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ABSTRACT

Millions of women continue to die during and soon after childbirth, even where the knowledge and resources to avoid this are available. We posit that raising the share of women in parliament can trigger action. Leveraging the timing of gender quota legislation across developing countries, we identify sharp sustained reductions of 8–12 percent in maternal mortality. Investigating mechanisms, we find that gender quotas lead to increases in percentage points of 5–8 in skilled birth attendance and 4–8 in prenatal care utilization, alongside a decline in fertility of 6–7 percent and an increase in the schooling of young women of about 0.5 years. The results are robust to numerous robustness checks. They suggest a new policy tool for tackling maternal mortality.

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An online appendix is available at http://www.nber.org/data-appendix/w30103

1 Introduction

Maternal mortality, defined as the death of women within 42 days of childbirth, remains a looming global health problem well into the 21st century. It is estimated to account for 830 deaths per day and more than 216 deaths per 100,000 live births globally (Ceschia and Horton, 2016). In sub-Saharan Africa, the maternal mortality ratio (MMR) exceeds the rate in developed countries a century ago (Alkema et al., 2016; Loudon, 1992).[1] A woman's lifetime risk of maternal death (the probability that a 15 year old woman will eventually die from a maternal cause) is 1 in 190 today, but there are dramatic variations across the world, the risk being 1 in 5400 in high income countries and 1 in 45 in low income countries (WHO, 2019). Moreover, maternal mortality is only the tip of an iceberg, the mass of which is maternal morbidity (Koblinsky et al., 2012).[2] There is no single cause of death and disability for men aged 15–44 that is close in magnitude.

Reducing maternal mortality is of both intrinsic and functional value, as it favorably influences women's human capital attainment, employment, and growth (Albanesi and Olivetti, 2016, 2014; Jayachandran and Lleras-Muney, 2009; Bloom et al., 2015). A broad stream of research has documented the importance of population health for economic growth, via life expectancy and human capital accumulation (Soares, 2005; Weil, 2007; Ashraf et al., 2009; Shastry and Weil, 2003; Bloom et al., 2004; Lorentzen et al., 2008; Aghion et al., 2010).

Persistence of high rates of maternal mortality is striking given that the knowledge and technology needed to dramatically reduce it have been available for nearly a century, and the costs of intervention are relatively small (Cutler et al., 2006; Loudon, 1992). The causes of maternal mortality are well understood, and have not varied a lot through the course of history. Skilled care before, during and after childbirth can prevent about three-fourths of maternal deaths (WHO, 2019; Hunt and Bueno De Mesquita, 2007). Rather than obstetricians and gynecologists, this requires relatively low-cost primary care during pregnancy and midwives at delivery (Bhalotra et al., 2019; Lorentzon and Pettersson-Lidbom, 2021; Tikkanen et al., 2020). In recognition of this, the Millennium Development Goals (MDG) set in the year 2000, included as a target for 2015 universal access to reproductive health services. Progress was made, but it fell short of this target (Zureick-Brown et al., 2013).

As more than 95% of maternal mortality occurs in developing countries, a natural explanation may be that low income has constrained progress. However, there is considerable variation in levels and in rates of decline of MMR conditional upon income, see for example Ritchie (2020). Among low income countries, MMR in Rwanda is three times lower than in Chad despite it being poorer. Among high income countries, the United States has the highest MMR despite its considerable wealth.[3] Although aggregate income displays a positive association with each of female and male life expectancy, it exhibits only a weak relationship with the ratio of

[1] MMR is defined as maternal deaths per 100,000 live births. In sub-Saharan Africa in 2015 it was 547; in the US in 1936 it was 555.

[2] For every woman who dies from obstetric complications, approximately 30 more suffer injuries, infection and disabilities (Hunt and Bueno De Mesquita, 2007). In 1999, for example, the WHO estimated that over 2 million women living in developing countries remain untreated for obstetric fistula, a devastating injury of childbirth.

[3] In 2015, MMR was 26.4 in the USA compared with 9.2 in the UK and 4.4 in Sweden per 100,000 live births (Kassebaum et al., 2016). Bucking the global tide, the US has seen an increase in MMR of about 50% since 2000 (MacDorman et al., 2016; Mann et al., 2018), also see Amitabh Chandra's tweet here).

female to male life expectancy, a proxy for excess deaths of women associated with reproduction (Appendix Figure B1).[4] Overall, it seems that other factors are at play.

We put forward the hypothesis that the paucity of women policy-makers has constrained progress. In particular, we argue that male-dominated parliaments have not sufficiently prioritized maternal mortality reduction. This may reflect both preferences and information constraints. Women leaders may be innately more concerned about MMR because they identify with the risks, or have clearer information on the risks (Ashraf et al., 2020; Powley, 2007). The broad stylized facts line up with our hypothesis: since 1990, MMR has shown an unprecedented fall of 44%, a period in which the share of women in parliament has risen unusually rapidly, from under 10% to more than 20% (Figure 1a). We study whether these trends are causally related.

To do this, we leverage the abrupt legislation of reserved seat quotas for women in parliament. A wave of quota adoption swept through developing countries, with 22 countries reforming during 1990–2015, the period we analyse. We first demonstrate that quota legislation leads to a sharp increase in the share of parliamentary seats held by women. We then show that quota adoption is associated with an 8–12% decline in the maternal mortality ratio (MMR). The dynamic impacts are persistent, in fact increasing over time, consistent with women who enter parliament after quotas remaining through a five-year term, and with the efforts of parliamentarians cumulating over time. Quota impacts are increasing in the pre-intervention level of MMR, consistent with the low hanging fruit argument. They are also increasing in the share of seats reserved, and finding this dose-response relationship increases our confidence that we capture impacts of quota adoption rather than a confounder.

We use data on maternal mortality that were recently released by the WHO (Alkema et al., 2016, 2017), merged with data on gender quota legislation, the share of women in parliament and a host of other data that allow us to analyse mechanisms and confounders. The main analysis relies upon the de Chaisemartin and D'Haultfœuille (2020) estimator which produces unbiased estimates of dynamic effects when treatment effects are heterogeneous across units or time, avoiding the potential bias arising in conventional difference-in-differences estimates. The identifying assumption is that the country-specific timing of quota adoption is quasi-random. The main threat to identification is that an underlying trend in gender progressivity may have triggered quota legislation and also led to lower MMR, with the quotas having no causal impact on MMR. This would be in line with broader evidence that the timing of legislation is not random but, instead, that legislation is passed when social preferences have evolved to support it (Doepke and Zilibotti, 2005).

We probe this and other threats to identification. The placebo coefficients show no evidence of differential pre-trends. We show that the main results hold if we use estimators that make weaker assumptions about parallel trends, including a standard synthetic control approach (Abadie et al., 2010) and the synthetic difference-in-differences approach (Arkhangelsky et al., 2021) and, following Rambachan and Roth (2020), we estimate

[4] Duflo (2012) notes: "other than pre-birth and in early childhood, women are most likely to be missing relative to men in childbearing years." Of the 6 million missing women each year, 21% are in their reproductive years (Wong, 2012). Our own estimates show that GDP growth is MMR-reducing, but less effective than implementation of gender quotas.

bounds on the dynamic effects after relaxing the parallel trends assumption. We further estimate a limited information maximum likelihood (LIML) model in which MMR is regressed on the share of women in parliament, instrumented with quota legislation. Following Conley et al. (2012), we estimate bounds on these estimates, allowing that the exclusion restriction is violated, as would be the case if quota adoption had a direct impact on MMR conditional on the share of women in parliament because quota adoption proxies an underlying move towards gender equality. These results show that increasing the share of women in parliament by 1 percentage point (pp) generates a drop in MMR of 1.5–2%. Estimates of Anderson-Rubin confidence sets show that this finding holds even after accounting for the instrument being weak.

We also directly investigate the evolution of a policy environment supportive of gender equality. We find no evidence that quota adoption is preceded by an uptick in any of 18 distinct indicators of women's economic rights, civil liberties and property rights. We nevertheless also confirm that our estimates are not sensitive to conditioning upon these indicators. Our findings are not entirely surprising – while the progression of gender equality is likely to eventually culminate in increased attention to women's reproductive health, this is likely to be a slow process. In contrast, quotas that give women *instrumental* power to action improvements can produce sharp changes. We demonstrate this, showing that the decline in MMR following quota adoption is mirrored in improved coverage of reproductive health services and movements in other relevant mechanisms.

We use a similar approach to investigate democratization and seven potential drivers of quota adoption identified in the political science literature (Krook, 2010) as potential confounders. We additionally demonstrate robustness of the estimates to inclusion of country-specific linear trends and region$\times$year fixed effects. To further support identification and affirm the broad generality of our findings, we leverage the quasi-random state-level adoption of gender quotas in local government in India (Iyer et al., 2012), where we identify a significant relationship of broadly similar magnitude.

We address issues associated with the data we use. First, as the main analysis uses aggregate data, we confirm that our findings are not driven by changes in the composition of women giving birth. To do this we created a pseudo panel from the full reproductive histories of women available in harmonized microdata including 10,837,442 births of 3,079,298 individual women surveyed in 34 different years in 82 countries, 14 of which implemented quotas. We find no significant changes in the age or educational composition of mothers after quota adoption, or in the sex ratio at birth, an indicator of fetal (and hence maternal) health. We nevertheless demonstrate robustness of the results to conditioning upon the demographic composition of mothers. Second, the best available country-year data on MMR are estimated (due to gaps in vital statistics) and published with uncertainty bounds (Alkema et al., 2016). We show estimates using a double-bootstrap procedure resampling over the uncertainty intervals to calculate the standard errors. We additionally show results restricting the original sample to unmodelled (original) data and using a measure of MMR that we construct from the microsurvey data.

Turning to mechanisms, we identify quota-led improvements in reproductive health services coverage, the key determinant of MMR highlighted in scientific research and promoted by World Health Organization (WHO) guidelines (Grépin and Klugman, 2013; Kruk et al., 2016). We identify increases in coverage of skilled birth attendance of 5 to 8 percentage points (6–9.6%), of antenatal care of 4 to 8 percentage points (4.8–9.6%) and an increase in contraceptive coverage of 1.7 percent. We find no impact on adoption of abortion legislation (relevant because unsafe abortion is a major cause of MMR (Girum and Wasie, 2017; Clarke and Mühlrad, 2021)), possibly because there are religious barriers to abortion law which women leaders may not be able to surmount. We further investigate whether an increased presence of women in parliament modified demand side determinants of MMR, including women's rights, female education and fertility (Bhalotra and Clarke, 2013; Girum and Wasie, 2017). We find no discernible impacts of quotas on women's rights but we identify an increase of 0.5 years in the education of young women and a decline in the total fertility rate of 6–7%, consistent with the observed expansion of contraceptive coverage and women's schooling. In addition to being a likely mechanism for the decline in maternal death risk per birth (the definition of MMR), the decline in fertility will have a *scale effect*, tending to reduce the number of maternal deaths at any level of risk per birth. We estimate that the scale effect leads to an additional decline in the maternal death count that is roughly 64% of the impact of quotas on maternal deaths per birth.[5]

In view of broader concerns about reducing MMR in low-resource settings, a natural question is whether these results rely upon women parliamentarians raising resources. To assess this, we investigate whether gender quotas led to a reallocation of resources within the health budget, an increase in public health expenditure, and an increase in total resources. We find no evidence that gender quotas arrest declines in male reproductive-age mortality, mortality from tuberculosis (which is gender-neutral) or infant mortality – thus no evidence of substitution. We find some evidence of an increase in health spending, albeit this is sensitive to specification. We find no evidence that more resources became available – there is no impact of gender quotas on GDP or international development assistance for maternal health (DAH). We discuss why large changes in outcomes are feasible without a large increase in resources, highlighting the low costs of relevant interventions (Banke-Thomas et al., 2020; Dupas, 2011), and the slack generated by inefficiency and corruption that a motivated leader can put to use (Folbre, 2012; Brollo and Troiano, 2016; Baskaran et al., 2018). In section 7, we cite evidence consistent with leaders being able to move the outcomes they prioritize by building consensus, provoking legislation and improving policy design and delivery, including better targeting and greater outreach.

To summarize, using longitudinal cross-country data across a period of 25 years that encompasses periods of dramatic but uneven decline in maternal mortality (and additionally using cross-state longitudinal variation within India), we provide new evidence that gender quotas that raise the share of women in parliament lead to substantial declines in maternal mortality. The overall decline in MMR of 8–12% compares favorably with the 44% global decline in MMR that occurred over the 25-year period, averaged over countries that did and did not implement quotas. Similarly, the 5–8 percentage point increase in birth attendance, the 4–8 percentage point increase in prenatal care, and the 1.7 percentage point increase in modern contraceptive use that we demonstrate

[5]Our back-of-the-envelope calculations suggest that more than 8000 maternal deaths a year were averted by quotas lowering maternal deaths per birth and an additional 5669 maternal deaths a year were averted on account of the impact of quotas on the number of births.

flow from quota passage compare well with the 12, 13 and 9 percentage point increases (respectively) achieved through the recent 25 years. The decline in MMR of 44% since 1990 fell well short of the MDG target decline of 75% (Hogan et al., 2010; Kassebaum et al., 2014), and yet the new Sustainable Development Goals (SDGs) have set a higher target (of less than 70 per 100,000 live births by 2030). This is a clear flag that some policy innovation is needed – and we suggest gender quotas. Our results indicate that women leaders are more effective than men in implementing the known recipes for success in this domain.

We propose that gender quotas can be an effective policy tool for maternal mortality reduction. Current international strategy to address MMR is focused upon extending reproductive health coverage, but there is no recognition among policy makers of the political economy constraints on achieving this. We provide the first systematic analysis of the impacts of the recent wave of implementation of gender quotas across countries.[6] This is important because the broader evidence on the success of quotas is mixed (Coate and Loury, 1993; Besley et al., 2017; Pande and Ford, 2012; Niederle, 2016; Van der Windt et al., 2018).

Our findings cohere with previous evidence that increasing the share of women politicians influences policy choices in favor of policies that align with the preferences of women (Chattopadhyay and Duflo, 2004; Taylor-Robinson and Heath, 2003; Swers, 2005; Clots-Figueras, 2012; Baskaran and Hessami, 2019). It also resonates with research showing that women politicians are more likely than men to invest in public health (Miller, 2008; Bhalotra and Clots-Figueras, 2014), and that women voters prioritize public health while male voters prioritize low taxes (Campbell, 2004). In contrast to most public health outcomes, maternal mortality is unique to women and thus easy to overlook in a male-dominated parliament, but naturally targeted towards or "assignable" to women.

The rest of this paper is organized as follows. In section 2 we provide a discussion of maternal mortality initiatives and of the implementation of parliamentary gender quotas. Section 3 describes the data, and section 4 lays out the empirical strategy. We present results in section 5, robustness checks in section 6 and analysis of mechanisms in sections 7 and 8. We conclude in section 9.

2 The Policy Landscape

Maternal Mortality. Although women have been dying in and around childbirth since the origin of life, international initiatives to tackle maternal mortality are recent. The Safe Motherhood Conference held in 1987 in Nairobi was the first of a series of international meetings that highlighted the need for global action on maternal mortality. Strategies for achieving this goal included making family planning universally available, providing prenatal care and trained assistance at delivery, and ensuring access to emergency obstetric care (Starrs, 2006). Subsequent events calling for action include the World Summit for Children in 1990 and the International Conference on Population and Development in 1994. In September 2000, the United Nations General Assembly adopted the UN Millennium Declaration and articulated the Millennium Development Goals (MDGs). MDG 5

[6] See, for instance, the review by Pande and Ford (2012), who discuss the cross-country implementation of quotas, but provide evidence based only on implementation of local government quotas in India.

called for a three-quarters reduction in MMR between 1990 and 2015. A second target of achieving universal access to reproductive health by 2015 was added in a subsequent reformulation of the MDGs. The Sustainable Development Goals agreed in 2015 set new, more ambitious targets to be achieved by 2030. However, no new policies to hasten progress have been proposed.

Gender Quotas. In response to growing awareness of women's rights in civil society, in 1990 the UN Economic and Social Council set a target of 30% female representation in decision making bodies by 1995 (Pande and Ford, 2012). The passage of gender quotas followed this and accelerated after the unanimous signing of the Beijing Platform for Action by all UN delegates at the Fourth World Conference on Women in 1995 (Inter-Parliamentary Union, 2015; Krook, 2010). Since 1990, 22 countries in sub-Saharan Africa, the Middle East, and South and East Asia have implemented constitutionally protected quotas reserving seats in parliament for women, mostly after 1995. We observe an uptick in quotas particularly after year 2000, driven by sub-Saharan Africa.[7] The existing evidence on reserved seat quotas is dominated by analysis of the randomized implementation of gender quotas (for headship together with council membership) in local government in India.[8] We also provide estimates for the Indian quotas, alongside our analysis of women's membership of parliament.

The focus of our study is on these reserved seat quotas but we shall provide a brief analysis of impacts of candidate quotas on women's share in parliament and MMR. Since 1990 the number of countries with candidate list quotas for women has also risen sharply, from 1 to 46.[9] Candidate quotas were passed mostly in middle- and higher-income countries (see Appendix Figure A1) and have weaker impacts on representation, see for example, Bagues and Campa (2017).

3 Data and Descriptive Statistics

Maternal Mortality. Maternal mortality was not consistently measured until recently, imposing an impediment to evidence-based prevention efforts. The MDGs set quantitative targets to be monitored, and this triggered a multi-agency effort to gather data on MMR. In this paper we use the first harmonized time series estimates of MMR across 183 countries, released in 2015 by the United Nations Maternal Mortality Estimation Inter-Agency Group (MMEIG), covering the period 1990–2015. These estimates combine available data from vital statistics, special inquiries, surveillance sites, population-based household surveys, and census files. They use Bayesian methods to combine these data and fill gaps (Alkema et al., 2016, 2017). We conduct a sensitivity check that

[7] The countries implementing quotas are: Afghanistan, Algeria, Bangladesh, Burundi, China, Djibouti, Eritrea, Haiti, Iraq, Jordan, Kenya, Morocco, Niger, Pakistan, Rwanda, Saudi Arabia, South Sudan, Sudan, Swaziland, Tanzania, Uganda and Zimbabwe. Uganda is the only country which reserved seats before 1990, in 1989. Four other countries: Samoa, Kosovo, Somalia and Taiwan have implemented quotas during 1990–2015, but are left out due to data restrictions. Three other countries implemented quotas more recently and fall outside our study period: Nepal in 2016, UAE in 2019 and Egypt in 2020.

[8] In their pioneering study, Chattopadhyay and Duflo (2004) showed that women leaders were responsive to the needs of women citizens, as expressed in council meetings. Direct observation of council meeting minutes confirms this level of interaction (Parthasarathy et al., 2019), also see Iyer et al. (2012).

[9] Candidate quotas set a minimum for the share of women on candidate lists, either as a legal requirement or a measure written into the statutes of individual political parties.

allows for this in inference, and we also show results using MMR data derived from the Demographic and Health Surveys (DHS).

Gender Quotas and Women's Share in Parliament. We collected information on country-specific adoption of quotas up until 2005 from Dahlerup (2005), and updated these to 2015 using the Global Database of Quotas for Women. The data include the date of legislation and the share of seats reserved for women. We obtained the share of women in parliament from the World Development Indicators (WDI), the UN Millennium Development Goals (MDG) Indicators and the ICPSR dataset compiled by Paxton et al. (2008). Figure 1b shows that aggregate trends in women's share in parliament track trends in quota coverage.

Other Data. Data on a range of intermediate outcomes (mechanisms) and controls, including measures of women's rights or gender equality, political variables and indicators of reproductive health coverage were compiled from diverse sources, see Appendix A, where we also discuss the MMR and micro-fertility data drawn from the DHS, and state-level quota adoption, women in government, and MMR data for India.

Descriptive Statistics. The analysis sample contains (at most) 178 countries, through 1990–2015. Table A1 provides summary statistics. Appendix Figure B2 plots the world distribution of average MMR in the analysis period. The geographic spread of reserved seat quotas is in Figure A1 and the trend in gender quota implementation in Figure A2. Quota size varied across countries and Figure B3 displays the distribution. The median (mean) gender quota is 21% (20%). Casual inspection suggests support for our hypothesis that reserved seat quotas are associated with MMR decline. Comparing country pairs with similar GDP per capita in 1990, selecting one which implemented reserved seat quotas before 2010 and one which did not, we find that the quota-implementing country tends to witness a larger decline in maternal mortality in 1990–2010. Thus, Burundi did better than Malawi, Kenya did better than Zimbabwe, and Niger did better than the DRC. A more formal approach is discussed next.

4 Empirical Strategy

The share of women in parliament has increased fairly smoothly (Figure 1a), making it hard to isolate its effects from those of other gradually evolving trends. We therefore leverage the abrupt implementation of quotas. The main results are obtained using the de Chaisemartin and D'Haultfœuille (2020) estimator. However, we also provide the standard event study estimates, the Goodman-Bacon decomposition, bounds following Rambachan and Roth (2020) and estimates based on synthetic controls (Abadie et al., 2010; Arkhangelsky et al., 2021). We begin by setting out the standard event study model (Jacobson et al., 1993), that allows us to track outcome trends before and after quota implementation. The estimated equation is:

$$\begin{aligned} Y_{ct} &= \alpha + \sum_{l=2}^{10+} \beta_l^{lead} Quota_c \times 1\{lead_t = l\} + \sum_{k=0}^{10+} \beta_k^{lag} Quota_c \times 1\{lag_t = k\} \\ &\quad + \boldsymbol{X_{ct}} \gamma + \mu_t + \phi_c + \varepsilon_{ct}. \end{aligned} \tag{1}$$

The outcome Y_{ct} varies at the country c and year t level. The outcomes are initially the proportion of women in parliament (first stage compliance) and the natural logarithm of the maternal mortality ratio. We model a series of additional outcomes including intermediate outcomes (potential mechanisms), placebo outcomes and potential confounders. $Quota_c$ is 1 if a country ever adopted a quota, and this is interacted with a full set of leads and lags with respect to the year the quota was adopted. We include 10 lags and leads, the tenth term including all years greater than 10, and the first lead is omitted as the base category. We provide results varying this window down to 5 and 8 years. We include country and year fixed effects (ϕ_c and μ_t), and cluster standard errors at the country level (Bertrand et al., 2004).

Parallel trends. The β^{lag} coefficients capture dynamic impacts and the β^{lead} coefficients provide a partial test of the identifying assumption of no differential trends. This is only a partial test because, to estimate unbiased parameters we require parallel trends between treated and non-treated units *in the absence of treatment*. Parallel pre-trends support this assumption but cannot be used to test what would have happened at the time of the reform had the reform not been implemented (Kahn-Lang and Lang, 2018). In view of concerns about the inclusion of unit-specific linear trends (Goodman-Bacon, 2019), we use the Rambachan and Roth (2020) "Honest DiD" estimator that provides bounds on the post-quota coefficients under the scenario that any prevailing (even if imprecisely estimated) trends in the pre-treatment period between quota and non-quota countries are projected forward into the post-quota period (rather than assuming parallel trends going forward). We also use two estimators that make weaker parallel trends assumptions than our main approach. We implement the strategy of generating a synthetic control for each treated country (following Abadie et al., 2010; Cavallo et al., 2013) by matching on pre-quota trends in maternal mortality, giving preference to countries within the same sub-region of the world (see Appendix B). This synthetic control procedure chooses for each treated country its own synthetic control, and then pools pre- and post-treatment differences across countries to generate a single event-style plot. We additionally use the recent synthetic difference-in-differences approach of Arkhangelsky et al. (2021), which allow us to generate a synthetic counterfactual to optimally align pre-treatment trends for *each* quota year specific adoption group, resulting in a treatment vs. synthetic counterfactual comparison for each quota adoption period, as well as an aggregate ATT estimate.[10]

Staggered adoption. A series of recent papers analyze the inference problem when treatment is staggered across units (countries) over time, creating multiple experiments. If there are heterogeneous treatment effects across countries or time, estimates obtained using the conventional difference in difference estimator may be biased.[11] We address this by using using the dynamic estimator of de Chaisemartin and D'Haultfœuille (2020),

[10] Arkhangelsky et al. (2021) provide a computational implementation of their methods for a single adoption period, and describe how the theory underlying this estimator extends to staggered adoption designs in their Appendix A. We implement this staggered adoption estimator, extending their code to house multiple adoption dates, as well as valid block bootstrap inference for the aggregate ATT estimate (Pailañir and Clarke, 2022).

[11] Already treated units can act as controls for later treated units because their treatment status does not change. However, if there are changes in treatment effects over time, these get subtracted from the TWFE estimate, potentially biasing the single coefficient estimator away from the true treatment effect. Goodman-Bacon (2021) shows that the usual fixed effect estimator recovers a weighted average of all possible pairs of the underlying TWFE estimator. Extending this work, de Chaisemartin and D'Haultfœuille (2020) demonstrate that when treatment effects are heterogeneous, some of

which provides unbiased estimates. It uses groups whose treatment status is stable to infer the trends that would have affected switchers if their treatment had not changed. Specifically, we report their 'DID$_M$' estimate, which is based on a series of time-specific aggregated estimates. This estimator considers changes immediately surrounding quota adoption. It can be extended to estimate full dynamic impacts if rather than considering changes between t and $t-1$, we consider changes between $t+k$ and $t-1$ for $k = 1, 2, \ldots, 10$ years post reform, and similarly, placebo estimates can be presented showing changes between pre-reform periods[12] in which quota adopters have not yet been exposed.[13] Inference is consistently based on a block bootstrap procedure, resampling over countries. Given the preferred properties of the de Chaisemartin and D'Haultfœuille (2020) estimator in cases with heterogeneous impacts of treatment, we use this estimator as our main specification. Standard event studies described in equation 1 are provided as supplementary results.

Sensitivity to estimator. We supplement the dynamic models described above with single coefficient estimates of a two-way fixed effects (TWFE) specification in which the independent variable is defined as one for all years following the implementation of a quota for implementing countries, and zero before. It is set to zero for all countries that do not implement quotas in the sample period. The TWFE specification will tend to estimate an average of treatment effects that over-weights short-run effects and under-weights long-run effects (Borusyak and Jaravel, 2017). Since our event study estimates show that treatment effects increase with time since the quota event, in our setting the TWFE model will tend to produce conservative estimates. We provide a formal decomposition of the two-way FE estimator following Goodman-Bacon (2021), quantifying how much identifying variation is drawn from a pure treatment versus control comparison and how much is drawn from variation in treatment timing. We additionally document how this single coefficient estimate compares to pooled coefficients from the event study, the aggregate dynamic estimates from de Chaisemartin and D'Haultfœuille's "DID_M" estimator, and estimates using the synthetic difference-in-differences approach of Arkhangelsky et al. which make comparisons in a more 'local' way, giving more weight to control units which share more similar past trends with treated units, and more weight to time periods which are more similar to treated periods. Both the "DID_M" estimator and the synthetic DID estimator are robust to the biases inherent in standard two-way fixed effects models.[14]

Additional robustness checks. We directly investigate the threat to identification posed by the possibility that quota adoption responds to societal preferences becoming more pro-female, and that it is these underlying shifts in preferences that drive MMR decline. We similarly investigate democratization and a series of predictors

these weights might be negative.

[12] We follow de Chaisemartin and D'Haultfœuille (2020) precisely. Our main estimates present pre-treatment "placebo" estimates considering shifts by one period at a time, *e.g.* from pre-period $t-j$ to pre-period $t-j+1$. As a robustness check, we show a model with the "long placebos" defined in de Chaisemartin and D'Haultfoeuille (2022), considering all shifts compared to the final pre-treatment period.

[13] The conventional event study pre-trends test (Autor, 2003) has been shown to be invalid when treatment effects are heterogeneous (Abraham and Sun, 2018) but placebo coefficients estimated following de Chaisemartin and D'Haultfœuille (2020) are robust to this.

[14] Nevertheless, the two-way fixed effects model provides a useful summary statistic. It is also helpful in assessing results for some intermediate outcomes (mechanisms) for which the data are relatively sparse, and when we use the Rambachan and Roth (2020) and IV estimators to compute bounds.

of quota adoption discussed in the political science literature. We replicate the results for the staggered adoption across the Indian states of gender quotas in local area councils. This is useful not only because it is another setting but also because, in this case, quota implementation has been shown to have been quasi-random. The main estimates include the many (high and low income) countries that do not pass quotas in the sample period, as this expands the set of good comparisons available to identify trends (Borusyak and Jaravel, 2017). In different specification checks, we first drop the 51 (never treated) high income countries from the sample, and then restrict further to African countries, thus making the sample more homogeneous.[15] The baseline estimates control only for country and year fixed effects. We display sensitivity of the estimates to adding a series of controls, $\boldsymbol{X_{ct}}$, including country-specific linear trends, region-year fixed effects, education and demographic characteristics of mothers, ethnic fractionalization and potentially endogenous intermediate outcomes including international development assistance for maternal health, GDP, public health expenditure, indicators of women's rights and gender equality, and political predictors of quota implementation. The potentially endogenous variables are consistently included as the baseline (pre-quota, in fact pre-1995) value of the variable multiplied by an indicator for post-quota years. This allows us to capture their direct impact on maternal mortality, also allowing that this impact changes after quota adoption. We also show results restricting to a balanced panel and we address uncertainty in the MMR data by removing from the sample all modelled data, adjusting inference for the uncertainty, and by using the DHS to construct comparable survey data measures of MMR.

5 Results

Main results. The de Chaisemartin and D'Haultfœuille (2020) estimates are presented in Figure 2. They are obtained by aggregating estimates of outcome changes between adopters and non-adopters, comparing periods surrounding adoption. Panel (a) shows a discrete jump in women's parliamentary representation in the year after quotas are implemented. Panel (b) similarly shows a break in the coefficient series, with maternal mortality falling more rapidly after quota implementation. Panels (c) and (d) show that the patterns are broadly similar in models with no time-varying controls. The post-quota changes are persistent. Figure 3 shows numerous specification checks, which we elaborate in the next section. The standard event study estimates (equation 1) are appended as Figure A3 and they too provide broadly similarly estimates.

Table 1 demonstrates that our results are not sensitive to the choice of estimator. We show estimates from the two-way fixed effects single coefficient model, the weighted average of the dynamic coefficients from de Chaisemartin and D'Haultfœuille (2020), where the weights are the number of countries contributing to the coefficient for each year, and estimates from the synthetic difference-in-differences model. Following quota adoption we see an increase in the proportion of women in parliament of 5.7 to 6.6 percentage points which, relative to the baseline average in 1985–1990 of 9%, represents about a 64% increase. We see a fall in the maternal mortality ratio of 7.2 to 12.7%.[16]

[15] Our estimates account for heterogeneity across countries. Still, in an earlier working paper Bhalotra et al. (2019), we document results weighting by country population and the broad patterns are similar.

[16] The median (mean) gender quota is 21% (20%). The estimated impacts of quotas on the proportion of women in par-

After quotas are legislated, the share of women in parliament can only change at the next election. For every country, we identified the years between quota legislation and the next election. The mode and median are zero years, the mean is 1.3.[17] The event study plot shows a slight jump five years after the quota is passed, consistent with the subsequent election presenting a further opportunity to increase the share of women. Once women are in parliament, any changes they make will need to translate to the field and cumulate to have a discernible population-level impact on maternal mortality. Our finding that impacts of quota adoption on MMR increase with time since quota in Figures 2 and A3 are consistent with this. Ten years out, MMR was 13% lower in countries that passed quotas.

Our estimates are of a similar order of magnitude to the historical introduction of occupational licensing requirements for U.S. midwives (Anderson et al., 2020), the 10-year impact of Brazil's Bolsa Familia conditional cash transfer program (Rasella et al., 2021), and the 4-year impact of Brazil's expansion of community-based primary care (Bhalotra et al., 2019). In contrast, our estimates were larger than those from a large-scale program in India providing financial incentives for institutional birth (Janani Suraksha Yojana), which had no impact on maternal mortality (Powell-Jackson et al., 2015), and about 40% as large as the impact of introducing antibiotics to treat high-risk, childbirth-related maternal infections in America in the late 1930s, when MMR was at a similar level to modern day Sub-Saharan Africa (Jayachandran et al., 2010).

The placebo coefficients are tightly centered on zero, showing no prevailing differential trends in MMR prior to quota adoption.[18] We show in the next section that the bounds on the dynamic effects on MMR are consistent with our main findings when we relax the pre-trends assumption, and that our estimates hold with the synthetic control and synthetic DiD approaches. We also show that our findings hold in bounded IV estimates that allow that the exclusion restriction is violated and that the instrument is weak. We examine numerous potential predictors of quota adoption and show that they do not trend upward before the event of quota adoption, that they do not predict MMR and that our estimates are robust to controlling for them.

Heterogeneity in impact. Although international conventions suggested a target of 30%, the mandated quota varied across countries, see Figure B3 and Table B1. Leveraging this variation, we find a clear "dose-response" (Figures B7 and B8). This increases our confidence that we identify impacts of quotas rather than an omitted variable, and it is also relevant to policy making. Estimates from the single-coefficient model (Table A5, panel B) indicate that quotas of less than 10% increase the share of women in parliament by 2.8 percentage points, and generate a 0.6% reduction in MMR, while quotas of 20–30% increase the share of women in parliament by

liament are smaller than the entire size of quotas. In quota implementing countries the pre-quota share of women in parliament was not always zero, the average was 7.9%, rising to 20.9% post-quota (median: 6.2 and 21.0%). Taking all countries, the mean was 14.1%, median 11.5% (see Figures B5a and B5b for full distributions). See Figure B6 for temporal variation by country. In Rwanda we see a jump in line with quota legislation but from a high baseline, while Djibouti shows a sharp jump from zero to quota attainment. The event studies of course show the unrestricted dynamic coefficients. We also show a series of estimates leaving out one country at a time.

[17] In a few cases, it took time from quota passage until fulfillment. In Niger, for instance, the quota was in 2000 but the next election in 2004.

[18] This is also the case when we use the long placebo indicated in de Chaisemartin and D'Haultfoeuille (2022), see Appendix Figure B4.

6.8 percentage points and reduce MMR by 13.4%. Baseline shares of women in parliament varied, as a result of which a given quota size had different impacts on the increase in the share of women in parliament. Baseline rates of MMR also varied considerably and we find larger declines where base rates were higher and there was more room for reduction. Estimates using terciles of baseline MMR are in Figure B8, panel B. We find a 7.7% reduction in MMR in countries with a low baseline rate and a 15.9% reduction in countries with a high baseline rate (Table A5, panel A).[19]

Candidate list quotas We used a similar approach to investigate impacts of candidate list quotas. These result in significant but smaller increases in women's share in parliament than reserved seat quotas. This is what we would expect since candidate quotas do not guarantee seats in parliament, also see Pande and Ford (2012); Bagues and Esteve-Volart (2012). We find no impact on MMR (Appendix Figures B9–B10). This seems plausible both because of their smaller impact on women's political representation, and because the countries implementing candidate list quotas during the study period (predominantly in Latin America) had achieved dramatic declines in MMR prior to quota implementation. This is backed by our estimates for reserved seat quotas showing quota impacts decreasing in baseline MMR, consistent with diminishing returns to policy intervention.[20]

Sub-national estimates for India. Implementation of gender quotas in India was randomized at the village level, creating an opportunity for identification of their impact. However there are no MMR data at the village level. We leverage the fact that state level legislative reform implementing the village level gender quotas has been shown to be as good as randomly assigned (Iyer et al., 2012). Using the staggered adoption of quotas across India's states yields the estimates in Figure A15. Although less precise given the more limited variation available, these estimates, which rely on a source of identification that is distinct from our main cross-country analyses, confirm our main finding of quota-led declines in maternal mortality of 14.2% (Table B11). This result is robust to leaving out one state at a time, see Figure B27.

6 Robustness Checks

Alternative estimators and pre-trends. The placebo coefficients in the main results shown above are not significantly different from zero. The estimates stand up to including country-specific linear trends and region×year fixed effects (Figure 3). However, if standard tests of pretrends are underpowered, we might fail to capture the evolution of a relevant unobservable trend. To address this concern, we follow the "Honest DiD" procedure of Rambachan and Roth (2020) and estimate upper and lower bounds on the dynamic effects For both women in parliament (Figure 4a) and MMR (Figure 4b) these bounds are informative at 95% or, in some cases in later

[19]We examine variation in baseline MMR and quota size jointly in Table A5 panels A and B respectively, see columns 5 and 8.

[20]If we were willing to assume that any omitted trends predicting quota implementation are the same for candidate and seat quotas then our finding no impact on MMR of candidate quotas serves to undermine the concern that omitted variables drive the result that reserved seat quotas lead to MMR decline. In any case, we consider omitted trends carefully in the next section.

periods, at 90%.[21]

Motivated by the possible concern that countries that do and do not adopt quotas are likely to be quite different, we generate synthetic controls for each treated country so as to achieve a match on pre-treatment levels of maternal mortality, drawing controls from the same geographic region as the treated country, and hence potentially subject to similar regional shocks. We aggregate estimates of treatment leads and lags across all treatment–synthetic control pairs, and implement an inference procedure based on clustered permutation to generate confidence intervals for these estimates. Appendix B details the procedures. The results in Figure 5 show no evidence of differential pre-trends, and a significant post-reform decline in MMR. We further re-estimate the models using the synthetic difference-in-differences procedure of Arkhangelsky et al. (2021) and we observe, if anything, slightly larger effects, see panel D of Table 1. This procedure involves generating quota adoption year specific synthetic controls, placing more weights on countries and years which are more similar to treatment countries and periods. A selection of year-specific treatment units and synthetic controls is presented in Appendix Figure B11, along with details of their construction. These figures reveal gradual declines in MMR in treated units that are larger than in the respective synthetic controls.

Table 1 shows that summary effect sizes from alternative estimators are fairly similar in magnitude. This suggests that, in our setting, the potential bias in the single coefficient two-way FE model discussed in Goodman-Bacon (2021) and de Chaisemartin and D'Haultfœuille (2020) is in fact small. To illuminate this we provide the Goodman-Bacon (2021) decomposition of the identifying variation into its treatment vs. pure control and differential timing components (Table A3). We find that 95.4% of the estimated effect derives from the double-difference comparison of treated with never-treated units. The drop in MMR (of about 7%) is similar when we compare early to late adopters (prior to adoption) to that obtained when comparing aggregate TWFE estimates of treated vs. never treated countries, albeit the weight attached to the latter is much greater (panel (b)).[22] The reason that results from two-way FE are close to those from the alternative estimators which more completely isolate the pure treatment versus control comparisons is that the majority of weights in the decomposition are attached to comparing treated to pure control units in a TWFE setting, with the share of units with 'negative weights' (de Chaisemartin and D'Haultfœuille, 2020) being small (Table 1). Figure A4 also reveals that the treatment versus pure-control estimates are quite closely clustered around the average effect (indicated by the dashed red line), which suggests that the observed reduction in MMR is observed broadly, rather than being driven by outliers. Indeed, when we present leave-one-out estimates in Figures B12–B15, these are indistinguishable from the main results, establishing that they are not driven by any particular country.

So far we have displayed reduced form estimates. We also estimated LIML regressions of MMR on the share

[21] This exercise runs off the TWFE estimator. Below, we discuss that, in our setting, it delivers estimates similar to the preferred estimator.

[22] On the other hand the TWFE estimate of late vs. earlier adopters (whose treatment status remains fixed at 1) is virtually zero – which makes sense given that both units have now adopted quotas in this comparison. A very small portion of the estimator comes from comparing treated units with a unit that adopted prior to the beginning of our data (Uganda adopted in 1989), and once again the pooled estimate here is negligible which again makes sense as both units have adopted quotas.

of women in parliament, instrumented with quota implementation.[23] If quotas were proxying an omitted variable the exclusion restriction would fail, but inference can still proceed on the premise of "plausible exogeneity", which delivers bounds on the IV estimates (Conley et al., 2012). The estimates in Table A4 provide the scaled impact of women's parliamentary representation among compliers. The IV point estimates indicate that a 1 percentage point increase in women's share in parliament is associated with a 1.5 to 2.0% decrease in MMR. In estimating bounds, we allow the adoption of quotas to have a direct impact on MMR of up to -1% over and above its impact on MMR via women in parliament. The estimated bounds are informative, indicating a 0.01% to 3.7% reduction in maternal mortality for a 1 percentage point increase in the share of women in parliament. We provide weak IV robust Anderson-Rubin confidence sets in Table A4 and plot the implied rejection probabilities for the null across a range of null hypotheses in Figure B16, both of which suggest that the observed relationship between women in parliament and MMR is robust to corrections for weak instruments. These results add to the overall weight of the evidence provided in this section.

Women's rights as predictors of gender quota legislation. The multiple approaches to investigating pre-trends discussed in the preceding section allay the key identification concerns. We nevertheless directly investigate the possibility that our results derive from social preferences evolving gradually to favor gender equality, with gender quota legislation being one manifestation of this. This is plausible in view of historical evidence that laws are often passed once society is ready to comply with them (Doepke and Zilibotti, 2005; Platteau and Wahhaj, 2014). To measure social preferences and gender progressiveness in the policy environment, we pulled together data on 18 indicators of gender progressivity in the political, economic and civil domains including indices of women's civil liberties, access to justice, economic rights, women's protests and the passage of abortion law (the full set of indicators is visible in the Figures and defined in the Data Appendix A). To examine the concern that the timing of quota adoption is a response to an upward drift in women's position, we use the same empirical strategy as for the main analysis, focusing on whether any of these 18 indicators shows an uptick prior to quota adoption. The estimates are in Figures A5 and A6 (and the equivalent event study estimates are in Figures A7 and A8). The placebo coefficients of the de Chaisemartin and D'Haultfœuille estimates allow us to reject a positive pre-trend for each of the 18 indicators – and thus to reject that quotas were adopted following improvements in other measures of equality for women. We bolster this evidence by regressing quota adoption on the full set of empowerment variables. We provide the F-statistic for joint significance of these variables, showing that it is small (Table A2).

Our findings are plausible. While the progression of gender equality in society is likely to eventually culminate in increased attention to women's reproductive health, this is likely to be a slow process. In contrast, we discover that giving women instrumental power to directly influence policy can effect sharp change. We will pursue evidence of this in the section on mechanisms, where we use the same identification strategy to test for impacts of discontinuous changes in women's political power on a series of intermediate outcomes that (a) can

[23]The instrument does not always pass a weak instrument test. We use LIML as the instrument is weak and LIML tends to be more robust to weak instrument bias. As our estimates are just identified, the weighting parameter is close to one and LIML converges towards two-stage least squares.

be influenced by policy and (b) are known to bring MMR down. In contrast, as discussed above, few of the 18 indicators of women's equality are associated with MMR decline (Tables B2–B3).

One may be concerned that the findings reflect noisy measurement of the progressivity indicators. This concern is allayed in the specifications that involve putting them together on the right-hand side, or creating an index of them. It is further mitigated by evidence that the measures are meaningful insofar as we see improvements *after* quotas are legislated in indices of women's political rights, political participation, power distribution by gender, and exclusion by gender. We discuss the post-quota (dynamic) coefficients in the Mechanisms section, as any changes that occur after quota adoption potentially reflect mechanisms by which quota adoption leads to MMR decline.

Political variables as predictors of gender quota legislation. If not women's rights then what is it that determines adoption of quota legislation? We could find no systematic quantitative analysis of this question, but, case studies in the political science literature indicate the possible relevance of pressure from international organizations (proxied by overseas development assistance), occasions of broader constitutional reform including regime transitions post-conflict reconstruction and the presence of peace-keeping forces (Krook, 2010; Baines and Rubio-Marin, 2005).[24] One might imagine that these same factors may have had direct impacts on MMR decline. To establish whether the estimated impacts of quota adoption might instead reflect political changes, we follow the same approach as for women's rights, scrutinizing pre-trends in seven measures of the political environment, see Figure A9 (and see Figure A10 for the standard event studies). There are no significant placebo coefficients. The concern that our results are driven by political variables is further undermined by their bearing no clear association with MMR decline (Table B4).

Controlling for potential predictors of quota adoption. As discussed, we find no evidence that the 25 potential predictors, including women's rights and the political variables, predict quota adoption, or that they predict MMR. We nevertheless control in the main analysis for an index of the baseline (pre-quota, 1995) value of the indicators interacted with a dummy for post-quota years.[25] We find that the estimated impact of quotas on MMR is larger and not statistically significantly different from the baseline estimate, see Figure 3, as well as full dynamic estimates with inference in Figures A11 and A12. We also tested a series of placebo trend breaks, three to ten years before the date of quota legislation in each country, and find no evidence of a pre-legislation break in the trend in MMR (Table B5).

Endogenous changes in the composition of women giving birth. If gender quotas lead to a shift in the composition of births such that women with lower baseline risks of maternal death are over-represented after quotas, then this could explain our finding of lower MMR. A compositional shift is not implausible given that we

[24]Rwanda is a case in point. Women make up 62 percent of Rwanda's national legislature, the highest share in the world, and this happened as part of a major constitutional reform in the wake of the genocide. In Nepal there was similarly a major jump in the share of women in parliament in 2008 to 32.8% in the wake of political transformation.

[25]We use standardized versions of the underlying variables. We create separate indices, one for women's rights, one for the political variables and one for resources, which includes GDP, health expenditure per capita, and overseas development assistance for maternal health. We show that the results are not sensitive to whether we use these pre-quota indices times post-quota, or if instead we just include contemporaneous measures of the variables, see Table A6.

find an impact of quotas on fertility (Section 7). We cannot investigate compositional change using aggregate country-year data on fertility, we need information on mother characteristics. To achieve this, we created a psuedo panel of births in the DHS data based on 10,837,442 births for 3,079,298 individual women from 82 countries surveyed in a total of 34 different years.[26] We model quota impacts on birth rates of women of different education and age categories, and on the sex ratio (male/female) of births, a proxy for maternal health (Waldron, 1983; Low, 2000). We find no significant shifts in composition by any of these measures, see Figures B21–B24.[27] Our estimates are robust to controlling for time-varying measures of the age and educational composition of mothers, see Figure 3 and Figure B19 panel d).

Measurement of maternal mortality. Since MMR varies considerably across countries, proportional changes implied by using logarithms will exaggerate achievements in countries with lower baseline rates (Deaton, 2006), and we showed that treatment effects exhibit heterogeneity by baseline rates. We estimated an alternative model replacing the logarithm with the level of the MMR ratio, and the results hold (see Table A5).[28] A second issue is that the MMR data we use derive from vital statistics and demographic survey data, with gaps filled using modelled predictions (Alkema et al., 2017, 2016; World Health Organization, 2015; Wilmoth et al., 2012). About 76% of the country-year observations are original survey data points, the remaining 24% being imputed. The data come with uncertainty intervals. We examine sensitivity of our estimates to this using three approaches. First, Figure 3 shows that removing countries for which all observations are imputed has no substantive impact on the findings. Second, we directly account for this uncertainty, using a double-bootstrap procedure re-sampling over the uncertainty intervals to calculate the standard errors.[29] The results are in Table A7. Allowing for correlation within country reduces the estimated uncertainty. For the preferred de Chaisemartin and D'Haultfœuille (2020) DID$_M$ estimator and the synthetic DID, with a triangular resampling procedure, the estimates tend to uphold the main results.[30] Third, we use an alternative measure of MMR which we derive

[26]The DHS are available for 14 of 22 reforming countries and 59 of 156 non-reforming countries. Construction of the pseudo-panel on fertility is described in the supplementary data section, and total fertility rate (TFR) is constructed as a weighted average of age-specific fertility rates.

[27]The sex ratio at birth is also an indicator of sex-selective abortion. If women leaders acted to inhibit the selective abortion of girls then we would expect to see an increase in the proportion of girls at birth. On the other hand, we show that women leaders improve prenatal care coverage, and this should produce an increase in the share of boys, as boys are known to be more vulnerable to adverse fetal conditions (Waldron, 1983; Low, 2000). On balance, we see no change. This may in fact just reflect that sex-selective abortion, while important in India and China, has not been a salient issue in sub Saharan Africa.

[28]The estimated impact is larger on account of outliers. If we winsorize MMR at 500 deaths per 100,000 live births, top-coding 806 observations with values above 500, the effect sizes in the levels model broadly agree with effect sizes in logs, see Table B10 (where we additionally document other cutoffs, as well as dropping rather than winsorizing at the top end).

[29]We initially resample observations in a (clustered) bootstrap over the countries in the original panel. We then resample the particular MMR realization for each country from within the entire uncertainty interval reported along with the official MMR statistics. This accounts for the (normal) sampling variation, and the uncertainty in the MMR data series. Appendix C describes the re-sampling algorithms and assumptions. Table A7 replicates the estimates from Table 1, showing p-values for the impact of quotas on MMR associated with a range of re-sampling procedures that reflect different assumptions relating to the distribution of maternal mortality in the uncertainty intervals presented by the MMEIG, detailed in Appendix C.

[30]Assuming a normal distribution is more demanding, and the revised p-values tend to be higher if we use the two-way FE model or the level rather than the log of MMR.

from survey-based reports of sister deaths of DHS respondents, following the procedure detailed in Bhalotra and Clarke (2019). The DHS maternal mortality module was implemented for 44 countries, of which 11 implemented quotas, and it covers the analysis period, 1990–2015. Using these data, we find a similar pattern of results (Figures A13 and A14). While less precisely estimated, the effects are larger, consistent with the DHS countries having higher MMR on average.

Another possible concern is that the availability and quality of MMR data is endogenous. If women parliamentarians, motivated by a concern to reduce MMR, improve surveillance and tracking of MMR ("we measure what we treasure"), this will render our estimates conservative. For instance, if women parliamentarians act to expand surveillance coverage by counting maternal deaths in remote under-developed areas then, other things equal, measured MMR will tend to increase. However, in principle this could go the other way if women politicians face more scrutiny of MMR and they react by pulling back on counting maternal deaths. Alternatively, if women politicians act to reduce the variance of (mean zero) measurement error in MMR, this could make finding significant effects more likely. We acknowledge this problem without directly addressing it, but the fact that we identify mechanisms consistent with actual reductions in MMR suggests that our findings are less likely to be driven by changes in MMR measurement.

Sensitivity to sample and clustering. We explained earlier that including never-treated countries in the estimation sample aids identification of dynamic effects. We nevertheless assessed sensitivity to dropping the 51 high income countries and found that this yields essentially identical estimates (Figures B17–B20), consistent with the MMR profile of these countries being relatively flat.[31] When we further restrict the sample only to countries in Africa, we again find results consistent with those in the full sample (Figure 3), consistent with the majority of countries adopting quotas being African. To assess sensitivity to changes in the composition of countries in the panel, we dropped the 7 countries that passed quotas after 2005 to create a balanced sample with the baseline window of 10 years pre and post-quota. The estimates are again unchanged (Figure 3). Estimates based on shorter time-horizons of 5 and 8 years pre- and post-quota adoption also agree with the baseline results using a 10 year window (Tables B7, B8 and B9).

In Section 7 we will analyse mechanisms. For some mechanisms variables, the data are sparser than for the main results. We therefore re-estimate the main results on the common sample, see Figure B26. The results are less precise in this smaller sample but the broad patterns remain. Inference in our main specifications treats the data as independent across countries, but not within countries. To address the potential concern that quota implementation was temporally correlated, we estimate event studies with two-way clustering (Cameron et al., 2011) of standard errors by both country and by year, see Figures B19–B20. While the confidence intervals are now wider, we still observe statistically significant effects.

[31] The sum of negative weights increases by 50% when we remove high income countries, but it remains small.

7 Mechanisms

Reproductive health coverage. We investigate whether gender quotas influence the reproductive health services that have become conventional wisdom for policy (WHO, 2014; Jamison et al., 2013), based on scientific consensus on their relevance to MMR reduction. Antenatal care is critical to identifying life threatening conditions such as pre-eclampsia and eclampsia early on, and having births attended by a skilled professional can reduce mortality from uterine bleeding and post-partum infection (WHO, 2014; Jamison et al., 2013).[32] Contraceptive coverage may reduce fertility, and high fertility is a proximate cause of MMR (Girum and Wasie, 2017).[33] Contraceptive coverage can also lower MMR without changing fertility by lengthening birth spacing or by substituting unsafe abortion (Miller and Valente, 2016).

Figure 6 panels (a)–(c) shows increased rates of coverage along these three dimensions of reproductive health in the years following quotas (and this also holds in the standard event study, see Figure A16). The single coefficient models (Table B12) show statistically significant increases in the share of coverage (in percentage points) of 5.8 in skilled birth attendance, 4.7 in prenatal care and (less precise estimates) of 1.7 in modern contraceptive use.[34] Univariate descriptive associations of MMR with reproductive health coverage indicators on our analysis sample are in Table B13. A 1 percentage point increase in the share of attended births, prenatal care and access to contraception respectively is associated with declines in MMR of 4.4%, 4.0% and 6.3%, magnitudes that make the identified impacts on MMR plausible.[35]

Abortion legislation, women's rights, and women's economic participation. We investigate whether political power for women led to pro-female legislation being enacted, or to women being more likely to join the labor force. We find no evidence that quotas result in improvements in women's rights or economic participation– so these are unlikely to be relevant mechanisms. The only outcome of the eighteen we consider that responds to gender quotas is an political participation of women. This evidence is gathered from analysis of the indices of gender-related progressivity in Figure A5 and Figure A6. Specification checks based on standard event studies and Rambachan and Roth (2020) are provided as Figures A7, A8, A17 and A18. We also reproduce these figures conditioning upon country-specific linear trends, see Figures B28–B30.[36] In the discussion of identification, we

[32] Among the few causal studies available, Lorentzon and Pettersson-Lidbom (2021) estimates that a 1% increase in the share of midwife-assisted home-births decreased MMR by 2% in 19$^{\text{th}}$ century Sweden, and Anderson et al. (2020) document that occupational licensing of midwives reduced MMR by 6-8 percent in the US in the early 20$^{\text{th}}$ century.

[33] Fertility or parity tends to be correlated with age and we are not aware of causal analysis separately identifying parity and age risks. There is descriptive evidence that the health of women is depleted by repeated and closely spaced pregnancies, and that MMR is J-shaped in age, being high among adolescents, lower when a women is aged in her 20s, and then increasing at older ages such that it is highest for women over the age of 35 (Restrepo-Méndez and Victora, 2014).

[34] These results are robust to the alternative estimators discussed, except for contraceptive cover which shows an increase with de Chaisemartin and D'Haultfœuille (2020) but not with Rambachan and Roth (2020).

[35] These variables measure population coverage (quantity) but if alongside the expansion of coverage parliamentarians act to improve the quality of facilities (better medical equipment, information or staff training), the coefficients on coverage will capture this too. Since the reproductive health coverage data come from an entirely different source than data on MMR, our finding that it responds to quota implementation reinforces the validity of the results.

[36] In Figure A6, the only outcome of the set we investigate that responds to quota reform is women's labor market participation, which shows a decline. However, once we condition upon country-specific linear trends there is no impact. The results for the other outcomes are not sensitive to country-specific trends.

pointed to the placebo coefficients of these estimates, and now we focus on the dynamic post-quota coefficients. One of the outcomes we investigate that merits particular attention is legalization of abortion, which directly related to women's reproductive health because barriers to safe abortion lead to unsafe abortion, a major cause of MMR (Girum and Wasie, 2017). Using data from Elías et al. (2017), we find no impact of quotas on abortion legislation, see Figures A6 and A8. A possible reason is that there are strong religious barriers to abortion law which women leaders may not be able to surmount.

Education and fertility. We investigated whether an increased presence of women in parliament modified female education and fertility, among demand side determinants of MMR. Bhalotra and Clarke (2013) provide quasi-experimental evidence that expanding female education brings MMR down, and many studies document a positive association of fertility with MMR (Girum and Wasie, 2017). We find an increase of 0.5 years in the education of young women and a decline in the total fertility rate of 6–7%, consistent with the observed expansion of contraceptive coverage and women's schooling.[37]

On education, we study girls and boys aged 15–19 at the time of the reform, finding that attainment increases significantly more for girls than for boys (Figure 7, Table B14). This results is in line with evidence from India that quotas for women in local government led to an increase in girl's schooling, the suggested channel being an increase in female aspirations (Beaman et al., 2009). The decline in fertility is accompanied by a (noisy) increase in birth spacing of 2 months (modelled using the DHS microdata), see Figure 6, consistent with the documented expansion of contraceptive coverage.[38]

Fertility – parity and scale effects. Since high fertility, defined as the number of children per woman, is associated with higher MMR risk per birth, a decline in fertility is a plausible mechanism for the observed decline in maternal death risk per birth (MMR). In addition, a decline in fertility will have a *scale effect*, tending to reduce the number of maternal deaths at any level of risk per birth. Table B15 provides a back-of-the-envelope calculation of the number of maternal deaths averted on account of the impact of quotas on (a) MMR or risk per birth and (b) TFR or the number of births.[39] Our estimates are that from the baseline of 92,928 total deaths per year, post-quota deaths would fall to 84,843 (a fall of 8085 deaths) if only considering the MMR (per birth) channel, to 87,259 if only considering the scale effect of fertility (a fall of 5669 deaths), and to 79,668 (a fall of 13,260 deaths) if considering the total effect of gender quotas on the maternal death count, as mediated by both fertility and MMR decline. The scale effect (that is not captured in MMR decline) is roughly 43% of the total change in the death count.[40]

[37]Substantively similar results are observed in standard event-study estimates (Figure A16), in "Honest DiD" bounds (Figure 8) and when using a common sample across all mechanism variables (Figure B26, though note that a small number of variables have sparse coverage, so estimation is noisier).

[38]We displayed results earlier that indicate no significant change in the composition of mothers giving birth, though the broad patterns suggest that the decline in fertility stemmed from women over the age of 30.

[39]To do this we use data for the year before the imposition of quotas and sum across all quota countries to get baseline statistics of about 35 million births, and around 93 thousand maternal deaths in a year, corresponding to 266 deaths per 100,000 births. We apply the estimated declines of 8.2% in maternal deaths per birth (Table 1) and 6.1% in fertility (Table B12, column 4).

[40]It is 64% of the decline in deaths captured by MMR. Table B15 should be seen as an accounting exercise, varying MMR

Political change. Earlier, we discussed political factors as potential predictors of quota legislation, focusing in Figure A9 on the placebo coefficients. We now study dynamic effects of quotas on the same range of political outcomes. We see that quota adoption is associated with a significant increase in the years that a regime is in power, and a corresponding decline in the probability of regime transition. In other words, after gender quota adoption, there is greater political stability, consistent with both being associated with constitutional reform.[41] What is relevant for interpretation of our results is that our results hold conditional upon controls for regime stability.[42]

We also investigated democratization, defined as in Boix et al. (2013). This is related to regime transition and is empirically relevant as developing countries (which dominate in quota adoption) often transition in and out of democracy. The results are in Figure B38. There is no evident tendency for quota adoption to increase democratization, or for democratization to lower MMR, albeit democratization has been shown to lead to lower infant mortality (Kudamatsu, 2012). The main estimates control for pre-quota democratization interacted with a post-quota trend, see Table 1, and Figure 2, panels (a) and (b). We perform a stricter test, controlling for a full set of lags and leads to democratic transitions. Our estimate of impacts of quotas on MMR is robust to this, see Figure B38.[43]

8 Resources

In this section we first address the substantive question of whether increasing the share of women in parliament benefits population health in general, or MMR in particular. This is related to the question of how any

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from left to right (hence different values for MMR in left and right columns), and varying fertility from top to bottom (hence changes in numbers of births in the top and bottom cells). Note that the 8085 and the 5669 fewer estimated deaths in the top right and bottom left cells (necessarily) do not add to the 13260 fewer deaths in the bottom left cell, given that this cell considers movements together, so lower rates of MMR will be applied to fewer births, resulting in fewer deaths than when summing partial movements.

[41] Clayton et al. (2017), for instance, argue that quotas are instituted with the intention of ensuring regime stability by bolstering support from women. She writes "In Uganda, the ruling National Resistance Movement party implemented quotas in 1989 as part of a wider strategy to ensure regime stability and strengthen support among various social groups." Other authoritarian or semi-authoritarian regimes have similarly adopted gender quotas to bolster political support, see, e.g., Panday (2008) on Bangladesh; Meena (2004) on Tanzania; and Longman (2006) on Rwanda. In Eswatini, Kenya and Zimbabwe, quotas were brought about by the adoption of new constitutions, as was also the case in Nepal and Rwanda as they emerged from conflict. Our reading of the evidence, albeit for a few countries, is that violence and political instability create support for stable and inclusive governance and an appetite for constitutional reform.

[42] This is the case irrespective of whether we control for country-time varying regime stability, or for the pre-quota indicator of regime stability multiplied by an indicator for post-quota years (along with similar controls for the other potential quota predictors), see Figure 2 and Table 1. Recall also that we find no association of regime stability with MMR decline (Table B4), and no evidence that regime stability preceded quota adoption (see the pre-trends in Figure A9). We investigated this further by re-estimating the main model stratifying by whether or not a country had a regime transition within 4 years pre or post quota adoption. While we have wider confidence intervals in the sample that transition, we do see a clear pattern of declining maternal mortality following quota adoption in both groups (Figure B37).

[43] A number of the quota implementing countries in our sample were non-democratic. This is relevant because previous evidence on women's sway in policy making has mostly emerged from democratic regimes, in line with models of politician behavior that admit a role for politician identity (Besley and Coate, 1997). Our results are consistent with women acting upon their innate preferences, potentially motivated by the mission of public service rather than by electoral motives. This is discussed further in the next section.

improvements are resourced. We therefore investigate if gender quota adoption is associated with increased resources, overall, and for health. We close the section with reference to existing studies, primarily single-country studies, with a view to illuminating how gender quotas can result in MMR reduction without a large increase in resources.

Other population health outcomes. In view of previous evidence that women leaders prioritize health (Miller, 2008; Bhalotra and Clots-Figueras, 2014; Bhalotra et al., 2019), we investigate whether gender quotas led to generalized improvements in population health or possibly detrimental impacts on other health outcomes, as would be the case if the observed improvements in MMR were achieved by allocating resources away from other population health priorities. To assess this we investigated adult male (and female) mortality, mortality from TB,[44] and infant mortality, which is widely regarded as a marker of population health in infectious disease environments such as those which characterize developing countries. DID_M and event study plots are in Figures B31–B32 and the TWFE coefficients in Table B14, columns 4–9.

Mortality among adult males, tuberculosis and infant mortality show no significant change following gender quota adoption, with quite tightly estimated zeros until at least 5 years post-quota.[45] There is some evidence of adult female mortality declining, but this is not statistically significant. This does not surprise us because any policies targeting causes of adult mortality among women other than MMR (TB, accidents, etc.) are not easily targeted to women. The reason that MMR is a conceptually clean outcome to study is that reproductive health services that address maternal mortality are by definition targeted at women.[46] Overall, the evidence points to (a) no deterioration in the other population health outcomes studied, and thus no evidence of substitution, and (b) gender quotas being more effective at improving women's reproductive health and survival than in addressing other population health indicators. We suggest that both priorities and the potential to target women can explain why gender quotas have their largest impact on MMR.

[44] We chose this as it is high prevalence and gender-neutral. If anything, incidence and death rates from TB are higher among men than women. In 2017 close to 6 million adult men contracted TB and around 840,000 died from it. This compares with an estimated 3.2 million adult women who fell ill and almost half a million who died from TB (WHO, September 2018).

[45] In the case of infant mortality, which is based on microdata from the DHS (available for 68 countries and thus, a subsample), which allows us to generate estimates for boys and girls separately, we find some evidence that gender quotas lead to lower infant mortality for girls of, on average, 12 percent, although the DID_M and event study estimates are imprecise (Figures B31 and B32), two-way fixed effects are insignificant (Table B14, columns 7–9), and the honest DiD estimator suggests no effect at least up to the eighth lag (Figure B33). As for adult all-cause mortality and TB, so for infant mortality, it is difficult for policy to discriminate between boys and girls. For instance, policies to address infant mortality include provision of clean water and access to medical professionals or drugs, which are not amenable to targeting by the sex of the child. This said, it is not surprising we see some moderation of infant mortality. If women parliamentarians improve efficiency and targeting in delivery of reproductive health services this could lead to a decline in infant mortality – directly because maternal and child health services are often bundled in the same clinics, and indirectly because efforts to improve maternal health often translate into better child health (Aizer and Currie, 2014). One can rationalize larger effects on girl infants, for instance, by reference to Oster (2009) but we do not detail this as the estimates are not significant.

[46] Mean adult female mortality is 168 per 1,000 female population with range 34.3 to 685), while male mortality is 240 per 1,000 male population, with range 58.8 to 753.7 (Table A1). The contrast with MMR is striking: mean MMR in the global sample is 233 per 100,000 births (a very similar mean to male mortality), however with range, 3 to 2890 (more than 4 times wider than male mortality). The range demonstrates the potential for reduction.

Resources and resource allocation. We examine whether quota adoption led to an increase in available resources (Figure 6), which would constitute economic mechanisms. We find no evidence of an increase in GDP or in international development assistance for maternal health (DAH). We looked at DAH as this was increased after the year 2000 in response to MMR being included in the MDGs (Dieleman et al., 2016). It is of some interest to analyse the estimated coefficient on GDP. There is no impact of GDP on the share of women in parliament, but GDP has a significant direct impact on MMR. A 1% increase in current GDP is associated with a decline in MMR of around 0.33% (Table B6).[47]

We then test for increases in state health expenditure. When this is measured as a share of GDP, the estimates are imprecise, see Figure 6g for the de Chaisemartin and D'Haultfœuille estimates, and the standard event study plot in Figure A16g, also see Figures B34 which fills in missing data. Alternative estimates normalizing on population rather than GDP (Figure B35) show a significant increase (panels g, h). The single coefficient model indicates an increase of 0.89 percentage points or about 14% following quota implementation (Table B12, column 7). As there is some weak evidence of pre-trends in the figures we account for this by producing "Honest DiD" bounds (Figure 8g) and now we see that the bounds are positive, with a lower bound of about 0.7 percentage points. Overall, there is some evidence of an increase in state health expenditure. However, our estimates are robust to controlling for GDP, DAH and health spending, see Figure 3 and the corresponding DID_M and event study plots in Figures B17–B20. Thus MMR reduction does not rely upon increasing public expenditure.

Evidence on mechanisms from previous research. In this section, we refer to a broader literature which illustrates the mechanisms by which leaders are able to move the outcomes they prioritize. These include legislation, advocacy, parliamentary (or council committee) debate, building consensus, information campaigns, role model effects (that raise the aspirations of younger women), committing resources to areas they prioritize, targeting existing resources, and improving coordination, management and efficiency of public service delivery (for example, by setting targets for civil servants). Women may act differently from men in these matters because they have different preferences, different information sets, are less corrupt, or have greater intrinsic motivation. This is potentially reinforced by women citizens feeling more able to raise their concerns with women representatives and raising their aspirations when exposed to female leaders.[48]

On women changing the debate, recent analysis of text data illuminates the differentiated political preferences of women and the fact that they bring to parliament (or local council meetings), the issues that they care about (Clayton et al., 2017; Lippmann, 2020; Baskaran and Hessami, 2019; Bhalotra et al., 2019). Women in India's state legislatures have been shown to influence pro-female legislation (Clots-Figueras, 2012). Gender quotas in India have been associated with women citizens being more likely to be heard (Iyer et al., 2012;

[47] A crude back-of-the-envelope calculation assuming log-linearity (conditional on country and year FEs) suggests that to achieve the roughly 10% reduction in MMR that we estimate as flowing from quota adoption, GDP would have to increase by nearly 30%. This illustrates the force of women's political power.

[48] Voting is often along party lines, and platforms too are often influenced by party leaders, so voting is not necessarily the most useful measures of political preferences.

Parthasarathy et al., 2019). Using US data, Gagliarducci and Paserman (2016) show that women are better at consensus-building, which is relevant if they want not only to generate debate but to achieve policy action. Building consensus for action on maternal mortality is likely to be harder than for action on child mortality. For example, one area where there is agreement between Republicans and Democrats in the US is early childhood investment.[49]

There is some evidence that women bring resources to domains they prioritize, in particular, to health (Miller, 2008; Bhalotra and Clots-Figueras, 2014). However resources may not be key for four reasons. First, policy action on the margins we identify in our analysis of mechanisms (namely, expanding the cadre of skilled prenatal and birth attendants and educating young women) is relatively low-cost because wages of the relevant personnel are low in developing countries. In particular, midwives, nurses and teachers can make a large difference to these outcomes– see Banke-Thomas et al. (2020) on costs of extending prenatal care, and Andrabi et al. (2020) on costs of extending schooling. Second, there is considerable scope to improve public services by limiting leakage of public funds on account of corruption. Women politicians in Brazil and India have been shown to be less corrupt than men, and less likely to distort policy to achieve electoral gains (Brollo and Troiano, 2016; Baskaran et al., 2018). Third, there is evidence of substantial slack that good resource management can transform into productivity (Bloom et al., 2014, 2015), including in the public health domain (Propper and Van Reenen, 2010). In developing countries, public services including health services have been shown to suffer high rates of staff absenteeism (World Bank, 2003; Das and Hammer, 2014; Chaudhury et al., 2006). Correcting this sort of inefficiency does not require material resources as much as motivated governance. Women in politics appear to be more intrinsically motivated. If women have more information on MMR (Ashraf et al., 2020), they may also be better able to target resources. Fourth, our measures of reproductive health coverage are not purely supply-side measures, they also reflect uptake. Previous work suggests that low-cost outreach, information provision and education of women can improve uptake (Miller, 2008; Dupas, 2011; Bhalotra and Clots-Figueras, 2014; Bhalotra et al., 2019; Beaman et al., 2009; Currie and Moretti, 2003).[50]

[49] Evidence that women leaders have more information on women's issues is also in an article by UN Women, which cites Chirisa, a woman elected to the Zimbabwean parliament as saying that she "hopes to use her knowledge and experience to familiarize other MPs with the gender equality and women's rights priorities that will make a difference to women's lives." "I know what these issues are and I know where to go to get information and support from the women's movement" (UNWomen, 2013).

[50] The literature provides several examples of significant achievements in public health achieved at low cost, driven by outreach and education. Miller (2008) cites evidence from historical studies that women conducted door to door information campaigns in early 20th century America, to encourage families to boil drinking water, and this contributed to sharp reductions in infant mortality. Bhalotra and Clots-Figueras (2014) show that breastfeeding rates in Indian districts increase when women legislators are elected from the district. Bhalotra et al. (2019) show that creation of primary care clinics tasked with outreach and prevention led to substantial drops in MMR and infant mortality in Brazil. Beaman et al. (2012) show that gender quotas in India lead to higher female education through an aspirations channel and Currie and Moretti (2003) and Bhalotra and Clarke (2013) show that educating women leads to increased prenatal care and lower MMR respectively. Bhalotra et al. (2017, 2021) show that a home visiting program providing information on preventive health behaviours improved infant health, later life chroinic disease and future earnings. Fitzsimons et al. (2016) show that information on diet changes nutritional outcomes. Dupas (2011) shows that information on the risk of HIV infection by partner's age led to a decrease in teen pregnancy in Kenya.

9 Conclusion

This paper provides compelling evidence that the political empowerment of women can effect rapid maternal mortality decline. Thus gender quotas may be a powerful at-scale means of modifying policy priorities in favor of maternal health. While significant progress has been made, especially since 2000, preventable maternal mortality remains high, the lifetime risk of maternal mortality being 1 in 45 among women in low income countries. Despite a wave of gender quota implementation, 130 countries in the world have none. There is thus substantial room for maneuver. Our findings have implications for the recently launched Global Health 2035 report, and the ambitious Sustainable Development Goals. We show that SDG 3.1 targeting maternal mortality reduction is complementary to SDG 5.5 targeting an increase in women's political representation.

We estimate that adoption of gender quotas generates a decline of 8–12% in the maternal mortality ratio. This 'intent to treat' effect captures an average over the 22 countries which legislated a reserved share of seats in parliament for women between 1990 and 2015. (We find a broadly similar magnitude, averaging over the Indian states, which adopted local government gender quotas at different dates.) Our first stage estimates indicate that quota adoption led to a sharp increase in the share of women in parliament of 5–8 percentage points. This is an average which varied across countries with quota size, the baseline share of women in parliament and the baseline rate of MMR. Combining the reduced form and first stage effects provides estimates of the impact of women parliamentarians on MMR: our IV estimates indicate that a 1 percentage point increase in women in parliament reduces MMR by 1.5 to 2%.[51] A back of the envelope calculation using our estimates and rates of maternal mortality at the end of our sample period suggests that, going forward, the adoption of parliamentary gender quotas in all non-adopting countries could reduce rates of maternal mortality in Africa by 7.1%, in Oceania by 1.6%, in Asia by 1.3%, in the Americas by 0.8%, and in Europe by 0.1%.[52]

[51] The elasticity of MMR to women in parliament is smaller because a 1% increase in women in parliament is considerably smaller than a 1 percentage point increase. Our estimates imply that a 1% increase in women in parliament reduces MMR by 0.11 to 0.13%.

[52] These projections are relatively crude, simply mapping estimated effects based on country-specific maternal mortality ratios from Table A5 into each country's maternal mortality ratio at 2015, provided that women's representation at baseline is below the quota size. This implies reductions of about 31 maternal deaths per 100,000 live births in Africa, and closer to 1 per 100,000 live births in Asia, the Americas and Oceania.

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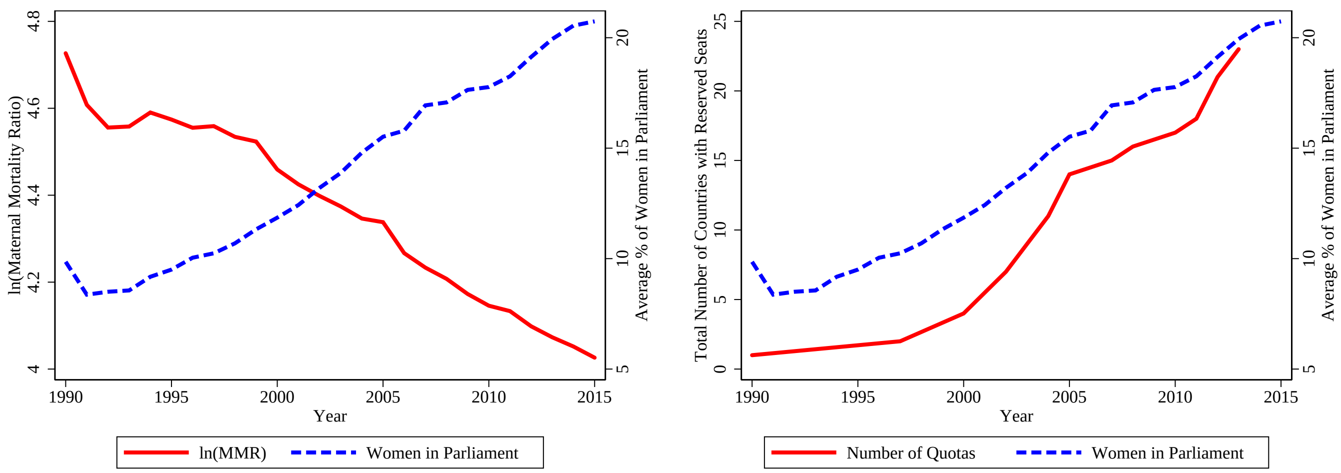
Figures and Tables

Figure 1: Trends in gender quotas, women in parliament and maternal mortality

(a) Women in parliament and ln(maternal mortality ratio) (b) Reserved seats and women in parliament

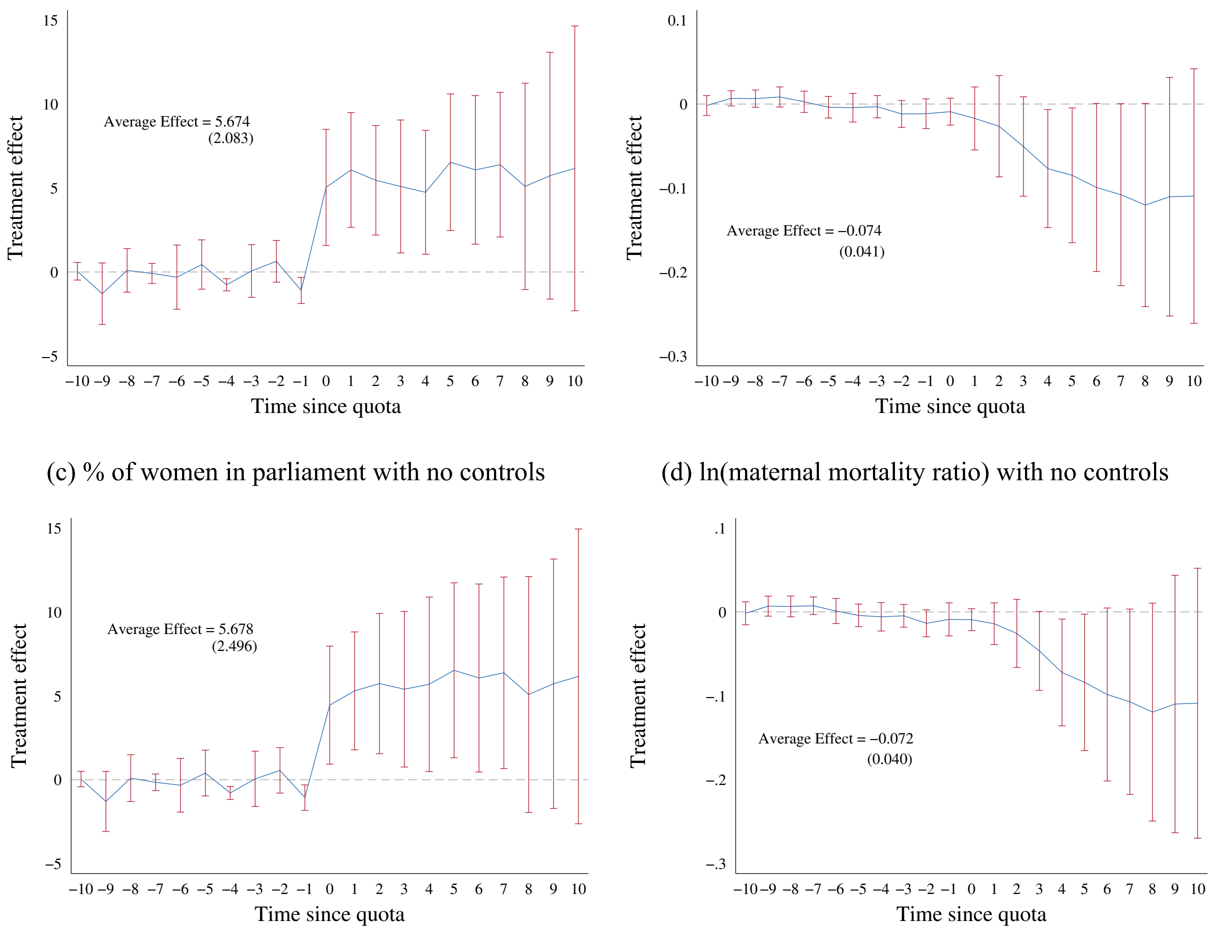


Notes: Raw trends in number of countries with parliamentary gender quotas, the percentage of women in parliamentary seats and the log of the maternal mortality ratio. Data sources are provided in the Data Appendix. The sample is a global sample of 178 countries for which we have annual data through 1990–2015.

Figure 2: Gender quota impacts: de Chaisemartin and D'Haultfœuille's DID_M estimator and placebos

(a) % of women in parliament with time-varying controls (b) ln(maternal mortality ratio) with time-varying controls

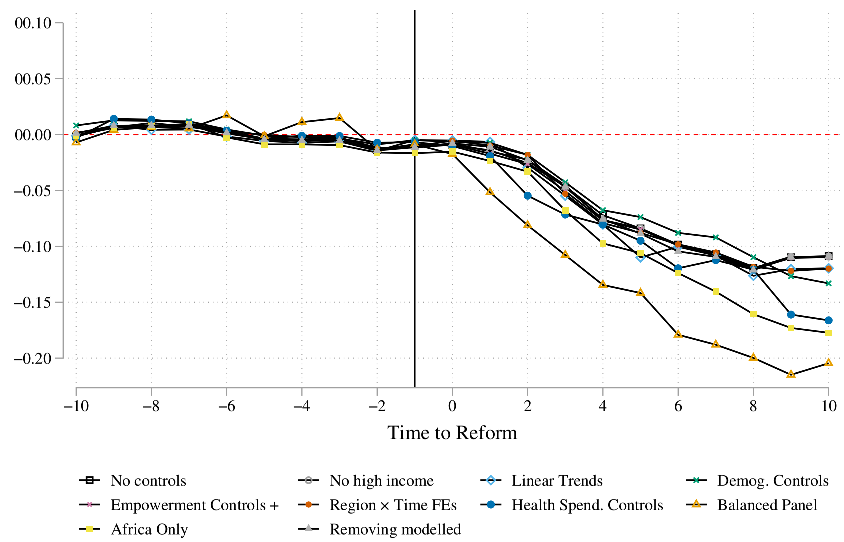
(c) % of women in parliament with no controls (d) ln(maternal mortality ratio) with no controls



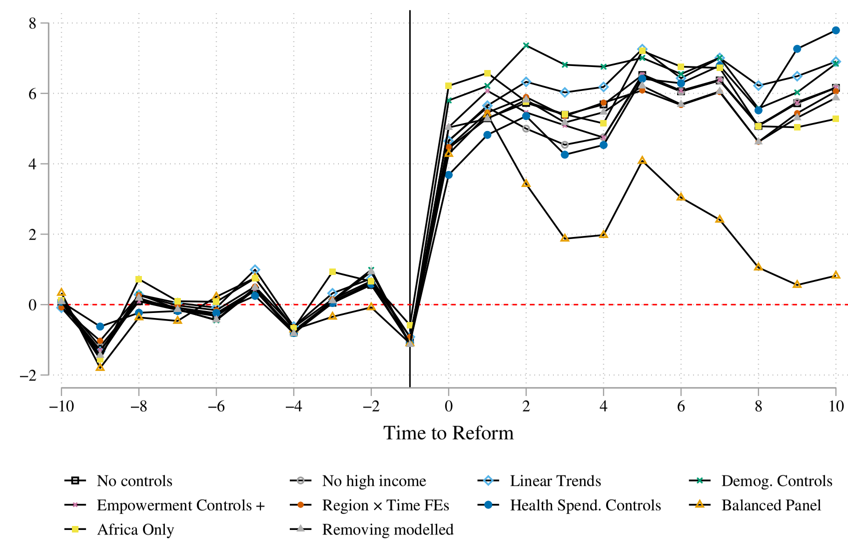
Notes: Panels (a) to (d) present estimates of the impact of quotas on women in parliament and maternal mortality following de Chaisemartin and D'Haultfœuille (2020). This consists of estimating aggregate impacts comparing all changers with non-changers surrounding the time of reform. Plots present 10 placebo estimates comparing switchers and non switchers in pre-reform periods (when switchers had not yet changed), and dynamic effects showing impacts between $t+k$ and $t-1$ $\forall k \in \{0, 1, \ldots, 10\}$. Panels (a)–(b) present models controlling for baseline resource and democracy controls interacted with a post quota dummy for women in parliament (panel (a)) and ln(MMR) (panel (b)). Panels (c) and (d) present models with no controls. Additional specifications are provided in Figure 3 below. All inference is conducted using a block bootstrap, and average effects and standard errors are estimated by pooling all immediate and dynamic effects.

Figure 3: Robustness of de Chaisemartin and D'Haultfœuille (2020) DID_M estimates to alternative measures and specifications

(a) ln(MMR)



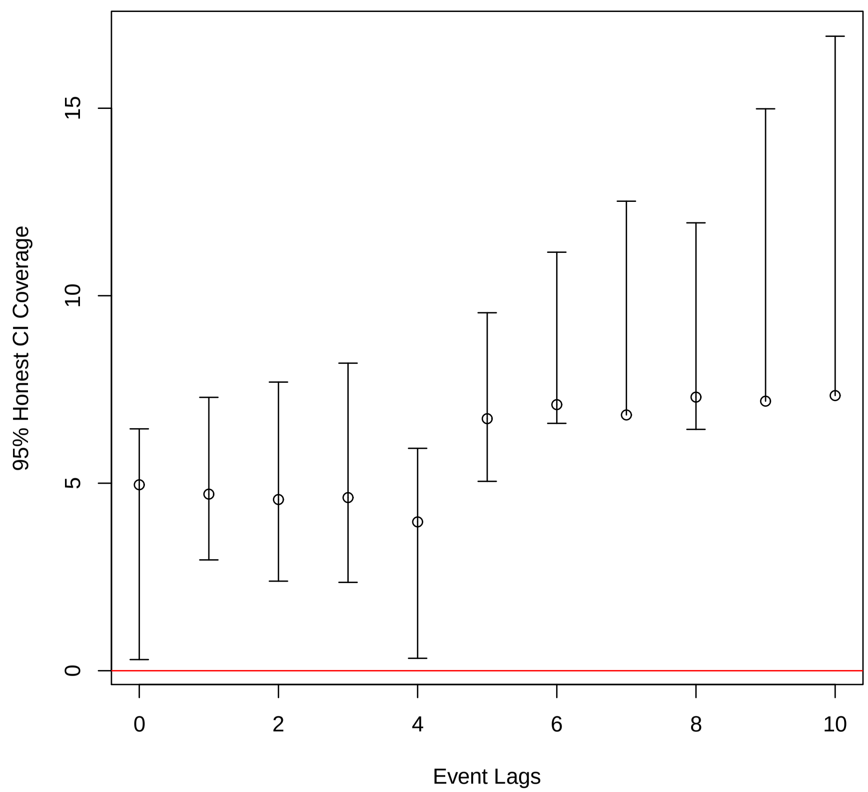
(b) % Women in Parliament



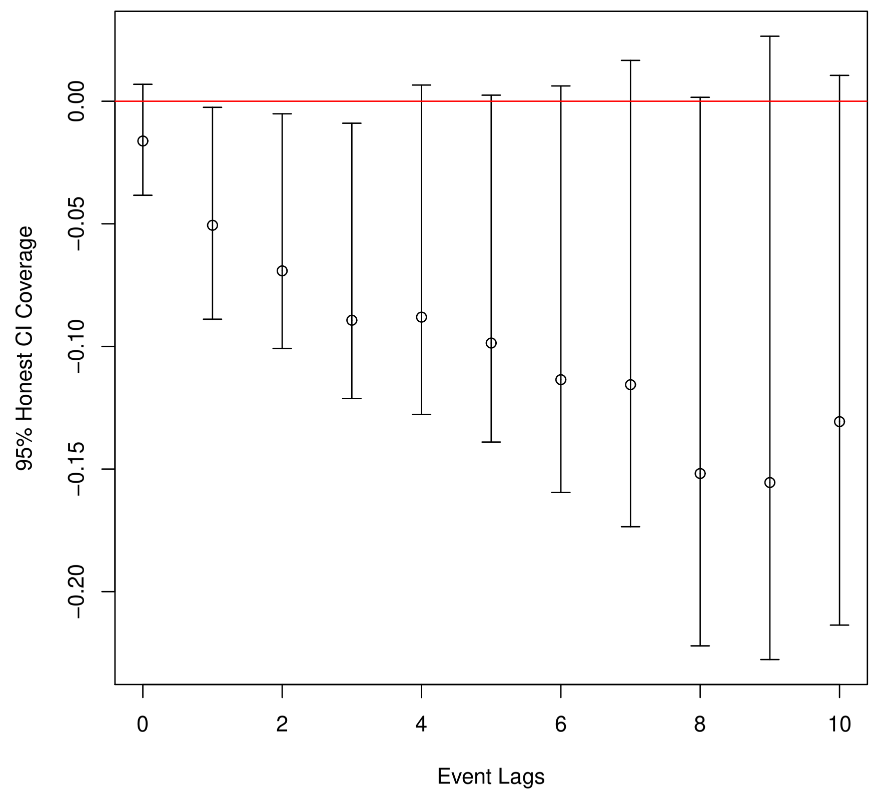
Notes: Alternative plots graph DID_M estimates surrounding the passage of quota reforms. Coefficients indicated by a hollow black square correspond to baseline de Chaisemartin and D'Haultfœuille (2020) estimates without controls. Alternative specifications are documented as labelled in the graph legend. Balanced panel refers to a sample consisting only of countries which adopted quotas prior to 2005 and as such exist in the entire range of quota post-treatment lags. "Removing modelled" removes from the sample any countries based only on modelled maternal mortality data. "Empowerment Controls +" controls for 25 variables measuring predictors of quotas indicated by the political science literature, and measures of empowerment and women's rights. Controls are consistently specified using a baseline index based on z-scores of each variable (standardized such that higher values capture more positive movements of the underlying measure), interacted with post-quota adoption indicators. Alternative specifications discussed, including point estimated as well as confidence intervals are documented in Appendix Figures B17 and B18.

Figure 4: Post-quota coefficients based on “Honest DiD”: women in parliament and maternal mortality

(a) Percent of women in parliament



(b) ln(maternal mortality ratio)

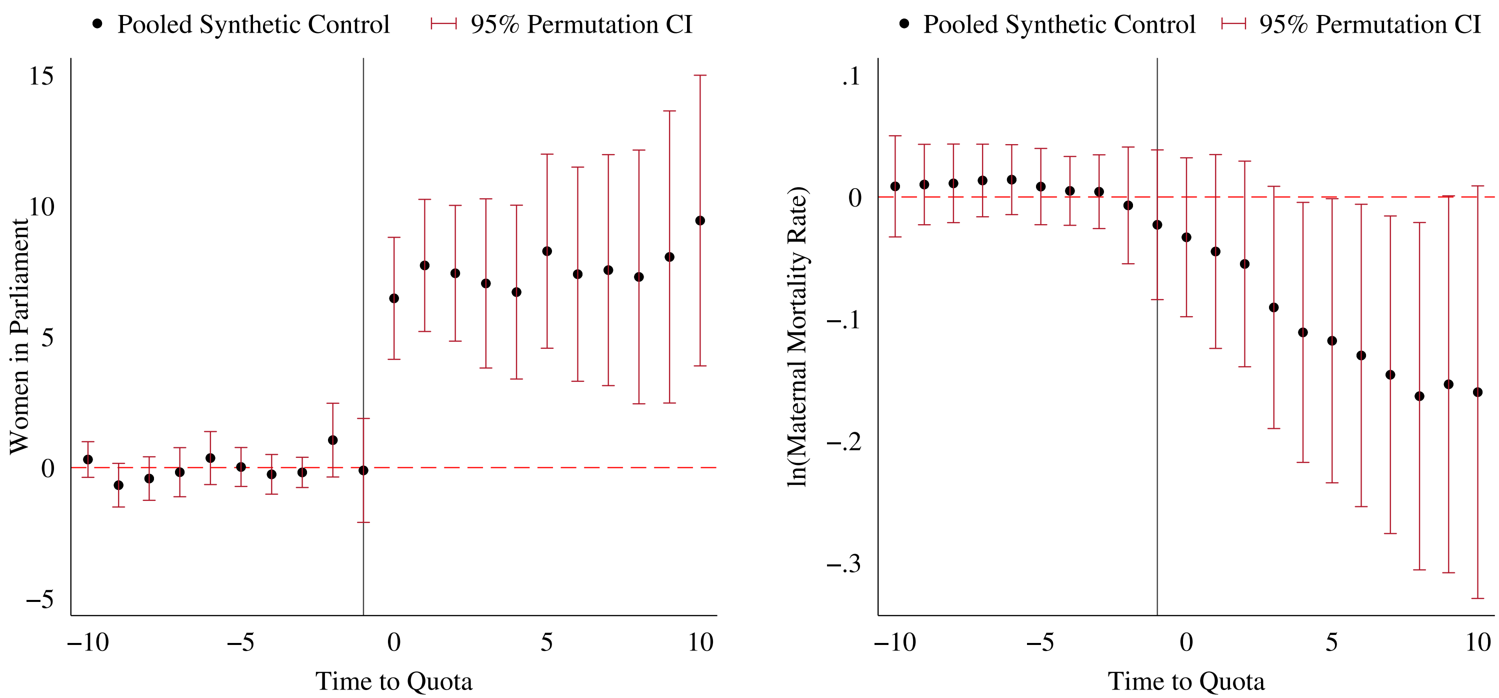


Notes: Each post-quota coefficient from event study specification 1 is documented, along with valid inference under Rambachan and Roth (2020)’s “Honest DiD” methods. These correspond to the specifications presented in Figure A3, panels (a) and (b). Here, in place of assuming parallel trends in quota and non-quota countries, valid 95% confidence intervals are constructed under the assumption that post-quota trends in quota countries relative to non-adopters would have followed their prevailing path from the pre-quota period, permitting violations of standard parallel trend assumptions.

Figure 5: Matched synthetic controls

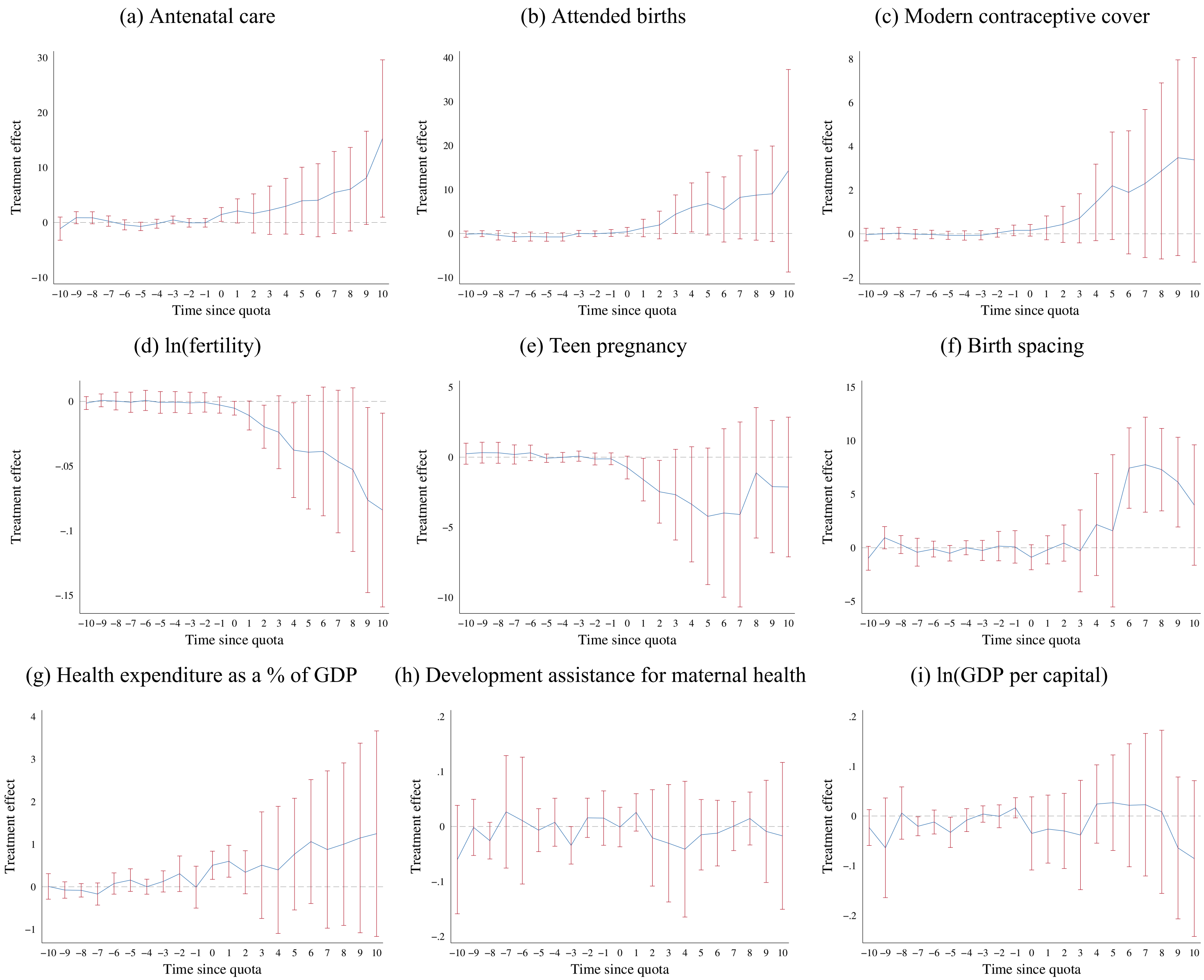
(a) Percent of women in parliament

(b) ln(maternal mortality ratio)



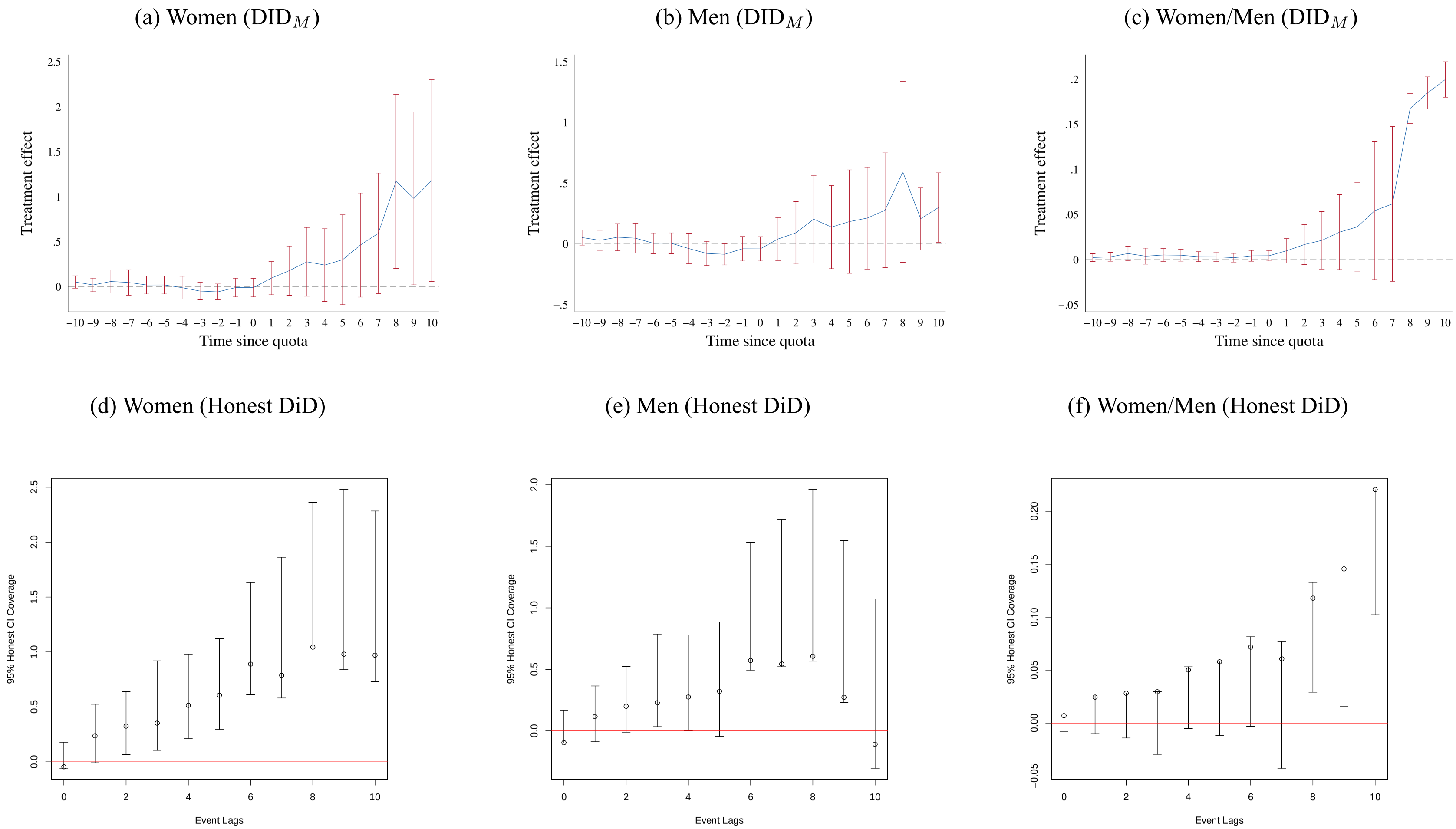
Notes: Refer to notes to Appendix B. Coefficients are estimated based on a pooled synthetic control approach where for each quota country a synthetic control is chosen based on leads of the variable of interest (up to period -3), over-weighting units which come from the same region as the country of interest. Averages of each lag and lead are taken across all treatment–synthetic control matches. Inference is conducted by permutation, where each permutation consists of randomly assigning the same distribution of quota reforms (blocked by countries to ensure identical treatment paths over time) but to non-reforming countries.

Figure 6: Mechanisms: de Chaisemartin and D'Haultfœuille estimates for impacts of gender quotas on intermediate outcomes



Notes: de Chaisemartin and D'Haultfœuille (2020) DID_M estimates of intermediate outcomes as a function of the passage of gender quotas are presented. Antenatal coverage and birth attendance refer to the percentage of coverage, are accessed from the World Bank databank, and are only available for a sub-sample of years for each country (an unbalanced panel from 1990–2015). Fertility rates refer to the expected births per women (the total fertility rate) and are recorded as World Bank databank indicator SP.DYN.TFRT.IN. Birth spacing is calculated as the average time to subsequent births for all women giving birth in a country and year based on DHS fertility rosters. Health expenditure is expressed as a percent of GDP, and is accessed from the World Health Organization's National Health Accounts (NHA) data series. Proportion of development assistance for health that goes towards maternal health is provided by the Institute for Health Metrics and Evaluation (IHME) Development Assistance for Health Database. The log of GDP per capita is PPP adjusted and measured in 2011 international dollars. All other details follow those of estimates presented in Figure 2.

Figure 7: Gender quotas and schooling (15–19 year-olds)

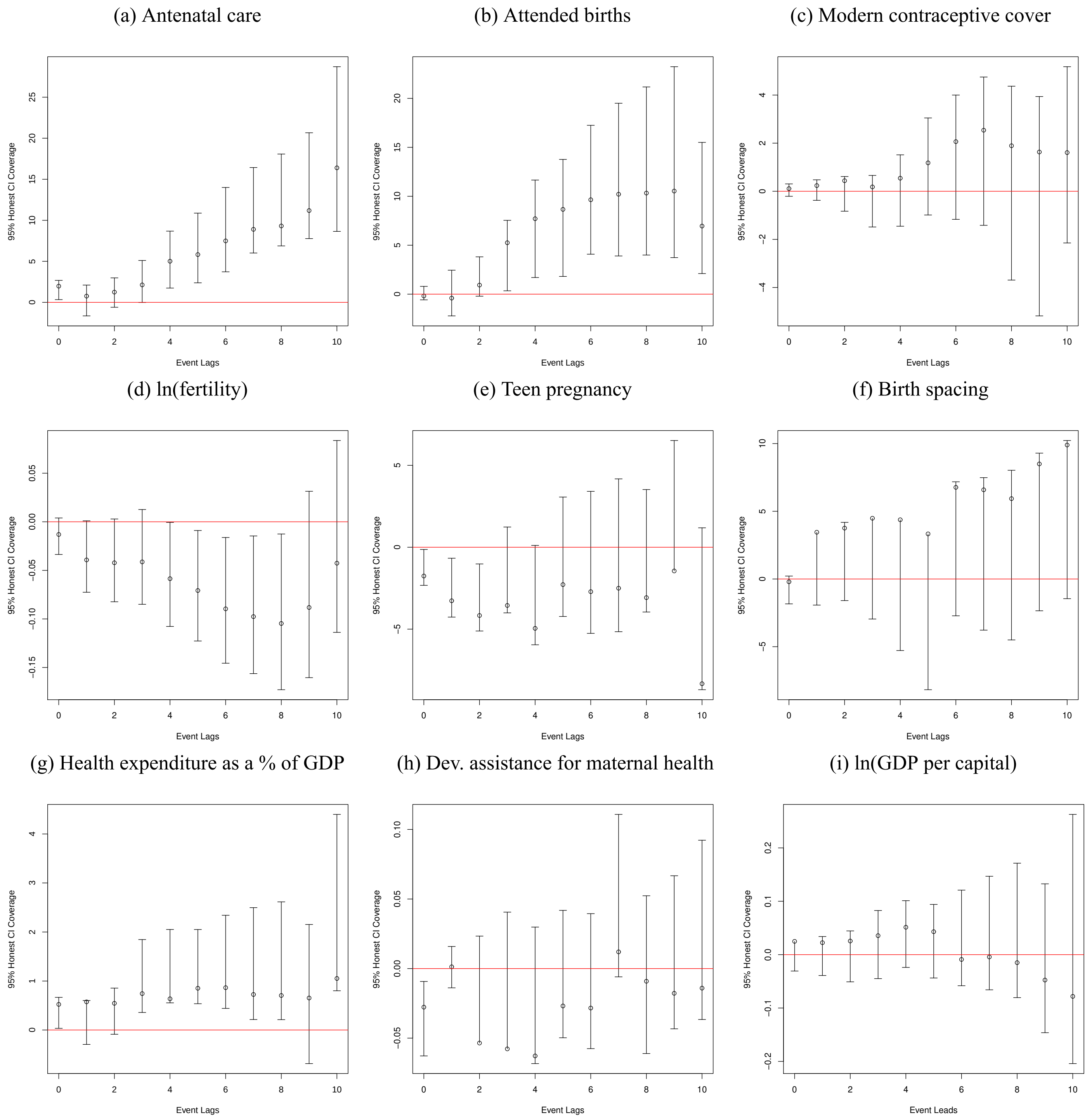


(a) Women (DID$_M$) (b) Men (DID$_M$) (c) Women/Men (DID$_M$)

(d) Women (Honest DiD) (e) Men (Honest DiD) (f) Women/Men (Honest DiD)

Notes: Impacts of reserved seat quotas on female education, male education, and the ratio of female to male education are presented. Panels (a)-(c) present estimates using de Chaisemartin and D'Haultfœuille's DID_M estimator, and panels (d)-(f) present estimates based on Rambachan and Roth's Honest DiD estimator. Education measures the average years of schooling for 15–19 year-olds.

Figure 8: Mechanisms: Post-quota coefficients based on "honest DiD"



Notes: Each post-quota coefficient from event study 1 is documented, along with valid inference under Rambachan and Roth (2020)'s "Honest DiD" methods. Here, in place of assuming parallel trends in quota and non-quota countries, valid 95% confidence intervals are constructed under the assumption that post-quota trends in quota countries relative to non-adopters would have followed their prevailing path from the pre-quota period, permitting violations of standard parallel trend assumptions.

Table 1: Two-way FE, de Chaisemartin and D'Haultfœuille, pooled event studies, and synthetic DID estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcome: ln(MMR)					
Method A: Two-way FE Model						
Reserved Seats	-0.082	-0.155*	-0.075	-0.106*	-0.071	-0.197*
	(0.051)	(0.090)	(0.056)	(0.056)	(0.055)	(0.119)
Method B: DID_M Estimates						
Reserved Seats	-0.072*	-0.074*	-0.072*	-0.074*	-0.080*	-0.082
	(0.043)	(0.043)	(0.043)	(0.043)	(0.047)	(0.050)
Method C: Pooled Event Study						
Reserved Seats	-0.079**	-0.153	-0.076*	-0.106*	-0.058	-0.184
	(0.039)	(0.093)	(0.042)	(0.058)	(0.045)	(0.147)
Method D: Synthetic DID						
Reserved Seats	-0.127*	-0.131**	-0.129*	-0.103	-0.128	-0.120*
	(0.067)	(0.067)	(0.072)	(0.064)	(0.080)	(0.065)
Negative Weights	-0.005	-0.143	-0.019	-0.006	-0.012	-0.364
Observations	4335	4241	4335	4241	4335	4241
	Outcome: Women in Parliament					
Method A: Two-way FE Model						
Reserved Seats	5.793***	6.297	6.071**	6.077**	6.038***	7.426
	(2.167)	(4.540)	(2.478)	(2.645)	(2.145)	(5.526)
Method B: DID_M Estimates						
Reserved Seats	5.678**	5.674***	5.678**	5.674***	5.167**	5.128***
	(2.222)	(1.880)	(2.222)	(1.880)	(2.154)	(1.872)
Method C: Pooled Event Study						
Reserved Seats	6.622***	7.175	6.940***	7.079**	6.242***	7.841
	(1.862)	(4.871)	(2.015)	(3.314)	(1.891)	(7.282)
Method D: Synthetic DID						
Reserved Seats	8.281***	7.775***	8.361**	7.950**	7.661***	7.287***
	(2.611)	(2.399)	(3.597)	(3.246)	(2.552)	(2.744)
Negative Weights	-0.005	-0.143	-0.019	-0.006	-0.012	-0.364
Observations	4335	4241	4335	4241	4335	4241
Controls (baseline):						
Empowerment & Predictors		Y				Y
Democracy			Y			Y
Resources				Y		Y
Region×year FE					Y	Y

Notes: Each cell presents results from a separate individual reduced form estimate varying estimation procedures (in rows) and included controls (in columns). The top panel considers the impact of reserved seats on maternal mortality, while the bottom panel considers the impact of reserved seats on women in parliament. Two-way FE models refers to linear regression controlling for country and year fixed effects. DID_M refers to pooled estimates based on de Chaisemartin and D'Haultfœuille. Pooled event study estimates impacts pooling all post-event study coefficients (from lag 0 to lag 10+). Synthetic DID implements Arkhangelsky et al. (2021). In each case, standard errors are estimated clustering by country or using a block bootstrap by country. Controls are consistently generated using baseline (pre-1995 measures) interacted with a post-quota dummy. At the base of each panel we present the magnitude of the negative weights attached to the two-way FE estimate, following de Chaisemartin and D'Haultfœuille (2020). * p<0.10; ** p<0.05; *** p<0.01.

Appendix Figures and Tables

List of Figures

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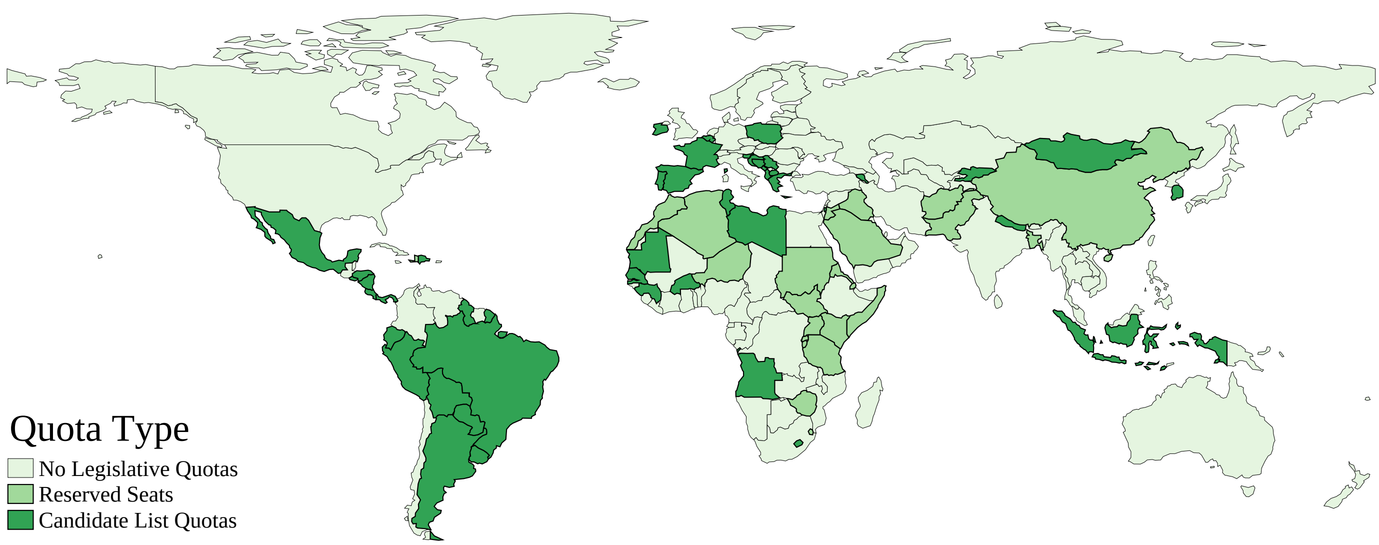
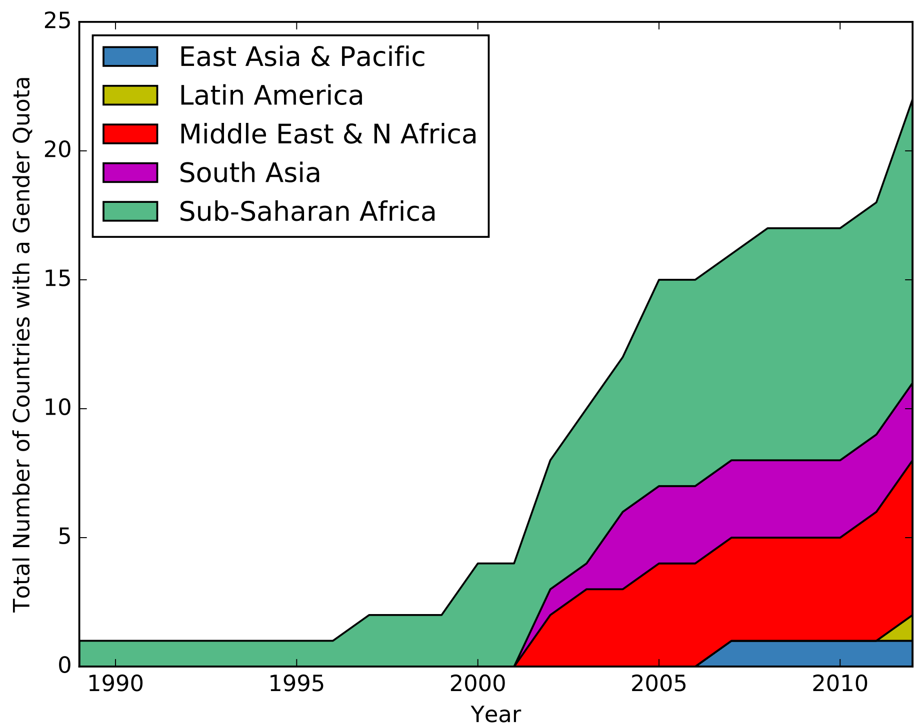
Appendix Figures

Figure A1: Quota coverage: 1990–2015



Notes: Geographic distribution of countries implementing a quota for reserved seats in parliament and candidate list quotas. Data is compiled from Dahlerup (2005) and updated with information for recent years from the online quotaproject.org database developed and maintained by the International Institute for Democracy and Electoral Assistance (IDEA), the Inter-Parliamentary Union, and Stockholm University. This database was consulted on 19th of July, 2016.

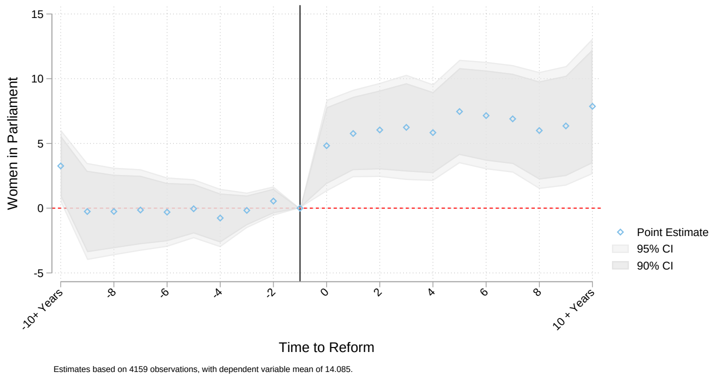
Figure A2: Reserved seat quota timing: 1990–2012



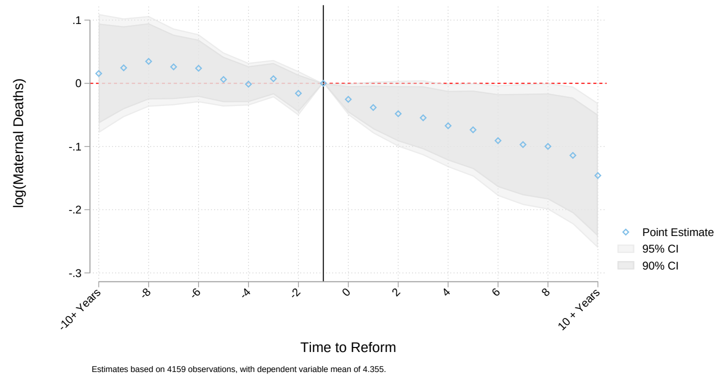
Notes: Timing of the implementation of reserved seats is documented by geographic region. Additional notes related to quota adoption are provided in Figure A1.

Figure A3: Gender quota impacts on women in parliament and maternal mortality

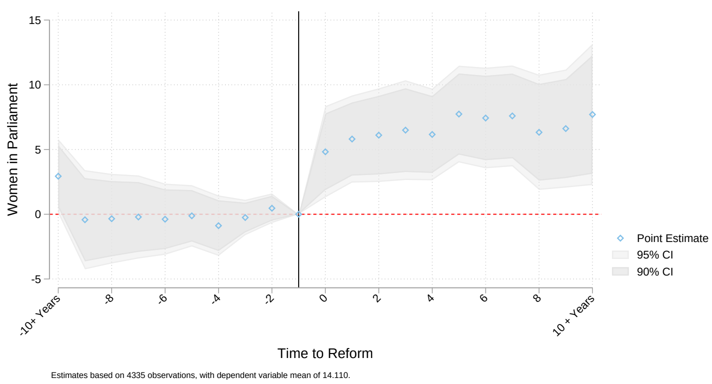
(a) Percent of women in parliament with time-varying controls



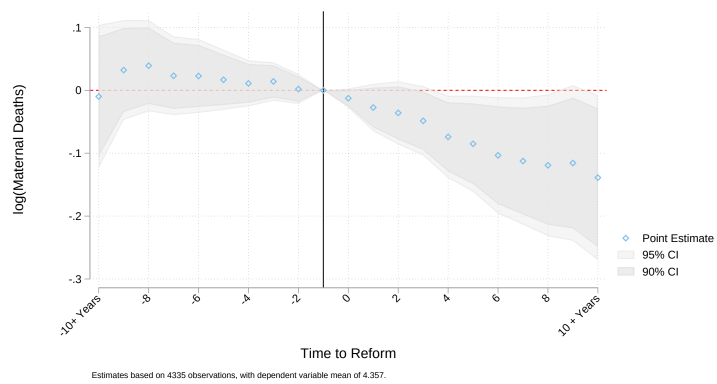
(b) ln(maternal mortality ratio) with time-varying controls



(c) Percent of women in parliament with no controls



(d) ln(maternal mortality ratio) with no controls

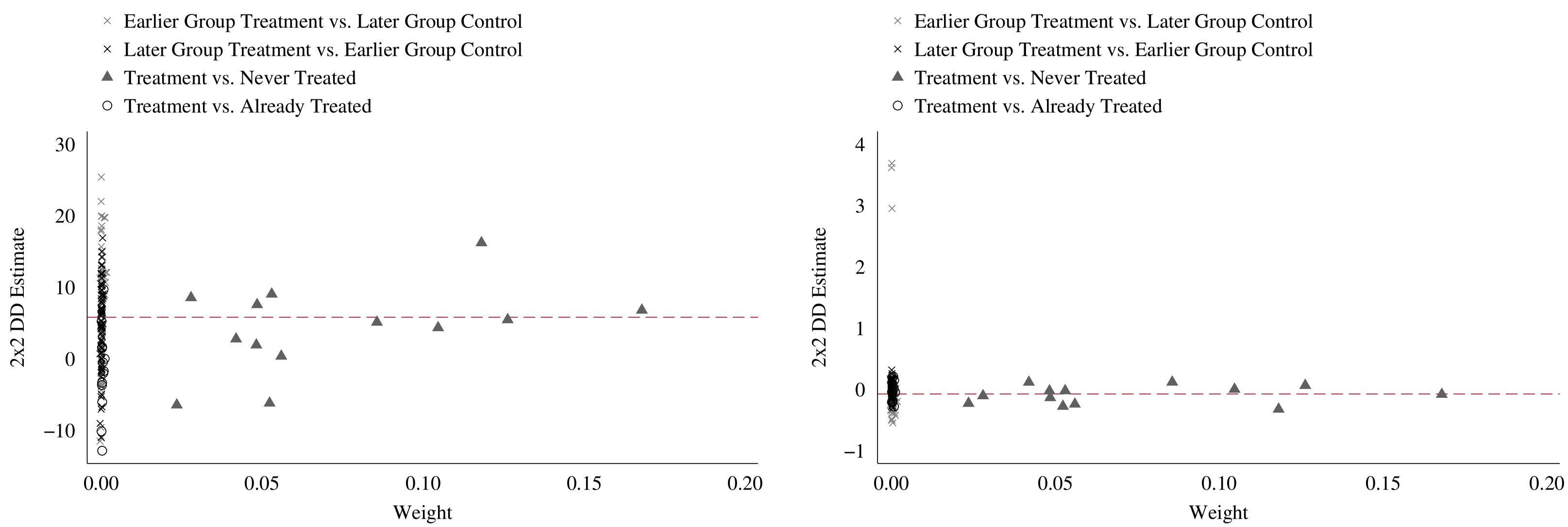


Notes: Point estimates of the lag and lead terms in the event study specification described in equation 1 are presented, along with their 95% confidence intervals. Estimates are conditional on country and year fixed effects. Panels (a) and (b) additionally control for the log of GDP per capita and an indicator of whether the country is a democracy. Time periods greater than 10 years from the reform date are displayed as a single "10 +" indicator. Standard errors are clustered by country. The omitted base category is taken as 1 year prior to the reform, indicated by the solid vertical line.

Figure A4: Goodman-Bacon (2021) decomposition based on 2×2 difference-in-difference models

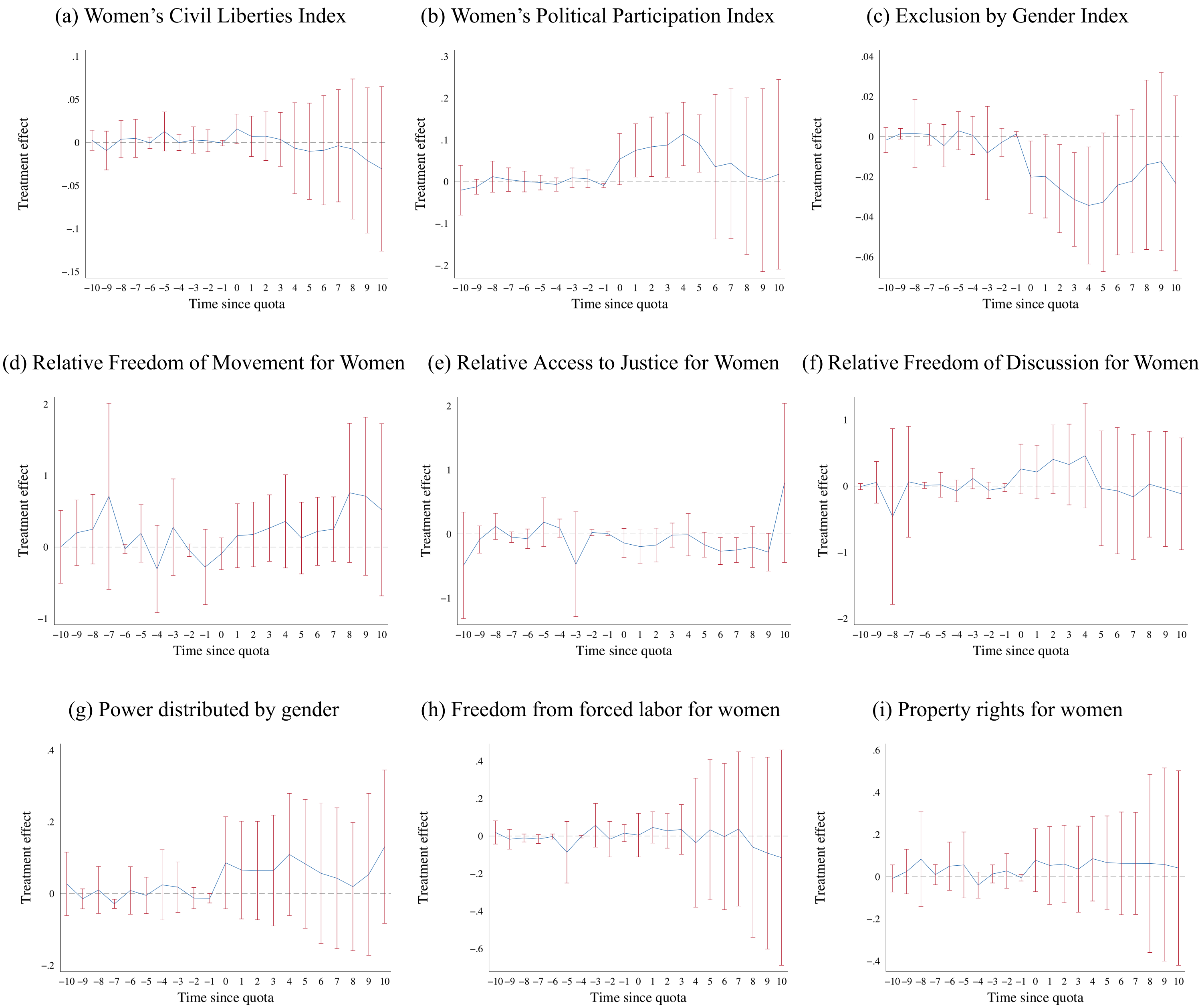
(a) Percent of women in parliament

(b) ln(maternal mortality ratio)



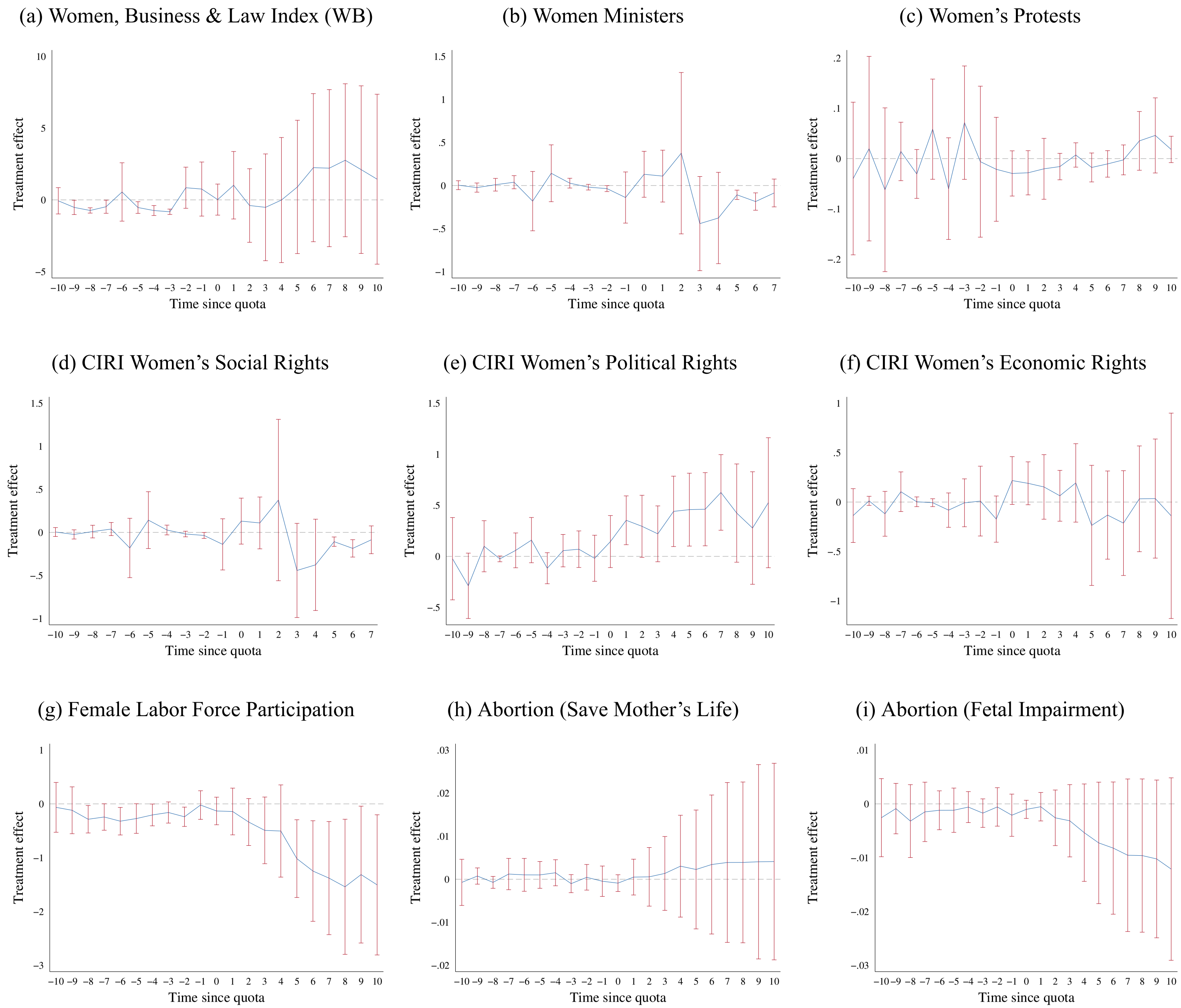
Notes: Figures document the Goodman-Bacon decomposition into a series of 2 × 2 difference-in-differences models depending on the type of comparison unit. Plotted × symbols represent cases where identification is drawn from timing-only comparisons. Darker shaded × symbols represent comparisons between earlier-treated units (as treatment) and later-treated units (as controls). Lighter shaded × symbols represent (problematic) comparisons between later-treated units (as treatment) and earlier treated units (as controls). Triangular symbols represent comparisons between treated (quota adopters) versus untreated pure controls (never adopters), with alternative estimates depending on the timing of adoption. Finally, a small number of hollow circles represent comparisons between units which adopted quotas before the beginning of the panel versus units which later became treated. Here each point on the graph considers an alternative adoption time period. The global decomposition for each of these four groups is given in Table A3.

Figure A5: de Chaisemartin and D'Haultfœuille estimates: women's rights and social standing from "Varieties of Democracy" data



Notes: de Chaisemartin and D'Haultfœuille (2020) DID_M estimates examine the variation of measures of women's rights and measures of relative social standing based on data collected in the Variety of Democracy dataset (Coppedge et al., 2020). When generating ratios of women to male outcomes (in panels (d)-(f)) these are Winsorized at the 1st and 99th percentiles to avoid outliers in cases where the denominator is very small. We follow suggested practices in removing a small number of observations which are coded by 3 or fewer country experts (Coppedge et al., 2020) in "C" type variables from the VDEM data which are based on the opinions of country experts. Lead (placebo) and lag (dynamic) effects are estimated for each variable.

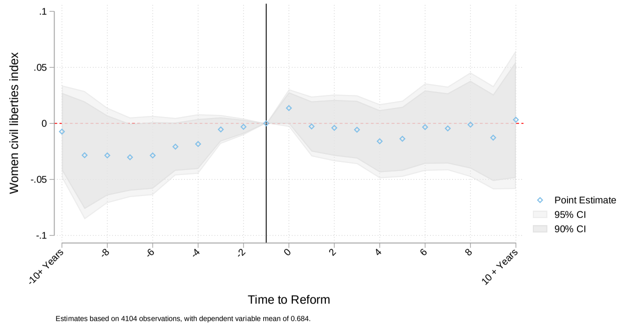
Figure A6: de Chaisemartin and D'Haultfœuille estimates: women's rights, empowerment, and women in politics



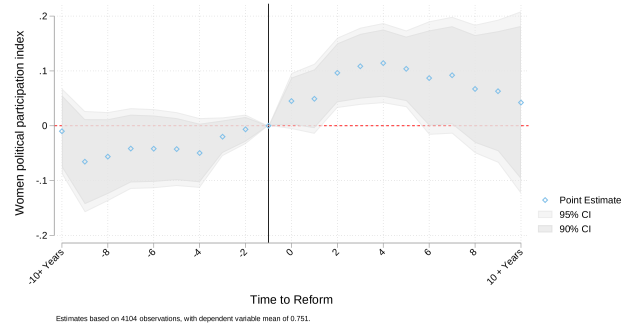
Notes: de Chaisemartin and D'Haultfœuille (2020) DID_M estimates are presented, with outcomes describing women's rights, empowerment or measures of participation in politics. Panel (a) is a recently generated index provided by the World Bank capturing Women's participation in Business and Lawmaking. Women Minister data is drawn from Inter-Parliamentary Union (IPU) Women in Politics data, women's protests as a share of all protests are calculated from data shared by Bell et al. (2019). Rights indexes are additionally defined by Cingranelli et al., and used in panels (d)-(f). Female labor force participation is drawn from the World Development Indicators, and abortion laws are coded based on Elías et al. (2017).

Figure A7: Event study estimates: Women's rights and social standing from "Varieties of Democracy" data

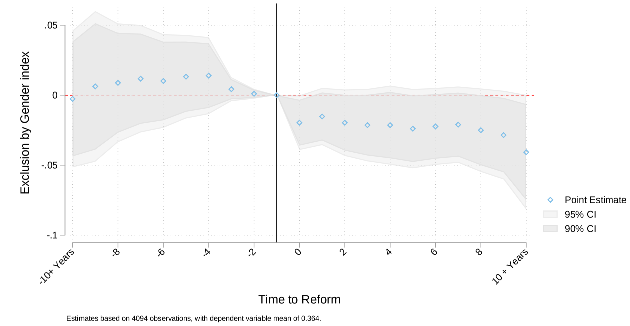
(a) Women's Civil Liberties Index



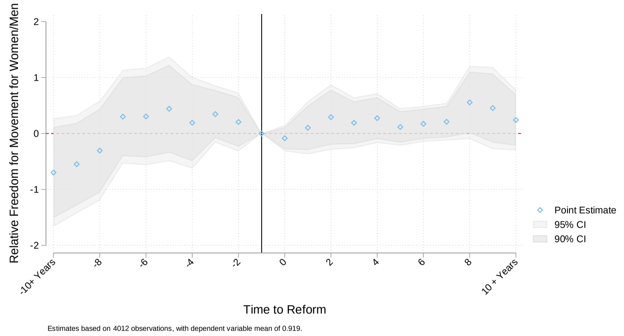
(b) Women's Political Participation Index



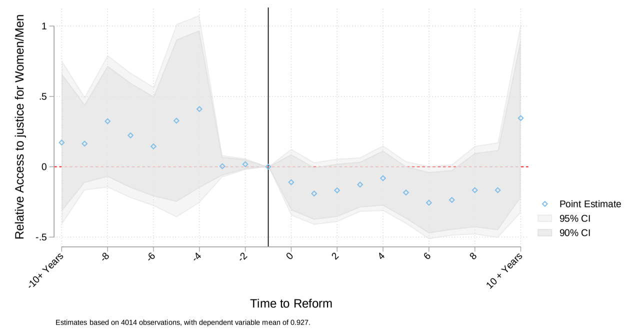
(c) Exclusion by Gender Index



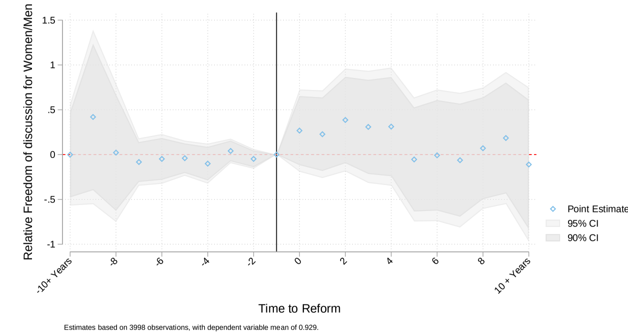
(d) Relative Freedom of Movement for Women



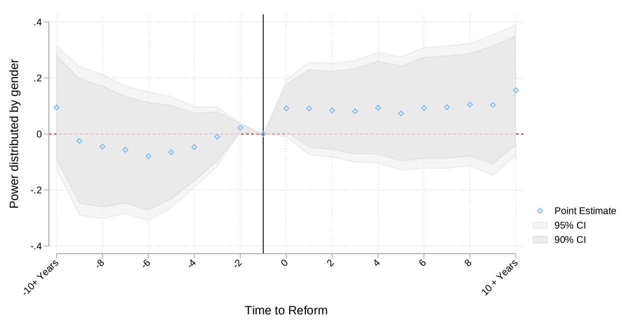
(e) Relative Access to Justice for Women



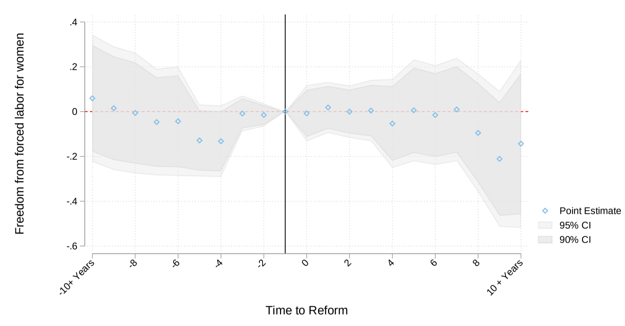
(f) Relative Freedom of Discussion for Women



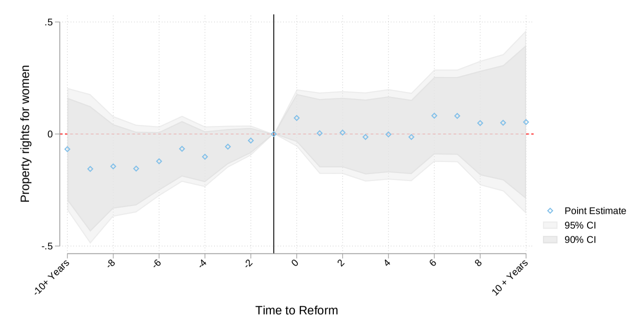
(g) Power distributed by gender



(h) Freedom from forced labor for women



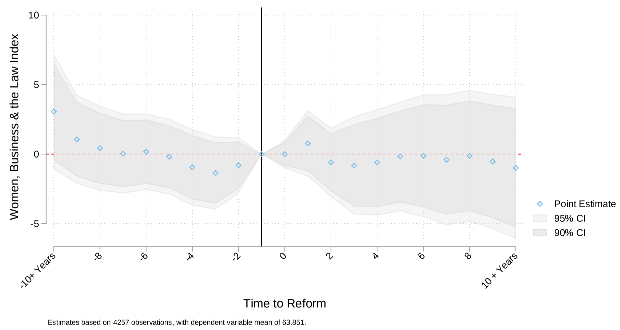
(i) Property rights for women



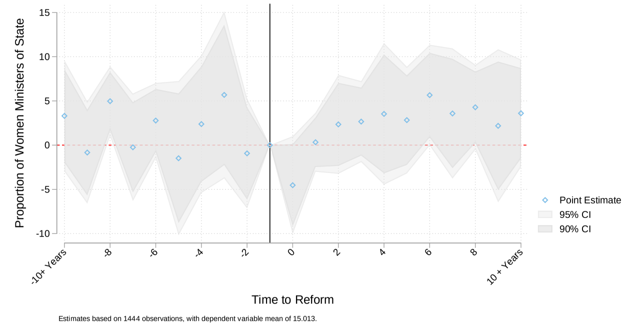
Notes: Refer to Notes to Figure A5. Here models based on identical outcome variables are estimated, however using the standard event study implementation described in equation 1.

Figure A8: Event study estimates: Women's rights, empowerment, and women in politics

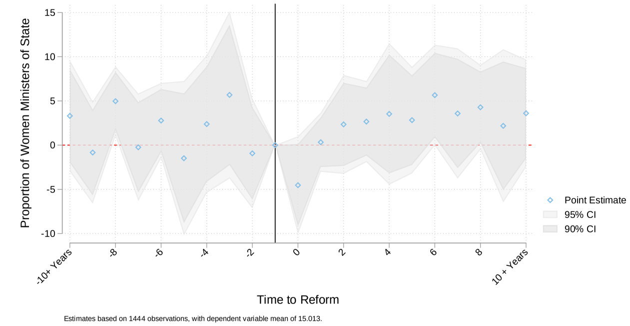
(a) Women, Business & Law Index (WB)



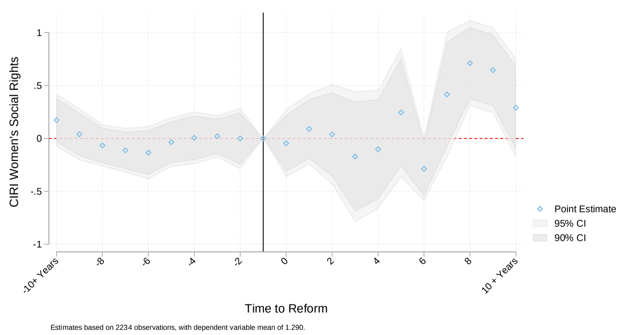
(b) Women Ministers



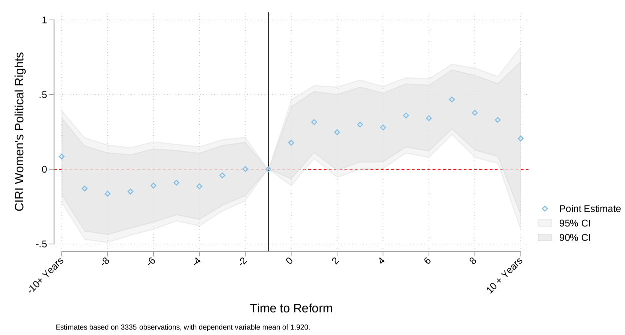
(c) Women's Protests



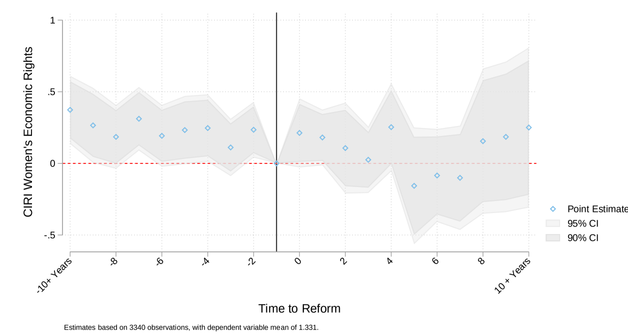
(d) CIRI Women's Social Rights



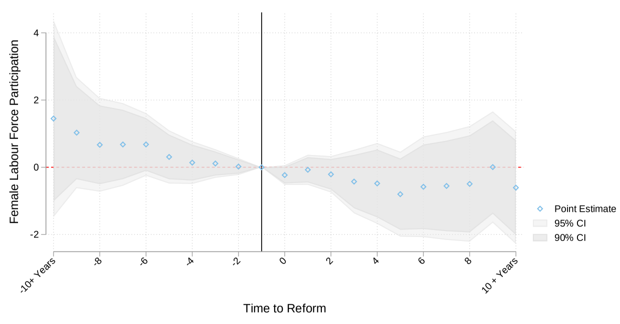
(e) CIRI Women's Political Rights



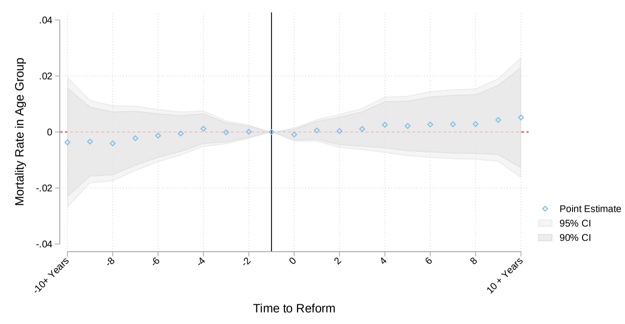
(f) CIRI Women's Economic Rights



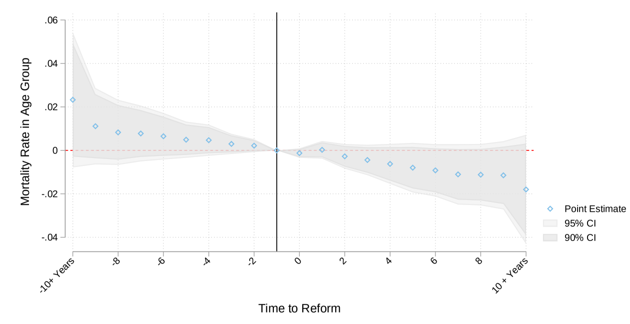
(g) Female Labor Force Participation



(h) Abortion (Save Mother's Life)



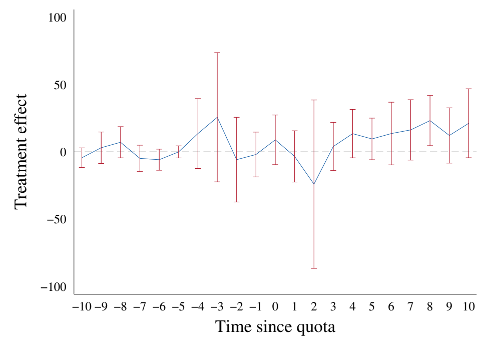
(i) Abortion (Fetal Impairment)



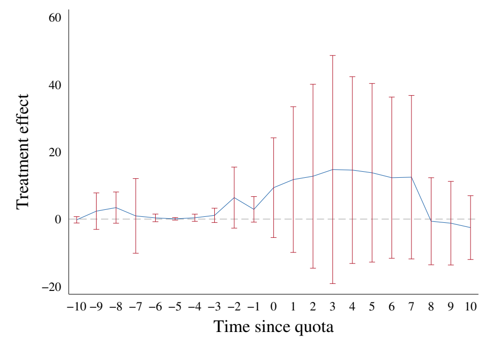
Notes: Refer to Notes to Figure A6. Here models based on identical outcome variables are estimated, however using the standard event study estimator following equation 1.

Figure A9: de Chaisemartin and D'Haultfœuille's DID_M estimator and predictors from political science literature

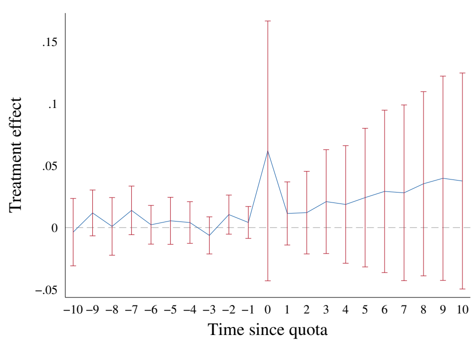
(a) Overseas Development Assistance p.c.



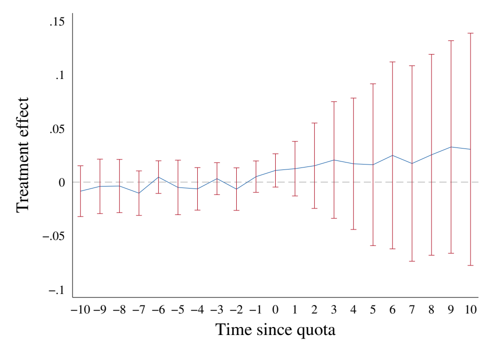
(b) Peace Keeper Presence



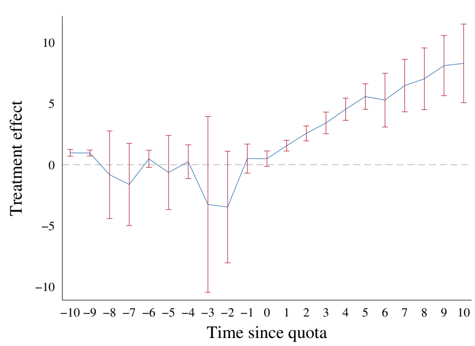
(c) Right Wing Executive



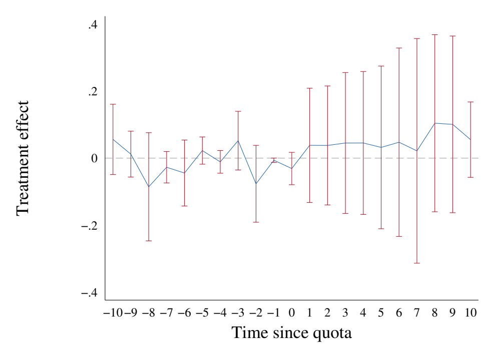
(d) Left Wing Executive



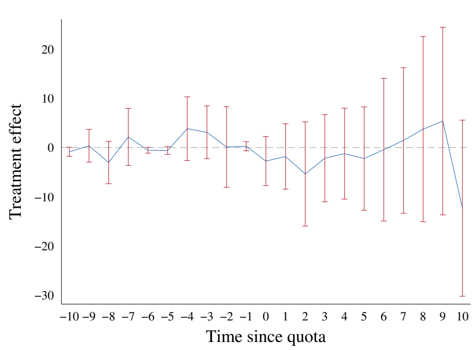
(e) Years in Power



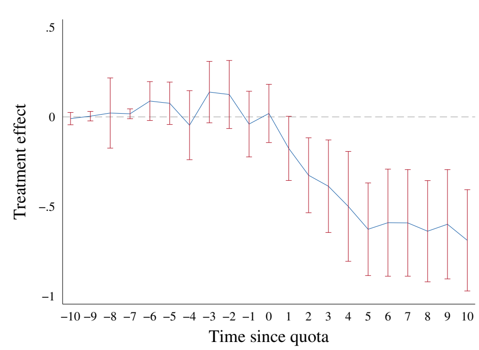
(f) Herfindahl Index



(g) Opposition's Vote Share



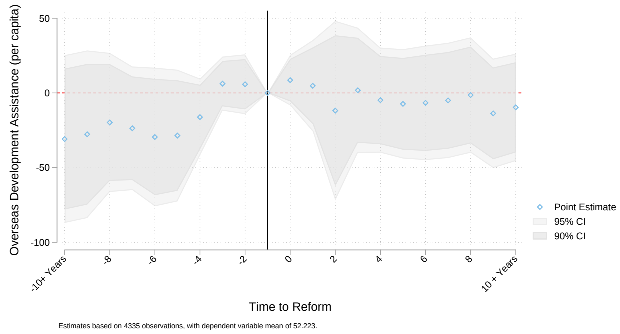
(h) Regime in Transition



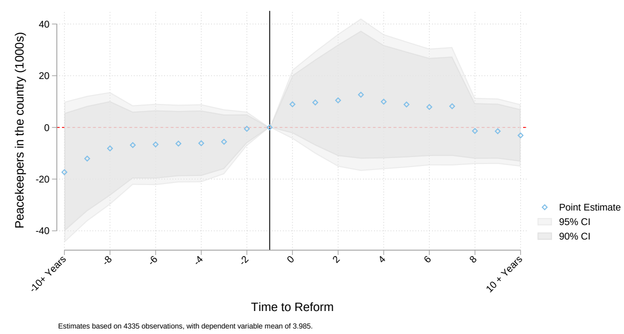
Notes: de Chaisemartin and D'Haultfœuille (2020) DID_M estimates of potential quota predictors suggested in the political science literature are displayed. All estimation details follow those described in notes to Figure 2. Overseas Development Assistance (ODA) is measured as per capita net inflows in current US dollars, and is generated from the World Bank Data Bank. Peacekeepers (measured in 1000s) are from the IPI Peacekeeping Database, and political measures including the orientation of leader's party, the leader's time in power, Herfindahl Index of parties, vote shares and regime types and changes are recorded by the Database of Political Institutions. Indicators for the executive's political leaning are coded from the Database of Political Institutions, based on a classification of leaders into left (31.4%), right (22.8%), center (7.4%) or not-applicable (38.4%).

Figure A10: Event studies: reserved seat quotas and predictors from political science literature

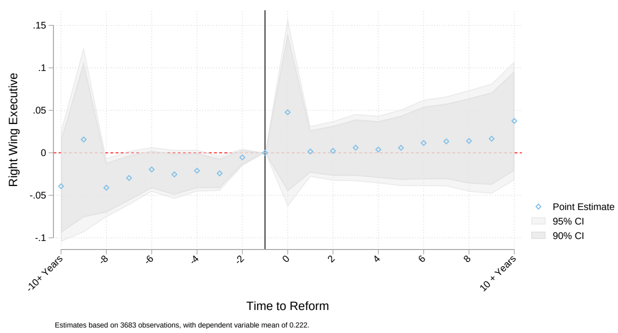
(a) Overseas Development Assistance p.c.



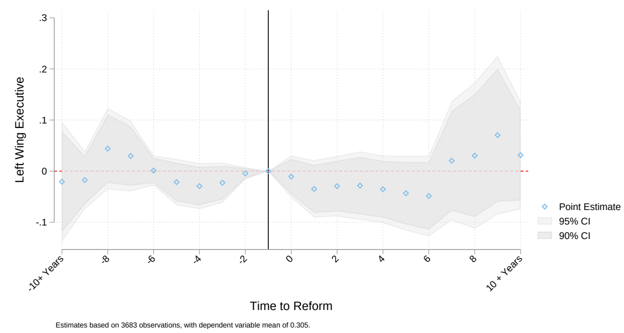
(b) Peace Keeper Presence



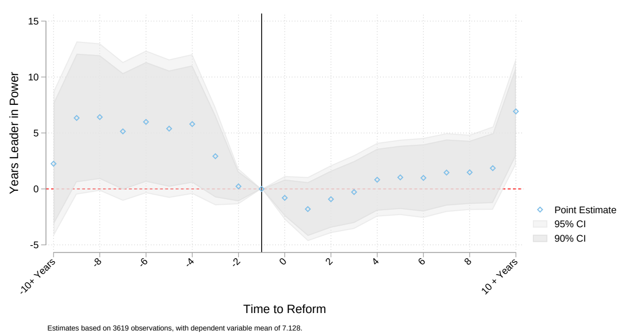
(c) Right Wing Executive



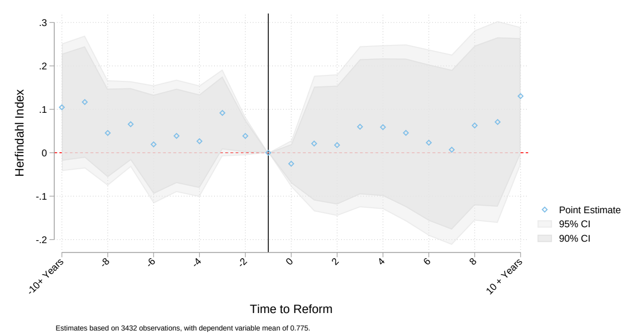
(d) Left Wing Executive



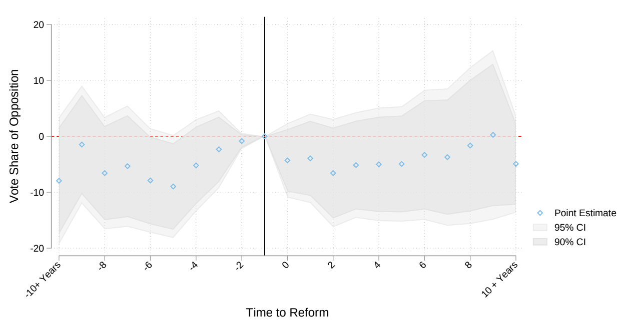
(e) Years in Power



(f) Herfindahl Index



(g) Opposition's Vote Share

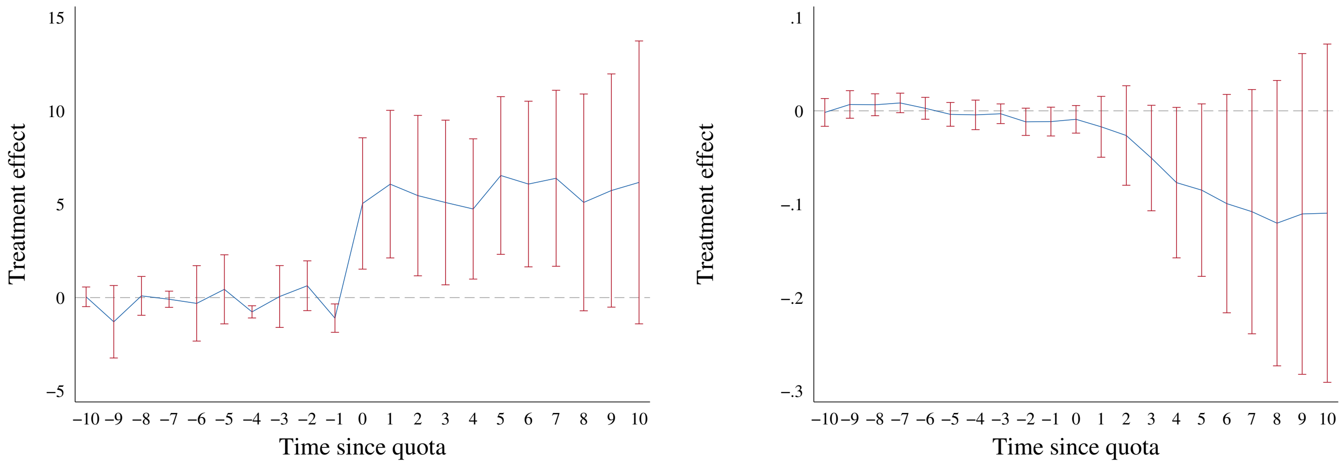


(h) Regime in Transition

Notes: Figure replicates models of variation in quota predictors as flagged in the political science literature presented in Figure A9, however now estimating based on standard event studies. Refer to notes to Figure A9 for variable definitions.

Figure A11: DID_M Estimates conditioning on potential quota predictors – gender quota impacts on women in parliament and maternal mortality

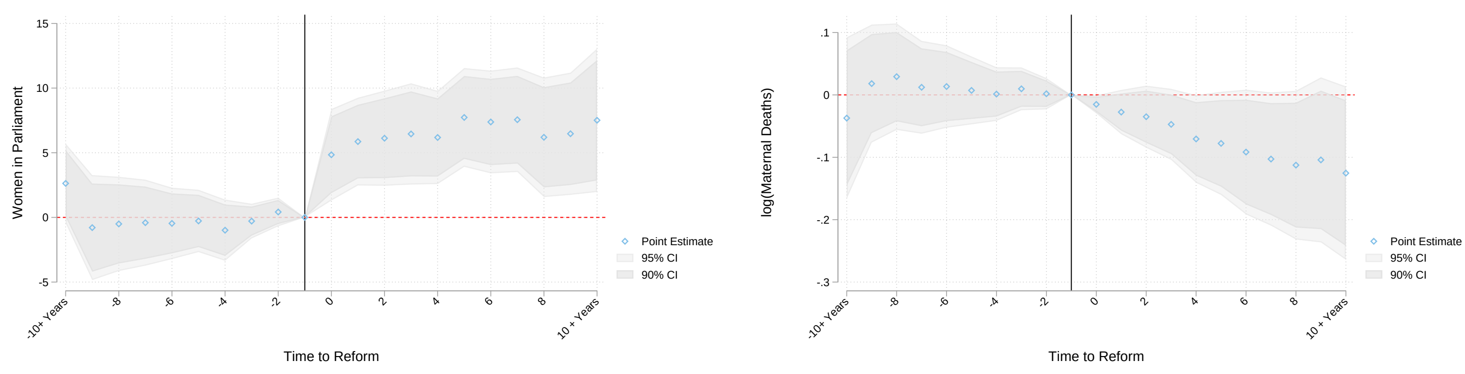
(a) Percent of women in parliament (b) ln(maternal mortality ratio)



Notes: Plots present de Chaisemartin and D'Haultfœuille (2020) DID_M estimates replicating those in Figure 2, however now controlling for indexes constructed from baseline measures of 7 potential predictors of quota timing from the political science literature (Figure A10) and for 18 indicators of women's rights (Figures A7, A8) interacted with post quota indicators. Two separate index×post quota variables are constructed given different phenomena of interest: a first index considering quota predictors, and a second considering empowerment controls. Standard errors are based on a block bootstrap by country.

Figure A12: Conditioning on potential quota predictors – gender quota impacts on women in parliament and maternal mortality

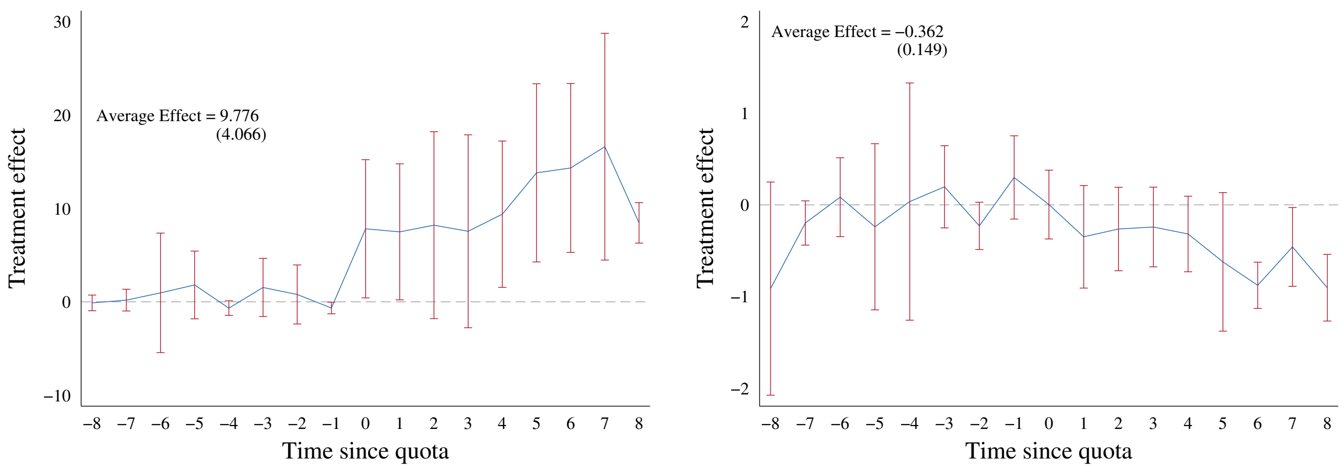
(a) Percent of women in parliament (b) ln(maternal mortality ratio)



Notes: Event studies replicate those in Figure A3, however now controlling for indexes based on baseline measures of 7 potential predictors of quota timing from the political science literature (Figure A10) and for 18 indicators of women's rights (Figures A7, A8) interacted with post quota indicators. Two separate index×post quota variables are constructed given different phenomena of interest: a first index considering quota predictors, and a second considering empowerment controls. Point estimates of the lag and lead terms in the event study specification described in equation 1 are presented, along with their 95% confidence intervals. Estimates are conditional on country and year fixed effects. Time periods greater than 10 years from the reform date are displayed as a single "10 +" indicator. Standard errors are clustered by country. The omitted base category is taken as 1 year prior to the reform, indicated by the solid vertical line.

Figure A13: DHS microdata – gender quota impacts on women in parliament and MMR (DID_M Estimates)

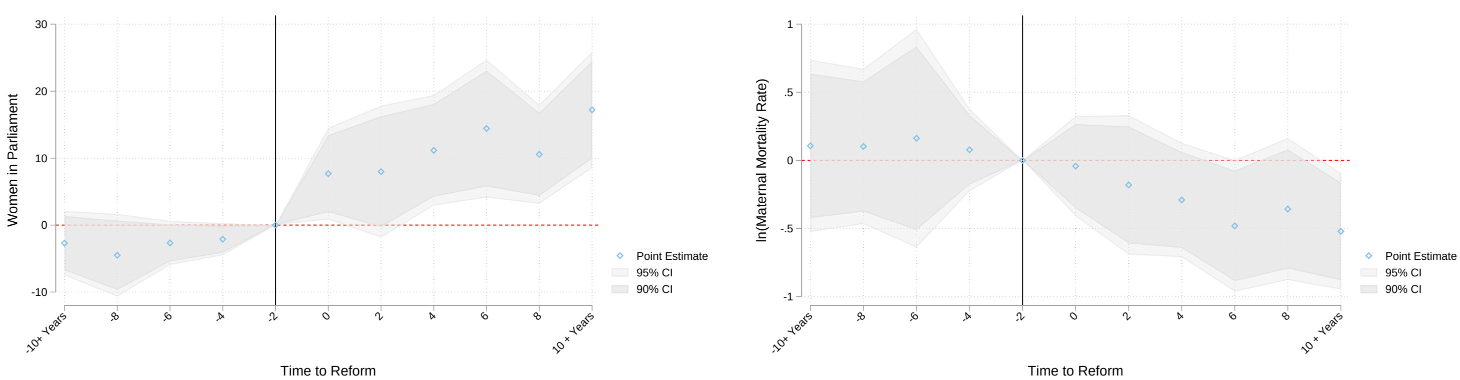
(a) Percent of women in parliament (b) ln(maternal mortality ratio)



Notes: Specification replicates Figure 2, however now replacing world-wide estimates of MMR with maternal mortality calculated from microdata reports from the DHS.

Figure A14: DHS microdata – gender quota impacts on women in parliament and MMR (event study estimates)

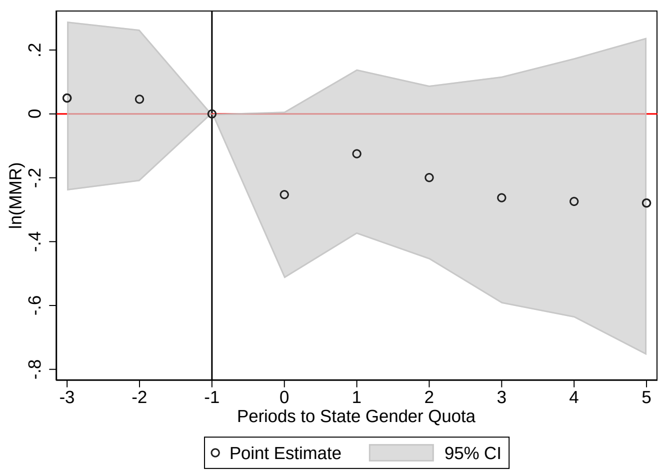
(a) Percent of women in parliament (b) ln(maternal mortality ratio)



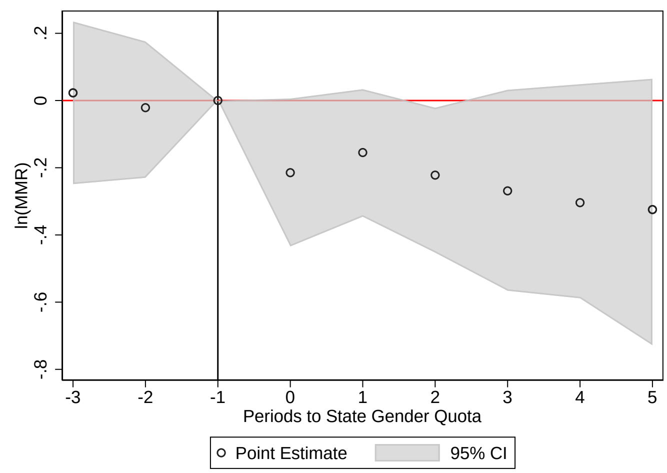
Notes: Specification replicates Figure A3, however now replacing world-wide estimates of MMR with maternal mortality calculated from microdata reports from the DHS. As the DHS maternal mortality module is available in only a subsample of the DHS countries (44 of 68 countries with publicly available surveys), we estimate using 2 year lags/leads to reduce noise. Substantively similar results obtain if using yearly lags and leads.

Figure A15: Event study analysis of reserved seats for women in large Indian states

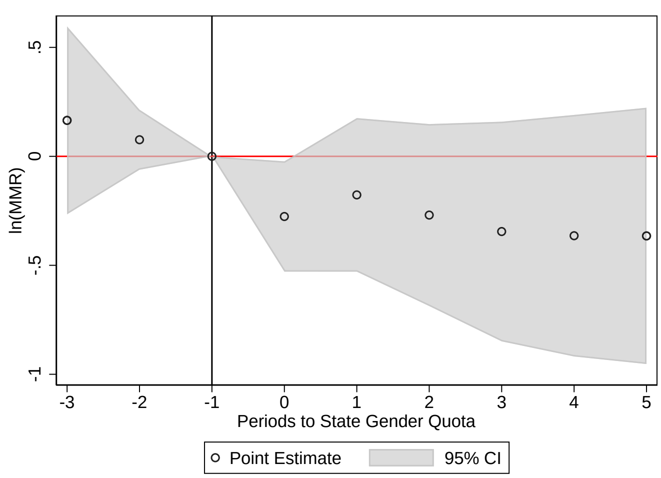
(a) Unweighted Estimates



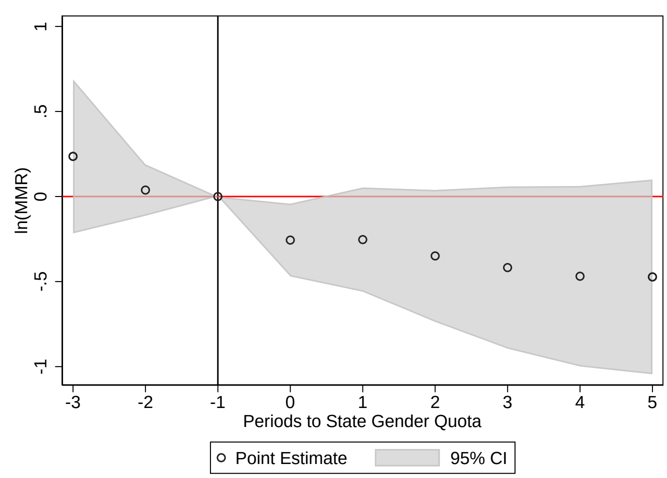
(b) Weighted Estimates



(c) Unweighted Estimates (with linear trends)



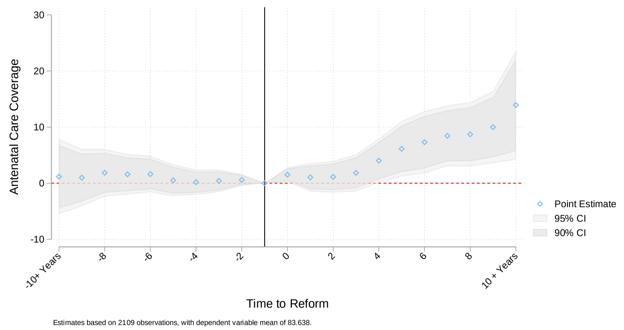
(d) Weighted Estimates (with linear trends)



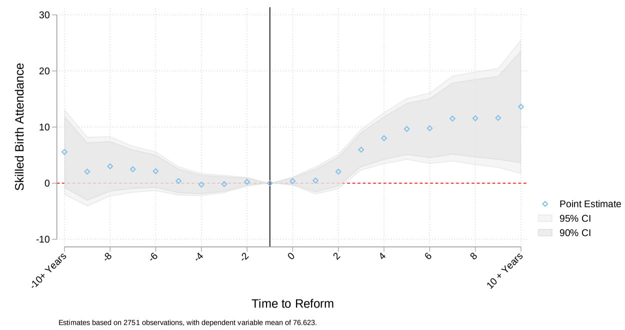
Notes: Event studies are conducted using all available state-level estimates of maternal mortality in India, which are generated provided in the Office of the Registrar General & Census Commissioner's Sample Registration System (SRS) Bulletins. State level reforms refer to the reservation of seats for women in local councils, as described in Iyer et al. (2012). All standard errors are based on wild bootstrapped clustered standard errors (clustered by state), given the relatively low number of states.

Figure A16: Mechanisms: Impacts of gender quotas on intermediate outcomes

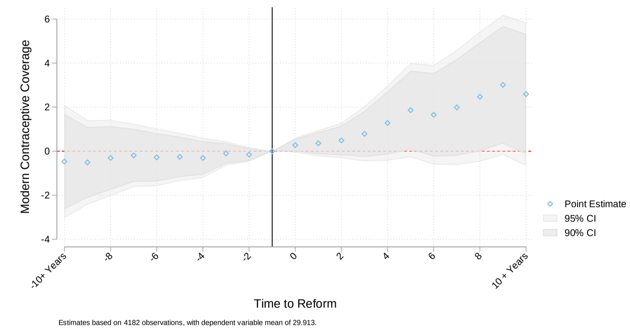
(a) Antenatal care



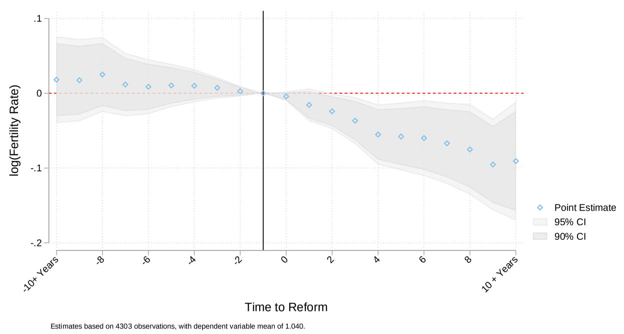
(b) Attended births



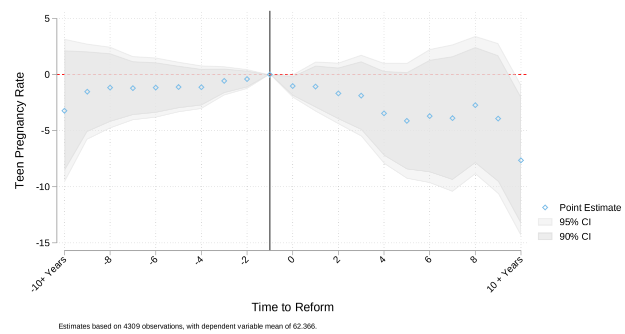
(c) Modern contraceptive cover



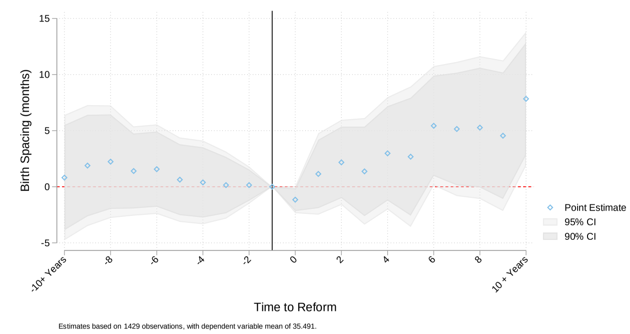
(d) ln(fertility)



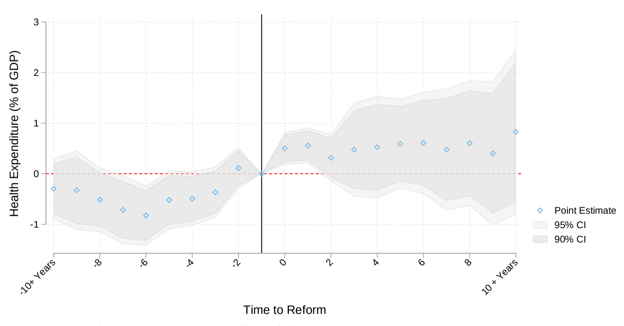
(e) Teen pregnancy



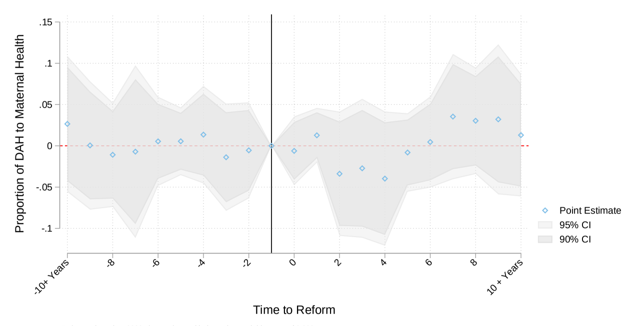
(f) Birth spacing



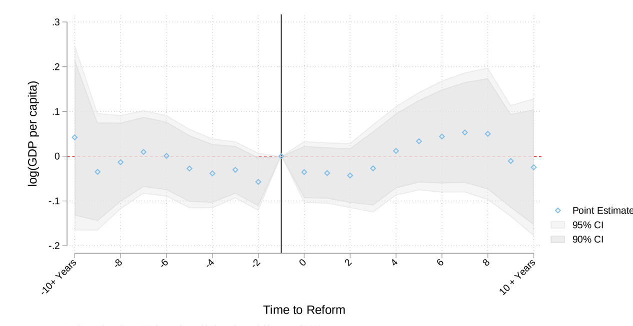
(g) Health expenditure as a % of GDP



(h) Development assistance for maternal health



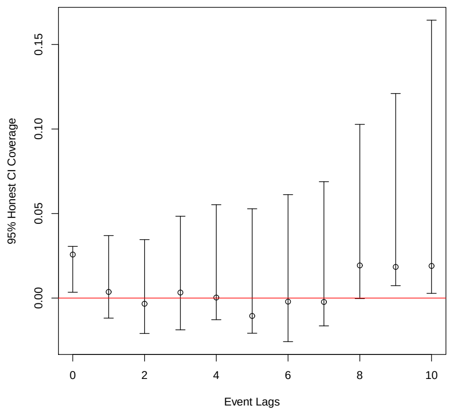
(i) ln(GDP per capital)



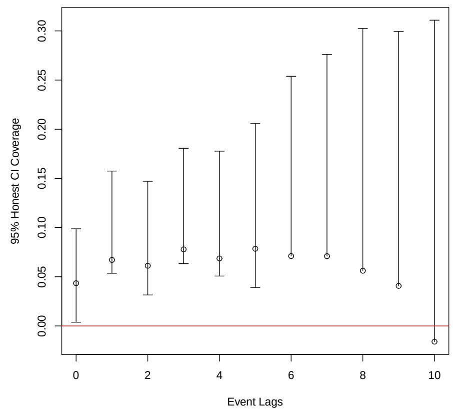
Notes: Event-study estimates of intermediate outcomes replicate those from 6, however now following specification 1. Estimates in Panels A and B use a linearly interpolated measure, event studies based on non-imputed values are available in Appendix Figure B36. All other details follow those of event studies in Figure A3, and variable definitions in Figure 6.

Figure A17: Rambachan and Roth estimates: women's rights and social standing from "Varieties of Democracy" data

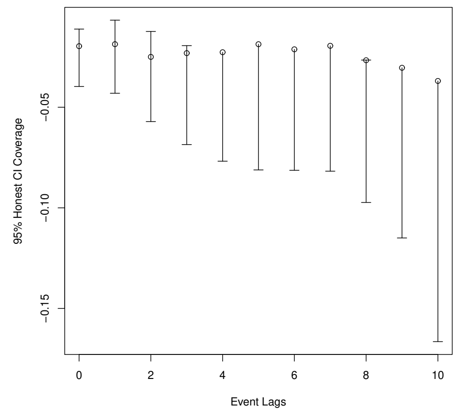
(a) Women's Civil Liberties Index



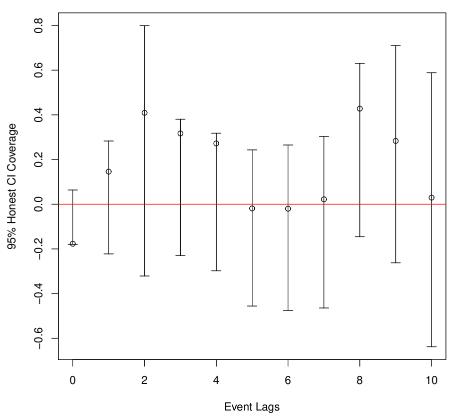
(b) Women's Political Participation Index



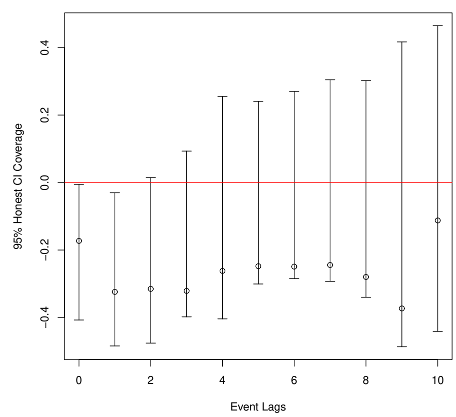
(c) Exclusion by Gender Index



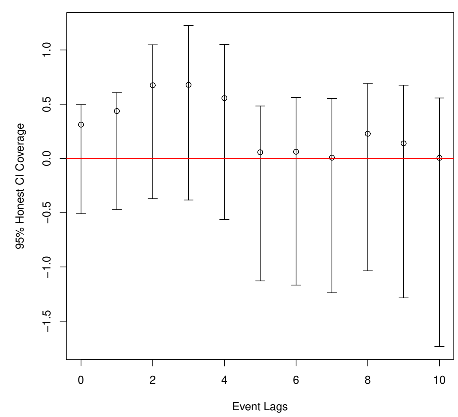
(d) Relative Freedom of Movement for Women



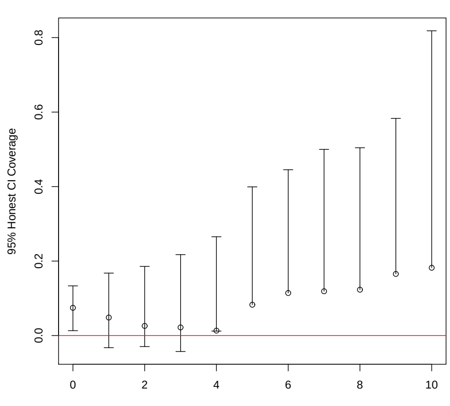
(e) Relative Access to Justice for Women



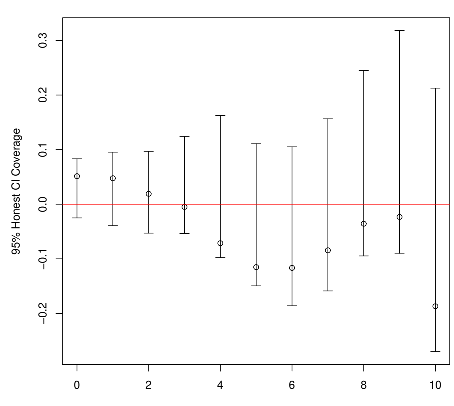
(f) Relative Freedom of Discussion for Women



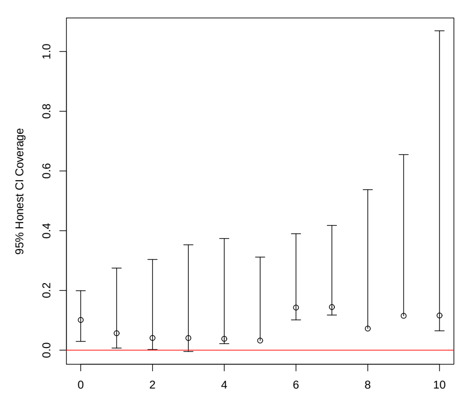
(g) Power distributed by gender



(h) Freedom from forced labor for women



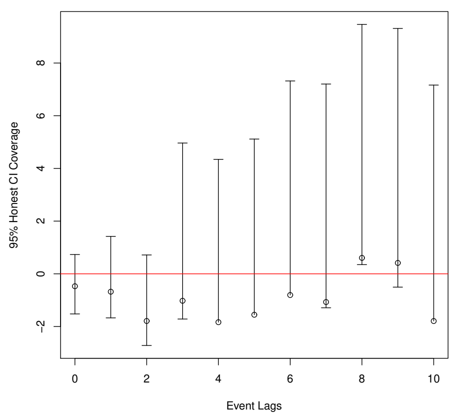
(i) Property rights for women



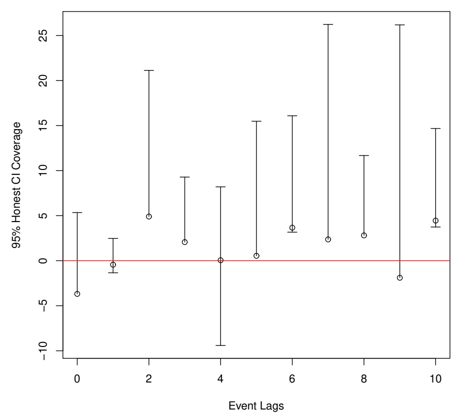
Notes: Refer to Notes to Figure A7. Here models based on identical outcome variables are estimated, however in this case, each post-quota coefficient from event study 1 is documented, along with valid inference under Rambachan and Roth (2020)'s "Honest DiD" methods. Additional notes are available in Figure 4.

Figure A18: Rambachan and Roth estimates: women's rights, empowerment, and women in politics

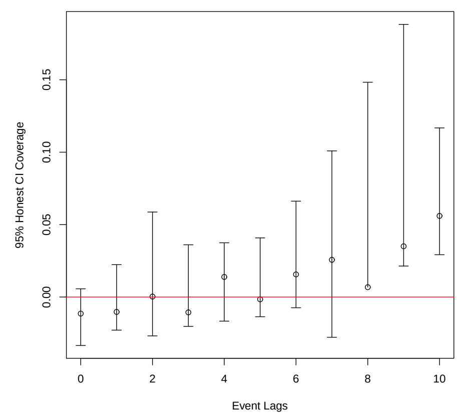
(a) Women, Business & Law Index (WB)



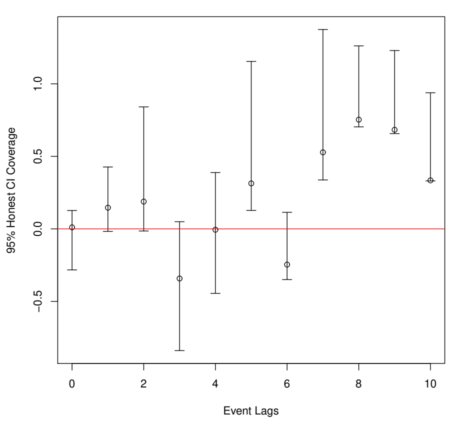
(b) Women Ministers



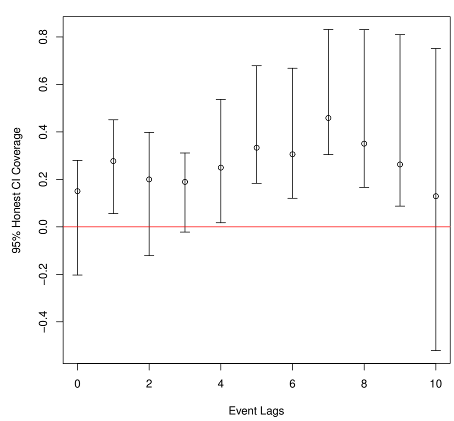
(c) Women's Protests



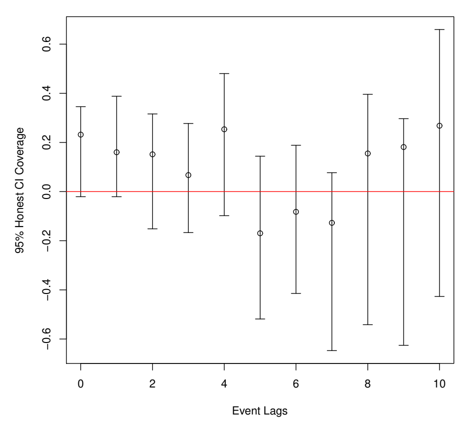
(d) CIRI Women's Social Rights



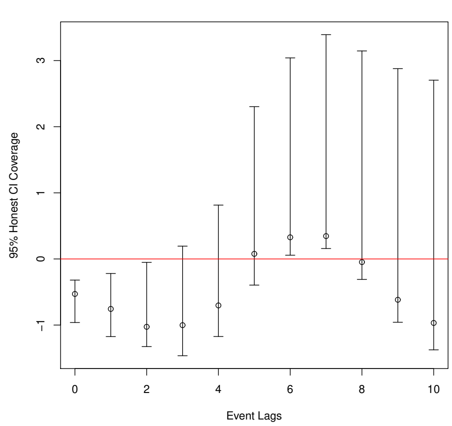
(e) CIRI Women's Political Rights



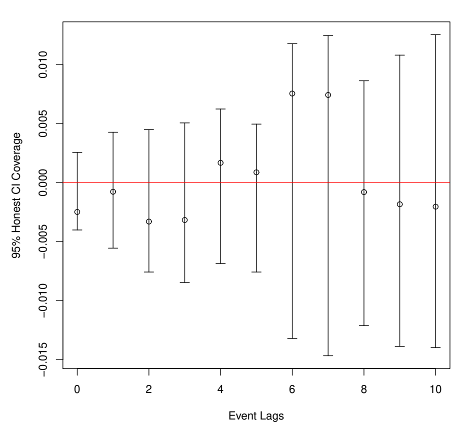
(f) CIRI Women's Economic Rights



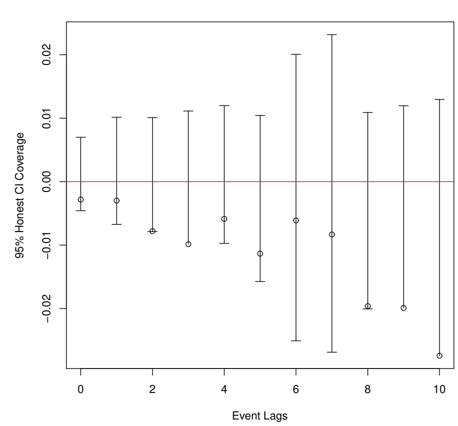
(g) Female Labor Force Participation



(h) Abortion (Save Mother's Life)



(i) Abortion (Fetal Impairment)



Notes: Refer to Figure A8. Here models based on identical outcome variables are estimated, however in this case, each post-quota coefficient from event study 1 is documented, along with valid inference under Rambachan and Roth (2020)'s "Honest DiD" methods. Additional notes are available in Figure 4.

Appendix Tables

Table A1: Summary statistics for reserved seat analysis

	Obs.	Mean	Std. Dev.	Min.	Max.
Panel A: Full Sample					
% Women in Parliament	4186	14.06	10.45	0.00	63.80
Maternal Mortality Ratio	4186	236.03	325.90	3.00	2890.00
Reserved Seats	4186	0.06	0.23	0.00	1.00
Male Mortality Rate (15-60)	4126	240.90	120.48	58.80	753.70
Female Mortality Rate (15-60)	4126	168.08	116.62	34.32	685.03
ln(GDP per capita)	4186	8.90	1.22	5.51	11.77
Democracy (dichotomous)	4159	0.59	0.49	0.00	1.00
Percent of Pregnancies Receiving Prenatal Care	662	84.04	17.75	15.40	100.00
Percent of Births Attended by Skilled Staff	1199	83.33	24.16	5.00	100.00
Health Expenditure as a % of GDP	3124	6.24	2.39	0.72	17.10
Female Infant Mortality Rate (DHS subsample)	1067	0.08	0.05	0.00	0.60
Male Infant Mortality Rate (DHS subsample)	1066	0.10	0.05	0.00	0.33
Birth rates per 1,000 population	4160	24.30	11.74	7.60	55.56
Panel B: Reserved Seat Sample					
% Women in Parliament	501	14.37	11.71	0.00	63.80
Maternal Mortality Ratio	501	447.46	293.09	12.00	1340.00
quotaRes	501	0.48	0.50	0.00	1.00
Male Mortality Rate (15-60)	501	311.09	156.24	75.79	753.70
Female Mortality Rate (15-60)	501	255.54	136.63	66.03	685.03
ln(GDP per capita)	490	7.97	0.94	6.20	10.83
Democracy (dichotomous)	496	0.16	0.37	0.00	1.00
Percent of Pregnancies Receiving Prenatal Care	122	75.01	21.29	24.80	99.10
Percent of Births Attended by Skilled Staff	120	58.04	30.52	7.70	99.90
Health Expenditure as a % of GDP	354	5.51	2.20	0.81	11.59
Female Infant Mortality Rate (DHS subsample)	246	0.09	0.04	0.00	0.23
Male Infant Mortality Rate (DHS subsample)	247	0.10	0.05	0.00	0.33
Birth rates per 1,000 population	475	34.00	9.44	11.90	55.56
Panel C: No Reserved Seat Sample					
% Maternal Mortality Ratio	3834	205.46	316.78	3.00	2890.00
quotaRes	3860	0.00	0.03	0.00	1.00
Male Mortality Rate (15-60)	3748	230.66	110.60	58.80	663.36
Female Mortality Rate (15-60)	3748	155.26	107.16	34.32	626.09
ln(GDP per capita)	3696	9.03	1.19	5.51	11.77
Democracy (dichotomous)	3835	0.63	0.48	0.00	1.00
Percent of Pregnancies Receiving Prenatal Care	556	86.23	16.17	15.40	100.00
Percent of Births Attended by Skilled Staff	1117	86.49	21.45	5.00	100.00
Health Expenditure as a % of GDP	2843	6.32	2.39	0.72	17.10
Female Infant Mortality Rate (DHS subsample)	854	0.08	0.05	0.00	0.60
Male Infant Mortality Rate (DHS subsample)	852	0.09	0.05	0.00	0.26
Birth rates per 1,000 population	3805	23.07	11.40	7.60	52.75

Notes: Refer to Data Appendix A for a full description of each variable and its source. The Maternal Mortality Ratio is measured as deaths per 100,000 live births. For comparison, the male and female mortality rates for 15–60 year-olds is expressed as per 1,000 male and female adults respectively. Reserved seats is a binary variable taking one for each country and year pair where a quota was implemented, and 0 otherwise.

Table A2: Quota Adoption and Women's Inclusion Measures

	Quota Adoption		Quota Adoption$_{(t+1)}$		Quota Adoption$_{(t+2)}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Women civil liberties index	-0.012	0.023	-0.004	0.021	0.025	0.031
	[0.057]	[0.035]	[0.060]	[0.034]	[0.057]	[0.034]
Women political participation index	-0.030	0.014	0.035	-0.001	0.024	-0.012
	[0.029]	[0.017]	[0.031]	[0.017]	[0.029]	[0.017]
Exclusion by Gender index	-0.051	-0.013	-0.066	0.025	-0.082	0.044
	[0.059]	[0.036]	[0.063]	[0.035]	[0.059]	[0.035]
Relative Freedom of Movement	0.001	-0.001	0.001	0.000	0.003*	0.002
	[0.002]	[0.001]	[0.002]	[0.001]	[0.002]	[0.001]
Relative Access to Justice	-0.001	-0.001	-0.002	-0.001	-0.000	-0.001
	[0.003]	[0.002]	[0.003]	[0.002]	[0.003]	[0.002]
Relative Freedom of Discussion	0.003	0.002	-0.002	-0.001	-0.001	-0.002
	[0.002]	[0.002]	[0.003]	[0.002]	[0.002]	[0.002]
Power distributed by gender	0.008	-0.003	-0.010	-0.001	-0.009	0.003
	[0.009]	[0.005]	[0.010]	[0.005]	[0.009]	[0.005]
Freedom from forced labor for women	0.015	-0.000	-0.001	0.001	-0.015	0.000
	[0.010]	[0.006]	[0.011]	[0.006]	[0.010]	[0.006]
Property rights for women	-0.001	0.002	0.003	0.001	0.003	-0.001
	[0.010]	[0.006]	[0.011]	[0.006]	[0.010]	[0.006]
Women, Business & Law Index	0.000	-0.000	-0.001	0.000	-0.000	-0.000
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
CIRI Women's Political Rights	0.001		-0.009		-0.008	
	[0.006]		[0.006]		[0.006]	
CIRI Women's Economic Rights	0.005		-0.008		-0.001	
	[0.005]		[0.005]		[0.005]	
CIRI Women's Social Rights	-0.005		0.004		0.000	
	[0.005]		[0.005]		[0.005]	
Women Ministers	-0.000		0.000		0.000	
	[0.000]		[0.000]		[0.000]	
Female Labour Force Participation	-0.001	-0.000	-0.000	-0.000	0.000	-0.000
	[0.001]	[0.000]	[0.001]	[0.000]	[0.001]	[0.000]
Abortion (Save Woman's Life)	0.004		-0.007		0.003	
	[0.035]		[0.037]		[0.035]	
Abortion (Fetal Impairment)	-0.011		0.003		-0.000	
	[0.028]		[0.030]		[0.028]	
Women's Protest	-0.012	-0.007	-0.006	0.002	0.020*	0.007
	[0.012]	[0.008]	[0.013]	[0.008]	[0.012]	[0.008]
Observations	1678	3700	1678	3700	1678	3700
Number of Countries	128	165	128	165	128	165
Explanatory Power	0.009	0.008	0.007	0.007	0.011	0.012
Joint test (F statistic)	0.772	0.767	0.590	0.695	0.903	1.246
Joint test (p-value)	0.735	0.836	0.909	0.911	0.576	0.152
Country and year FEs	Y	Y	Y	Y	Y	Y
Transformed FEs		Y		Y		Y

Notes: Each specification regresses an indicator of a reserved seat quota being adopted at a certain time horizon (contemporaneously, or 1 or 2 years into the future) on a series of women's empowerment and political measures. "Transformed FEs" refers to models which separate variables with considerable missingness out into fixed effects, including a separate FE for observations with missing information, so as to allow for greater data coverage. These variables are the CIRI rights measures, propotion of women ministers, and measures of abortion legality. Explanatory power refers to the within variation explained by these variables, while joint tests refers to the test of joint significance of all RHS measures. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table A3: Weights and Estimates from the Goodman-Bacon (2021) decomposition

	Weight	Estimate
Panel A: Women in Parliament		
Earlier Treated vs. Later Control	0.024	9.337
Later Treated vs. Earlier Treated	0.015	6.614
Treated vs. Never Treated	0.954	5.739
Treated vs. Already Treated	0.007	-0.614
Difference-in-difference Estimate	5.799	
Panel B: ln(MMR)		
Earlier Treated vs. Later Control	0.024	-0.072
Later Treated vs. Earlier Treated	0.015	-0.007
Treated vs. Never Treated	0.954	-0.076
Treated vs. Already Treated	0.007	-0.018
Difference-in-difference Estimate	-0.075	

Notes: The Goodman-Bacon (2021) decomposition above displays the weights and components making up the global "single coefficient" TWFE model. Decompositions are documented for the percent of women in parliament (panel A) and the natural logarithm of the MMR (panel B). Each components' weight is given along with the point estimate for this comparison. The global estimate is displayed at the foot of each panel.

Table A4: Reserved seats as an IV for women in parliament

	(1) ln(MMR)	(2) ln(MMR)	(3) ln(MMR)
Panel A: LIML Estimates			
% Women in Parliament	-0.015**	-0.020***	-0.015**
	[0.007]	[0.007]	[0.008]
F-Statistic First Stage	7.966	4.753	7.054
p-value First Stage	0.005	0.031	0.009
Weak IV-Robust A-R Confidence Set	[-.031153, .001431]	[-.055575,-.006106]	[-.036705, -.00006]
95% CI from Conley et al. (2012)	[-0.031;0.002]	[-0.037;-0.005]	[-0.032;0.001]
90% CI from Conley et al. (2012)	[-0.029;-0.001]	[-0.035;-0.007]	[-0.029;-0.002]
Panel B: First-Stage Estimates			
Reserved Seat Quota	5.925***	5.144**	6.661***
	[2.099]	[2.360]	[2.508]
Mean of Dep. Var.	4.357	4.397	4.335
Observations	4335	3212	3420
Number of Countries	178	156	171
Controls:			
Democracy & growth	N	Y	N
Empowerment & predictors	N	N	Y

These are instrumental variables regressions in which implementation of gender quotas is used to instrument women in parliament. The first stage regression of women in parliament on reserved seats is displayed in panel B of the table, while F-Statistics of the first stage and the associated p-value are presented below the principal estimates in Panel A. The first-stage F-stats are not consistently larger than standard rules of thumb. To avoid weak instrument problems, we use LIML rather than 2SLS as LIML has improved small sample properties when the IV is weak. Additionally, we present weak IV robust Anderson-Rubin confidence sets, which do not assume identification of coefficients. Power in this setting is graphed in Figure B16). The key take-away is that we can rule out values of zero or, in other words, our finding in the IV setting is robust to weak IV. The displayed coefficients give the effect of an additional percentage of women in parliament on rates of maternal mortality, where women in parliament is instrumented with reserved seats. The 90% and 95% confidence intervals from Conley et al. (2012) are robustness tests, where we allow the instrument to be imperfect in the sense that the exclusion restriction holds approximately but not exactly, allowing quotas to have a direct effect in reducing MMR that is *not* mediated by women in parliament of 0.01 (or 1%). We use Conley et al. (2012)'s Union of Confidence Intervals (UCI) approach, with this process also based on LIML estimation. Each regression includes country and year fixed effects and clusters standard errors by country. Column 1 provides baseline models without controls beyond country and year FE. In column 2 the regressions additionally control for time-varying (and potentially endogenous) GDP and democratization, while column 3 controls for an index of women's empowerment measures at baseline, interacted with year fixed effects. * p<0.10; ** p<0.05; *** p<0.01.

Table A5: Gender Quotas: TWFE impacts on women in parliament and maternal mortality

	% Women in Parliament			ln(Maternal Mortality Ratio)			Maternal Mortality Ratio		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Intensity by Baseline MMR									
Reserved Seats	5.793***			-0.082			-106.107**		
	[2.167]			[0.051]			[43.036]		
Reserved Seats × Baseline MMR		1.008**			-0.020**			-30.209***	
		[0.420]			[0.010]			[6.219]	
Reserved Seats (Low Baseline MMR)			4.727*			-0.077			18.191
			[2.548]			[0.051]			[21.456]
Reserved Seats (Mid Baseline MMR)			2.994*			-0.024			-84.875***
			[1.563]			[0.061]			[17.319]
Reserved Seats (High Baseline MMR)			10.067**			-0.159			-277.155***
			[4.761]			[0.120]			[93.778]
Mean of Dep. Var.	14.110	14.167	14.110	4.357	4.351	4.357	233.425	224.620	233.425
Observations	4335	4203	4335	4335	4203	4335	4335	4203	4335
Number of Countries	178	167	178	178	167	178	178	167	178
R-Squared	0.465	0.470	0.470	0.547	0.547	0.548	0.270	0.361	0.309
Panel B: Intensity by Quota Size									
Reserved Seats	5.793***			-0.082			-106.107**		
	[2.167]			[0.051]			[43.036]		
Reserved Seats × Quota Size		0.290**			-0.005			-6.421***	
		[0.115]			[0.003]			[2.352]	
Reserved Seats (0-10]%			2.809**			-0.006			-25.035
			[1.247]			[0.048]			[27.354]
Reserved Seats (10-20]%			7.516***			-0.069			-60.227**
			[2.291]			[0.074]			[28.187]
Reserved Seats (20-30]%			6.810*			-0.134			-181.188**
			[3.642]			[0.082]			[76.308]
Mean of Dep. Var.	14.110	14.110	14.110	4.357	4.357	4.357	233.425	233.425	233.425
Observations	4335	4335	4335	4335	4335	4335	4335	4335	4335
Number of Countries	178	178	178	178	178	178	178	178	178
R-Squared	0.465	0.468	0.467	0.547	0.547	0.548	0.270	0.288	0.285

Difference-in-differences (two-way fixed effect) estimates of the impact of reserved seats in parliament on women in parliament (columns 1-3), the log of the maternal mortality ratio (columns 4-6), and MMR in levels (columns 7-9) are displayed. In each case country and year fixed effects are included. Baseline two-way fixed effect models are included in columns (1), (4) and (6), and then models studying heterogeneous impacts are presented there-after. Heterogeneity is examined by baseline maternal mortality levels in panel A, and by the size of the gender quota in panel B. In each case heterogeneity considers simple interactions between the reserved seat dummy and the intensity variable (baseline MMR or quota size respectively), and then separate intensity groups for low, medium and high intensity groups. Standard errors clustered by country are displayed in parentheses. * p<0.10; ** p<0.05; *** p<0.01.

Table A6: Two-way FE, DID_M, pooled event studies, and synthetic DID estimates with contemporaneous controls

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcome: ln(MMR)					
Method A: Two-way FE Model						
Reserved Seats	-0.082	-0.082	-0.080	-0.070	-0.071	-0.058
	(0.051)	(0.050)	(0.051)	(0.049)	(0.055)	(0.054)
Method B: DID_M Estimates						
Reserved Seats	-0.072*	-0.074*	-0.074*	-0.069	-0.080*	-0.084*
	(0.043)	(0.043)	(0.044)	(0.044)	(0.047)	(0.050)
Method C: Pooled Event Study						
Reserved Seats	-0.079**	-0.079**	-0.078**	-0.076**	-0.058	-0.053
	(0.032)	(0.032)	(0.038)	(0.037)	(0.036)	(0.044)
Method D: Synthetic DID						
Reserved Seats	-0.127**	-0.131**	-0.129*	-0.103	-0.128*	-0.120*
	(0.062)	(0.061)	(0.074)	(0.063)	(0.073)	(0.071)
Negative Weights	-0.005	-0.006	-0.005	-0.006	-0.012	-0.012
Observations	4335	4335	4305	4324	4335	4294
	Outcome: Women in Parliament					
Method A: Two-way FE Model						
Reserved Seats	5.793***	5.378***	5.712**	5.747***	6.038***	5.551***
	(2.167)	(1.997)	(2.227)	(2.159)	(2.145)	(2.047)
Method B: DID_M Estimates						
Reserved Seats	5.678**	5.242***	5.671**	5.674**	5.167**	4.764**
	(2.222)	(2.034)	(2.304)	(2.226)	(2.154)	(2.036)
Method C: Pooled Event Study						
Reserved Seats	6.622***	6.054***	6.546***	6.603***	6.242***	5.665***
	(1.863)	(1.722)	(1.800)	(2.025)	(1.858)	(1.613)
Method D: Synthetic DID						
Reserved Seats	8.281***	7.775***	8.361**	7.950**	7.661**	7.287**
	(3.041)	(2.793)	(3.535)	(3.401)	(3.099)	(2.846)
Negative Weights	-0.005	-0.006	-0.005	-0.006	-0.012	-0.012
Observations	4335	4335	4305	4324	4335	4294
Controls (contemporaneous):						
Empowerment & Predictors		Y				Y
Democracy			Y			Y
Resources				Y		Y
Region×year FE					Y	Y

Notes: Refer to notes to Table 1. Identical models are estimated, however in this case rather than including baseline×post-quota controls, contemporaneous versions of all control variables are included. * p<0.10; ** p<0.05; *** p<0.01.

Table A7: Alternative inference procedures for measures of maternal mortality in principal aggregate models

	de Chaisemartin and D'Haultfœuille DID_M Estimator		Two-way FE Estimator		Arkhangelsky et al.'s Synthetic DID Estimator	
	ln(MMR) (1)	MMR (2)	ln(MMR) (3)	MMR (4)	ln(MMR) (5)	MMR (6)
Reserved Seats (Point Estimate)	-0.072	-86.46	-0.082	-106.10	-0.127	-57.16
p-value Bootstrap	0.110	0.018	0.128	0.016	0.057	0.101
p-value Triangular Correction	0.099	0.017	0.198	0.020	0.165	0.308
p-value Triangular Correction by Country	0.102	0.021	0.124	0.015	0.050	0.094
p-value Normal Correction	0.325	0.034	0.451	0.142	0.426	0.460
p-value Normal Correction by Country	0.115	0.031	0.138	0.027	0.059	0.117
Mean of Dep. Var.	4.357	233.425	4.357	233.425	4.186	182.757
Observations	4,335	4,335	4,335	4,335	3,068	3,068
Number of Countries	178	178	178	178	118	118

Notes: Resample procedures are implemented following Appendix C. These are based on single coefficient estimators from de Chaisemartin and D'Haultfœuille (2020) (columns 1-2), two way FE estimates (columns 3-4) and Arkhangelsky et al. (2021) (columns 5-6) based on the (required) balanced panel of observations. Point estimates are presented, and below p-values associated with each point estimate, based on different procedures for re-sampling the uncertainty of measures of maternal mortality. In each case, re-samples are taken over country clusters, as treatment is defined at the level of the country.