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Women's Political Representation and Fertility: Evidence from Gender Quotas in Sub-Saharan Africa

By Stefanie Pechar, Renate Hartwig and Jan Priebe*

We study how women's political representation affects fertility in sub-Saharan Africa. Leveraging the staggered adoption of reserved-seat gender quotas across the region and a heterogeneity-robust difference-in-differences estimator, we find that quotas increased women's parliamentary representation by 13.3 percentage points and reduced the total fertility rate by 0.23 births per woman. The effect follows a policy-to-behavior pathway: resources shift toward family planning and reproductive health, contraceptive use increases, and unmet need declines; individual-level data further show delayed marriage and later first births. Women's political representation thus translates into greater policy emphasis on reproductive health, illustrating how legislative representation shapes policy outcomes.

Keywords: Family Planning, Female Political Representation, Fertility, Gender Quotas, Reserved Seats, Sub-Saharan Africa.

JEL Classifications: J13, J16, D72, I18, O15.

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I. Introduction

Sub-Saharan Africa (SSA) is in the midst of the world's last major fertility transition. Fertility has fallen substantially over recent decades, from around 6.7 births per woman in the early 1980s to an estimated 4.3 today (United Nations, 2025). Yet the transition is incomplete. Fertility remains the highest of any world region, its decline has been slower, and the pace of change has varied sharply across countries (Bongaarts, 2008; Schoumaker, 2019). Beyond these aggregate patterns, fertility outcomes also reveal a persistent gap between preferences and realized behavior: many women report desired family sizes below their realized fertility (Günther and Harttgen, 2016), and unmet need for contraception is among the highest in the world (United Nations, 2022). Understanding why reproductive preferences translate into different outcomes across contexts therefore requires attention not only to the constraints individuals face in making fertility decisions, but also to the institutions that shape the policies, resources, and services available to them.

An extensive economics literature points to a range of socioeconomic drivers that shape fertility outcomes in SSA and elsewhere, including female education (Kebede et al., 2019; Vogl, 2025), women's labor market participation (Zipfel, 2026; Bandiera et al., 2020; Jensen, 2012), and access to modern contraception (Karra et al., 2022; Ashraf et al., 2014; Joshi and Schultz, 2013; Miller, 2010), among others. While this work documents the micro-behavioral channels and specific policy interventions that influence fertility, far less is known about the political and institutional environment that determines whether such measures are adopted in the first place. Whether and how vigorously they are pursued depends on political priorities and resource allocation, and therefore on who holds political power. This matters because women and men often differ in their fertility preferences: across SSA, women consistently report wanting fewer children than men (Ashraf et al., 2014), and the gap between their stated ideals is the largest of any world region. In contexts where preferences diverge this sharply, the broader institutional setup—particularly the representation of women in political institutions—may critically determine which reproductive priorities are ultimately translated into public policy.

In this paper, we examine to what extent the rise in female political participation at the national level has impacted fertility in SSA. Our analysis builds upon an established literature in economics linking politicians' identities to their preferences over public policies (Besley and Coate, 1997; Burgess et al., 2015; Osborne and Slivinski, 1996; Pande, 2003) and a substantial body of evidence showing that female politicians reallocate public resources towards domains such as health, education, and child welfare that are of concern to women (Anukriti et al., 2026; Beaman et al., 2012; Bhalotra and Clots-Figueras, 2014; Bruce et al., 2022; Bochenkova et al., 2023; Chattopadhyay and Duflo, 2004). We focus on fertility as the primary outcome because it captures the cumulative effects of reproductive-health policies and services across multiple behavioral margins in a domain where men's and women's preferences diverge sharply. We test whether reserved-seat gender quotas, by mechanically raising women's presence in national legislatures, shift policy priorities toward reproductive health and family planning, and whether these shifts generate downstream changes in fertility.

We assembled a longitudinal cross-country dataset ($N = 43$ countries) covering the period

1990–2023, combining annual data on total fertility rates (TFRs) with information on women's parliamentary representation, gender quota legislation, and a range of reproductive-health, demographic, and socioeconomic indicators.

Our identification strategy leverages the staggered adoption of reserved-seat gender quotas across SSA countries. The parameter we identify directly is the average effect of quota adoption on fertility among adopting countries, relative to countries that never adopt a reserved-seat quota. Because reserved seats mechanically allocate parliamentary seats to women, this effect also speaks to the role of representation itself.[1] Quotas provide the discrete variation our design requires: the share of women in parliament has risen gradually across most of SSA, making it difficult to separate the effect of representation from broader co-evolving trends. Reserved-seat quotas, by contrast, generate discrete, country-specific increases in female representation at known dates. Implementing this design, however, requires an estimator that accommodates the staggered timing of adoption across countries. We therefore employ the difference-in-differences estimator of Callaway and Sant'Anna (2021).

Our results show that reserved-seat quota legislation produced a sharp and sustained increase in the share of women in parliament, raising representation by 10–15 percentage points over the post-treatment period. Building on this first-stage effect, we find that quota adoption led to a gradual decline in TFRs, with treatment effects increasing in magnitude over the years following adoption. The average treatment effect on the treated (ATT) across post-treatment periods corresponds to a reduction in the TFR of 0.23 births per woman.

To corroborate our main findings, we conduct a number of robustness and placebo checks. First, the timing of quota adoption appears quasi-random with respect to fertility trends: we find no differential pre-trends between adopting and non-adopting countries, and placebo tests assigning counterfactual adoption dates several years before the reform reproduce no effect. Second, the estimates are not driven by selection into quota adoption (following Bhalotra et al., 2023) or by post-conflict recovery dynamics among countries that adopted quotas after civil conflict. Third, the results are robust to the alternative estimators of de Chaisemartin and D'Haultfoeuille (2020) and Arkhangelsky et al. (2021), an alternative fertility measure, the choice of comparison group, and the exclusion of individual countries. Finally, two findings suggest that the effect operates through representation rather than through the passage of quota legislation itself: the fertility response tends to be larger where quotas mandated a bigger increase in women's representation and a placebo exercise using candidate-list quotas, which regulate party nominations without guaranteeing seats and therefore do little to raise representation, show no effect. Neither test rules out every institutional consequence of adopting a reserved-seat system, but both are difficult to reconcile with an effect running entirely outside the representation channel.

To shed light on the mechanisms behind our findings, we conduct further analyses using survey and administrative data. We begin with the policy environment, the outcomes that fall within legislators' direct sphere of influence. We find that quota adoption led to a targeted resource allocation toward reproductive health: (i) higher development assistance for health

[1]We discuss the exclusion restriction in Section IV in detail.

earmarked for family planning, (ii) stronger universal health coverage for reproductive, maternal, and child health, and (iii) some increase in contraceptive commodity supply. Total aid, overall health aid, and general health coverage are unchanged, indicating selective prioritization rather than a broad expansion or a general donor response to quota adoption. These policy shifts are matched by behavioral and demographic responses. At the country level, we document (i) higher modern contraceptive use, (ii) lower unmet need for family planning, (iii) a higher share of women whose family-planning demand is satisfied, and (iv) lower child mortality. At the individual level, using DHS birth histories, we find that quota exposure is associated with delayed marriage, later first births, and fewer children among younger women. Together these patterns point to a shift in the timing of fertility, concentrated among women exposed during their formative reproductive years. This is consistent with the greater prioritization of reproductive and child health by policymakers and with the cumulative, back-loaded profile of the TFR effect. By contrast, the structural determinants of fertility move in opposite directions: female labor-force participation rises modestly, whereas educational attainment shows no gains and if anything trends negative. Together they provide little evidence of a broad improvement in women's socioeconomic position, making this an unlikely alternative channel.

Our paper contributes to two strands of research. First, we add to the literature on the economic and political determinants of fertility in SSA. Prior work has studied human capital and labor market drivers, including education (Kebede et al., 2019; Vogl, 2025), female labor supply (Zipfel, 2026; Bandiera et al., 2020; Jensen, 2012), and structural transformation (Büttner et al., 2024), as well as institutional and normative drivers such as social norms (Dupas et al., 2025) and property rights and inheritance norms (Donald et al., 2024; Lambert and Rossi, 2016; Fontenay et al., 2025). A related line of work examines how specific policy interventions have shaped fertility, including colonial population policies (Canning et al., 2022; Dupas et al., 2023), social pensions (Rossi and Godard, 2022), tuition-fee abolition (Burlando and Bbaale, 2022), and expansions of contraceptive access (Ashraf et al., 2014; Karra et al., 2022; Dupas et al., 2025). This work takes the policy environment as given. We ask instead where it comes from: how the gender composition of those who govern shapes the priority given to reproductive health and family planning. We show that reserved-seat quotas, and the representation they generate, shift resources toward this domain, and that the fertility decline we estimate is consistent with this reallocation as the operative channel.

Second, we advance the literature on women's political representation and development outcomes. A growing body of (quasi-)experimental evidence from LMICs establishes that female legislators tend to prioritize policy areas that disproportionately affect women. Studies from India document higher public goods provision in health and education (Anukriti et al., 2026; Chattopadhyay and Duflo, 2004; Clots-Figueras, 2012), reductions in child mortality (Bhalotra and Clots-Figueras, 2014), and improvements in girls' schooling (Beaman et al., 2012). Evidence from Brazil points to reductions in violence against women (Bochenkova et al., 2023) and fewer deaths during the Covid-19 pandemic (Bruce et al., 2022). Despite this growing literature, its geographic coverage remains narrow, with most studies concentrated in a small number of contexts. We extend this literature to sub-Saharan Africa, where gender quotas have sharply increased women's representation but their substantive outcomes remain largely unexplored. The region provides a particularly informative setting because reproductive health is both central to development and a policy domain in which men's and women's preferences diverge markedly.

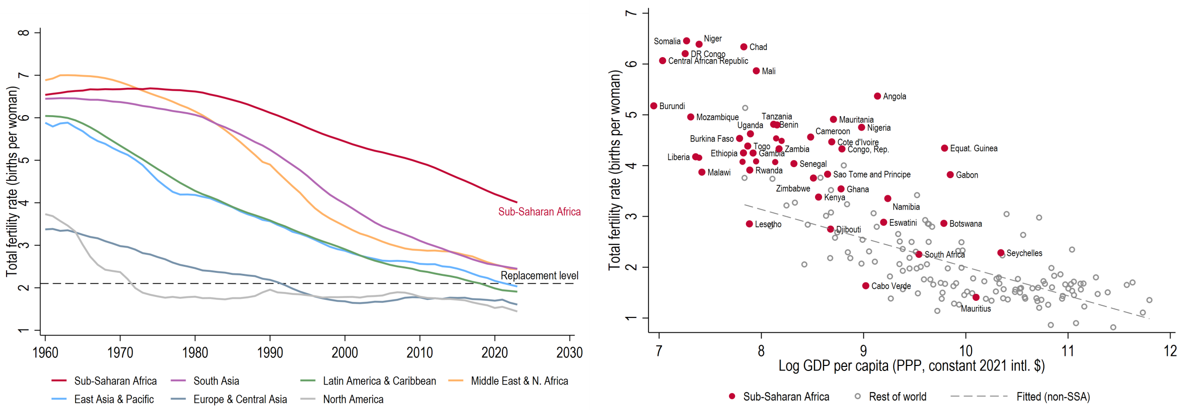
The study closest to ours is that of Bhalotra et al. (2023), who apply the same staggered quota adoption design to a global sample (1990–2015) to study the impact on maternal mortality, where fertility enters only as an input to maternal risk. We study fertility itself. The outcomes are linked, since fewer births mean fewer maternal deaths at any given risk, but they differ in kind: maternal mortality measures survival conditional on childbearing, whereas fertility determines whether, when, and how often childbearing occurs, and thereby shapes women's schooling, labor supply, and economic opportunities over the life course. We also focus on a single region. Sub-Saharan Africa combines the world's highest fertility with the widest adoption of reserved-seat quotas, and its fertility transition is widely treated as distinct rather than as a lagged version of the Asian or Latin American experience (Bongaarts, 2017; Kebede et al., 2019). Restricting attention to this setting keeps the demographic and policy context comparable across treated and control countries, and lets us isolate the specific channels through which representation translates into fertility. Because many adoptions in the region came only after 2010, it also requires a longer panel: ours runs through 2023 and covers ten adoptions, five of them after 2010, leaving enough post-adoption horizon to trace the response as it builds.

The remainder of the paper proceeds as follows. Section II provides institutional and demographic background on fertility and women's political representation in SSA. Section III introduces the data sources and presents descriptive evidence. Section IV describes the empirical strategy. Section V presents the main estimates of the effect of gender quotas on fertility. Section VI summarizes robustness and sensitivity analyses. Section VII examines the policy and behavioral channels underlying the estimated effects. Section VIII complements this analysis with micro-level evidence on individual fertility behavior. Section IX concludes.

II. Background

A. Fertility Patterns in Sub-Saharan Africa

Fertility trends in SSA differ markedly from those observed elsewhere in the world. Panel (a) of Figure 1 plots unconditional TFRs by world region since 1960. While every other region has converged toward or fallen below replacement level, SSA has maintained a TFR well above it, standing at approximately 4.3 births per woman today (United Nations, 2025). While the region's TFR has declined since the early 1980s, the pace of decline has been considerably slower than elsewhere, widening the gap between SSA and the rest of the world over time.



(a) TFR by world region, 1960–2023 (b) TFR and GDP per capita (2023)

Figure 1. Fertility in Sub-Saharan Africa in Global Perspective

Notes: Panel (a) plots the total fertility rate (births per woman) by World Bank world region from 1960 to 2023. The dashed horizontal line marks the replacement level of 2.1. Panel (b) plots the total fertility rate against log GDP per capita (PPP, constant 2021 international dollars) across countries in 2023. Sub-Saharan African countries are shown in red and labeled; all other countries appear as hollow circles. The dashed line is a linear fit estimated on non-sub-Saharan African countries only.

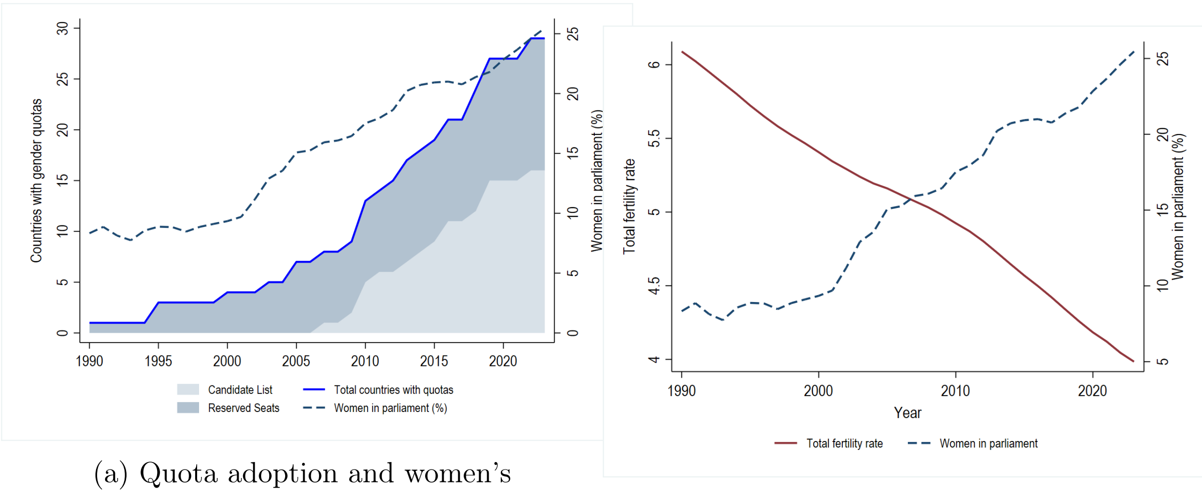
As pointed out by Bongaarts (2017), conditional TFRs are uniquely high in SSA as well. Panel (b) of Figure 1 plots TFRs against log GDP per capita for SSA and non-SSA countries. The fitted line for non-SSA countries indicates the expected income-fertility gradient. With very few exceptions, SSA countries lie above this line, implying that at any given income level their fertility is substantially higher compared to LMICs in other parts of the world.

SSA's distinctiveness is not limited to fertility rates; it extends to access to reproductive healthcare and health technologies, and to preferences over fertility. Regarding the former, we show in Figure A1a that the unmet need for family planning—the share of women who want to avoid or delay pregnancy but are not using an effective contraceptive method—is among the highest in the world in SSA countries. Regarding the latter, it is well established that SSA countries exhibit strong preferences for having children (a high number of wanted children) and the largest absolute gap between men's and women's desired fertility worldwide, with women reporting substantially lower desired fertility than men (see Figure A1b). Moreover, and as discussed by Günther and Harttgen (2016), unwanted births in SSA countries remained at around two per woman over 1985–2011, while falling to almost zero in other regions, suggesting that women in SSA are systematically less able to translate their fertility preferences into birth outcomes. Taken together, these patterns suggest that fertility in SSA has remained high not only because of demand-side factors, but also because women face greater challenges in translating their preferences into realized outcomes.

B. Female Political Representation and Gender Quotas

Over the past three decades, women's parliamentary representation has increased substantially across SSA. This rise has often been attributed to the adoption of electoral gender quotas since the mid-1990s, in response to international commitments such as the Beijing Platform for Action and subsequent African Union initiatives (Pande and Ford, 2012; IPU, 2025). As of 2026, 31 countries in the region have legislated some form of electoral gender quota (Gender Quotas Database, 2025). Our estimation sample covers the 28 adopters observed through 2023 (see Table A1 in the Appendix for details). By 2023, close to half of all SSA countries had a share of women in national parliament above the global average of 26.5 percent, with Rwanda standing out as the global leader at 61.3 percent (see Appendix Figure A3).

This rise in female representation has coincided with a gradual decline in fertility across the region. Figure 2 shows that between 1990 and 2023, the average share of women in parliament rose from below ten to around 25 percent, while the average TFR fell from around 5.8 to 4.3 births per woman. Without claiming causality, we observe a strong negative correlation between women's parliamentary representation and fertility across SSA over this period.



(a) Quota adoption and women's representation

(b) TFR and women's representation

Figure 2. Quota adoption, women's representation, and fertility in SSA

Notes: Panel (a) plots the cumulative number of sub-Saharan African countries that have adopted electoral gender quota legislation by type measured on the left axis together with the average share of women in parliament across the region (dashed line, right axis), from 1990 to 2023. Panel (b) plots the average total fertility rate (solid line, left axis) and the average share of women in parliament (dashed line, right axis) across sub-Saharan African countries, from 1990 to 2023.

III. Data

For our analysis, we construct a country-year panel covering 43 countries in sub-Saharan Africa (main sample) over the period 1990–2023. The dataset combines information on the share of women in national parliaments and the adoption of gender quota legislation with annual data on TFR.

Fertility. Our primary analysis uses population-level fertility, measured by the TFR, which captures the average number of births a woman would have over her lifetime given current age-specific fertility rates (main sample). The TFR provides a comprehensive overview of fertility behavior and enables consistent cross-country comparisons over time. We draw these data from the World Bank's World Development Indicators (WDI). To complement this, we also examine individual-level fertility patterns using retrospective birth histories from the Demographic and Health Surveys (DHS). We assembled all publicly available DHS surveys conducted in sub-Saharan Africa between 2000 and 2023, comprising 85 surveys across 32 countries (sub-sample of countries) and a sample of 410,252 women. Appendix Figure A2 maps the two analysis samples (main sample vs. sub-sample).

Gender Quotas and Women's Political Representation. We gather country-specific information on the adoption of gender quotas in sub-Saharan Africa using the Gender Quotas Database, a collaborative project by the International Institute for Democracy and Electoral Assistance (IDEA), the Inter-Parliamentary Union (IPU), and Stockholm University. This database documents the existence and type of gender quotas in national legislatures. Where adoption years were unavailable, we conduct a country-by-country review using secondary sources to reconstruct the timing of legislation and its first implementation in parliamentary elections. Based on this information, we restrict our analytical sample to countries that legislated quotas after 2000, ensuring a minimum pre-treatment window of ten years—corresponding to at least one and in most cases two electoral cycles—to assess parallel trends. Appendix Table A1 provides an overview of the countries in our sample that legislated gender quotas, including the year of legislation and the type of quota adopted. We complement the quota data with annual measures of women's legislative representation, defined as the percentage of seats held by women in the lower or single house of parliament, sourced from the IPU database.

Other Data. We compiled data on a set of intermediate outcomes and potential quota predictors from various data sources. These include GDP per capita, sexual and reproductive health policies, development assistance and public spending on health and family planning, imports of contraceptive commodities, reproductive health indicators, child mortality, and measures of women's rights and gender equality, among others. Appendix Table A2 provides summary statistics, and Appendix Table A4 lists all variables together with their respective source.

IV. Empirical Strategy

We aim to estimate the effect of reserved-seat gender quotas on fertility, and to assess what that effect implies for the role of women's political representation. A central identification challenge is that the share of women in parliament has increased gradually over time and often in parallel with broader social, economic, and demographic changes, making it difficult to isolate its independent effect on fertility. To address this concern, we exploit the staggered adoption of nationally legislated gender quotas across countries in sub-Saharan Africa. Our identification relies in particular on reserved-seat quotas, which mechanically allocate a share of parliamentary seats to women and thus produce discrete increases in women's representation.[2] Appendix

[2]Gender quotas take two main forms: candidate-list quotas require political parties to nominate a minimum share of female candidates but do not guarantee electoral outcomes; whereas reserved-seat quotas allocate a fixed share of parliamentary seats directly to women. In our main analysis, we focus on the latter, which generate discrete, verifiable increases in representation; candidate list quotas, which generate no comparable increase, serve as a placebo in Section VI.

Figure A4 plots the raw share of women in parliament around each country's quota legislation, illustrating the discrete, country-specific jumps that this variation exploits. The timing of gender quota legislation is arguably orthogonal to short-run changes in fertility and has been used to estimate the effects of women's political representation on other outcomes (Bhalotra et al., 2023). The parameter we identify directly is therefore the average effect of reserved-seat quota adoption on fertility among adopting countries, relative to countries that never adopt a reserved-seat quota. Because reserved seats mechanically raise women's parliamentary presence, we interpret this reduced-form effect as operating through the increase in representation that quotas generate. That interpretation requires that reserved-seat quotas affect fertility only through the seat share they create, and not through other institutional consequences of adopting a reserved-seat system—an assumption we cannot test directly, but which the candidate-list placebo and the quota-bite dose-response in Section VI are designed to probe.

The staggered timing of quota adoption across countries generates variation in both the onset and duration of treatment exposure and introduces the possibility of heterogeneous treatment effects across countries and over time. In such settings, conventional two-way fixed effects (TWFE) estimators can be biased: because already-treated units are implicitly used as comparison units for later-treated units, the estimated coefficients are weighted averages of group- and period-specific effects that may carry negative weights (de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak et al., 2024). We therefore adopt the heterogeneity-robust difference-in-differences estimator of Callaway and Sant'Anna (2021) as our main estimator. Beyond being robust to these biases, it traces the full dynamic path of treatment effects, which is important in our setting, where any response to policy may take time to materialize.

Let G_c denote the year in which country c first legislates a reserved-seat gender quota; countries that never adopt a reserved-seat quota during the sample period have $G_c = \infty$ and constitute the control group.[3] The estimand is the group-time average treatment effect, $ATT(g,t) = \mathbb{E}\big[Y_{ct}(g) - Y_{ct}(\infty) \mid G_c = g\big]$, the average effect in year t for the cohort of countries that adopted a quota in year g, where $Y_{ct}(g)$ and $Y_{ct}(\infty)$ denote potential outcomes under adoption in year g and under never-adoption, respectively. Under the assumptions of parallel trends and no anticipation, $ATT(g,t)$ is identified by comparing the change in outcomes for cohort g, between the baseline year $g-1$ and year t, with the corresponding change among never-treated countries:

$$ATT(g,t) = \mathbb{E}\big[\, Y_{ct} - Y_{c,g-1} \mid G_c = g \,\big] - \mathbb{E}\big[\, Y_{ct} - Y_{c,g-1} \mid G_c = \infty \,\big]. \tag{1}$$

Because already-treated countries are never used as controls, (1) avoids the "forbidden comparisons" that bias TWFE estimators under heterogeneous treatment effects. We use never-treated countries as the comparison group because the sample contains a sufficiently large set of never-adopting countries comparable to the adopters (Callaway and Sant'Anna, 2021), which are

[3]Countries that adopt candidate-list quotas contribute to the control group in the years before their own adoption and are dropped thereafter, so that later candidate-list adoption does not confound the comparison. Section VI reports a sensitivity check restricting the control group to countries that adopted no quota of any type. The year of quota legislation for each treated country is listed in Appendix Table A1.

moreover unaffected by anticipation of their own future adoption. As a sensitivity analysis, we expand the comparison group to also include not-yet-treated countries ($G_c > t$).

To summarize the group-time effects, we aggregate them into dynamic treatment effects. Letting $e = t - g$ index the number of years since adoption, $\theta(e)$ is the cohort-size-weighted average of the group-time effects observed e years after adoption:

$$\theta(e) = \sum_{g} \Pr\left(G_c = g \mid G_c + e \leq T\right) ATT(g, g + e), \tag{2}$$

where T denotes the final year of the panel. For the post-treatment periods ($e \geq 0$), $\theta(e)$ traces the dynamic response of the outcome to quota adoption, with each coefficient measured cumulatively relative to the reference period $e = -1$; the pre-treatment coefficients ($e < 0$) serve as placebo tests of the parallel-trends and no-anticipation assumptions, which we discuss below. Our headline parameter is the average post-treatment effect, $\overline{ATT}$, obtained by averaging the group-time effects $ATT(g, t)$ over all post-treatment periods ($t \geq g$), with each effect weighted by the relative size of its adoption cohort. We estimate these effects first for the share of women in parliament and then for the total fertility rate, and subsequently for the intermediate and placebo outcomes examined in Section VII.

The estimation sample comprises treated countries ($N = 10$), with the remaining never-treated countries ($N = 33$) serving as controls. Standard errors are clustered at the country level, and inference is based on the multiplier bootstrap, implemented as a wild bootstrap with 500 replications.

Parallel Trends and Anticipation. Our identification strategy relies on the parallel trends assumption, which requires that, in the absence of quota adoption, outcome trends in adopting countries would have evolved similarly to those in non-adopting countries. Under this assumption, any post-adoption divergence in outcomes between the two groups can be attributed to the causal effect of increased female political representation. We assess its plausibility by examining the pre-treatment coefficients of the event study, reported under both a varying and a universal base period.[4] Because $e = -1$ is the reference period, the no-anticipation assumption further requires no divergence between treatment and control countries in the years immediately before adoption. To probe both, we conduct a placebo test that re-estimates the event study after shifting each treated country's adoption date backward by several years, using only its genuinely pre-treatment observations, and examine whether systematic dynamics emerge around the placebo date.

Selection and Confounding. Following Bhalotra et al. (2023), we test whether quota adoption is systematically preceded by changes across a range of gender-equality indicators. We

[4]By default, the `csdid` implementation of the Callaway and Sant'Anna (2021) estimator adopts a varying base period, in which each pre-treatment coefficient is measured relative to the immediately preceding period. The universal base period, in which all pre-treatment coefficients are instead measured relative to the single reference period $e = -1$, is obtained with the `long2` option. Because the default varying base period can make pre-trend violations harder to detect, we also report the universal-base coefficients (Roth, 2026).

further examine whether the results are driven by post-conflict recovery dynamics among the subset of countries that adopted quotas following civil conflict.

Alternative Estimators, Samples, and Measurement. We assess the robustness of our estimates to the choice of estimator, sample, and outcome measure. We re-estimate the dynamic effects using a standard event-study specification, an alternative heterogeneity-robust estimator of de Chaisemartin and D'Haultfoeuille (2020) and the synthetic difference-in-differences estimator of Arkhangelsky et al. (2021), which relaxes the strict parallel-trends requirement by matching pre-treatment trajectories. We further examine sensitivity to a balanced event-study window that excludes post-2013 adopters, to alternative comparison groups (not-yet-treated and never-any-quota controls), to the exclusion of any single treated country in leave-one-out analyses, and to measuring fertility with the crude birth rate rather than the TFR.

Adoption vs. Representation. Finally, we examine whether the fertility response is driven by the rise in women's representation, rather than by the passage of quota legislation itself. We exploit variation in the strength of the mandated shock—the quota "bite"—to test whether larger increases in representation are associated with larger fertility responses. We also implement a placebo using candidate-list quotas, which share the legislative form of reserved-seat quotas but do not generate discrete increases in representation.

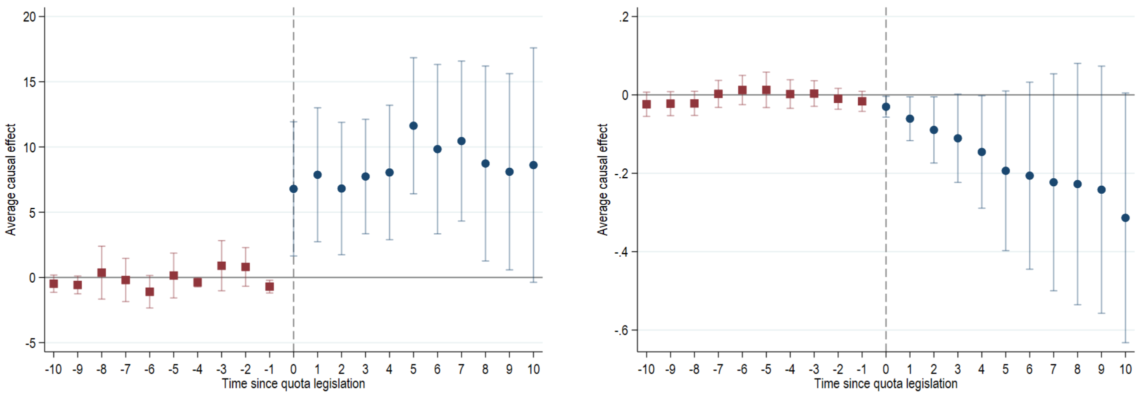
V. Main Results

Figure 3 presents dynamic treatment effects and placebo estimates of the effect of reserved-seat quota legislation on the share of women in parliament and TFR following Callaway and Sant'Anna (2021). Panel (a) shows a discrete jump in the share of women in parliament in the year following reserved-seat quota legislation, indicating a statistically significant increase in female parliamentary representation post-adoption. We estimate an increase in the proportion of women in parliament ranging from 10 to 15 percentage points (pp) across the post-treatment years, with an average treatment effect of 13.3 percentage points for countries that eventually adopted a quota. This effect is statistically significant at the 5% level.

Panel (b) presents the corresponding dynamic effects on TFR. In contrast to the first stage, TFR shows no discrete change immediately after adoption. Instead, the treatment coefficients decline gradually in the years following quota adoption. The average treatment effect across post-treatment periods corresponds to a reduction in the TFR of 0.23 births per woman, statistically significant at the 10% level.[5] The magnitude of the effect grows steadily over the post-treatment decade and is largest ten years after adoption. This gradual pattern is consistent with the time required for policy and behavioral responses to translate into population-level fertility effects. The confidence intervals widen at longer horizons partly because fewer cohorts are observed there, but largely for a mechanical reason: each coefficient is a cumulative difference relative to the baseline period $g-1$, so its sampling variance increases with the horizon.[6]

[5]Because the estimate is identified from only ten treated countries, its precision is necessarily limited. We examine its robustness to the number and composition of treated countries in Section VI.

[6]Holding the set of contributing cohorts fixed—a balanced sample of cohorts adopting between 2000



(a) Women in parliament (%)

(b) Total fertility rate

Figure 3. Reserved seats quota impacts: Dynamic effects and placebos

Notes: Panels (a) and (b) present estimates of the impact of quotas on the share of women in parliament and the total fertility rate following Callaway and Sant'Anna (2021). The plots present 10 pre-trend estimates and 10 dynamic effects. Pre-trend estimates compare treatment and control countries in pre-quota periods when countries that eventually adopted a quota had not yet changed their treatment status. The dynamic effects show impacts between $g+e$ and $g-1$ $\forall$ $e \in \{0, 1, ..., 10\}$ using never-treated countries as controls. The event-study figures report pointwise 95% confidence intervals.

Across all panels of Figure 3, the pre-treatment coefficients function as placebo estimates and lie close to zero, consistent with parallel trends and no anticipation between treated and control countries prior to quota adoption. For TFR, this conclusion is reinforced under a universal base period, which captures cumulative pre-trends (Appendix Figure A6), and by a placebo test that shifts each treated country's adoption date backward and finds no systematic dynamics around the false adoption date (Appendix Figure A7).[7] If the fertility decline simply reflected pre-existing trends rather than the effect of increased representation, we would expect the same downward pattern to appear at these placebo dates, where no quota has yet been adopted. Instead, the placebo event studies show no pattern resembling our estimated effects. For women's representation, by contrast, the placebo reveals a mild negative pre-trend at the longer shifts, consistent with quotas being adopted where female representation had been lagging. Because representation is the treatment channel rather than the outcome underlying our causal claim, this pattern does not threaten identification; if anything, it renders the first-stage estimate conservative. We probe the robustness of our results further in the next section.

VI. Robustness and Sensitivity

This section shows that our main results are robust across a range of checks. We first address the central identification concerns: selection into quota adoption and post-conflict confounding. We

and 2013, each observed over the full ten-year window—the intervals widen in the same way (Appendix Figure A5).

[7]For each treated country we set the placebo adoption year to k years before the true year of legislation ($k = 3, 5, 10$), drop all observations from the true adoption year onward, and re-estimate the event study; never-treated countries remain the comparison group. Treated countries that lack a sufficient pre-treatment window for a given shift are excluded from that shift, so the number of treated cohorts declines as the shift lengthens.

find no evidence that either drives the estimates. We then confirm that the results do not depend on the estimator, the sample, or the measure of fertility. Finally, we tie the fertility response to representation itself: the effect scales with the size of the quota-induced representation shock and vanishes for candidate-list quotas that generate no such shock.

Selection and Confounding. The central identification concern is that quota adoption reflects a gradually evolving shift toward gender equality or broader socioeconomic development that could independently affect fertility. We test whether adoption is preceded by systematic changes in a range of potential confounders: dimensions of women's rights and status from the Varieties of Democracy (V-Dem) dataset, including freedom of discussion, access to justice and property rights, civil liberties, and civil society participation, alongside broader socioeconomic conditions, namely GDP per capita, female labor force participation, and female education. The estimates are presented in Figure A8. Across all indicators, we find no evidence of pre-trends, suggesting that quota adoption is not preceded by broader institutional or economic changes that could confound our main estimates. This is also conceptually plausible: gradual gains in gender equality and socioeconomic development affect reproductive outcomes only slowly, whereas quotas generate a discrete, well-timed change in women's political authority, providing a more direct channel through which policy priorities can shift.

A related concern is that several countries adopted quotas amid post-conflict or post-crisis constitutional reforms, which could affect fertility through replacement fertility, health-infrastructure rebuilding, or aid inflows. Among our ten treated countries, five adopted quotas within a decade of major conflict or crisis (Rwanda, Burundi, Kenya, Zimbabwe, Niger); the remainder had no conflict history or long gaps between conflict and adoption. We probe this in two ways: (a) excluding all five post-conflict/crisis adopters; and (b) controlling for years since the end of conflict, thereby absorbing post-conflict recovery dynamics directly while retaining the full sample. In both, the results are broadly consistent with the baseline (Appendix Figure A9): the first-stage effect remains robust, and the TFR estimates roughly retain the magnitude and temporal profile of our main estimates, though less precisely. The loss of precision is most pronounced in panel (b). Several treated countries adopted quotas soon after conflict, so years since conflict and time since adoption move closely together; controlling for the former absorbs variation that also identifies the treatment effect, widening the confidence intervals. Two features reinforce this. First, post-conflict recovery would, if anything, predict a temporary fertility *increase* through replacement fertility, not the sustained decline we observe. Second, the effect operates through a targeted increase in family-planning and reproductive-health resources, not the broad rise in aid and public spending that would accompany post-conflict recovery (Section VII).

A further concern is compositional: by lowering maternal mortality, quotas could change which women survive through their reproductive years, and thus measured fertility. This is unlikely to drive our results and, if anything, works against them—fewer maternal deaths keep more women in the reproductive population, pushing measured fertility *up*, not down. In any case, we find no significant effect on maternal mortality in our SSA sample (Appendix Figure A10).

Alternative Estimators, Samples, and Measurement. Beyond these identification checks, our estimates are also robust to the choice of estimator. We first implement the synthetic difference-in-differences (SDID) estimator of Arkhangelsky et al. (2021), which constructs a weighted combination of control countries to match each treated country's pre-adoption trajectory in representation and fertility. By matching pre-treatment paths it relaxes the strict parallel-trends requirement, and its placebo-based inference remains reliable when the number of treated units is small—a useful property in our setting. The SDID estimates are close to our main results: a 13.25 percentage points increase in representation and a 0.32-birth reduction in the TFR. We further benchmark against the heterogeneity-robust estimator of de Chaisemartin and D'Haultfoeuille (2020), which reproduces the same sign and back-loaded time profile as our main estimates. Event-study plots for both alternative estimators are presented in Figure A11. Table 1 reports the aggregate effect from each estimator, including a static two-way fixed-effects (TWFE) benchmark; all agree on the sign and come close to our preferred estimates in magnitude. The de Chaisemartin and D'Haultfoeuille (2020) estimates (dCDH) and static TWFE aggregates are somewhat smaller because both weight the early post-adoption years—where the back-loaded effect has not yet fully materialized—more heavily than our preferred specification. That even the naive TWFE benchmark yields a similar aggregate indicates the result is not an artifact of our estimator choice; the heterogeneity-robust estimators are nonetheless preferable for recovering the back-loaded dynamic profile that a single-coefficient specification cannot reveal. We further verify that the results are not an artifact of dating treatment at legislation: re-anchoring each country's treatment to the first parliamentary election held under the quota yields a larger first stage and a dynamic TFR profile of the same shape and magnitude (Appendix Figure A14). Finally, the results hold when fertility is measured by the crude birth rate rather than the TFR (Appendix Figure A15).

The estimates are equally insensitive to sample composition. We (i) restrict the sample to a balanced ten-year window by excluding post-2013 adopters, (ii) use not-yet-treated rather than never-treated controls, and (iii) restrict controls to countries that adopted no quota of any kind. Across all three specifications, the estimates are largely unchanged (Appendix Figure A16). Leave-one-out analyses that sequentially drop each treated country preserve the negative TFR effect throughout (Appendix Figures A17 and A18). The one influential case is Rwanda, which combines the largest representation increase with one of the steepest fertility declines. Excluding it attenuates the effect (representation below 10 pp, TFR below 0.2 births) but does not overturn it.

Adoption vs. Representation. A natural test of whether the fertility response operates through representation is whether it scales with the strength of the representation shock the quota delivers. We first confirm that the first stage is not cosmetic: re-estimating the effect on women's parliamentary representation separately for subgroups of treated countries, split at the median of pre-adoption representation and of the quota "bite", we find that representation rises in *every* subgroup following adoption (Appendix Figure A19). The quotas move women into parliament wherever they are adopted, not only in a favorable subset of countries. We

Table 1—Effects of reserved-seat quotas across alternative estimators

	(1) Callaway & Sant'Anna	(2) Synthetic DiD	(3) de Chaisemartin & D'Haultfœuille	(4) TWFE
Outcomes				
Women in parliament (pp)	13.29**	13.25***	12.06**	8.93*
	(6.221)	(3.182)	(6.194)	(4.434)
Total fertility rate	-0.23*	-0.32*	-0.14	-0.22
	(0.127)	(0.169)	(0.102)	(0.184)
Countries treated	10	10	10	10

Notes: The table reports the average treatment effect of reserved-seat quota adoption on the share of parliamentary seats held by women and on the total fertility rate, estimated with four different estimators. Column (1) our preferred specification, uses the estimator of Callaway and Sant'Anna (2021); column (2) the synthetic difference-in-differences estimator of Arkhangelsky et al. (2021); column (3) the estimator of de Chaisemartin and D'Haultfoeuille (2020), for which we report the average total effect across post-treatment horizons; and column (4) a static two-way fixed effects specification. Each estimator aggregates the dynamic effects using its own weighting scheme. Standard errors are clustered at the country level in columns (1), (3), and (4), and obtained from the placebo procedure of Arkhangelsky et al. (2021) in column (2).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

then exploit the intensive margin directly. For each treated country we define the quota *bite* as the legislated target minus the country's own average representation in the five years before adoption, and plot each country's ATT on the TFR against its bite (Figure A20). Consistent with a representation channel, countries facing a larger bite exhibit larger fertility declines: the relationship is negative across the full set of treated countries. We report this pattern transparently rather than as a headline result. With only ten treated countries, the gradient is sensitive to influential cases. Excluding Rwanda flattens the fit substantially (dashed line). Beyond Rwanda, the country panel is simply too thin to sustain precise heterogeneity statements. We therefore read the bite relationship as corroborating the direction of the mechanism rather than as a precise dose-response estimate.

A candidate-list placebo provides a cleaner test of the same idea. Candidate-list quotas regulate party nominations but do not guarantee seats, and typically generate little discernible increase in representation (Pande and Ford, 2012). If our estimates reflected a spurious correlation between quota legislation and fertility rather than a genuine representation effect, these quotas should still move fertility; they do not (Figure A20). Because they share the legislative form of reserved-seat quotas but not their first-stage effect, this null points to the increase in representation—not the passage of legislation—as the operative channel, consistent with the (suggestive) bite gradient among reserved-seat adopters.

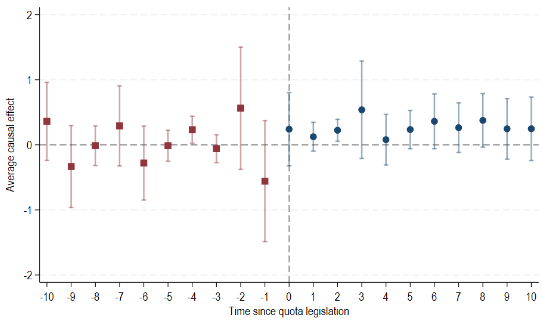
VII. Mechanisms

The results presented in the previous sections show that gender quotas significantly increased women's representation in national parliaments and led to a reduction in fertility at the population level. This raises two central questions, which we take in causal order. First, which changes in the allocation of public resources and policy follow quota adoption? Second, how do these shifts relate to the demographic and behavioral changes that underlie the observed decline in fertility?

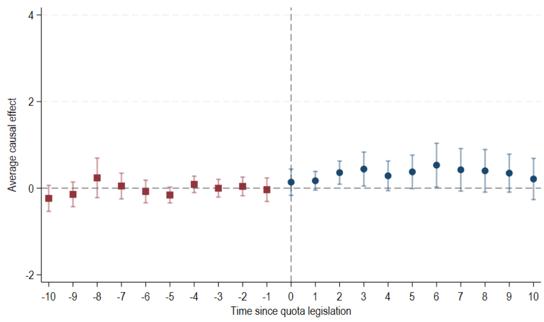
A. Political and Policy Channels

Resource Allocation. We begin with the policy environment, the outcomes within legislators' direct sphere of influence. If increased female representation shifts policy priorities, such changes should be observable first in the financing and provision of reproductive-health services. On the supply side, improvements in reproductive outcomes depend on adequate financing, the effective coverage of family-planning programs, and the availability of contraceptive commodities. We examine each in turn, in every case distinguishing *targeted* prioritization of fertility-relevant programs from a *broad* expansion of health-sector resources.

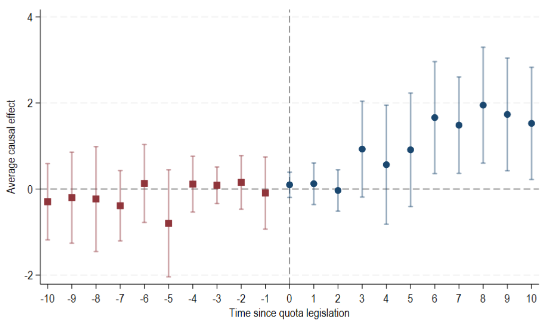
Concerning financing, a natural concern is that quota adoption does not itself reallocate resources but instead coincides with greater donor support, for instance if adopting countries are rewarded with aid conditioned on governance or demographic targets. Figure 4 separates this general-conditionality channel from targeted reallocation. Total official development assistance (ODA) shows no systematic response to adoption (Panel a), and overall development assistance for health (DAH) is likewise flat (panel b): neither broad aid nor broad health aid rises after countries adopt quotas, and the coefficients remain statistically indistinguishable from pre-treatment levels throughout the event window. The response is confined to the family-planning component of DAH (panel c), which turns positive after adoption with a slight delay and rises over the post-treatment window, on the same back-loaded horizon as the fertility effect itself. This sectoral concentration is precisely what a representation-driven reprioritization predicts: women legislators steering resources toward reproductive health. It is inconsistent with a general conditionality reward, which would lift aid broadly rather than in the single sector aligned with the policy priority.



(a) Total ODA



(b) Development assistance for health (DAH)

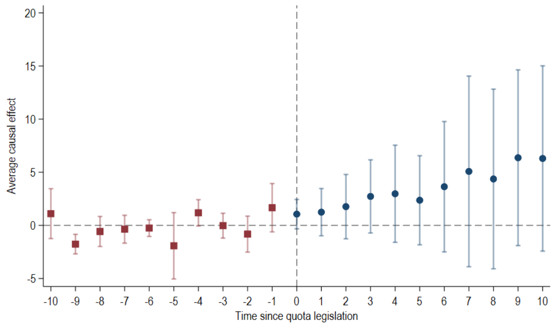


(c) DAH for family planning

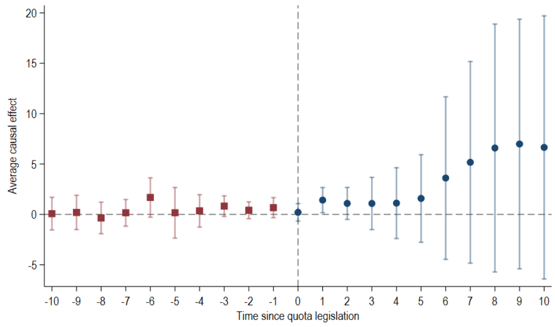
FIGURE 4. RESOURCE ALLOCATION: DEVELOPMENT ASSISTANCE

Notes: The figure presents event-study estimates of the effect of reserved-seat gender quotas on supply-side mechanisms, following Callaway and Sant'Anna (2021), with never-treated countries as controls. Panel (a) shows total ODA, panel (b) total development assistance for health (DAH), and panel (c) shows the family-planning component of DAH. All other estimation details correspond to those reported in Figure 3.

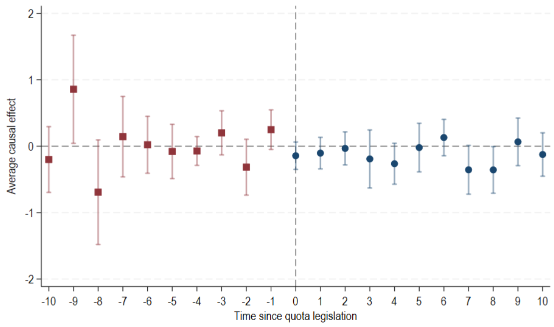
Service Coverage and Commodity Supply. Prioritization should manifest not only in financing but also in the coverage of services and the flow of commodities. Figure 5 presents dynamic effects for four such indicators. Aggregate universal health coverage shows no systematic post-treatment change (Panel a), whereas the coverage index specific to reproductive, maternal, and child health rises following adoption (panel b). Together, these patterns again indicate targeting of reproductive services rather than a broad expansion of the health system.



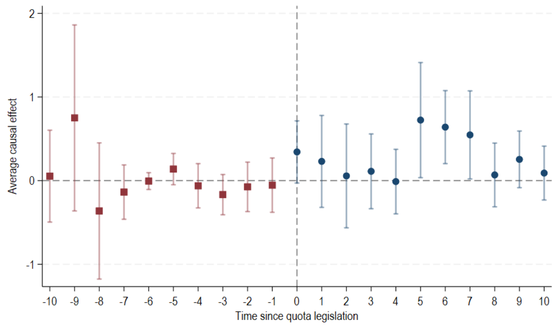
(a) Universal Health Coverage (UHC)



(b) UHC for reproductive and child health



(c) Imports condoms



(d) Imports hormonal contraception

Figure 5. Supply-side mechanisms: Targeted reproductive health provision

Notes: The figure presents event-study estimates of the effect of reserved-seat gender quotas on supply-side mechanisms, following Callaway and Sant'Anna (2021), with never-treated countries as controls. Panels (a) and (b) report effects on universal health coverage indices; panels (c) and (d) on import values of condoms and hormonal contraceptive commodities. All other estimation details correspond to those reported in Figure 3.

The trade data provide complementary, though inherently noisier, evidence on commodity supply. Condom imports show no discernible post-treatment change (panel c), whereas hormonal contraceptive imports rise roughly five years after adoption, with a pronounced mid-period spike and gradual tapering thereafter (panel d). This discrete pattern is consistent with the institutional features of international contraceptive supply chains, where multi-year budgeting cycles, centralized tenders, and periodic bulk shipments tend to produce lumpy rather than smooth increases in recorded imports. The differential across methods may also reflect that family-planning programs often prioritize female-controlled methods such as hormonal contraceptives for expanding access, whereas condom procurement is tied more closely to HIV and STI prevention than to family-planning priorities. While the trade evidence is noisy and the timing subject to uncertainty, the direction is consistent with an increase in contraceptive commodity supply following quota adoption.

Legislative Orientation. To probe the political channel more directly, we examine the cross-sectional relationship between women's parliamentary representation and country-level reproductive health policy environments, using data from the Global Contraception Policy Atlas. Figure 6 reports this relationship for two composite indices: the Contraception Access Index, summarizing the policy and service environment for women seeking contraception, and the Political Performance and Leadership Index, summarizing government commitment to sexual and reproductive health and rights. Across SSA countries, both indices are positively associated with the share of women in parliament. The pattern reflects both international policy commitments and domestic access arrangements: countries with greater female parliamentary representation are more likely to have made and tracked international family planning commitments, to partner with global financing facilities, and to back contraceptive access through stock-out monitoring systems, government funding for contraception, and coverage under national health insurance schemes (see Appendix Figure A21 for results at the individual component-level). While this descriptive evidence is necessarily cross-sectional and cannot establish causality, it is consistent with our event-study results.

B. Behavioral and Demographic Response

Having shown that quota adoption reallocated resources toward family planning and reproductive health, we now examine whether it also produced measurable behavioral and demographic responses. A decline in fertility ultimately reflects changes in reproductive behavior and child survival, as well as potential shifts in women's socioeconomic status. We therefore examine whether gender quotas affect intermediate outcomes that the literature identifies as central to fertility transitions (see, e.g., Bongaarts and Hodgson, 2022; Vogl, 2025; Zipfel, 2026; Büttner et al., 2024). We distinguish between proximate reproductive and child-health outcomes—where political attention can translate relatively quickly into behavioral change—and structural determinants such as female education and labor-market participation, which depend on longer-horizon, more complex reforms and which we take up separately below.

Figure 7 presents the event-study estimates for the proximate outcomes. The use of modern contraception increases significantly following quota adoption, with an average treatment effect of 6.2 percentage points, rising to approximately 11 percentage points by year ten (Panel a). Consistent with this, unmet need for contraception falls by 2.5 percentage points (Panel b), and the share of demand for family planning satisfied by modern methods—the summary measure combining the two—rises by 2.6 percentage points (Panel c).

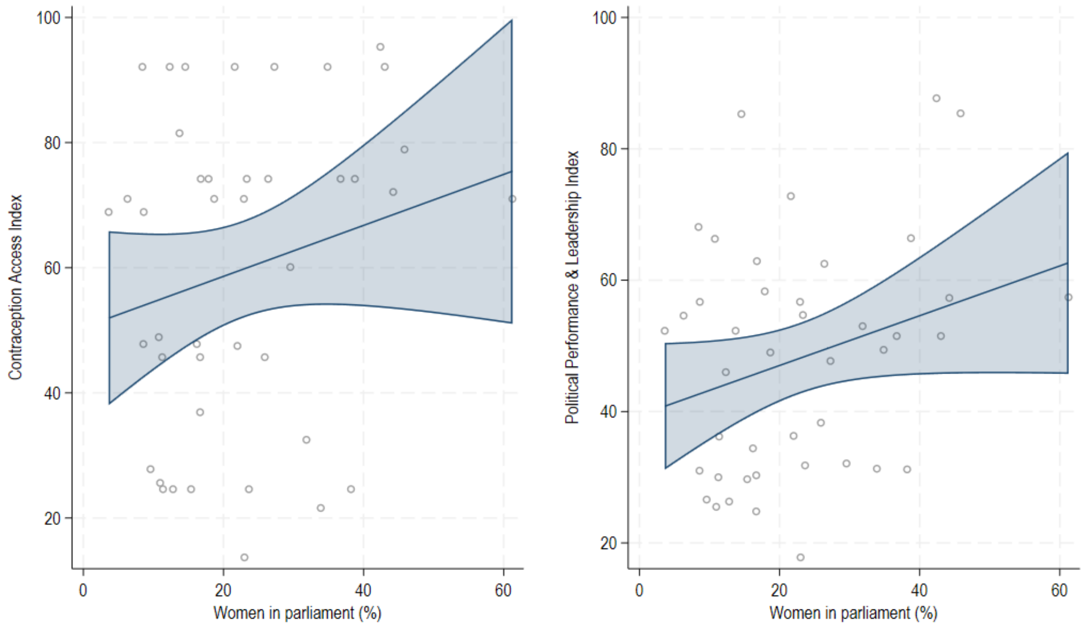
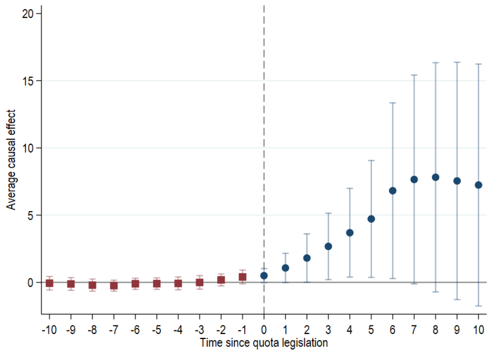


Figure 6. Women's representation and the SRH policy environment

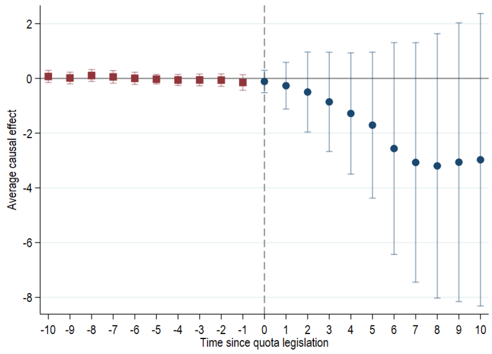
Notes: The figure plots two composite indices from the Global Contraception Policy Atlas against the share of women in parliament across SSA countries. Panel (a) reports the Contraception Access Index; Panel (b) reports the Political Performance and Leadership Index. Both indices are scored on a 0–100 scale, with higher values indicating a more supportive policy environment. Solid lines are linear fits with 95% confidence intervals shaded; each circle represents one country. Appendix Figure A21 reports component-level results for the individual policy indicators underlying the indices.

Child mortality also declines following quota adoption (-0.21 log points, roughly a 19 percent reduction; Panel d), consistent with the child-survival channel. Because fertility and child mortality are jointly determined, we read this as reinforcing rather than independent of the fertility response: fewer and better-spaced births lower mortality risk, while lower child mortality in turn weakens parents' incentive for precautionary or "insurance" fertility (Kalemli-Ozcan, 2002; Doepke, 2005).

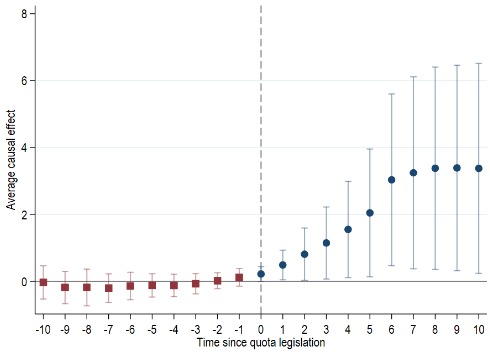
Figure 8 reports the structural determinants. If quotas primarily operated through broad improvements in women's socioeconomic position, we would expect consistent positive shifts in both education and employment. We do not. Female educational attainment trends slightly negative over the post-treatment decade, reaching about -0.5 years by year ten—counter to the predicted direction, though imprecise and with confidence intervals spanning zero (Panel a). Female labor-force participation is positive and turns significant at intermediate horizons



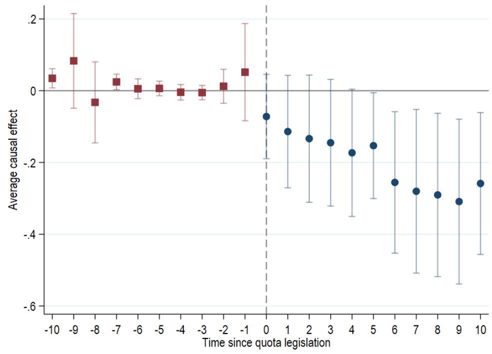
(a) Use of modern contraception



(b) Unmet need for contraception



(c) Demand for FP satisfied

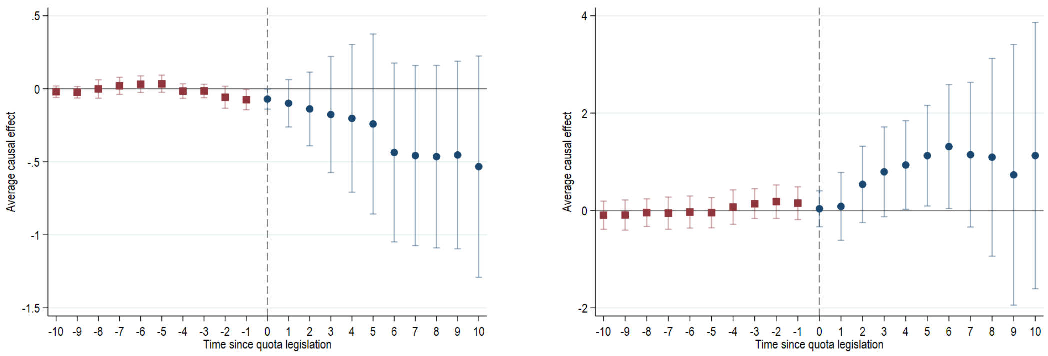


(d) Child mortality (log)

FIGURE 7. EFFECTS ON REPRODUCTIVE, MATERNAL AND CHILD HEALTH OUTCOMES
Notes: The figure presents event-study estimates of the effect of reserved-seat gender quotas on intermediate outcomes related to reproductive, maternal, and child health, following Callaway and Sant'Anna (2021), with never-treated countries as controls. Panels (a), (b), and (c) are expressed in percentage points. Panel (d) reports the natural logarithm of child mortality, so coefficients approximate proportional changes. Variable definitions and sources are listed in Appendix Table A4. All other estimation details correspond to those reported in Figure 3.

(roughly four to six years after adoption) before the intervals widen again (Panel b); it moves in the expected direction, but only modestly. This points to the reproductive-health and policy channels documented above as the primary mechanism, with at most a secondary contribution from a rising opportunity cost of childbearing. It also fits the political economy of reform in resource-constrained settings: contraceptive and child-health services can be financed through targeted sectoral spending within existing health budgets, whereas meaningful change in education or labor-market structures requires complementary governance and institutional reforms that targeted policy levers alone cannot deliver (Glewwe and Muralidharan, 2016; Pritchett et al., 2013). Finally, because fertility and women's socioeconomic outcomes are jointly determined, any structural effects may materialize only over horizons longer than our data capture.

Across all mechanism outcomes we find no evidence of differential pre-trends. The effects that plausibly account for the fertility decline are concentrated in the reproductive-health and policy channels: contraceptive uptake and reduced unmet need emerge within the first few post-treatment years, while the child-mortality decline accumulates over the decade. We find no detectable effect on the structural determinants of fertility. The overall pattern is consistent with a gradual, policy-driven channel rather than a broad transformation of women's socioeconomic position: enhanced female representation triggers a coordinated reproductive- and child-health policy response.



(a) Years of schooling (females)

(b) Female labor market participation

FIGURE 8. EFFECTS ON FEMALE EDUCATION AND LABOR MARKET PARTICIPATION

Notes: The figure presents event-study estimates of the effect of reserved-seat gender quotas on structural determinants of fertility, following Callaway and Sant'Anna (2021), with never-treated countries as controls. Panel (a) reports effects on female educational attainment and Panel (b) on female labor force participation. All other estimation details correspond to those reported in Figure 3.

VIII. MICRO-LEVEL EVIDENCE ON FERTILITY BEHAVIOR

The country-level evidence points to a behavioral response (rising contraceptive use and falling unmet need) but cannot reveal which women adjust their fertility, or how. We now turn to individual-level DHS data, from which we construct a woman-year panel linked to the timing of quota adoption. To isolate women whose reproductive decisions could plausibly have responded to quotas, we focus on those aged 12–25 at the time of adoption, their formative reproductive years. We interpret these estimates as descriptive of cohort-level fertility patterns among women differentially exposed to quota legislation rather than as causal effects, given the limitations inherent in retrospective panel designs.

Descriptive Evidence. Before turning to regression estimates, we present descriptive evidence on marriage and fertility timing across cohorts differentially exposed to quota legislation. Figure A22 presents Kaplan–Meier survival curves for the transition to marriage and first birth, separately for women exposed and unexposed to reserved-seat quota legislation during their formative reproductive years. The curves reveal a consistent delay pattern: women in quota-exposed countries exhibit a lower probability of having transitioned into marriage and first birth by any given age, with the separation between the two groups most pronounced during the peak years of early marriage and fertility—roughly between ages 15 and 22. A log-rank test rejects equality of the survivor functions across exposure groups. Notably, the curves converge at later ages, suggesting that the difference operates primarily through a delay in the onset of these transitions rather than a permanent reduction in the probability of ever marrying or having children. These descriptive patterns motivate the regression analysis that follows.

Retrospective Panel Estimates. We estimate the effect of exposure to gender quota legislation on women's cumulative number of children using a two-way fixed effects model. The baseline specification is:

$$Y_{i,c,k,t} = \beta \text{Quota}_{c,k,t-5} + \alpha_t + \gamma_c + \delta_k + \epsilon_{i,c,k,t}, \tag{3}$$

where $Y_{i,c,k,t}$ is the cumulative number of children of woman i, from country c, born in cohort k, observed at age t. $Quota_{c,k,t-5}$ is a binary indicator equal to one if a gender quota was introduced in country c at least five years before the woman is observed at age t, and zero otherwise. α_t, γ_c and δ_k are fixed effects for age at observation, country, and birth cohort, respectively. Standard errors are clustered at the country level. The five-year lag reflects the expectation that cumulative fertility adjusts only gradually after the policy change.

To capture heterogeneity in treatment effects, we estimate two sets of interactions. First, we interact quota exposure with a woman's age at the time of observation, which allows us to trace differential responses across the reproductive life cycle. Second, and more central to our interpretation, we interact quota exposure with women's age at the time of quota legislation, thereby distinguishing between younger women who had not yet begun childbearing and women whose fertility trajectories were already partly determined when the policy was introduced.

Table 2 presents the results. Columns (1) and (2) show no significant average effect of quota exposure on cumulative fertility when averaging across all ages and cohorts. Columns (3) and (4) introduce the interaction with age at observation. The negative reserved-seats coefficient indicates that exposed women have fewer children at younger ages, consistent with the delayed onset of childbearing in the survival curves (Figure A22). The positive age interaction, in turn, shows this gap narrowing as they grow older. The effect is thus largest at young ages and diminishes over the reproductive life cycle, mirroring the convergence of the Kaplan–Meier curves. This is consistent with delayed first births, but these estimates cannot distinguish genuine catch-up—women reaching the same completed fertility, only later—from a permanent reduction that merely appears smaller at older ages because older women had less reproductive time remaining when the policy arrived.

Columns (5) and (6) speak more directly to this by interacting exposure with a woman's age at quota legislation rather than at observation. The negative reserved-seat coefficient now captures the reduction among women who were youngest when the legislation passed—those at the very start of their reproductive lives, for whom the survival curves show the sharpest separation. The positive interaction indicates that the effect attenuates for women who were older at adoption, whose trajectories were largely determined before the policy change. This is our cleanest micro-level finding: the fertility-reducing effect is concentrated among women for whom the quota arrived early enough to shape their reproductive decisions.

Table 2—Retrospective panel: Quota impact on number of children

	(1)	(2)	(3)	(4)	(5)	(6)
Reserved seats	-0.0894	-0.0662	-0.767**	-0.684**	-0.0527	-0.0537
	(0.175)	(0.177)	(0.305)	(0.289)	(0.705)	(0.685)
Reserved x Age			0.0257**	0.0257**		
			(0.0124)	(0.0128)	(0.0124)	
Reserved x Age legislation					0.114**	0.114**
					(0.0326)	(0.0318)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year FE	No	Yes	No	Yes	No	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
N	8294264	8294264	8294264	8294264	4306229	4306229
Adj. R^2	0.535	0.536	0.523	0.525	0.143	0.150

Notes: This table reports estimates obtained from a two-way fixed effects specification following Equation 3. The analysis used retrospective panel data constructed from DHS birth histories. Standard errors are clustered at the country level. *** p < 0.01, ** p < 0.05, * p < 0.10.

Cross-sectional Corroborating Evidence. To further confirm the delay pattern, we complement the individual-level analysis with cross-sectional evidence on early fertility and marriage outcomes. We estimate linear probability models for three outcomes, namely being married before age 18, having a first child before age 18, and the cumulative number of children by age 25, as a function of country-level quota exposure, controlling for country and birth cohort fixed effects.

The results, presented in Table A3, are broadly consistent with the retrospective panel estimates. Women in countries exposed to quota legislation are significantly less likely to have a first child before age 18 and have significantly fewer children by age 25. The probability of marriage before age 18 also points in the same direction but is not statistically significant. While these cross-sectional estimates are descriptive and cannot fully account for time-varying country-level confounders, the pattern corroborates the delay in childbearing identified in the panel analysis. The stronger and more consistent effects on fertility outcomes relative to marriage timing suggest that quota-induced policy changes operate primarily by expanding access to contraception and family planning services, rather than by shifting marriage markets or social norms. This interpretation is consistent with the mechanism evidence presented in Section VII, where we find significant effects on contraceptive prevalence and unmet need but no detectable impact on broader structural outcomes. Taken together, the micro-level evidence paints a consistent picture across the three complementary analyses outlined above.

IX. Conclusion

This paper examines whether reserved-seat gender quotas, and the increase in women's parliamentary representation they produce, affect fertility in sub-Saharan Africa. Exploiting the staggered adoption of reserved-seat gender quota legislation across the region since 2000, we find that quota adoption produces a sharp and sustained rise in women's parliamentary representation of 13.3 percentage points, and a decline in the TFR of 0.23 births per woman on average. This decline in fertility does not appear immediately: it is small in the first years after adoption and grows steadily, reaching its full magnitude toward the end of the post-adoption decade. This timing is consistent with the years required for policies to be implemented, for reproductive behavior to adjust, and for those adjustments to accumulate into measurable changes in population-level fertility.

The mechanisms we identify follow a policy-to-behavior chain. At the policy level, quota adoption led to a targeted reallocation toward reproductive health: development assistance earmarked for family planning, reproductive-health service coverage, and contraceptive commodity imports all rise. The broader aggregates (total development assistance, overall health aid, and aggregate health coverage) remain largely unchanged. This sectoral concentration indicates reprioritization within the reproductive-health domain rather than a broad upgrading of public services or a general donor response to quota adoption. Consistent with these policy shifts, quota adoption also raises modern contraceptive use and reduces unmet need for family planning, accompanied by declines in child mortality. Evidence on structural channels is weaker and mixed: female labor-force participation rises modestly, turning significant at intermediate horizons, whereas female educational attainment shows no improvement, with estimates if anything trending negative. Because the two move in opposite directions, they do not indicate a broad improvement in women's socioeconomic position. Micro-level evidence from pooled Demographic and Health Surveys indicates that these aggregate effects operate substantially through delayed fertility timing: later marriage and first births, concentrated among younger women. The retrospective data we use, however, cannot resolve whether this represents a pure delay or a reduction in completed fertility.

The findings should be interpreted with appropriate caution. Our estimates rest on the ten SSA countries that adopted reserved-seat quotas during our study period, which generate the discrete and verifiable increases in representation that our identification strategy requires. The placebo test using candidate-list quotas is consistent with the fertility response operating through the increase in representation itself, rather than the legislative act of quota adoption. However, several countries in the region have reached comparable or higher levels of female parliamentary representation through other channels (electoral-system features, party-level commitments, or sustained mobilization) and our design cannot directly estimate the policy and fertility consequences of representation in those settings. The reserved-seat adopters are also not a random draw from the region, and several did so amid broader constitutional or post-conflict transitions. Our empirical strategy addresses these concerns through pre-trend tests, alternative estimators, post-conflict robustness checks, and selection tests; the sign and dynamic profile of the effect are consistent across specifications. Whether similar policy responses would follow from representation gains achieved through other means, or in countries at different stages of the fertility transition, remains an open question.

From a policy perspective, our results indicate that reserved-seat quotas, and the increases in women's parliamentary representation they produce, translate into measurable improvements in the reproductive-health policy environment and, ultimately, into lower period fertility. In a region where unmet need for family planning is widespread, this pattern is consistent with women's improved ability to align fertility outcomes with their own reproductive preferences, rather than with any externally imposed demographic target. That distinction is central to how the finding should be read. Fertility therefore does not serve as a normative target. It is a downstream indicator of whether changes in policy priorities and in access to reproductive-health services translate into realized reproductive behavior. We are nonetheless cautious about strong prescriptive conclusions: gender-quota legislation is one instrument among many, and the effects we identify unfold through a gradual, multi-year process of policy and behavioral change that depends on supporting institutions. We therefore read our findings less as a case for quotas as a fertility-policy tool than as evidence for a more general mechanism: when women hold a larger share of legislative power, their preferences become more visible in policymaking, on reproductive health and likely well beyond. Closing the gender gap in political representation is a goal in its own right; identifying the other domains in which representation generates similar dynamics is an important direction for future work.

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Appendix A

A. Appendix to Sections II and III

Table A1—List of countries in our sample that adopted gender quotas

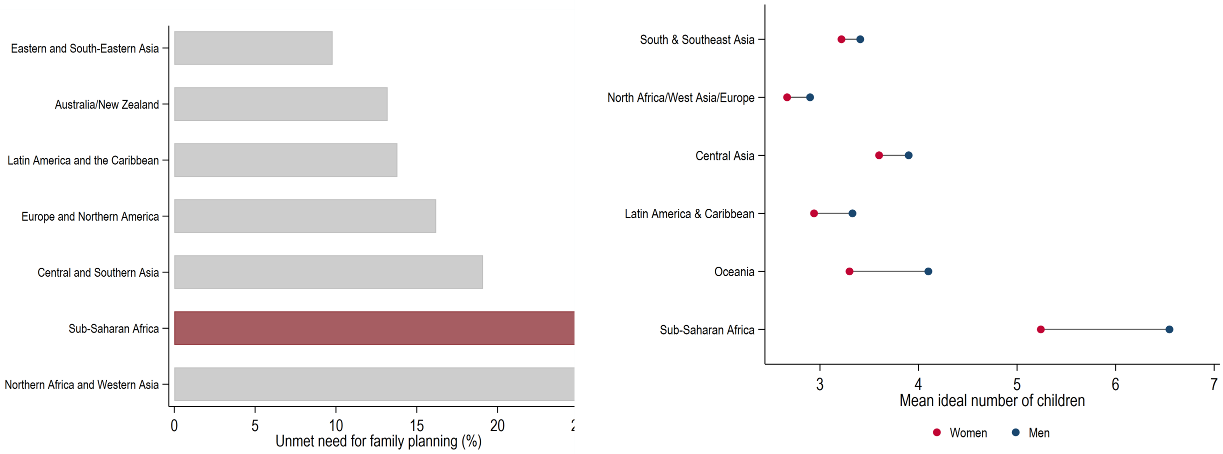
Country	Quota type	Year	Quota target (%)
Benin	Reserved seats	2019	22
Burundi	Reserved seats	2005	30
Eswatini	Reserved seats	2005	30
Djibouti	Reserved seats	2018	30
Kenya	Reserved seats	2010	33
Niger	Reserved seats	2000	25
Rwanda	Reserved seats	2003	30
Sierra Leone	Reserved seats	2022	30
Somalia	Reserved seats	2012	30
Zimbabwe	Reserved seats	2013	21
Angola	Legislated Candidate Quota	2010	30
Burkina Faso	Legislated Candidate Quota	2009	30
Cape Verde	Legislated Candidate Quota	2019	40
Central African Republic	Legislated Candidate Quota	2016	35
Chad	Legislated Candidate Quota	2019	30
Rep. Congo	Legislated Candidate Quota	2007	30
Côte d'Ivoire	Legislated Candidate Quota	2019	30
Gabon	Legislated Candidate Quota	2016	30
Guinea	Legislated Candidate Quota	2010	30
Guinea-Bissau	Legislated Candidate Quota	2018	36
Lesotho	Legislated Candidate Quota	2011	30
Liberia	Legislated Candidate Quota	2014	30
Mali	Legislated Candidate Quota	2015	30
Senegal	Legislated Candidate Quota	2010	50
Sao Tomé & Príncipe	Legislated Candidate Quota	2022	40
Togo	Legislated Candidate Quota	2013	50

Notes: The table lists countries in our analytical sample that adopted gender quotas, alongside the type of quota, year of legislation, and quota target. Only countries that legislated quotas after 2000 are included. Countries that adopted quotas prior to 2000 (e.g., Tanzania in 1995 and Uganda in 1989, both with reserved seats) are excluded to ensure sufficient pre-treatment observations for placebo tests to assess parallel trends.

Table A2—Summary Statistics

	Mean	Std. Dev.	Min.	Max.
Panel A: Full sample				
% Women in Parliament	14.78	11.57	0.00	63.75
Reserved seats	0.09	0.29	0.00	1.00
Total Fertility Rate	5.03	1.42	1.32	7.84
Contraceptive prevalence: any modern method (percent)	21.62	18.61	0.00	69.30
Unmet need for modern contraception	30.22	8.52	10.00	49.10
Total demand for family planning	41.10	11.38	18.40	66.30
Mean years of schooling females	4.15	2.34	0.37	9.43
Female labor force participation	57.24	16.57	15.38	90.45
Child mortality rate (under 5)	106.82	60.21	11.60	478.90
GDP per capita int. $ PPP(log)	7.81	1.00	5.54	10.44
Panel B: Reserved seats sample				
% Women in Parliament	26.13	16.54	1.20	63.75
Reserved seats	1.00	0.00	1.00	1.00
Total Fertility Rate	4.97	1.60	2.61	7.83
Contraceptive prevalence: any modern method (percent)	33.52	23.24	0.50	68.60
Unmet need for modern contraception	24.61	8.82	10.00	46.20
Total demand for family planning	40.81	13.63	20.40	61.60
Mean years of schooling females	4.85	2.84	0.67	9.09
Female labor force participation	57.17	17.96	18.26	81.09
Child mortality rate (under 5)	86.21	42.83	39.90	227.80
GDP per capita int. $ PPP(log)	7.70	0.86	6.32	9.33
Panel C: No reserved seats sample				
% Women in Parliament	13.61	10.24	0.00	46.70
Reserved seats	0.00	0.00	0.00	0.00
Total Fertility Rate	5.03	1.40	1.32	7.84
Contraceptive prevalence: any modern method (percent)	20.38	17.62	0.00	69.30
Unmet need for modern contraception	30.80	8.28	10.70	49.10
Total demand for family planning	41.13	11.13	18.40	66.30
Mean years of schooling females	4.07	2.26	0.37	9.43
Female labor force participation	57.25	16.42	15.38	90.45
Child mortality rate (under 5)	108.90	61.32	11.60	478.90
GDP per capita int. $ PPP(log)	7.82	1.01	5.54	10.44

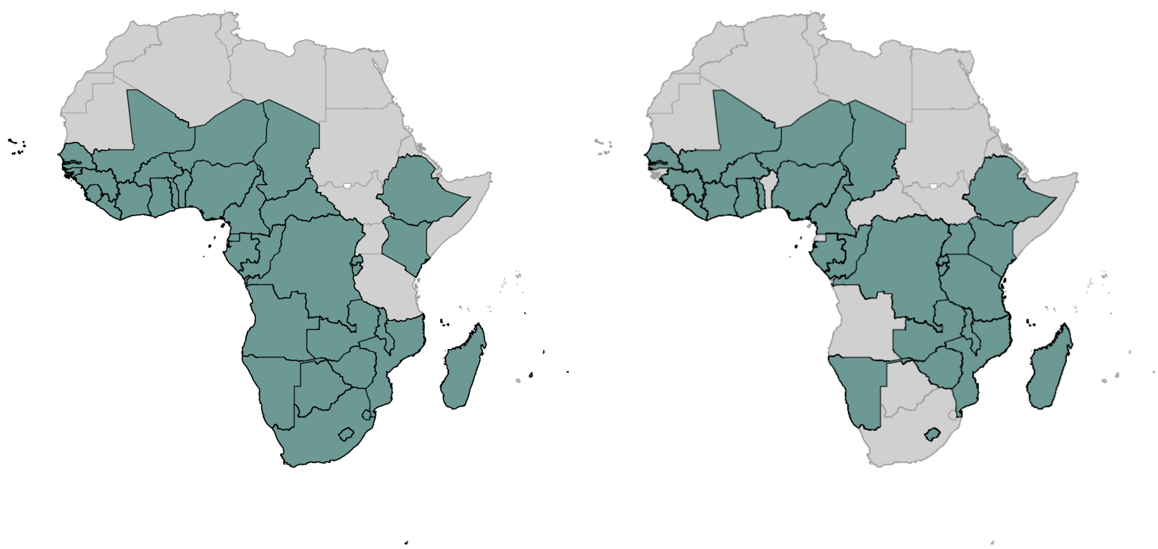
Notes: This table presents summary statistics for the main variables used in the analysis. Panel A reports statistics for the full sample, Panel B for the subsample of country-year observations with reserved seats for women in parliament, and Panel C for observations without reserved seats. All variables are defined in detail in Table A4 in Data Appendix D.



(a) Unmet need for family planning (b) Gender gap in fertility preferences

FIGURE A1. UNMET NEED AND FERTILITY PREFERENCES BY WORLD REGION

Notes: Panel (a) shows the share of women aged 15–49 who are married or in a union with an unmet need for family planning (any modern method) across world regions, in 2023. Data source: UN Population Division, *World Contraceptive Use 2023*. Panel (b) shows the mean ideal number of children reported by women (red) and men (blue) across the world regions with the connecting line indicating the size of the gender gap. Data source: DHS STATcompiler.



(a) Main sample (b) Sub-sample (DHS sample)

FIGURE A2. SAMPLE OVERVIEW

Notes: Panel (a) displays the countries included in the main analysis sample, covering 43 countries in sub-Saharan Africa. Panel (b) shows the countries included in the micro-level analysis using Demographic and Health Surveys (DHS) data described in Section VIII, covering 32 countries in sub-Saharan Africa.

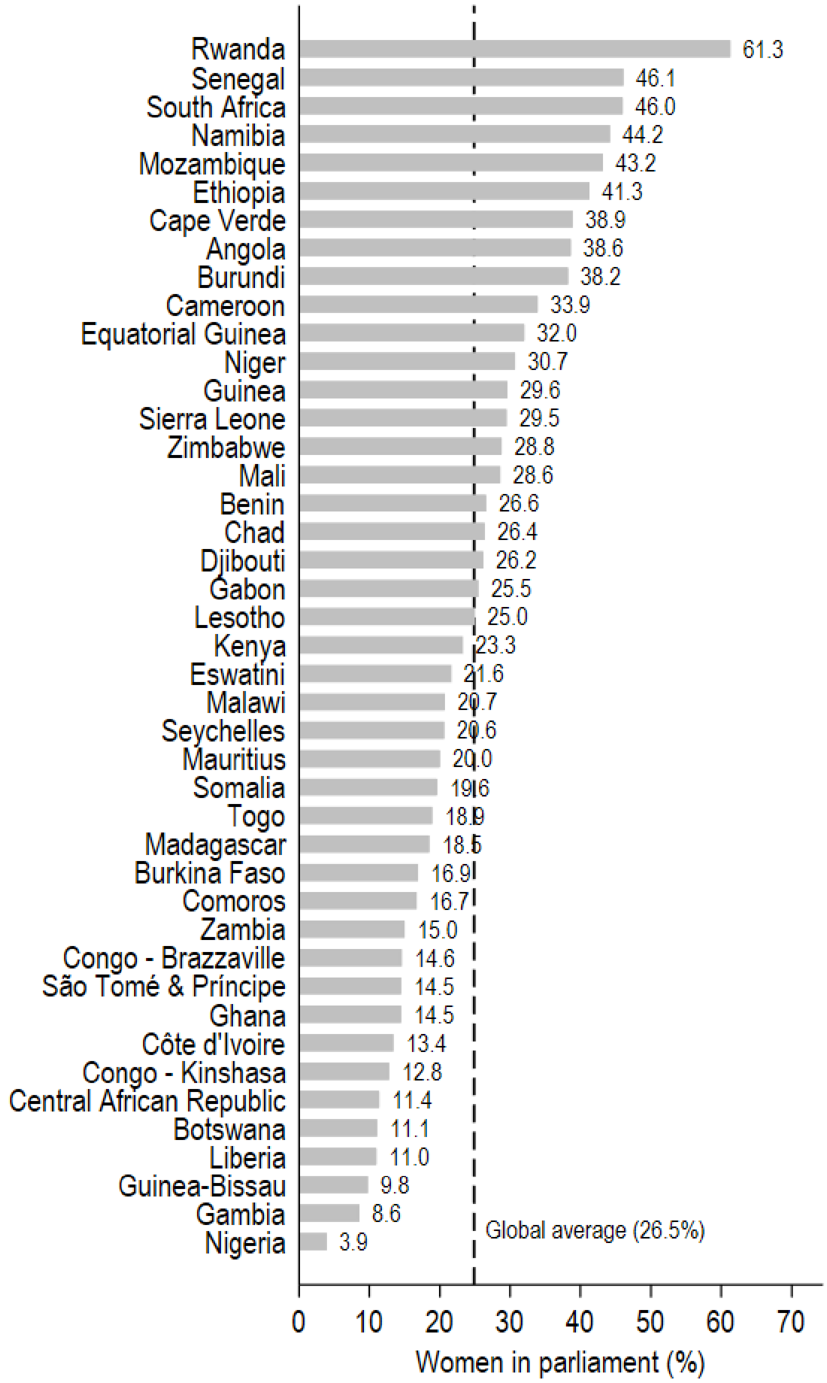


Figure A3. Country ranking

Notes: The figure ranks the sub-Saharan African countries included in our sample by the share of seats held by women in the (lower or single) house of the national parliament as of 2023. Countries are ordered in descending order. The dashed vertical line indicates the global average (26.5%). Data source: Inter-Parliamentary Union (IPU).

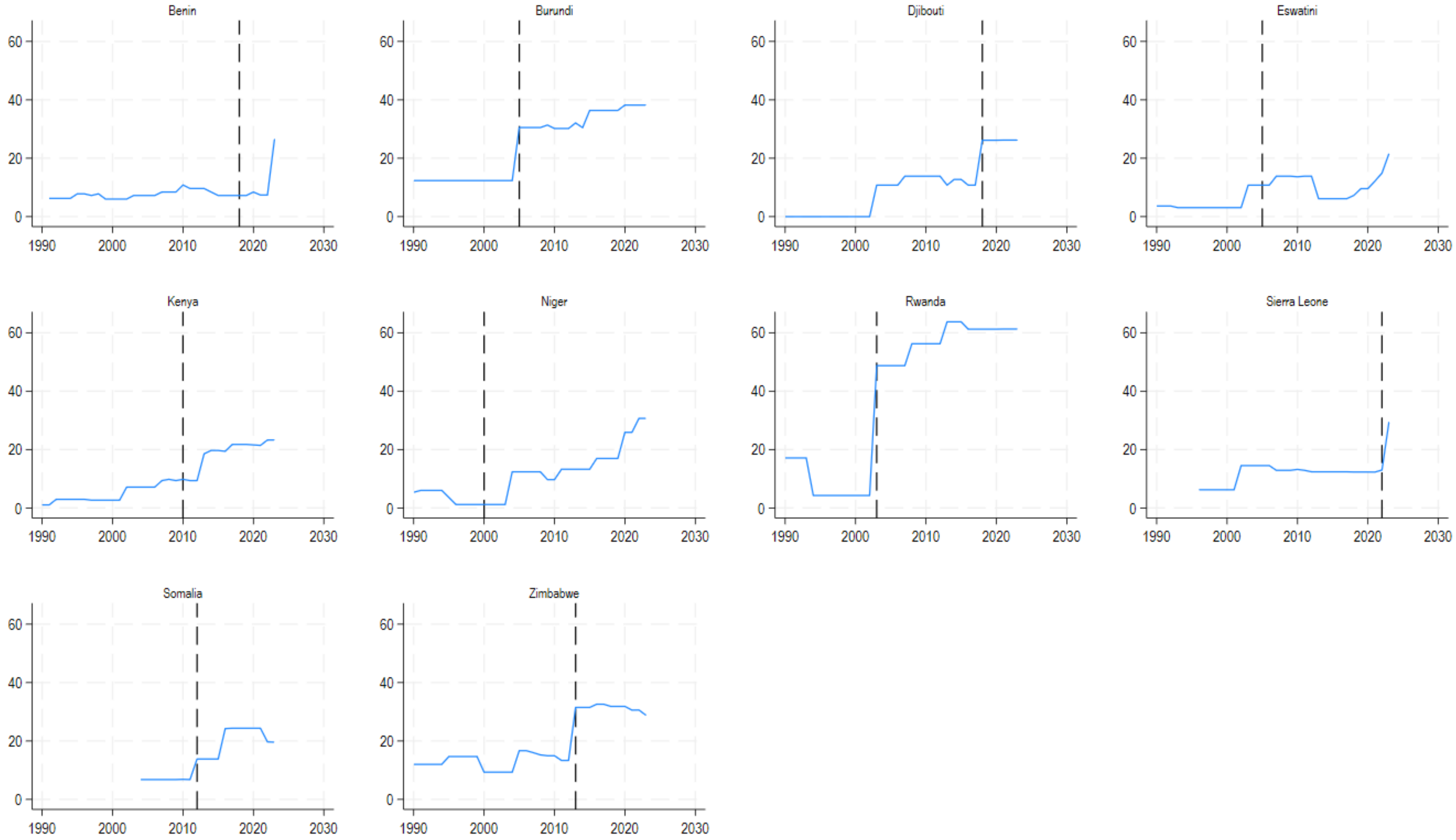
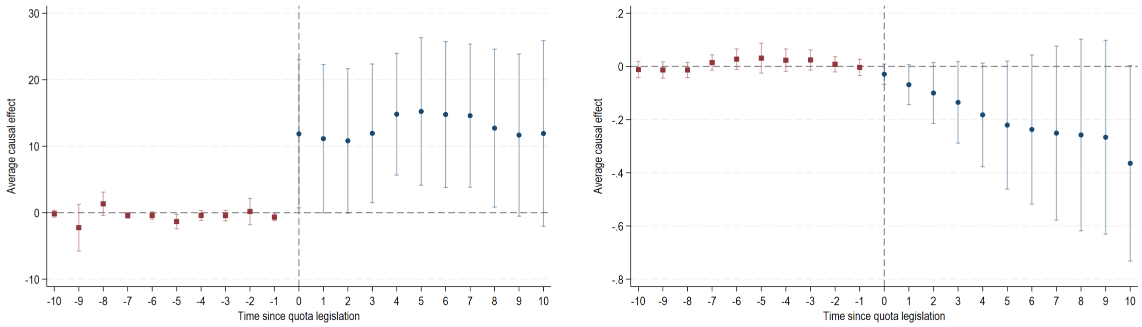


Figure A4. Women's parliamentary representation around quota legislation, by treated country

Notes: Each panel plots the share of parliamentary seats held by women (in percent) against year for one of the ten countries in our sample that legislated a reserved-seat gender quota after 2000. The vertical dashed line marks the year of quota legislation. All panels share a common vertical axis, so the magnitude of the increase is comparable across countries. The series document the discrete, country-specific jumps in women's representation that the identification strategy exploits; their timing relative to the legislation year varies because seats are allocated at the first parliamentary election held under the quota, which in several countries follows legislation by one or more electoral cycles. Data source: Inter-Parliamentary Union.

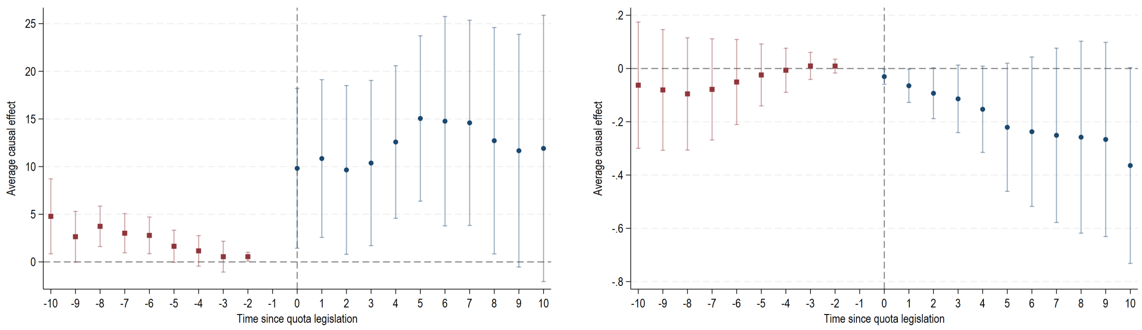


(a) Women in parliament (%)

(b) Total fertility rate

Figure A5. Balanced sample of adoption cohorts (2000–2013)

Notes: The figure reproduces the main event study (Figure 3) on a balanced sample restricted to treated countries that adopted reserved-seat quotas between 2000 and 2013, each observed over the full ten-year window before and after adoption. Restricting to these cohorts holds the set of contributing treated countries fixed across all event times, so that changes in the width of the confidence intervals across horizons cannot reflect changes in sample composition. Panel (a) reports estimates for the share of women in parliament and Panel (b) for the total fertility rate. Never-treated countries serve as the comparison group, and the plots report pointwise 95% confidence intervals. All other estimation details correspond to those reported in Figure 3.



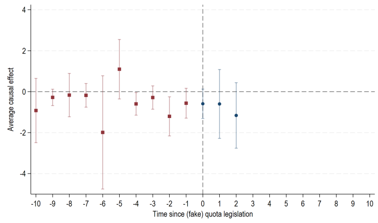
(a) Women in parliament (%)

(b) Total fertility rate

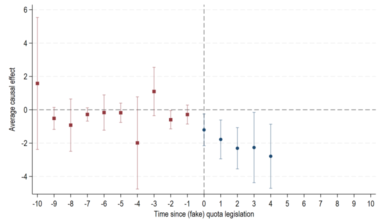
Figure A6. Event-study estimates with a universal base period

Notes: The figure reproduces Figure 3 using a universal (common) base period for the pre-treatment coefficients (`long2` option of `csdid`). Each pre-treatment coefficient then measures the cumulative difference between treated and control countries relative to $e = -1$.Post-treatment coefficients and all other estimation details are unchanged.

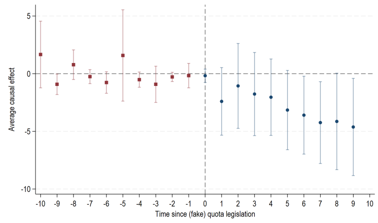
First stage: Women in parliament



(a) $k = 3$

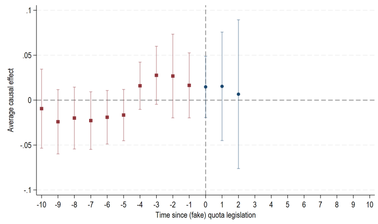


(b) $k = 5$

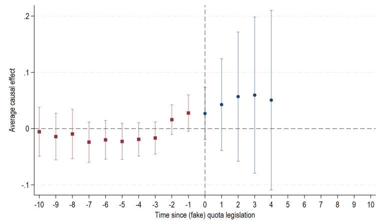


(c) $k = 10$

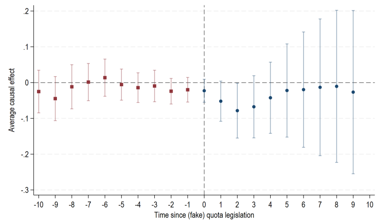
Second stage: Total Fertility Rate



(d) $k = 3$



(e) $k = 5$



(f) $k = 10$

Figure A7. Placebo: Shifting the Treatment Date backward

Notes: Each panel reports a placebo event-study assigning every treated country a counterfactual adoption date k years before its true year of legislation, estimated on observations prior to true adoption; never-treated countries remain the comparison group. The top row (a–c) reports the share of women in parliament and the bottom row (d–f) the total fertility rate, for $k = 3$, 5, and 10 years. Both lead and lag coefficients fall within the true pre-treatment window. All other estimation details as in Figure 3.

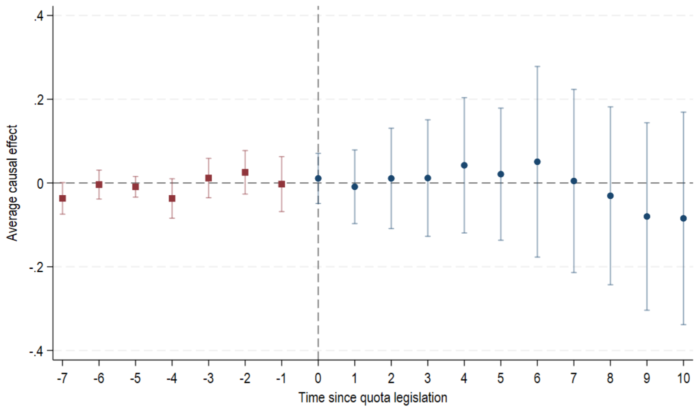
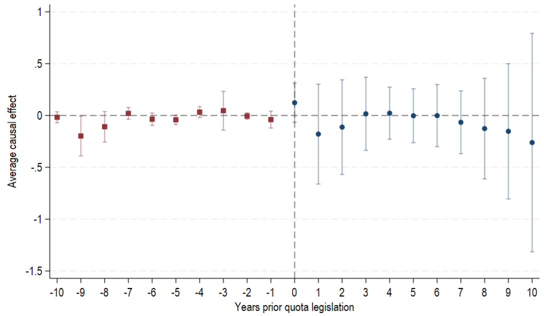
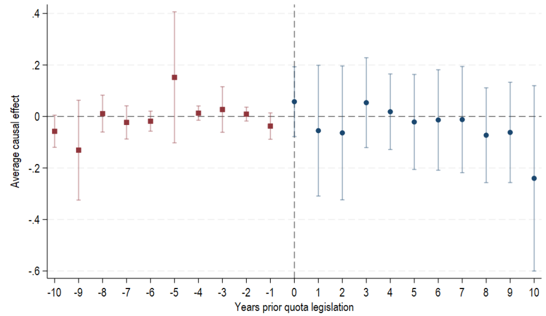


Figure A10. Reserved-seat quota impacts on maternal mortality

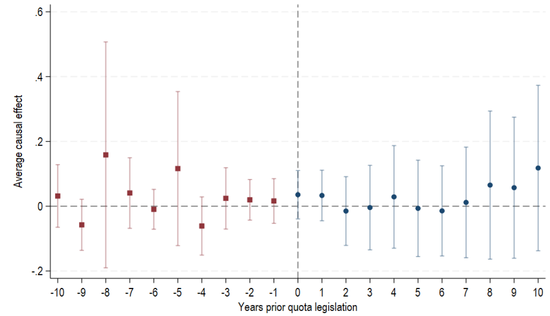
Notes: The figure plots dynamic treatment effects of reserved-seat quota legislation on the maternal mortality ratio, estimated following Callaway and Sant'Anna (2021). All other estimation details correspond to those reported in Figure 3.



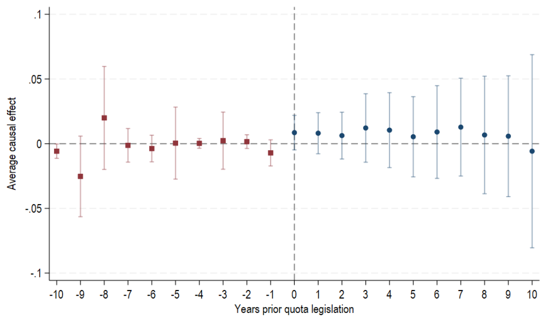
(a) Freedom of discussion



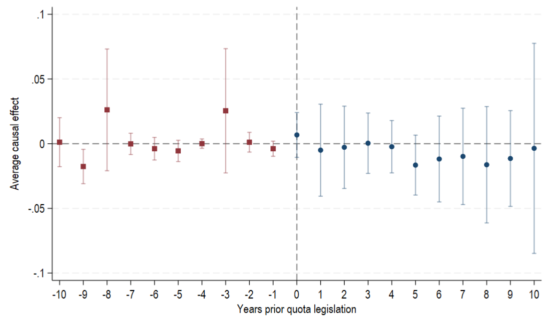
(b) Access to justice



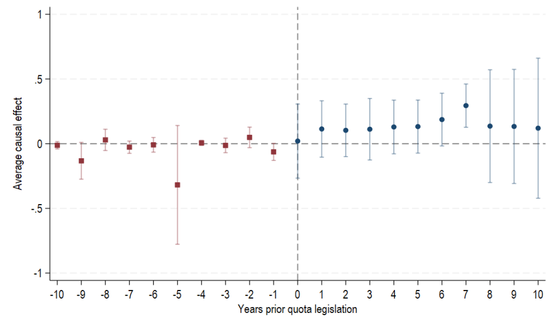
(c) Property rights



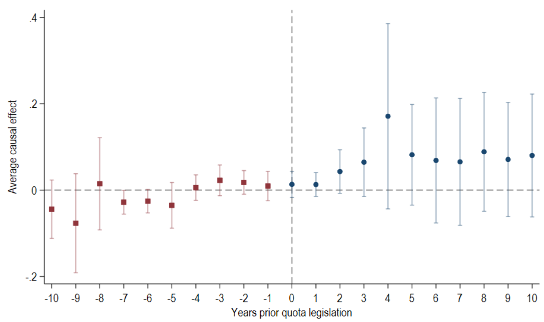
(d) Civil liberties



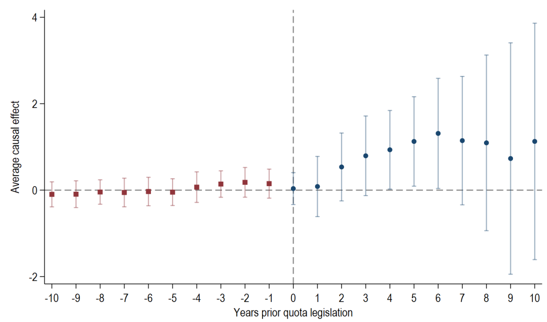
(e) Civil society participation



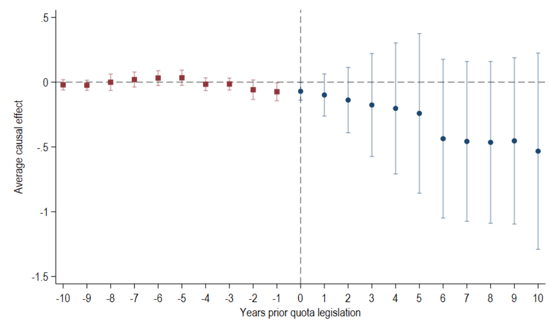
(f) Freedom forced labor



(g) GDP per capita (log)



(h) Female labor market participation

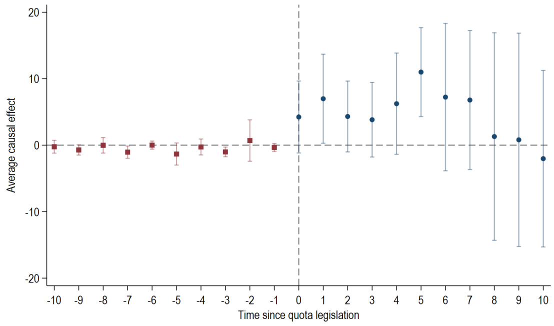


(i) Years of schooling (female)

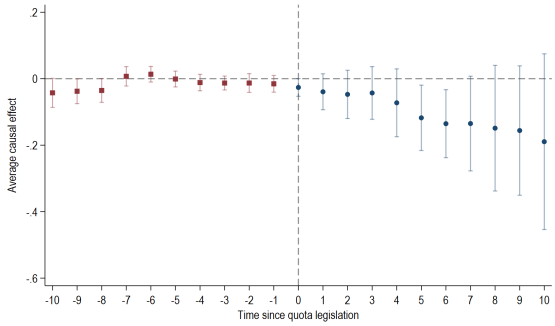
FIGURE A8. ROBUSTNESS: SELECTION INTO GENDER QUOTAS

Notes: The figure presents event-study estimates testing for pre-adoption trends in potential confounders of the fertility response. The gender-equality dimensions (Panels (a) – (f)) are drawn from the Varieties of Democracy (V-Dem) dataset; the remaining panels report female educational attainment, female labor-force participation, and GDP per capita. Detailed definitions and sources for each variable are provided in Table A4. All additional estimation details

(A) Sensitivity to exclusion of post-conflict and post-crisis countries

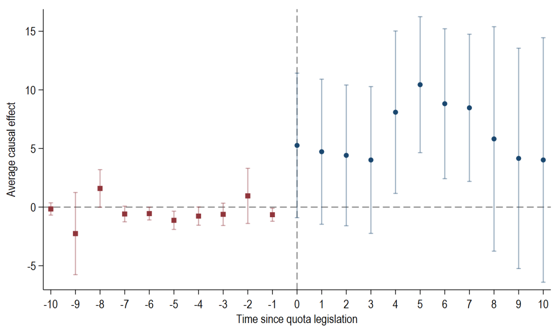


Women in parliament (%)

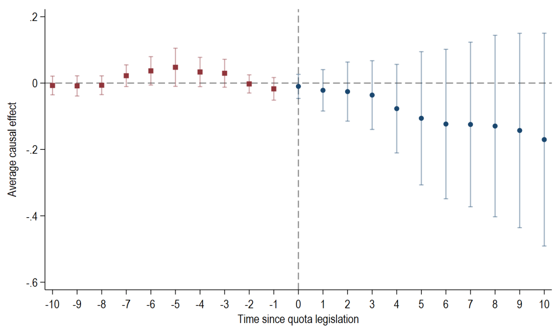


Total fertility rate

(B) Controlling for post-conflict recovery dynamics



Women in parliament (%)

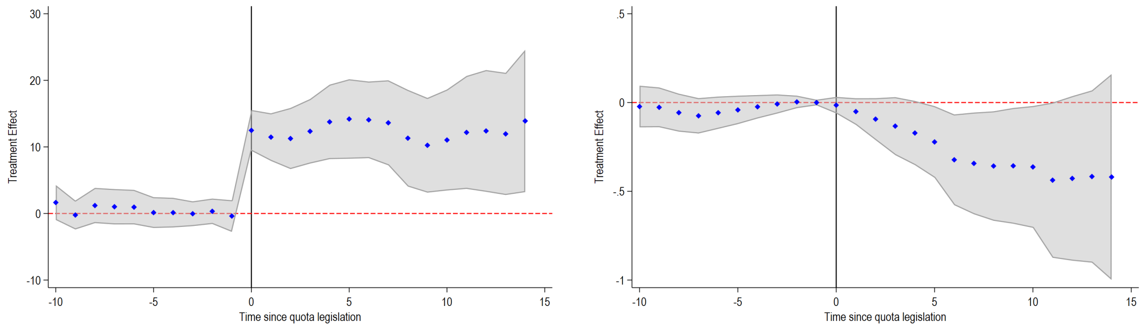


Total fertility rate

Figure A9. Robustness to post-conflict confounds

Notes: This figure reports robustness checks addressing the concern that quota adoption may coincide with post-conflict or post-crisis institutional transitions. Panel (a) excludes all five countries where quota adoption occurred within ten years of a major conflict or political crisis (Rwanda, Burundi, Kenya, Zimbabwe, and Niger), retaining only countries with no conflict history or with large temporal gaps between conflict and quota adoption (Eswatini, Benin, Djibouti, Sierra Leone, and Somalia). Panel (b) includes all treated countries but controls for post-conflict recovery dynamics by adding the number of years since the end of armed conflict as a time-varying covariate, set to zero for countries with no conflict history and for observations prior to conflict termination. All specifications follow Callaway and Sant'Anna (2021) using never-treated countries as controls.

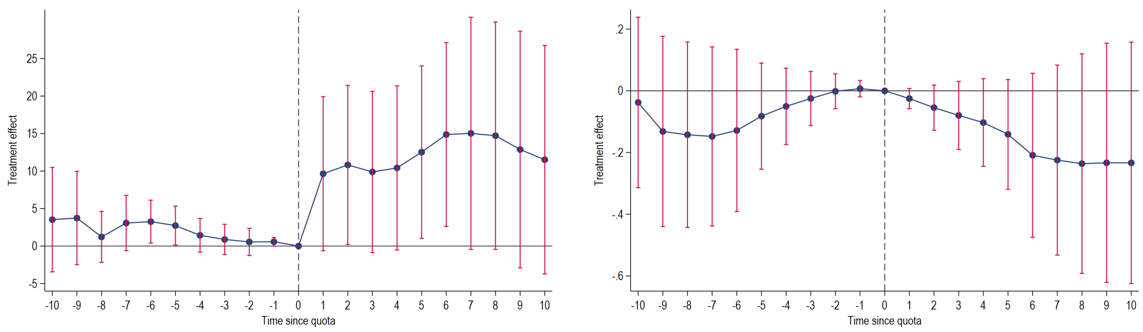
Synthetic Difference-in-Differences



(a) Women in parliament (%)

(b) Total fertility rate

Estimates following de Chaisemartin and D'Haultfoeuille (2020)

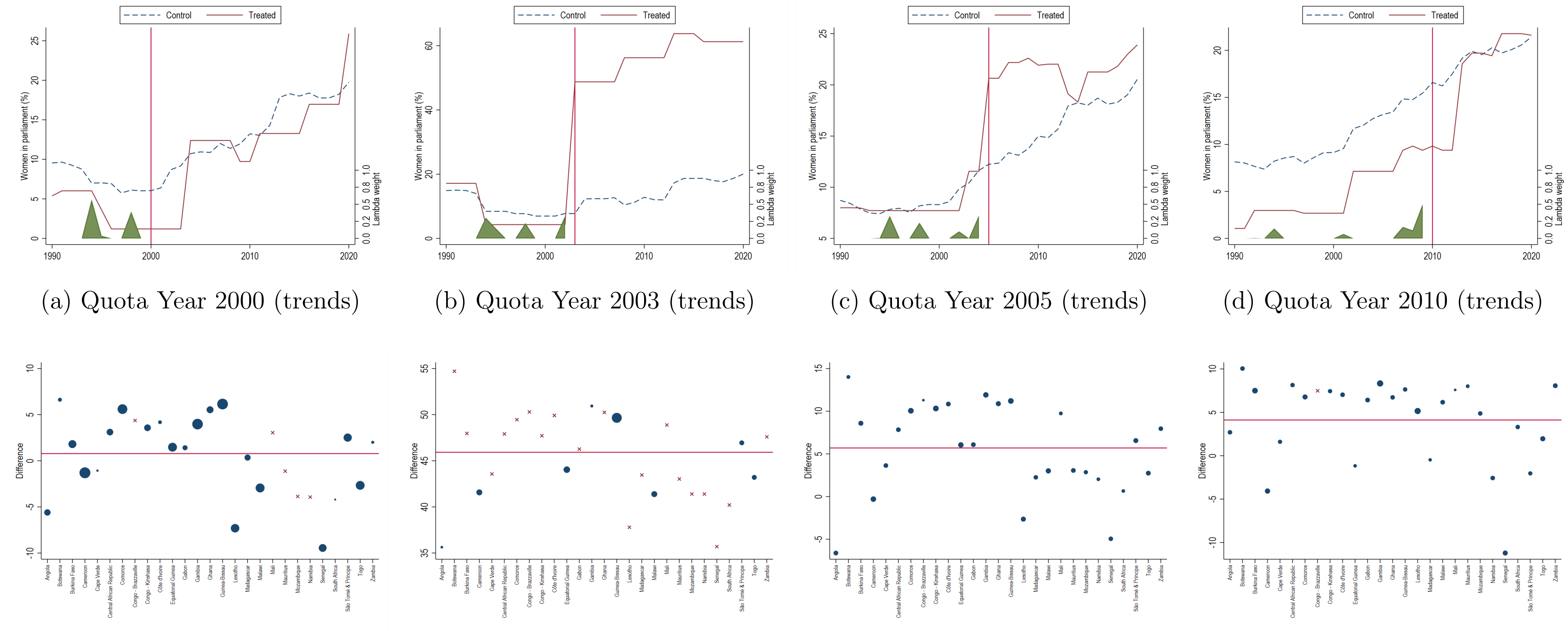


(c) Women in parliament (%)

(d) Total fertility rate

Figure A11. Alternative Estimators: Event-study plots

Notes: The figure displays event-study estimates of the effect of gender quota adoption on women's parliamentary representation and the total fertility rate using alternative estimators to those employed in our main results. Panel (a) and (b) show the results using the synthetic difference-in-differences estimator proposed by Arkhangelsky et al. (2021). Appendix Figures A12 and A13 report the corresponding pre- and post-legislation trends for treated countries and their synthetic controls, along with the weights assigned to each untreated country in their construction. Panel (c) and (d) display estimates using the estimator proposed by de Chaisemartin and D'Haultfoeuille (2020) which is robust to heterogeneous treatment effects in staggered adoption settings.

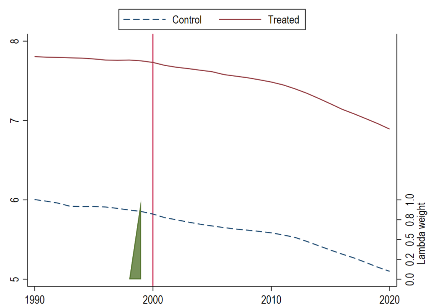


(a) Quota Year 2000 (trends) (b) Quota Year 2003 (trends) (c) Quota Year 2005 (trends) (d) Quota Year 2010 (trends)

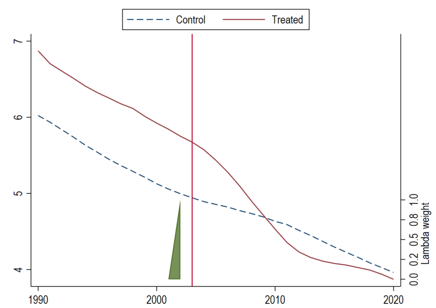
(e) Quota Year 2000 (weights) (f) Quota Year 2003 (weights) (g) Quota Year 2005 (weights) (h) Quota Year 2010 (weights)

Figure A12. Synthetic Diff-in-Diff: Outcome Trends & Weights (Women in parliament)

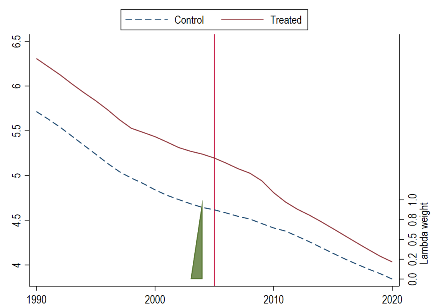
Notes: The figure presents adoption-specific estimates obtained using the Synthetic Difference-in-Differences estimator following Arkhangelsky et al. (2021). The top panels display trends in the share of women in parliament for treated units and their synthetic controls, together with the corresponding time-specific weights. The lower panels report the weights assigned to each untreated country, where point size reflects the magnitude of the weight. They also show country-specific observed differences, calculated as $\hat{\delta}_{tr} - \hat{\delta}_i$, with the vertical line indicating the weighted average of these differences, corresponding to the estimated treatment effect. Observations receiving zero weight are denoted by an "×" symbol. The overall synthetic difference-in-differences estimate is computed as the weighted average of adoption-year-specific estimates. Implementation follows Clarke et al. (2023).



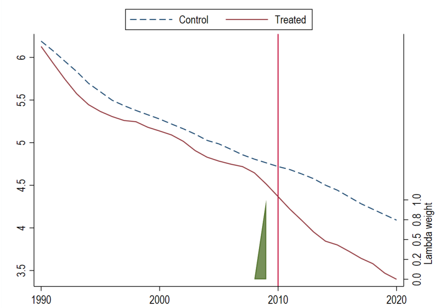
(a) Quota Year 2000 (trends)



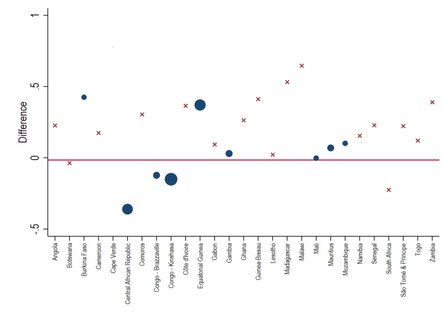
(b) Quota Year 2003 (trends)



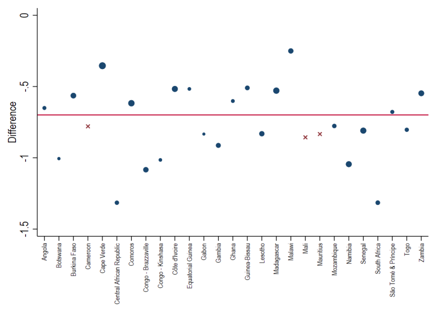
(c) Quota Year 2005 (trends)



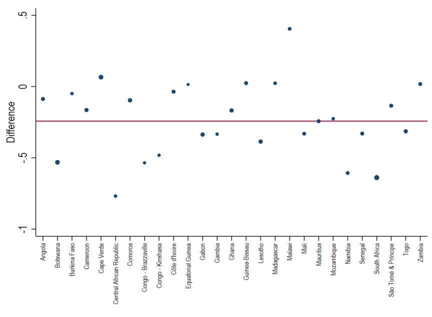
(d) Quota Year 2010 (trends)



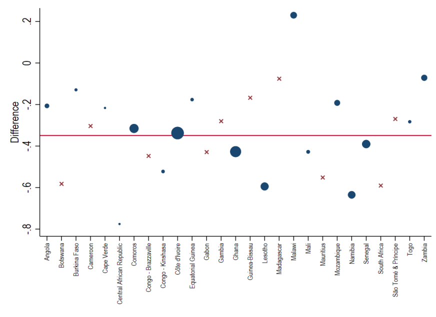
(e) Quota Year 2000 (weights)



(f) Quota Year 2003 (weights)



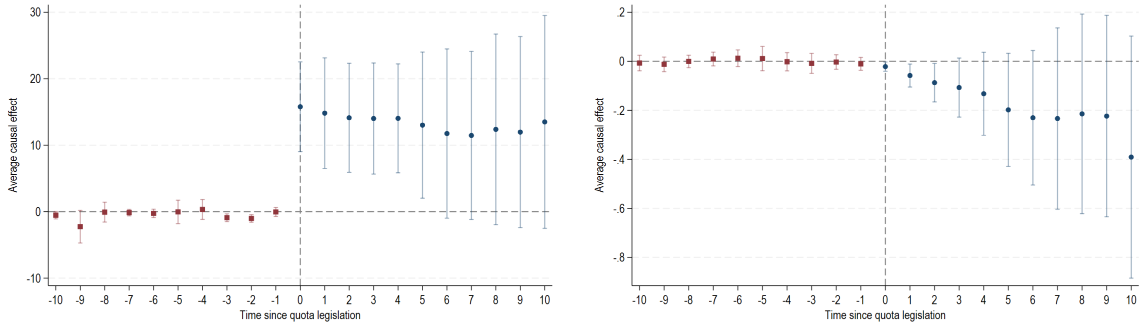
(g) Quota Year 2005 (weights)



(h) Quota Year 2010 (weights)

Figure A13. Synthetic Diff-in-Diff: Outcome Trends & Weights (Total Fertility Rate)

Notes: The figure presents adoption-specific estimates obtained using the Synthetic Difference-in-Differences estimator following Arkhangelsky et al. (2021). The top panels display trends in total fertility rates for treated units and their synthetic controls, together with the corresponding time-specific weights. The lower panels report the weights assigned to each untreated country, where point size reflects the magnitude of the weight. They also show country-specific observed differences, calculated as $\hat{\delta}_{tr} - \hat{\delta}_i$, with the vertical line indicating the weighted average of these differences, corresponding to the estimated treatment effect. Observations receiving zero weight are denoted by an "×" symbol. The overall synthetic difference-in-differences estimate is computed as the weighted average of adoption-year-specific estimates. Implementation follows Clarke et al. (2023).



(a) Women in parliament (%)

(b) Total fertility rate

FIGURE A14. TREATMENT DATED AT QUOTA IMPLEMENTATION

Notes: The figure reproduces the main event study (Figure 3), re-dating each treated country's quota adoption from the year of legislation to the year of the first parliamentary election held under the reserved-seat quota. All additional estimation details correspond to those reported in Figure 3.

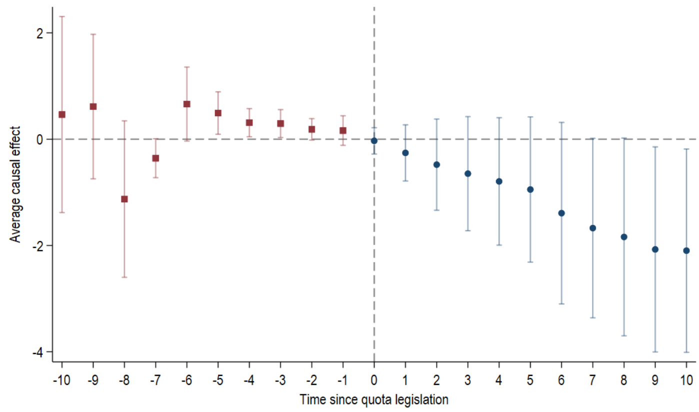
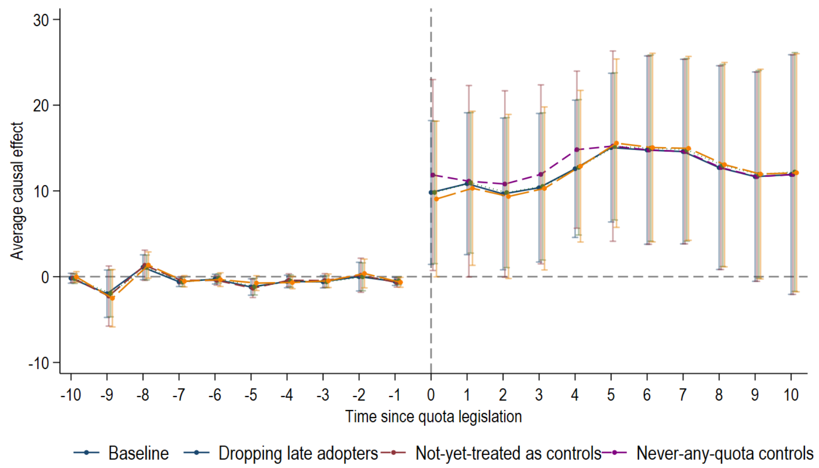
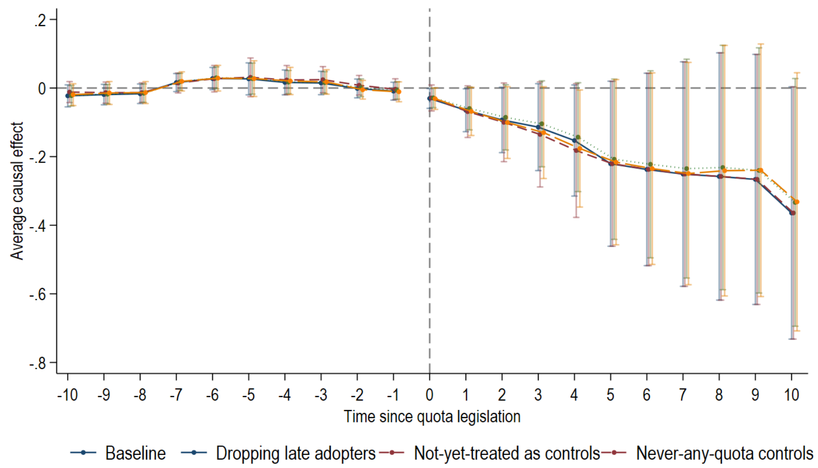


FIGURE A15. ALTERNATIVE FERTILITY MEASURE: CRUDE BIRTH RATE

Notes: This figure replicates the main event-study analysis presented in Panel (b) of Figure 3 using the crude birth rate (CBR), defined as the annual number of births per 1,000 population, as the dependent variable in place of the total fertility rate. All additional estimation details correspond to those reported in Figure 3.



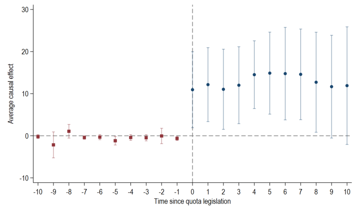
(a) Women in Parliament (%)



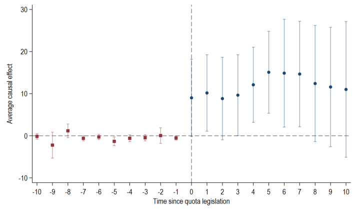
(b) Total Fertility Rate

Figure A16. Sensitivity to sampling

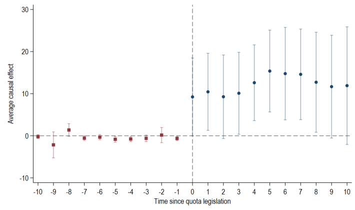
Notes: The figure assesses the sensitivity of the main estimates to alternative sample constructions. Panel (a) reports effects on the share of seats held by women in national parliaments and Panel (b) on the total fertility rate. All specifications are estimated following Callaway and Sant'Anna (2021); the sample construction underlying each estimate is indicated in the legend. *Baseline* reproduces the main specification of Figure 3, using never-treated countries as controls. *Dropping late adopters* restricts the sample to a balanced event-time window of ten years before and after adoption by excluding countries that legislated quotas after 2013, so that every treated country is observed over the full window. *Not-yet-treated as controls* uses not-yet-treated countries, rather than never-treated countries, as the comparison group. *Never-any-quota controls* restricts the comparison group to countries that adopted neither a reserved-seat nor a candidate-list quota during the observation period. All additional estimation details correspond to those reported in Figure 3.



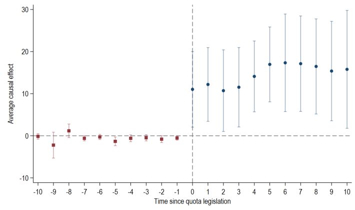
(a) Benin



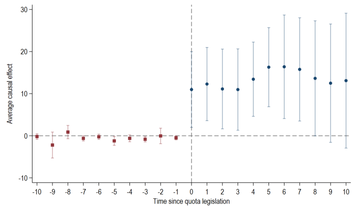
(b) Burundi



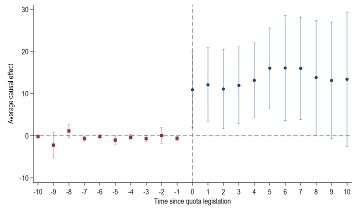
(c) Djibouti



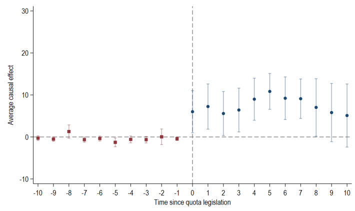
(d) Eswatini



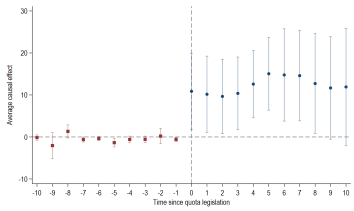
(e) Kenya



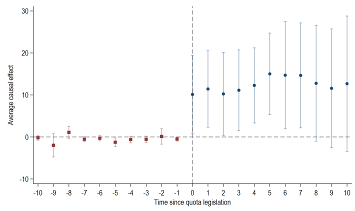
(f) Niger



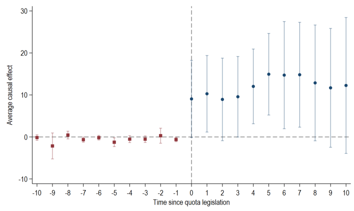
(g) Rwanda



(h) Sierra Leone



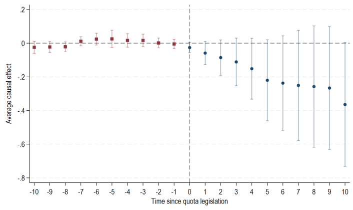
(i) Somalia



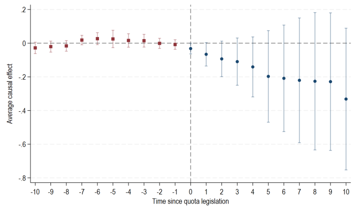
(j) Zimbabwe

Figure A17. Leave-one-out analysis: Women in parliament (%)

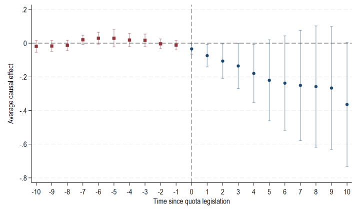
Notes: Plots replicate Panel (a) of Figure 3, leaving out one quota-adopting country at a time. The title of each plot indicates the country excluded when estimating each model. All additional estimation details correspond to those reported in Figure 3.



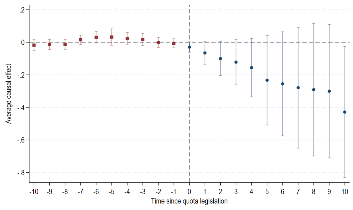
(a) Benin



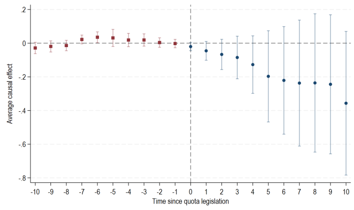
(b) Burundi



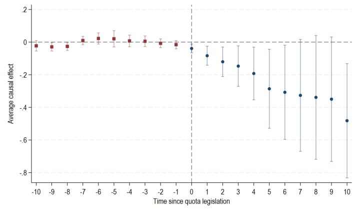
(c) Djibouti



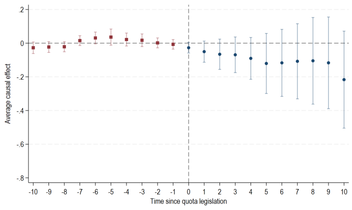
(d) Eswatini



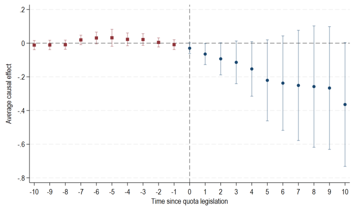
(e) Kenya



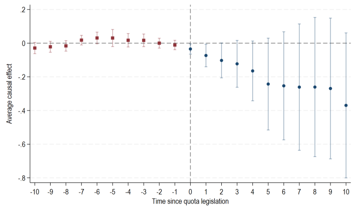
(f) Niger



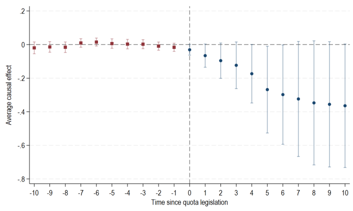
(g) Rwanda



(h) Sierra Leone



(i) Somalia

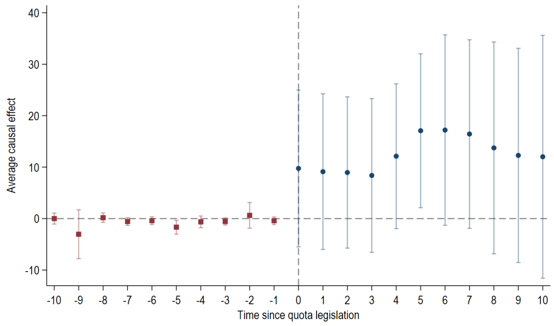


(j) Zimbabwe

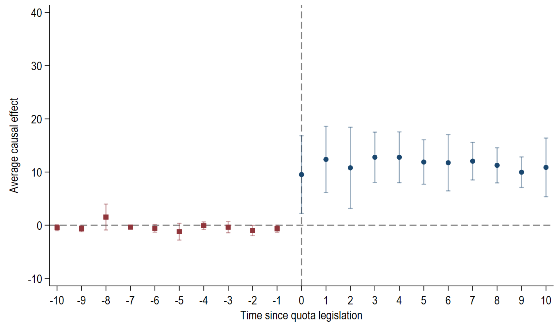
FIGURE A18. LEAVE-ONE-OUT ANALYSIS: TOTAL FERTILITY RATE

Notes: Plots replicate Panel (b) of Figure 3, leaving out one quota-adopting country at a time. The title of each plot indicates the country excluded when estimating each model. All additional estimation details correspond to those reported in Figure 3.

Pre-adoption representation

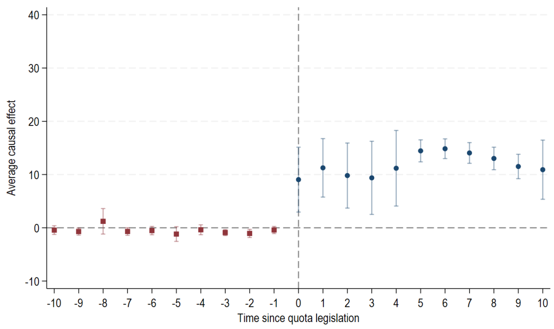


(a) Low pre-adoption representation

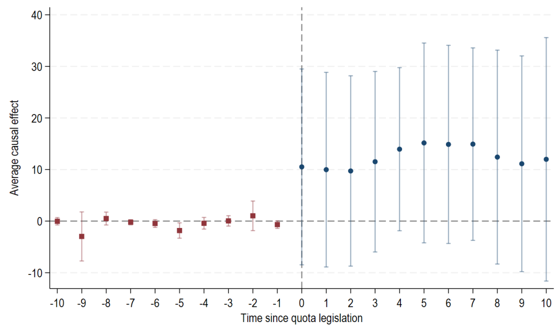


(b) High pre-adoption representation

Quota bite



(c) Low quota bite

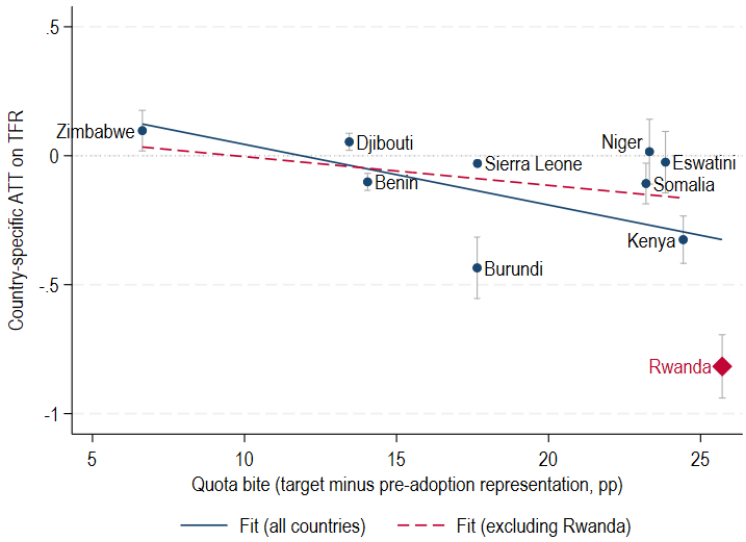


(d) High quota bite

Figure A19. Treatment Intensity: Subgroup Effects on Representation

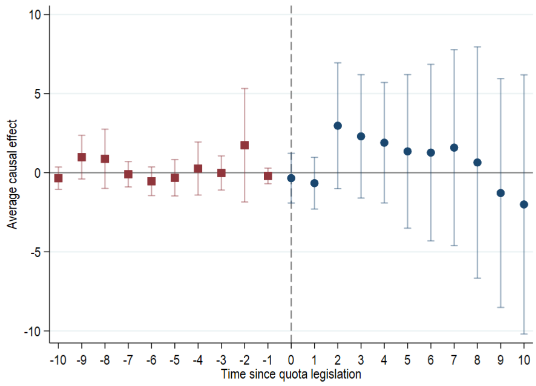
Notes: The figure reports event-study estimates of the effect of reserved-seat gender quotas on the share of women in parliament (percentage points) following Callaway and Sant'Anna (2021), separately for subgroups of treated countries. Panels (a)–(b) split treated countries at the median of their average parliamentary representation in the five years before adoption; panels (c)–(d) split at the median of the quota *bite*, defined as the legislated target minus that pre-adoption representation. The median is taken across the ten treated countries; never-treated countries serve as controls in every subgroup. All additional estimation details correspond to those reported in Figure 3.

Fertility response by quota bite

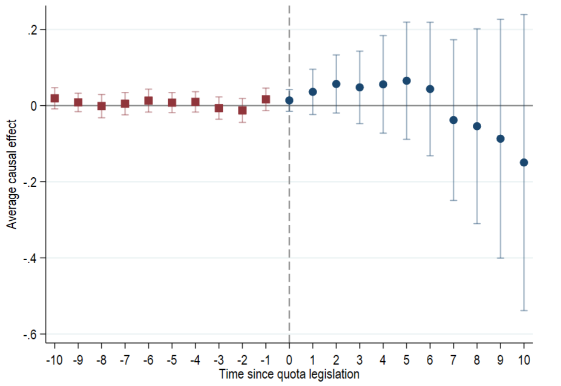


(a) ATT on TFR against quota bite

Candidate list quotas



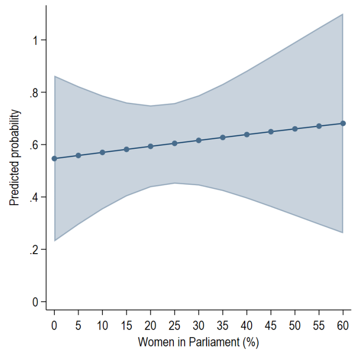
(b) Women in parliament (%)



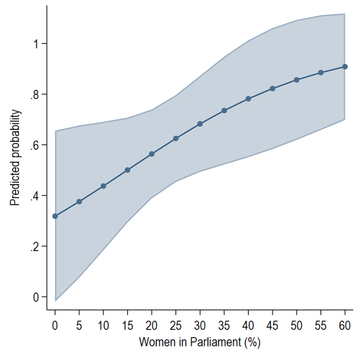
(c) Total fertility rate

Figure A20. Treatment Intensity and Placebo Tests

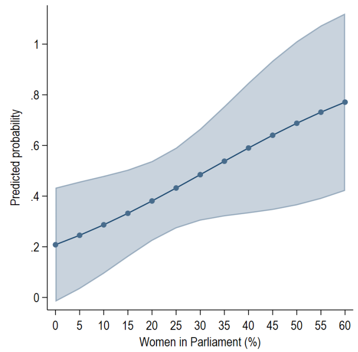
Notes: Panel (a) plots each treated country's average treatment effect on the total fertility rate (TFR) against the quota *bite*, defined as the legislated target minus the country's own average share of women in parliament over the five years preceding adoption ($K \in \{-5, \ldots, -1\}$)—a design-based measure of the mandated increase in representation, independent of the realized fertility path. Country-specific ATTs are estimated separately for each treated country against the never-treated controls following Callaway and Sant'Anna (2021), taking the overall post-treatment aggregate. The solid line is the linear fit across all ten treated countries (slope $= -0.024$) and the dashed line excludes Rwanda (slope $= -0.011$). Panels (b)–(c) present placebo estimates of the impact of candidate-list quotas on the share of women in parliament and the TFR, also following Callaway and Sant'Anna (2021), using never-treated countries as controls. All additional estimation details correspond to those reported in Figure 3.



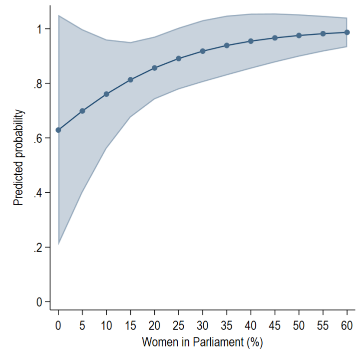
(a) Restricted access



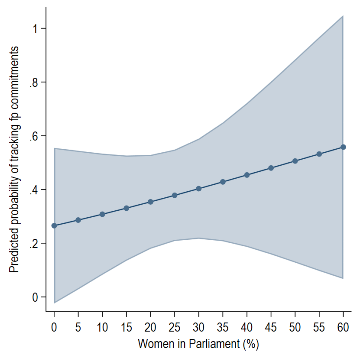
(b) Stock-out monitoring



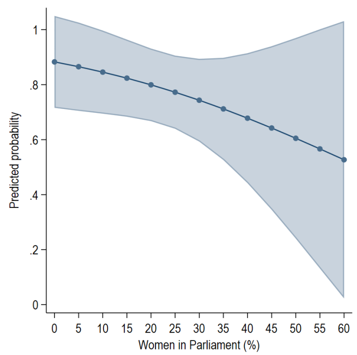
(c) Global finance partner



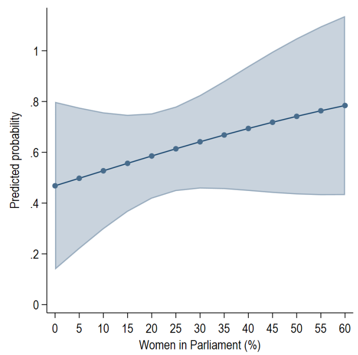
(d) Int. FP commitments



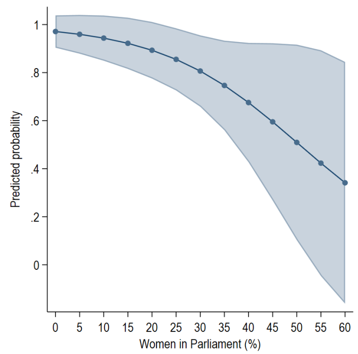
(e) Tracking commitments



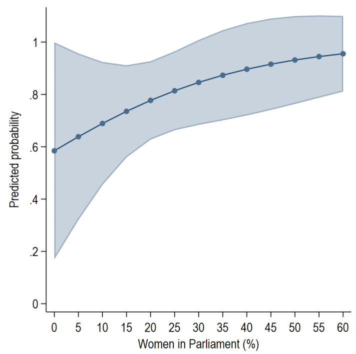
(f) Com. supply strategy



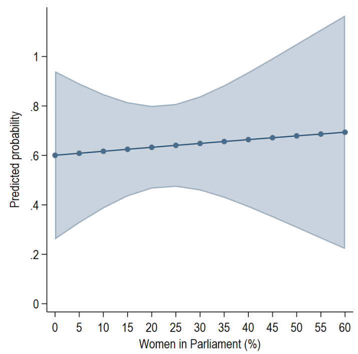
(g) Supply committee



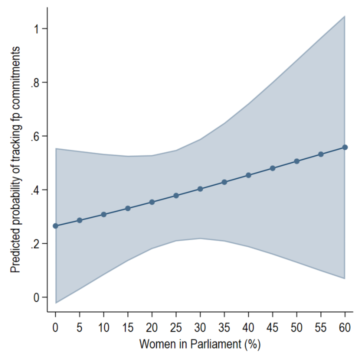
(h) Essential medicine list



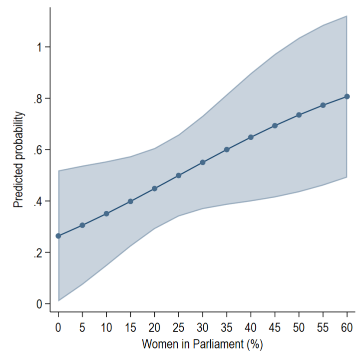
(i) Gov. funding CP



(j) Supplies budget line



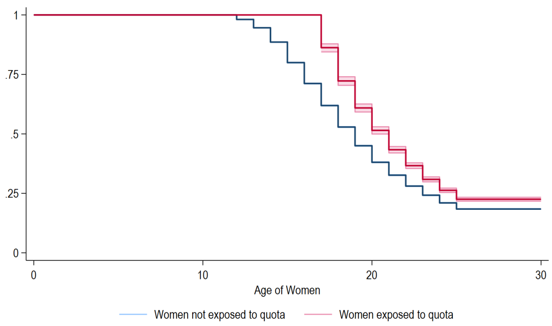
(k) Fees charged



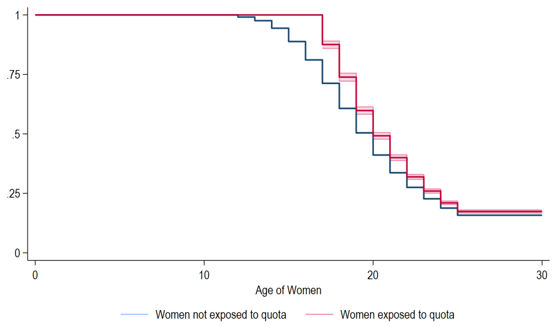
(l) CP health insurance

Figure A21. Mechanisms: Legislative activity on SRH policies

C. Appendix to Section VIII



(a) Marriage



(b) First birth

Figure A22. Kaplan-Meier Survival Curves: Marriage and Fertility Timing

Notes: The figure presents Kaplan-Meier survival curves for the transition to first marriage (Panel a) and first birth (Panel b), separately for women exposed and not exposed to reserved-seat quota legislation during their formative reproductive years. Each curve traces the share of women who remain unmarried (Panel a) or childless (Panel b) at each age. Data source: Demographic and Health Surveys (DHS).

Table A3—Cross-sectional estimates on Marriage and Fertility Onset

	(1)	(2)	(3)	(4)	(5)	(6)
	Married < 18 years		First child < 18 years		Number of births by 25	
Q_{12-17}	-0.0126	-0.0109	-0.0259***	-0.0246***		
	(0.0106)	(0.00887)	(0.00773)	(0.00740)		
Q_{12-24}					-0.165**	-0.149**
					(0.0700)	(0.0698)
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-survey wave FE	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	No	Yes	No	Yes	No	Yes
Interview Age FE	No	No	No	No	Yes	Yes
N	434477	434477	434477	434477	434477	434477
Adj. R^2	0.0751	0.0847	0.0413	0.0477	0.0780	0.0899

Notes: Cross-sectional regressions using the DHS sample described in Section VIII. Columns (1)–(2) report the probability of marrying before age 18 and columns (3)–(4) the probability of a first birth before age 18, both based on quota exposure before age 18. Columns (5)–(6) report the cumulative number of children, based on exposure between ages 12 and 25. Standard errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.10.

D. Data Appendix

Table A4—Variable Descriptions

Variable (Source)	**Description**
Fertility measures	
Total fertility rate *(World Bank)*	Represents the average number of children born to a woman if she were to live to the end of her childbearing years. It is measured in births per woman and is updated annually. (WDI, indicator SP.DYN.TFRT.IN)
Individual-level birth history data *(Demographic and Health Surveys)*	Retrospective fertility histories derived from Demographic and Health Surveys (DHS), containing complete birth records for each respondent. These data are used to construct key fertility outcomes, including total number of children ever born, age at first birth, age at first marriage, and birth spacing intervals
Political representation	
Women in parliament *(Inter-Parliamentary Union)*	Percentage of seats in the lower or single chamber of the legislature held by women, derived from the Inter-Parliamentary Union (IPU) archive of monthly rankings of women in national parliaments.
Data on Gender Quotas *(Gender Quotas Database)*	Data on gender quota legislation are gathered from the Global Database of Gender Quotas in parliaments worldwide, a joint project of International IDEA, the Inter-Parliamentary Union and the Stockholm University. The data include information on countries that adopted a gender quota, including the type of quota (i.e., reserved seats or candidate list quota), the year of legislation, and the percentage target specified in quota legislation. Indicator for the introduction of a gender quota. https://www.idea.int/data-tools/data/gender-quotas-database
Quota predictors	

Continued on next page

Table A4 continued

Variable *(Source)*	Description
Women's Rights (*V-Dem*)	Measures of women's rights and measures of relative social standing are based on indicators provided by the Variety of Democracy dataset (Version 16). We use the following indicators: - *Freedom of discussion (v2cldiscw)*: Extent to which women are free to openly discuss political matters in private and public settings without fear of repression. - *Access to justice (v2clacjstw)*: Degree to which women have equal and effective access to the legal system and judicial remedies. - *Property rights (v2clprptyw)*: Extent to which women enjoy secure and equal rights to own and control property. - *Women civil liberties (v2x_gencl)*: Composite measure capturing women's overall civil liberties, including freedoms of movement, expression, and association. - *Women civil society participation (v2x_gencs)*: Degree to which women are able to participate in civil society organizations and public life. - *Freedom from forced labor (v2clslavef)*: Extent to which women are protected from forced labor, servitude, and human trafficking.
Intermediate outcomes	
Universal health coverage index *(Global Health Observatory)*	0–100 scale measuring access to essential health services across multiple domains: reproductive/child health, infectious diseases, noncommunicable diseases, service capacity. https://www.who.int/data/gho
Official Development Assistance (ODA) *(World Bank)*	Net ODA received (current US$): total amount of grants and low-interest loans given to a country by governments and international groups to help grow its economy and improve living standards
Development aid for health *(IHME Development Assistance for Health Database)*	Annual estimates of financial flows directed toward health in low- and middle-income countries for the period 1990–2025. We use measures of total DAH as well as DAH specifically allocated to family planning. https://ghdx.healthdata.org/record/ihme-data/development-assistance-health-database-1990-2025
Import contraceptive commodities *(UN Comtrade)*	Import values (in US$) of contraceptive goods derived from UN Comtrade data, including condoms (HS code 401410; rubber, vulcanised, other than hard rubber, sheath contraceptives) and hormonal contraceptives (HS code 300660; pharmaceutical goods, chemical contraceptive preparations based on hormones or spermicides). We which accommodates the zeros present in a non-trivial share of country-years while approximating a log specification elsewhere. https://comtradeplus.un.org/

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Table A4 continued

Variable ***(Source)***	**Description**
Reproductive health indicator *(UN World Contraceptive Use)*	Nationally representative estimates of (i) contraceptive prevalence: any modern method (percent), defined as the share of women of reproductive age currently using modern contraceptive methods; (ii) total demand for family planning (percent); and (iii) unmet need for family planning: any modern method (percent), measuring the share of women who wish to delay or avoid pregnancy but are not using modern contraception. https://www.un.org/development/desa/pd/world-contraceptive-use
Child mortality *(World Bank)*	Under-five mortality rate, measured as the probability that a child born in a given year dies before reaching the age of five, expressed per 1,000 live births. Data are obtained from the World Bank's World Development Indicators (WDI), indicator code SH.DYN.MORT.
Women's education *(Global Data Lab)*	Mean years of schooling (MYS) for the female population aged 25 and older, reflecting the prevailing educational attainment of adult women in a society. https://globaldatalab.org/shdi/metadata/mschf/
Female labor market participation *(World Bank)*	The proportion of the female population aged 15–64 that is economically active, that is, all women who supply labor for the production of goods and services during a specified period. Data are based on International Labour Organization (ILO) modeled estimates and are available from the World Bank's World Development Indicators (WDI), indicator code SL.TLF.CACT.FE.ZS.
Contraception Policy *(Global Contraception Policy Atlas)*	National contraception policy and political leadership indicators: Restricted access: access is de facto restricted by social status (e.g. marital status, age). Stock-out monitoring: the state monitors supply stock-outs. Global financing partner: the country is a partner of the Global Financing Facility. International FP commitments: the country has made international family planning commitments. Tracking commitments: the country is on track to meet them. Commodity supply strategy: a national supply strategy is in place. Supply committee: a functional committee oversees that strategy. Essential medicines list: at least five contraceptives are on the National Essential Medicines List. Government funding: government funds are allocated to contraception. Supplies budget line: a dedicated budget line for supplies exists. Fees charged: a fee is charged in the public sector. Coverage by national health insurance: contraceptives are covered.
Other variables	
GDP per capita *(World Bank)*	Gross domestic product per capita, measured in purchasing power parity (PPP) current international dollars. Data are obtained from the World Bank's World Development Indicators (WDI), indicator code NY.GDP.PCAP.PP.CD.

Table A5—DHS surveys used in Micro-level Analysis

Country	Waves	Years
Burkina Faso	BF4, BF6, BF8	2003, 2010, 2021
Burundi	BU6, BU7	2010, 2017
Cameroon	CM4, CM6, CM7	2004, 2011, 2018
Chad	TD4, TD6	2004, 2015
Comoros	KM6	2012
Republic of the Congo	CG5, CG6	2005, 2012
Democratic Republic of the Congo	CD5, CD6	2007, 2013
Côte d'Ivoire	CI8	2021
Ethiopia	ET6, ET7	2003, 2008
Gabon	GA3, GA6, GA7	2000, 2012, 2021
Gambia	GM6, GM7	2013, 2019
Ghana	GH4, GH5, GH6, GH8	2003, 2008, 2014, 2022
Guinea	GN4, GN6, GN7	2005, 2012, 2018
Kenya	KE6, KE8	2014, 2022
Lesotho	LS4, LS5, LS6	2004, 2009, 2014
Liberia	LB5, LB6, LB7	2007, 2013, 2019
Madagascar	MD4, MD5, MD7	2004, 2009, 2021
Malawi	MW4, MW5, MW7	2000, 2010, 2016
Mali	ML4, ML5, ML6, ML7	2001, 2006, 2012, 2018
Mozambique	MZ4, MZ6, MZ8	2003, 2011, 2022
Namibia	NM4, NM5, NM6	2000, 2006, 2013
Niger	NI5, NI6	2006, 2012
Nigeria	NG4, NG5, NG6, NG7	2003, 2008, 2013, 2018
Rwanda	RW4, RW6, RW7	2005, 2010, 2019
Senegal	SN4, SN6, SN7, SN8	2005, 2010, 2018, 2023
Sierra Leone	SL5, SL6, SL7	2008, 2013, 2019
São Tomé & Príncipe	ST5	2008
Tanzania	TZ4, TZ5, TZ7, TZ8	2004, 2010, 2015, 2022
Togo	TG6	2014
Uganda	UG4, UG5, UG6, UG7	2000, 2006, 2011, 2016
Zambia	ZM4, ZM5, ZM6, ZM7	2001, 2007, 2014, 2018
Zimbabwe	ZW7	2015

Notes: This table lists all sub-Saharan African countries and corresponding DHS survey waves included in the micro-level analysis described in section VIII. Only publicly available standard DHS surveys with full birth histories and consistent coverage of key demographic variables were used. Survey years refer to the DHS interview year.