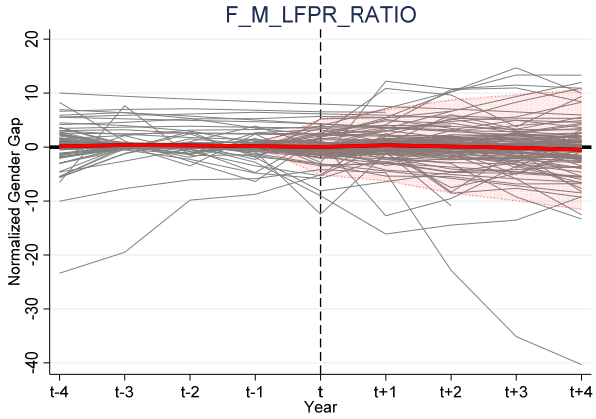
exhibited a parallel progression of gender and economic variables in the four years preceding the adoption of the IMF program.

Figure 4.
Aggregate Treatment Effects of IMF-Supported Programs on Gender Inequality
Individual IMF Program Country Effects (gray, see Tables A.1.1. – A.8.2)
Aggregated Effect (bold red) and 95% Confidence Intervals (shaded red)

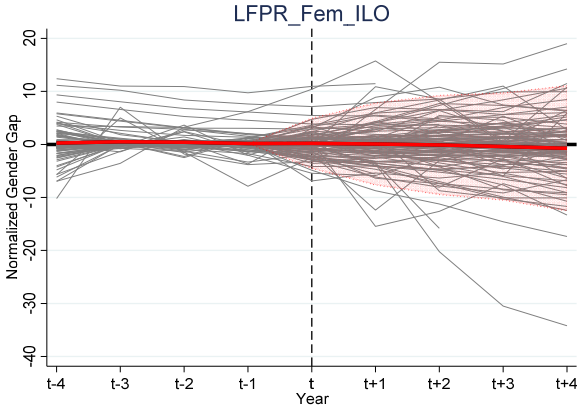
4a. Adolescent Fertility Rate



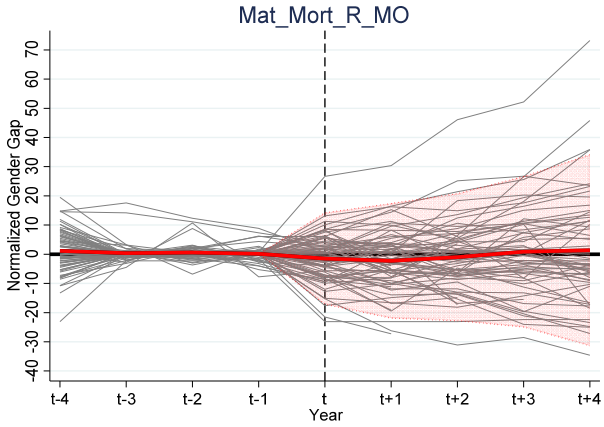
4b) Fem./Male Labor Force Participation



4c. Female Labor Force Participation

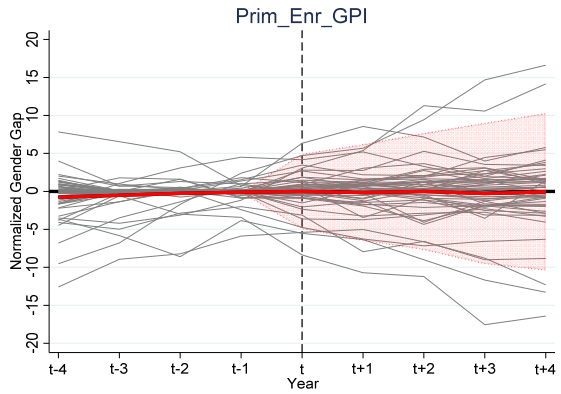


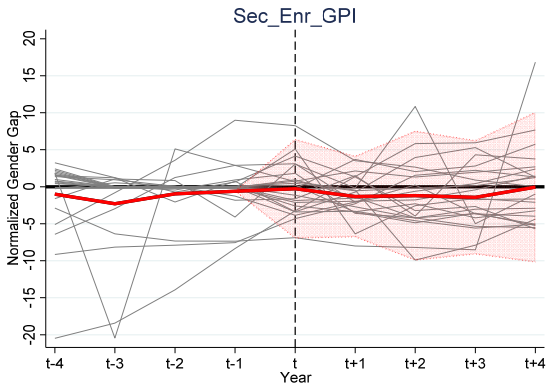
4d. Maternal Mortality Rate



4e. Primary Enrol. Gender Parity Index

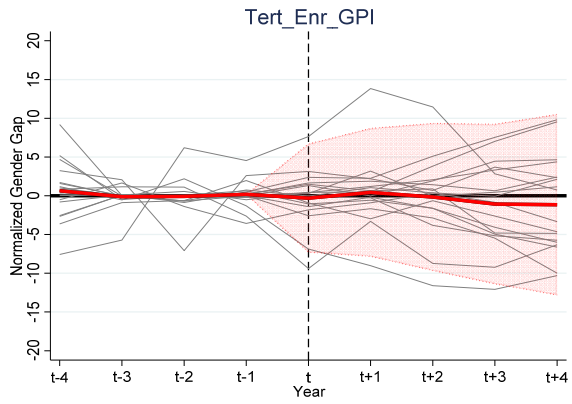
4f. Secondary Enrol. Gender Parity Index

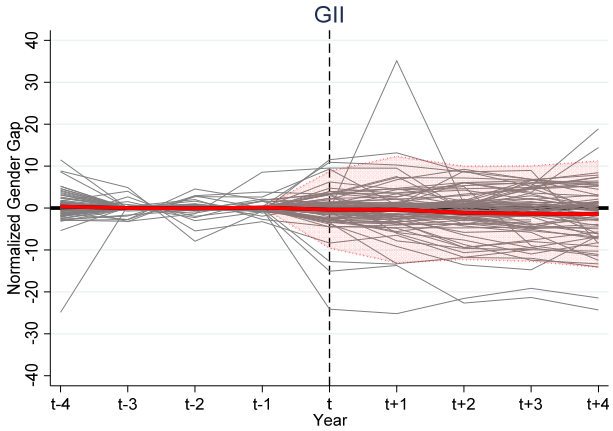




4g. Tertiary Enrol. Gender Parity Index

4h. UNDP Gender Inequality Index





Results in Figures 4.a to 4.h are most comparable to the previous approach in the literature, where authors estimate average effects for pools of program countries at times using control dummies for pools of countries without programs, often in event studies. In these cases, "crisis" countries were either researcher-selected or identified by prominent "economic crisis" databases. These approaches also produce a single aggregated effect for all program countries. The key difference is that our aggregated common effect is based on each program country's individually matched control group, while the previous literature estimated effects based on a pool of program countries and a pooled control group.

III.4 Limiting Donor Pools to "Crisis-Only" Countries

Previous approaches to evaluating the effects of IMF programs on gender utilized researcher-specified control groups, as indicated in our discussion of Table 1. These control groups are, at times, geographically motivated and/or focus on countries that also experienced unusual economic volatility. One issue is that definitions of "crises" are researcher-specific, whose priors then influence the donor pools and the resulting effect estimates. Synthetic Control Methods, on

the other hand, are data-driven, focusing only on the task of producing the best-matching control group whose gender and economic trajectories best mirror those of individual IMF program countries in the pre-program years. As a robustness exercise, we thus explore results using SCM where the donor pools are limited to "crisis" countries only.

Any approach that limits the donor pool to "crisis" countries only encounters several obstacles. First, Laeven and Valencia (2018) and Nguyen et al. (2022) survey economic "crisis" indicator databases and highlight the many differences that distinguish how individual researchers' priors identify such "crises." Even if criteria/indicators are identical across studies, the cut-off thresholds often differ (e.g., do inflation "crises" feature 20 or 25 percent inflation?). Hence, while there is some overlap among databases of research-selected "crisis" countries, there also exist significant differences across databases that reflect researchers' priors. To minimize the ambiguity, we identify "crisis" countries as any country that is associated with the onset of any type of "economic crisis" in any one of the three major "crisis" databases by Reinhart et al. (2021), Laeven and Valencia (2018), and Nguyen et al. (2022).

Restricting the donor pool to "crisis-only" countries presents a significant challenge for all program evaluation studies, not just SCM applications: in most years, the number of countries experiencing a researcher-identified "crisis" without also receiving an IMF program is too small. This limits the donor pool to the degree that our benchmark SCM methodology cannot be implemented. This same problem is encountered by all policy evaluation studies that attempt to identify effects of IMF programs (e.g., GDP growth studies, see e.g., Kuruc (2018) and the voluminous literature on event studies in IMF program evaluation cited therein). The issue is well known to the existing literature, which thus resorted to event-study approaches.[16] We follow the previous literature and execute SCM as an event study where each IMF-supported program

[16] While the IMF program evaluation literature generally also employs event studies (see Kuruc 2018), the literature evaluating IMF program effects on gender has focused on selection approaches that are orthogonal to SCM's data-driven selection. Following Przeworski and Vreeland (2000), various variants of Heckman selection or IV regressions have been introduced with researcher-specified selection models to estimate a probability of receiving treatment. While SCM attempts to match the perfect counterfactual, researcher-specified selection models focus on underlying causes that led to the adoption of programs (see, e.g., Kern, Reinsberg, and Lee, 2024). While SCM addresses selection bias and unobservables by creating an excellently fitting synthetic control that mimics the IMF-supported program country before the intervention, the researcher-specified selection approach directly models the selection process in hopes of accounting for unobserved factors that influence selection and outcome. SCM usually takes into account data from a number of years before program adoption, while Heckman selection usually employs shorter lag structures. The fact that in any given year few countries encounter economic crises without adopting an IMF program is a limiting factor for both approaches.

country's synthetic control is chosen from the donor pool of countries that are associated with an "economic crisis" between 1990 and 2022. We continue to require that donor pool countries cannot have an IMF-supported program in the four years pre- and post-program adoption for each treated country.

Results from our "crisis-only" donor pool approach are reported in Table 2c. They are again qualitatively identical to our findings in Table 2. The summary statistics for "crisis-only" donor pools indicate that only 8% of programs exhibit positively significant gender effects, 6% show negative, significant effects, and 86% of the programs do not show statistically significant effects. The aggregated averaged treatment effect Figures for "crisis-only" donor pools are qualitatively similar to Figures 4a-h and are thus omitted. The total number of programs in Table 2c is smaller than in Table 2 because more IMF-supported program countries produced non-unique weighting matrices in the context of the event methodology.

Table 2c
Effect Result Summary by Gender Indicator
Event Study Approach, "Crisis-Only" Donor Pools

	Reserve Coverage			
	Programs	Positive Significant	Negative Significant	Not Significant
Adolescent Fertility Rate (-)	68	3	5	60 (88%)
Female/Male Labor Force Participation (+)	57	9	4	44 (77%)
Female Labor Force Participation (+)	61	11	5	45 (74%)
Maternal Mortality Rate (-)	40	4	1	35 (88%)
Primary Enrollment GPI (+)	39	4	5	30 (77%)
Secondary Enrollment GPI (+)	17	0	0	17 (100%)
Tertiary Enrollment GPI (+)	20	2	4	14 (70%)
Gender Inequality Index (-)	43	4	5	34 (79%)
Sum	345	37 (11%)	29 (8%)	279 (81%)

Notes: See notes for Table 2.

Using the event study approach, we also cannot reject the null hypothesis that IMF programs have no effect on gender inequality since gender outcomes were not statistically significantly different from those observed in controls that feature similar country characteristics but no programs.

III.5 Longer Matching Time Horizons

In its efforts to find good matches for program countries' pre-treatment outcome trajectories, SCM fits systematic components and idiosyncratic errors. While fitting idiosyncratic errors, especially in bias-correction, SCM is also fitting the error term, which may raise suspicions of overfitting. Although systematic shocks occur in our application, by definition, close to the program year, we also present robustness results for longer pre-program matching periods (6, 8, 12 years). In an abbreviated version of Table 2, we report only the total summary statistic over all gender indicators. The cost of increasing the matching periods is that we lose more and more program countries. Recall that, in order to isolate the effects of each program, we do not admit countries that had another IMF-supported program in the matching period. The benefit of longer matching periods, on the other hand, is that more weight is placed on systematic effects and overfitting error becomes progressively less likely. While the number of programs we can examine reduces to only 27 in the case of a 12 year matching period, it is interesting to note that this is not necessarily an unusually small sample given previous literature outlined in Table 1.

Table 2d
Impact of Matching Duration (4-12 years) on Gender Effects
Total Number of Effects Across All Gender Indicators

		Reserve Coverage		
	Programs	Positive Significant	Negative Significant	Not Significant
4 Year Matching	615	44 (7%)	42 (7%)	529 (86%)
6 Year Matching	295	21 (7%)	26 (9%)	248 (84%)
8 Year Matching	123	8 (7%)	16 (13%)	99 (80%)
12 Year Matching	27	1 (4%)	3 (11%)	23 (85%)

Notes: See notes for Table 2.

Those programs that can be examined, as we increase the duration of the matching period, show the same pattern of statistical significance that we outlined in Tables 2, 2b, and 2c.

IV) Conclusion

In this paper, we seek to estimate the effects of IMF-supported programs on gender inequality. To isolate the impact of IMF-supported programs on gender, we employ Synthetic Control Methods (SCM) to construct custom-tailored control groups for each program country. In our summary of the previous literature in Table 1, we argue that counterfactuals were either missing or specified

with some imprecision. Imprecisions affect, however, post-program gender effect estimates since it is unclear whether program reforms, or whether country characteristics such as low reserves, high debt, and excessive government spending may have affected gender, even in countries that did not adopt IMF-supported programs.

We examine up to 125 programs across eight different gender indicators from 1994 to 2022 to obtain a total of 615 estimates for gender effects in IMF-supported program countries. Each control group is tailored to each individual IMF-supported program country to achieve an optimal match with regard to gender inequality and key other economic indicators such as reserves, debt, and government expenditures. Annex Tables A.1.1-A.8.2 and the associated visual representation of the Tables in Figures 4.a to 4.h highlight the quality of the match between each program country's pre-crisis gender indicator and economic trends with its control group. We find that, for more than 80% of the countries (depending on the gender indicator), we cannot reject the null hypothesis that IMF programs have no effect on gender inequality since gender outcomes were not statistically significantly different from those observed in controls that feature similar fiscal spending, debt, and reserves trajectories but no programs. Among programs that exhibit statistically significant effects on gender inequality, we find that effects are negative and positive in roughly equal proportions.

While the presence of an IMF-supported program may thus be associated with absolute changes in gender inequality, in our data, using Synthetic Control Methods, we are unable to document that IMF-supported programs produce a statistically significant effects on gender, when we compare the outcomes to countries with similar gender, reserve, debt, and government expenditure profiles in the pre-program years but that did not adopt an IMF-supported program. Our findings also indicate that the gender effects of IMF-supported programs on gender inequality are heterogeneous; they can be at times large and both positive and negative – but still not statistically significantly different from effects observed in control groups with similar gender, reserve, debt and government expenditure profiles.

Heterogeneity may arise because IMF-supported programs differ, which highlights the importance of conducting the analysis at the individual country level. However, we find no systematic effects of the type of IMF-supported program on gender outcomes. Future research may want to explore why countries experience positive/negative (though statistically insignificant)

effects and whether the direction of change can be linked to any specific country characteristic or particular program conditionality.

Recent IMF strategies on social spending (IMF 2019) and on mainstreaming gender into the IMF's core operations (IMF 2022) have been designed to mitigate potential adverse effects on vulnerable populations. Unfortunately, these new IMF strategies are too recent to be examined for effects in our study, due to data constraints. Our finding that gender inequality declines or increases in some cases in the presence of IMF-supported programs suggests that attention to policy design can lead to better outcomes.

Annex A. Country-by-Country Results

Table A.1.1: Effects of IMF-Supported Programs on Gender Inequality: Adolescent Fertility Rate
A positive gender impact gap implies a higher adolescent fertility rate (births per 1,000 women ages 15-19) in program countries compared to the control group.
Synthetic Control Predictors: Adolescent Fertility (lagged), Reserve Coverage (lagged).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Bolivia	1994	2.54	0.05	0.04	0.05	0.23	1.79	6.99	10.24	11.25
2	Turkey	1994	-6.8	0.23	-0.61	0.19	8.11	18.05	17.96	22.86	24.55
3	Ghana	1995	-1.88	-0.01	-0.05	0	3.14	2.85	8.18	11.44	13.27
4	Haiti	1995	-2.68	-0.02	-0.02	-0.02	-0.94	4.4	9.98	14.42	17.88
5	Mexico	1995	0.13	0.03	0.02	0.04	-2.3**	-1.14**	-2.31**	-3.17**	-4.14**
6	Madagascar	1996	-5.47	-1.84	1.75	5.6	8.14	8.17	8.41	9.7	8.79
7	Mozambique	1996	-5.09	-0.62	-0.04	2.98	8.62	9.71	11.71	15.36	14.95
8	Tanzania	1996	0.92	-0.28	-0.39	2.24	4.24	6.35	5.15	5.85	6.95
9	Venezuela	1996	-0.72	0.05	0.06	0.05	4.2*	6.63*	9.58*	15.79*	14.57*
10	Yemen A. Rep.	1996	-1.22	2.75	-1.36	-6.26	-8.93	-8.11	-12.8	-11.52	-13.88
11	Guinea	1997	2.04	0.04	-0.59	8.9	8.15	5.83	9	6.1	6.74
12	Albania	1998	-18.21	-0.55	0.61	0.06	-3.08	-4.06	-5.38	-34.89	-12.58
13	Brazil	1998	1.02	-0.33	-0.29	0.76	1.54	0.59	1.19	2.02	-0.2
14	Cape Verde	1998	4.69	0.04	0	0.03	0.11	-1.88	-4.33	-6.05	-5.77
15	Gambia	1998	1.01	1.17	1.17	-2.4	-2.13	-2.43	-5.58	-4.26	-5.75
16	Rwanda	1998	-6.67	-0.37	0.63	0.24	2.85	5.54	10.4	15.26	16.23
17	Zimbabwe	1998	1.42	-0.12	-0.29	0.41	1.23	2.48	3.11	5.73	6.47
18	Cambodia	1999	23.16	0.04	-0.23	0.57	2.8	5.23	14.37	12.49	4.49
19	Colombia	1999	-2.83	-0.03	-0.06	0.01	4.75	3.59	4.64	7.85	6.99
20	Honduras	1999	-1.66	-0.42	0.21	1.64	0.77	-0.01	-0.48	-2	-2.89
21	Ecuador	2000	1.04	-0.44	1.2	0.15	-3.02	-3.25	-1.09	0.73	4.55
22	Gabon	2000	-0.74	-0.07	0.32	-0.08	-2.56	-4.39	-4.14	-4.48	-4.78
23	Malawi	2000	-5.79	-1.39	1.12	3.49	4.34	4.06	4.72	7.35	9.75
24	Nigeria	2000	-2.82	-0.99	0.36	2.76	3.49	3.94	7.31	10.23	8.15
25	Papua N. Guinea	2000	-4.8	0.04	-0.14	0.14	3.78	6.91	7.44	6.63	5.33
26	Armenia	2001	-2.76	0.05	-0.05	0.07	-2.58	10.26	14.63	17.03	13.1
27	Azerbaijan	2001	0.65	-0.01	0.01	-0.01	4.79	9.57	16.18	24.67*	37.55*
28	Ethiopia	2001	-0.48	-0.88	4.15	-1.37	4.02	1.78	-3.49	-2.81	-4.47
29	Georgia	2001	-3.14	0.02	-0.01	-0.08	-4.61	4.04	11.94	18.51	36.18
30	Laos	2001	0.45	0.01	0.01	0	-1.86	-4.76	-6.01	-1.92	-0.24
31	Lesotho	2001	1.78	0.06	0.06	0.05	2.71	7.1	13.25	12.57	17.58
32	Madagascar	2001	-2.09	-0.41	0.58	0.84	1.6	2.94	5.03	3.74	3.36
33	Sierra Leone	2001	-3.57	-0.78	1.09	1.83	1.64	2.67	4.66	1.35	0.78
34	Sri Lanka	2001	3	-0.93	-0.6	4.35	11.89	8.76	4.78	-1.04	-6.77
35	Vietnam	2001	6.78	-0.02	-0.03	0	-1.58	-0.43	-4.23	1.87	4.14
36	Dem. Rep Congo	2002	-4.13	-0.56	0.92	0.64	2.14	4.77	4.12	4.42	6.4
37	Dominica	2002	-14.98	0.13	-2.18	1.6	-5.02	-7.49	-8.44	-8.07	-5.8
38	Guatemala	2002	2.79	-0.47	0.44	0.75	-1.16	-5.74	-8.17	-10.05	-11.88
39	Uganda	2002	0.89	1.38	-1.21	-2.76	-5.19	-7.19	-8.22	-10.82	-10.37
40	Bangladesh	2003	-2.43	0.63	-0.61	-1.02	1.34	1.66	-7.13	-7.47	-3.72
41	Bolivia	2003	-0.58	0	-0.03	0.01	0.81	1.87	2.43	3.54	3.75
42	Dom. Republic	2003	-2.27	-0.01	0.04	-0.06	0.73	-1.39	-2.12	-3.35	-1.6
43	Nepal	2003	-2.23	0.01	0.04	0.01	3.67	3.32	5.63	4.65	0.68
44	Paraguay	2003	-3.44	0.08	-0.01	0.11	1.97	1.13	0.14	0.87	-0.72
45	Burundi	2004	1.31	0	-0.05	0.27	-3.32*	-8.03*	-12.78**	-16.39**	-20.68**
46	Congo	2004	-0.53	0.09	-0.64	0.61	0.97	0.37	2.72	5.03	6.09
47	Honduras	2004	-0.21	-0.01	-0.01	-0.01	0.19	-1.1	-1.45**	-3.84**	-5.7**
48	Mozambique	2004	-0.53	-0.24	0.21	0.57	-1.25	-1.6	-0.91	-1.46	-0.48
49	Ukraine	2004	6.86	0.01	0.06	0.03	0.97	0.79	2.1	5.19	9.91
50	Zambia	2004	1.95	0.18	-0.47	0.27	0.12	-1.39	-1.66	-2.56	-3.34
51	Cameroon	2005	7.67	4.76	2.07	4.07	2.55	4.29	5.06	3.37	3.18

52	Iraq	2005	-0.29	-1.35	-0.34	3.79	5.87	10.96	11.05	6.96	9.18
53	S Tome & Principe	2005	0.08	0.13	-0.5	0.25	-1.16	-2.19	-1.73	0.52	4.2
54	Grenada	2006	-2.32	-0.04	-0.04	-0.04	2.06	0.58	0.62	-1.87	-3.53
55	Haiti	2006	3.4	0.03	-0.19	0.07	-0.17	-5.01	-7.78	-8.82	-10.86
56	Madagascar	2006	8.31	7.43	8.14	6.84	10.08	13.17	12.39	13.9	12.61
57	Moldova	2006	4.54	0.01	0.01	0.01	-2.66	-9.63	-19.58	-16.48	-17.82
58	Gambia	2007	-1.68	0.01	-0.05	0.15	0.22	-0.68	2.54	-0.44	-0.22
59	Guinea	2007	0.56	-0.64	0.48	2	4.66	8.41	10.17	13.64	11.8
60	Nicaragua	2007	-0.22	0.01	0.01	0.01	-.93*	-1.5*	-1.36*	-0.04	-1.04
61	Djibouti	2008	1.73	-0.02	0.05	-0.07	1.81	4.23	6.36	5.27	4.16
62	Iceland	2008	2.03	0.03	0.01	0.05	0.04	2.09	-2.93	-11.98	-15.71
63	Liberia	2008	-4.18	-0.14	2.03	5.67	7.89	9.92	11.79	10.84	8.88
64	Seychelles	2008	-1.48	0.43	-2.19	1.21	1.93	3.31	-12.48	-8.35	-11.68
65	Belarus	2009	-2.91	-0.24	-0.02	0.52	1.71	-6.28	-6.69	-2.51	-7.59
66	Bosnia & Herzeg.	2009	0.76	0.98	-1.1	-2.79	-5.38	-11.21	-26.4	-25.12	-37.17
67	Comoros	2009	1.67	0.02	0.02	0.02	-1.01	-1.98	-4.97	-7.87	-10.95
68	Costa Rica	2009	2.42	0	0	0	-3.62	-8.73	-9.69	-8.24	-14.26
69	Cote D'Ivoire	2009	1.18	0.14	-0.35	0.06	0.52	4.66	2.27	-0.69	-0.53
70	Dem. Rep Congo	2009	-2.45	0	-0.03	0.2	0.65	1.87	4.24	4.73	2.68
71	El Salvador	2009	0.88	0	0.01	0	-0.43	0.13	1.03	1.65	1.92
72	Ethiopia	2009	6.24	1	-0.62	-2.83	-9.39	-7.48	-8.28	-17.84	-24.67
73	Ghana	2009	3.94	-0.01	0.35	-0.26	-2.33	-0.92	0.53	1.05	2.39
74	Guatemala	2009	3.51	0.01	0.02	0.01	0.3	0.52	3.98	5.65	-2.74
75	Maldives	2009	1.72	-0.27	-1.19	1.82	8.04*	5.71	-1.33	-6.31	-17.03
76	Mongolia	2009	-18.58	0	-0.32	0.19	3.65	7.11	17.49	53.53	42.29
77	Romania	2009	-1.29	-0.11	-0.06	-0.06	2.41	-14.3*	-27.63*	-30.69*	-42.73*
78	Sri Lanka	2009	5.82	0.48	-0.3	-1.88	6.55	11.88	8.32	5.24	-5.84
79	Tajikistan	2009	-1.4	0	-0.36	0.35	7.81**	11.2*	15.14	7.31	10.21
80	Tanzania	2009	0.55	0.1	-0.29	0.06	0.77	3.03	2.16	0.86	5.47
81	Greece	2010	2.43	0.44	0.51	0.49	-0.54	-6.41	-11.22	-11.73	-11.8
82	Ireland	2010	-5.75	-0.17	-0.17	-0.18	-7.02	-17.67	-22.13	-28.24	-33.64
83	Jamaica	2010	8.33	-0.04	-0.04	-0.04	-6.18	-6.55	-5.22	-6.85	-12.51
84	Lesotho	2010	-6.73	0.02	0.02	0.02	-1.51	-0.78	7.44	11.58	11.87
85	Solomon Islands	2010	0.05	0.02	0.01	0.02	-0.05	-0.31	.86*	2.62**	2.99**
86	Yemen A. Rep.	2010	3.11	1	-1.42	-1	-2	-4.38	-5.21	-5.43	-6.18
87	Kenya	2011	0.56	-0.02	-0.03	-0.02	-3.19*	-3.17	-3.91	-5.85	-9.32
88	Macedonia	2011	3.31	0.03	-0.39	0.26	-7.49	-5.23	-14.08	-11.55	-12.36
89	St Kitts & Nevis	2011	1.27	-0.05	-0.04	-0.04	0.1	-0.77	-1.15	0.08	1.47
90	Bangladesh	2012	-0.57	0.57	4.05	-5.88	-4.05	-4.02	-12.38	-15.43	-12.66
91	Gambia	2012	-1.41	0.59	-0.45	-1.03	-2.79	-5.19	-8.65	-11.81	-12
92	Jordan	2012	2.27	-0.36	2.14	-1.94	0.37	2.14	1.61	1.9	0.93
93	Morocco	2012	3.69	0.02	0.01	0.01	-5.87	-5.64	-6.04	-3.23	-2.38
94	Cyprus	2013	-3.83	-0.72	-1.76	2.78	-7.26	-11.44	-3.88	12.9	24.2
95	Pakistan	2013	0.95	0.16	-0.41	0.26	0.02	0.23	0.05	1.31	1.55
96	Albania	2014	-8.04	-0.01	0	0	2.69	5.33	0.63	-2.12	-4.74
97	Seychelles	2014	-9.36	0.1	0.1	0.09	-2.68	-0.08	8.82	11.66	12.4
98	Afghanistan	2016	5.07	0.45	-1.14	0.41	1.72	0.44	-0.82	-1.75	-3.33
99	Egypt	2016	-3.1	0.02	0.01	0.02	-1.84	2.67	0.78	-4.3	-0.72
100	Iraq	2016	3.16	0	-0.11	0	0.22	-1.27	-3.42	-4.69	-5.89
101	Madagascar	2016	2.45	2.16	1.14	1.06	0.28	-0.46	0.33	0.33	1.77
102	Moldova	2016	-6.85	-4.42	3.42	8.76	9.43	3.43	0.16	-7.42	-9.86
103	Rwanda	2016	-1.04	-0.06	-0.07	-0.07	-5.49*	-7.7	-11.46	-5.5	1.22
104	Sri Lanka	2016	6.71	0.27	-1.22	0.56	2.98	4.53	4.11	2.26	4.09
105	Suriname	2016	-0.66	-0.2	-0.05	0.78	7.74*	14.7*	11.07*	8.13*	8.36*
106	Cameroon	2017	0.51	-0.23	0.3	0.1	0.64	1.08	0.98	1.26	1.73
107	Mauritania	2017	1.48	-0.01	-0.01	-0.01	-0.52	1.68	1.65	3.26	3.55
108	Mongolia	2017	-5.54	-5.07	1.23	9.65	11.12	8.81	2.52	-5.17	-7.77
109	Barbados	2018	1.92	-0.05	-0.09	-0.01	4.12	3.09	3.94	4.21	4.28
110	Armenia	2019	-11.71	0.02	-0.09	-0.11	-0.11	2.37	5.28	8.6	
111	Congo	2019	0.06	3.89	5.44	4.53	3.59	2.39	1.81	1.51	
112	Ecuador	2019	4.11	-0.04	0.14	-0.35	-2.84	-4.5	-3.37	-2.75	
113	Pakistan	2019	0.07	-0.11	-0.15	-0.15	-1.22*	-1.05	-1.62	-1.7	

114	Gambia	2020	2.17	-0.16	-0.17	-0.15	3.19	4.44	2.85		
115	Costa Rica	2021	12.06	1.85	-0.57	-4.88	-7.7	-10.01			

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.1.2: Effects of IMF-Supported Programs on Gender Inequality: Adolescent Fertility Rate, Robustness

A positive gender impact gap implies a higher adolescent fertility rate (births per 1,000 women ages 15-19) in IMF program countries compared to the control group.

Synthetic Control Predictors: Adolescent Fertility (lagged), Reserve Coverage (lagged), Government Consumption, Reserves (Percent of External Debt).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Bolivia	1994	1.30	0.05	-0.39	0.36	0.13	-0.83	4.08	6.76	7.90
2	Turkey	1994	-10.29	0.32	-1.33	0.76	7.94	7.61	11.44	13.30	12.17
3	Ghana	1995	2.62	1.20	-1.35	-0.57	1.89	-1.08	3.76	3.74	-0.60
4	Mexico	1995	0.00	0.02	-0.09	-0.01	-1.96*	-.66*	-1.5*	-2.08*	-3.23*
5	Madagascar	1996	-5.62	-1.97	1.63	5.48	8.03	8.07	8.31	9.61	8.70
6	Mozambique	1996	-5.04	-0.58	-0.01	3.01	8.65	9.74	11.74	15.39	14.98
7	Tanzania	1996	0.81	-0.38	-0.49	2.15	4.16	6.27*	5.07	5.78	6.88
8	Venezuela	1996	-3.35	-0.26	0.11	0.57	7.15	10.05*	11.92*	18.24*	18.71
9	Yemen A. Rep.	1996	2.10	5.00	0.60	-6.12	-9.65	-9.35	-14.66	-14.68	-17.40
10	Guinea	1997	2.04	0.04	-0.59	8.90	8.15	5.83	9.00	6.10	6.74
11	Indonesia	1997	-0.43	-0.02	0.01	-0.04	-0.25	-3.35	-1.71	-2.18	-2.91
12	Thailand	1997	-4.24	-0.04	-0.03	0.01	-5.45	-3.06	-13.57	-27.26	-25.45
13	Albania	1998	-18.33	-1.65	2.65	0.84	-9.43	-20.47	-26.12	-47.81	-28.59
14	Brazil	1998	0.52	-0.55	-0.89	2.07	4.20	4.68	5.78	5.24	1.89
15	Gambia	1998	3.06	1.20	0.61	-3.14	-3.59	-3.62	-6.39	-6.19	-7.41
16	Rwanda	1998	-2.40	-0.87	2.21	-0.22	-4.42	-9.22	-14.47	-16.97	-22.08
17	Zimbabwe	1998	1.51	-0.06	-0.62	0.91	2.14	3.47	4.01	5.12	4.68
18	Cambodia	1999	18.72	0.12	-0.66	1.46	3.48	8.98	17.77	17.27	12.07
19	Colombia	1999	-4.69	-0.08	0.05	0.11	3.51	3.27	2.14	1.99	1.62
20	Turkey	1999	-3.13	0.00	0.01	-0.01	1.85	1.72	2.51	-5.19	-8.49
21	Ecuador	2000	-0.46	-0.75	1.33	0.65	-2.97	-5.05	-3.82	-2.28	0.65
22	Papua N. Guinea	2000	-1.85	0.13	-0.68	0.68	3.94	5.87	6.44	7.11	7.18
23	Armenia	2001	16.05	4.20	-6.08	-6.75	-14.33	-10.32	-11.36	-11.38	-14.94
24	Azerbaijan	2001	6.77	0.76	0.83	-2.32	-6.13	-2.46	2.33	6.08	21.80
25	Georgia	2001	10.10	3.87	-2.34	-7.96	-20.24	-14.29	-9.28	-6.25	10.69
26	Sri Lanka	2001	2.54	-1.40	-1.30	3.97	10.27	6.32	2.48	-3.30	-9.34
27	Vietnam	2001	8.58	0.06	-0.17	0.07	-2.10	0.33	-2.12	6.85	13.43
28	Guatemala	2002	6.20	-0.63	1.53	3.98	3.61	-1.26	-3.03	-3.70	-2.41
29	Bolivia	2003	-1.05	0.01	-0.14	0.10	1.09	3.66	5.63	5.45	5.79
30	Dom. Republic	2003	1.86	3.91	5.54	7.38	7.63	5.93	5.31	6.25	10.40
31	Nepal	2003	5.47	7.54	10.27	9.23	8.91	6.34	3.03	-2.06	-2.88
32	Paraguay	2003	-2.71	0.15	-0.21	0.31	2.40	3.31	4.79	3.42	0.52
33	Burundi	2004	2.04	-0.59	0.58	0.65	-1.09	-6.34	-10.45	-14.12	-20.06
34	Honduras	2004	14.39	12.92	12.04	9.61	8.02	5.94	6.84	5.51	5.93
35	Ukraine	2004	7.32	-0.26	0.31	-0.55	-0.11	0.61	0.46	2.49	7.18
36	Haiti	2006	3.38	0.13	0.06	0.10	0.04	-6.27	-9.26	-10.92	-15.09
37	Moldova	2006	5.19	-0.09	-0.09	-0.09	-3.06	-9.54	-18.63	-15.38	-16.25
38	Gambia	2007	-1.77	-0.16	-0.28	0.27	-1.60	-4.73	-5.51	-10.34	-10.53
39	Guinea	2007	0.56	-0.64	0.48	2.00	4.66	8.41	10.17	13.64	11.80
40	Nicaragua	2007	-0.11	0.08	0.09	0.07	-.76*	-1.10	-0.82	0.52	-0.58
41	Pakistan	2008	2.27	-0.01	-0.01	0.00	-3.29	-4.19	-4.23	-8.40	-8.14
42	Belarus	2009	-4.70	-1.05	0.10	2.43	5.33	-1.27	-1.37	2.72	-2.29
43	Bosnia & Herzeg.	2009	0.28	-2.71	-7.73	-7.85	-11.49	-17.96	-30.84	-35.24	-44.39

44	Comoros	2009	1.28	0.09	0.09	0.08	-0.14	-0.98	-3.41	-6.39	-9.62
45	Costa Rica	2009	2.20	-0.08	-0.08	-0.08	-3.06	-7.69	-8.69	-7.30	-13.64
46	Dem. Rep Congo	2009	-2.42	0.03	0.00	0.22	0.67	1.88	4.25	4.75	2.69
47	El Salvador	2009	-0.47	-0.05	0.15	-0.24	0.33	1.59	1.51	2.19	4.12
48	Ghana	2009	1.67	-0.08	0.77	-0.86	-1.98	-0.12	0.96	2.10	4.82
49	Guatemala	2009	3.82	-0.03	0.00	-0.05	0.13	0.01	3.11	4.23	-4.33
50	Mongolia	2009	-20.93	-2.61	-3.47	1.50	5.18	8.98	21.07	54.36	44.87
51	Romania	2009	-1.56	-0.14	0.05	0.06	2.35	-14.72**	-28.2**	-31.23**	-43.18**
52	Tajikistan	2009	2.10	-0.06	-0.17	0.43	8.63**	14.09**	20.97**	19.98**	20.58**
53	Tanzania	2009	0.53	0.23	-0.30	0.19	1.11	3.45*	2.34	1.15	5.79
54	Jamaica	2010	7.92	-0.13	-0.24	-0.11	-5.74	-7.01	-6.03	-8.24	-13.86
55	Solomon Islands	2010	-0.05	0.05	-0.04	0.07	0.10	-0.20	0.94	2.66*	3.77*
56	Yemen A. Rep.	2010	3.02	1.41	-0.01	-2.93	-3.97	-6.99	-11.90	-13.75	-15.28
57	Kenya	2011	0.74	0.11	0.10	0.11	-2.97*	-2.95*	-3.64	-5.56	-8.99
58	Macedonia	2011	-0.52	-1.97	-1.22	2.00	-4.64	-3.10	-11.25	-9.05	-10.21
59	Bangladesh	2012	-0.23	0.61	3.90	-5.98	-4.20	-4.29	-12.73	-15.82	-13.00
60	Gambia	2012	-1.32	0.53	-0.44	-1.07	-2.90	-5.29	-8.77	-11.97	-12.21
61	Jordan	2012	1.29	-0.51	2.36	-2.06	-0.45	1.18	0.19	0.12	-0.96
62	Morocco	2012	-0.89	0.01	0.00	0.03	-0.78	-5.09	-8.47*	-5.09	-2.58
63	Pakistan	2013	0.25	0.14	-0.67	0.17	-0.21	-0.28	-0.22	1.16	1.68
64	Albania	2014	-9.49	0.03	-0.08	0.11	1.62	4.31	-0.30	-2.36	-5.39
65	Ghana	2015	1.54	-0.03	-0.03	-0.02	6.63	7.90	9.51	12.80	13.13
66	Haiti	2015	3.10	-0.03	-0.04	-0.02	1.95	4.60	3.90	4.39	9.13
67	Egypt	2016	-3.18	-0.04	-0.06	-0.03	-2.99	-2.50	-2.16	-5.11	-3.49
68	Madagascar	2016	2.45	2.16	1.14	1.06	0.28	-0.46	0.33	0.33	1.77
69	Moldova	2016	-5.82	-4.72	4.01	9.32	15.14	14.46	10.70	-0.15	-3.77
70	Rwanda	2016	-1.82	0.07	0.03	0.05	-2.47	-4.44	-5.69	-3.53	0.54
71	Cameroon	2017	0.51	-0.23	0.30	0.12	0.68	1.11	1.00	1.29	1.76
72	Mauritania	2017	1.30	-0.14	-0.03	-0.07	-0.61	1.42	1.40	3.13	3.52
73	Mongolia	2017	-6.17	-5.53	1.41	9.34	10.17	8.59	2.41	-5.20	-7.96
74	Armenia	2019	-11.32	-0.02	-0.06	-0.24	-0.73	2.25	5.13	8.28	
75	Congo	2019	11.97	9.97	10.66	9.11	8.62	7.96	8.60	7.96	
76	Ecuador	2019	4.11	0.07	0.32	-0.17	-2.99	-4.74	-3.74	-3.18	
77	Honduras	2019	-1.34	0.13	0.12	0.12	1.74	4.11	5.46	6.35	
78	Pakistan	2019	0.48	-0.14	-0.14	-0.14	-0.45	-0.49	-1.46	-2.32	
79	Gambia	2020	0.60	0.10	0.02	0.11	5.97**	7.55**	5.51**		
80	Costa Rica	2021	12.37	2.04	-0.82	-5.07	-7.97	-10.66			
81	Kenya	2021	-2.26	0.03	0.04	0.02	0.64	-0.19			
82	Moldova	2021	2.20	0.01	-0.01	0.02	3.48	4.34			

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.2.1: Effects of IMF-Supported Programs on Gender Inequality: Ratio of Female to Male Labor Force Participation Rate

A positive gender impact gap implies a higher labor force participation of women relative to men in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Ratio of Female to Male Labor Force (lagged), Reserve Coverage (lagged).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Ghana	1995	0.64	0.57	0.38	0.24	0.12	-0.11	-0.39	-0.66	-0.95
2	Haiti	1995	-0.07	0.01	0.01	0.01	.6*	1.16*	1.69*	2.25*	2.62*
3	Djibouti	1996	1.31	0.19	-0.26	-0.13	-0.19	-1.19	-7.52	-4.90	-0.21
4	Madagascar	1996	-0.03	-0.03	-0.02	0.07	-0.02	-0.13	-0.22	-0.31	-0.58
5	Mozambique	1996	5.57	5.89	6.14	6.45	6.16	5.93	5.37	4.96	4.57
6	Tanzania	1996	-0.02	-0.06	-0.03	0.12	0.06	0.00	-0.01	0.00	-0.18
7	Venezuela	1996	1.57	0.29	-0.76	-0.16	1.37	1.11	-1.12	0.18	0.13
8	Yemen A. Rep.	1996	2.48	0.04	-1.16	0.95	3.92	7.13	10.29	14.68	9.99
9	Guinea	1997	0.20	0.08	0.09	-0.36	-0.70	-0.89	-0.97	-1.13	-1.69
10	Thailand	1997	0.60	-0.47	0.40	1.27	1.04	-1.92	-4.08	-4.62	-5.52
11	Albania	1998	0.90	0.00	0.00	0.00	-0.33	-0.54	-0.28	-0.14	0.12
12	Brazil	1998	-2.08	0.07	0.06	0.07	1.47	2.91	5.07	8.31	10.97
13	Cape Verde	1998	0.04	0.00	0.00	0.00	0.06	0.11	0.21	1.54*	1.36*
14	Rwanda	1998	-3.07	-0.08	-0.03	-0.12	-0.83	-2.80	-4.62	-4.17	-3.60
15	Zimbabwe	1998	-0.49	-0.02	-0.02	-0.02	-0.07	-0.08	-0.16	-0.31	-0.46
16	Cambodia	1999	0.32	0.25	-0.16	-0.58	-0.88	-1.31	-1.77	-2.18	-2.47
17	Colombia	1999	0.74	0.36	0.36	0.36	-1.56	-1.32	-1.06	-2.16	-0.77
18	Honduras	1999	2.35	0.09	0.13	0.09	-1.23	-4.25	-5.08	-5.87	-6.53
19	Turkey	1999	-1.37	0.46	-1.91	1.05	1.06	-12.73	-9.53	-3.53	-6.66
20	Chad	2000	0.01	0.00	0.00	0.00	.07*	.35**	.22**	.14*	-.13*
21	Ecuador	2000	1.46	-0.34	2.85	-1.92	-8.15	-6.42	-4.67	-2.85	3.24
22	Gabon	2000	0.01	-0.08	-0.08	-0.09	-0.16	0.12	0.58	1.20	1.94
23	Lithuania	2000	1.02	-0.04	-1.16	2.67	3.21	0.91	-1.88	0.34	-3.47
24	Malawi	2000	0.41	0.12	-0.15	-0.30	-0.48	-0.76	-0.80	-0.59	-0.37
25	Nigeria	2000	0.04	0.01	0.02	0.01	-.2*	-.81*	-1.33*	-1.73	-2.11
26	Papua N. Guinea	2000	0.22	0.50	0.82	0.86	0.89	0.69	0.59	0.66	0.75
27	Azerbaijan	2001	0.10	0.00	0.03	-0.01	-0.07	1.43*	0.19	0.20	-0.11
28	Ethiopia	2001	0.02	0.08	0.08	0.08	0.39	0.76	1.18	1.57*	1.94*
29	Georgia	2001	0.97	-0.37	2.45	-1.54	1.08	-2.35	0.07	2.60	-0.22
30	Laos	2001	-0.06	-0.08	-0.10	-0.10	-0.16	-0.10	0.13	0.21	0.14
31	Lesotho	2001	-0.11	-0.01	0.37	-0.16	-1.59	-2.20	-2.85	-3.15	-4.63
32	Madagascar	2001	0.17	0.13	0.11	-0.02	-0.05	-0.21	-0.05	0.08	0.22
33	Sierra Leone	2001	3.45	3.70	3.88	3.66	3.54	3.59	3.84	3.26	3.19
34	Sri Lanka	2001	-6.57	7.67	-1.58	-1.44	-3.48	-3.04	-1.00	-2.52	-7.66
35	Vietnam	2001	0.02	-0.03	0.75	-0.50	-0.48	-0.53	-0.76	-1.58	-1.78
36	Dem. Rep Congo	2002	0.74	0.82	0.66	0.37	0.21	0.23	0.28	0.32	-0.50
37	Uganda	2002	0.06	0.00	-0.01	0.01	0.11	0.18	0.31	0.27	0.55
38	Bangladesh	2003	1.04	-0.11	-0.05	0.31	0.55	1.56	1.97	-0.19	2.61
39	Bolivia	2003	2.83	0.08	0.08	0.08	0.18	0.15	0.10	2.54	-0.57
40	Dom. Republic	2003	-1.88	-0.40	-1.34	1.61	-3.34	-3.02	-4.68	-4.86	-5.59
41	Paraguay	2003	1.17	-0.06	-0.06	-0.06	2.37	10.88*	9.64	3.75	9.38
42	Burundi	2004	5.52	5.24	4.95	4.70	4.29	3.72	2.94	3.99	4.92
43	Congo	2004	0.07	0.06	0.16	0.40	0.60	0.72	0.89	0.99	1.08
44	Mozambique	2004	10.02	9.41	8.86	8.44	8.00	7.53	7.03	6.50	5.96
45	Ukraine	2004	-0.51	0.01	0.01	0.01	0.39	0.50	0.75	-0.70	-2.38
46	Zambia	2004	0.13	-0.03	-0.04	-0.02	-0.02	-0.03	-0.19	-0.38	-0.44
47	Cameroon	2005	0.01	-0.01	-0.01	-0.01	0.01	-0.03	-0.01	-1.89**	-3.98**
48	Chad	2005	0.20	0.09	0.07	0.10	0.14	0.13	0.08	-0.03	-0.14
49	Malawi	2005	0.11	0.00	-0.02	0.00	0.01	-.87*	-1.79**	-2.75**	-3.76**
50	S Tome & Principe	2005	0.55	0.11	0.11	0.11	-1.98	-0.67	-0.92	-0.89	-1.94
51	Cent. Afr. Rep.	2006	-0.01	-0.10	-0.10	-0.10	-0.28	-0.45	-0.56	-0.55	0.05
52	Haiti	2006	0.18	0.02	0.00	0.02	0.35	0.72	1.34	1.87	2.58
53	Madagascar	2006	0.02	0.10	0.12	0.10	-0.04	-0.19	-0.36	-0.56	-0.78
54	Moldova	2006	-5.65	-1.23	1.01	3.91	-0.03	3.06	1.65	0.11	0.34
55	Sierra Leone	2006	3.03	3.21	2.54	2.39	2.21	1.99	1.74	1.46	1.14
56	Guinea	2007	2.20	0.89	-0.42	-1.75	-3.09	-4.43	-5.79	-7.15	-8.48
57	Nicaragua	2007	-2.01	-0.13	-0.13	-0.13	2.48	3.04	4.75	5.60	8.33
58	Djibouti	2008	0.13	0.17	0.56	-2.31	5.25	5.46	10.22	13.36	13.32

59	Iceland	2008	-1.92	0.00	-0.01	-0.01	-0.10	1.37	1.94	2.47	4.13
60	Liberia	2008	0.29	0.17	-0.09	0.10	-0.02	-0.10	-0.06	0.59	0.95
61	Pakistan	2008	-4.63	-2.60	-0.34	-4.74	2.04	4.13	10.67	10.96	8.37
62	Angola	2009	0.06	0.02	0.02	-0.04	-0.13	-0.23	-2.26**	-2.01**	-1.87*
63	Belarus	2009	0.23	0.01	0.00	0.01	0.01	0.12	0.24	0.50	0.85
64	Bosnia & Herzeg.	2009	0.53	0.01	-0.18	0.12	0.34	-0.81	2.12	0.20	-2.47
65	Comoros	2009	2.09	0.10	0.09	0.06	-2.07	-1.14	-1.54	-3.20	-3.86
66	Dem. Rep Congo	2009	1.13	0.31	0.34	-1.17	-3.12	-5.37	-5.48	-6.83	-6.78
67	Ethiopia	2009	0.38	-0.09	-0.09	-0.09	-0.07	0.00	0.06	-0.13	0.92
68	Ghana	2009	0.19	0.06	0.07	-0.17	-0.48	-0.84	-0.81	-0.90	-1.47
69	Guatemala	2009	0.20	0.10	0.10	0.11	-0.10	0.33	-8.34**	-.72*	-8.27*
70	Maldives	2009	0.04	-0.01	-0.01	-0.01	.23*	-0.13	-0.46	-0.06	0.45
71	Mongolia	2009	0.14	0.10	0.10	0.10	-0.60	-1.37	-2.15	-2.48	-2.86
72	Mozambique	2009	6.95	6.35	5.71	5.03	4.31	3.54	2.87	2.30	1.70
73	Romania	2009	0.05	0.13	0.26	-0.48	-1.18	-2.78	-0.06	-1.12	-1.80
74	Sri Lanka	2009	-0.10	0.00	0.00	-0.02	-0.14	-0.17	1.22*	-3.89**	2.76*
75	Tajikistan	2009	-0.03	0.03	0.03	-0.02	.25*	3.44**	6.63**	9.35**	12**
76	Tanzania	2009	0.20	0.56	0.03	-0.85	-1.88	-3.02	-3.70	-4.55	-5.40
77	Greece	2010	1.72	0.08	-0.28	0.19	2.68	4.86	4.44	2.65	4.40
78	Ireland	2010	-1.60	-0.01	-0.06	-0.01	0.60	0.33	-1.03	-1.09	-2.07
79	Jamaica	2010	0.19	-0.09	-0.11	-0.09	0.41	0.80	2.29*	2.08	2.04
80	Solomon Islands	2010	0.23	0.13	0.02	-0.05	-0.20	-0.22	-0.05	0.06	0.44
81	Kenya	2011	0.02	0.00	0.00	0.00	-0.08	-0.13	-0.18	-0.03	0.06
82	Macedonia	2011	2.61	-0.10	-0.10	-0.10	3.56	2.24	5.22	3.00	2.01
83	Bangladesh	2012	0.48	0.01	-0.16	0.10	-0.05	0.09	3.04	2.86	3.42
84	Guinea	2012	3.65	0.28	0.31	-0.87	-12.44	-1.12	0.91	-3.30	-6.04
85	Jordan	2012	-1.98	0.12	0.23	-2.84	-9.03	-16.12	-14.46	-13.53	-9.20
86	Morocco	2012	-0.11	-0.13	0.03	-0.27	-1.96	-0.42	0.95	-1.01	-5.43
87	Cyprus	2013	-1.32	-0.08	-0.01	-0.22	-0.92	0.71	1.71	0.85	0.96
88	Pakistan	2013	-3.02	-0.51	-2.26	-4.83	-5.84	-1.94	-3.05	-9.23	-13.33
89	Tunisia	2013	3.65	0.53	-1.39	0.72	-0.66	0.20	0.74	2.63	1.50
90	Albania	2014	-1.71	2.12	-0.81	-3.47	-5.31	0.56	6.00	3.30	5.53
91	Chad	2014	1.35	0.01	0.00	-0.01	0.16	-0.11	-0.38	-1.46	-2.17
92	Ghana	2015	0.79	0.00	0.01	0.00	-0.42	0.13	0.24	0.19	0.04
93	Haiti	2015	0.15	-0.10	-0.21	0.32	1.63**	1.24*	0.90	0.28	0.43
94	Mozambique	2015	2.98	2.40	1.76	1.38	0.75	1.05	1.36	1.66	2.08
95	Afghanistan	2016	-23.38	-19.49	-9.84	-8.76	-5.14	2.54	-1.31	-5.84	-12.55
96	Egypt	2016	-10.04	-7.70	-6.06	-5.84	-4.18	-4.76	-22.85	-35.13	-40.30
97	Madagascar	2016	0.07	0.04	0.11	0.02	0.25	0.39	0.44	0.52	0.29
98	Moldova	2016	4.48	4.00	2.98	0.73	0.22	0.42	1.19	0.17	-1.71
99	Rwanda	2016	-2.99	-0.01	0.01	0.01	0.39	0.54	-0.62	-5.06	-0.86
100	Sri Lanka	2016	-5.52	0.10	-0.40	0.59	-1.35	-0.15	-8.56	-7.62	-9.30
101	Suriname	2016	0.07	-0.01	-0.01	0.00	-0.01	-.55*	0.05	0.26	-0.12
102	Cameroon	2017	-0.23	-0.02	0.00	0.00	-0.10	0.15	0.23	0.64	1.22
103	Mauritania	2017	0.20	0.01	-0.04	0.02	-0.87	-1.98	-2.35	-1.93	-2.71
104	Angola	2018	-1.88	-0.09	-0.14	0.02	-0.07	1.06	1.65	2.13	0.91
105	Barbados	2018	-1.22	-0.08	0.00	-0.19	-0.83	1.25	1.29	1.67	-0.49
106	Armenia	2019	0.89	0.00	0.00	0.00	2.06	5.11	3.49	1.18	1.05
107	Congo	2019	4.18	4.58	5.23	5.26	5.68	5.51	5.78	5.36	5.33
108	Ecuador	2019	-5.44	-0.28	0.79	-0.26	0.09	-3.39	-1.95	0.46	-2.19
109	Eq. Guinea	2019	0.72	0.12	0.10	0.15	-0.15	-0.47	-0.46	-0.58	-0.42
110	Honduras	2019	1.70	0.30	-3.49	2.42	-2.87	12.20	10.77	11.48	10.89
111	Pakistan	2019	8.30	1.11	-2.65	-6.38	1.89	3.57	3.23	4.96	3.59
112	Gambia	2020	-0.25	0.06	0.10	0.11	0.91	0.30	1.66	1.35	
113	Costa Rica	2021	-5.62	-1.16	1.52	0.50	-1.42	-0.88	-10.86		
114	Kenya	2021	1.55	2.16	3.13	3.26	3.82	4.00	4.08		
115	Madagascar	2021	2.61	2.46	2.62	2.52	2.47	2.18	2.11		
116	Moldova	2021	5.89	6.42	5.30	3.51	2.21	5.40	6.66		
117	Suriname	2021	-1.26	0.16	0.16	0.16	0.57	1.63	1.70		
118	Cape Verde	2022	0.57	0.04	0.06	0.12	0.43	0.55			
119	Georgia	2022	0.29	-0.02	0.04	-0.01	-0.83	-1.44			
120	Macedonia	2022	-4.59	0.11	0.45	-0.76	-1.51	1.92			
121	Mozambique	2022	6.63	6.98	7.10	6.84	6.53	6.44			
122	Nepal	2022	-1.10	-0.03	0.00	-0.02	-0.38	-0.11			

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.2.2: Effects of IMF-Supported Programs on Gender Inequality: Ratio of Female to Male Labor Force Participation Rate, Robustness

A positive gender impact gap implies a higher labor force participation of women relative to men in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Ratio of Female to Male Labor Force (lagged), Reserve Coverage (lagged), Government Consumption, Reserves (Percent of External Debt).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Mexico	1995	0.12	0.01	0.01	0.01	3.14*	1.55*	5.46*	4.23*	2.64*
2	Madagascar	1996	0.09	0.05	-0.01	-0.02	-0.14	-0.26	-0.34	-0.31	-0.22
3	Mozambique	1996	5.57	5.89	6.14	6.45	6.16	5.93	5.37	4.96	4.57
4	Tanzania	1996	0.01	-0.01	0.01	0.13	0.09	0.08	0.13	0.29	0.45
5	Venezuela	1996	2.84	0.50	-1.23	-0.34	1.27	0.93	-4.38	-2.67	-4.04
6	Guinea	1997	0.58	0.26	0.14	-0.41	-0.80	-1.02	-0.91	-0.61	-0.72
7	Indonesia	1997	-1.09	0.06	-0.03	0.03	-1.17	1.59	1.57	2.38	-0.86
8	Thailand	1997	-0.62	-1.25	0.10	1.39	2.25	0.42	-0.56	0.16	-0.62
9	Albania	1998	1.08	0.02	-0.17	0.02	0.02	0.16	0.64	1.00	1.13
10	Brazil	1998	-1.90	0.17	-0.15	0.20	1.81	3.83	6.16	10.39	11.36
11	Gambia	1998	-0.14	-0.11	-0.12	-0.11	-0.06	0.05	0.20	1.18	0.82
12	Rwanda	1998	-3.46	0.00	0.12	-0.15	-0.94	-1.47	-1.37	0.02	0.96
13	Zimbabwe	1998	-0.48	-0.04	0.10	0.11	0.08	0.24	0.53	0.63	0.63
14	Cambodia	1999	0.50	0.29	-0.21	-0.69	-0.95	-1.17	-1.35	-1.78	-2.15
15	Colombia	1999	-0.03	-0.16	0.14	-0.24	-1.94	-1.54	-1.36	-2.18	-0.05
16	Honduras	1999	2.15	-0.03	0.11	-0.03	-1.31	-4.31	-5.00	-5.67	-6.29
17	Turkey	1999	2.86	1.52	-3.18	-1.52	2.66	-6.19	-2.62	3.02	0.15
18	Chad	2000	0.16	-0.01	-0.13	0.04	0.32	0.64	0.25	-0.15	-0.95
19	Ecuador	2000	1.34	-0.27	3.19	-1.62	-7.72	-5.77	-4.22	-2.68	2.93
20	Gabon	2000	0.18	0.01	-0.05	0.04	0.12	0.28	0.62	1.13	1.70
21	Armenia	2001	0.05	-0.10	-0.07	-0.12	-0.14	-0.35	-0.50	-0.73	-0.88
22	Azerbaijan	2001	2.03	2.42	2.87	3.04	3.84	5.83	5.16	5.57	6.27
23	Georgia	2001	-0.04	-0.50	2.85	-1.32	2.56	-1.32	0.83	2.25	0.31
24	Madagascar	2001	9.79	9.83	9.90	9.98	10.32	10.51	10.96	11.39	11.85
25	Sri Lanka	2001	-6.33	7.43	-1.71	-1.63	-4.33	-3.97	-1.61	-3.67	-10.20
26	Vietnam	2001	5.77	6.05	7.06	5.89	6.50	6.79	6.97	6.45	6.96
27	Guatemala	2002	-0.42	-0.18	-0.18	-0.18	0.47	0.29	0.56	0.94	1.46
28	Uganda	2002	-0.19	0.07	0.37	0.31	0.31	0.41	0.49	0.60	1.32
29	Bangladesh	2003	-0.32	-0.03	-0.09	-0.19	-0.37	-0.62	-0.92	-4.61	-2.36
30	Bolivia	2003	3.10	0.03	0.03	0.03	0.13	0.23	0.24	2.49	-0.95
31	Dom. Republic	2003	-2.07	-0.30	-1.32	1.79	-3.07	-2.33	-4.44	-4.24	-4.75
32	Nepal	2003	-0.33	-0.12	-0.12	-0.11	-0.41	-0.02	0.26	2.02	3.84
33	Paraguay	2003	0.60	0.00	0.01	-0.08	2.22	10.14	8.98	1.42	5.93
34	Congo	2004	5.84	6.58	7.36	8.16	8.94	9.68	10.50	11.31	12.11
35	Honduras	2004	0.17	0.11	0.11	0.11	0.13	0.18	-7.5**	-9.09**	-6.02**
36	Ukraine	2004	-0.63	-0.27	0.15	0.42	1.06	1.37	2.04	0.87	-0.18
37	Cameroon	2005	0.55	1.42	2.31	3.23	4.19	5.01	5.91	4.88	3.67
38	Chad	2005	0.27	0.08	-0.09	-0.22	-0.31	-0.59	-0.82	-1.02	-1.19
39	Cent. Afr. Rep.	2006	0.17	-0.02	0.04	-0.08	-0.43	-0.76	-1.03	-0.98	-0.48
40	Haiti	2006	0.26	0.11	0.04	0.12	0.20	0.44	0.87	1.37	2.48
41	Madagascar	2006	4.01	4.29	5.22	5.19	4.98	4.76	5.24	5.67	6.19
42	Sierra Leone	2006	9.45	9.78	9.93	9.69	9.43	9.15	9.52	9.85	10.25
43	Gambia	2007	-0.71	-0.03	-0.03	-0.02	0.33	1.50	1.85	2.91	3.43
44	Guinea	2007	3.01	2.10	0.13	-1.87	-3.91	-5.22	-6.60	-7.89	-9.58
45	Nicaragua	2007	-0.65	0.06	0.18	0.10	2.06	3.68	4.73	6.63	8.82
46	Pakistan	2008	-5.53	-3.52	-1.28	-5.90	1.12	3.11	9.74	10.22	7.74
47	Angola	2009	6.56	6.35	6.17	6.68	7.17	7.76	5.87	5.31	5.15
48	Belarus	2009	0.46	0.02	0.03	-0.04	-0.32	-0.59	-0.64	-1.24	-1.57
49	Bosnia & Herzeg.	2009	0.52	0.12	-0.11	0.18	-1.13	-2.99	0.11	-1.88	-2.24
50	Comoros	2009	1.86	0.14	0.14	0.04	-2.02	-1.59	-1.52	-2.87	-3.73
51	Costa Rica	2009	0.94	0.03	0.03	0.03	0.73	-0.50	4.13	11.46	12.26
52	Dem. Rep Congo	2009	7.20	6.17	5.08	4.66	4.14	3.66	2.79	0.95	0.60
53	El Salvador	2009	-1.02	-0.03	-0.03	-0.03	-0.11	-0.36	-0.63	0.26	2.03

54	Ghana	2009	3.44	2.87	2.29	2.41	2.48	2.63	2.40	1.23	0.46
55	Guatemala	2009	-0.33	0.04	0.03	0.04	-0.03	-0.26	-7.51**	-.24**	-8.1**
56	Mongolia	2009	1.13	0.01	0.02	-0.03	-1.01	-2.01	-3.14	-4.40	-3.95
57	Romania	2009	1.02	0.20	0.22	-0.61	-1.46	-3.59	-1.40	-2.91	-3.49
58	Tajikistan	2009	0.49	0.04	0.05	-0.16	-0.19	2.69	5.91	8.39	10.94
59	Tanzania	2009	7.49	7.74	6.95	6.80	6.56	6.37	5.77	4.23	3.03
60	Jamaica	2010	0.45	0.11	0.07	0.10	0.57	0.94	2.39	2.14	1.94
61	Solomon Islands	2010	4.93	4.79	6.48	8.43	10.50	10.34	10.21	9.98	9.74
62	Kenya	2011	0.03	0.00	0.00	0.01	-0.06	-0.11	-0.14	-0.18	-0.22
63	Macedonia	2011	3.06	0.05	0.04	0.01	4.00	2.42	5.11	3.16	0.58
64	Bangladesh	2012	0.38	-0.04	-0.24	0.03	-0.01	0.09	2.12	2.15	2.67
65	Cent. Afr. Rep.	2012	0.76	0.00	0.00	0.00	-1.50	-1.18	-0.50	-0.93	-0.75
66	Gambia	2012	0.37	0.00	0.00	0.01	0.26	3.40	6.71	9.81	13.27*
67	Guinea	2012	1.47	0.35	0.32	-1.53	-5.83	-6.35	-8.30	-11.26	-13.66
68	Jordan	2012	-2.35	-0.24	-0.10	-3.13	-9.27	-16.35	-14.73	-13.75	-9.41
69	Morocco	2012	-0.01	-0.12	-0.19	-0.18	-1.23	0.10	0.67	-1.11	-5.64
70	Pakistan	2013	-9.65	-4.36	-3.18	-3.00	-3.48	0.65	0.46	-5.35	-8.92
71	Tunisia	2013	0.36	-0.39	-1.51	0.45	-0.68	1.31	1.64	3.30	2.81
72	Albania	2014	-1.90	2.47	-0.85	-3.37	-5.07	1.00	6.59	3.75	5.93
73	Chad	2014	0.84	0.05	-0.01	-0.19	-0.67	-0.88	-1.11	-1.99	-2.98
74	Ghana	2015	0.42	0.00	0.22	-0.14	-0.80	-0.43	-0.05	0.15	0.16
75	Haiti	2015	-0.16	-0.42	0.11	0.54	2.34	1.81	2.03	2.12	2.33
76	Mozambique	2015	10.09	7.74	7.48	6.56	5.46	5.95	6.65	6.69	7.04
77	Egypt	2016	-10.22	-7.87	-6.25	-6.00	-4.33	-4.91	-23.01	-35.29	-40.46
78	Madagascar	2016	2.36	2.67	2.36	1.88	2.31	2.84	2.70	2.84	2.72
79	Moldova	2016	9.87	9.76	8.21	5.48	5.15	5.74	6.25	5.17	3.42
80	Rwanda	2016	-0.13	0.09	0.12	0.04	.98*	1.88**	1.01*	-1.7*	2.29**
81	Cameroon	2017	-0.06	0.10	0.10	0.11	0.52	1.50	1.15	1.42	1.81
82	Mauritania	2017	0.47	0.16	0.13	0.14	-0.27	-0.84	-1.00	-0.70	-1.15
83	Mongolia	2017	-0.13	0.00	0.00	-0.01	1.02*	-5.84**	-3.41**	-4.05**	-5.62**
84	Angola	2018	-0.75	-0.10	0.02	0.10	0.19	0.62	1.28	1.63	0.03

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.3.1: Effects of IMF-Supported Programs on Gender Inequality: Female Labor Force Participation Rate

A positive gender impact gap implies a higher Female Labor Force Participation Rate in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Female Labor Force Participation (in percent, lagged), Reserve Coverage (lagged).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Bolivia	1994	-1.73	0.02	0.02	0.03	0.41	0.64	1.12	1.37	2.45
2	Cent. Afr. Rep.	1994	-0.33	-0.03	0.03	-0.04	0.28	0.07	-0.69	-1.58	-2.20
3	Chad	1994	0.12	0.01	0.07	-0.02	-.64**	-1.75**	-3.04**	-4.44**	-5.6**
4	Malawi	1994	0.03	-0.14	0.22	0.05	0.12	0.50	0.29	-0.08	-0.30
5	Sierra Leone	1994	0.20	0.07	0.19	0.02	0.27	0.10	-0.30	-0.81	-1.01
6	Uganda	1994	0.27	-0.02	0.07	-0.03	0.29	0.17	-0.36	-1.02	-1.36
7	Ghana	1995	-0.30	0.08	-0.14	-0.12	-0.07	-0.44	-0.96	-1.38	-1.60
8	Haiti	1995	-0.15	-0.01	-0.01	-0.01	0.08	-0.81	-1.28	-1.48	-1.64
9	Mexico	1995	0.52	-0.07	-0.07	-0.08	1.50	-0.02	3.55	1.43	-1.07
10	Djibouti	1996	1.54	0.29	-0.48	-1.04	-2.35	-4.08	-7.82	-9.14	-6.69
11	Madagascar	1996	-0.54	-0.23	0.25	0.76	1.07	1.33	1.69	1.94	1.60
12	Mozambique	1996	-1.08	-0.35	0.42	1.12	1.77	2.48	2.76	2.94	2.97
13	Tanzania	1996	-0.63	-0.26	0.27	0.82	1.12	1.40	1.78	2.07	1.82
14	Venezuela	1996	0.38	0.21	-0.24	0.14	1.23	1.56	2.66	2.20	3.08
15	Yemen A. Rep.	1996	2.70	0.12	-1.22	1.27	3.62	5.96	8.37	11.00	2.75
16	Guinea	1997	-0.19	-0.08	-0.08	-0.08	-0.21	-0.11	-1.00	-2.84	-3.44
17	Thailand	1997	-0.18	-0.56	0.66	0.78	1.26	-1.34	-3.13	-3.78	-3.82
18	Albania	1998	2.42	0.11	-0.65	0.44	-0.73	-2.10	-2.03	-2.08	-2.40
19	Brazil	1998	-2.15	0.27	-1.52	0.57	1.73	4.06	7.35	10.56	14.23
20	Cape Verde	1998	-0.10	0.03	0.03	0.02	0.26	0.09	-0.54	1.48*	0.26
21	Gambia	1998	0.12	0.20	0.20	0.20	0.24	0.79	1.60	3.17	4.70
22	Rwanda	1998	-1.06	-0.06	-0.06	-0.06	-0.17	-1.09	-2.23	-2.19	-1.92
23	Zimbabwe	1998	-0.30	-0.05	-0.05	-0.05	-0.02	-0.15	-0.59	-0.75	-0.68
24	Cambodia	1999	-0.59	0.05	0.00	-0.03	-0.18	-0.37	-0.42	-0.63	-0.81
25	Colombia	1999	0.26	-0.03	0.11	-0.10	-2.14*	-2.37	-2.47	-4.64	-2.09
26	Honduras	1999	0.68	0.02	0.02	0.01	-1.47	-6.38	-7.40	-8.44	-9.47
27	Turkey	1999	-1.29	0.32	-2.48	1.04	0.57	-15.46	-12.62	-7.40	-10.72
28	Chad	2000	0.19	0.01	-0.10	0.01	-0.15	-0.01	-0.39	-0.72	-1.58
29	Ecuador	2000	1.81	-0.50	3.64	-2.03	-5.44	-5.87	-2.24	0.61	10.79
30	Gabon	2000	0.23	0.11	0.11	0.11	0.37	0.38	0.83	1.19	1.84
31	Lithuania	2000	0.81	0.08	-0.63	0.62	-2.94	-6.13	-8.07	-2.67	-7.79
32	Malawi	2000	0.29	0.05	-0.05	-0.12	-0.94	-1.43	-1.56	-1.12	-0.70
33	Nigeria	2000	0.05	0.01	0.03	-0.02	0.07	0.04	0.32	0.54	0.93
34	Papua N. Guinea	2000	0.26	-0.01	0.02	-0.05	0.16	-3.93**	-7.00**	-9.93**	-12.45**
35	Azerbaijan	2001	0.65	0.18	0.06	-0.66	-3.01	-4.68	-7.30	-10.29	-11.67
36	Ethiopia	2001	-0.63	-0.20	0.13	-0.24	0.31	1.11	2.30	3.48	4.61
37	Georgia	2001	1.70	-0.41	2.37	-1.52	2.42	-0.65	2.65	3.26	-0.73
38	Laos	2001	0.15	0.12	0.12	0.12	0.26	0.14	-0.02	-0.27	-0.53
39	Lesotho	2001	0.20	-0.08	0.16	-0.27	-1.6*	-2.12	-2.81*	-3.04	-4.63
40	Madagascar	2001	3.46	3.38	3.18	2.01	1.31	0.92	1.14	1.34	1.47
41	Sierra Leone	2001	-0.07	0.00	0.00	0.00	-.32*	-.65*	-.49*	1.56**	.29*
42	Sri Lanka	2001	-10.22	7.03	-1.83	-3.82	-3.25	-2.62	-1.81	-1.10	-0.47
43	Vietnam	2001	0.29	-0.12	0.74	-0.74	1.05	1.03	1.03	0.51	2.66
44	Dem. Rep Congo	2002	-0.04	0.02	0.02	0.02	0.07	0.15	0.21	0.28	-1.41*
45	Uganda	2002	0.38	0.11	0.11	0.11	0.43	0.98	1.60	1.91	2.41
46	Bangladesh	2003	-0.56	-0.01	-0.08	0.18	0.37	0.70	0.54	-1.26	-0.69
47	Bolivia	2003	3.77	0.04	0.04	0.04	0.03	0.26	0.32	5.32	0.73
48	Dom. Republic	2003	-0.67	-0.16	-0.52	0.73	-6.88**	-5.37*	-6.34	-5.28	-5.07
49	Nepal	2003	-0.24	0.22	0.22	0.22	0.18	0.52	0.84	0.01	0.99
50	Paraguay	2003	0.60	-0.11	-0.11	-0.11	-0.18	10.89**	8.53**	.47**	6.47*
51	Burundi	2004	2.57	0.93	-0.44	-1.34	-2.32	-3.50	-4.87	-3.88	-2.91
52	Congo	2004	-0.07	-0.08	-0.07	-0.07	-0.08	-0.10	-0.09	-0.09	-0.10
53	Mozambique	2004	3.95	2.66	1.72	1.35	0.99	0.55	0.06	-0.47	-0.92
54	Ukraine	2004	-0.01	-0.02	-0.02	-0.02	.26*	.3*	0.34	0.62	-0.08
55	Zambia	2004	-0.16	0.00	-0.01	0.00	0.18	0.20	0.21	0.22	0.06
56	Cameroon	2005	0.31	-0.06	0.03	0.13	0.17	0.13	0.08	-2.96	-6.39

57	Chad	2005	0.10	0.02	0.00	0.03	0.01	-0.05	-0.06	-0.24	-0.49
58	Malawi	2005	0.26	-0.07	0.05	0.17	0.24	-1.57	-3.48	-5.41	-7.57
59	S Tome & Principe	2005	-0.48	-0.09	-0.09	-0.09	-1.51	-0.01	-0.08	-0.24	-1.28
60	Cent. Afr. Rep.	2006	0.31	0.10	0.12	0.11	-0.20	-0.77	-1.35	-1.12	0.37
61	Haiti	2006	0.02	0.05	0.09	0.14	0.42	0.71	1.02	1.34	1.68
62	Moldova	2006	-0.72	0.18	0.01	0.17	4.74	6.97	6.56	6.17	11.50
63	Sierra Leone	2006	-0.39	-0.74	0.93	-0.53	-2.45	-3.57	-4.74	-5.91	-6.87
64	Guinea	2007	-1.13	-0.01	-0.01	0.00	2.21	-0.08	-4.20	-8.58	-9.12
65	Nicaragua	2007	0.07	0.00	0.00	0.00	1.28**	3.51**	4.55**	6.97**	8.47**
66	Djibouti	2008	0.29	-0.72	-1.29	-3.33	-1.78	-3.18	-3.96	-6.03	-9.08
67	Iceland	2008	4.18	3.29	2.85	1.53	0.01	-1.61	-1.35	-1.98	-1.27
68	Liberia	2008	5.92	4.54	3.16	1.82	1.11	0.42	0.32	0.33	0.08
69	Pakistan	2008	-1.69	-0.76	0.25	-2.06	1.67	4.32	7.72	7.34	6.31
70	Angola	2009	9.35	8.05	6.77	6.12	5.46	5.35	4.36	3.80	2.31
71	Belarus	2009	0.13	-0.01	-0.02	0.03	0.07	0.07	0.05	0.46	1.80
72	Bosnia & Herzeg.	2009	1.58	0.26	-0.89	0.65	-0.39	-0.06	-1.47	-1.14	-2.78
73	Comoros	2009	0.62	-0.08	-0.08	-0.07	-0.45	-0.21	0.04	0.13	-0.14
74	Dem. Rep Congo	2009	3.93	2.09	0.25	-1.26	-2.79	-4.03	-5.22	-6.59	-8.17
75	El Salvador	2009	-0.32	-0.10	-0.09	-0.09	-0.83	-0.83	-2.23	-1.59	-0.40
76	Ethiopia	2009	7.98	6.61	5.27	4.56	3.85	3.69	3.65	3.35	2.11
77	Ghana	2009	2.33	1.18	0.05	-0.64	-1.33	-1.63	-1.84	-2.24	-4.48
78	Maldives	2009	-0.50	-0.01	-0.01	-0.02	0.36	2.81	4.36	5.63	7.05
79	Mongolia	2009	-2.89	-0.06	0.34	-0.19	2.38	5.90	6.57	5.62	6.00
80	Romania	2009	-2.58	0.02	0.02	0.02	0.40	0.65	0.62	1.18	0.88
81	Sri Lanka	2009	-0.07	-0.03	-0.03	-0.04	-0.09	-0.04	0.09	-3.97*	-0.54
82	Tajikistan	2009	2.10	1.06	-0.17	-1.36	-1.44	0.87	2.78	4.04	5.53
83	Greece	2010	0.82	0.09	-0.25	0.02	2.15	1.11	-0.39	-1.54	-1.79
84	Ireland	2010	-2.49	0.09	-0.04	-0.25	0.15	-0.02	-0.76	0.88	0.38
85	Jamaica	2010	-0.69	0.10	0.20	-0.02	-0.06	1.80	1.56	3.42	5.18
86	Macedonia	2011	0.13	0.06	0.06	0.07	3.52**	-.53*	2.71*	0.59	-2.70
87	Bangladesh	2012	0.19	-0.08	-0.08	-0.08	0.59	0.28	1.57	1.45	1.71
88	Gambia	2012	-0.11	0.12	0.12	0.12	0.54	4.1*	6.43**	8.42**	10.61**
89	Guinea	2012	2.76	0.00	0.01	-0.02	-4.87	-8.72	-11.25	-14.58	-17.36
90	Morocco	2012	2.15	0.66	-0.93	-1.73	-4.18	-4.06	-4.83	-7.64	-13.32
91	Cyprus	2013	-2.30	-0.13	0.08	-0.21	0.30	3.15	2.05	-0.75	-1.33
92	Pakistan	2013	-4.82	0.11	0.12	0.12	1.33	2.41	6.39	2.51	-1.37
93	Tunisia	2013	-0.87	0.16	-0.36	0.20	0.29	2.94	3.16	4.03	3.18
94	Albania	2014	-5.72	5.01	-1.45	-7.88	-2.52	6.61	15.51	15.16	18.99
95	Chad	2014	0.63	-0.09	-0.10	-0.10	-0.87	-1.34	-2.15	-3.26	-4.45
96	Ghana	2015	4.17	0.01	-0.02	-0.02	-1.02	-1.29	-1.17	-1.31	-1.64
97	Mozambique	2015	11.14	10.21	8.38	7.62	7.15	7.88	8.57	9.91	9.95
98	Afghanistan	2016	-6.96	-3.56	3.24	6.17	10.39	15.74	8.05	0.13	-6.40
99	Egypt	2016	-6.94	0.00	0.12	-0.26	-0.35	-3.65	-20.23	-30.49	-34.21
100	Moldova	2016	-0.23	-1.39	2.73	2.03	0.59	-2.33	-4.23	-2.50	-0.84
101	Rwanda	2016	-0.32	0.00	0.02	-0.03	-0.29	-0.47	-1.58	-5.95*	5.9*
102	Sri Lanka	2016	-4.25	0.21	-1.01	0.70	-0.53	1.53	-8.72	-10.08	-10.08
103	Suriname	2016	-1.31	-0.09	-0.09	-0.08	1.17	0.87	1.02	-0.18	-2.92
104	Cameroon	2017	1.39	0.01	-0.02	0.02	-0.07	-0.07	0.15	1.22	0.41
105	Mauritania	2017	-0.13	0.08	0.00	0.00	-1.09*	-1.22	-1.35	0.18	-0.89
106	Mongolia	2017	-2.07	-0.05	0.22	-0.18	2.02	-1.41	5.46	4.56	-0.67
107	Angola	2018	0.30	0.01	0.03	0.03	0.33	-0.01	2.38	4.08*	1.19*
108	Barbados	2018	-0.64	-0.23	0.82	-0.40	-2.71	-4.17*	-4.62	-4.14	-5.02
109	Armenia	2019	0.08	0.10	0.10	0.10	3.35**	5.51**	4.81**	2.49**	1.83**
110	Congo	2019	0.89	0.84	-0.02	-0.23	-0.32	4.35	2.23	2.41	3.03
111	Ecuador	2019	-6.18	-0.09	1.41	-1.03	-1.69	-12.37	-3.06	-2.11	-3.80
112	Eq. Guinea	2019	0.62	0.01	-0.22	0.09	-0.45	-3.32	-3.03	-0.74	0.00
113	Honduras	2019	4.65	0.35	-2.26	1.38	-2.77	1.15	5.17	5.11	3.82
114	Pakistan	2019	5.34	1.82	0.45	-3.69	3.08	8.89	10.78	7.45	6.41
115	Gambia	2020	-0.75	-0.01	0.04	0.00	2.48	1.88	1.41	0.65	
116	Costa Rica	2021	-5.85	-1.34	2.52	-0.29	-3.01	-3.59	-16.35		
117	Kenya	2021	5.86	4.46	3.11	3.08	2.92	3.29	3.86		
118	Moldova	2021	2.59	-0.30	0.00	0.74	3.00	3.96	5.06		
119	Suriname	2021	-0.54	-0.13	-0.12	-0.13	-1.19	-3.30	-2.78		

120	Serbia	2022	1.98	-0.72	0.00	1.27	3.36	2.53
121	Cape Verde	2022	-3.26	-0.11	-0.11	-0.11	0.81	1.68
122	Georgia	2022	2.03	-0.80	0.71	0.67	-2.22	-2.45
123	Macedonia	2022	-3.96	-0.13	-0.11	-0.13	-3.51	-3.16
124	Mozambique	2022	12.46	11.08	10.98	9.81	11.00	11.52
125	Nepal	2022	-1.66	-0.16	0.24	0.02	-2.27	-5.57

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.3.2: Effects of IMF-Supported Programs on Gender Inequality: Female Labor Force Participation Rate, Robustness

A positive gender impact gap implies a higher Female Labor Force Participation Rate in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Female Labor Force Participation (in percent, lagged), Reserve Coverage (lagged), Government Consumption, Reserves (Percent of External Debt).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Bolivia	1994	-1.69	-0.05	-0.15	-0.02	0.55	0.70	1.46	2.18	3.60
2	Cent. Afr. Rep.	1994	-0.10	-0.05	0.02	-0.03	0.18	0.15	0.25	0.48	0.71
3	Chad	1994	-0.02	-0.04	-0.03	0.01	-.84*	-1.71*	-2.6*	-3.2*	-3.81*
4	Sierra Leone	1994	-0.03	-0.03	0.00	0.05	0.12	0.20	0.23	0.59	.98*
5	Uganda	1994	0.31	-0.03	0.03	0.08	0.12	0.48	0.82	1.45	2.07
6	Ghana	1995	4.84	4.63	4.41	4.18	3.96	3.72	3.49	3.24	3.00
7	Mexico	1995	0.96	0.00	0.01	0.00	1.13	-0.96	2.40	0.20	-1.94
8	Madagascar	1996	-0.89	-0.29	0.32	0.85	1.73	2.64	3.67	4.63	5.65
9	Mozambique	1996	-1.17	-0.41	0.35	1.11	1.93	2.88	3.36	3.75	4.18
10	Tanzania	1996	-0.99	-0.33	0.32	0.87	1.69	2.55	3.54	4.47	5.46
11	Venezuela	1996	1.10	0.32	-0.58	-0.11	0.82	1.27	0.86	0.29	0.47
12	Guinea	1997	-0.35	-0.12	-0.02	0.27	0.49	1.01	1.87	2.71	3.65
13	Indonesia	1997	-2.13	0.00	-0.11	0.02	-1.50	1.17	1.87	3.35	0.59
14	Thailand	1997	-1.51	-0.93	0.15	1.37	2.02	-0.22	-1.38	-0.45	0.23
15	Albania	1998	3.10	0.31	-0.62	0.56	-0.04	-0.62	-0.30	-0.45	-0.49
16	Brazil	1998	-0.93	0.52	-1.75	0.42	2.50	5.90	9.14	12.26	16.04
17	Gambia	1998	-0.15	-0.21	-0.21	-0.20	0.05	0.52	0.92	2.54	2.50
18	Rwanda	1998	-1.23	-0.09	-0.11	-0.09	-0.08	0.13	0.44	1.81	1.92
19	Zimbabwe	1998	0.03	0.01	0.01	0.08	.31*	0.74	1.09	1.41	1.75*
20	Cambodia	1999	0.08	0.39	0.04	-0.29	-0.58	-0.91	-1.23	-1.67	-2.10
21	Colombia	1999	0.10	-0.18	0.05	-0.22	-1.61*	-.9*	-0.50	-2.20	0.60
22	Honduras	1999	1.23	-0.01	0.00	0.01	-0.68	-4.70	-5.77	-6.85	-7.91
23	Turkey	1999	4.60	1.83	-3.70	-1.69	1.26	-10.00	-7.41	-4.18	-8.22
24	Chad	2000	0.25	0.04	-0.12	0.14	0.26	0.46	0.13	-0.23	-1.12
25	Ecuador	2000	1.56	-0.27	4.29	-1.31	-5.13	-5.22	-2.06	0.29	9.42
26	Gabon	2000	-0.16	-0.04	-0.04	-0.03	-0.36	-0.81	-0.89	-0.92	-0.79
27	Papua N. Guinea	2000	-0.75	-0.28	0.33	0.48	1.28	-1.72	-4.52	-7.48	-10.43
28	Armenia	2001	0.23	0.10	0.13	0.09	-0.18	-0.85	-1.41	-2.09	-2.52
29	Azerbaijan	2001	1.27	0.78	0.51	-0.36	-2.84	-4.62	-7.40	-10.59	-12.12
30	Georgia	2001	1.56	-0.25	2.55	-1.65	2.50	-0.86	2.14	2.09	-1.96
31	Sierra Leone	2001	0.00	0.08	0.09	0.39	0.15	-0.21	0.07	2.21	1.09
32	Sri Lanka	2001	-10.26	7.23	-1.65	-4.17	-3.74	-3.91	-4.09	-4.88	-5.43
33	Vietnam	2001	0.52	0.08	0.66	-1.26	0.26	-0.14	-0.59	-1.67	0.01
34	Dem. Rep Congo	2002	-0.45	-0.07	0.02	0.15	0.35	0.56	0.72	0.71	-0.67
35	Guatemala	2002	-1.46	-0.05	-0.02	-0.03	0.42	0.58	0.88	-0.22	0.27
36	Uganda	2002	-1.80	0.00	-0.01	0.00	0.33	0.61	0.83	-0.33	1.44
37	Bangladesh	2003	-1.02	-0.19	-0.31	0.15	0.49	1.12	0.84	-1.10	-0.47
38	Bolivia	2003	3.13	-0.09	-0.06	-0.09	0.41	0.83	0.87	7.02	2.45
39	Dom. Republic	2003	-0.51	-0.10	-0.61	0.75	-6.67	-5.20	-6.16	-5.03	-4.90

40	Nepal	2003	-0.63	-0.11	-0.16	-0.11	0.06	0.40	0.82	0.21	1.18
41	Paraguay	2003	0.48	-0.11	-0.12	-0.12	-0.11	10.26*	7.64*	-0.68	4.99*
42	Congo	2004	-0.24	-0.10	0.03	0.14	0.27	0.39	0.40	0.43	0.46
43	Honduras	2004	-0.14	0.01	0.01	0.01	-0.15	0.67	-7.85*	-10.91*	-8.67*
44	Ukraine	2004	0.13	0.03	0.02	0.03	0.07	0.04	-0.10	-0.06	-0.79
45	Cameroon	2005	7.33	7.59	7.87	8.10	8.35	8.39	8.43	5.44	2.14
46	Chad	2005	0.39	0.20	0.02	-0.18	-0.35	-0.57	-0.79	-0.99	-1.22
47	Cent. Afr. Rep.	2006	0.13	-0.02	0.00	-0.05	-0.39	-1.01*	-1.64**	-1.41**	0.04
48	Haiti	2006	-0.33	-0.04	0.09	0.27	0.58	1.20	2.02	2.62	3.45
49	Moldova	2006	-2.52	0.19	-0.24	0.15	4.72	9.96	9.91	9.67	13.38
50	Sierra Leone	2006	-0.80	-0.54	1.20	-0.31	-1.99	-3.72	-5.14	-6.78	-8.15
51	Gambia	2007	-1.46	-0.07	-0.08	-0.07	-0.30	0.89	1.08	1.89	2.93
52	Guinea	2007	-1.18	0.00	-0.05	0.00	3.43	1.32	-2.80	-6.96	-7.33
53	Nicaragua	2007	-0.01	-0.03	-0.03	-0.03	1.26**	3.51**	4.56**	7.05**	8.62**
54	Pakistan	2008	-1.80	-0.65	0.22	-1.94	2.07	5.37	9.23	9.01	8.00
55	Angola	2009	9.35	8.05	6.77	6.12	5.46	5.35	4.36	3.80	2.31
56	Belarus	2009	0.02	0.01	-0.01	0.03	.37*	.67*	.94*	2.1**	3.13**
57	Bosnia & Herzeg.	2009	1.33	0.14	-0.85	0.48	-0.74	-0.62	-2.09	-1.85	-3.61
58	Comoros	2009	0.56	0.05	0.05	0.06	-0.24	-0.03	0.52	1.32	1.42
59	Costa Rica	2009	1.23	0.01	0.01	0.01	0.30	-1.92	3.30	13.92	13.35
60	Dem. Rep Congo	2009	3.93	2.07	0.19	-1.34	-2.89	-4.15	-5.36	-6.74	-8.32
61	El Salvador	2009	-1.17	-0.03	-0.03	-0.03	1.31	6.38	2.13	4.41	5.63
62	Ghana	2009	2.33	1.16	0.01	-0.69	-1.39	-1.70	-1.92	-2.33	-4.57
63	Guatemala	2009	0.51	0.12	0.12	0.13	0.12	0.45	-2.76	9.16	-4.59
64	Mongolia	2009	-0.79	0.06	0.42	-0.53	-0.14	1.95	5.13	5.58	2.65
65	Romania	2009	-1.28	-0.02	0.00	-0.04	-1.46	-0.98	-1.35	-1.15	-1.08
66	Tajikistan	2009	1.81	0.68	-0.11	-1.54	-2.59	-1.19	1.06	2.01	3.31
67	Jamaica	2010	-0.49	-0.05	0.01	-0.36	-0.50	1.43	1.29	3.21	4.97
68	Kenya	2011	0.02	0.06	0.06	0.06	0.15	0.18	0.25	0.22	0.22
69	Macedonia	2011	1.09	0.01	0.02	0.01	2.58	0.20	2.62	0.29	-3.60
70	Bangladesh	2012	0.09	0.06	0.03	0.10	.93**	1.74**	2.87**	3.73**	4.65**
71	Cent. Afr. Rep.	2012	0.19	0.09	0.09	0.10	-0.38	-0.50	-0.38	-0.41	-0.24
72	Gambia	2012	-0.66	-0.01	-0.01	-0.02	-0.07	2.80	4.64	6.12	7.73
73	Guinea	2012	2.82	0.04	-0.01	-0.07	-4.92	-8.78	-11.32	-14.63	-17.40
74	Morocco	2012	2.27	0.70	-0.98	-1.89	-4.49	-4.44	-5.48	-8.31	-14.10
75	Pakistan	2013	-6.92	-3.75	-4.67	-5.89	-7.59	-9.04	-7.33	-14.06	-19.92
76	Tunisia	2013	0.36	-0.30	-1.08	0.88	-1.90	-2.83	-3.51	-3.44	-5.57
77	Albania	2014	-5.18	5.47	-0.93	-7.45	-2.18	7.02	15.97	15.61	19.44
78	Chad	2014	-0.45	-0.06	-0.07	-0.15	-1.2*	-1.91	-3.06*	-4.15*	-5.35*
79	Ghana	2015	1.53	0.03	-0.01	-0.02	-0.64	-0.29	0.26	0.41	0.51
80	Haiti	2015	-0.38	0.05	0.03	0.08	0.19	0.20	1.05	1.09	-0.61
81	Mozambique	2015	11.14	10.21	8.38	7.62	7.15	7.88	8.57	9.91	9.95
82	Egypt	2016	-2.52	-3.25	-4.02	-8.69	-10.89	-16.95	-35.15	-47.55	-51.23
83	Moldova	2016	-0.13	-1.29	2.82	2.11	0.65	-2.29	-4.21	-2.49	-0.85
84	Rwanda	2016	-1.41	0.00	0.04	-0.05	-0.66	-0.34	-1.76	-5.93	4.77
85	Cameroon	2017	1.42	0.00	-0.01	0.00	-0.01	0.25	0.17	0.67	0.17
86	Mauritania	2017	0.19	0.05	-0.03	-0.04	-1.15**	-1.25*	-1.11*	0.13	-0.36
87	Mongolia	2017	-2.37	-0.16	0.16	-0.31	1.82	-1.56	5.57	4.43	-0.49
88	Angola	2018	0.40	0.17	0.11	0.02	-0.08	-0.35	1.00	2.57	0.29

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.4.1: Effects of IMF-Supported Programs on Gender Inequality: Maternal Mortality Rate

A positive gender impact gap implies a higher rate of pregnancy-related deaths (per 100,000 live births) in the in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Maternal Mortality (lagged), Reserve Coverage (lagged).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Congo	2004	1.12	0.02	0.02	0.02	-0.61	-3.60	-0.48	0.36	-3.71
2	Mozambique	2004	7.09	0.04	0.02	0.03	4.55	3.59	3.08	1.24	0.78
3	Ukraine	2004	8.75	-1.14	0.49	0.89	4.03	0.32	18.36	20.29	19.60
4	Zambia	2004	10.25	0.07	0.07	0.06	10.09	16.50	21.45	25.61	35.87
5	Cameroon	2005	-8.14	0.54	-0.32	-0.01	13.15*	16.26	14.88	17.57	23.26
6	Iraq	2005	-3.75	-0.32	-0.34	-0.33	-4.27	3.64	0.97	0.86	-5.76
7	Malawi	2005	9.22	3.17	0.45	-3.98	-9.93	-8.13	6.38	26.58	45.83
8	S Tome & Principe	2005	5.48	0.29	0.31	0.30	7.56	11.27	4.25	4.30	14.51
9	Cent. Afr. Rep.	2006	14.59	14.16	11.14	7.34	3.89	3.71	0.78	0.35	-6.13
10	Grenada	2006	-10.80	-4.51	10.73	-7.72	-5.65	-5.56	-6.58	-4.25	-5.34
11	Haiti	2006	0.56	-2.82	8.81	-2.77	-3.90	1.77	7.27	10.68	13.46
12	Madagascar	2006	-2.18	-0.11	-0.10	-0.11	-2.13	-1.58	-1.75	0.89	0.05
13	Moldova	2006	19.49	3.30	-6.82	1.75	-3.82	-12.27	-9.51	-19.51	-23.41
14	Gambia	2007	1.64	0.03	0.13	-0.16	5.49	2.76	1.19	1.65	2.82
15	Guinea	2007	4.00	1.21	-0.26	-2.00	-7.11	-10.44	-8.12	-9.03	-3.72
16	Nicaragua	2007	-2.37	0.20	2.35	-1.62	-0.65	-1.83	-0.97	5.10	5.49
17	Djibouti	2008	-0.16	1.00	-3.66	0.69	2.18	-5.68	-3.21	1.47	0.91
18	Iceland	2008	-23.10	-2.27	-2.27	-1.55	-23.07	-23.07	-23.07	-22.35	-22.35
19	Liberia	2008	2.83	-0.11	0.56	-0.17	-7.68	-8.05	-2.13	5.81	8.70
20	Pakistan	2008	-2.90	0.06	-0.45	-0.14	1.11	10.76	9.52	10.49	12.64
21	Angola	2009	3.62	-0.03	-0.03	-0.04	-2.32	-2.61	-5.29	-6.33	-6.64
22	Comoros	2009	-5.34	0.07	0.07	0.08	-12.01	-5.21	-7.44	-2.16	-2.33
23	Costa Rica	2009	1.26	-0.40	0.27	0.27	-3.81	-0.29	-2.63	-2.07	-3.63
24	Cote D'Ivoire	2009	-10.73	0.12	0.20	0.04	1.02	9.58	13.61	19.40	24.19
25	Dem. Rep Congo	2009	0.76	0.02	0.42	-0.46	-0.96	5.45	8.84	15.05*	16.81*
26	El Salvador	2009	-7.74	-1.39	2.93	-1.73	2.23	2.64	12.41	16.45	21.66
27	Ethiopia	2009	8.32	1.99	0.99	-5.29	-8.58	-13.94	-13.54	-20.38	-27.12
28	Ghana	2009	5.01	-0.03	-0.03	-0.03	2.81	6.03	7.13	8.39	11.30
29	Maldives	2009	-5.74	0.37	-2.81	1.27	7.96	-1.63	4.43	-3.41	-3.21
30	Mongolia	2009	-8.18	0.81	-1.65	0.48	-4.18	-1.62	0.14	2.49	-1.45
31	Romania	2009	14.83	1.82	-1.55	-4.00	-9.33	1.80	-0.91	-3.11	-3.49
32	Sri Lanka	2009	-0.97	0.44	-0.87	0.69	-7.06	-19.46*	-4.70	-11.24	-16.63
33	Tajikistan	2009	0.67	-0.25	0.05	-3.17	-9.44	-3.02	-5.43	-8.17	-18.49
34	Tanzania	2009	-1.56	0.05	-1.12	1.43	-17.63*	-14.84	-13.10	-19.71	-11.00
35	Ireland	2010	-7.26	-3.14	1.94	6.06	6.06	15.26	6.06	-8.23	10.17
36	Jamaica	2010	6.48	-0.77	2.02	0.07	-0.87	-1.67	5.48	18.07	35.77
37	Solomon Islands	2010	-4.00	0.44	0.45	0.46	3.20	3.98	10.61	7.37	11.42
38	Yemen A. Rep.	2010	7.88	-0.26	-0.32	-0.31	6.13	-3.12	-6.28	12.15	9.31
39	Kenya	2011	2.63	0.08	-0.58	0.48	8.53	14.39	25.18*	26.75*	23.55*
40	Portugal	2011	-6.07	-0.89	0.36	0.85	-3.95	8.28	-2.12	0.23	11.71
41	Bangladesh	2012	11.97	0.02	-0.16	0.12	-12.71	-10.17	-3.94	-0.39	-1.86
42	Cent. Afr. Rep.	2012	4.36	3.11	-3.04	-0.20	-7.88	-9.08	-12.98	-13.20	-17.19
43	Gambia	2012	6.19	-0.12	-0.15	-0.09	0.55	6.76	1.84	7.29	5.10
44	Guinea	2012	-1.18	-0.51	-1.44	2.32	-4.60	-6.32	-8.69	-6.60	-9.66
45	Jordan	2012	-0.08	-0.13	-0.11	-0.17	8.47**	8.23**	7.8**	10.44**	6.45**
46	Morocco	2012	2.88	-0.16	-0.16	-0.13	-1.32	-2.04	1.56	-0.46	-4.14
47	Cyprus	2013	2.93	0.27	0.41	0.62	26.67**	30.35**	46.09**	52.18**	73.21**
48	Pakistan	2013	7.57	0.07	0.04	0.06	-2.78	1.52	-5.46	-4.75	-8.73
49	Tunisia	2013	-0.82	0.17	0.41	0.23	11.29**	9.94**	5.17**	11.3*	15.03**
50	Albania	2014	11.34	-0.60	2.34	-1.11	1.79	0.04	4.13	10.27	-17.37
51	Chad	2014	14.79	17.58	12.33	8.87	0.58	0.99	1.31	-2.63	-4.85
52	Ghana	2015	1.56	0.26	0.26	0.26	1.94	-5.64	1.75	2.96	-8.02
53	Haiti	2015	8.59	-0.02	-0.02	-0.02	1.81	1.66	-7.18	-5.09	-4.80
54	Mozambique	2015	5.98	0.32	0.29	0.29	-2.87	-9.96	-13.77	-21.12	-24.80

55	Afghanistan	2016	-4.48	0.67	-2.00	1.03	0.57	-6.52	-6.57	-10.13	-6.17
56	Egypt	2016	1.54	0.90	0.78	0.66	-2.45	-2.71	-7.12	-5.72	-7.07
57	Iraq	2016	-3.07	-0.12	-0.11	-0.10	-15.26*	-26.2**	-31.08*	-28.52*	-34.58*
58	Madagascar	2016	4.33	-1.28	-0.57	2.72	-5.65	-11.87	-11.86	-16.88	-17.70
59	Moldova	2016	-8.55	-2.09	2.78	0.35	-7.48	-5.65	-15.34	-24.00	-24.99
60	Rwanda	2016	-1.63	0.01	-0.53	0.22	-3.90	-9.91*	-5.25*	-3.59	-9.03
61	Suriname	2016	-4.12	-0.28	0.02	-0.32	-15.09*	-16.6*	-18.18*	-14.28	-10.31
62	Cameroon	2017	-1.31	-0.04	-0.05	-0.04	5.7**	1.99	4.65	11.52	
63	Guinea	2017	0.56	0.01	0.06	0.00	-8.13**	-5.44**	-7.63**	-5.31*	
64	Mauritania	2017	-7.58	-0.15	-0.15	-0.16	0.07	3.18	-6.58	7.65	
65	Mongolia	2017	-0.42	1.26	1.12	1.32	6.35*	-5.20	-16.85*	-15.48*	
66	Angola	2018	0.41	0.09	0.11	0.08	-6.16**	-16.81**	-14.88**		
67	Barbados	2018	-2.78	0.03	0.03	0.02	-2.28	3.57	-17.29		
68	Malawi	2018	-13.29	-0.03	3.09	-2.63	5.84	-7.67	3.59		
69	Armenia	2019	6.64	0.01	0.00	0.00	0.27	-3.91			
70	Congo	2019	4.69	-0.30	0.81	0.18	-21.50	-27.25			
71	Ecuador	2019	-5.34	0.18	0.22	0.19	8.34	-4.97			
72	Eq. Guinea	2019	10.25	-1.99	1.35	6.24	2.78	-6.64			
73	Ethiopia	2019	2.51	0.07	1.74	-2.19	-8.70	-11.11			
74	Liberia	2019	8.35	-2.07	2.05	4.38	2.54	3.25			
75	Pakistan	2019	-1.40	-0.09	-0.10	-0.07	-8.4*	-19.15*			

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.4.2: Effects of IMF-Supported Programs on Gender Inequality: Maternal Mortality Rate, Robustness

A positive gender impact gap implies a higher rate of pregnancy-related deaths (per 100,000 live births) in the in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Maternal Mortality (lagged), Reserve Coverage (lagged), Government Consumption, Reserves (Percent of External Debt).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Congo	2004	-0.15	1.01	0.35	-0.82	-2.70	-5.12	-2.22	-2.16	-7.13
2	Honduras	2004	11.57	0.10	0.04	0.09	-8.74	-3.14	2.55	-1.81	3.47
3	Mozambique	2004	2.34	-0.08	-0.07	-0.07	-0.18	4.37	5.51	7.00	6.50
4	Ukraine	2004	7.34	0.03	0.00	0.02	0.95	-2.55	19.53	19.38	17.12
5	Cameroon	2005	1.37	2.16	-0.77	1.03	11.1*	13.39*	15.74*	15.55*	22.93
6	Haiti	2006	2.48	-3.57	9.79	-2.01	-1.01	4.97	9.95	9.35	19.16
7	Madagascar	2006	1.91	0.81	0.78	-0.72	-1.35	-1.15	-2.48	-3.23	0.92
8	Gambia	2007	15.97	11.90	5.81	6.53	4.93	-3.48	-19.74	-7.55	-4.35
9	Nicaragua	2007	-2.05	-0.12	1.79	-2.47	-3.19	-5.58	-6.91	-0.23	0.84
10	Pakistan	2008	-1.68	0.03	-0.72	-0.22	0.47	9.85	9.21*	10.06*	12.52*
11	Angola	2009	4.73	1.13	3.15	-4.28	-1.35	-5.50	-6.08	-7.66	-8.51
12	Comoros	2009	-5.07	0.02	0.02	0.01	-12.96	-9.14	-7.82	-2.28	0.67
13	Costa Rica	2009	4.17	-0.35	0.34	0.34	-7.07	-4.87	-10.08	-8.57	-8.57
14	Dem. Rep Congo	2009	-1.73	0.90	1.87	-3.08	-15.77*	-1.61	2.51	8.50	18.18
15	El Salvador	2009	-5.83	-0.14	4.18	-0.57	4.14	3.46	12.63	17.66	22.92
16	Ghana	2009	3.47	0.00	0.01	0.00	2.58	4.83	6.79	8.23	11.60
17	Guatemala	2009	7.70	-0.24	-0.23	-0.24	-2.46	-2.06	0.53	1.66	-2.16
18	Mongolia	2009	-7.86	0.88	-2.08	0.65	-0.64	-1.72	1.64	3.64	-1.94
19	Mozambique	2009	-3.58	-0.01	0.05	-0.07	-5.69	-7.02	-6.23	-9.19	-8.83
20	Romania	2009	15.33	0.93	-1.09	-3.97	-9.84	0.93	1.20	-1.50	-0.47
21	Tajikistan	2009	0.31	-0.45	0.97	-1.38	-8.08*	-1.42	-4.97	-6.88	-17.08
22	Tanzania	2009	-1.39	0.05	-1.26	1.20	-17.44*	-14.82	-13.31	-19.99	-11.18
23	Jamaica	2010	7.52	-0.27	2.40	0.31	-3.35	-4.69	0.22	9.77	25.10
24	Solomon Islands	2010	-12.26	0.37	-0.68	-0.14	4.63	4.81	10.00	9.36	13.49
25	Yemen A. Rep.	2010	-3.25	-0.56	1.13	-0.34	6.04	12.3*	18.43**	17.38*	23.23*
26	Kenya	2011	2.30	0.03	-1.68	1.77	5.71	9.16	21.05	18.66	13.14
27	Bangladesh	2012	8.86	0.66	-1.62	0.87	-12.26	-11.59	-5.61	-3.50	-6.78
28	Gambia	2012	8.58	-3.05	2.78	3.53	4.85	16.07	15.48	24.36	25.69
29	Jordan	2012	-3.06	-0.24	0.14	0.07	3.41	7.36	7.44	13.21	12.80

30	Morocco	2012	-1.06	-0.08	-0.05	-0.04	-0.94	-1.25	0.76	-0.95	-8.03
31	Pakistan	2013	1.18	-0.11	-0.20	-0.05	2.54	5.65*	0.20	3.21	-3.38
32	Tunisia	2013	-2.51	0.25	0.43	0.07	11.80	8.20	5.58	6.55	10.03
33	Albania	2014	-7.26	-1.06	1.09	-0.34	1.49	-5.13	5.06	7.23	-21.43
34	Ghana	2015	3.61	-0.72	1.38	-0.62	-0.36	-7.47	-2.14	1.65	-8.94
35	Haiti	2015	9.09	-2.30	1.04	2.16	1.85	3.55	-6.97	-0.43	-2.84
36	Mozambique	2015	11.68	0.01	-0.06	0.30	-5.31	-11.93	-17.28	-23.57	-25.70
37	Egypt	2016	7.55	0.29	-1.00	-0.42	-10.02	-12.52	-16.63	-19.13	-24.29
38	Madagascar	2016	-3.69	-3.16	0.06	2.01	-1.48	-7.25	-4.63	-8.40	-13.43
39	Moldova	2016	-8.40	-8.10	2.93	-0.23	-9.54	-10.75	-11.19	-29.18	-26.69
40	Rwanda	2016	-2.56	0.65	-0.40	0.71	-2.31	-9.42	-2.41	-0.89	-10.63
41	Cameroon	2017	-2.68	0.09	-0.43	0.62	6.09	1.68	6.60	10.01	
42	Guinea	2017	-2.65	-0.64	0.89	2.84	-4.56	0.09	-3.73	-0.36	
43	Mauritania	2017	-6.83	-0.20	-0.19	-0.18	-2.97	2.57	-4.50	3.31	
44	Mongolia	2017	-2.23	0.38	0.06	0.65	5.12	-6.89	-19.72**	-20.53*	
45	Angola	2018	1.06	-0.18	0.03	-0.14	-5.59	-13.77	-11.32		
46	Armenia	2019	3.18	0.02	0.28	-0.11	2.51	-1.06			
47	Congo	2019	0.28	-0.98	1.82	1.71	-15.59	-24.67			
48	Ecuador	2019	-3.33	0.32	0.45	0.45	8.71	1.04			
49	Ethiopia	2019	1.96	-0.05	2.15	-2.52	-9.05	-17.17			
50	Pakistan	2019	-2.94	-0.09	-0.20	0.03	-3.54	-11.61			

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.5.1: Effects of IMF-Supported Programs on Gender Inequality: Primary Enrollment Gender Parity Index

A positive gender impact gap implies a higher ratio of girls to boys enrolled in public and private primary schools in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Primary Enrollment Gender Parity (lagged), Reserve Coverage (lagged)

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Bolivia	1994	-0.08	0.00	-0.12	0.15	-.52*	-.53*	.33*	.28*	0.60
2	Mexico	1995	1.00	0.07	-0.21	-0.02	0.10	0.69	1.36	1.23	1.72
3	Djibouti	1996	-4.48	-0.58	-3.00	-3.41	-8.38	-10.70	-11.22	-17.55	-16.41
4	Tanzania	1996	-0.25	0.00	-0.01	0.00	-0.13	1.02	0.62	1.47	-0.08
5	Indonesia	1997	-1.15	0.12	0.08	0.09	0.66	-3.44	-0.91	-1.04	-0.26
6	South Korea	1997	2.23	0.68	1.30	0.98	1.46	1.18	0.12	-1.24	-0.43
7	Albania	1998	1.21	0.04	0.32	-0.06	-2.05	-1.31	0.17	2.76	1.30
8	Gambia	1998	-2.23	-1.37	0.32	1.52	3.46	2.18	2.08	3.07	0.99
9	Ecuador	2000	-0.71	0.00	0.00	0.00	-0.32	0.65	1.22	1.96	1.65
10	Lithuania	2000	0.83	0.01	0.00	0.02	0.96	0.42	0.41	0.82	1.20
11	Azerbaijan	2001	-1.68	-0.39	0.57	-0.56	0.15	-0.27	-4.08	-1.29	-2.92
12	Georgia	2001	-1.53	-0.14	0.05	0.64	6.34*	8.54	7.15	4.00	5.79
13	Laos	2001	2.02	0.96	0.25	-2.40	-4.74	-6.27	-7.18	-6.57	-6.31
14	Lesotho	2001	7.82	6.55	5.22	0.78	0.19	0.74	1.59	-0.03	0.58
15	Uganda	2002	-0.20	-0.50	0.11	1.37	2.70	1.63	1.87	2.72	3.57
16	Bolivia	2003	0.29	-0.08	-0.12	-0.07	.72*	1.46*	1.66*	1.54*	1.59*
17	Nepal	2003	-9.54	-6.79	-1.72	2.40	4.71	5.69	9.42	14.67	16.61
18	Paraguay	2003	1.29	0.09	0.09	0.09	0.06	0.38	0.27	0.00	-0.74
19	Burundi	2004	-3.51	-4.20	-3.12	-1.16	3.07	5.36	11.28	10.56	14.14
20	Congo	2004	-1.66	0.01	0.49	-0.38	-0.16	-1.22	-4.34	-1.85	-1.88
21	Ukraine	2004	-0.28	0.22	0.03	0.24	-0.23	-0.12	-0.47	-0.71	-0.58
22	Cameroon	2005	-0.73	0.26	-0.75	-0.06	-3.63	-3.74	-3.11	-2.28	-3.30
23	Madagascar	2006	0.87	0.19	0.19	0.19	0.68	0.71	1.21	1.50	1.58
24	Gambia	2007	-1.68	1.05	3.09	4.50	4.17	5.19	3.27	1.68	2.65
25	Guinea	2007	-6.81	-3.49	-1.37	1.03	-0.48	0.05	-0.45	-3.55	1.42
26	Iceland	2008	0.89	-0.01	0.00	0.29	-0.03	-0.91	1.24	-1.64	-1.71
27	Pakistan	2008	-12.59	-8.94	-8.22	-5.89	-5.41	-5.02	-6.64	-8.79	-12.30
28	Seychelles	2008	1.47	-0.25	0.40	1.81	1.15	1.10	2.90	3.12	1.56
29	Belarus	2009	-1.34	0.00	0.00	0.00	0.49	1.23	1.14	1.10	1.40
30	El Salvador	2009	0.44	-0.02	1.62	-0.79	-0.78	-1.78	-1.46	-2.09	-0.90
31	Ethiopia	2009	-3.82	-0.24	-0.05	0.41	1.87	0.61	-0.67	-2.52	-2.88
32	Maldives	2009	1.35	0.05	0.09	-0.15	0.87	0.88	2.75	1.21	2.25

33	Mongolia	2009	0.07	0.27	0.26	-0.22	-0.57	-1.69	-2.11	-1.89	-1.98
34	Mozambique	2009	-3.28	-1.35	0.05	-0.88	-0.92	-1.47	-1.13	-3.11	-3.15
35	Sri Lanka	2009	0.12	0.00	0.00	0.00	-.89**	-1.92**	-3.87**	-1.91**	-2.65**
36	Tajikistan	2009	0.59	-0.10	-0.10	-0.10	0.34	0.81	0.35	1.15	1.25
37	Tanzania	2009	0.59	0.00	0.00	-0.01	0.88	3.04	3.65	2.00	4.13
38	Ireland	2010	0.27	-0.05	0.05	0.07	0.26	-0.37	-0.45	-0.93	-0.81
39	Lesotho	2010	0.99	0.02	-0.03	0.00	-2.34	-3.26	-2.87	-2.74	-4.06
40	Portugal	2011	1.73	0.08	0.15	0.10	1.37	-0.14	-1.14	-1.93	-0.72
41	St Kitts & Nevis	2011	-0.82	-0.10	-0.10	-0.10	2.79*	2.76	5.27	3.56	3.42
42	Gambia	2012	4.00	0.71	0.50	-0.20	-0.02	-1.00	0.85	2.32	3.83
43	Guinea	2012	-3.66	-5.82	-8.61	-3.84	-5.52	-6.34	-9.00	-11.67	-13.28
44	Morocco	2012	-0.16	0.01	-0.01	0.01	-0.11	-0.16	-0.85	-1.81*	-1.34
45	Cyprus	2013	-0.02	0.05	-0.03	-0.02	.48*	1.01*	.15*	-.12*	-.77*
46	Pakistan	2013	-4.09	-4.98	-2.88	-2.01	-3.13	-7.95	-6.57	-9.00	-8.84
47	Albania	2014	-0.57	0.10	0.10	0.10	1.86	1.50	2.08	2.31	2.98
48	Seychelles	2014	0.12	1.78	1.59	-0.92	-0.33	1.48	1.94	4.46	5.49
49	Ghana	2015	-0.75	0.00	0.00	0.01	0.29	1.04	0.87	0.57	0.82
50	Mozambique	2015	0.68	-0.51	-0.08	0.26	-0.13	0.12	0.28	0.19	0.55
51	Suriname	2016	-2.17	0.02	0.00	-0.03	-1.52	0.30	-0.07	0.97	2.14
52	Mongolia	2017	-0.69	0.10	0.10	0.10	-0.08	0.04	0.33	0.43	0.53
53	Bolivia	1994	-0.08	0.00	-0.12	0.15	-.52*	-.53*	.33*	.28*	0.60
54	Mexico	1995	1.00	0.07	-0.21	-0.02	0.10	0.69	1.36	1.23	1.72
55	Djibouti	1996	-4.48	-0.58	-3.00	-3.41	-8.38	-10.70	-11.22	-17.55	-16.41
56	Tanzania	1996	-0.25	0.00	-0.01	0.00	-0.13	1.02	0.62	1.47	-0.08
57	Indonesia	1997	-1.15	0.12	0.08	0.09	0.66	-3.44	-0.91	-1.04	-0.26
58	South Korea	1997	2.23	0.68	1.30	0.98	1.46	1.18	0.12	-1.24	-0.43
59	Albania	1998	1.21	0.04	0.32	-0.06	-2.05	-1.31	0.17	2.76	1.30
60	Gambia	1998	-2.23	-1.37	0.32	1.52	3.46	2.18	2.08	3.07	0.99
61	Ecuador	2000	-0.71	0.00	0.00	0.00	-0.32	0.65	1.22	1.96	1.65
62	Lithuania	2000	0.83	0.01	0.00	0.02	0.96	0.42	0.41	0.82	1.20
63	Azerbaijan	2001	-1.68	-0.39	0.57	-0.56	0.15	-0.27	-4.08	-1.29	-2.92
64	Georgia	2001	-1.53	-0.14	0.05	0.64	6.34*	8.54	7.15	4.00	5.79
65	Laos	2001	2.02	0.96	0.25	-2.40	-4.74	-6.27	-7.18	-6.57	-6.31

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.5.2: Effects of IMF-Supported Programs on Gender Inequality: Primary Enrollment Gender Parity Index, Robustness

A positive gender impact gap implies a higher ratio of girls to boys enrolled in public and private primary schools in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Primary Enrollment Gender Parity (lagged), Reserve Coverage (lagged), Government Consumption, Reserves (Percent of External Debt).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Azerbaijan	2001	-0.24	-0.18	0.91	-0.45	-0.54	-1.59	-4.91	-2.05	-3.89
2	Georgia	2001	0.14	1.98	2.72	4.20	7.87	8.32	5.75	3.38	4.19
3	Uganda	2002	-0.94	-1.53	0.09	1.19	2.54	1.27	1.41	2.15	3.05
4	Bolivia	2003	-0.18	0.13	0.16	0.21	0.92	1.54	1.65	1.71	1.74
5	Nepal	2003	-9.58	-6.61	-1.39	2.97	5.34	6.40	10.07	15.30	17.11
6	Paraguay	2003	0.46	0.19	-0.09	0.14	-0.19	1.14	1.72	0.83	-0.49
7	Burundi	2004	-3.51	-4.20	-3.12	-1.16	3.07	5.36	11.28	10.56	14.14
8	Congo	2004	-1.67	-0.16	0.49	-0.41	0.72	-0.38	-4.07	-2.03	-2.72
9	Ukraine	2004	-0.47	0.69	0.30	0.00	-0.53	-0.29	-0.32	-0.02	0.46
10	Cameroon	2005	-0.75	0.25	-0.76	-0.07	-3.65	-3.75	-3.12	-2.28	-3.32
11	Madagascar	2006	0.77	0.09	0.10	0.09	0.47	0.33	0.94	1.29	1.20
12	Gambia	2007	-1.82	0.99	3.19	4.80	5.04	6.24	4.14	2.02	3.14
13	Guinea	2007	-6.81	-3.49	-1.37	1.03	-0.48	0.05	-0.45	-3.55	1.42
14	Pakistan	2008	-16.04	-11.96	-9.83	-5.81	-5.78	-5.84	-7.35	-8.10	-7.49
15	Belarus	2009	-1.34	-0.01	0.01	0.02	0.51	1.24	1.15	1.11	1.41
16	Costa Rica	2009	0.11	-0.01	-0.01	0.00	0.02	-0.20	0.11	0.18	0.61

17	El Salvador	2009	0.53	0.07	1.71	-0.71	-0.79	-1.79	-1.51	-2.15	-1.08
18	Guatemala	2009	-1.20	0.00	0.00	0.00	0.90	1.47	2.27	1.46	1.02
19	Mongolia	2009	0.36	0.33	-0.08	-0.55	-1.51	-2.46	-2.73	-3.07	-3.38
20	Mozambique	2009	-4.74	-1.81	1.13	-0.45	-0.93	-1.31	0.23	0.02	-0.33
21	Tajikistan	2009	0.78	0.09	0.10	0.11	0.55	1.02	0.55	1.35	1.44
22	Tanzania	2009	0.55	0.10	0.10	0.10	0.84	3.33	3.40	1.92	3.91
23	Gambia	2012	4.00	0.70	0.52	-0.22	-0.04	-1.02	0.84	2.31	3.83
24	Guinea	2012	-3.66	-5.82	-8.61	-3.84	-5.52	-6.34	-9.00	-11.67	-13.28
25	Morocco	2012	-0.89	-0.02	-0.02	0.07	0.60	0.45	0.30	-0.87	-0.32
26	Pakistan	2013	-4.09	-4.98	-2.88	-2.01	-3.13	-7.95	-6.57	-9.00	-8.84
27	Tunisia	2013	-0.74	-0.10	-0.09	-0.09	-0.09	0.10	0.07	0.40	-0.22
28	Albania	2014	-0.71	0.01	0.00	0.01	1.46	1.23	1.62	1.32	1.92
29	Ghana	2015	-0.67	0.00	0.01	0.01	0.30	1.22	1.15	0.95	1.09
30	Mozambique	2015	1.00	-0.50	0.00	0.25	-0.08	0.20	0.47	0.50	1.01
31	Mongolia	2017	-0.80	0.09	0.16	0.07	0.33	0.08	-0.26	-0.13	-0.45

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.6.1: Effects of IMF-Supported Programs on Gender Inequality: Secondary Enrollment Gender Parity Index

A positive gender impact gap implies a higher ratio of girls to boys enrolled in public and private secondary schools in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Secondary Enrollment Gender Parity (lagged), Reserve Coverage (lagged)

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	South Korea	1997	0.69	-0.13	0.03	-0.15	-2.13	-1.96	-1.78	-0.41	1.46
2	Bangladesh	2003	-6.42	-2.96	1.25	2.90	3.11	-2.08	0.23	-3.65	-3.99
3	Bolivia	2003	2.23	-0.04	-0.02	0.02	-1.87	-1.53	-1.33	-1.74	-1.64
4	Paraguay	2003	1.55	-0.11	-0.10	-0.10	-0.24	3.70	2.06	2.81	5.75
5	Ukraine	2004	0.53	-0.15	-0.02	-0.16	5.09	-6.35	-2.55	-1.58	-2.11
6	Cameroon	2005	1.83	-20.44	5.11	2.90	0.69	-0.45	-9.90	-7.89	-4.38
7	Iceland	2008	1.66	-0.04	-0.02	0.00	-3.39	-3.32	-4.58	-5.59	-5.07
8	Seychelles	2008	3.24	1.22	-0.71	0.65	4.15	1.33	-3.22	-2.67	-5.68
9	Costa Rica	2009	0.42	0.04	-0.05	0.08	0.47	-0.55	-0.18	0.15	1.26
10	El Salvador	2009	0.99	0.00	-0.01	0.00	-1.15	-2.71	-4.22	-3.03	-2.91
11	Ethiopia	2009	-20.48	-18.42	-13.92	-8.34	-3.25	1.37	5.83	5.95	7.68
12	Guatemala	2009	0.77	0.01	0.03	0.00	0.22	-3.44*	-4.86**	-3.58	-5.23*
13	Mozambique	2009	-9.16	-8.15	-7.90	-7.54	-4.30	-2.30	1.55	2.22	1.31
14	Romania	2009	1.84	-0.01	-0.01	-0.01	-2.59	-3.37	-2.30	-4.20	-3.25
15	Tajikistan	2009	2.41	0.02	-0.44	0.93	1.81	-3.59	-4.21	-3.63	-5.06
16	Lesotho	2010	-5.11	-0.81	3.58	9.00	8.26	3.52	2.66	0.90	0.01
17	Portugal	2011	2.02	-0.11	-0.12	-0.11	0.95	-1.38	-4.19	-5.34	-5.52
18	St Kitts & Nevis	2011	-1.60	1.05	0.85	-4.09	3.06	-0.18	10.86	-5.00	-0.97
19	Albania	2014	1.39	0.95	0.05	-1.25	-3.85	-2.06	-0.61	0.42	2.40
20	Seychelles	2014	1.52	1.20	-2.06	0.76	-3.37	-0.72	3.96	5.29	1.29
21	Ghana	2015	-0.09	-0.10	-0.12	-0.07	0.76	2.13*	1.32*	1.97*	2.7*
22	Mozambique	2015	-0.27	0.54	0.11	-1.83	-1.62	1.52	-3.95	4.33	3.76
23	Cameroon	2017	-2.87	-6.35	-7.34	-7.36	-6.89	-8.01	-8.20	-8.53	16.85

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.6.2: Effects of IMF-Supported Programs on Gender Inequality: Secondary Enrollment Gender Parity Index, Robustness

A positive gender impact gap implies a higher ratio of girls to boys enrolled in public and private secondary schools in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Secondary Enrollment Gender Parity (lagged), Reserve Coverage (lagged), Government Consumption, Reserves (Percent of External Debt).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Cameroon	2005	6.08	-21.29	6.64	4.88	2.78	1.21	-6.12	-3.45	1.18
2	Costa Rica	2009	0.97	0.10	-0.39	0.29	1.18	0.72	0.05	1.42	2.22
3	El Salvador	2009	0.38	0.04	0.28	-0.02	-0.76	-2.19*	-3.03*	-2.82*	0.55
4	Guatemala	2009	1.16	0.06	0.13	0.07	0.95	-3.84	-5.83	-5.06	-6.66
5	Mozambique	2009	-9.16	-8.15	-7.90	-7.54	-4.30	-2.30	1.55	2.22	1.31
6	Romania	2009	2.18	-0.08	-0.18	-0.05	-0.38	-1.60	-3.95	-3.68	-5.14
7	Tajikistan	2009	1.45	-0.56	-0.37	1.42	1.74	-1.58	-1.90	-0.52	-2.28
8	Albania	2014	1.16	0.66	-0.46	-1.36	-4.06	-2.38	-1.13	-0.01	1.95
9	Ghana	2015	-3.43	-3.99	-2.69	-0.86	0.75	2.23	2.43	3.42	3.99
10	Mozambique	2015	-9.14	-6.84	-5.44	-5.91	-3.92	-0.92	-6.30	2.47	1.38

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.7.1: Effects of IMF-Supported Programs on Gender Inequality: Tertiary Enrollment Gender Parity Index

A positive gender impact gap implies a higher ratio of females to males enrolled in public and private post-secondary education in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Tertiary Enrollment Gender Parity (lagged), Reserve Coverage (lagged).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Ukraine	2004	0.58	0.13	-0.07	0.17	1.66*	1.88*	1.43*	.65*	2.42*
2	Madagascar	2006	1.18	-0.53	2.18	-1.30	-6.88	-9.02	-11.61	-12.08	-10.30
3	Moldova	2006	-3.64	-0.88	-0.62	1.96	-0.93	-2.97	-0.63	-2.61	-4.64
4	Iceland	2008	-0.81	0.09	-0.18	0.10	0.30	3.19	-0.05	0.46	1.16
5	Belarus	2009	1.01	-0.04	-0.66	0.74	-0.05	-0.27	-3.82	-5.15	-5.75
6	Bosnia & Herzeg.	2009	4.68	-0.03	-0.03	0.01	-1.40	-0.18	-1.63	-4.06	-6.01
7	El Salvador	2009	1.59	0.00	-0.01	-0.01	-0.03	1.05	0.38	-0.06	2.12
8	Mongolia	2009	5.19	-0.49	0.08	0.63	-1.05	-0.57	-1.60	-4.96	-6.62
9	Romania	2009	-2.55	0.08	0.09	0.21	1.48	0.65	0.90	-4.81	-4.86
10	Ireland	2010	0.82	1.16	1.12	-2.62	-9.39	-3.30	-8.73	-9.22	-6.32
11	Morocco	2012	9.19	0.02	0.02	0.03	-2.59	-1.68	-2.86	-5.47	-10.00
12	Cyprus	2013	-7.57	-5.72	6.19	4.54	7.64	13.83	11.47	2.82	0.74
13	Tunisia	2013	-2.66	0.10	0.56	-0.51	0.41	1.19	2.07	3.33	4.37
14	Albania	2014	1.71	-0.08	-0.12	-0.11	0.43	0.49	3.82	7.02	9.54
15	Madagascar	2016	-0.51	1.66	-1.39	-3.58	-1.81	-0.90	0.36	-0.85	-3.34
16	Moldova	2016	0.22	0.13	-0.87	0.50	2.37	2.26	5.10	7.54	9.80
17	Sri Lanka	2016	3.25	2.07	-7.08	2.60	3.15	2.20	0.69	3.69	2.34
18	Mongolia	2017	0.02	0.12	-0.69	-0.04	1.33	-0.02	1.93	4.47	4.65
19	Ukraine	2004	0.58	0.13	-0.07	0.17	1.66*	1.88*	1.43*	.65*	2.42*

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.7.2: Effects of IMF-Supported Programs on Gender Inequality: Tertiary Enrollment Gender Parity Index, Robustness

A positive gender impact gap implies a higher ratio of females to males enrolled in public and private post-secondary education in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Tertiary Enrollment Gender Parity (lagged), Reserve Coverage (lagged), Government Consumption, Reserves (Percent of External Debt).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Madagascar	2006	-2.30	-2.00	3.55	-1.58	-3.58	-3.99	-3.10	-4.10	-2.56
2	Moldova	2006	-3.55	-1.07	-0.34	2.06	-1.25	-2.51	-0.12	-2.56	-4.16
3	Belarus	2009	0.13	-1.43	-0.96	2.01	0.74	0.20	-1.10	-3.38	-4.25
4	Bosnia & Herzeg.	2009	3.19	-0.31	0.55	0.11	-0.36	0.48	-1.11	-2.12	-2.73
5	El Salvador	2009	1.85	0.11	0.09	0.11	-0.06	1.13	1.24	0.86	2.49
6	Mongolia	2009	3.69	-0.19	-0.21	0.78	-0.20	0.10	-2.86	-4.87	-6.38
7	Romania	2009	-0.85	0.04	0.15	0.19	1.42*	1.56	-0.05	-5.65*	-5.96*
8	Morocco	2012	8.59	-0.16	-0.04	-0.12	-2.29	-0.55	-1.41	-3.86	-8.23
9	Tunisia	2013	-2.69	0.08	0.57	-0.48	0.45	1.25	2.12	3.41	4.45
10	Albania	2014	1.67	0.11	0.10	0.07	0.59	0.63	3.59	6.44	8.70
11	Madagascar	2016	-1.32	1.44	-1.28	-3.24	-1.86	-1.73	-2.03	-3.64	-6.35
12	Moldova	2016	0.22	0.12	-0.65	0.66	2.90	2.51	5.19	7.12	9.44
13	Mongolia	2017	0.20	0.46	-0.76	-0.48	1.41	1.33	4.56	7.56	7.66

Note: *, **, and *** represent significance at the 0.10, 0.05, and 0.01 levels, respectively.

Table A.8.1: Effects of IMF-Supported Programs on Gender Inequality: United Nations Gender Inequality Index (GII)

A positive gender impact gap implies a lower GII, and hence lower gender inequality, in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Gender Inequality Index (lagged), Reserve Coverage (lagged).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Turkey	1994	-2.81	-1.66	-0.45	2.15	1.16	1.30	0.31	-0.35	1.65
2	Ghana	1995	0.80	-0.06	0.19	0.16	1.40	0.48	0.40	0.24	2.15
3	Mexico	1995	-0.85	-0.04	0.05	-0.09	-0.48	-0.69	-2.37	-4.13	-1.23
4	Venezuela	1996	-1.15	0.02	-0.07	0.05	0.64	0.75	0.80	-1.30	1.52
5	Indonesia	1997	-0.76	0.11	0.10	0.10	-1.59	-0.17	0.85	5.14	7.39
6	Thailand	1997	0.65	0.01	0.00	0.01	-3.45	-3.39	-1.79	-2.06	-7.11
7	Brazil	1998	0.51	-0.03	0.04	-0.02	0.03	0.33	2.07	0.97	-0.77
8	Cambodia	1999	1.07	0.00	-0.01	0.00	2.28	0.15	0.06	6.61	5.99
9	Ecuador	2000	8.85	4.88	-7.95	-0.75	3.97	2.63	6.02	3.39	-0.56
10	Gabon	2000	0.59	0.12	-0.15	0.24	-2.74	0.85	0.96	0.51	0.57
11	Lithuania	2000	1.08	0.18	0.28	-0.89	11.54	13.16	8.65	8.98	-8.45
12	Armenia	2001	-2.94	-2.74	4.56	1.92	3.43	7.54	-0.36	0.32	-0.66
13	Azerbaijan	2001	0.15	-0.02	0.03	-0.05	1.99*	6.77**	9.23**	6.82**	5.39**
14	Sri Lanka	2001	0.84	-3.25	2.96	3.83	3.29	4.70	6.11	3.76	14.45
15	Vietnam	2001	-0.61	0.10	0.11	0.14	-1.55	-1.89	-2.21	-3.94	-2.62
16	Guatemala	2002	-2.24	2.69	-3.01	-0.89	0.40	1.04	1.44	5.97	5.65
17	Bangladesh	2003	-2.91	-3.16	-1.38	8.56	9.54	9.49	-0.59	-1.61	-1.16
18	Bolivia	2003	-1.97	-0.08	-0.11	-0.15	1.09	0.75	6.09	6.54	8.42
19	Dom. Republic	2003	0.16	0.21	-0.33	-0.59	4.74*	3.51*	4.26*	2.04	3.12
20	Nepal	2003	1.41	-0.03	-0.01	-0.01	-0.67	-0.02	1.67	0.59	-6.19
21	Paraguay	2003	1.40	-0.72	-0.19	1.62	-2.11	-2.72	-1.75	-1.83	-4.20
22	Burundi	2004	-1.71	-0.01	0.03	-0.02	0.36	-1.53	-1.53	-1.46	-3.00
23	Honduras	2004	-2.68	-1.92	2.89	-0.04	-3.48	-13.30	-11.81	-12.00	-14.08
24	Mozambique	2004	-1.52	0.11	0.12	0.11	-1.04	0.32	0.51	1.35	1.25
25	Ukraine	2004	-1.16	-0.29	-1.21	1.60	1.36	0.79	-3.83	-1.45	-2.44
26	Zambia	2004	-1.36	-0.10	-0.10	-0.09	0.97	4.39	4.64	4.89	5.29

27	Cameroon	2005	3.13	0.03	-0.09	0.09	3.89	4.61	1.59	2.26	-0.11
28	Iraq	2005	-0.19	-0.08	-0.23	0.05	3.29*	-2.82*	-5.81**	-5.4**	-6.95*
29	Malawi	2005	1.06	0.36	0.96	-1.93	0.93	1.38	3.31	4.67	-0.17
30	Haiti	2006	2.39	-0.01	-0.01	0.00	6.21*	4.46*	5.34	6.04	6.45
31	Madagascar	2006	1.16	0.28	-0.74	-0.11	0.09	-0.37	0.30	-0.78	-1.33
32	Moldova	2006	4.63	-0.01	0.04	-0.10	-3.81	-9.15	-13.55	-14.72	-6.56
33	Nicaragua	2007	0.74	0.01	-0.07	0.10	-1.65	-1.93	-4.27	-5.78	-12.47
34	Iceland	2008	3.12	0.04	-0.33	0.34	3.17	-6.04	-10.64	-9.46	-6.85
35	Pakistan	2008	-0.07	1.52	-2.00	-0.10	-0.06	-2.18	-1.38	-0.27	1.18
36	Bosnia & Herzeg.	2009	1.62	-0.14	0.75	-0.58	-15.07	-13.71	-22.68	-21.35	-24.32
37	Costa Rica	2009	0.15	-0.05	0.31	-0.20	-2.90	-3.27	-2.39	-4.68	-3.88
38	Dem. Rep Congo	2009	-2.45	0.02	-0.19	0.34	-0.37	0.53	1.74	6.79	7.87
39	El Salvador	2009	5.23	-0.14	0.87	-0.50	-2.54	-2.09	2.10	-1.77	0.41
40	Ethiopia	2009	0.48	0.00	0.12	-0.07	-2.97	-2.64	-1.12	0.02	-1.36
41	Ghana	2009	0.71	-0.05	-0.46	0.13	-1.18	-0.54	-0.59	-1.54	1.37
42	Guatemala	2009	0.64	-0.09	-0.32	0.00	0.26	0.70	1.46	-0.83	1.32
43	Maldives	2009	1.55	-0.07	-0.65	0.26	10.91	10.29	8.62	6.88	4.48
44	Mongolia	2009	-5.39	0.22	-2.27	1.82	3.40	3.59	5.14	-8.97	-11.08
45	Romania	2009	1.32	0.00	0.00	0.00	-2.87	-3.42	-8.69	-12.46	-13.32
46	Sri Lanka	2009	4.64	0.10	0.12	0.08	3.20	7.24	7.19	6.66	3.04
47	Tajikistan	2009	0.77	-0.03	-0.09	0.21	-2.62	-0.51	-2.72	-5.05	-7.35
48	Tanzania	2009	-0.51	-0.12	-0.11	-0.12	-2.72	-1.45	-2.58	-0.91	0.72
49	Greece	2010	5.17	0.08	1.65	-1.17	-0.54	-3.25	-9.43	-7.65	-5.69
50	Ireland	2010	3.72	-0.08	-0.30	-0.32	-1.01	-7.84	-9.63	-9.91	-10.29
51	Jamaica	2010	0.19	0.04	0.05	0.04	-0.29	-2.54**	.23*	4.91**	7.34**
52	Lesotho	2010	1.12	0.00	0.00	0.02	1.23	2.13	2.97	4.53	6.06
53	Kenya	2011	3.43	0.12	0.05	0.10	0.16	3.39	0.66	0.83	0.44
54	Bangladesh	2012	11.48	0.01	-0.06	-0.02	-1.52	1.35	0.62	0.98	0.26
55	Jordan	2012	1.82	0.20	-0.61	0.09	2.31	2.66	6.03	5.75	-0.95
56	Morocco	2012	-0.56	-0.14	0.64	-0.31	3.51	2.16	0.39	-3.77	-6.18
57	Cyprus	2013	-1.30	-2.75	2.73	2.65	9.33	0.66	8.83	5.19	18.89
58	Pakistan	2013	-1.80	-0.10	-0.12	-0.09	0.79	0.64	0.58	2.23	-0.30
59	Tunisia	2013	1.12	-0.46	0.59	-0.11	0.56	-1.93	-6.46	-9.51	-11.42
60	Albania	2014	-1.89	0.33	0.33	0.31	1.44	2.10	-1.77	-4.87	-4.50
61	Haiti	2015	-1.95	0.08	0.09	0.08	-0.48	35.17**	1.02*	1.53*	-.61*
62	Mozambique	2015	0.67	0.00	0.00	0.00	0.21	1.05	1.15	-0.73	-1.67
63	Egypt	2016	-2.57	-0.21	-0.20	-0.20	-24.12**	-25.19*	-21.58*	-19.17*	-21.45
64	Iraq	2016	2.30	0.85	-0.50	-0.74	-3.21	0.79	0.86	-0.11	0.96
65	Madagascar	2016	0.04	0.00	0.00	0.00	.52**	.26*	.8*	4.54**	5.26**
66	Moldova	2016	-3.18	-0.19	1.00	-0.61	-2.55	-5.60	-3.59	-4.42	-4.95
67	Rwanda	2016	4.25	0.02	-0.01	0.03	0.14	-0.78	1.06	2.94	0.61
68	Sri Lanka	2016	4.02	0.04	-0.04	0.06	-0.97	-1.66	-0.22	3.59	2.84
69	Suriname	2016	-2.06	0.39	2.04	-2.72	-4.43	-4.69	-4.72	-4.54	-1.79
70	Cameroon	2017	0.00	-0.02	-0.04	0.00	0.18	-0.88	-1.52	-1.35	-0.93
71	Mauritania	2017	0.55	0.03	-0.06	-0.02	-2.35	-0.37	-3.01	-1.45	-1.24
72	Mongolia	2017	2.62	-0.01	0.14	0.00	-2.68	-5.85	-7.64	-10.94	-8.23
73	Angola	2018	1.06	0.05	-0.05	0.08	-1.25	-2.55	-2.19	-1.77	-2.76
74	Barbados	2018	-1.23	0.07	0.07	0.07	-3.41	-4.83	-9.42	-7.96	-9.14
75	Armenia	2019	1.70	4.03	-5.46	-3.24	-8.35	-6.47	-11.76	-9.93	
76	Congo	2019	0.02	0.10	-0.11	0.08	-4.55**	-4.05**	-3.69**	-5.25**	
77	Ecuador	2019	1.02	-0.01	-0.05	0.08	-0.58	-3.96	-2.91	-2.83	
78	Liberia	2019	0.62	-0.12	0.55	-0.31	-3.43	-1.27	-0.73	-0.36	
79	Pakistan	2019	-1.80	-0.09	-0.12	-0.09	-3.38	-3.78	-3.94	-3.28	
80	Gambia	2020	-24.90	0.52	0.18	-0.62	-0.35	-0.43	-0.56		
81	Costa Rica	2021	8.65	0.08	0.07	0.08	-0.87	-1.56			
82	Kenya	2021	-1.05	-0.06	-0.06	-0.05	0.10	-0.14			
83	Madagascar	2021	0.13	-0.02	0.01	0.01	1.33**	1.13**			
84	Moldova	2021	-1.64	-0.03	-0.02	-0.08	-12.73**	-13.39**			
85	Suriname	2021	1.24	0.01	0.01	0.02	0.32	0.67			

Table A.8.2: Effects of IMF-Supported Programs on Gender Inequality: United Nations Gender Inequality Index (GII)

A positive gender impact gap implies a lower GII, and hence lower gender inequality, in IMF-supported programs compared to the control group.

Synthetic Control Predictors: Gender Inequality Index (lagged), Reserve Coverage (lagged), Government Consumption, Reserves (Percent of External Debt).

	Country	Year(t)	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
1	Brazil	1998	-0.32	-0.20	0.21	-0.05	-1.10	0.02	-1.14	-3.40	1.74
2	Ecuador	2000	4.87	7.18	-7.65	-0.68	2.51	2.14	3.28	2.96	7.63
3	Gabon	2000	0.33	-0.02	-0.72	0.48	-2.68	-0.81	0.15	-2.61	-1.07
4	Armenia	2001	-4.06	-3.78	5.10	1.89	-0.06	1.52	-7.89	-9.10	-11.38
5	Azerbaijan	2001	-17.71	-13.62	-1.91	-5.45	-3.00	0.00	1.36	13.35	7.63
6	Sri Lanka	2001	-2.33	-5.19	0.64	4.21	2.32	3.01	1.77	-0.06	1.01
7	Vietnam	2001	-25.28	-22.19	-9.55	-8.71	-11.24	-9.55	-9.27	4.49	1.40
8	Guatemala	2002	-1.59	2.65	-4.35	-1.47	-0.64	-0.87	-0.26	2.94	1.61
9	Bangladesh	2003	-2.05	-2.99	-1.22	8.26	8.29	6.84	-2.38	-3.27	-2.88
10	Bolivia	2003	-1.64	-0.06	-0.15	-0.14	-0.25	-1.06	2.62	1.23	3.28
11	Dom. Republic	2003	2.17	0.45	0.00	-1.22	5.99	13.92	14.00	13.67	16.32
12	Nepal	2003	1.88	0.05	0.14	0.08	-0.65	-0.24	1.13	0.01	-6.40
13	Paraguay	2003	1.34	-0.98	0.06	1.94	-2.83	-4.76	-2.51	-2.55	-4.34
14	Burundi	2004	0.06	-0.10	0.32	-0.12	0.48	-1.70	-2.01	-2.46*	-4.63*
15	Honduras	2004	-2.64	-1.76	3.09	0.18	-3.24	-12.97	-11.42	-11.63	-13.70
16	Mozambique	2004	0.36	0.00	0.08	0.01	-0.90	0.40	0.24	0.55	-0.28
17	Ukraine	2004	-1.80	-2.52	-2.37	2.17	4.32	1.58	-2.75	4.06	3.74
18	Cameroon	2005	3.10	0.02	-0.24	0.16	3.79	4.47	1.60	2.16	0.65
19	Haiti	2006	-1.05	-0.18	-0.11	0.12	7.23**	7.8**	9.69**	8.7**	10.89*
20	Madagascar	2006	-2.66	-0.04	-1.25	1.36	2.07	2.34	2.20	-0.29	0.99
21	Moldova	2006	4.20	0.81	-1.37	-9.96	-11.02	-14.64	-16.56	-17.52	-10.53
22	Nicaragua	2007	-4.55	-0.61	-0.22	1.22	0.47	-1.25	-7.02	-9.19	-17.03
23	Pakistan	2008	0.30	3.18	-0.16	1.77	1.43	-2.34	-0.31	1.28	1.12
24	Costa Rica	2009	-0.35	-0.04	0.28	-0.20	-3.24	-3.73	-2.87	-4.72	-3.89
25	Dem. Rep Congo	2009	2.42	5.89	6.50	6.34	3.02	6.95	9.21	9.67	9.82
26	El Salvador	2009	5.26	-0.23	0.84	-0.53	-1.11	-0.41	3.03	-1.77	-0.40
27	Ghana	2009	1.07	0.08	-0.40	0.28	-0.37	0.10	0.43	-1.36	1.38
28	Guatemala	2009	0.81	0.06	-0.23	0.17	0.60	1.19	2.32	0.00	1.29
29	Mongolia	2009	-5.41	0.46	-2.66	2.04	3.75	3.49	5.42	-8.75	-10.91
30	Mozambique	2009	-1.04	0.01	0.04	0.01	-2.27	0.40	2.42	2.05	0.51
31	Romania	2009	1.11	0.11	0.11	0.12	-2.44	-3.44	-8.19	-11.35	-12.87
32	Tajikistan	2009	0.68	-0.08	-0.09	0.23	-2.67	-0.51	-2.68	-4.97	-7.22
33	Tanzania	2009	-0.95	-0.10	-0.03	-0.11	-1.48	-0.32	-1.34	-0.57	0.04
34	Jamaica	2010	0.27	-0.01	0.01	0.00	-0.12	-1.94*	1.54*	2.62*	4.13*
35	Kenya	2011	3.60	0.22	-0.62	0.26	0.03	1.63	-1.75	-1.26	-2.63
36	Bangladesh	2012	11.11	0.02	-0.05	0.03	-1.56	0.51	-0.30	0.40	-0.25
37	Jordan	2012	2.87	0.22	-0.61	0.08	1.52	2.63	4.69	4.66	-1.60
38	Morocco	2012	1.01	0.02	1.22	-0.58	0.76	1.24	-0.25	-4.07	-6.79
39	Pakistan	2013	-1.32	-0.05	0.28	-0.10	-0.04	0.07	0.23	1.17	1.51
40	Tunisia	2013	1.14	-0.40	0.71	0.08	0.73	-1.66	-6.21	-9.19	-11.13
41	Albania	2014	-2.60	-0.02	0.05	-0.01	-0.60	0.27	-4.77	-8.55	-9.25
42	Ghana	2015	-0.93	-0.07	-0.07	-0.07	-0.06	-0.75	-2.04	-1.84	-1.73
43	Haiti	2015	-2.67	-0.40	-0.27	0.99	0.21	35.84	1.64	2.48	5.30
44	Mozambique	2015	0.27	-0.09	-0.10	-0.10	-0.30	0.76	1.42	-0.06	-0.10
45	Madagascar	2016	-0.21	-0.09	-0.10	-0.09	0.62	0.04	0.62	4.34	4.80
46	Moldova	2016	-2.91	-0.21	0.94	-0.67	-2.18	-5.40	-3.38	-3.72	-4.22
47	Rwanda	2016	2.07	0.12	0.03	0.12	-0.76	-2.04	-1.00	-0.04	-3.44
48	Cameroon	2017	0.37	0.54	-0.10	0.36	0.18	-0.27	-0.72	-0.10	-0.14
49	Mauritania	2017	7.70	7.21	6.89	6.56	4.43	6.56	5.25	7.38	7.54
50	Mongolia	2017	2.51	-0.02	0.31	0.02	-2.41	-5.51	-7.56	-10.42	-7.90
51	Angola	2018	-0.76	-0.07	-0.25	0.08	-1.10	-0.05	1.80	2.36	2.08
52	Armenia	2019	1.57	3.89	-5.60	-3.36	-8.47	-6.59	-11.89	-10.04	
53	Congo	2019	6.46	6.62	5.13	5.96	3.15	3.97	4.14	3.31	

54	Ecuador	2019	1.11	-0.12	-0.19	0.01	-0.72	-3.99	-2.96	-2.98
55	Honduras	2019	-0.49	0.02	0.01	0.02	1.85	0.15	-1.69	0.05
56	Pakistan	2019	-1.09	0.02	-0.36	0.05	-2.91	-3.74	-3.95	-3.49
57	Gambia	2020	-23.47	0.72	0.31	-0.94	-0.49	-0.56	-0.73	
58	Costa Rica	2021	7.70	-0.26	-0.27	-0.25	-0.67	-1.97		
59	Kenya	2021	-0.46	0.09	0.10	0.11	0.45	-0.35		
60	Madagascar	2021	-0.02	-0.26	0.39	0.13	1.63	1.47		

Annex B. Data Sources

Table B.1: Data Sources, Variables, and Codenames

DATABASE	SOURCE	VARIABLES / DATABASE CODENAMES
WDI DATABASE (GENDER INDICATORS)	World Development Indicator (WB 2023a)	Adolescent Fertility Rate o WB: SP.ADO.TFRT Female/Male Labor Force Participation o WB: SL.TLF.CACT.FM.ZS Female Labor Force Participation o WB: SL.TLF.CACT.ZS Maternal Mortality Rate o WB: SH.STA.MMRT Primary Enrollment GPI o WB: SE.ENR.PRIM.FM.ZS Secondary Enrollment GPI o WB: SE.ENR.SECO.FM.ZS Tertiary Enrollment GPI o WB: SE.ENR.TERT.FM.ZS
UNITED NATIONS DEVELOPMENT PROGRAMME	United Nations (2025)	Gender Inequality Index (GII)
WDI DATABASE (CONTROL VARS)	World Development Indicator (WB 2023a)	Total reserves in months of imports o WB: FI.RES.TOTL.MO Total reserves (% of total external debt) o WB: FI.RES.TOTL.DT.ZS Government final consumption expenditure (% of GDP) o WB: NE.CON.GOVT.ZS
IMF LENDING DATABASE	IMF Commitment Database (IMF 2024d)	IMF-supported Programs o https://www.imf.org/external/np/fin/tad/query.aspx
WORLD BANK INCOME CLASSIFICATION DATABASE	Country Classifications by Income Level (WB 2023b)	World Bank Income Classification o http://databank.worldbank.org/data/download/site-content/OGHIST.xls

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