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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 8-10 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.

JEL Classification: I14, I15, O15

Keywords: Maternal mortality, women's political representation, gender, Quotas, reproductive health services, Fertility, schooling

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Maternal Mortality and Women's Political Power*

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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 to maternal death and disability. 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; Pettersson-Lidbom, 2014; 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

[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.

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 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).[5] 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 parliamentary gender quotas that has swept through developing countries since the mid-1990s. Figure 1b shows that aggregate trends in women's share in parliament track trends in quota coverage (we later provide evidence of causal effects of quotas on this share). We combine information on quota legislation with the first harmonized time series data on maternal mortality across countries for 1990–2015.[6] The main results are as follows. Passage of parliamentary gender quotas leads to a sharp increase in the share of parliamentary seats held by women, demonstrating compliance with quotas. The maternal mortality ratio (MMR) exhibits a sharp decline of about 8.2%.[7] The dynamic impacts are persistent,

[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).

[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.

[5] Ashraf et al. (2020) show that men in Zambia have less accurate information on maternal mortality risk than their partners. It seems plausible then that male policymakers similarly under-estimate MMR risk relative to female policymakers. Powley (2007) cites evidence from Rwanda that women politicians claimed that their experience as mothers was their motivation for joining politics. Moreover, both male and female parliamentarians cited the experience of women parliamentarians as mothers as relevant to their performance.

[6] We analyse legislation reserving a share of seats in parliament for women. We provide brief descriptive statistics and analysis for candidate quotas which 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.

[7] Only so as to provide a summary effect size, we report the two-way fixed effects regression estimates in the body of the paper, but the reader is consistently referred to the dynamic effects shown with alternative estimators. We later show

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 share of seats reserved and in the pre-intervention level of MMR.

The identifying assumption is that the country-specific timing of quota adoption is quasi-random. In addition to showing the standard event study estimates, we consistently show estimates of placebo effects using the estimator of de Chaisemartin and D'Haultfœuille (2020), and these mitigate concerns over omitted variables.[8] The de Chaisemartin and D'Haultfœuille (2020) estimator also 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 (Goodman-Bacon, 2021). Estimates of bounds on the dynamic effects following Rambachan and Roth (2020) confirm that the main results hold if we relax the parallel trends assumption. They also hold if we follow an alternative procedure that creates synthetic controls for each treated unit, by matching on pre-trends in MMR (Abadie et al., 2010).[9]

All of this evidence indicates that MMR was not declining until gender quotas were adopted and that, for varying dates of adoption by country, it declined a lot after quotas were adopted. One might still postulate that an underlying trend in gender progressivity might have triggered quota legislation and also led to lower MMR, with gender 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). To investigate this, we test whether gender quota adoption is preceded by an upturn in gender progressivity. Using 18 indicators of women's economic rights, civil liberties, property rights and access to justice as markers of attitudinal shifts, we find limited evidence of this in the standard event study pre-trends. This is even clearer in the placebo coefficients estimated following de Chaisemartin and D'Haultfœuille (2020). We also show that these indicators of gender equality are, in general, not predictive of lower MMR, the way that quota adoption is. This is not entirely surprising – while the progression of gender equality is

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diagnostics which suggest that the two-way FE estimates do not suffer the biases highlighted in the recent literature (Table 1).

[8]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 tests of the placebo coefficients estimated following de Chaisemartin and D'Haultfœuille (2020) are robust to this.

[9]In a further specification check, we estimate a 2SLS model in which MMR is regressed on the share of women in parliament, instrumenting the latter with quota legislation. Following Conley et al. (2012), we estimate bounds on the IV 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. We find that the upper and lower bounds are negative, consistent with quotas leading to a decline in MMR.

likely to eventually culminate in increased attention to women's reproductive health, this is likely to be a slow process. In contrast, parliamentary gender quotas give women *instrumental* power to action improvements. 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 moved on to conduct a similar exercise for predictors of quota adoption indicated in the (largely qualitative) political science literature (Krook, 2010). None of the seven potential predictors we study exhibits a statistically significant blip in the run up to quota adoption. Nevertheless, as we may be under-powered to detect pre-trends, we investigate robustness of our estimates to controlling flexibly for the full suite of 25 variables, 18 relating to gender equality and 7 to political determinants. The coefficient of interest is a bit larger and not significantly different from the baseline coefficient.

The estimates are subject to a series of other tests. We document decomposition of the two-way FE estimator following Goodman-Bacon (2021). The share of units with negative weights is small. The results indicate limited bias in the single coefficient model estimates so we display these. The individual ("2×2") treatment effects are quite tightly clustered around the pooled treatment effect, which suggests the reduction in maternal mortality is observed across adopting countries. We also perform a leave-one-out analysis, which confirms that no one country is driving the average results. We show that our findings are robust to a series of controls, including country-specific linear trends, region×year fixed effects and potentially endogenous time-varying democratization, Development Assistance for Maternal Health (DAH), GDP and share of GDP dedicated to public health. We investigate sensitivity of the estimates to dropping high income countries, restricting the sample to be balanced before vs. after quota adoption, to alternative windows, to a constant sample for the main results and the mechanisms, and to weighting by country population. The results hold up in all of these cases. As a possible concern with aggregate data is that our findings are driven by compositional change, we investigated changes in the composition of women giving birth. To do this we used the full reproductive histories of women available in the multi-country Demographic and Health Surveys (DHS) microdata to create pseudo panels. 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 also demonstrate robustness of the results to conditioning upon the demographic composition of mothers.

Since 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 survey measure of MMR, albeit for a restricted sample of countries for which the DHS are available. We replicate our findings in India, where the timing of state-level legislation mandating gender quotas in local government has been shown to be as good as randomly assigned (Iyer et al., 2012). Finally, we recognize that poor surveillance and tracking of MMR may be a marker of it being a low policy priority. If women parliamentarians address this, improving surveillance by capturing maternal deaths more comprehensively, then our estimates will be biased downward.

Turning to mechanisms, we identify quota-led improvements in reproductive health services coverage, the key determinant of MMR highlighted in scientific research and that the World Health Organization (WHO) recommends action on (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%) and of antenatal care of 4 to 8 percentage points (4.8–9.6%). The standard event studies and the dynamic coefficients of de Chaisemartin and D'Haultfœuille (2020) suggest an increases in contraceptive coverage of 1.7 percent, though this result does not hold with the honest DiD bounds.[10] We investigated whether gender quotas lead to the passage of abortion legislation which is relevant because barriers to safe abortion lead to unsafe abortion, a major cause of MMR (Girum and Wasie, 2017), but we found no evidence of this, possibly because there are strong religious barriers to abortion law which women leaders may not be able to surmount.

We further investigated whether an increased presence of women in parliament modified demand side determinants of MMR, including women's rights, female education and fertility. We find no discernible impacts of quotas on women's rights including for instance property rights. We find an increase of 0.5 years in the education of young women, which Bhalotra and Clarke (2013) establish as a mechanism, showing quasi-experimental evidence that expanding female education brings MMR down. We identify a decline in the total fertility rate of 6–7% (consistent with the observed expansion of contraceptive coverage and women's schooling). We also find an increase in birth spacing of 2 to 5 months, though this is not robust to the honest DiD check. A considerable literature documents a positive association of fertility with MMR (Girum and Wasie, 2017). In addition to being a likely mechanism for the decline in maternal death risk per birth (the definition of MMR), the decline

[10]Reproductive health coverage data come from an entirely different source than data on MMR, so our finding that it responds to quota implementation reinforces the validity of the results. There is scientific consensus on the key role that antenatal care and attended delivery play in MMR reduction, and the WHO recommends universal access to them (WHO, 2014; Jamison et al., 2013). Among the few causal studies available, Pettersson-Lidbom (2014) estimates that a 1% increase in the share of midwife-assisted home-births decreased MMR by 2% in 19th century Sweden, and Anderson et al. (2016) document that occupational licensing of midwives reduced MMR by 6-8 percent in the US in the early 20th century. Antenatal care is critical to identifying eclampsia and pre-eclampsia, which are among the proximate causes of MMR.

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.[11]

Acknowledging broader concerns about reducing MMR in low-resource settings, we now discuss how the improvements in reproductive health, fertility and education outcomes that we document might have been achieved. First, we explain why these changes are feasible without a major increase in resources, and then we investigate whether gender quotas increased available resources. Resource costs are low because supply side expansions are facilitated by low wages (Banke-Thomas et al., 2020) and, on the demand side, low cost information programs on the benefits of preventive behaviors coupled with female schooling can increase uptake.[12] Conditional upon resources, there is potential to reduce MMR by reducing inefficiency and corruption. Women may achieve larger reductions in MMR because they have different preferences (Baskaran and Hessami, 2019; Baskaran et al., 2018; Lippmann, 2020; Clayton et al., 2017; Okuyama, 2018), different information sets (Ashraf et al., 2020), are less corrupt, or have greater intrinsic motivation (Folbre, 2012; Brollo and Troiano, 2016; Baskaran et al., 2018), or because women citizens feel more able to raise their concerns with women representatives (Parthasarathy et al., 2019). In section 7, we discuss evidence that is broadly consistent with leaders being able to move the outcomes they prioritize by influencing building consensus, provoking legislation and improving policy design and delivery, including better targeting and greater outreach.

Although we emphasize that resources are not necessarily a significant constraint on action, we investigated whether gender quotas led to an increase in available resources or a reallocation of resources within the health budget. We find no impact of gender quotas on GDP, the share of GDP dedicated to health spending, or international development assistance for maternal health (DAH).[13] However, in some specifications we find evidence of an increase in state health expenditure, consistent with previous evidence that women prioritize health spending (Miller, 2008; Bhalotra and Clots-Figueras, 2014; Bhalotra et al., 2019; Campbell, 2004). To inves-

[11] 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.

[12] 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, Bhalotra et al. (2017, 2021) show that a home visiting program providing information on preventive health behaviours improved infant health and future outcomes. Bhalotra and Clots-Figueras (2014) provide evidence that electing women to India's state assemblies leads to an increase in both public health infrastructure and utilization of services. Currie and Moretti (2003) show that educated women are more likely to use prenatal care and have fewer births.

[13] International aid is thought to have made a significant contribution since 2000, following the Millennium Development Goals agreement, which included MMR reduction (Dieleman et al., 2016).

tigate whether there were generalized improvements in health or possibly detrimental impacts on other health outcomes (because a larger share of the health budget was channeled towards MMR reduction), we examined impacts of gender quotas on male mortality in the reproductive age band (a crude analogue of MMR), on tuberculosis (a widespread but gender-neutral condition among adults) and on infant mortality (a widely used marker of population health). These other indicators of population health do not record significant changes following passage of gender quotas.

To summarize, using longitudinal cross-country data across a period of 25 years that encompasses periods of dramatic but uneven decline in maternal mortality, we provide compelling new evidence that gender quotas that raise the share of women in parliament can lead to substantial declines in maternal mortality. The overall decline in MMR of 8–10% 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 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.

Our study makes two key contributions. We are the first to 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 appears to be no recognition among policy makers of the political economy constraints on achieving this. Second, we provide what would appear to be the first systematic analysis of the impacts of the recent wave of implementation of gender quotas across countries.[14] 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;

[14]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.

Swers, 2005; Clots-Figueras, 2012; Baskaran and Hessami, 2019).[15] 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). Women disproportionately bear the costs of bad health: Most women have at least one birth, women incur a risk of dying in childbirth that in quota-adopting countries in our sample is around 1 death in every 220 births, they bear the burden of replacing children that die (Bhalotra and Van Soest, 2008), and they are the main caregivers for sick adults and children. MMR represents a cross between these priorities– women and public health. In contrast to most public health outcomes, MMR 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 section 7. We conclude in section 8.

2 The Policy Landscape

Maternal Mortality. Although women have been dying in and around childbirth since the origin of life, international initiatives to tackle this 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 called for a three-quarters reduction in MMR between 1990 and 2015. A second target of achieving universal access to

[15] This evidence emerges from studying impacts of women's suffrage, competitive election of women, and gender quotas. The evidence on reserved seat quotas is dominated by analysis of the randomized implementation of gender quotas in local government in India. Comparability of existing work with our analysis of gender quotas in national parliaments is limited for two reasons. First, the Indian quotas were for headship as well as membership of village councils, whereas the quotas we analyze are for membership of parliament. Second, representation in local government may have very different substantive impacts as village council members are often directly in contact with their constituents. 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). Evidence on quotas in richer countries mostly refers to candidate seat quotas, which operate differently and typically with less success, see for example, Bagues and Campa (2017).

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.[16] While the main focus of our study is reserved seat quotas, since 1990 the number of countries with candidate list quotas for women has also risen sharply, from 1 to 46. Candidate quotas were passed mostly in middle- and higher-income countries (see Appendix Figure A1), and we shall briefly provide a separate analysis of them.

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 time series estimates of MMR across 183 countries, released in 2015 by the United Nations Maternal Mortality Estimation Inter-Agency Group (MMEIG). These 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 allows for this in inference, and we also show results using MMR data derived from the Demographic and Health Surveys.

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

[16]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. Two other countries implemented quotas more recently and fall outside our study period: Nepal in 2016 and UAE in 2019.

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).

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 Demographic and Health Survey, and the state-level time series 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. We display estimates of flexible event study models (Jacobson et al., 1993), tracking outcome trends following quota implementation. The estimated equation is:

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

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).

The β^{lag} coefficients capture dynamic impacts and the β^{lead} coefficients test the identifying assumption of no differential pre-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 provide bounds on the post-quota coefficients using the Rambachan and Roth (2020) "Honest DiD" estimator that provides bounds 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 additionally 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).

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.[17] To address this, we show results using the dynamic estimator proposed by de Chaisemartin and D'Haultfœuille (2020), 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. We also 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. To do this we use data on numerous indicators of the progressive realization of women's rights. Using a similar approach, we investigate a series of

[17]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 DD 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 DD estimator. Extending this work, de Chaisemartin and D'Haultfœuille (2020) demonstrate that when treatment effects are heterogeneous, some of these weights might be negative.

predictors of quota adoption discussed in the political science literature.

We supplement the event study style plots with single coefficient estimates of a two-way fixed effect 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 (we alternatively refer to this as a 'difference in difference', or DD model). The DD 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 DD model will 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 estimate, and aggregate dynamic estimates from de Chaisemartin and D'Haultfœuille's "DID_M" estimator which do not suffer from these concerns.[18]

In a specification check, we drop the 51 high income countries from the sample so that the control group is more homogeneous. In general, though, we retain in the sample 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). 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, indicators of women's rights and gender equality, predictors of quota implementation identified by political scientists, demographics such as population structure and ethnic fractionalization and potentially endogenous intermediate outcomes including DAH, GDP and public health expenditure. We discuss a suite of other robustness checks in section 6, that were summarized in the Introduction.

5 Results

Main Results. Estimates of equation 1 are in Figure 2. 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. In both cases, the changes are persistent. The lead coefficients mitigate concerns about endogeneity of policy adoption as they are not

[18]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 IV estimator to compute bounds.

significantly different from zero. The point estimates from the single coefficient models (Table A2) indicate statistically significant effects on both outcomes. The proportion of women in parliament increases 5.8 percentage points which, relative to the average in 1985–1990 of 9%, represents a 64% increase.[19] The maternal mortality ratio falls by 8.2%.

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. 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. The evolution of impacts in Figure 2 are consistent with this. Ten years out, MMR was 13% lower in countries that passed quotas.

Heterogeneity in Impacts. 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" (Figure B7, panel A). The unweighted estimates from the single-coefficient model (Table B2) indicate that quotas of less than 15% increase the share of women in parliament by 2.8 percentage points, with point estimates suggesting a 3.5% reduction in MMR, quotas of 15 to 20% raise the share of women in parliament by 11.9 percentage points and reduce MMR by 3.7% quotas of 20–30% increase the share of women in parliament by 6.8 percentage points and reduce MMR by 13.4% (we note that these estimates are less precise than pooled estimates). Baseline rates of MMR varied considerably and we may expect that there is more room for MMR reduction in countries where it is initially high. Using terciles of baseline MMR, this is exactly what we find (Figure B7, panel B). We find a 7.7% reduction in MMR in countries with a low baseline rate and a 12.9% reduction in countries with a high baseline rate (Table B3).

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);

[19]The median (mean) gender quota is 21% (20%). The estimated impacts of quotas on the proportion of women in parliament 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 B4 and B5 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. In some countries, it took time from quota passage until fulfillment. In Niger, for instance, the quota was in 2000 but the next election in 2004. Event studies describe unrestricted dynamic coefficients, and we show a series of estimates leaving out one country at a time.

Bagues and Esteve-Volart (2012). We find no impact on MMR (Appendix Figure B8). 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]

6 Robustness Checks

In this section we first show results with alternative estimators that provide cleaner tests for pre-trends and unbiased estimates of dynamic effects under treatment effect heterogeneity. We then investigate women's rights and the political environment as potential drivers of quota adoption and MMR decline. We test for changes in the composition of women giving birth, and then we investigate sensitivity to changes in estimation sample, covariates, specification, and to measurement and surveillance of MMR.

Alternative estimators and pre-trends. Estimates following de Chaisemartin and D'Haultfœuille (2020) are in Figure 3A-3B, with a graduated set of controls. These aggregate estimates of changes in MMR between adopters and non-adopters, comparing periods surrounding adoption. The placebo coefficients are tightly centered on zero, showing no prevailing differences prior to quota adoption. The estimates are robust to controls and the averaged dynamic effects range between 7.2% and 10.3%, similar to the MMR decline indicated by the standard two-way FE models. Similarly, estimates of gender quotas on the percent of women in parliament vary from around 5–6 percentage points, lining up with those from two-way FE models. We additionally test 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 B4).

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, preferring 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. See Appendix B for details of the procedures. The results

[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.

in Figure 4 again show no evidence of differential pre-trends, and a significant post-reform decline in MMR. 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 (corresponding to 95% confidence intervals). For both women in parliament (Figure 5a) and MMR (Figure 5b) these bounds are informative at 95% or, in some cases in later periods, at 90%.

The aggregated estimate of de Chaisemartin and D'Haultfœuille (2020) provides a "single-coefficient" estimate pooling all post-treatment periods. Such a measure is useful as a summary effect size. Similar effects under alternative weighting schemes and assumptions can be generated from a standard two-way fixed effect model or by pooling lagged estimates from the standard event study. It turns out that these estimates are similar (Table 1), ranging from 7.2-8.2% in models without controls, and 9.5 to 10.3% in models with controls. Thus, in this setting, the bias in the single coefficient two-way FE model discussed in Goodman-Bacon (2021); de Chaisemartin and D'Haultfœuille (2020) appears to be small. This is illuminated by the Goodman-Bacon (2021) decomposition of the identifying variation into its treatment vs. pure control and differential timing components. We find that 95.4% of the estimated effect derives from double-difference comparison of treated with never-treated units (Table A3). The drop in MMR (of about 7%) is similar when we compare early to late adopters (prior to adoption) as when we compare aggregate DD estimates of treated vs. never treated countries, albeit the weight attached to the latter is much greater (panel (b)).[21] The fact that the majority of weights in this decomposition are from comparing treated to pure control units in a DD setting, and that the share of units with 'negative weights' of de Chaisemartin and D'Haultfœuille (2020) is small (Table 1) explains why the two-way FE estimate is close to alternative estimates which more completely isolate the pure treatment versus control comparisons. Figure A3 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 B9-B10, 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 2SLS regressions of MMR on the

[21] On the other hand the DD 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 which adopted in 1989), and once again the pooled estimate here is negligible which again makes sense as both units have adopted quotas.

share of women in parliament, instrumented with quota implementation. Now 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% 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 3.1% reduction in maternal mortality for a 1 percentage point increase in the share of women in parliament.[22] As the instrument does not always pass a weak instrument test, we do not rely upon this evidence but, rather, view it as adding to the overall weight of the evidence provided in this section.

Predictors of gender quota legislation. We investigate the possibility that gender quota legislation is an aspect of a more general trend of increasing redistribution towards women. History suggests that often laws are passed only when society is ready to comply with them (Doepke and Zilibotti, 2005; Platteau and Wahhaj, 2014). The material concern is that the reduction in MMR that we identify is not caused by having more women in parliament but, rather, by evolving social preferences. We pulled together data on 18 indicators of gender progressivity in the political, economic and civil domains to examine whether 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. Event study estimates regressing the indicator on the event of quota legislation are in Figures A4 and A5. The full range of variables is visible in the Figures, and they include indices of women's civil liberties, access to justice, economic rights, women's protests and the passage of abortion law. Similar results are observed using the de Chaisemartin and D'Haultfœuille estimator in Figures A6 and A7. We can reject a positive pre-trend for each of the 18 indicators. There is no evidence here that quotas were adopted following improvements in other measures of equality for women. At the same time there is 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.

These 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. Evidence

[22]The unweighted estimates produce estimates in an entirely negative range, whether or not India and China are in the sample, upholding the main results. However, if we drop India and China (because the population of any other country pales in comparison) and weight by population, then the bounds include zero.

consistent with a discontinuous change in political will lies in our finding that reproductive health coverage, women's education and fertility all move in an MMR-reducing direction following quota adoption (Table B5). In contrast, we find limited associations of the 18 indicators of women's equality with MMR decline (Tables B6-B7).

If it not women's rights then what is it that determines quota legislation? We could find no systematic quantitative analysis of this but, based on case studies, the political science literature indicates the possible relevance of pressure from international organizations (proxied by overseas development assistance), occasions of broader constitutional reform including transitions into democracy, post-conflict reconstruction and the presence of peace-keeping forces (Krook, 2010; Baines and Rubio-Marin, 2005).[23] One might imagine that these same factors had direct impacts on MMR decline– for instance, democratization has been shown to lead to lower infant mortality (Kudamatsu, 2012), and there were substantial increases in development assistance for maternal health (DAH) in the wake of MMR being agreed as a Millennium Development Goal (Dieleman et al., 2016). We will show that our estimates are robust to conditioning upon democratization and DAH but we nevertheless scrutinize pre-trends in seven measures of the political environment suggested in the literature, see Figure A8. While somewhat noisily estimated, and not always with significant pre-trends, the standard event studies reveal a tendency for quotas to be adopted following democratic transitions (panels (e) and (h)), an increase in overseas development assistance (panel (a)) and in more competitive political environments (panel (f)). However these associations are no longer evident in de Chaisemartin and D'Haultfœuille (2020) estimates in Figure A9. We also find no association between the political variables and MMR, undermining the concern that they might be the underlying drivers of MMR decline (Table B8). Importantly, we control for all of the 25 potential predictors, women's rights and political variables in the main analysis and the estimated impact of quotas on MMR is larger and not statistically significantly different from the baseline estimate, see Figure 6.

Other correlated shocks. We have shown, using several complementary tests, that the estimates are robust to concerns about differential pre-existing trends between treated and control units, and that numerous potential predictors of quotas are not the underlying driver of MMR decline. A potential threat to identification that remains is that gender quota legislation is either part of a package of other changes that occur at the same time or soon after and that are correlated with MMR. Some of these might be regarded as outcomes of the quota and thus part of the quota effect on MMR. For instance, if gender quotas led to higher GDP and this led to a

[23] 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.

decline in MMR then GDP would be a mechanism. We test GDP and other intermediate outcomes in section 7. Here, we show results conditional upon democratization, GDP, the share of GDP spent on public health and international development aid for maternal health (DAH), see Figures B11 (for maternal mortality) and B12 for women in parliament. We investigate compositional change in the next section, and Figure B11 panel (d) shows results estimates controlling for time-varying measures of the age and educational composition of mothers. To allow for unobservables, we show estimates conditional on country-specific linear trends and region×year fixed effects in panel (e). The coefficients of interest are, in general, not sensitive to controls.[24] A different concern is that inference in our specification treats the data as independent across countries, but not within countries. To address potential concerns 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 Figure B13. While the confidence intervals are now wider, we still observe statistically significant effects. We document the stability of point estimates across a wide range of alternative models in Figure A10, discussed at more length below.

It is notable that GDP has no impact on the share of women in parliament, but it has a significant direct impact – a 1% increase in current GDP is associated with a decline in MMR of around 0.33% (Table B5). A crude back-of-the-envelope calculation assuming log-linearity (conditional on country and year FEs), suggests that to achieve the 10% reduction in MMR that we estimate as flowing from quota adoption, GDP would have to increase by nearly 30%. Democracy has a direct impact on both outcomes, increasing women in parliament, and decreasing maternal mortality conditional on quotas, albeit only when the democracy score is above the mean.[25] 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). However, a number of the quota implementing countries in our sample were non-democratic.[26,27] Our results

[24] Recall that the sample contains 22 reforming countries, along with 156 non-reforming countries. We identify the 22 countries by isolating a common event/shock that they experienced, which is quota legislation. It seems unlikely that there are reinforcing confounding events that are common across these different countries, independent of quotas and predictive of MMR. Nevertheless, we have assessed sensitivity to a very large set of observable and unobservable shocks.

[25] Democracy raises women's share when the score is at least 6 on a scale 0–10, and directly impacts MMR when the score is 9 or 10.

[26] There is no clear agreement on the precise definition of democracy. Besley and Kudamatsu (2008) discuss two particular cut-offs based on democracy scores issued in Polity IV. According to the often-used definition where any non-zero Polity-IV score is classified as democratic, 7 of 22 quota adopting countries were non-democratic in the 5 years pre- and post-quota adoption (China, Eritrea, Morocco, Rwanda, Saudi Arabia, Swaziland and Uganda).

[27] In the case of Saudia Arabia, a monarchy, the 150-member Majlis Ash-Shura or the Shura Council, a consultative council, is the equivalent of a single lower house. The Shura is an advisory body that is appointed by the King for four-year terms. The members are usually chosen from among scholars, experts and specialists. The role of Shura council is to "review laws and question ministers." The quotas instituted in 2011 and implemented in 2013, led to the entry of women into the Shura, and led to three women becoming chairpersons of three Shura committees: Human Rights and

are consistent with women acting upon their innate preferences, potentially motivated by the mission of public service rather than by electoral motives.

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. Aggregate country-year data on fertility would not allow us to investigate this. To do 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.[28] We model quota impacts on birth rates of women of different education and age categories, and quota impacts 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 Figure B14 and B15.[29]

Sensitivity to sample. For reasons discussed earlier, we include all available never-treated countries in the estimation sample but we now assess sensitivity to dropping the 51 high income countries. This yields essentially identical estimates (Figure B13), consistent with the MMR profile of these countries being relatively flat.[30] 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 A10). Estimates based on shorter time-horizons of 5 and 8 years pre- and post-quota adoption also substantively agree with the baseline results using a 10 year window (Tables B9, B10, B11). As the available data for the mechanisms checks discussed in Section 7 are more sparse than for the main results, we re-estimate the main results on the common sample, see Figure B17. The results are less precise in this smaller sample but the broad patterns remain. As the countries in the sample vary considerably in population size, we re-estimate the equation with population weights, which also affords a test of model mis-specification Solon et al. (2015). This mis-specification refers, in our case, to a test of heterogeneity across

Petitions Committee, Information and Cultural Committee, **Health Affairs** and Environment Committee (Alharbi, 2015). Professor Lubna A Al-Ansary, who took over the Health Affairs and Environment committee is herself a medical doctor and a Professor of medicine with extensive experience promoting and supporting evidence-based health care in Gulf countries.

[28]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 TFR is constructed as a weighted average of age-specific fertility rates.

[29]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.

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

countries with different population sizes. We formally test this following Solon et al. (2015, footnote 11) and Deaton (1997, p. 72) and can reject the presence of mis-specification for both models examining women in parliament and maternal mortality rates. Since China and India are outliers in population size, the weighted estimates exclude them (China implemented quotas, India did not), see Figure B13, panels (e)–(f). So as to isolate changes ensuing from weighting from changes associated with removing these countries, we consistently show unweighted estimates on the reduced sample (Figure B13 panels (g)–(h)). When including time-varying controls, the point estimates in a single term DD model are larger with China and India excluded, and again larger when weighted. However, all changes in the sequence are not statistically meaningful (Table A2).

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). In the main analysis we showed heterogeneity by baseline rates. We now show results replacing the logarithm with the level of the rate. Table A2 show that the main results hold. 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 B12 (where we additionally document other cutoffs, as well as dropping rather than winsorizing at the top end).

As explained in the Data section, the global country panel of maternal mortality data that we use were generated using a range of vital and demographic data sources (Alkema et al., 2017, 2016; World Health Organization, 2015; Wilmoth et al., 2012). We replaced this with an alternative measure which we derived 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 our data cover 1990-2015, the same years as in the main analysis. Using this smaller sample of countries and the survey-based measure of MMR, we find a similar pattern of results (Figure 7). The timing of the decline is similar and, while less precisely estimated, the effects are larger, consistent with the DHS countries having higher MMR on average.

Given the multiplicity of data sources for some countries and the paucity in others, the UN estimates of MMR used in the main analysis are modelled, and thus come with their own uncertainty intervals. About 76% of the country-year observations are original survey data points, the remaining 24% being imputed. First, Figure A10 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.[31] The results in Table A5 show that estimates assuming a triangular distribution are generally less demanding than those assuming a normal distribution, and that allowing for correlation within country reduces the estimated uncertainty. When the dependent variable is in logarithms as in the main analysis, p-values based on the two distributional assumptions are larger than the standard bootstrapped p-values and fall between 0.062 and 0.146 (column 1). When the dependent variable is in levels, the new p-values fall between 0.051 and 0.083 (column 4).

A potential concern is that the availability and the quality of MMR data may be endogenous if women parliamentarians, motivated by a concern to reduce MMR, improve surveillance and tracking of MMR (we measure what we treasure). This will tend to 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.

Sub-national estimates for India. Implementation of gender quotas in India was randomized at the village level but there are no MMR data at the village level, and India has not implemented parliamentary quotas at state or national level. However, state level legislative reform implementing gender quotas in village councils has been shown to be as good as randomly assigned (Iyer et al., 2012). We used their state-year data on the timing of implementation (see data appendix A) to estimate Figure A11 which shows robustness to including linear trends and population weights. Although less precise given the more limited variation available, these estimates confirm our main finding of a decline in maternal mortality, of 14.2% (Table B13). This result is robust to leaving out one state at a time, see Figure B18.

7 Mechanisms

Supply Side. We first investigate whether gender quotas influence the reproductive health services that have become conventional wisdom for policy. Prenatal care can help identify 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). Contraceptive coverage

[31] 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 A5 replicates the estimates from Table A2, 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.

may reduce fertility, and high fertility is a proximate cause of MMR (Girum and Wasie, 2017).[32] Contraceptive coverage can also lower MMR without changing fertility by lengthening birth spacing or by substituting unsafe abortion (Miller and Valente, 2016).

Figure 8 panels (a)-(c) shows increased rates of coverage along all three dimensions of reproductive health in the years following quotas. The single coefficient models 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. Univariate descriptive associations of MMR with reproductive health coverage indicators on our analysis sample are in Table B14. 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%. 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.

We investigated whether political power for women led to pro-female legislation being enacted. Legalizing abortion is directly related to women's reproductive health, unsafe abortion accounting for a between 5 and 13 percent of maternal deaths. Using data from Elías et al. (2017), we find no impact of quotas on abortion legislation, see Figure A5. A possible reason is that there are strong religious barriers to abortion law which women leaders may not be able to surmount. In the context of testing our identifying assumption we earlier displayed event studies for seventeen other indices of gender-related progressivity, some but not all of which reflect legislative changes (for instance, property rights for women), see Figure A4 and the remainder of Figure A5. Specification checks based on de Chaisemartin and D'Haultfœuille (2020) and Rambachan and Roth (2020) are provided as Figures A6, A7, A12 and A13. In the discussion of identification, we pointed to the placebo estimates. Now, we point to the post-quota coefficients, to see if quotas result in improvements in women's rights or liberties as these are potential mechanisms. Other than political participation, quotas do not influence any of these outcomes.

Demand Side. We model the educational attainment of young women, birth spacing and fertility as potential mediators. We find that the education of girls aged 15–19 at the time of the reform increases by around 0.5 years (Figure A14 panel (a), Table B18). This is in line with evidence from India that quotas for women in

[32] 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).

local government led to an increase in girl's schooling, the authors suggesting the channel was an increase in female aspirations (Beaman et al., 2009). Evidence that increases in women's schooling lead to lower MMR is in Bhalotra and Clarke (2013). We also investigated the education of young men, identifying an upward tendency, smaller than that for women and not statistically significant (Figure A14 panel b). There was a significant increase in the ratio of female to male education post-quota (panel c). We find a decline in fertility of 6% and evidence suggestive of an increase in birth spacing of 2 months using the DHS microdata (Figure 8), consistent with expansion of contraceptive coverage. 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. Substantively similar results are observed in de Chaisemartin and D'Haultfœuille estimates (Figure 9), in "Honest DiD" bounds (Figure 10) and when using a common sample across all mechanism variables (Figure B17, though note that a small number of variables have sparse coverage, so estimation is noisier).

As discussed, high fertility, defined as the number of children per woman, is associated with higher risks per birth, so the decline in fertility is a likely 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 B19 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.[33] 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.[34]

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

[33]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 B16).

[34]It is 64% of the decline in deaths captured by MMR. Table B19 should be seen as an accounting exercise, varying MMR 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.

improvements in other population health outcomes. This is also relevant to identifying whether a renewed focus on MMR was associated with deterioration in other population health outcomes. We investigated adult male (and female) mortality, mortality from TB,[35] and infant mortality, which is widely regarded as a marker of population health in infectious disease environments such as those which characterize developing countries. The event study plots are in Figure B20 and the DD coefficients in Tables B20-B21.

Mortality among adult males shows no significant change following gender quota adoption, with quite tightly estimated zeros until at least 5 years post-quota. Since adult female mortality is not entirely driven by MMR, for closer comparison, we present estimates for adult female mortality. There is some evidence of this declining, but this is only statistically significant once we use population weights. 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 share of MMR in female adult mortality ranges from 0.002 (Finland, Greece and Poland) to 0.343 (Niger). 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.[36] We also observe no significant change in tuberculosis or infant mortality following quota adoption.[37] Overall, the evidence points to gender quotas being more effective at improving women's reproductive health and survival than in addressing other population health indicators, though we can reject any deterioration in them. We suggest that both priorities and the potential to target women may play a role.

Resources and Resource Allocation. We examined whether quota adoption led to an increase in available

[35]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).

[36]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.

[37]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 event study estimates are imprecise (Figure B20), two-way fixed effects are insignificant (Table B21), and the honest DiD estimator suggests no effect at least up to the eighth lag (Figure B21). 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.

resources (Figure 8), but find no evidence of an increase in GDP or in international development assistance for maternal health (DAH).[38] We looked at DAH as this was increased in response to the MDGs (one of which was MMR reduction) since 2000, although less so since 2010. We also examined national health expenditure as a share of GDP. The estimates are imprecise in the standard event study (Figure 8g) and the de Chaisemartin and D'Haultfœuille coefficient plot (Figure 9). As these data are only available for years 1995-2013 (for all countries), we show alternative estimates imputing missing data or normalizing on population rather than GDP (Figure B19). There is a significant increase in specifications modelling the level of health spending rather than its share (panels g, h). The single coefficient model indicates a significant increase of 0.89 percentage points or about 14% following quota implementation (Table B17). As there is some weak evidence of pre-trends in the figures we account for this by producing "Honest DiD" bounds (Figure 10g) 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 but since we find large impacts of quotas on maternal mortality conditional upon this (and conditional on GDP and DAH, see Figure B11), it seems that MMR reduction does not rely upon increasing public expenditure. In the next section, we highlight the low costs of public service expansion in developing countries, and the many margins for action that do not require expenditure.

Evidence in Previous Research. In this section, we refer to a broader literature which illustrates the mechanisms by which politicians can act. 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.[39]

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) and Lippmann (2020) analyze parliamentary text data for Uganda and France respectively, finding that female legislators are most active on women's issues. In France, men contribute more to

[38] In one of the many specifications, the single coefficient estimate obtained with population weights in Table B17), we observe a decline in DAH for maternal health. It is relevant to note here that even in quota adopting countries, DAH for maternal health is only a very small proportion of all health spending, around 500 times lower than government health spending. In 2015, US$36.4 billion was disbursed in DAH, and 9.8% was for maternal health (Dieleman et al., 2016).

[39] Voting is often along party lines, and platforms too are often influenced by party leaders, so these are not always useful measures of preferences.

military issues. Analyzing minutes of local council meetings, Baskaran and Hessami (2019) show that election of an additional woman leads to childcare being discussed more and to expansion of public childcare places. In a similar vein, using the text of UK House of Commons debates, Bhalotra et al. (2019) show that women are significantly more likely than men to talk about families, health and education, while men are more likely to talk about Europe, taxes and the military. Voting behavior in the UK is along party lines but they document gender differences in abstention and rebellion. They also show that women parliamentarians speak less than their male counterparts, conditional on numerical representation. Parthasarathy et al. (2019) show that women citizens in Indian village councils speak less often than men. Importantly, they show that gender quotas improve the likelihood that women citizens are heard. In line with this, Iyer et al. (2012) show that violence against women is more likely to be reported after gender quotas are introduced in local councils in India. Using data from Japan, Okuyama (2018) finds that female legislators are more likely to support petitions on gender issues and reproductive rights. This is important because, in contrast to the case of roll-call votes, legislators do not necessarily act along party lines when considering petitions. 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.[40] With the exception of Baskaran and Hessami (2019), these studies do not attempt to show movement on hard outcomes.

On women bringing resources to domains they prioritize, Miller (2008) shows that women's suffrage led to higher health expenditure. Bhalotra and Clots-Figueras (2014) show that women are more likely than men to build village-level health infrastructure. They also show that a range of maternal health-seeking behaviors improve under women leaders – including immunization, antenatal care, iron supplementation during pregnancy, giving birth in a government facility as opposed to at home (note: they establish no change in the probability of giving birth in a private facility). On legislation, Clots-Figueras (2012) shows that women in India's state legislatures influence pro-female legislation.

Policy action on the margins that we identify (expanding the cadre of skilled prenatal and birth attendants and educating young women) is relatively low-cost because wages are low in developing countries, and mid-

[40] 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).

wives, 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). There is also considerable scope to improve public services by limiting leakage of public funds on account of corruption. Women politicians in India and Brazil 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). There is also evidence of how much slack there is in many sectors 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 and less prone to the policy distortions created by electoral incentives (Baskaran et al., 2018; Brollo and Troiano, 2016). If women have more information on MMR (Ashraf et al., 2020), they may also be better able to target resources. Finally, 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). Women are more likely to exert this effort on MMR reduction if they prioritize it.[41]

8 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. Despite significant progress, especially since 2000, preventable maternal mortality remains high. The lifetime risk of maternal mortality is 1 in 45 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

[41] 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) show that educating women leads to increased health-seeking including prenatal care.

Development Goals. This paper shows that SDG 3.1 targeting maternal mortality reduction is complementary to SDG 5.5 targeting an increase in women's political representation.

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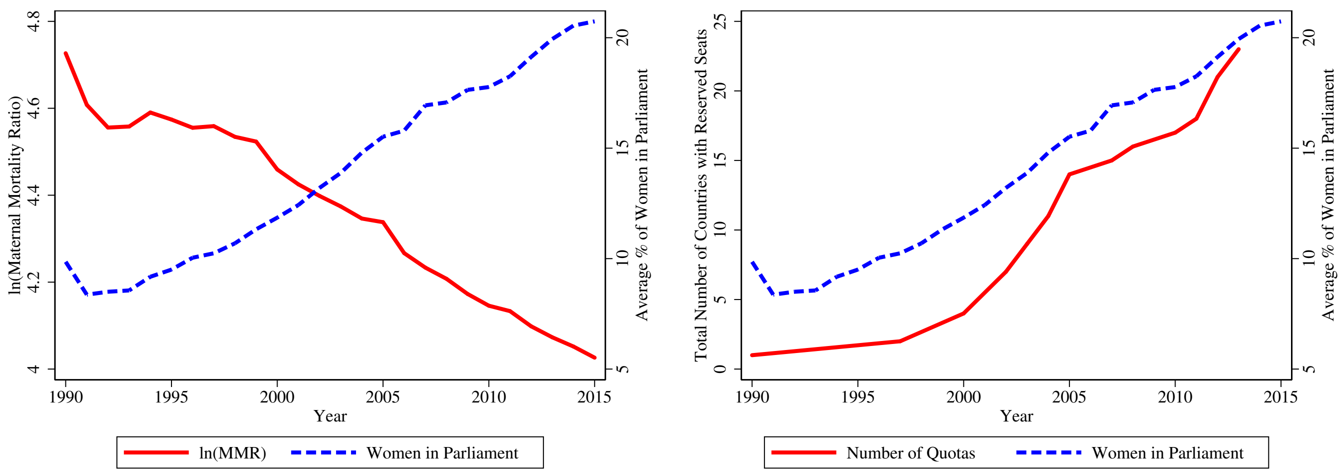
Figures

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

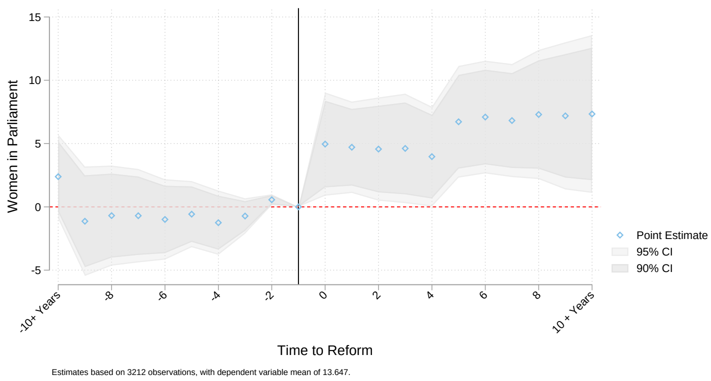
(a) Women in parliament and ln(maternal mortality ratio) (b) Reserved seats and women in parliament



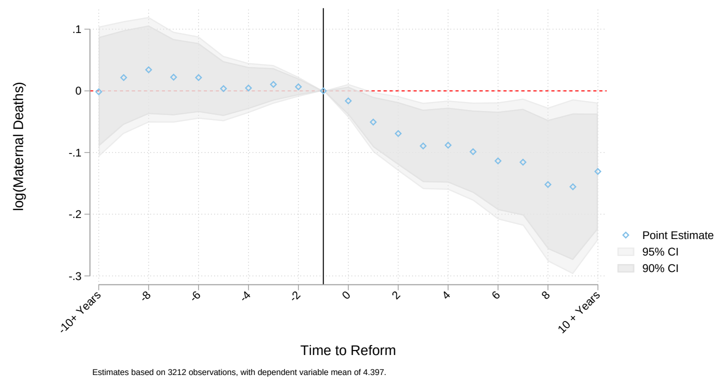
Notes: Raw trends in number of countries with parliamentary gender quotas, the percentage of women in parliamentary seats and the log of the maternal mortality ratio. Data sources are provided in the Data Appendix. The sample is a global sample of 178 countries for which we have annual data through 1990–2015.

Figure 2: Gender quota impacts on women in parliament and maternal mortality

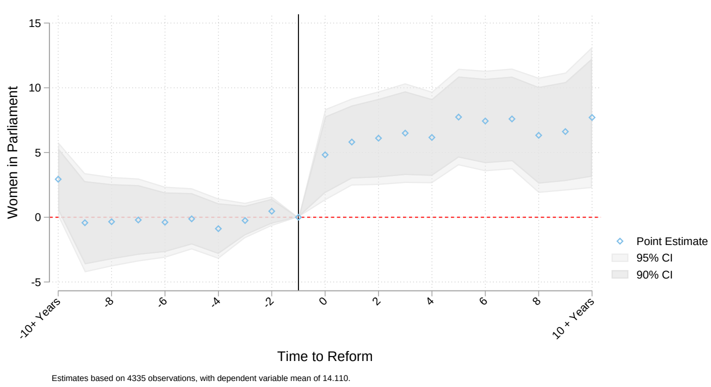
(a) Percent of women in parliament with time-varying controls



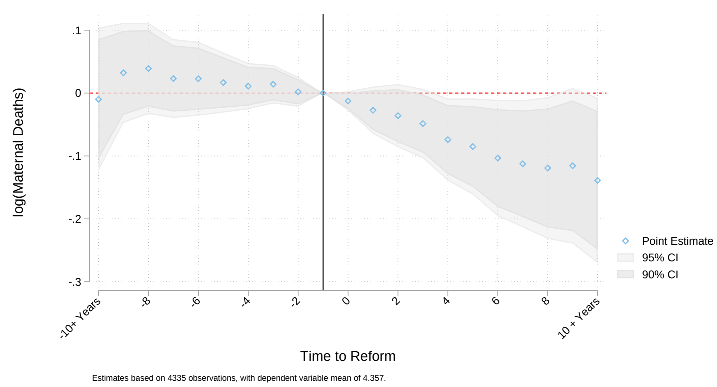
(b) ln(maternal mortality ratio) with time-varying controls



(c) Percent of women in parliament with no controls



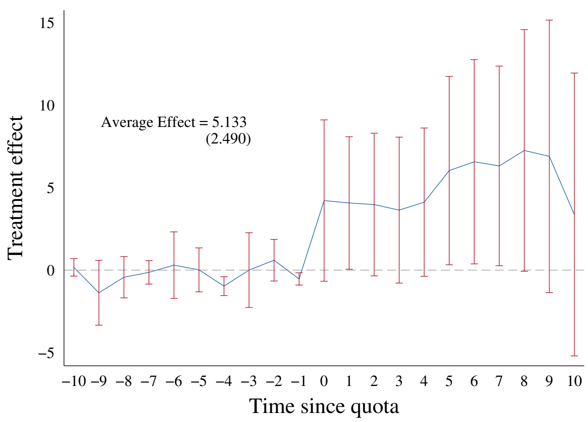
(d) ln(maternal mortality ratio) with no controls



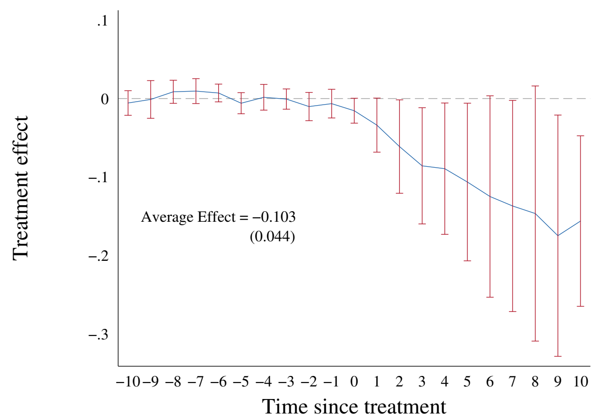
Notes: Point estimates of the lag and lead terms in the event study specification described in equation 1 are presented, along with their 95% confidence intervals. Estimates are conditional on country and year fixed effects. Panels (a) and (b) additionally control for the log of GDP per capita and democracy 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 3A: Gender quota impacts: de Chaisemartin and D'Haultfœuille's DID_M estimator and placebos

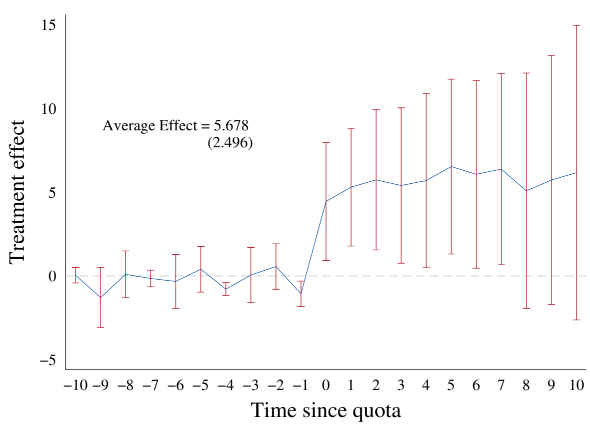
(a) Percent of women in parliament with time-varying controls



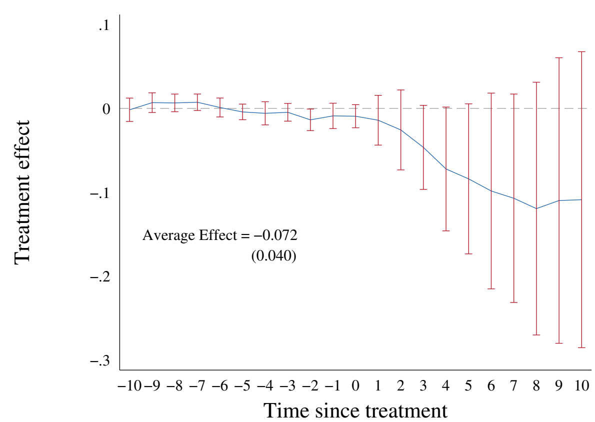
(b) ln(maternal mortality ratio) with time-varying controls



(c) Percent of women in parliament with no controls



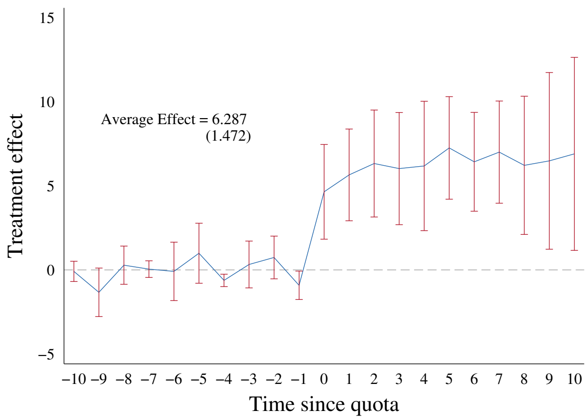
(d) ln(maternal mortality ratio) with no controls



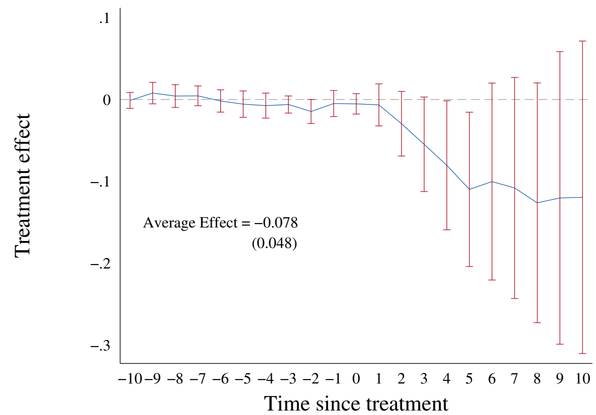
Notes: Panels (a) to (d) present estimates of the impact of quotas on women in parliament and maternal mortality following de Chaisemartin and D'Haultfœuille (2020). This consists of estimating aggregate impacts comparing all changers with non-changers surrounding the time of reform. Plots present 10 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 GDP and democracy levels for women in parliament (panel (a)) and ln(MMR) (panel (b)). Panels (c) and (d) present baseline models with no controls. Additional specifications are provided in Figure 3B below. All inference is conducted using a block bootstrap, and average effects and standard errors are estimated by pooling all immediate and dynamic effects. For reference, the proportion of all time by country ATTs which receive a negative weight according to de Chaisemartin and D'Haultfœuille (2020)'s criteria is 4.8%, with the sum of these negative weights being very small, at -0.0053.

Figure 3B: Gender quota impacts: de Chaisemartin and D'Haultfœuille's DID_M estimator and placebos

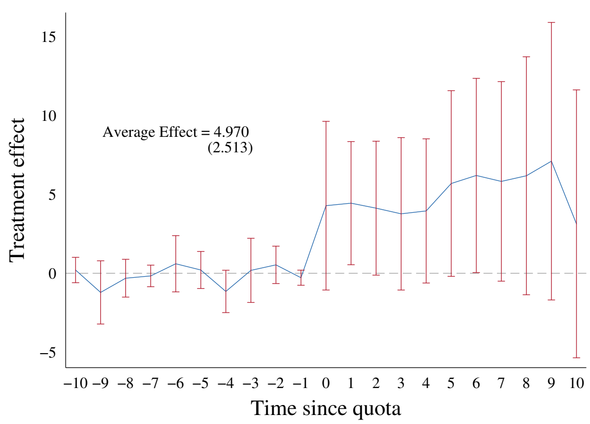
(a) Percent of women in parliament with country time trends



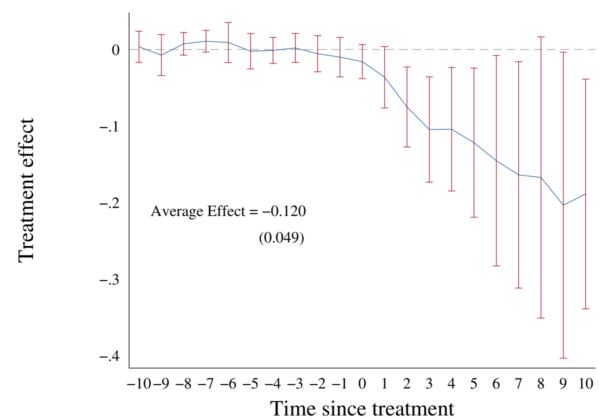
(b) ln(maternal mortality ratio) with country time trends



(c) Percent of women in parliament with subregion×year FEs



(d) ln(maternal mortality ratio) with subregion×year FEs

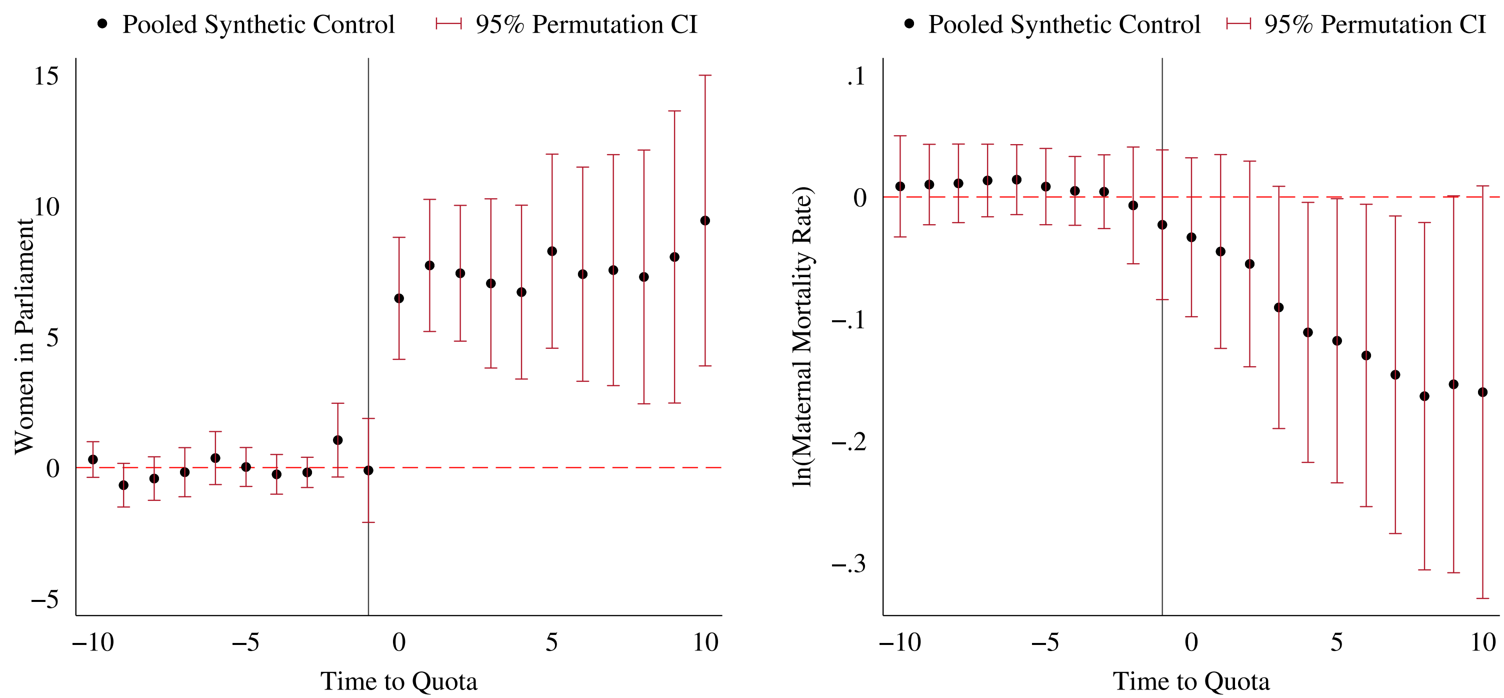


Notes: Refer to notes to Figure 3A. Alternative models are estimated where models add country-specific linear trends (panels a-b), and UN sub-region by year FEs (panels c-d). All other details follow those from Figure 3A.

Figure 4: Matched synthetic controls

(a) Percent of women in parliament

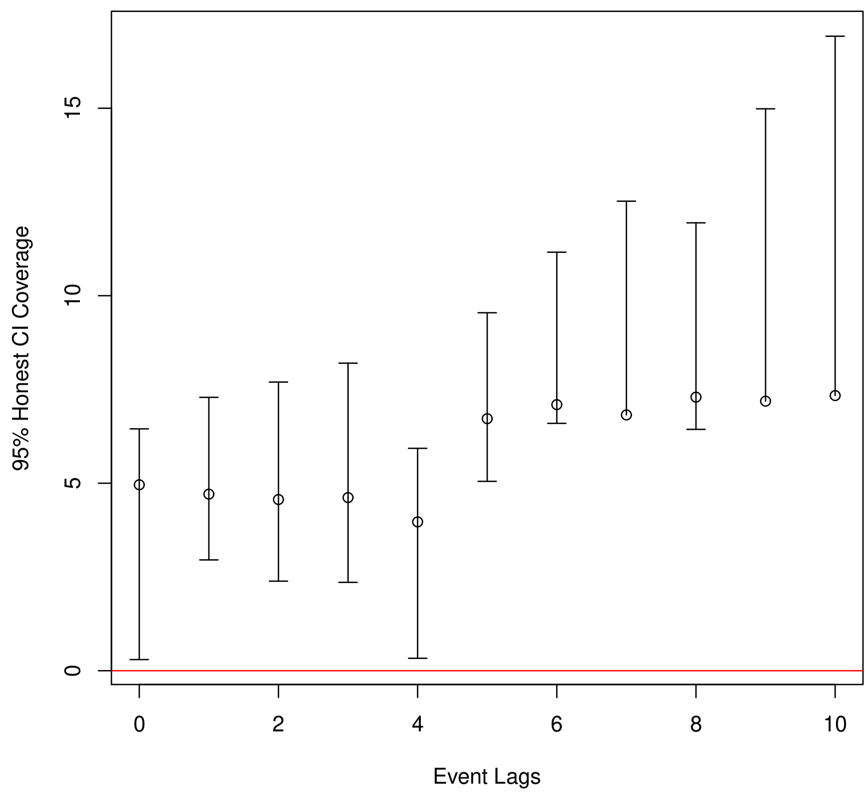
(b) ln(maternal mortality ratio)



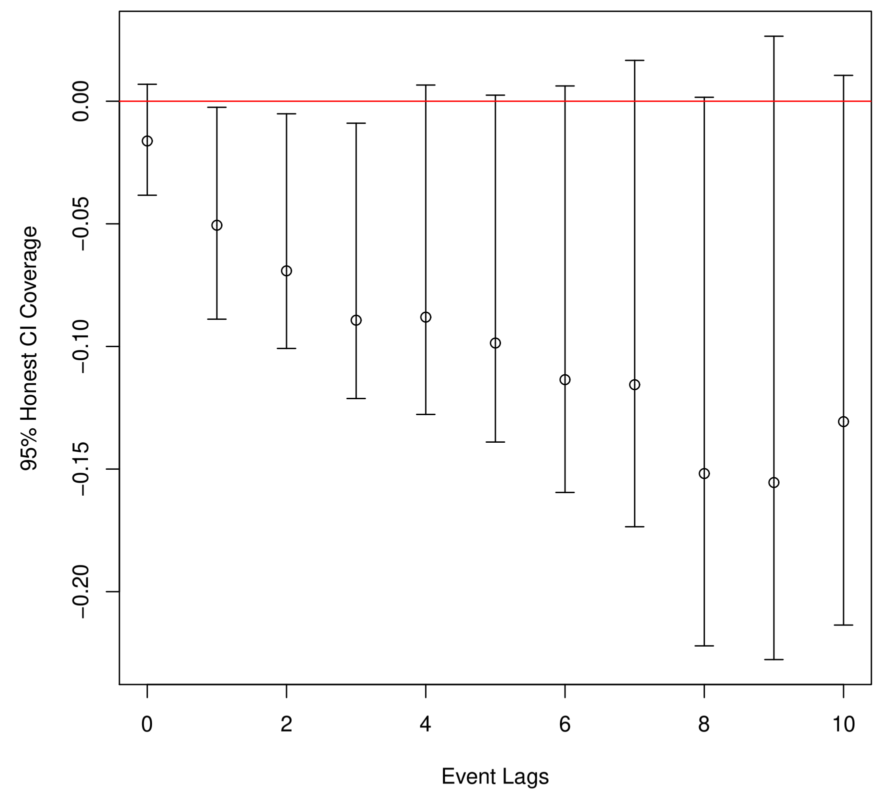
Notes: Refer to notes to Appendix B. Coefficients are estimated based on a pooled synthetic control approach where for each quota country a synthetic control is chosen based on leads of the variable of interest (up to period -3), over-weighting units which come from the same region as the country of 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 5: Post-quota coefficients based on “Honest DiD”: women in parliament and maternal mortality

(a) Percent of women in parliament



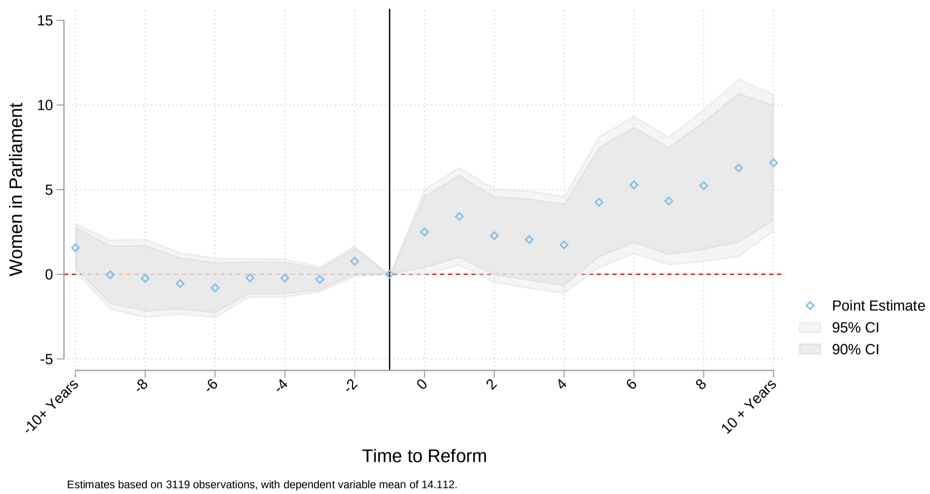
(b) ln(maternal mortality ratio)



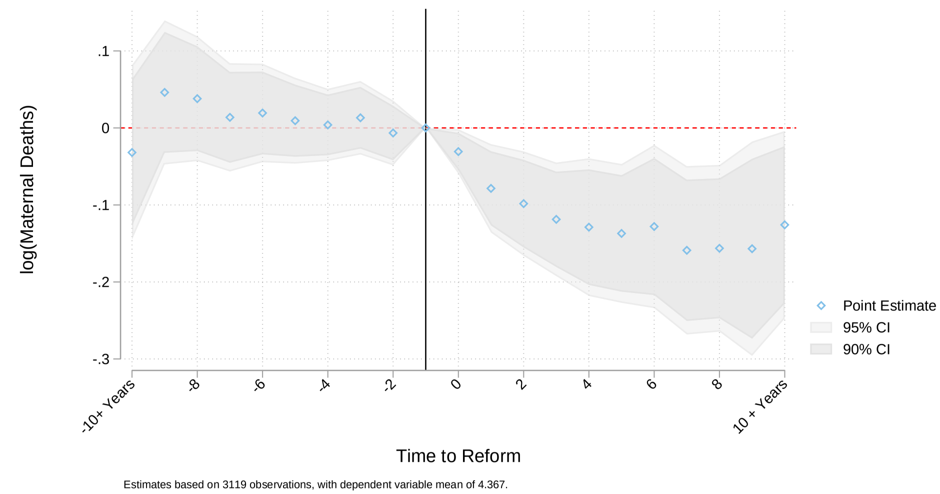
Notes: Each post-quota coefficient from event study specification 1 is documented, along with valid inference under Rambachan and Roth (2020)’s “Honest DiD” methods. These correspond to the specifications presented in Figure 2, panels (a) and (b). Here, in place of assuming parallel trends in quota and 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 6: 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)

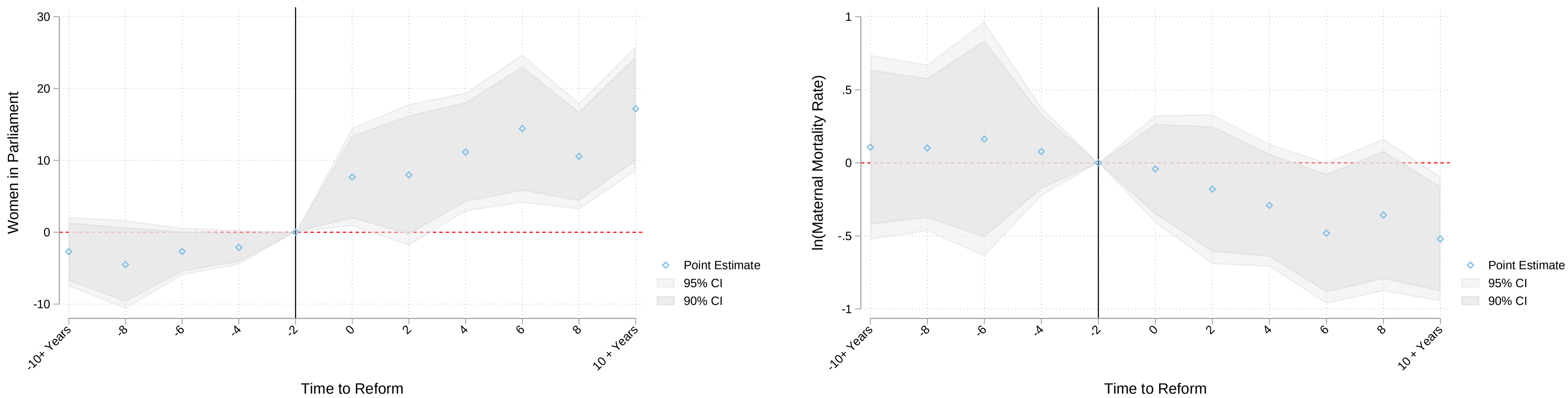


Notes: Event studies replicate those in Figure 2, however now controlling for 7 potential predictors of quota timing from the political science literature (Figure A8) and for 18 indicators of women's rights (Figures A4, A5). Point estimates of the lag and lead terms in the event study specification described 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 7: DHS microdata – gender quota impacts on women in parliament and MMR

(a) Percent of women in parliament

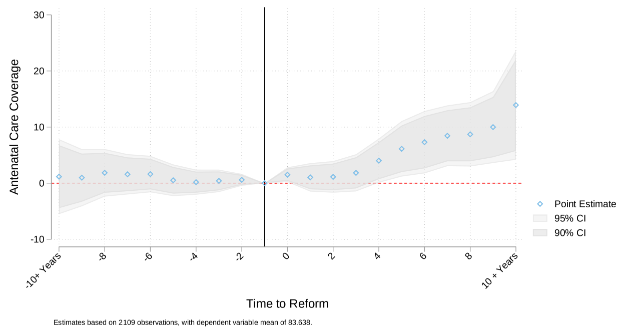
(b) ln(maternal mortality ratio)



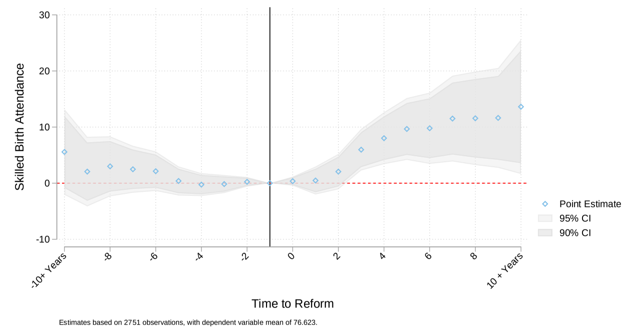
Notes: Specification replicates Figure 2, however now replacing world-wide estimates of MMR with maternal mortality calculated from microdata reports from the DHS. Controls for the log of GDP p.c. and democracy level are included as in Figure 2, panels (a) and (b). As the DHS maternal mortality module 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 8: Mechanisms: Impacts of gender quotas on intermediate outcomes

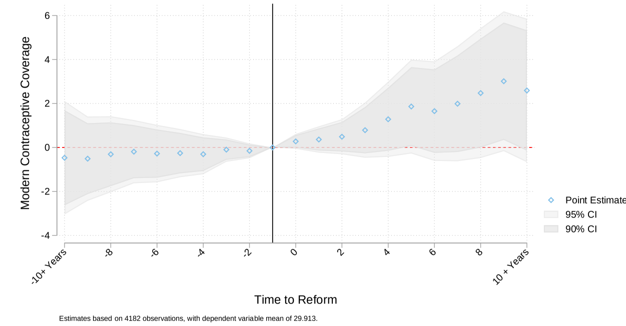
(a) Antenatal care



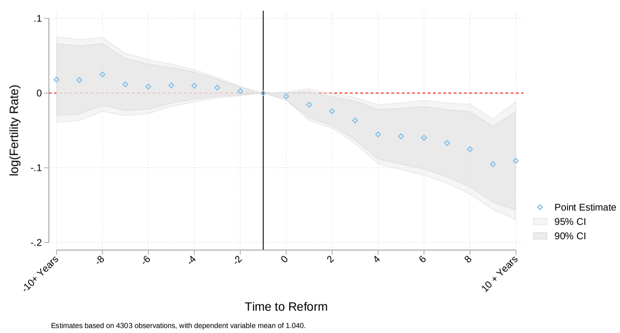
(b) Attended births



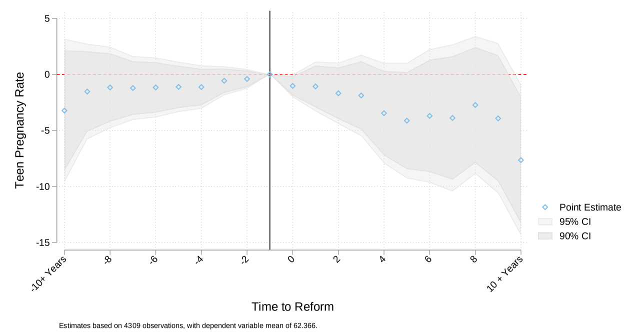
(c) Modern contraceptive cover



(d) ln(fertility)

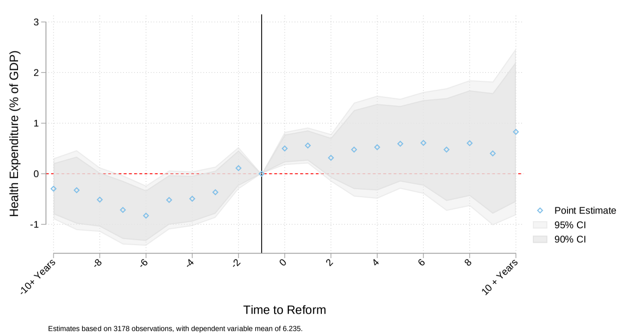


(e) Teen pregnancy

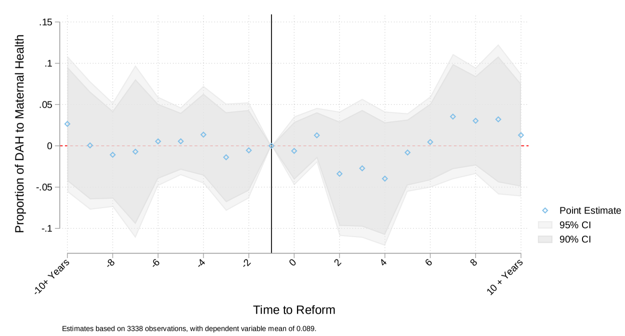


(f) Birth spacing

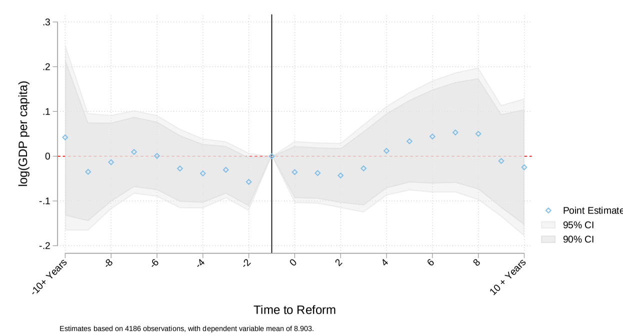
(g) Health expenditure as a % of GDP



(h) Development assistance for maternal health

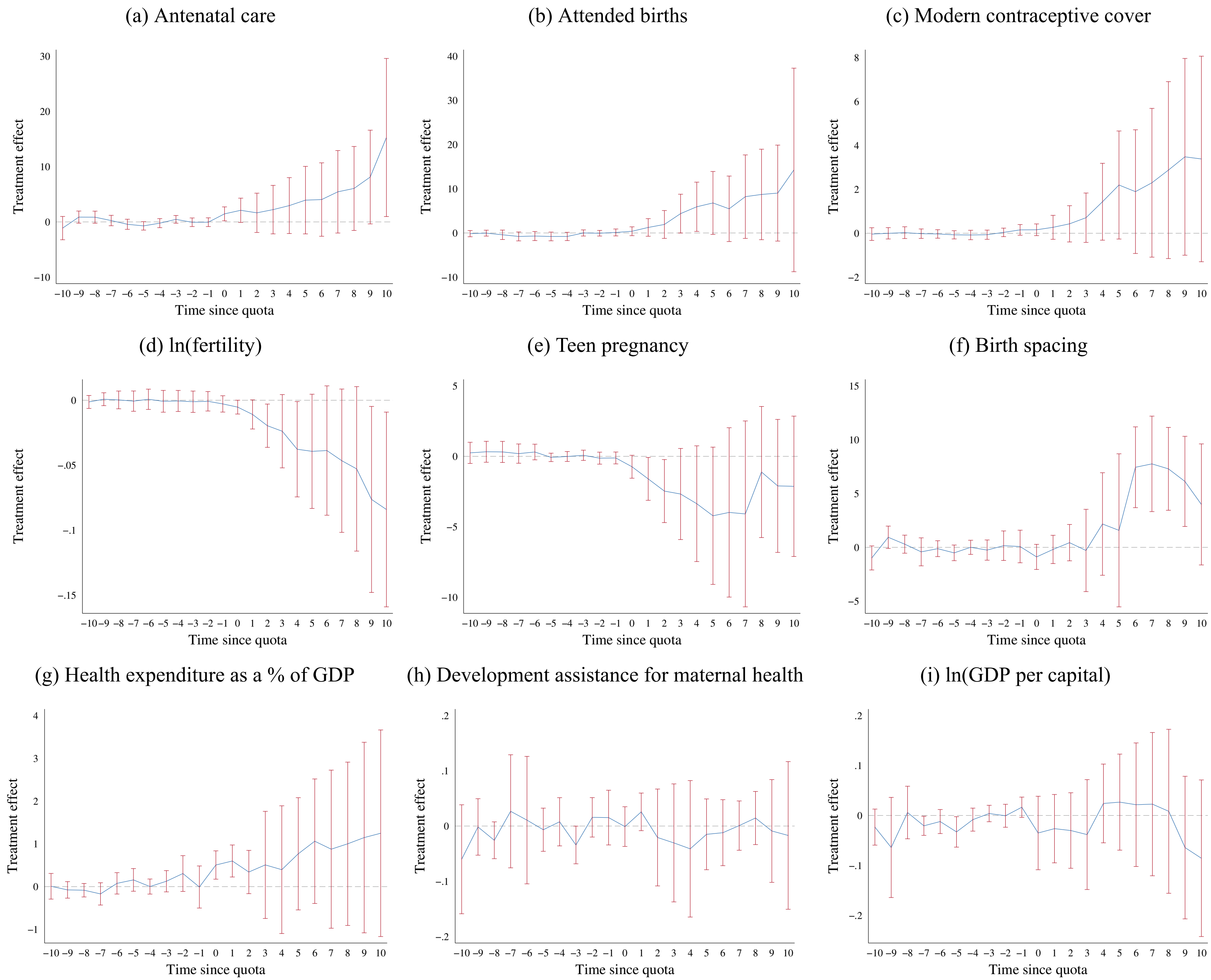


(i) ln(GDP per capital)



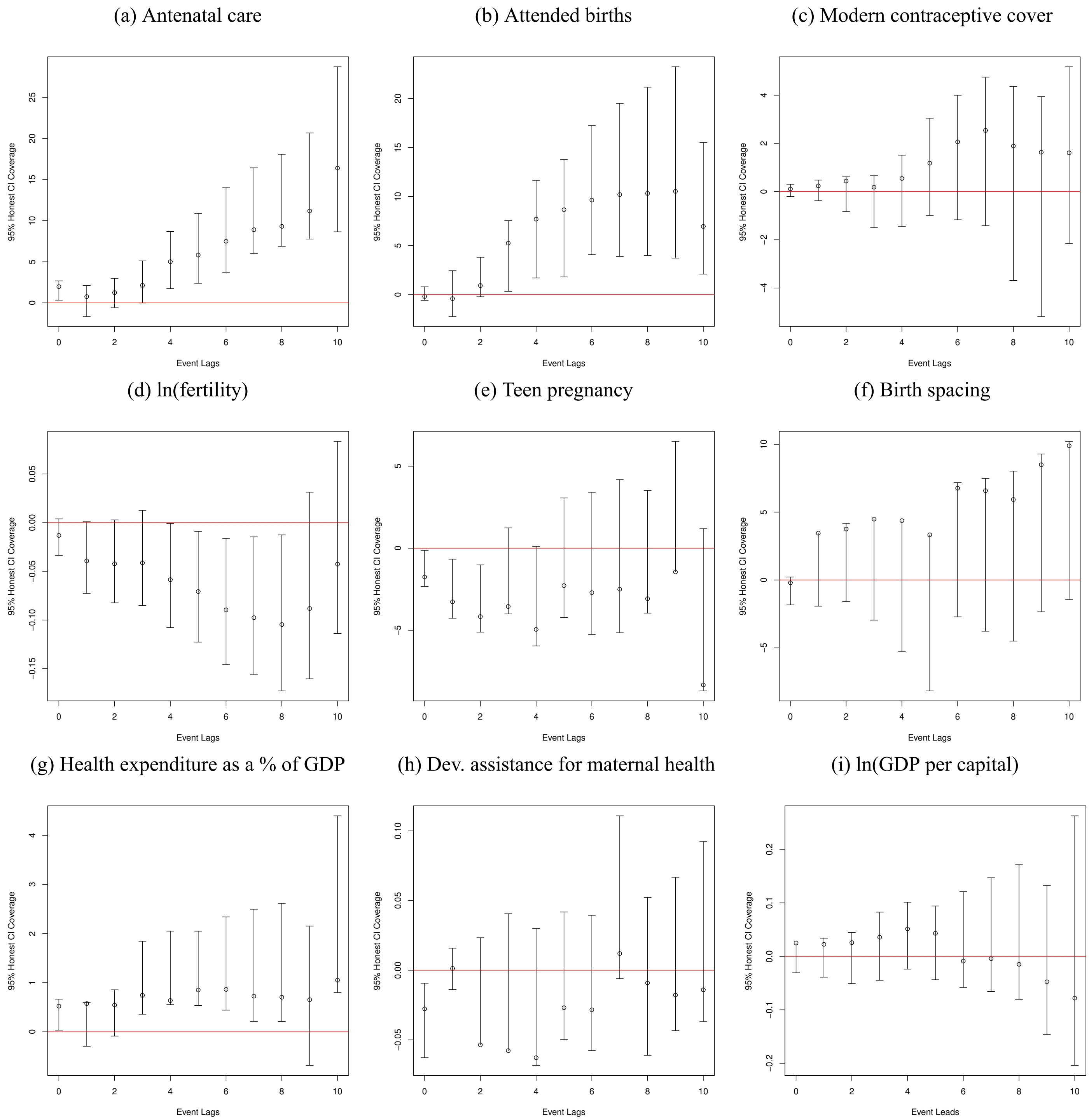
Notes: Event-study estimates of intermediate outcomes as a function of the passage of gender quotas, following specification 1. Antenatal coverage and birth attendance refer to the percentage of coverage, are accessed from the World Bank databank, and are only available for a sub-sample of years for each country (an unbalanced panel from 1990–2015). Estimates in Panels A and B use a linearly interpolated measure, event studies based on non-imputed values are available in Appendix Figure B22. 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 event studies in Figure 2.

Figure 9: Mechanisms: de Chaisemartin and D'Haultfœuille estimates for impacts of gender quotas on intermediate outcomes



Notes: Refer to Figure 8. Here models based on identical outcome variables are estimated, however using the de Chaisemartin and D'Haultfœuille (2020) DID_M estimator. Lead (placebo) and lag (dynamic) effects are estimated for each variable.

Figure 10: 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. In each case, these correspond to models conditioning on ln(GDP p.c.) and democracy fixed effects. 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.

Tables

Table 1: Two-way fixed effect estimates, de Chaisemartin and D'Haultfœuille estimates, and pooled event study

	Two-way FE (1)	DID_M (2)	Pooled Event (3)	Negative Weights (4)
Panel A: Baseline Model				
Women in Parliament	5.925***	5.678***	6.622***	-0.0053
	(2.056)	(1.733)	(1.849)	
log(Maternal Mortality)	-0.082	-0.072*	-0.079**	-0.0053
	(0.051)	(0.041)	(0.040)	
Panel B: Additional Controls				
Women in Parliament	5.144**	5.133**	5.934***	-0.0096
	(2.301)	(2.176)	(2.197)	
log(Maternal Mortality)	-0.095*	-0.103**	-0.098**	-0.0096
	(0.049)	(0.043)	(0.042)	

Notes: Each cell in column (1)–(3) presents an aggregate (single coefficient) estimate of the impact of quota reform on women in parliament or maternal mortality under alternative estimation strategies. Column (1) presents standard two-way fixed effect models where the indicated outcome is regressed on the existence of quotas and country and year fixed effects. Standard errors are clustered by country. Column (2) presents aggregate estimates pooling all "dynamic" (post-quota) estimates based on de Chaisemartin and D'Haultfœuille's "DID_M" estimator where standard errors are estimated using a block bootstrap by countries. Finally, column (3) estimates impacts pooling all post-event study coefficients (from lag 0 to lag 10+). Once again, standard errors are estimated using a block bootstrap over countries. Finally, column (4) presents the magnitude of the negative weights attached to the two-way FE estimate in column (1) as described by de Chaisemartin and D'Haultfœuille (2020). In panel (a) each model is estimated without any time varying controls, while in panel (b) ln(GDP) and democracy fixed effects are included.

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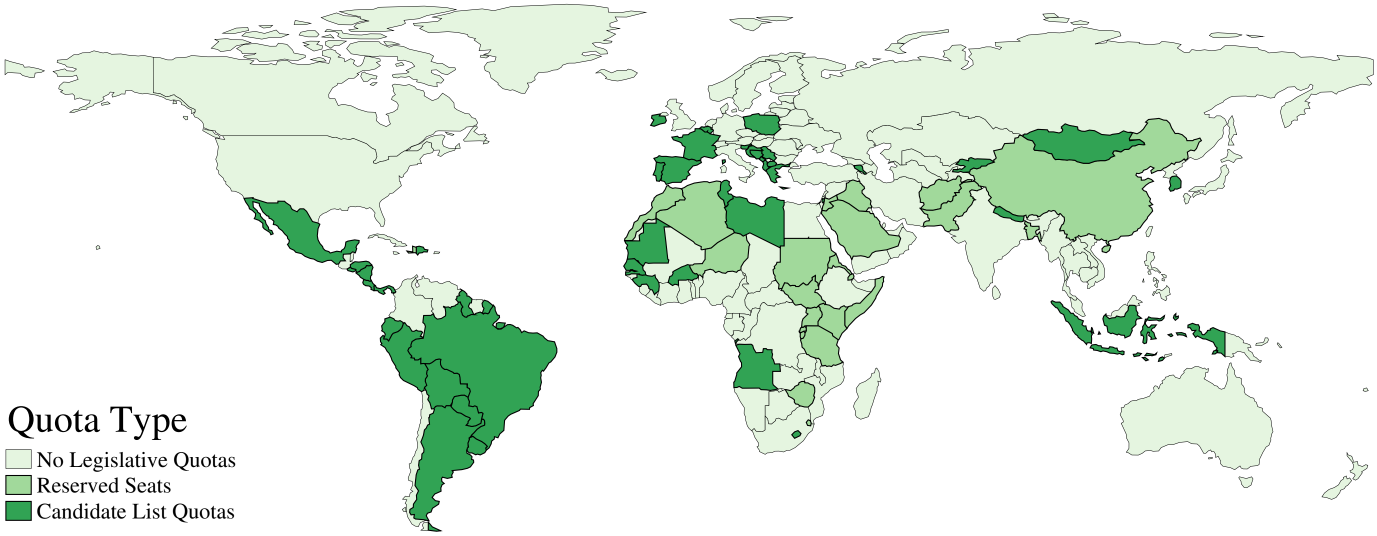
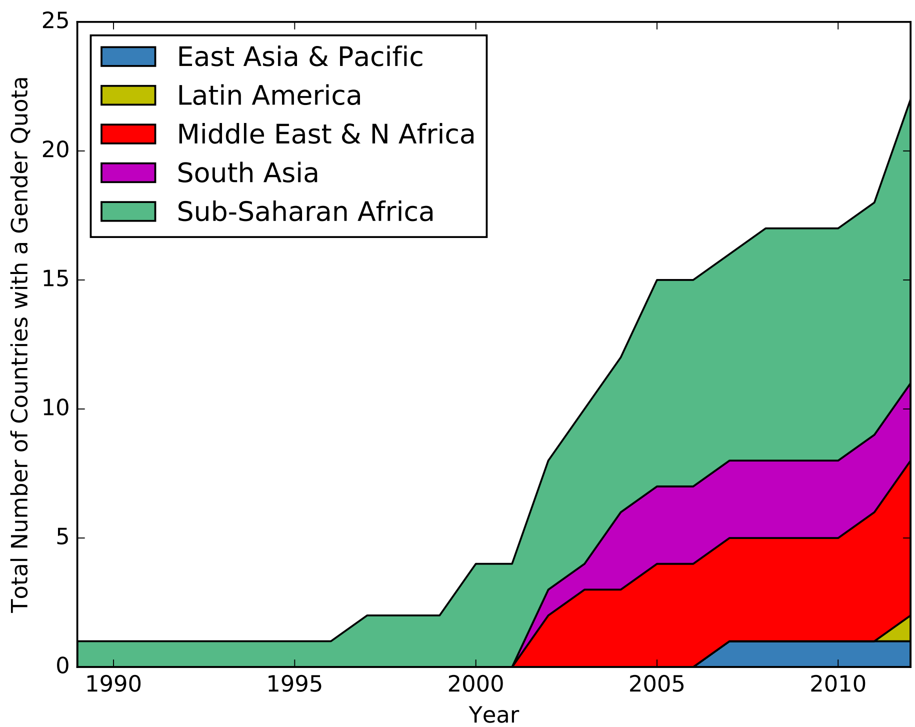
Appendix Figures

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.

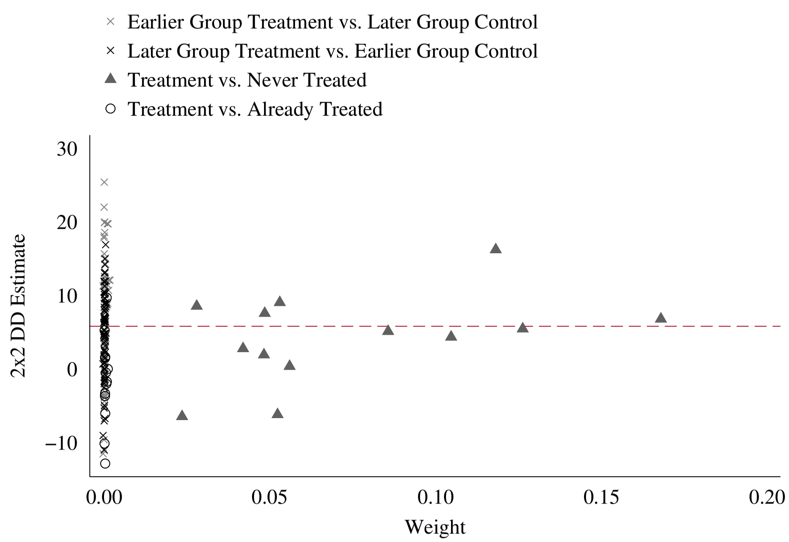
Figure A2: Reserved seat quota timing: 1990–2012



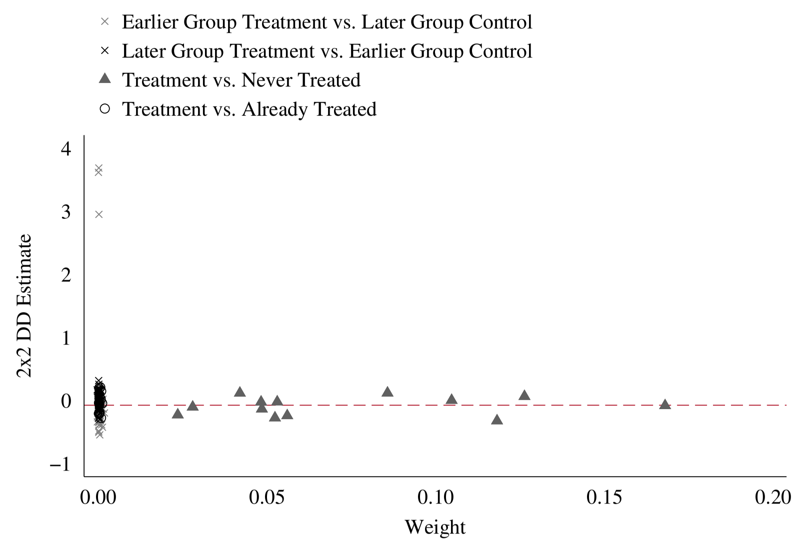
Notes: Timing of the implementation of reserved seats is documented by geographic region. Additional notes related to quota adoption are provided in Figure A1.

Figure A3: Goodman-Bacon (2021) decomposition based on 2×2 difference-in-difference models

(a) Percent of women in parliament

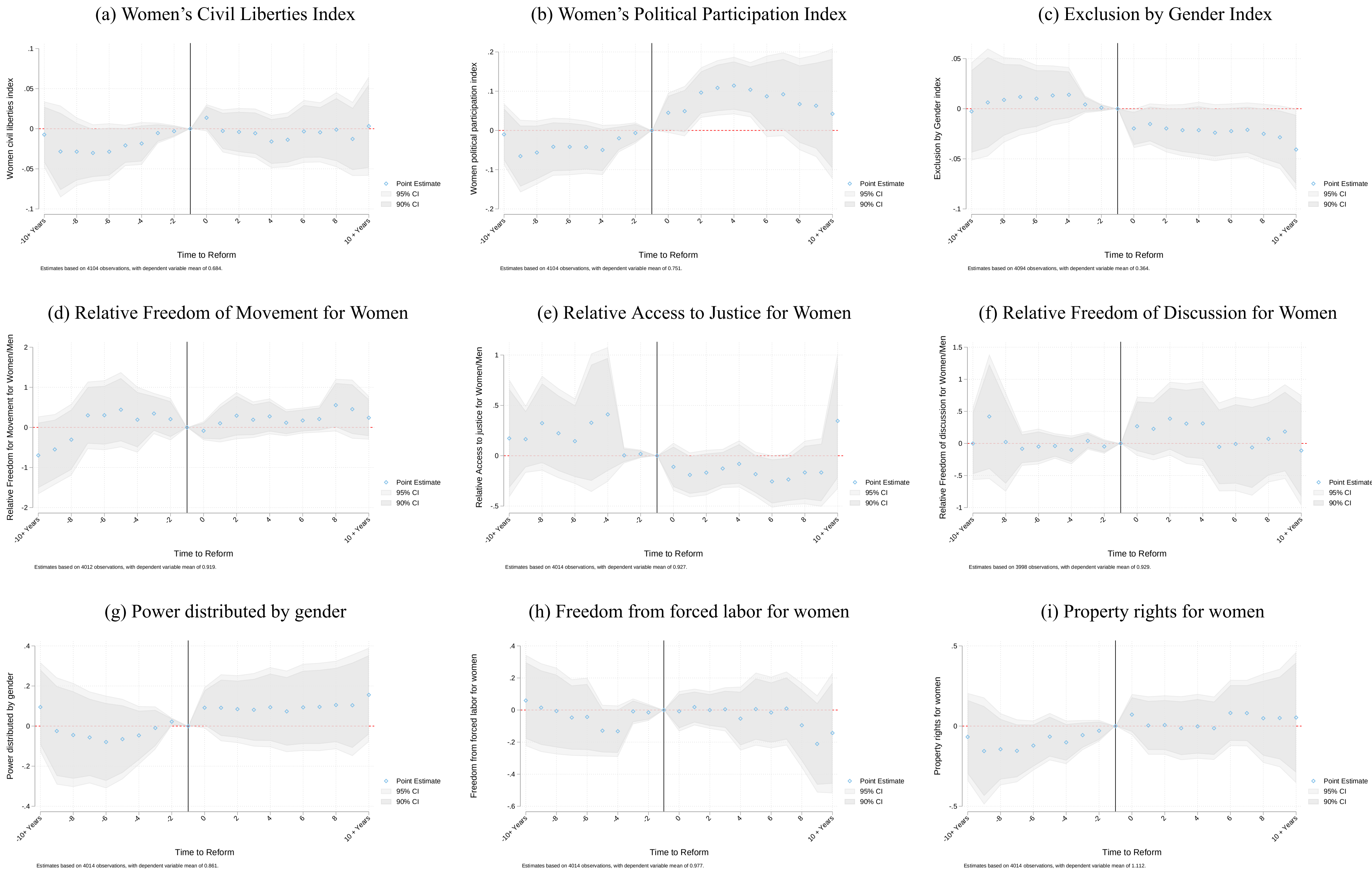


(b) ln(maternal mortality ratio)



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 $\times$ symbols represent cases where identification is drawn from timing-only comparisons. Darker shaded $\times$ symbols represent comparisons between earlier-treated units (as treatment) and later-treated units (as controls). Lighter shaded $\times$ 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.

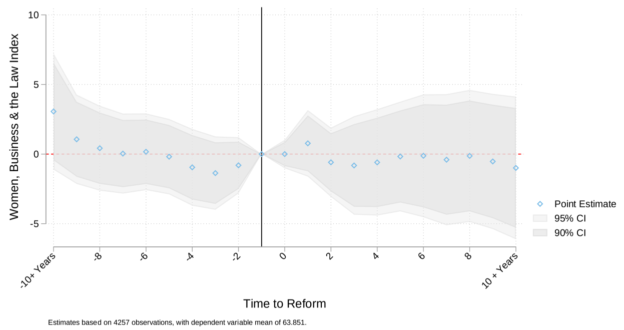
Figure A4: Women's rights and social standing from "Varieties of Democracy" data



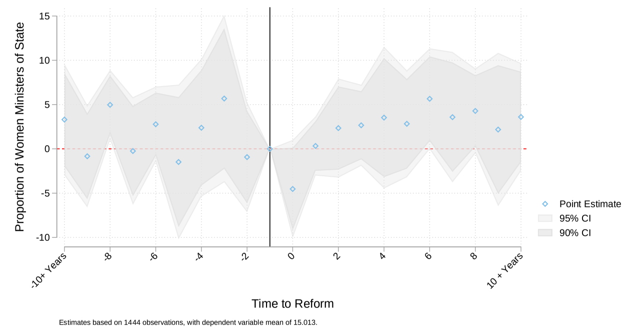
Notes: Event-study estimates examine the variation of measures of women's rights and measures of relative social standing based on data collected in the Variety of Democracy dataset (Coppedge et al., 2020). When generating ratios of women to male outcomes (in panels (d)-(f)) these are Winsorized at the 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. Each specification follows equation 1.

Figure A5: Women's rights, empowerment, and women in politics

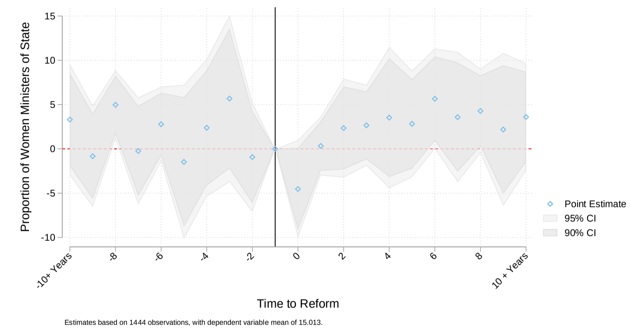
(a) Women, Business & Law Index (WB)



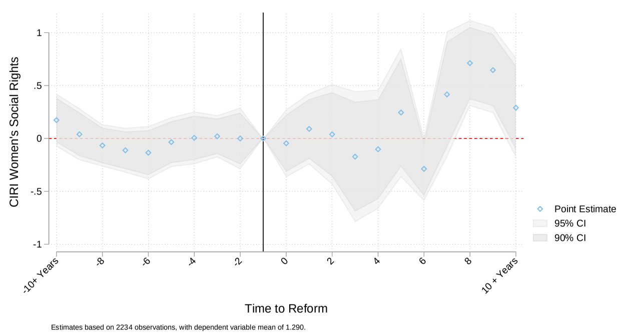
(b) Women Ministers



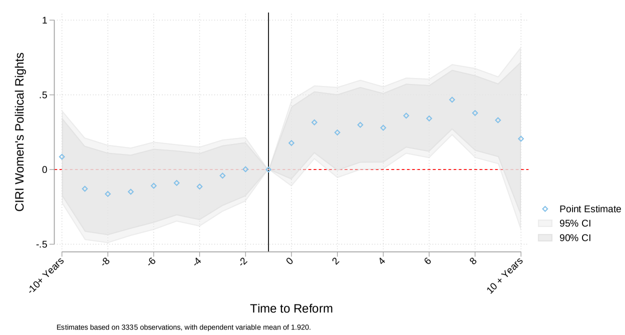
(c) Women's Protests



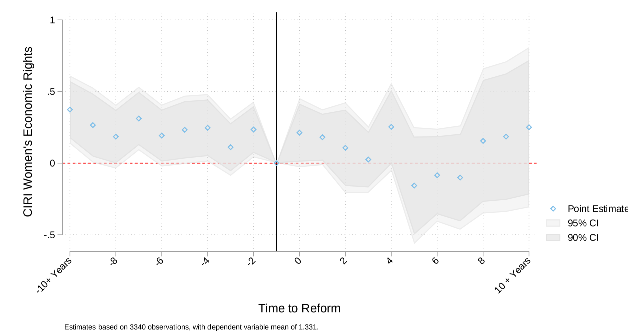
(d) CIRI Women's Social Rights



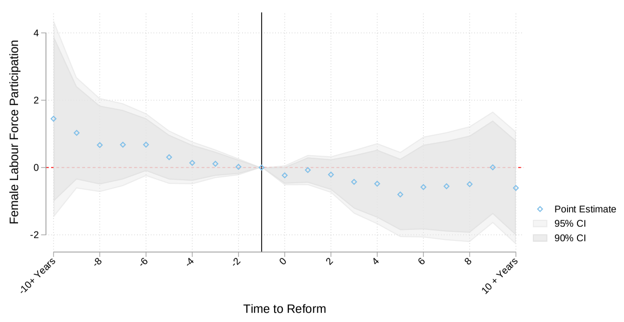
(e) CIRI Women's Political Rights



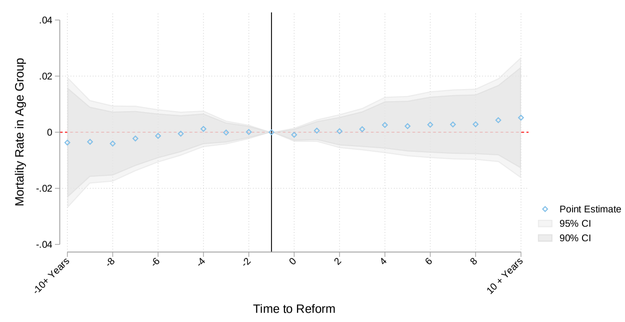
(f) CIRI Women's Economic Rights



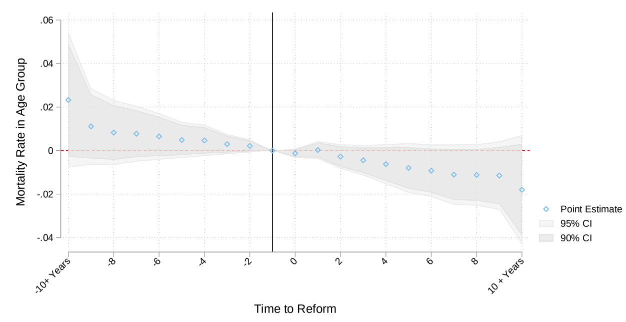
(g) Female Labor Force Participation



(h) Abortion (Save Mother's Life)



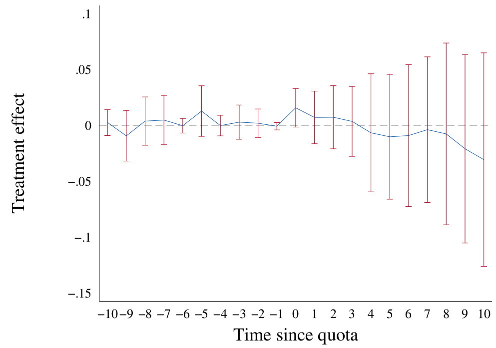
(i) Abortion (Fetal Impairment)



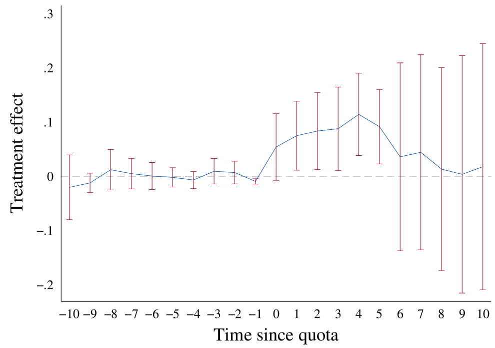
Notes: Event-study estimates follow specification 1, with outcomes describing women's rights, empowerment or measures of participation in politics. Panel (a) is a recently generated index provided by the World Bank capturing Women's participation in Business and Lawmaking. Women Minister data is drawn 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 A6: de Chaisemartin and D'Haultfœuille estimates: women's rights and social standing from "Varieties of Democracy" data

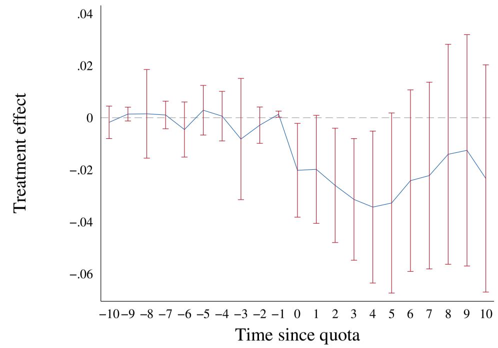
(a) Women's Civil Liberties Index



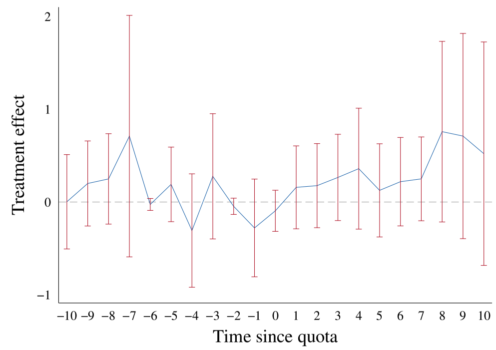
(b) Women's Political Participation Index



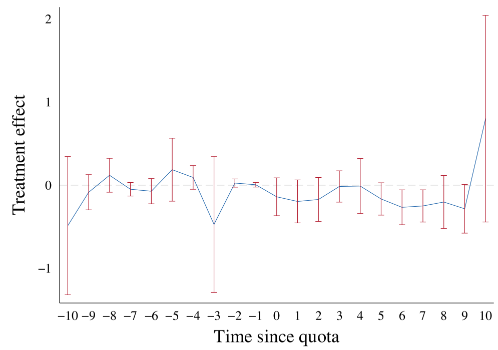
(c) Exclusion by Gender Index



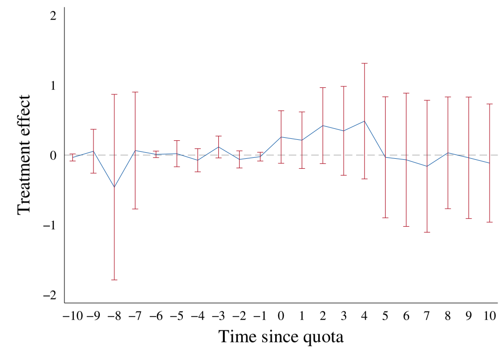
(d) Relative Freedom of Movement for Women



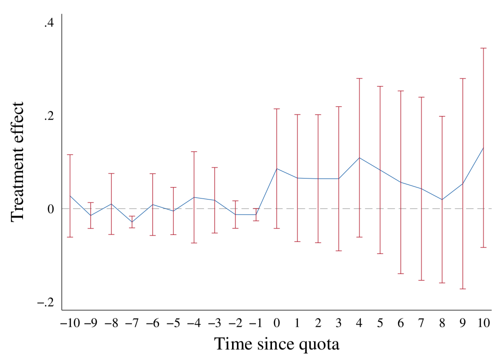
(e) Relative Access to Justice for Women



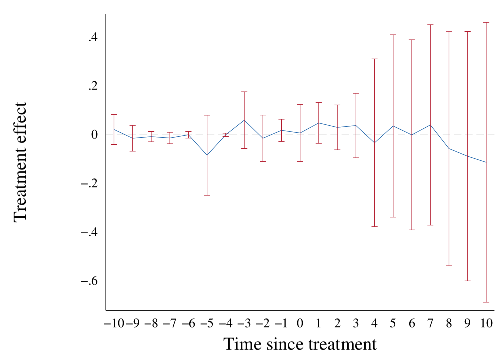
(f) Relative Freedom of Discussion for Women



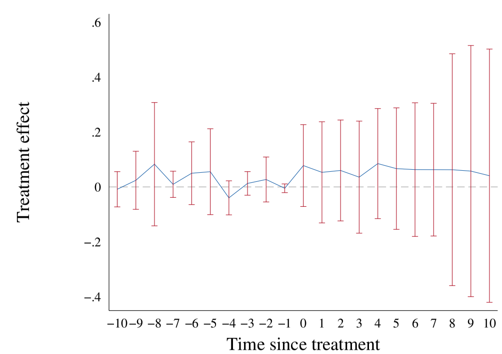
(g) Power distributed by gender



(h) Freedom from forced labor for women

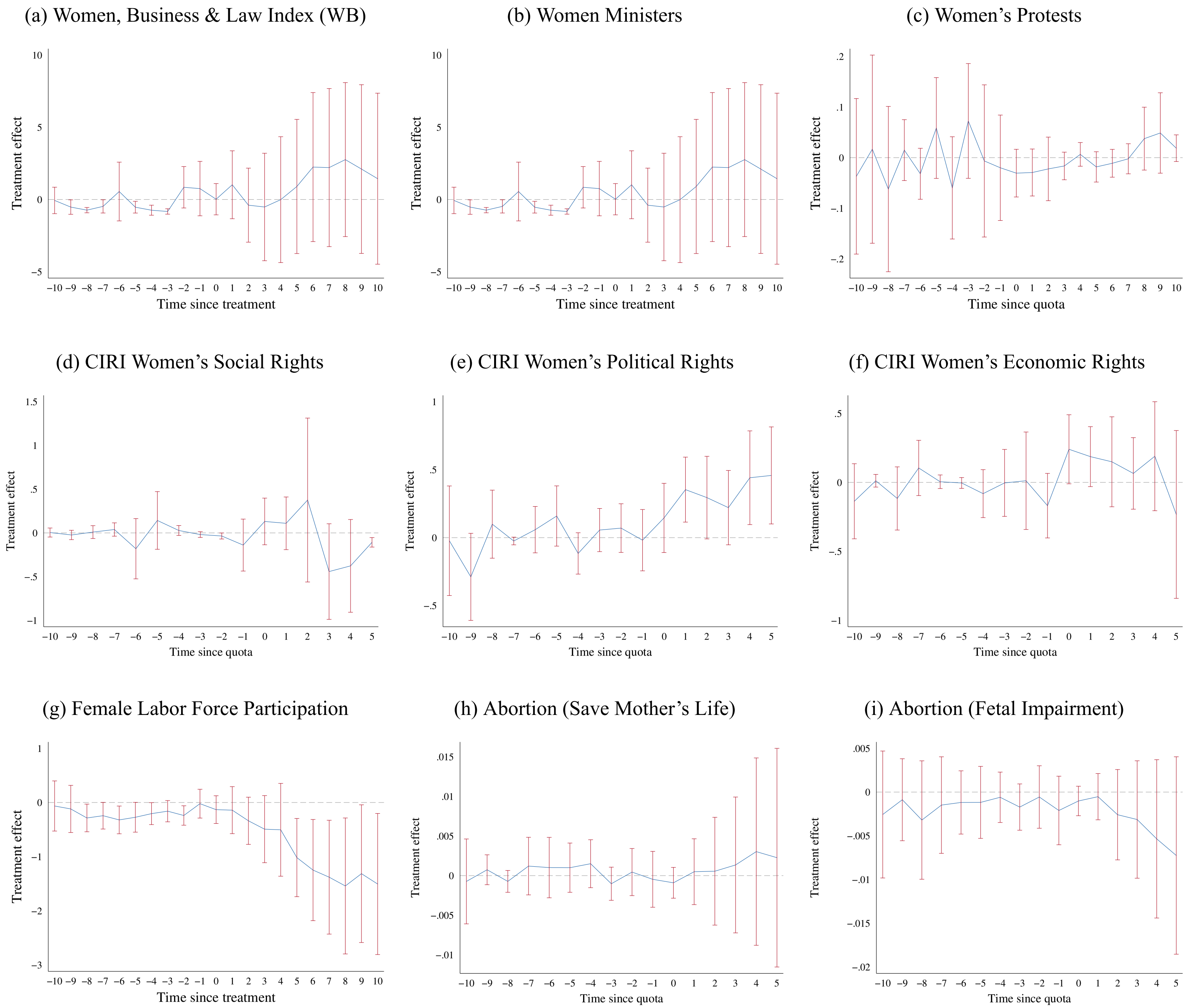


(i) Property rights for women



Notes: Refer to Notes to Figure A4. Here models based on identical outcome variables are estimated, however using the de Chaisemartin and D'Haultfœuille (2020) DID_M estimator. Lead (placebo) and lag (dynamic) effects are estimated for each variable.

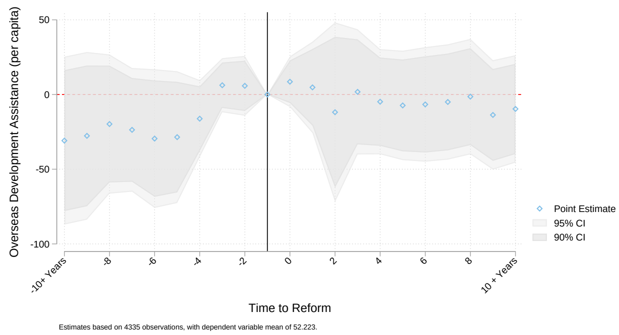
Figure A7: de Chaisemartin and D'Haultfœuille estimates: women's rights, empowerment, and women in politics



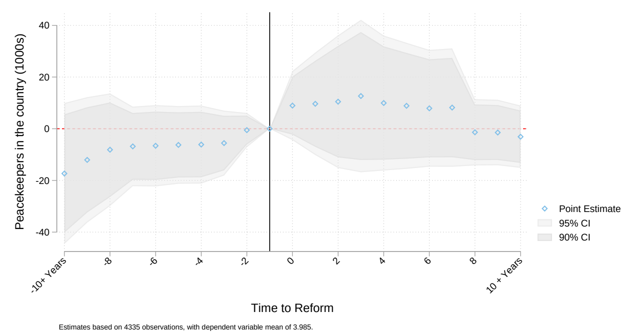
Notes: Refer to Figure A5. Here models based on identical outcome variables are estimated, however using the de Chaisemartin and D'Haultfœuille (2020) DID_M estimator. Lead (placebo) and lag (dynamic) effects are estimated for each variable.

Figure A8: Event studies: reserved seat quotas and predictors from political science literature

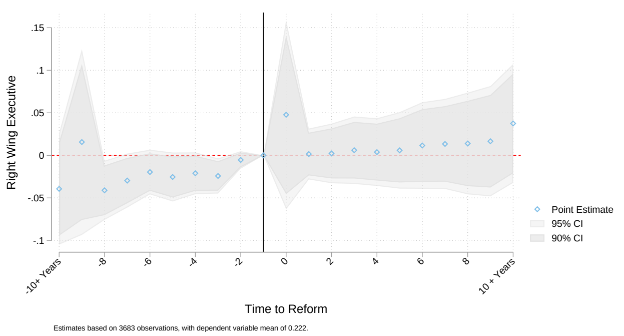
(a) Overseas Development Assistance p.c.



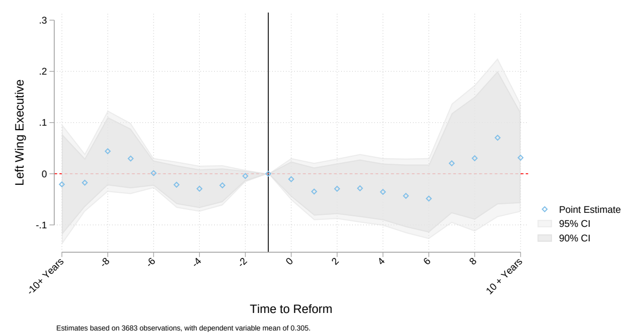
(b) Peace Keeper Presence



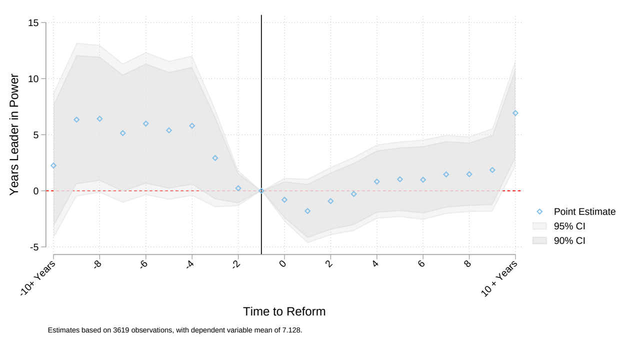
(c) Right Wing Executive



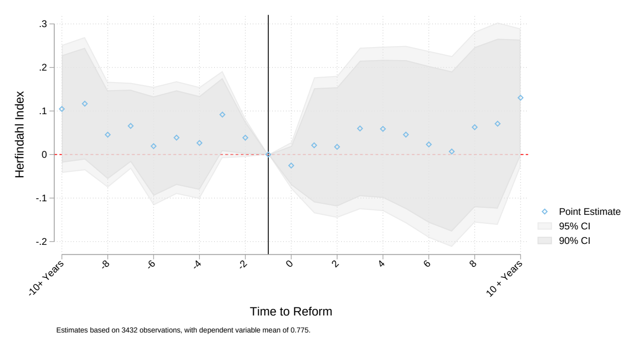
(d) Left Wing Executive



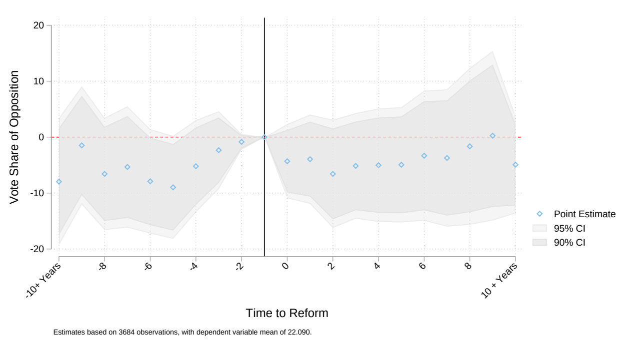
(e) Years in Power



(f) Herfindahl Index



(g) Opposition's Vote Share

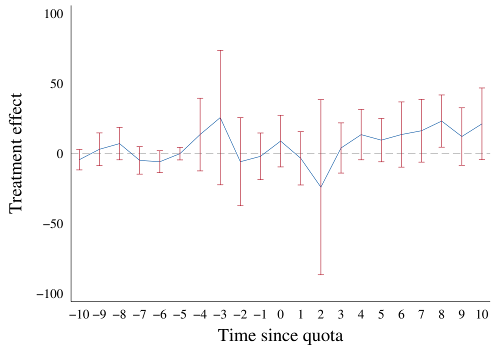


(h) Regime in Transition

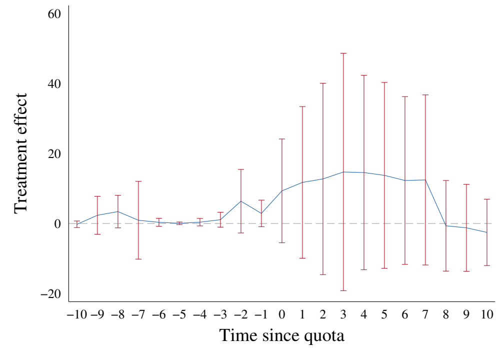
Notes: Event-study estimates of potential quota predictors suggested in the political science literature are displayed. All estimation details follow those described in notes to Figure 2. Overseas Development Assistance (ODA) is measured as per capita net inflows in current US dollars, and is generated 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 A9: de Chaisemartin and D'Haultfœuille's DID_M estimator and predictors from political science literature

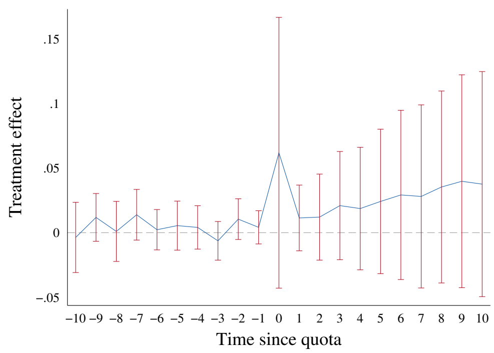
(a) Overseas Development Assistance p.c.



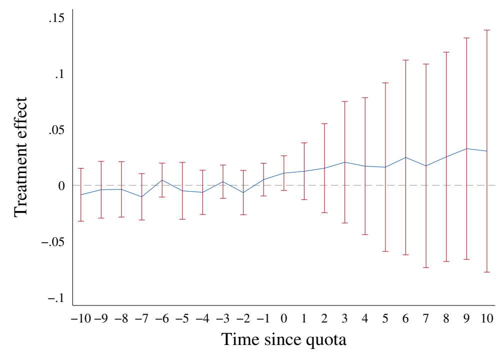
(b) Peace Keeper Presence



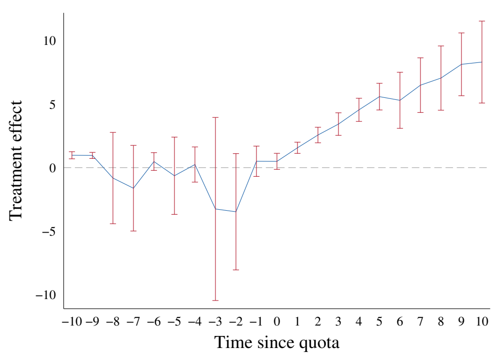
(c) Right Wing Executive



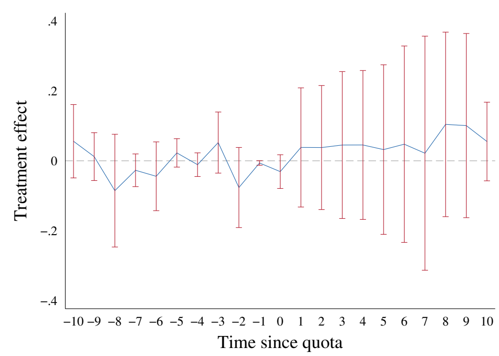
(d) Left Wing Executive



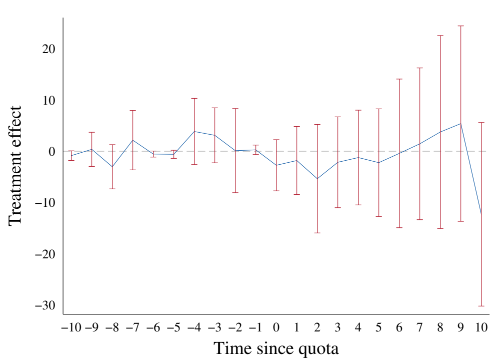
(e) Years in Power



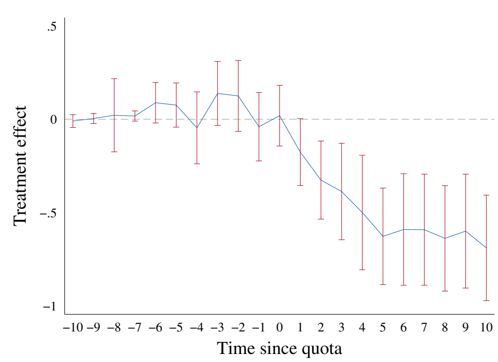
(f) Herfindahl Index



(g) Opposition's Vote Share



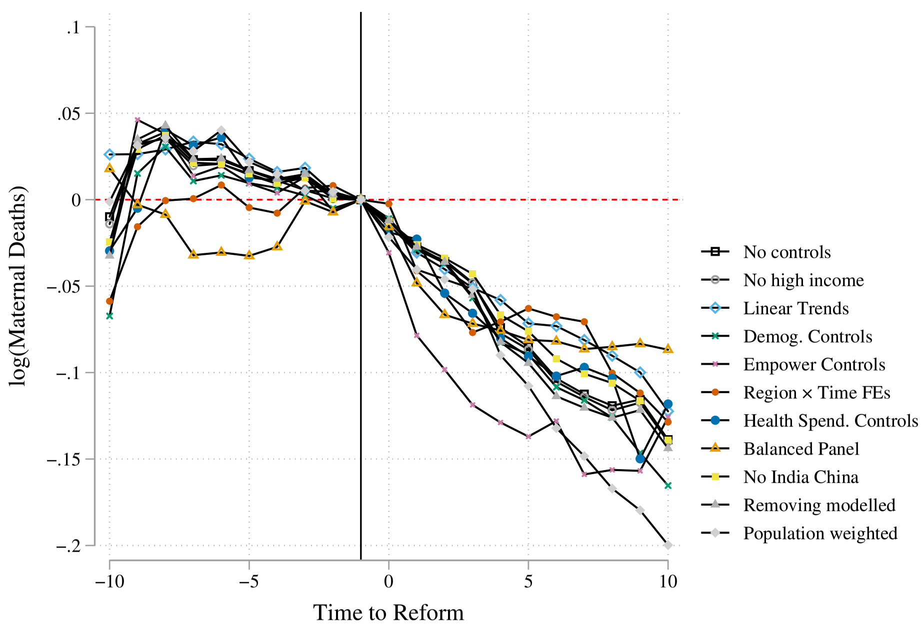
(h) Regime in Transition



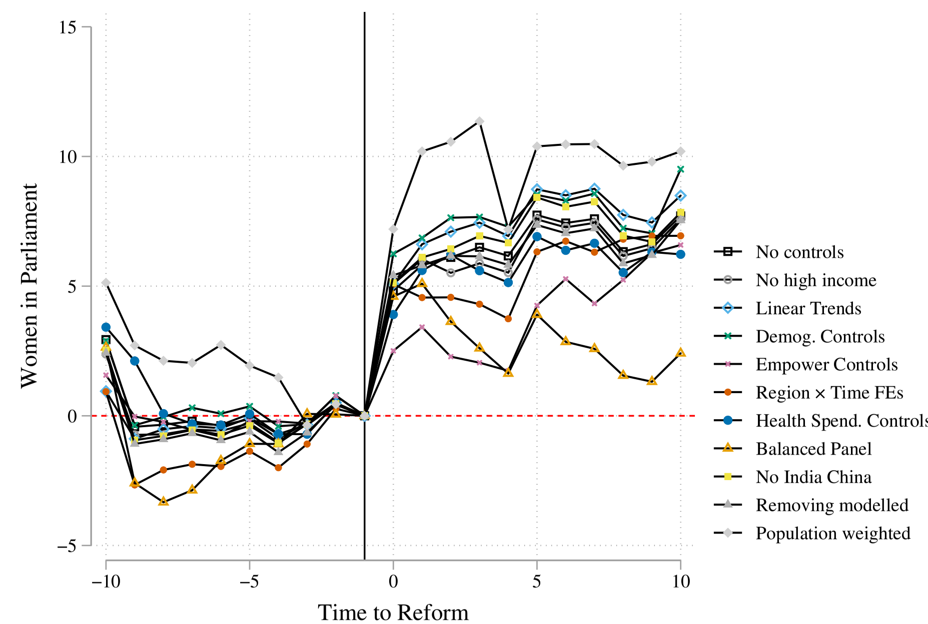
Notes: Figure replicates models of variation in quota predictors as flagged in the political science literature presented in Figure A8, however now estimating following de Chaisemartin and D'Haultfœuille (2020). Refer to notes to Figure A8 for variable definitions.

Figure A10: Robustness of event-study results to alternative measures and specifications

(a) ln(MMR)

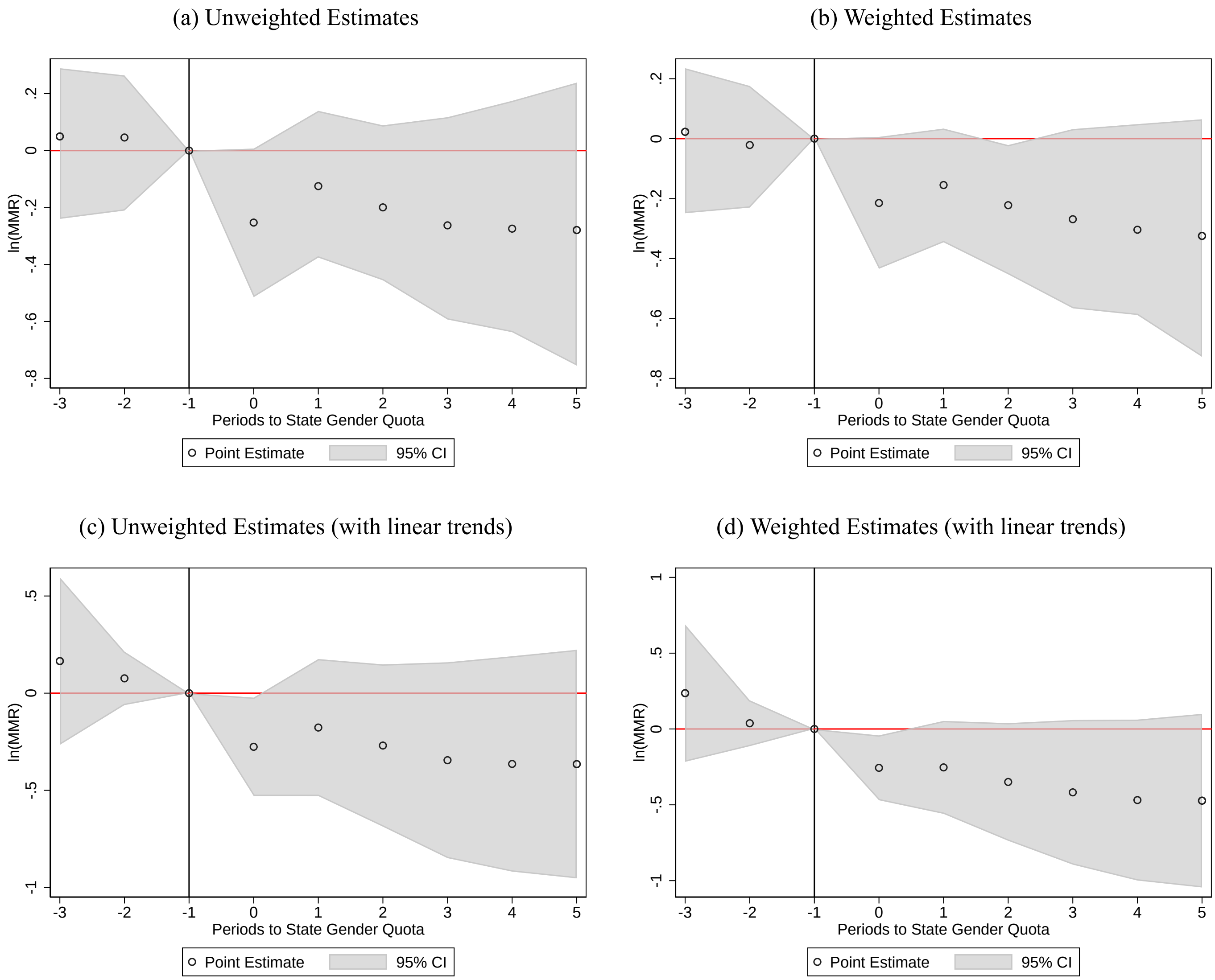


(b) % Women in Parliament



Notes: Alternative plots graph event study leads and lags surrounding the passage of quota reforms. Coefficients indicated by a hollow black square correspond to baseline event study models 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. Population weighted estimates are presented without India and China given their order of magnitude larger populations. Full alternative event study specifications discussed are documented in Appendix Figures B11-B13.

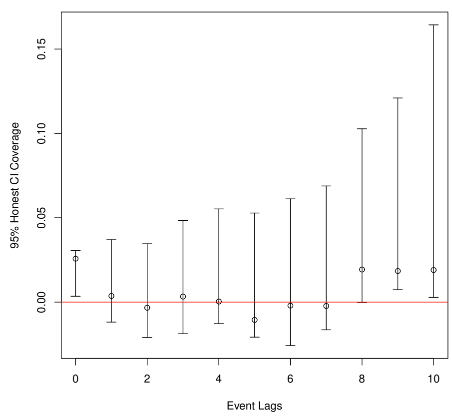
Figure A11: Event study analysis of reserved seats for women in large Indian states



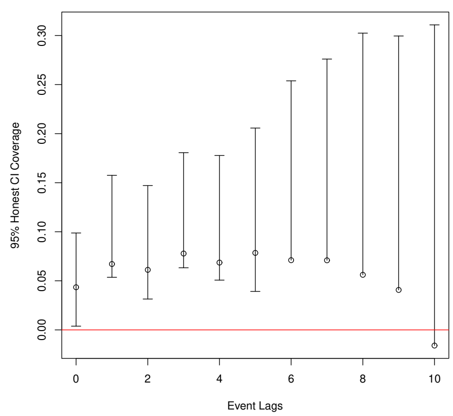
Notes: Event studies are conducted using all available state-level estimates of maternal mortality in India, which are generated provided in the Office of the Registrar General & Census Commissioner's Sample Registration System (SRS) Bulletins. State level reforms refer to the reservation of seats for 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 A12: Rambachan and Roth estimates: women's rights and social standing from "Varieties of Democracy" data

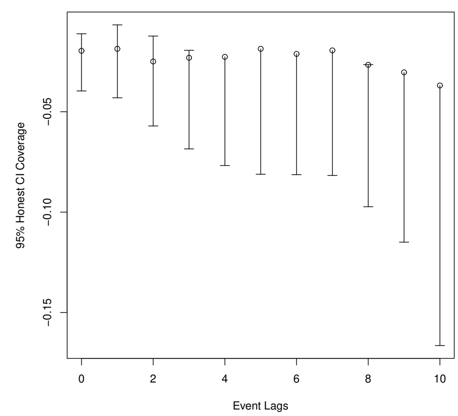
(a) Women's Civil Liberties Index



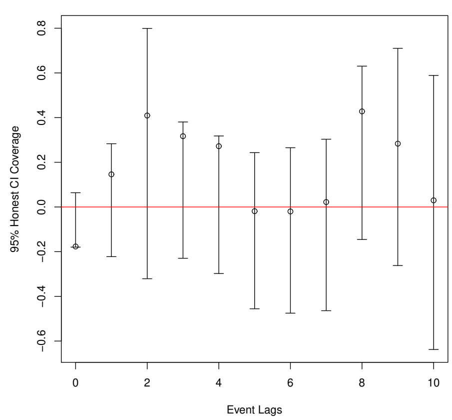
(b) Women's Political Participation Index



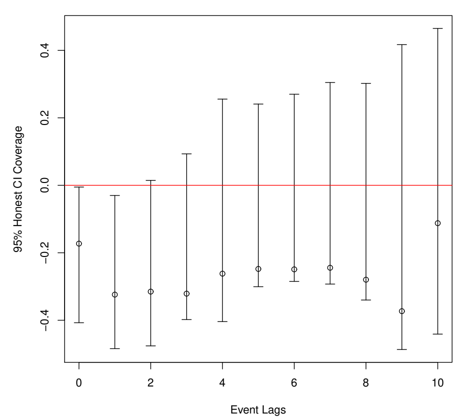
(c) Exclusion by Gender Index



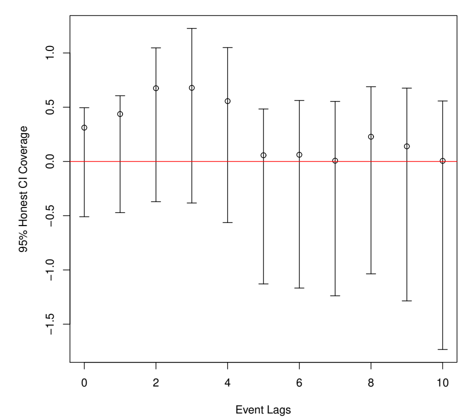
(d) Relative Freedom of Movement for Women



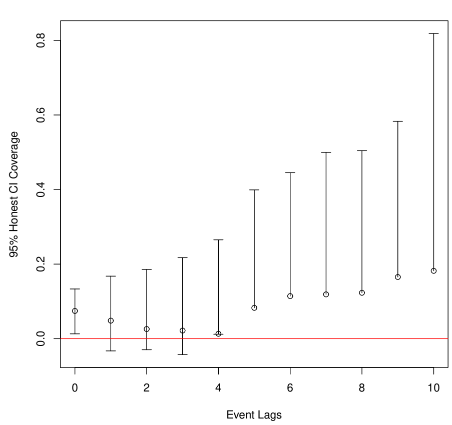
(e) Relative Access to Justice for Women



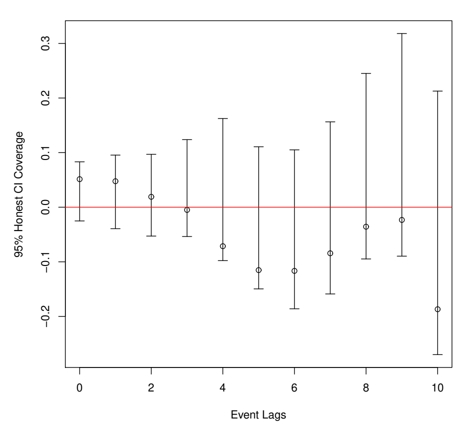
(f) Relative Freedom of Discussion for Women



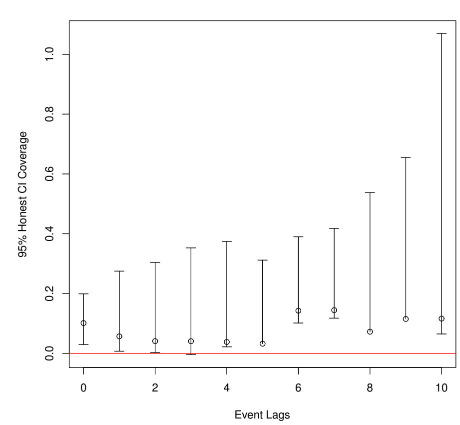
(g) Power distributed by gender



(h) Freedom from forced labor for women



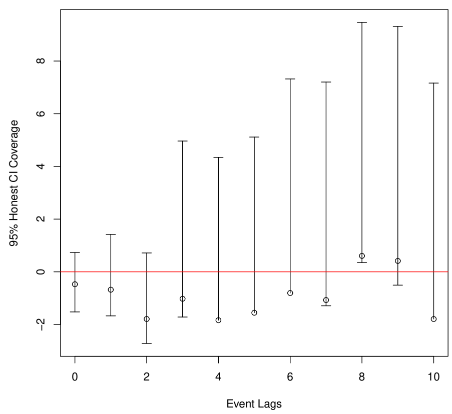
(i) Property rights for women



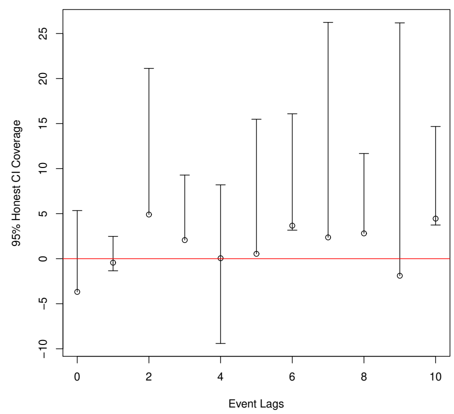
Notes: Refer to Notes to Figure A4. Here models based on identical outcome variables are estimated, however in this case, each post-quota coefficient from event study 1 is documented, along with valid inference under Rambachan and Roth (2020)'s "Honest DiD" methods. Additional notes are available in Figure 5.

Figure A13: Rambachan and Roth estimates: women's rights, empowerment, and women in politics

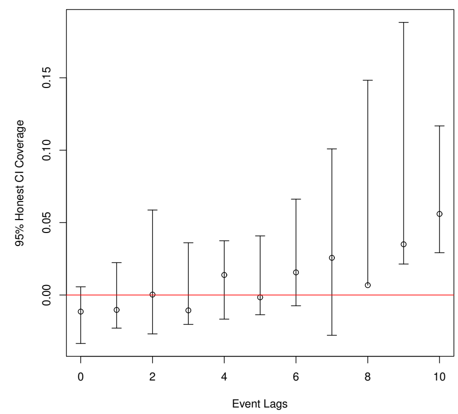
(a) Women, Business & Law Index (WB)



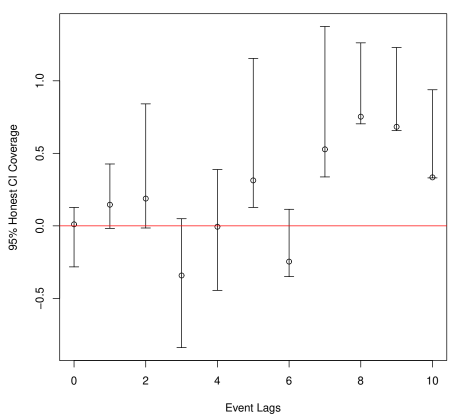
(b) Women Ministers



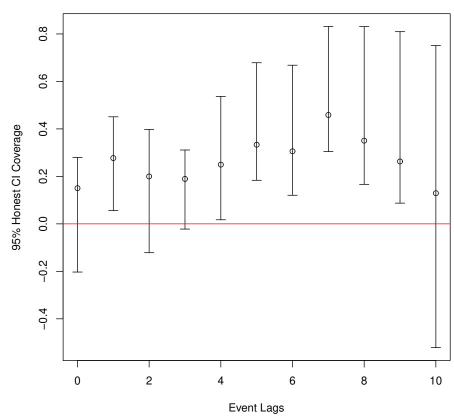
(c) Women's Protests



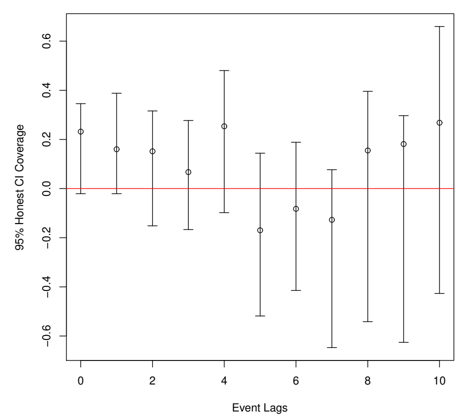
(d) CIRI Women's Social Rights



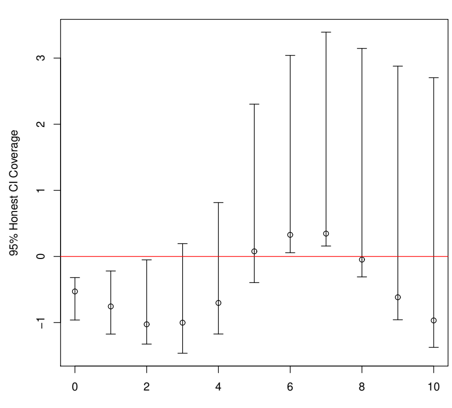
(e) CIRI Women's Political Rights



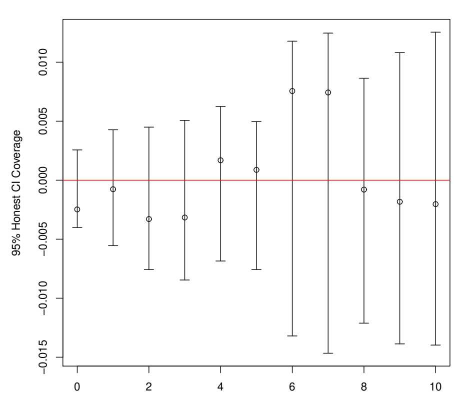
(f) CIRI Women's Economic Rights



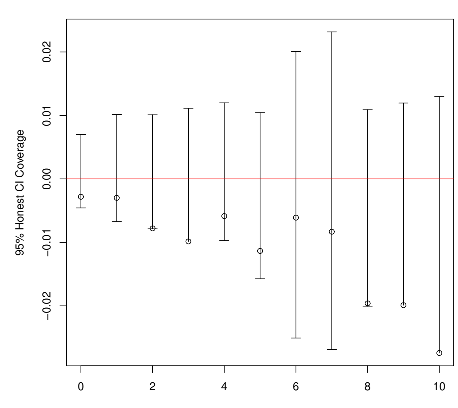
(g) Female Labor Force Participation



(h) Abortion (Save Mother's Life)



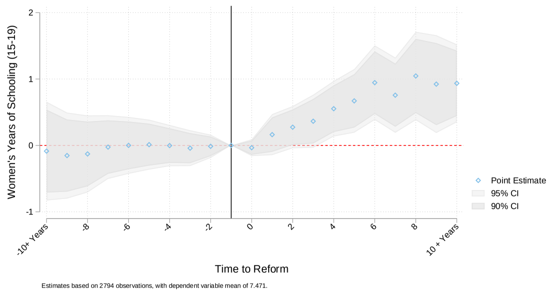
(i) Abortion (Fetal Impairment)



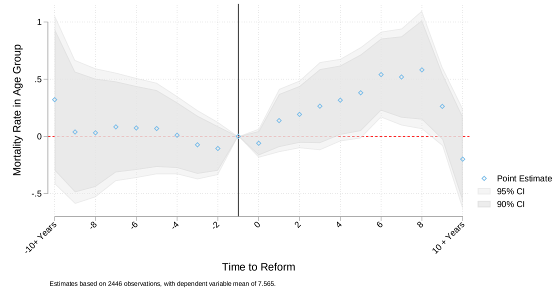
Notes: Refer to Figure A5. Here models based on identical outcome variables are estimated, however in this case, each post-quota coefficient from event study 1 is documented, along with valid inference under Rambachan and Roth (2020)'s "Honest DiD" methods. Additional notes are available in Figure 5.

Figure A14: Gender quotas and schooling (15–19 year-olds)

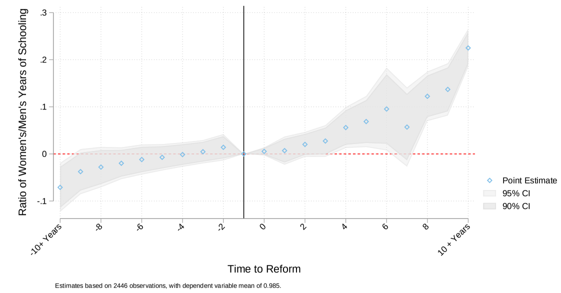
(a) Women (Event Study)



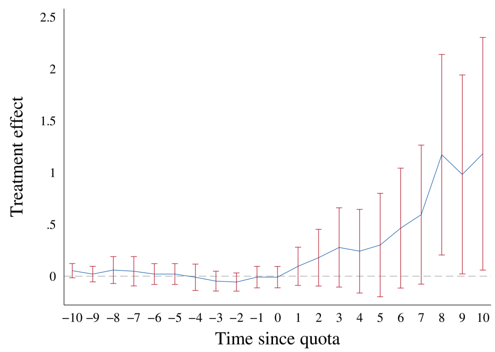
(b) Men (Event Study)



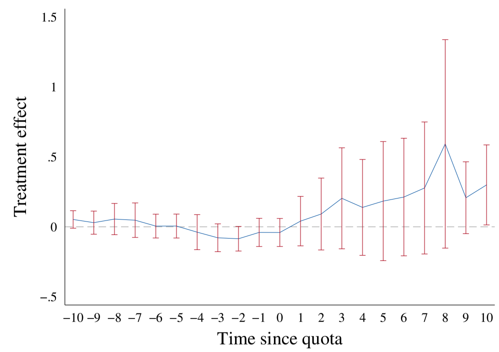
(c) Women/Men (Event Study)



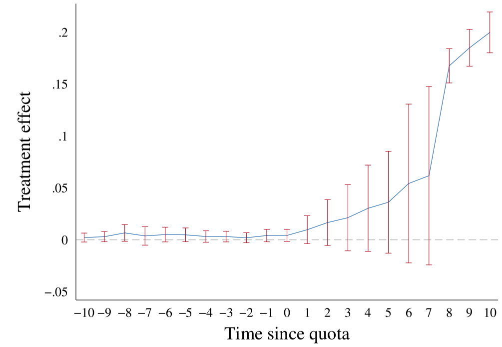
(d) Women (de Chaisemartin and D'Haultfœuille)



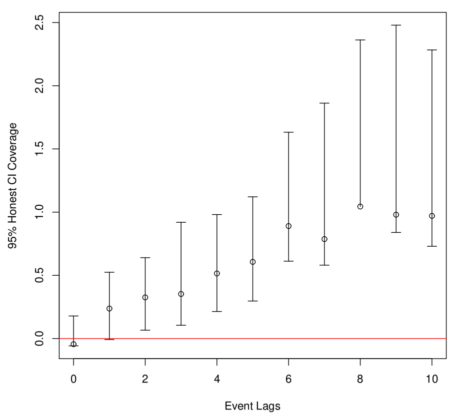
(e) Men (de Chaisemartin and D'Haultfœuille)



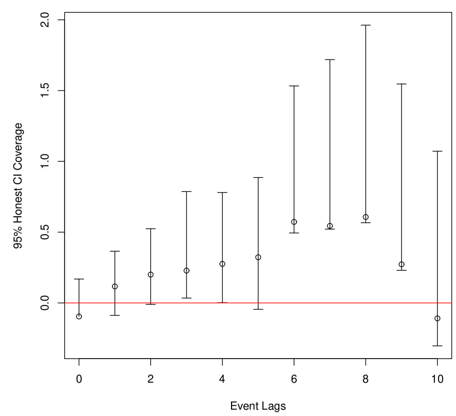
(f) Women/Men (de Chaisemartin and D'Haultfœuille)



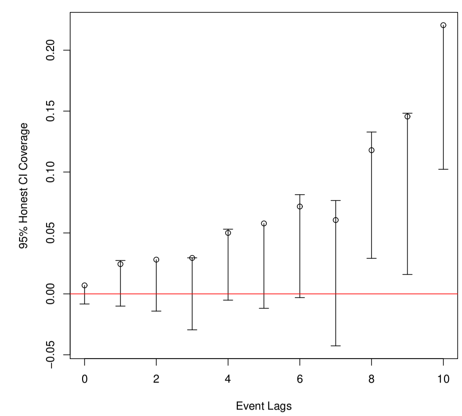
(g) Women (Honest DiD)



(h) Men (Honest DiD)



(i) 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 event study estimates, panels (d)-(f) present estimates using de Chaisemartin and D'Haultfœuille's DID_M estimator, and panels (g)-(i) present estimates based on Rambachan and Roth's Honest DiD estimator. Education measures the average years of schooling for 15–19 year-olds.

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
Polity IV Democracy score	3212	5.60	3.85	0.00	10.00
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	490	14.47	11.75	0.00	63.80
Maternal Mortality Ratio	490	442.02	290.50	12.00	1300.00
Reserved Seats	490	0.49	0.50	0.00	1.00
Male Mortality Rate (15-60)	490	310.47	157.86	75.79	753.70
Female Mortality Rate (15-60)	490	255.07	137.98	66.03	685.03
ln(GDP per capita)	490	7.97	0.94	6.20	10.83
Polity IV Democracy score	402	1.81	2.56	0.00	8.00
Percent of Pregnancies Receiving Prenatal Care	121	75.07	21.37	24.80	99.10
Percent of Births Attended by Skilled Staff	119	58.35	30.45	7.70	99.90
Health Expenditure as a % of GDP	351	5.52	2.20	0.81	11.59
Female Infant Mortality Rate (DHS subsample)	241	0.09	0.04	0.00	0.23
Male Infant Mortality Rate (DHS subsample)	242	0.10	0.05	0.00	0.33
Birth rates per 1,000 population	464	33.92	9.50	11.90	55.56
Panel C: No Reserved Seat Sample					
% Women in Parliament	3696	14.01	10.27	0.00	53.10
Maternal Mortality Ratio	3696	208.72	320.55	3.00	2890.00
Reserved Seats	3696	0.00	0.00	0.00	0.00
Male Mortality Rate (15-60)	3636	231.53	111.27	58.80	663.36
Female Mortality Rate (15-60)	3636	156.36	108.24	34.32	626.09
ln(GDP per capita)	3696	9.03	1.19	5.51	11.77
Polity IV Democracy score	2810	6.14	3.69	0.00	10.00
Percent of Pregnancies Receiving Prenatal Care	541	86.05	16.19	15.40	100.00
Percent of Births Attended by Skilled Staff	1080	86.08	21.68	5.00	100.00
Health Expenditure as a % of GDP	2773	6.33	2.40	0.72	17.10
Female Infant Mortality Rate (DHS subsample)	826	0.08	0.05	0.00	0.60
Male Infant Mortality Rate (DHS subsample)	824	0.09	0.05	0.00	0.26
Birth rates per 1,000 population	3696	23.09	11.43	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: Gender Quotas: DD 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: Full sample, no controls									
Reserved Seats	5.793***	6.473***	7.115***	-0.082	-0.073	-0.123*	-106.107**	-116.036**	-123.765***
	[2.167]	[2.165]	[2.468]	[0.051]	[0.052]	[0.074]	[43.036]	[44.583]	[26.979]
Mean of Dep. Var.	14.110	14.099	14.099	4.357	4.351	4.351	233.425	233.821	233.821
Observations	4335	4284	4284	4335	4284	4284	4335	4284	4284
Number of Countries	178	176	176	178	176	176	178	176	176
R-Squared	0.465	0.472	0.538	0.547	0.541	0.562	0.270	0.271	0.329
Panel B: Full sample, time-varying controls									
Reserved Seats	5.494**	6.278**	6.058**	-0.095*	-0.103**	-0.149**	-102.475**	-119.308**	-123.545***
	[2.481]	[2.510]	[2.619]	[0.049]	[0.051]	[0.070]	[49.720]	[50.301]	[21.773]
Mean of Dep. Var.	13.647	13.632	13.632	4.397	4.389	4.389	249.190	249.707	249.707
Observations	3212	3167	3167	3212	3167	3167	3212	3167	3167
Number of Countries	156	154	154	156	154	154	156	154	154
R-Squared	0.478	0.487	0.566	0.596	0.588	0.622	0.373	0.381	0.468
Population Weights	N	N	Y	N	N	Y	N	N	Y

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). In each case country and year fixed effects are included. Panel A presents estimations without controls, panel B includes time-varying controls of the log of PPP adjusted GDP per capita, and a democracy score. Unweighted (columns 1-2, 4-5, and 7-8), and population weighted specifications (column 3, 6 and 9) are displayed. When weighting, China and India are removed from the estimation sample, to avoid regression results being largely driven by these two countries, with a population an order of magnitude larger than other countries. The unweighted specification without these countries is displayed in columns 2, 5 and 8. Panel A consists of baseline estimates without any time-varying controls. Panel B additionally adds controls for the log of GDP per capita and fixed effects for democracy level (where available). Standard errors clustered by country are displayed in parentheses. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

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" DD 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: Full sample, no controls			
% Women in Parliament	-0.015**	-0.012*	-0.019
	[0.007]	[0.006]	[0.013]
F-Statistic First Stage	7.966	9.894	14.380
p-value First Stage	0.005	0.002	0.000
95% CI from Conley et al. (2012)	[-0.031;0.002]	[-0.025;0.003]	[-0.244;0.495]
90% CI from Conley et al. (2012)	[-0.029;-0.001]	[-0.024;0.001]	[-0.184;0.436]
Mean of Dep. Var.	4.357	4.351	4.351
Observations	4335	4284	4284
Number of Countries	178	176	176
Panel B: Full sample, time-varying controls			
% Women in Parliament	-0.020***	-0.019***	-0.025**
	[0.007]	[0.006]	[0.012]
F-Statistic First Stage	4.753	5.951	10.070
p-value First Stage	0.031	0.016	0.002
95% CI from Conley et al. (2012)	[-0.037;-0.005]	[-0.034;-0.005]	[-0.053;0.049]
90% CI from Conley et al. (2012)	[-0.035;-0.007]	[-0.032;-0.007]	[-0.046;0.042]
Mean of Dep. Var.	4.397	4.389	4.389
Observations	3212	3167	3167
Number of Countries	156	154	154
Population Weights	N	N	Y

Instrumental variables regressions are run where gender quotas are used to instrument women in parliament. The first stage regression of women in parliament on reserved seats is displayed in columns 1-3 of Table A2. F-Statistics of the first stage and the associated p-value are traditional tests of instrumental relevance. 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 is only close to holding. These confidence intervals are associated with the estimates where quotas are able to have a direct effect in reducing MMR that is *not* mediated by women in parliament of 0.01 (or 1%) using Conley et al. (2012)'s Union of Confidence Intervals (UCI) approach. Each regression includes country and year fixed effects and clusters standard errors by country. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table A5: Alternative inference procedures for measures of maternal mortality in principal diff-in-diff specification

	ln(Maternal Mortality Ratio)			Maternal Mortality Ratio		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Aggregate de Chaisemartin and D'Haultfœuille DID_M Estimate						
Reserved Seats (Point Estimate)	-0.103	-0.105	-0.084	-107.50	-113.60	-96.23
p-value Bootstrap	0.024	0.038	0.142	0.011	0.009	0.052
p-value Triangular Correction	0.112	0.145	0.246	0.037	0.035	0.095
p-value Triangular Correction by Country	0.028	0.041	0.152	0.012	0.011	0.055
p-value Normal Correction	0.380	0.395	0.464	0.113	0.110	0.203
p-value Normal Correction by Country	0.021	0.031	0.122	0.021	0.018	0.071
Panel B: Two-Way Fixed Effect Estimates						
Reserved Seats (Point Estimate)	-0.095	-0.103	-0.149	-102.48	-119.31	-123.54
p-value Bootstrap	0.061	0.065	0.036	0.050	0.026	0.001
p-value Triangular Correction	0.077	0.081	0.056	0.055	0.029	0.001
p-value Triangular Correction by Country	0.062	0.067	0.041	0.051	0.027	0.001
p-value Normal Correction	0.146	0.153	0.112	0.056	0.039	0.001
p-value Normal Correction by Country	0.062	0.067	0.038	0.083	0.047	0.001
Mean of Dep. Var.	4.397	4.389	4.389	249.190	249.707	249.707
Observations	3212	3167	3167	3212	3167	3167
Number of Countries	156	154	154	156	154	154
R-Squared	0.596	0.587	0.619	0.373	0.378	0.449
Population Weights	N	N	Y	N	N	Y

de Chaisemartin and D'Haultfœuille (2020) and difference-in-differences estimates of the impact of reserved seats in parliament on log of the maternal mortality ratio (columns 1-3), and MMR in levels (columns 4-6) replicate results from Figure 3A and Tables 1 A2 of the paper. 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.

Online Appendix – Not for Print

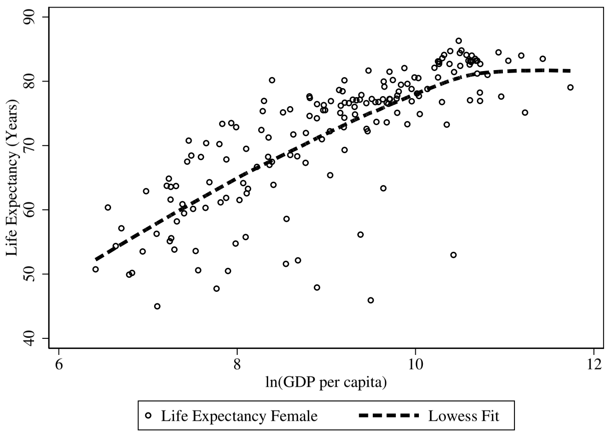
Maternal Mortality and Women's Political Participation

Sonia Bhalotra, Damian Clarke, Joseph Gomes, Atheendar Venkataramani

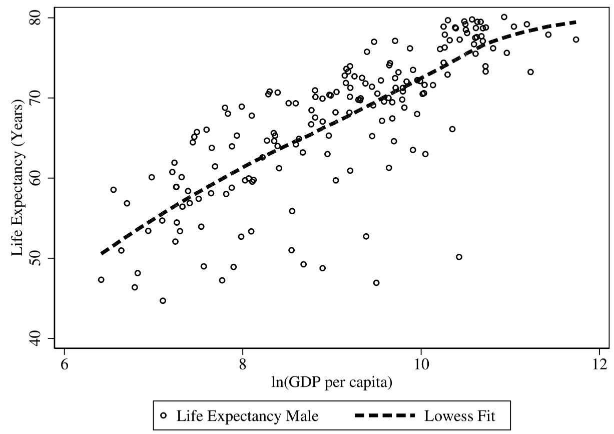
Online Appendix Figures

Figure B1: Life expectancy in 2010

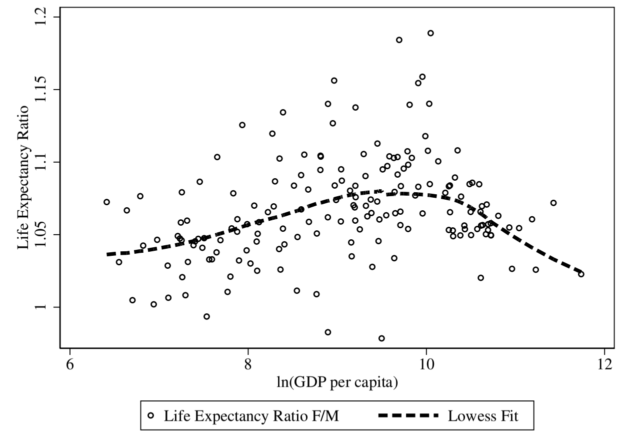
(a) Female life expectancy



(b) Male life expectancy

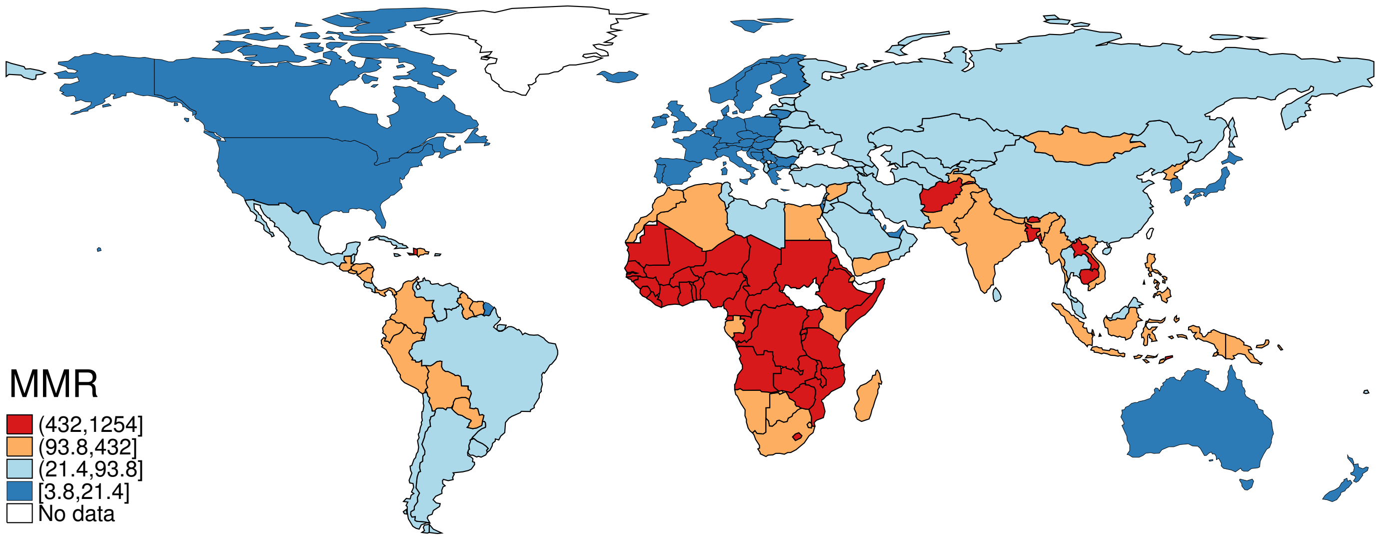


(c) Female/male life expectancy ratio



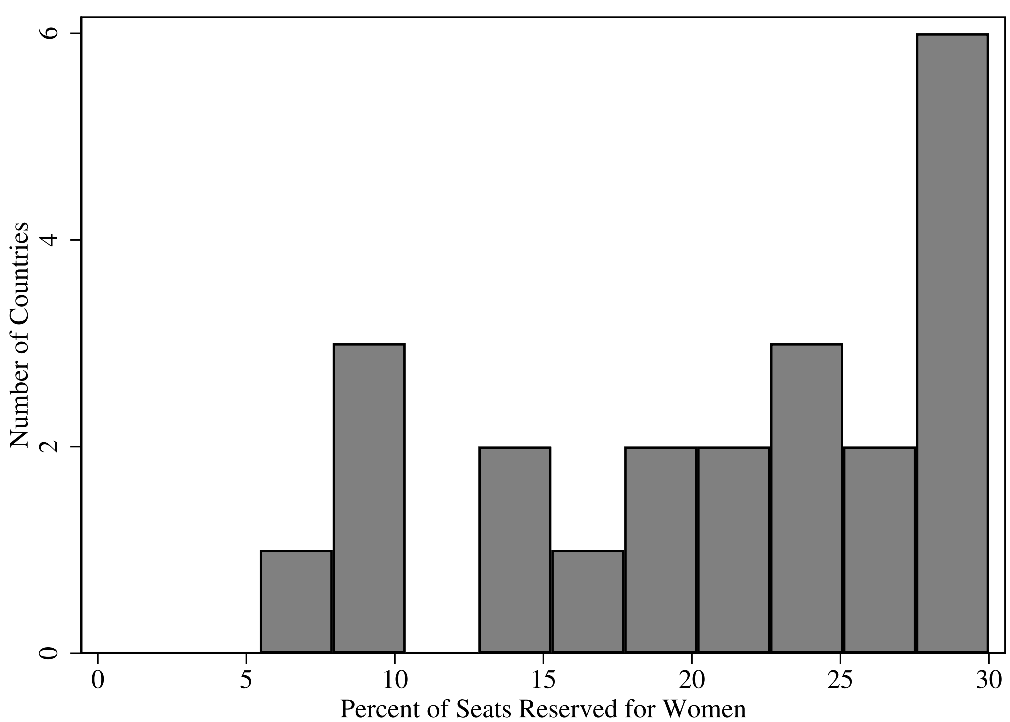
Notes: Life expectancy at birth and PPP-adjusted GDP per capita data are collated by the World Bank Data Bank. These indicators are SP.DYN.LE00.FE.IN (Female life expectancy) SP.DYN.LE00.MA.IN (Male life expectancy) and NY.GDP.PCAP.PP.KD (PPP adjusted GDP per capita). The life expectancy ratio is calculated as female life expectancy divided by male life expectancy for each country. Lowess fits are overlaid on scatter plots, using a bandwidth of 0.8 for local linear smoothing.

Figure B2: Maternal mortality ratio: 1990–2015



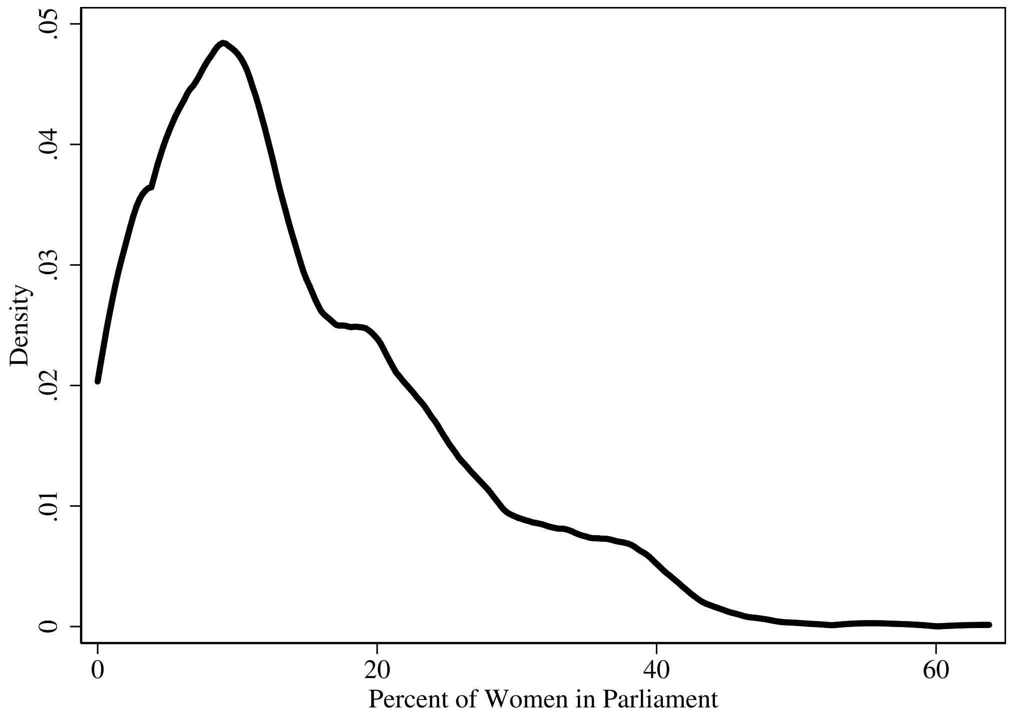
Notes: Average rates by country for the period 1990–2015. Values are calculated as deaths per 100,000 live births, and are provided by the MMEIG, an inter-agency project of the WHO, UNICEF, UNFPA, World Bank Group, and the United Nations Population Division.

Figure B3: Reserved seat quota sizes



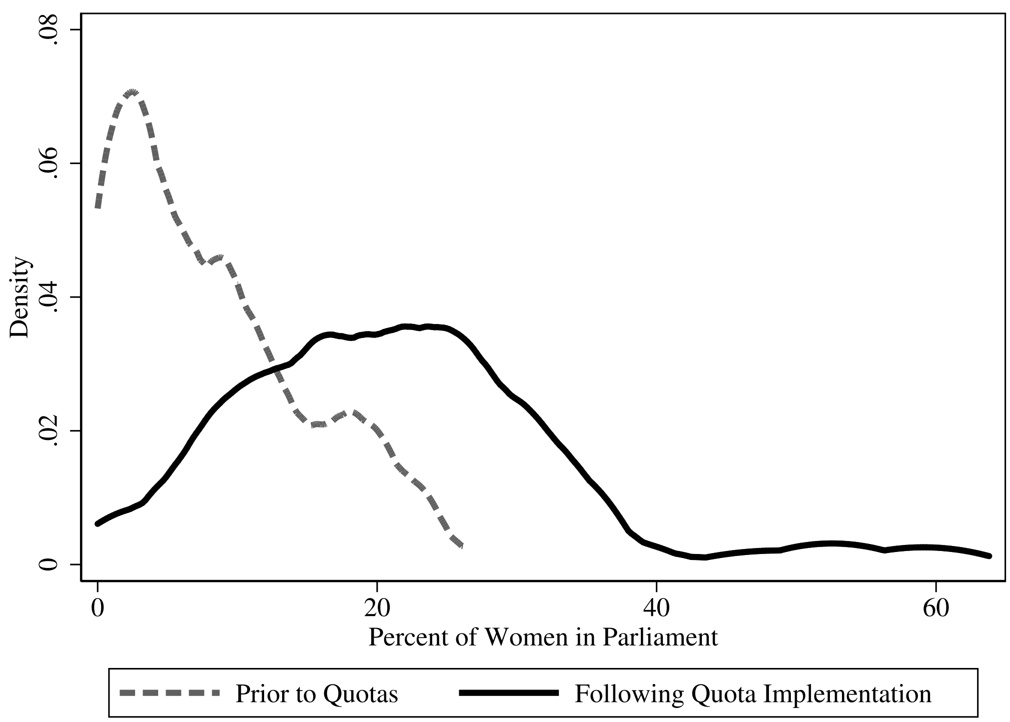
Notes: This histogram describes the quota size for each country which adopts a reserved seat quota. Each country (quota) is included as a single observation.

Figure B4: Proportion of women in parliament in all countries



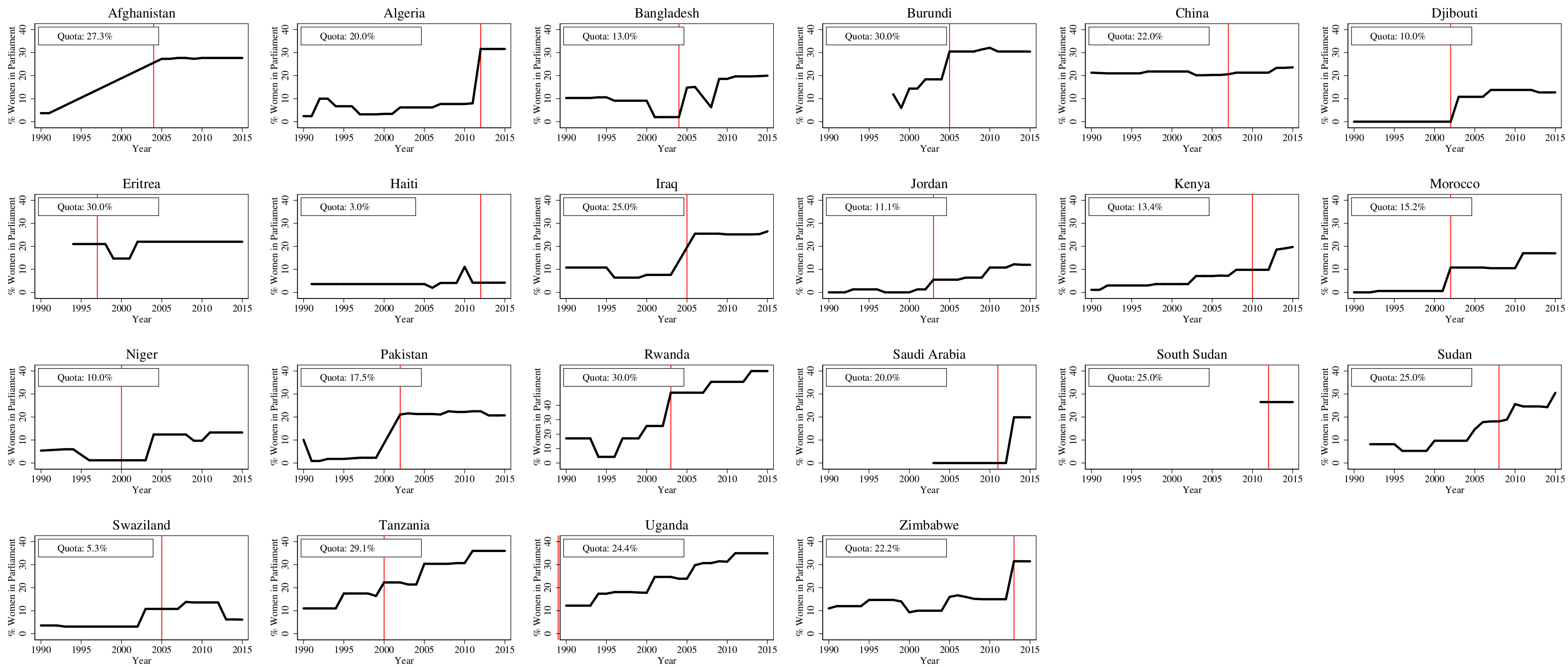
Notes: Density plots describe the proportion of women in parliament in all countries and years under study.

Figure B5: Proportion of women in parliament in countries with reserved seats



Notes: Density plots for the proportion of women in parliament in countries which at some point adopt a reserved seat quota. Plots are based on each country by year observation in the women in parliament data.

Figure B6: Country-specific changes in women in parliament after reserved seat quotas

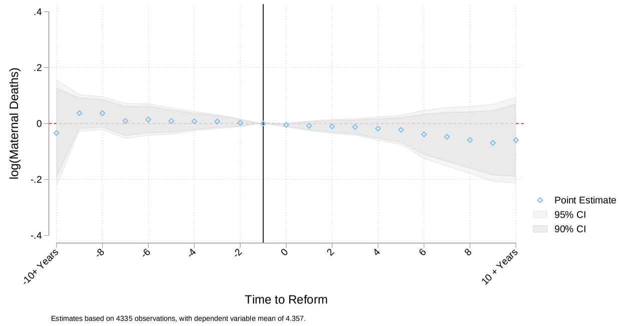


Notes: In each panel, the vertical lines display the recorded date of the passage of a reserved seat quota for women in the national parliament, and the plots show the evolution of the percentage of women in parliament. Each figure shares a common y-axis for ease of comparison.

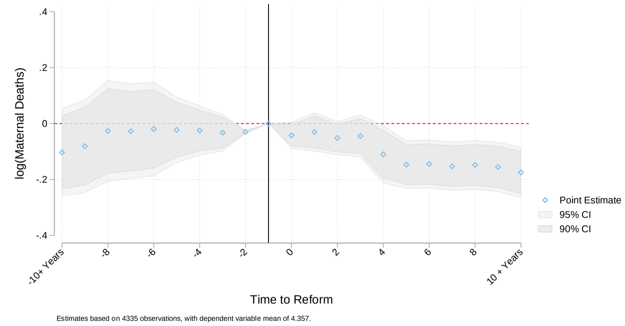
Figure B7: Impact of quotas on MMR by size of gender quota

Panel A: By Quota Size

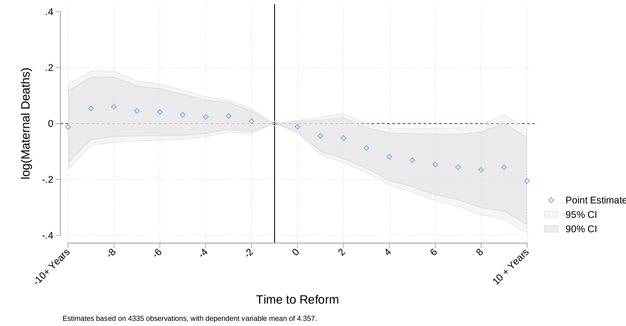
(a) Quota Size: 0-15



(b) Quota Size: 15-20

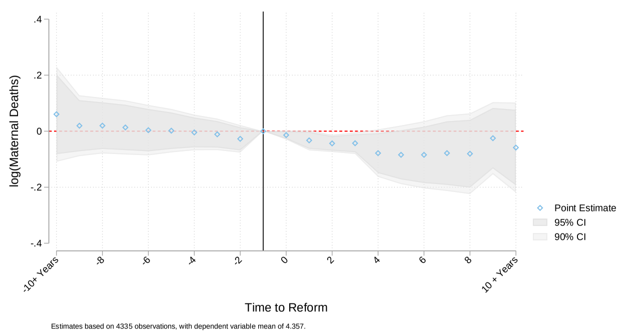


(c) Quota Size: 20-30

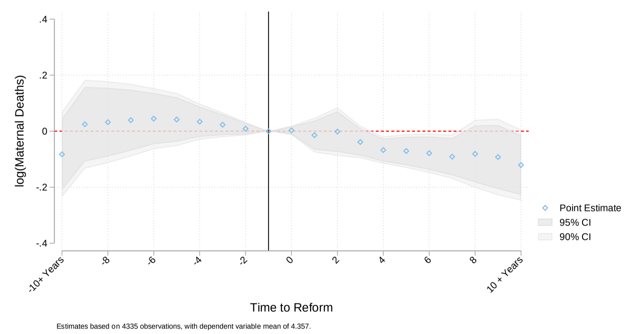


Panel B: By Baseline Rates of Maternal Mortality

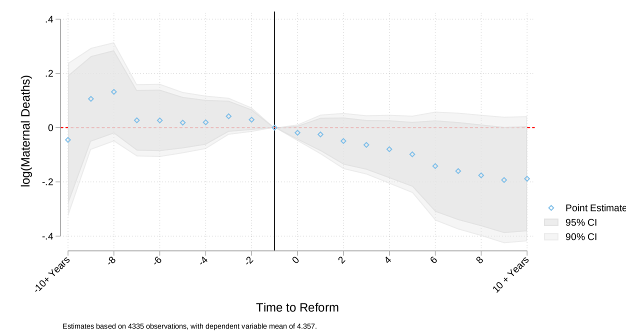
(d) “Low” baseline Rates (MMR <400)



(e) “Medium” baseline Rates (MMR [400,800))



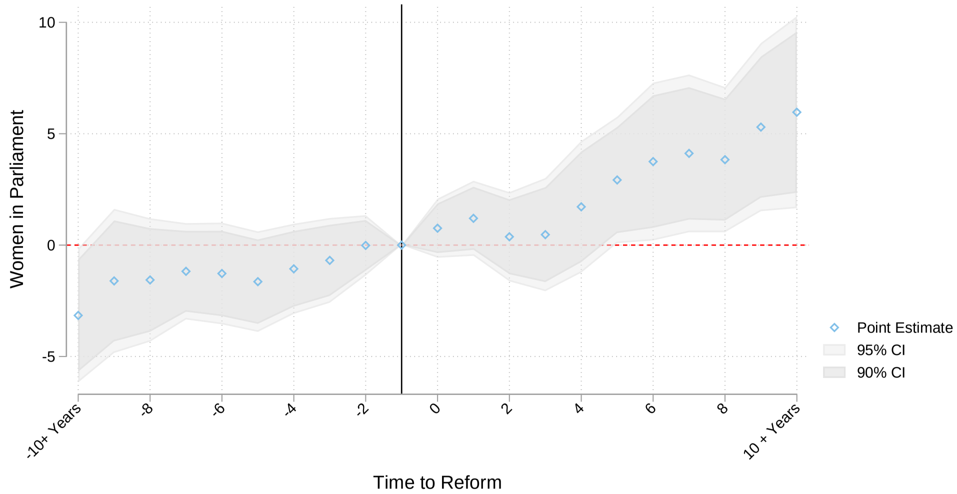
(f) “High” baseline Rates (MMR $\geq$800)



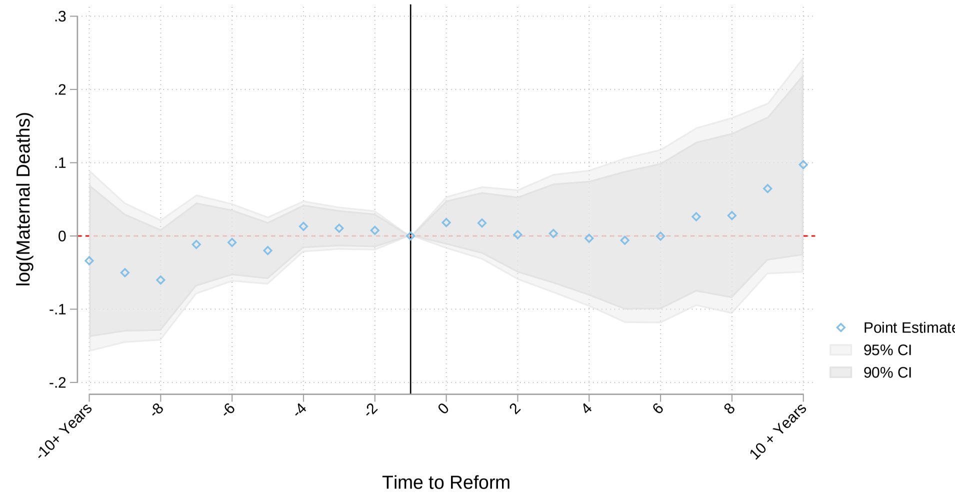
Notes: Event study estimates are presented separately for countries with a different proportion of seats reserved in their quota (Panel A) or with different rates of maternal mortality at baseline (Panel B). These are presented in three bins. Bins approximately separate quota countries into three evenly sized groups. Baseline rates of maternal mortality are calculated as average values in countries prior to the year 2000. Each set of coefficients is estimated in a single event study (one in Panel A and one in Panel B), and so are conditional on all other groups in the panel. In each case, the baseline group consists of countries which do not implement a quota. Remaining details follow Figure 2.

Figure B8: Candidate list quotas: event studies for women in parliament and maternal mortality

(a) Women in parliament

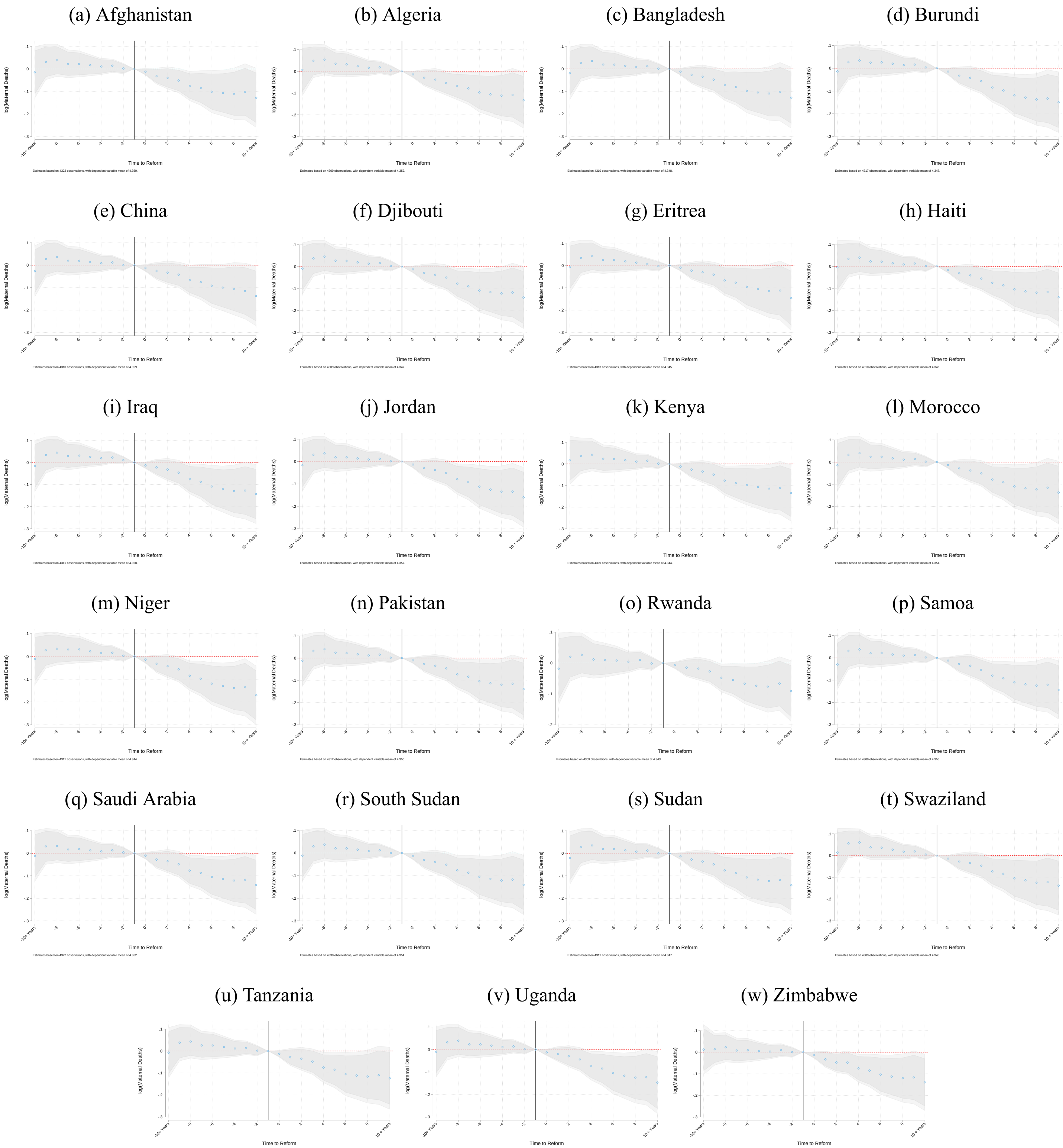


(b) ln(MMR)



Notes: Identical specifications are estimated as in Figure 2, however now with countries passing candidate list quotas. Difference-in-difference estimates from a single-coefficient model suggest an increase in 1.776 (standard error 2.172) in the proportion of women in parliament following reserved seat quotas, and a reduction of 0.017 (standard error 0.071) in log(MMR). Analogous values for unweighted estimates are 3.569 (s.e. 1.180) for women in parliament, and 0.032 (s.e. 0.055) for log(MMR). Countries implementing candidate list quotas in the period under study are Albania, Angola, Argentina, Armenia, Belgium, Bolivia, Bosnia and Herzegovina, Brazil, Burkina Faso, Costa Rica, Croatia, Dominican Republic, Ecuador, El Salvador, France, Greece, Guinea, Guyana, Honduras, Indonesia, Ireland, South Korea, Kyrgyz Republic, Lesotho, Macedonia, Mauritania, Mexico, Mongolia, Montenegro, Nepal, Nicaragua, Panama, Paraguay, Peru, Poland, Portugal, Senegal, Serbia, Slovenia, Spain, Tunisia and Uruguay.

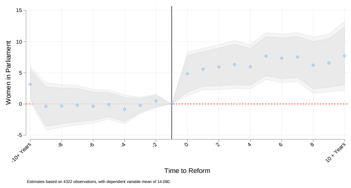
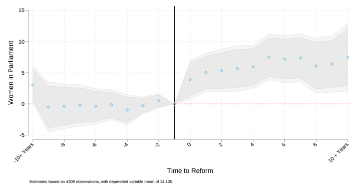
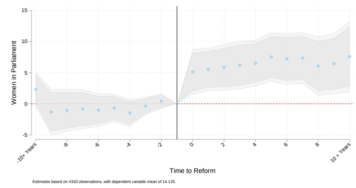
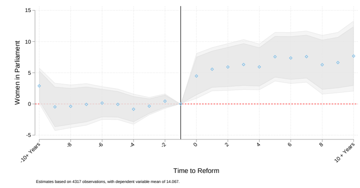
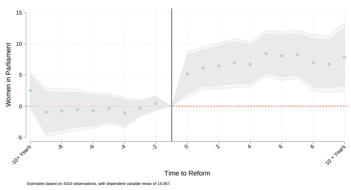
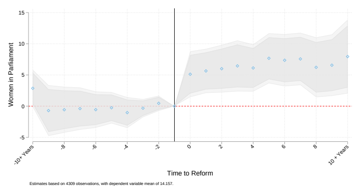
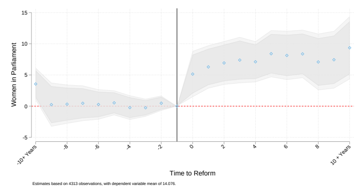
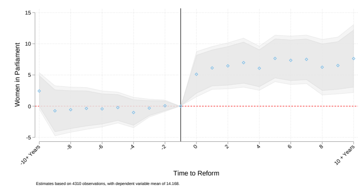
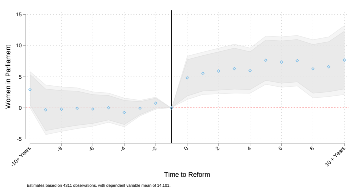
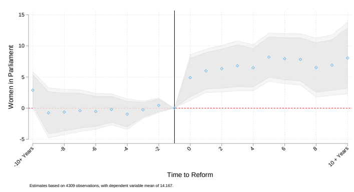
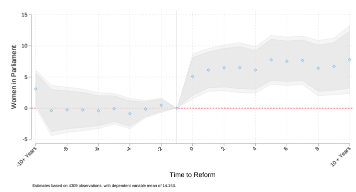
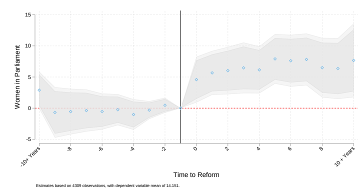
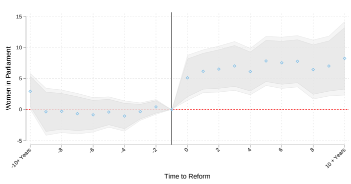
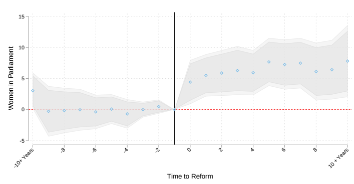
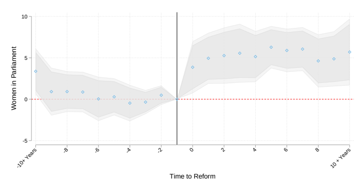
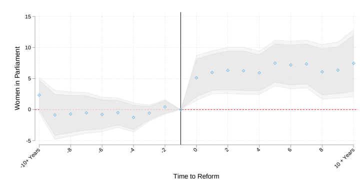
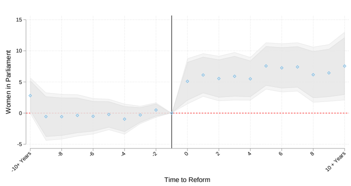
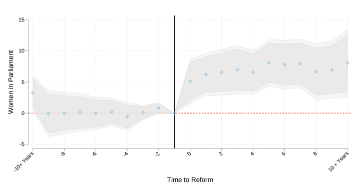
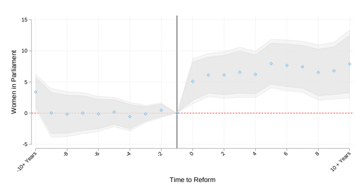
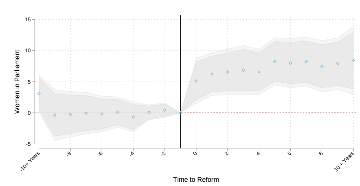
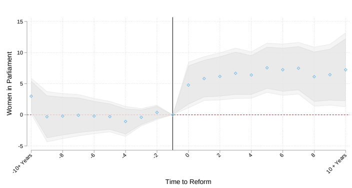
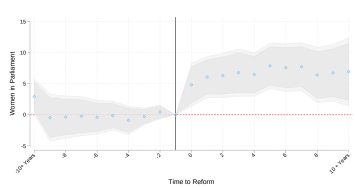
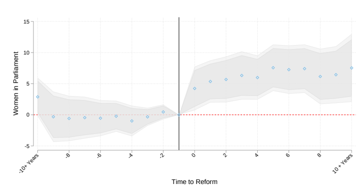
Figure B9: Leave-one-out analysis: maternal mortality



(a) Afghanistan (b) Algeria (c) Bangladesh (d) Burundi (e) China (f) Djibouti (g) Eritrea (h) Haiti (i) Iraq (j) Jordan (k) Kenya (l) Morocco (m) Niger (n) Pakistan (o) Rwanda (p) Samoa (q) Saudi Arabia (r) South Sudan (s) Sudan (t) Swaziland (u) Tanzania (v) Uganda (w) Zimbabwe

Notes: Plots replicate panel (a) of Figure 2, however leave out one quota-adopting country at a time. The title of each plot refers to the country which is removed when estimating each event study model. All additional details follow the baseline model presented in Figure 2.

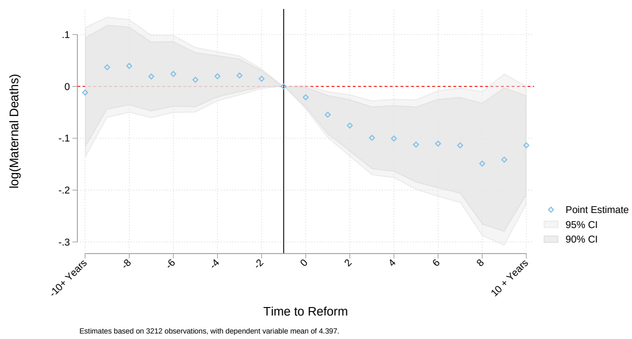
Figure B10: Leave-one-out analysis: women in parliament

(a) Afghanistan

(b) Algeria

(c) Bangladesh

(d) Burundi

(e) China

(f) Djibouti

(g) Eritrea

(h) Haiti

(i) Iraq

(j) Jordan

(k) Kenya

(l) Morocco

(m) Niger

(n) Pakistan

(o) Rwanda

(p) Samoa

(q) Saudi Arabia

(r) South Sudan

(s) Sudan

(t) Swaziland

(u) Tanzania

(v) Uganda

(w) Zimbabwe


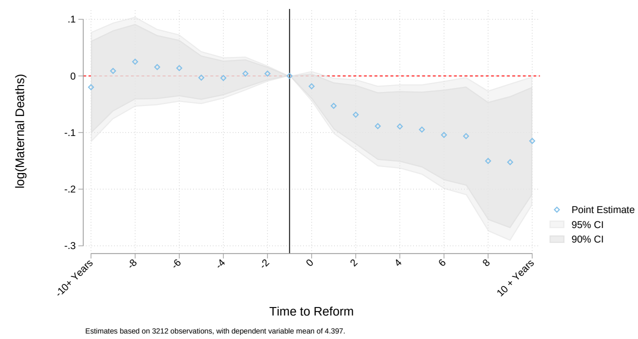
Notes: Plots replicate panel (b) of Figure 2, however leave out one quota-adopting country at a time. The title of each plot refers to the country which is removed when estimating each event study model. All additional details follow the baseline model presented in Figure 2.

Figure B11: Alternative specifications of quota event study (maternal mortality)

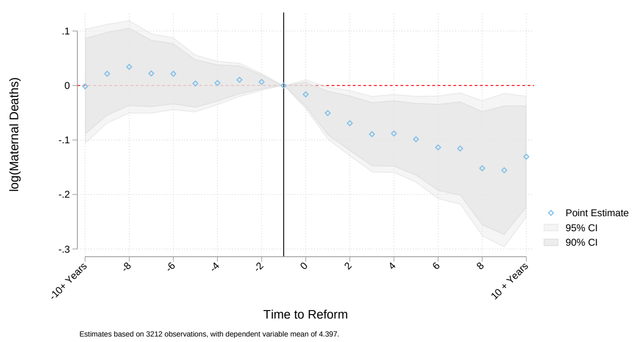
(a) Democracy control only



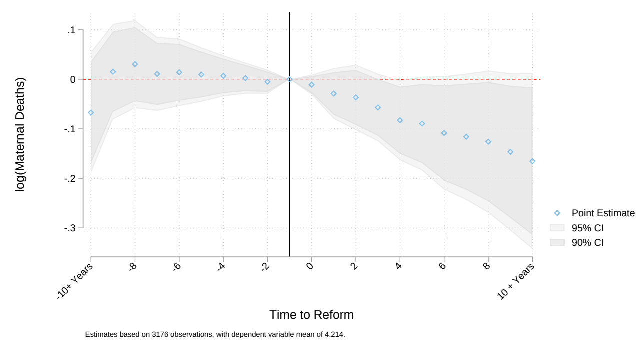
(b) GDP control only



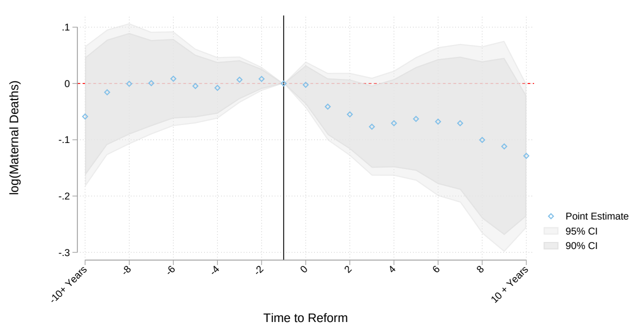
(c) GDP and democracy controls



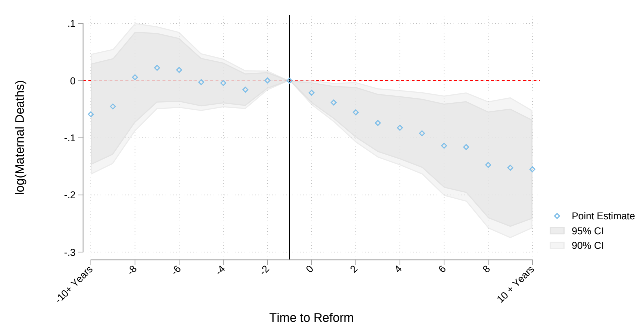
(d) Demographic controls



(e) Region by Year FEs



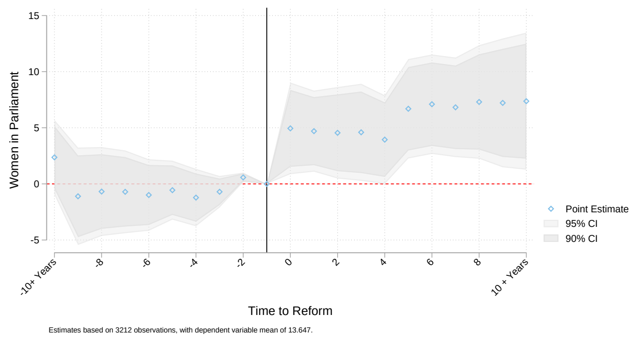
(f) Conditional on health expenditure



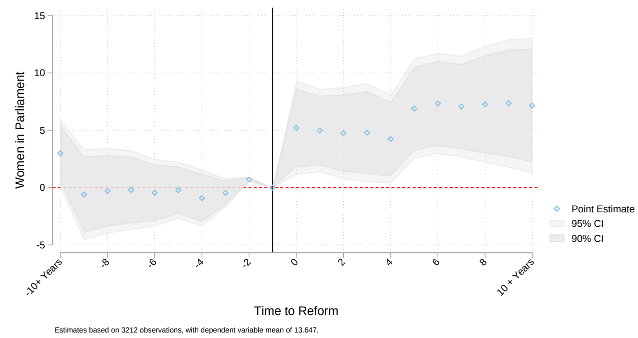
Notes: Each panel documents an alternative specification of the event study shown in Figure 2 panel (a). Specifications are shown with alternative controls. Demographic controls in panel (d) refer to time-varying controls of the education of fertile aged women, the proportion of the population in fertile ages, and the ethnic fractionalization of the population. Region by year FEs include separate year fixed effects for every sub-region based on the United Nations classification. Finally panel (f) additionally controls for health expenditure as a proportion of GDP, and Development Aid receipts for maternal health. Additional notes are provided in Figure 2.

Figure B12: Alternative specifications of quota event study (women in parliament)

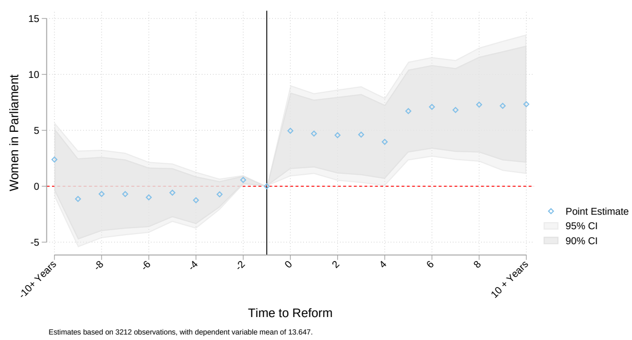
(a) Democracy control only



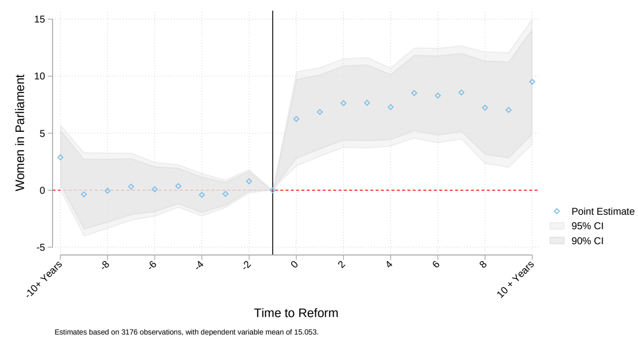
(b) GDP control only



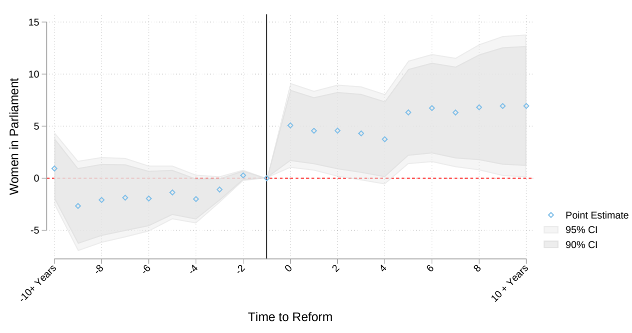
(c) GDP and democracy controls



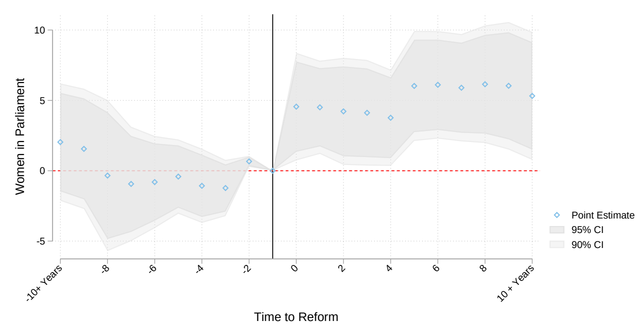
(d) Demographic controls



(e) Region by Year FEs



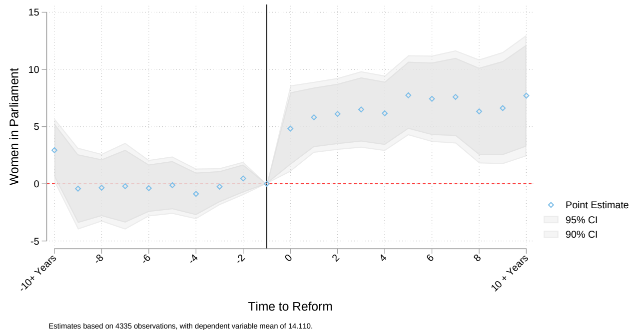
(f) Conditional on health expenditure



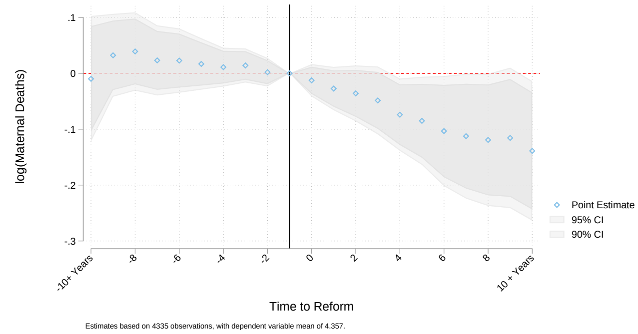
Notes: Refer to notes to Figure B11. Identical plots or shown where the outcome is women in parliament.

Figure B13: Alternative samples and modeling choices for event studies

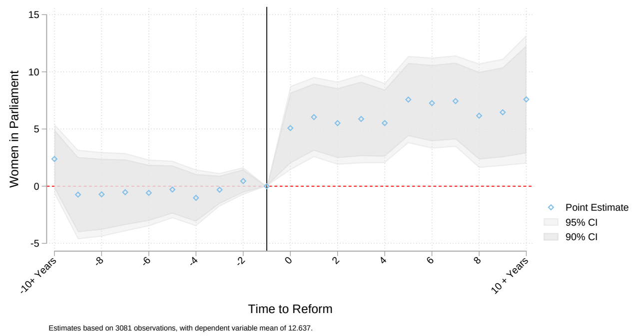
(a) Clustering by country and time – women in parliament



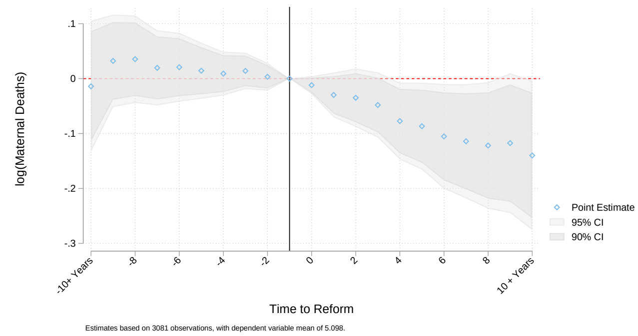
(b) Clustering by country and time – ln(MMR)



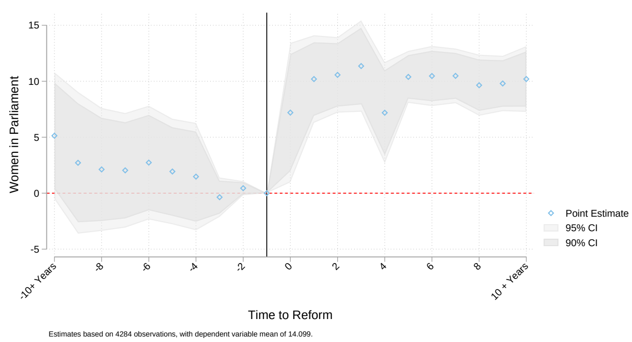
(c) Removing high income countries – women in parliament



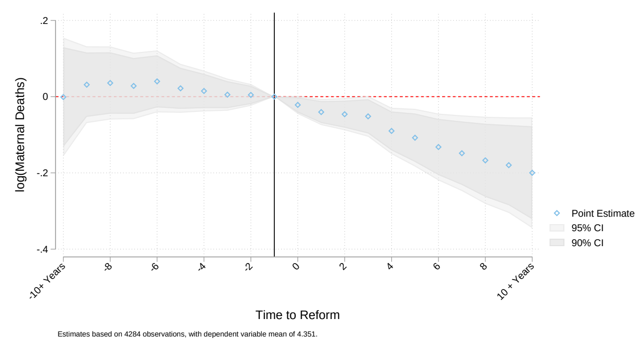
(d) Removing high income countries – ln(MMR)



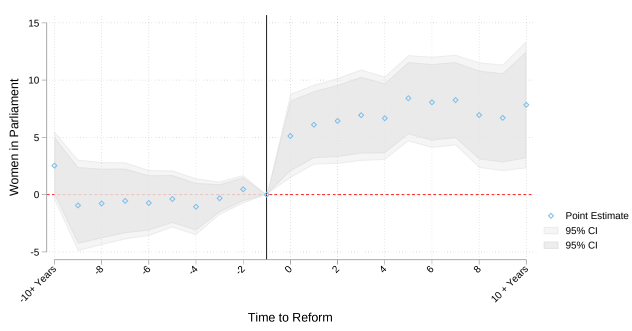
(e) Weighted estimates (No India/China) – women in parliament



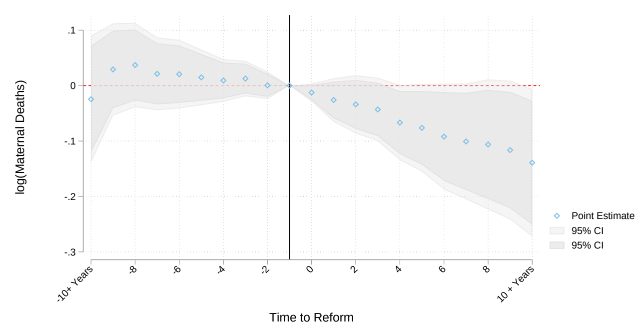
(f) Population-weighted estimates (No India/China) – ln(MMR)



(g) No India/China samples – women in parliament



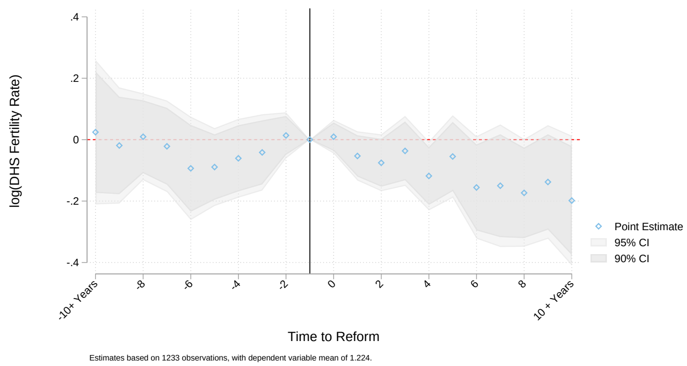
(h) No India/China samples – ln(MMR)



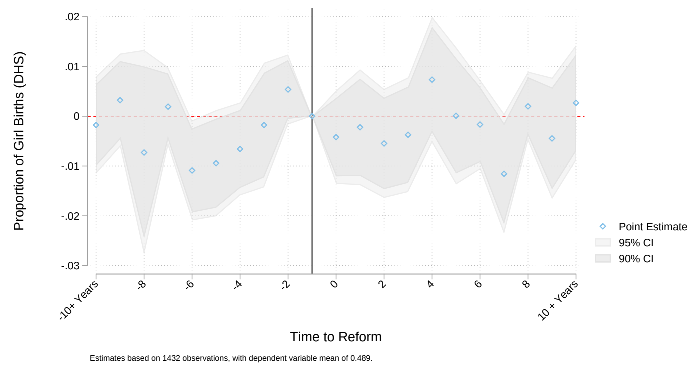
Notes: Each panel plots event study estimates of the impact of quota passage on women in parliament (left-hand column) or maternal mortality (right-hand column), based on alternative samples or modeling decisions. The top panel estimates standard errors with double clustering (by country and year). The second row removes high income countries from the control group. A static (2015) measure of high income is used to ensure consistency of the sample across years. The third row provides estimates using country population weights, where India and China are removed given that their population (weight) is an order of magnitude larger than most other countries. Estimates for the same unweighted sample are available in the final row.

Figure B14: Characteristics of births and mothers: DHS birth pseudo-panel

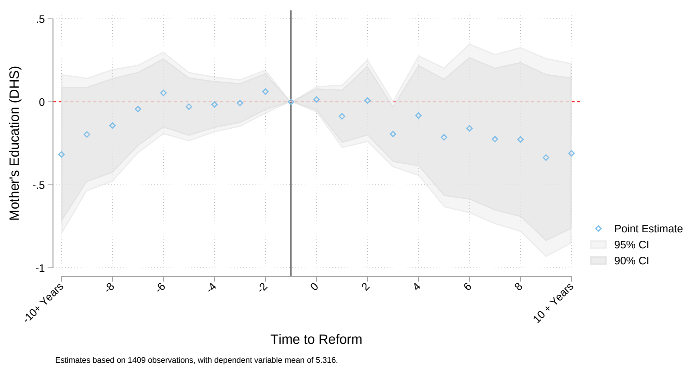
(a) log(Fertility Rates)



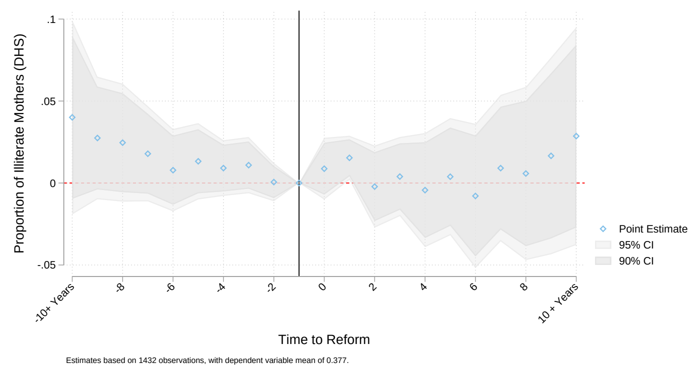
(b) Proportion of Girls



(c) Mother's Education



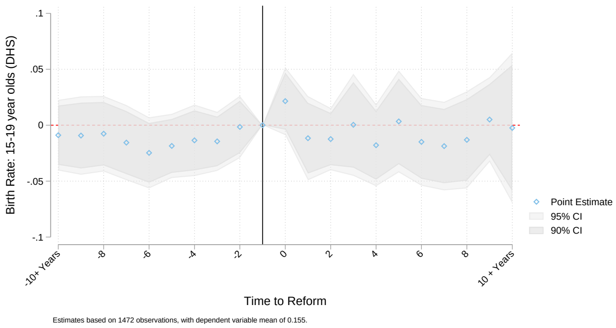
(d) Proportion of Illiterate Mothers



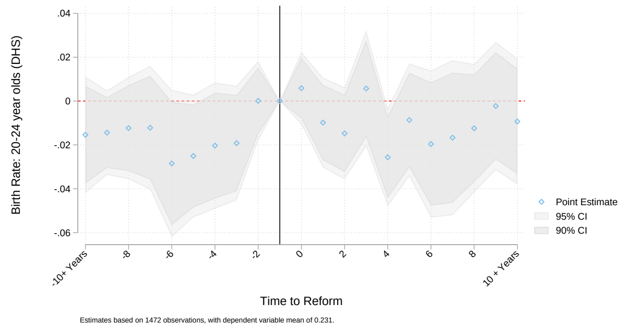
Notes: Event studies are estimated using country by year averages generated from a pseudo-panel of births created from Demographic and Health Survey data. Fertility rates refers to the log of the total fertility rate, calculated as the total number of births a woman would have if completing her fertile life based on current age-specific fertility rates. The proportion of girls refers to the proportion of girl births among all births. Mother's education refers to the average years of education of mothers (women having given birth) in each country and year. Proportion of illiterate mothers refers to the total proportion of all mothers (women having given birth) in each country and year who report not being able to read and write. All additional details follow notes to Figure 2.

Figure B15: Age-specific birth rates calculated from DHS birth pseudo-panel

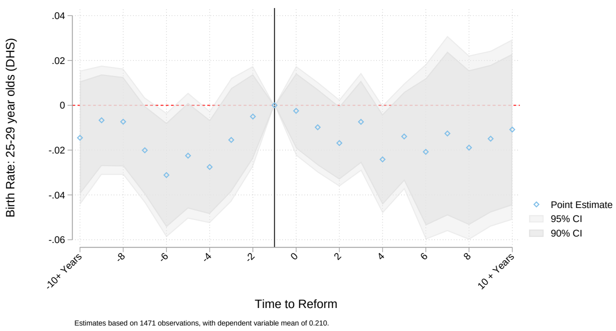
(a) 15–19



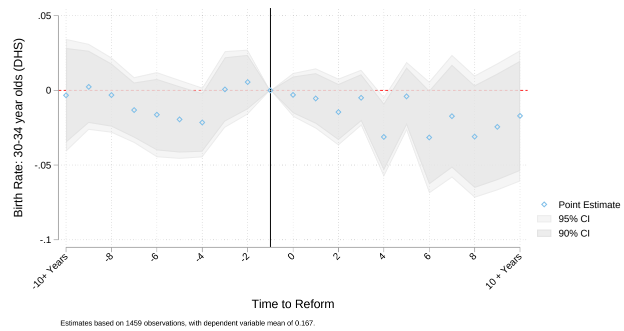
(b) 20–24



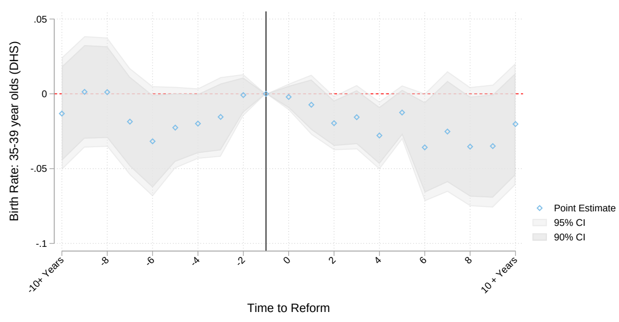
(c) 25–29



(d) 30–34



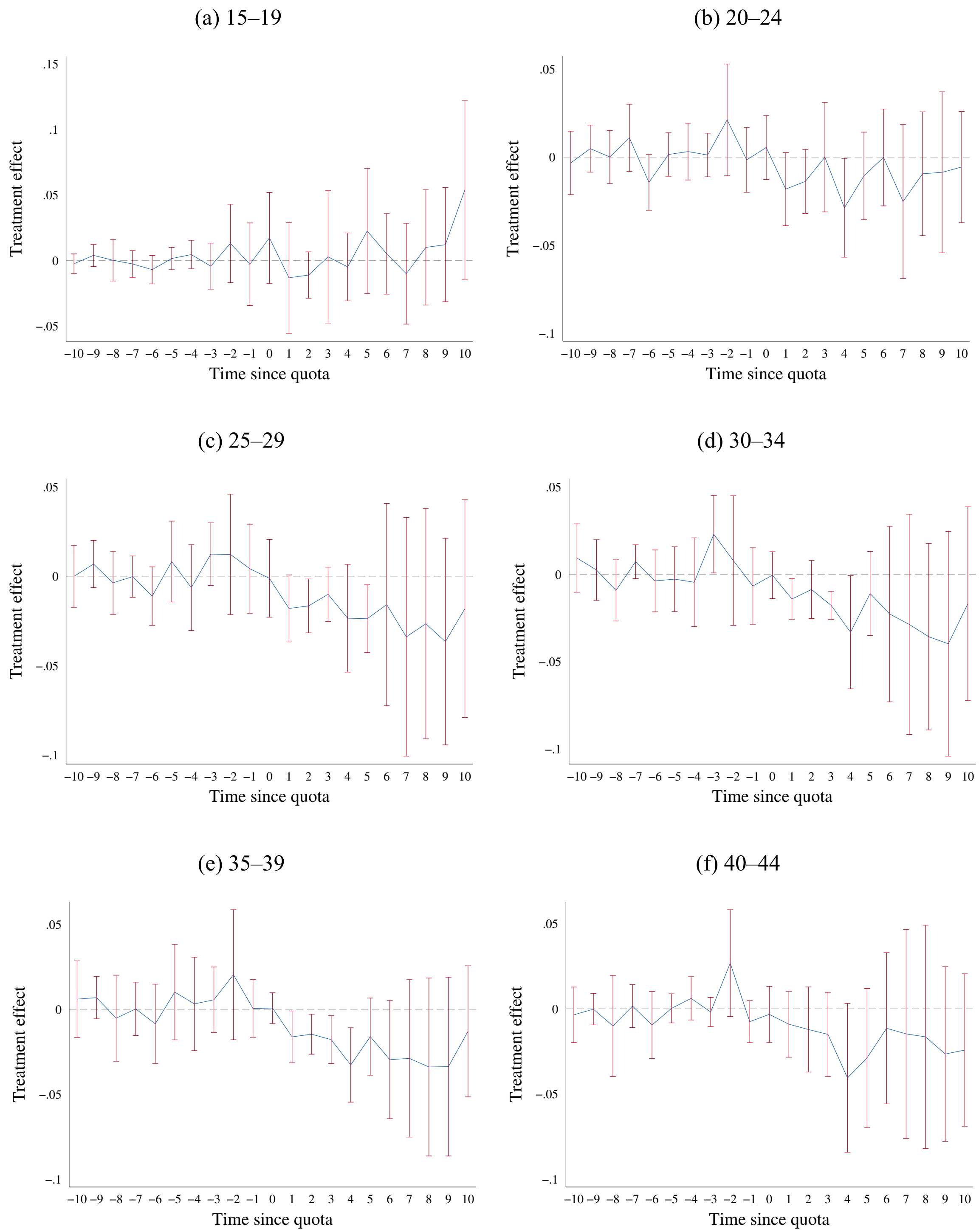
(e) 35–39



(f) 40–44

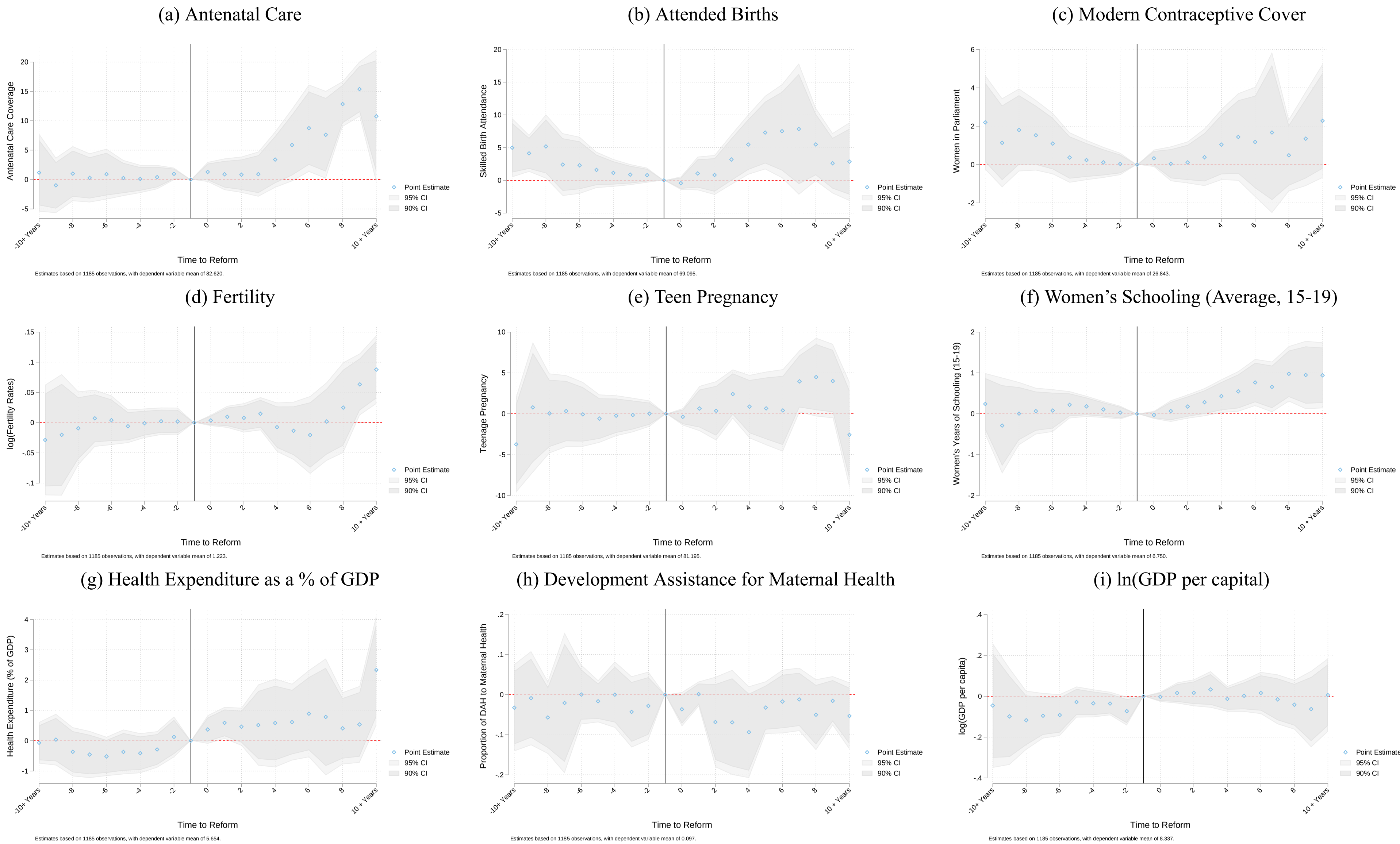
Notes: Event studies are estimated using country by year averages generated from a pseudo-panel of births created from Demographic and Health Survey data. Birth rates in each figure are calculated from birth indicators in the pseudo-panel based on women in each quinquennial age group in each country and year, where 1 indicates a surveyed woman (of this age) reported a birth in a given year, and 0 indicates they did not have a birth. Average birth rates across the full age range of 15–49 year-olds are documented in panel (a) of Figure B14.

Figure B16: de Chaisemartin and D'Haultfœuille estimates for impacts of gender quotas on age-specific birth rates calculated from DHS birth pseudo-panel



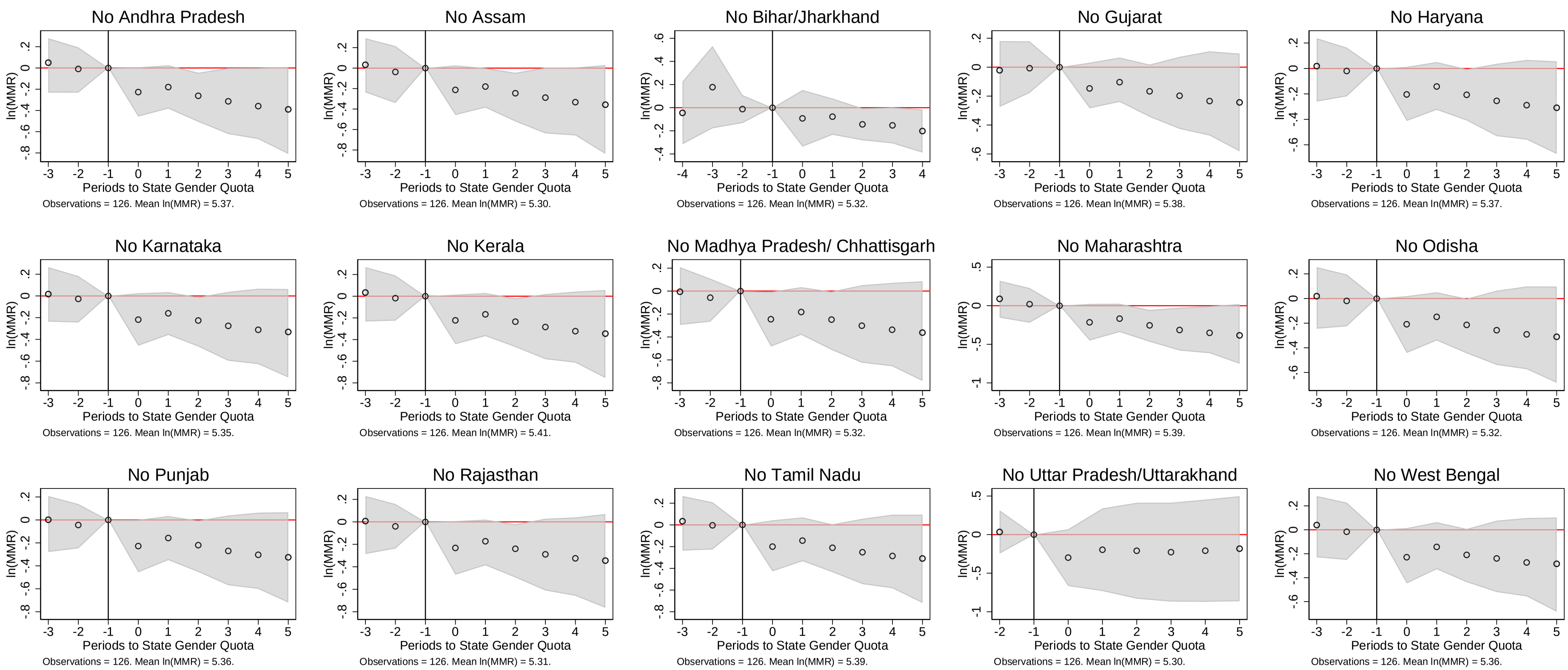
Notes: Figures consider quota passage and age specific birth rates calculated from the DHS. Each panel replicates plots from Figure B15, however estimation follows de Chaisemartin and D'Haultfœuille (2020)'s "DID_M" estimator.

Figure B17: Mechanisms: event studies for impacts of gender quotas on intermediate outcomes based on a constant sample



Notes: Event study estimates of intermediate outcomes replicate those displayed in the paper in Figure 8. However here all models work with a common sample containing observations for each intermediate outcome considered. For additional notes, refer to Figure 8.

Figure B18: Leave-one-out event study estimates (India state reforms)

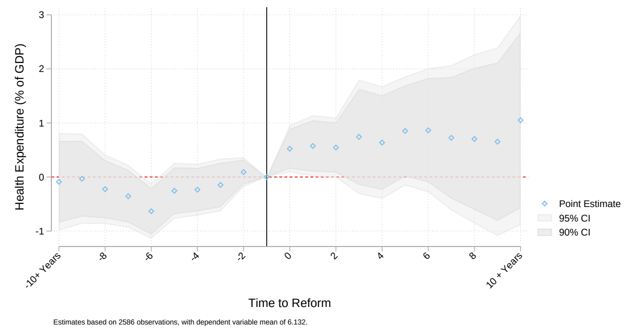


Notes: Refer to notes to Figure A11. Here identical event studies are estimated, however removing one state in each case on a leave-one-out basis. The left-out state is indicated in each subplot's title.

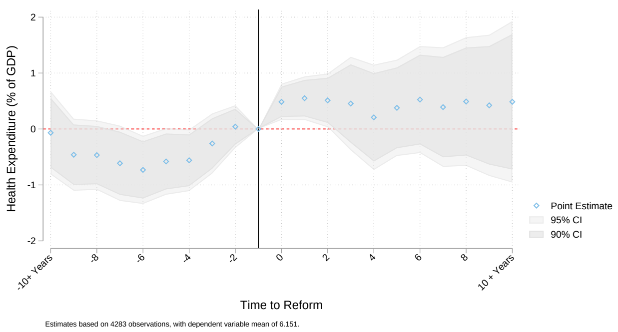
Figure B19: Health spending – various event study specifications

(a) Original model with no controls

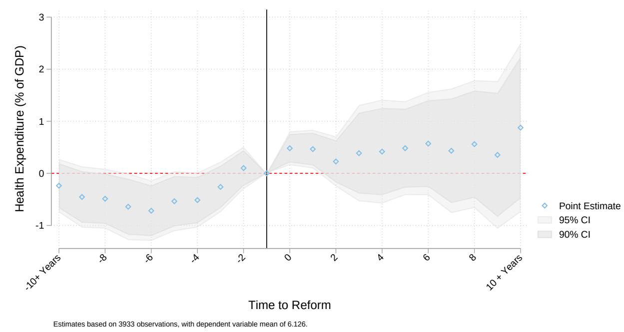
(b) Original model adding controls



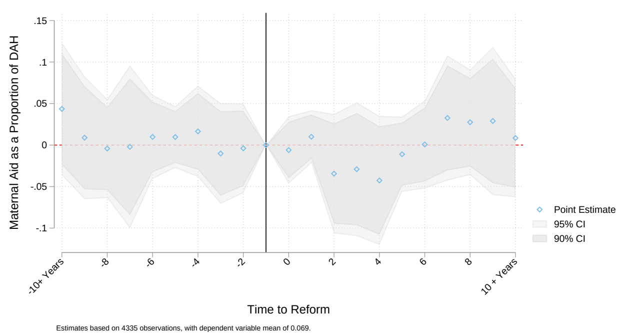
(c) Extrapolating to cover 1990-1994



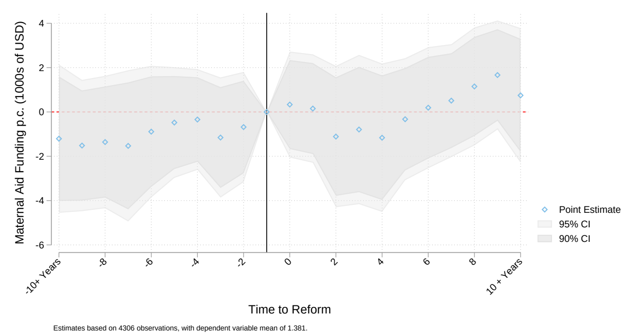
(d) Imputing 1990-1994 with 1995



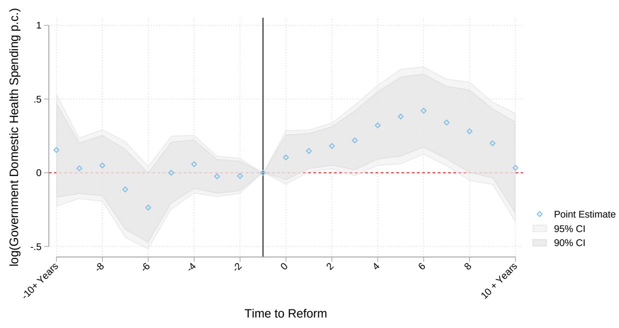
(e) Proportion of Aid to Maternal Health



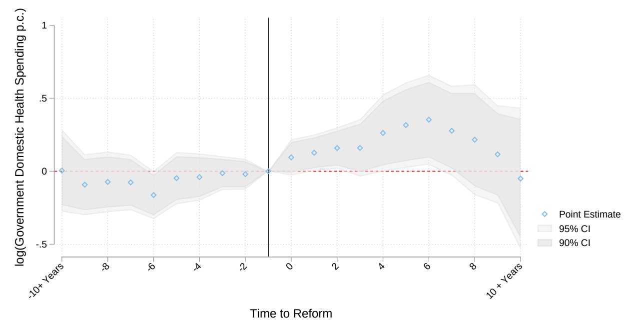
(f) Maternal Health Aid p.c.



(g) ln(Government Health Spending p.c.)

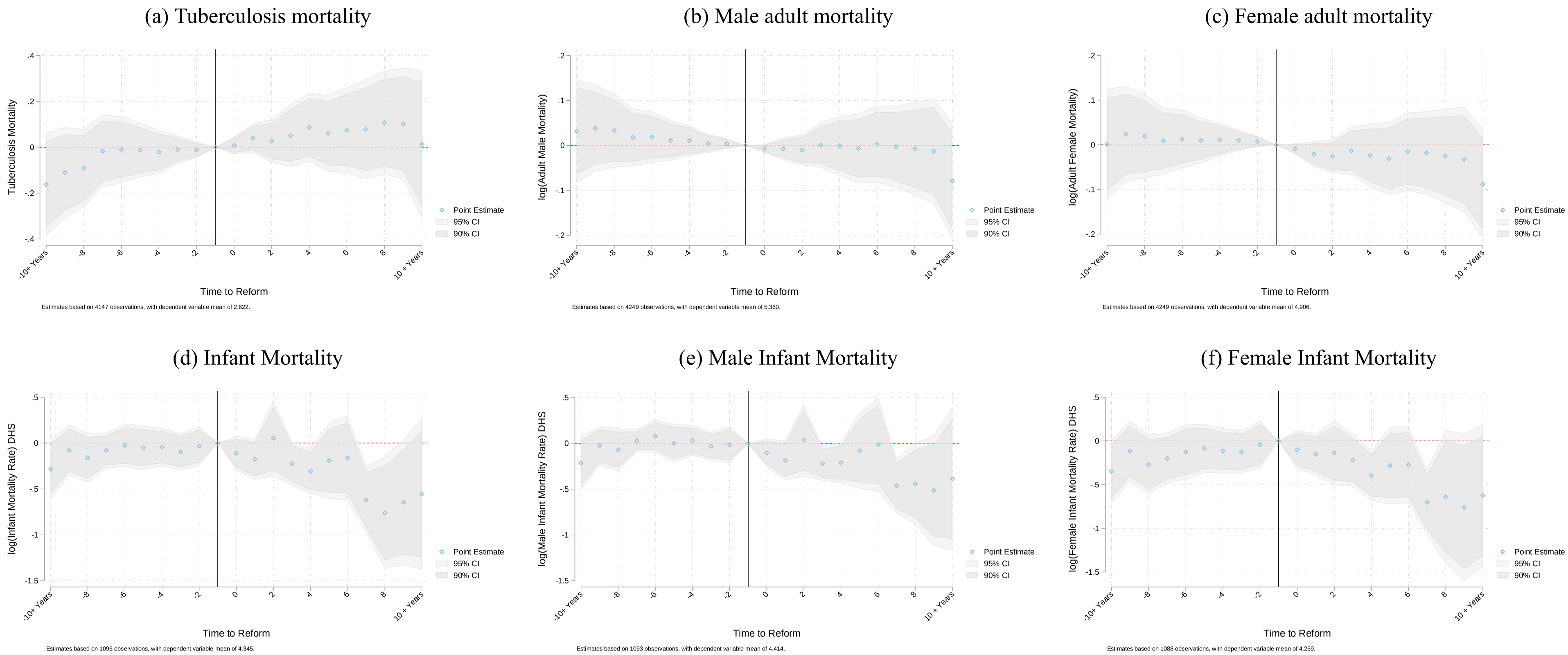


(h) ln(Gov't Health Spend) Imputing 1990-'99 with 2000



Notes: Alternative event-study models are displayed examining the impact of quota reform on health spending. A baseline model is displayed in Figure 8 (replicated in panel (a) here), and Honest DiD lags are presented in Figure 10. Health spending as a proportion of GDP is available world-wide from 1995 onwards. Panels (a) and (b) use original data removing 1990-1994 from the sample. Panels (c) and (d) linearly extrapolate to cover 1990-1994, or use values from 1995 to impute for 1990-1994. Panels (e)-(g) use alternative measures of health spending (proportion of all overseas aid receipts for health dedicate to maternal health, maternal health aid per capita, or the log of total government health spending per capita.

Figure B20: Event Studies for the impact of gender quotas on alternative health outcomes



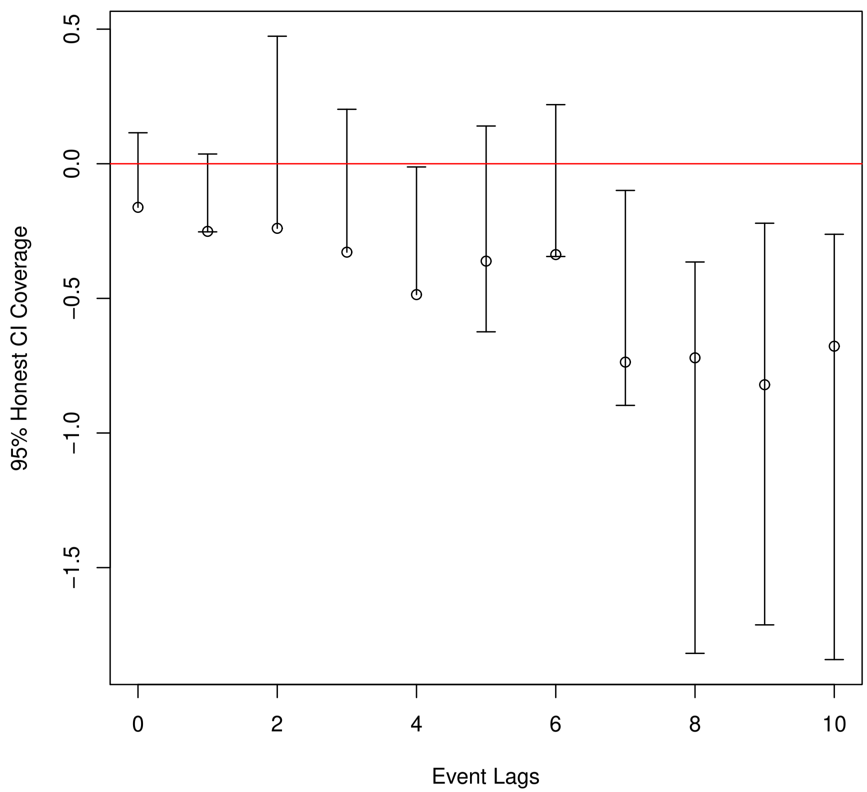
(a) Tuberculosis mortality (b) Male adult mortality (c) Female adult mortality

(d) Infant Mortality (e) Male Infant Mortality (f) Female Infant Mortality

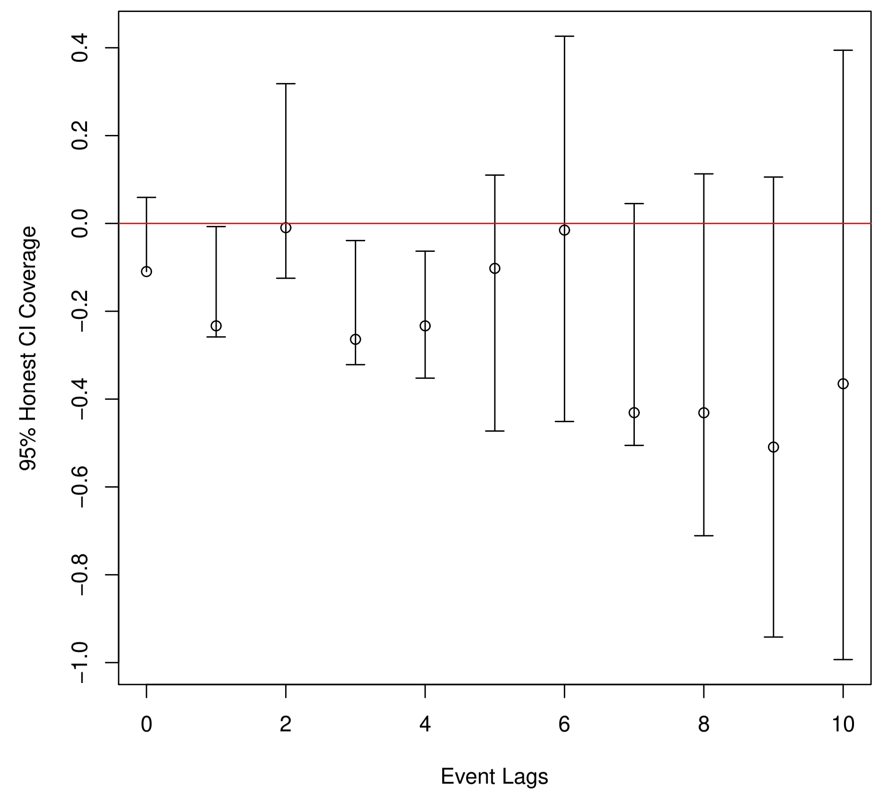
Notes: Identical event studies are plotted to those in Figure 2, however examining quota impacts on alternative health outcomes. These outcomes are the rate of death due to TB per 100,000 individuals, and infant mortality per 1,000 live births, male mortality per 1,000 adult males (ages 15–60), and female mortality per 1,000 adult females (ages 15–60). For comparison with Figure 6b, the natural logarithm of each variable is used, with the exception of TB mortality, where an inverse sine transformation is used, given a small number of country-year observations where a rate of zero is observed. Infant mortality is recorded from Demographic and Health Survey (DHS) microdata where mothers report their full fertility history, any children who have died, their child's age at death (if relevant) and the year of death (if relevant). We generate infant mortality rates using retrospective fertility and survival histories for DHS surveys in the 68 publicly available DHS countries.

Figure B21: Post-quota coefficients based on “Honest DiD”: female and male infant mortality using DHS microdata

(a) ln(female infant mortality)



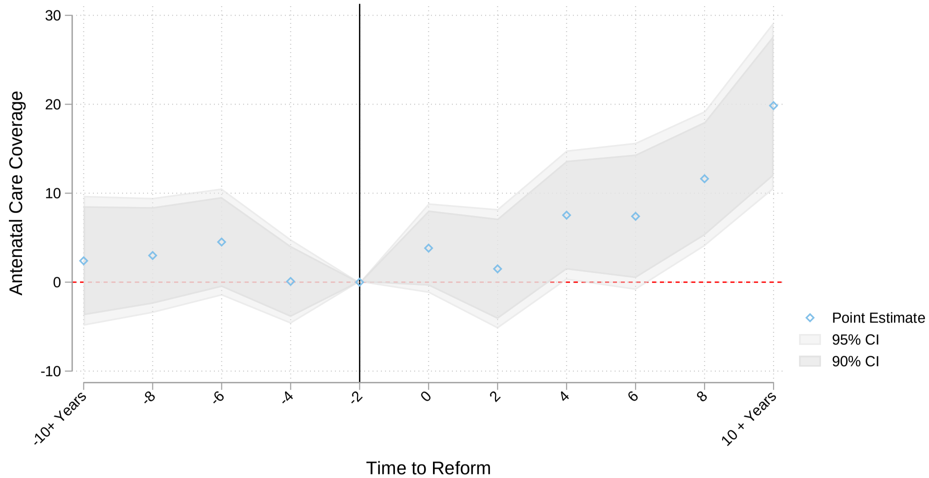
(b) ln(male infant mortality ratio)



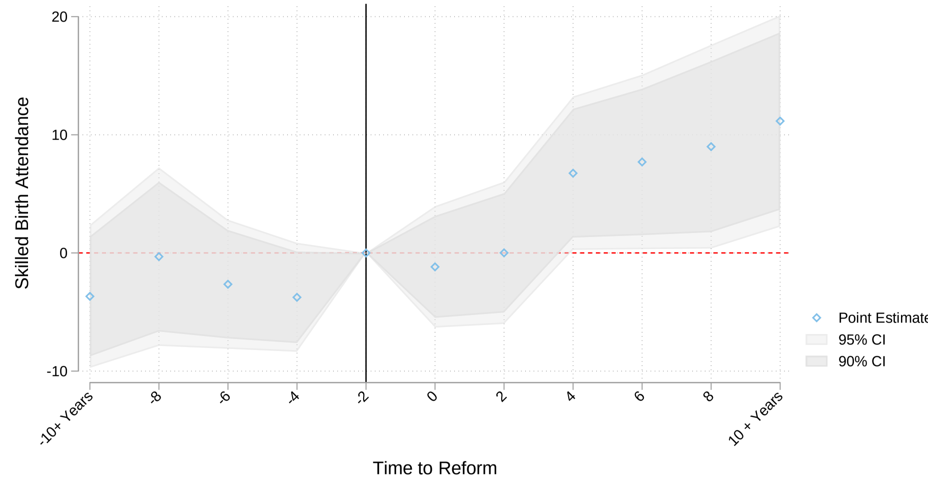
Notes: Refer to notes to 5. Identical procedures are estimated, however now examining female and male infant mortality measures considered in Figure B20 of the paper.

Figure B22: Mechanisms: event studies for impacts of quotas on non-interpolated antenatal care and birth attendance

(a) Antenatal Care Coverage



(b) Attended Births



Notes: Event studies replicate panels (a) and (b) of Figure 8 using non-interpolated data. Given the unbalanced coverage of antenatal care and attended birth measures by countries and years, we present estimates for these outcomes pooling in 2 yearly bins, rather than yearly bins, to avoid unbalanced coverage in particular lag and lead terms where possible.

Online Appendix Tables

Table B1: Quota countries: quota size, and quota baseline MMR level

Country	Quota Size		Baseline MMR	
	Size	Category	Value	Category
Afghanistan	27.3	High	821	High
Algeria	20	Medium	192	Low
Bangladesh	13	Low	481	Medium
Burundi	30	High	993	High
China	22	Medium	73	Low
Djibouti	10	Low	452	Medium
Eritrea	30	High	931	High
Haiti	3	Low	522	Medium
Iraq	25	High	85	Low
Jordan	11.1	Low	93	Low
Kenya	13.4	Low	708	High
Morocco	15.2	Medium	261	Low
Niger	10	Low	825	High
Pakistan	17.5	Medium	374	Low
Rwanda	30	High	1220	High
Saudi Arabia	20	Medium	20	Low
South Sudan	25	High	857	High
Sudan	25	High	626	Medium
Swaziland	5.26	Low	569	Medium
Tanzania	29.1	High	946	High
Uganda	24.4	Medium	673	High
Zimbabwe	22.2	Medium	475	Medium

Table B2: Intensive margin impacts of reserved seats (binned by quota size)

	% Women in Parliament			ln(Maternal Mortality Ratio)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Full sample, no controls						
Reserved Seats (0-15]%	2.766***	2.717***	2.893***	-0.035	-0.037	-0.165*
	[0.953]	[0.954]	[0.841]	[0.056]	[0.056]	[0.091]
Reserved Seats (15-20]%	11.901***	11.865***	10.855***	-0.037	-0.039	-0.083
	[1.849]	[1.856]	[1.217]	[0.073]	[0.073]	[0.074]
Reserved Seats (20-30]%	6.805*	8.298**	9.805***	-0.134	-0.118	-0.127
	[3.641]	[3.681]	[2.123]	[0.082]	[0.090]	[0.081]
Mean of Dep. Var.	14.110	14.099	14.099	4.357	4.351	4.351
Observations	4335	4284	4284	4335	4284	4284
Number of Countries	178	176	176	178	176	176
R-Squared	0.469	0.477	0.547	0.547	0.541	0.563
Panel B: Full sample, time-varying controls						
Reserved Seats (0-15]%	2.368*	2.363*	2.120***	-0.062	-0.063	-0.108*
	[1.263]	[1.276]	[0.792]	[0.038]	[0.038]	[0.062]
Reserved Seats (15-20]%	7.458***	7.366***	9.431***	-0.119***	-0.121***	-0.237**
	[2.418]	[2.404]	[1.143]	[0.042]	[0.043]	[0.092]
Reserved Seats (20-30]%	7.234*	9.074**	10.774***	-0.140	-0.160*	-0.133*
	[4.340]	[4.437]	[2.579]	[0.088]	[0.097]	[0.074]
Mean of Dep. Var.	13.647	13.632	13.632	4.397	4.389	4.389
Observations	3212	3167	3167	3212	3167	3167
Number of Countries	156	154	154	156	154	154
R-Squared	0.479	0.489	0.577	0.597	0.589	0.623
Population Weights	N	N	Y	N	N	Y

Difference-in-difference estimates of the impact of the size of the gender quota on women in parliament (columns 1-3) and maternal mortality (columns 4-6) are presented. Specifications follow Table A2, replacing the binary quota indicator with a binned indicator for quota size. Bins approximately separate quotas into three equal groups. Each independent variable shown is equal to zero whenever reserved seats for women are not in place in a country, and equal to one when a reserved seat quota is in place, and is of the magnitude indicated in the independent variable listed in the table. Remaining details are available as notes to Table A2. * p<0.10; ** p<0.05; *** p<0.01.

Table B3: Impacts of reserved seats by baseline MMR

	% Women in Parliament			ln(Maternal Mortality Ratio)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Full sample, no controls						
Reserved Seats (Low Baseline MMR)	4.728*	6.815***	9.404***	-0.077	-0.038	-0.077
	[2.548]	[2.121]	[1.308]	[0.051]	[0.040]	[0.072]
Reserved Seats (Mid Baseline MMR)	2.939*	2.896	3.081***	-0.043	-0.046	-0.215***
	[1.772]	[1.772]	[1.107]	[0.064]	[0.064]	[0.071]
Reserved Seats (High Baseline MMR)	9.319**	9.281**	9.046***	-0.129	-0.131	-0.094
	[4.321]	[4.321]	[2.703]	[0.112]	[0.112]	[0.095]
Mean of Dep. Var.	14.110	14.099	14.099	4.357	4.351	4.351
Observations	4335	4284	4284	4335	4284	4284
Number of Countries	178	176	176	178	176	176
R-Squared	0.469	0.475	0.543	0.547	0.541	0.564
Panel B: Full sample, time-varying controls						
Reserved Seats (Low Baseline MMR)	2.528	4.295**	8.227***	-0.059*	-0.072**	-0.181**
	[2.356]	[2.060]	[1.487]	[0.034]	[0.033]	[0.089]
Reserved Seats (Mid Baseline MMR)	4.138**	4.092**	2.317**	-0.079	-0.080	-0.143***
	[1.706]	[1.723]	[0.991]	[0.060]	[0.060]	[0.052]
Reserved Seats (High Baseline MMR)	8.225	8.221	8.515***	-0.164	-0.164	-0.106
	[5.059]	[5.026]	[3.145]	[0.102]	[0.103]	[0.081]
Mean of Dep. Var.	13.647	13.632	13.632	4.397	4.389	4.389
Observations	3212	3167	3167	3212	3167	3167
Number of Countries	156	154	154	156	154	154
R-Squared	0.479	0.485	0.572	0.597	0.589	0.622
Population Weights	N	N	Y	N	N	Y

Difference-in-difference estimates of the impact of the size of the gender quota on women in parliament (columns 1-3) and maternal mortality (columns 4-6) are displayed. Specifications follow Table A2, replacing the binary quota indicator with a series of quota indicators, indicating if a country adopted a reserved seat quota, and if its baseline maternal mortality was in the group (<400), the group (400-800) or the group ($\geq$ 800). Magnitudes approximately separate quotas into three equal groups. Each independent variable shown is equal to zero whenever reserved seats for women are not in place in a country, and equal to one when a reserved seat quota is in place, and baseline MMR is of the magnitude described. Remaining details are available as notes to Table A2. * p<0.10; ** p<0.05; *** p<0.01.

Table B4: Test of trend break in ln(maternal mortality) around quota passage

	True Model	Placebo Breaks (Year Pre-Quota Treated as Placebo)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Adoption	-10	-9	-8	-7	-6	-5	-4	-3
Pre-Quota Trend	0.001	0.005	0.005	0.005	0.005	0.006	0.006	0.007	0.007
	[0.006]	[0.006]	[0.006]	[0.006]	[0.007]	[0.007]	[0.008]	[0.008]	[0.008]
Post-Quota Trend	-0.012**	0.002	0.002	0.003	0.005	0.008	0.013	0.019	0.025
	[0.006]	[0.004]	[0.005]	[0.006]	[0.008]	[0.011]	[0.015]	[0.019]	[0.024]
Observations	4,335	4,090	4,090	4,090	4,090	4,090	4,090	4,090	4,090
R-Squared	0.547	0.520	0.520	0.520	0.520	0.520	0.520	0.520	0.520
Test of Trend Break (p-value)	0.12	0.39	0.36	0.45	0.98	0.54	0.36	0.31	0.29

Each column presents specifications where the log of the maternal mortality ratio is regressed on country and year fixed effects, and separate trends are fit in pre- and post-quota implementation periods. Column 1 presents the true model where trends are fit around quota implementation. Columns 2-9 present similar models using only pre-quota periods, with placebo trend breaks allowed at the number of years pre-quota implementation indicated in column headings. In each case we formally test for equality of trends around the true or placebo reform date, with p-values of this trend presented in the Table footer. Standard errors clustered by country are displayed in parentheses. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table B5: Mechanism variables and maternal mortality

	(1) ln(MMR)	(2) ln(MMR)	(3) ln(MMR)	(4) ln(MMR)	(5) ln(MMR)	(6) ln(MMR)	(7) ln(MMR)	(8) ln(MMR)	(9) ln(MMR)	(10) ln(MMR)
Antenatal Care	-0.005***									-0.005
	[0.002]									[0.003]
Attended Births		-0.004**								-0.003
		[0.002]								[0.002]
Modern Contraceptives			-0.004							-0.008
			[0.004]							[0.007]
Fertility Rates				0.095**						-0.093
				[0.047]						[0.106]
Teen Pregnancy					0.003					0.003
					[0.002]					[0.004]
Birth Spacing						-0.007				-0.003
						[0.005]				[0.003]
Health Expenditure							0.011			-0.018
							[0.010]			[0.011]
DAH Maternal Health								0.086		0.109
								[0.062]		[0.078]
log(GDP p.c.)									-0.338***	-0.478***
									[0.061]	[0.144]
Observations	2,109	2,751	4,182	4,303	4,309	1,429	3,178	3,338	4,186	915
R-Squared	0.989	0.987	0.987	0.987	0.987	0.968	0.991	0.978	0.989	0.986

Each column displays a regression of ln(MMR) on country and year FEs and a particular measure considered as a potential explanation of the observed impacts of quotas on maternal mortality. These are regressed column by column in columns 1-9, and jointly in column 10. Standard errors clustered by country are displayed in parentheses. * p<0.10; ** p<0.05; *** p<0.01.

Table B6: Women's rights and maternal mortality

	(1) ln(MMR)	(2) ln(MMR)	(3) ln(MMR)	(4) ln(MMR)	(5) ln(MMR)	(6) ln(MMR)	(7) ln(MMR)	(8) ln(MMR)
World Bank Women, Business & Law Index	0.000							-0.000
	[0.002]							[0.004]
CIRI Women's Economic Rights		0.009						0.025
		[0.014]						[0.033]
CIRI Women's Political Rights			-0.010					0.039
			[0.020]					[0.037]
CIRI Women's Social Rights				-0.002				0.021
				[0.016]				[0.051]
Female Labour Force Participation					0.002			0.004
					[0.005]			[0.008]
Proportion of Women Ministers						0.002**		-0.000
						[0.001]		[0.001]
Proportion of Women's Protests							-0.002	0.060
							[0.025]	[0.045]
Observations	4,257	3,322	3,335	2,234	4,234	1,444	4,138	355
R-Squared	0.987	0.990	0.990	0.993	0.987	0.989	0.987	0.997

Each column displays a regression of ln(MMR) on country and year FEs and a particular measure of women's rights or women's standing in society. These are regressed column by column in columns 1-7, and jointly in column 8. Standard errors clustered by country are displayed in parentheses. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table B7: Women's empowerment, rights and maternal mortality

	(1) ln(MMR)	(2) ln(MMR)	(3) ln(MMR)	(4) ln(MMR)	(5) ln(MMR)	(6) ln(MMR)	(7) ln(MMR)	(8) ln(MMR)	(9) ln(MMR)	(10) ln(MMR)
Women Civil Liberty Index	-0.227									-0.058
	[0.141]									[0.225]
Women Political Particip. Index		-0.064								-0.145
		[0.093]								[0.118]
Exclusion by Gender Index			0.266							0.226
			[0.287]							[0.325]
Relative Freedom of Movement Women				-0.016*						-0.016*
				[0.009]						[0.008]
Relative Access to Justice Women					0.001					0.000
					[0.009]					[0.009]
Relative Freedom of Discussion						0.007				0.008
						[0.008]				[0.009]
Power Distributed by Gender							0.023			0.068*
							[0.033]			[0.036]
Freedom from Forced Labour Women								-0.091***		-0.088*
								[0.034]		[0.049]
Property Rights for Women									-0.009	0.034
									[0.035]	[0.045]
Observations	4,104	4,104	4,094	4,012	4,014	3,998	4,014	4,014	4,014	3,963
R-Squared	0.988	0.988	0.988	0.988	0.988	0.988	0.988	0.988	0.988	0.988

Each column displays a regression of ln(MMR) on country and year FEs and a particular measure of women's empowerment or women's rights. These are regressed column by column in columns 1-9, and jointly in column 10. Standard errors clustered by country are displayed in parentheses. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table B8: Potential predictors of quota adoption and maternal mortality

	(1) ln(MMR)	(2) ln(MMR)	(3) ln(MMR)	(4) ln(MMR)	(5) ln(MMR)	(6) ln(MMR)	(7) ln(MMR)	(8) ln(MMR)	(9) ln(MMR)
Development Assistance p.c.	-0.000								-0.000
	[0.000]								[0.000]
Peace Keeper Presence		0.000							0.000
		[0.000]							[0.000]
Right Wing Executive			-0.004						0.027
			[0.022]						[0.034]
Left Wing Executive				0.023					0.043
				[0.020]					[0.031]
Years Exectuve in Power					-0.003				-0.003
					[0.002]				[0.002]
Herfindahl Index (parties)						-0.043			-0.036
						[0.048]			[0.049]
Opposition Vote Share							-0.001**		-0.001*
							[0.000]		[0.001]
Transitioning Regime								0.025	0.005
								[0.018]	[0.021]
Observations	4,335	4,335	3,683	3,683	3,619	3,432	3,684	4,335	3,420
R-Squared	0.987	0.987	0.988	0.988	0.988	0.989	0.988	0.987	0.989

Each column displays a regression of ln(MMR) on country and year FEs and a particular measure flagged as a predictor of quota adoption in the political science literature. These are regressed column by column in columns 1-8, and jointly in column 9. Standard errors clustered by country are displayed in parentheses. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table B9: Event study results with alternative time windows (ln(MMR))

	Lag/Lead Time Period Only			Accumulating Final Periods		
	(1)	(2)	(3)	(4)	(5)	(6)
10 Years Prior to Adoption	0.016			-0.010		
	[0.041]			[0.058]		
9 Years Prior to Adoption	0.027			0.032		
	[0.040]			[0.041]		
8 Years Prior to Adoption	0.034	0.035		0.039	0.002	
	[0.036]	[0.035]		[0.037]	[0.049]	
7 Years Prior to Adoption	0.022	0.024		0.023	0.023	
	[0.031]	[0.031]		[0.032]	[0.032]	
6 Years Prior to Adoption	0.020	0.022		0.023	0.023	
	[0.029]	[0.029]		[0.030]	[0.030]	
5 Years Prior to Adoption	0.016	0.018	0.022	0.017	0.017	0.007
	[0.024]	[0.023]	[0.023]	[0.024]	[0.024]	[0.039]
4 Years Prior to Adoption	0.011	0.013	0.016	0.011	0.011	0.011
	[0.018]	[0.017]	[0.016]	[0.019]	[0.019]	[0.019]
3 Years Prior to Adoption	0.016	0.018	0.020	0.014	0.014	0.014
	[0.014]	[0.014]	[0.013]	[0.016]	[0.016]	[0.016]
2 Years Prior to Adoption	0.004	0.005	0.007	0.002	0.002	0.002
	[0.011]	[0.010]	[0.009]	[0.012]	[0.012]	[0.012]
Year of Quota Adoption	-0.011	-0.011	-0.010	-0.013	-0.013	-0.013
	[0.008]	[0.008]	[0.007]	[0.008]	[0.008]	[0.008]
1 Year Following Adoption	-0.021	-0.020	-0.019	-0.027	-0.027	-0.027
	[0.019]	[0.018]	[0.016]	[0.019]	[0.019]	[0.019]
2 Years Following Adoption	-0.030	-0.029	-0.027	-0.036	-0.036	-0.035
	[0.027]	[0.025]	[0.024]	[0.026]	[0.025]	[0.025]
3 Years Following Adoption	-0.040	-0.039	-0.039	-0.049*	-0.049*	-0.048*
	[0.029]	[0.028]	[0.026]	[0.028]	[0.028]	[0.028]
4 Years Following Adoption	-0.058*	-0.056*	-0.054*	-0.074**	-0.075**	-0.075**
	[0.034]	[0.033]	[0.031]	[0.033]	[0.033]	[0.033]
5 Years Following Adoption	-0.068*	-0.066*	-0.066*	-0.085**	-0.085**	-0.119**
	[0.040]	[0.039]	[0.037]	[0.039]	[0.039]	[0.055]
6 Years Following Adoption	-0.079*	-0.076		-0.103**	-0.104**	
	[0.047]	[0.046]		[0.047]	[0.047]	
7 Years Following Adoption	-0.088*	-0.085*		-0.112**	-0.113**	
	[0.052]	[0.051]		[0.052]	[0.052]	
8 Years Following Adoption	-0.095*	-0.094*		-0.119**	-0.132**	
	[0.057]	[0.056]		[0.058]	[0.063]	
9 Years Following Adoption	-0.095			-0.116*		
	[0.062]			[0.063]		
10 Years Following Adoption	-0.102			-0.139**		
	[0.068]			[0.067]		
Mean of Dep. Var.	4.305	4.284	4.251	4.357	4.357	4.357
Observations	4187	4126	4025	4335	4335	4335
R-Squared	0.535	0.529	0.523	0.548	0.548	0.548

Coefficients and standard errors are displayed for lags and leads to the adoption of quotas. These follow the style of event studies displayed in Figure 2, however varying time periods considered. Columns 1-3 consider only observations within 10/8/5 years of quota adoption for quota countries, while columns 4-6 consider all observations, accumulating so that the final lag and lead refers to greater than or equal to this length from the policy adoption. All other details follow Figure 2. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table B10: Event study results with alternative time windows (women in parliament)

	Lag/Lead Time Period Only			Accumulating Final Periods		
	(1)	(2)	(3)	(4)	(5)	(6)
10 Years Prior to Adoption	1.535			2.939**		
	[1.430]			[1.465]		
9 Years Prior to Adoption	-0.215			-0.423		
	[1.873]			[1.955]		
8 Years Prior to Adoption	-0.161	-0.133		-0.343	2.110	
	[1.680]	[1.664]		[1.764]	[1.521]	
7 Years Prior to Adoption	-0.082	-0.056		-0.209	-0.199	
	[1.568]	[1.550]		[1.636]	[1.633]	
6 Years Prior to Adoption	-0.220	-0.195		-0.382	-0.377	
	[1.332]	[1.323]		[1.401]	[1.399]	
5 Years Prior to Adoption	-0.027	-0.004	-0.026	-0.121	-0.117	1.361
	[1.142]	[1.130]	[1.110]	[1.208]	[1.204]	[1.446]
4 Years Prior to Adoption	-0.783	-0.774	-0.790	-0.880	-0.872	-0.872
	[1.122]	[1.105]	[1.088]	[1.191]	[1.187]	[1.191]
3 Years Prior to Adoption	-0.300	-0.330	-0.391	-0.251	-0.246	-0.248
	[0.711]	[0.714]	[0.736]	[0.697]	[0.694]	[0.693]
2 Years Prior to Adoption	0.464	0.470	0.450	0.466	0.466	0.466
	[0.576]	[0.573]	[0.589]	[0.583]	[0.580]	[0.581]
Year of Quota Adoption	4.768***	4.763***	4.716***	4.826***	4.832***	4.836***
	[1.786]	[1.787]	[1.783]	[1.798]	[1.800]	[1.797]
1 Year Following Adoption	5.816***	5.788***	5.712***	5.812***	5.811***	5.817***
	[1.702]	[1.694]	[1.683]	[1.717]	[1.721]	[1.718]
2 Years Following Adoption	6.123***	6.068***	5.993***	6.109***	6.104***	6.113***
	[1.839]	[1.835]	[1.829]	[1.845]	[1.844]	[1.839]
3 Years Following Adoption	6.393***	6.307***	6.060***	6.498***	6.511***	6.547***
	[1.926]	[1.909]	[1.863]	[1.964]	[1.966]	[1.966]
4 Years Following Adoption	6.056***	6.001***	5.901***	6.165***	6.210***	6.276***
	[1.756]	[1.732]	[1.698]	[1.805]	[1.820]	[1.825]
5 Years Following Adoption	7.605***	7.555***	7.446***	7.740***	7.777***	7.519***
	[1.880]	[1.845]	[1.785]	[1.904]	[1.920]	[2.259]
6 Years Following Adoption	7.313***	7.270***		7.435***	7.484***	
	[1.940]	[1.898]		[1.982]	[2.001]	
7 Years Following Adoption	7.469***	7.423***		7.596***	7.643***	
	[1.946]	[1.905]		[1.986]	[2.004]	
8 Years Following Adoption	6.225***	6.242***		6.330***	7.323***	
	[2.221]	[2.172]		[2.271]	[2.540]	
9 Years Following Adoption	6.425***			6.619***		
	[2.271]			[2.325]		
10 Years Following Adoption	6.589**			7.708***		
	[2.658]			[2.781]		
Mean of Dep. Var.	14.144	14.149	14.153	14.110	14.110	14.110
Observations	4187	4126	4025	4335	4335	4335
R-Squared	0.459	0.453	0.445	0.470	0.468	0.467

Refer to notes to Table B9. All details follow this Table, however now with women in parliament as the dependent variable rather than the log of maternal mortality. * p<0.10; ** p<0.05; *** p<0.01.

Table B11: Gender quotas: DD impacts on women in parliament and maternal mortality (5 year pre/post sample)

	% Women in Parliament			ln(Maternal Mortality Ratio)			Maternal Mortality Ratio		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Five year pre-post sample, no controls									
Reserved Seats	6.040***	6.522***	8.514***	-0.046*	-0.041	-0.054	-56.135**	-61.006**	-56.606***
	[1.705]	[1.728]	[2.340]	[0.028]	[0.028]	[0.036]	[23.932]	[24.843]	[13.314]
Mean of Dep. Var.	14.153	14.169	14.169	4.251	4.242	4.242	218.745	218.432	218.432
Observations	4025	3988	3988	4025	3988	3988	4025	3988	3988
Number of Countries	178	176	176	178	176	176	178	176	176
R-Squared	0.445	0.447	0.505	0.523	0.519	0.525	0.219	0.216	0.221
Panel B: Five year pre-post sample, time-varying controls									
Reserved Seats	4.974***	5.583***	6.630***	-0.064**	-0.068**	-0.100**	-66.236**	-78.034**	-74.920***
	[1.878]	[1.930]	[2.045]	[0.030]	[0.032]	[0.045]	[32.125]	[32.469]	[15.220]
Mean of Dep. Var.	13.761	13.776	13.776	4.285	4.275	4.275	232.934	232.637	232.637
Observations	2981	2947	2947	2981	2947	2947	2981	2947	2947
Number of Countries	156	154	154	156	154	154	156	154	154
R-Squared	0.465	0.469	0.559	0.576	0.570	0.595	0.363	0.360	0.416
Population Weights	N	N	Y	N	N	Y	N	N	Y

Refer to notes to Table A2. Identical specifications are estimated, however removing from the sample all treated observations outside of a temporal window 5 years preceding and 5 years post-quota adoption. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table B12: Examining impacts on maternal mortality removing outliers

	Main	Winsorizing				Removing			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		500	90%	95%	99%	500	90%	95%	99%
Reserved Seats	-106.107**	-33.339**	-58.942***	-83.957***	-111.504***	-50.364***	-57.398***	-60.595***	-105.547**
	[43.036]	[14.065]	[17.676]	[25.159]	[42.685]	[16.849]	[18.094]	[17.587]	[42.612]
Observations	4,335	4,335	4,335	4,335	4,335	3,526	3,901	4,117	4,290
R-Squared	0.27	0.28	0.30	0.32	0.30	0.29	0.29	0.29	0.29
Mean of Dep. Var.	233.4	177.4	206.2	218.2	229.3	103.4	150.5	183.5	218.0
Baseline Mean	448.1	337.9	398.0	424.1	447.9	223.0	323.1	362.5	441.7
Implied Percentage	-23.7	-9.9	-14.8	-19.8	-24.9	-22.6	-17.8	-16.7	-23.9

Note: Column 1 estimates a standard two-way FE model regressing MMR in levels on a reserved seat indicator along with country and year fixed effects. Columns 2-5 Winsorize MMR at the level or percentile indicated in column headings, and columns 6-9 remove values for MMR above the level or percentile indicated in column headings, in each case re-estimating the model from column 1. A formal test of equality of coefficients between columns (1) and (2) based on seemingly unrelated regression is rejected with $p < 0.01$. Mean of Dep. Var. presents the mean of the transformed dependent variable, Baseline Mean presents the mean of the transformed dependent variable in quota countries prior to quota implementation, and Implied Percentage expresses the estimated reduction in MMR as a percent of the Baseline Mean. Standard errors clustered by country are displayed in parentheses. * p<0.10; ** p<0.05; *** p<0.01.

Table B13: Two-way fixed effect models: reserved seats and maternal mortality in India

	ln(MMR)		MMR	
	(1)	(2)	(3)	(4)
Reserved Seats for Women	-0.142*	-0.204*	-107.839***	-105.096***
	(0.080)	(0.115)	(18.042)	(22.944)
Observations	135	135	135	135
R-Squared	0.95	0.92	0.91	0.88
Population Weights		Y		Y

Each column documents coefficients from two-way fixed effect models where the natural logarithm of the maternal mortality ratio (columns 1-2) or the maternal mortality ratio (columns 3-4) is regressed on an indicator for the state's reserved seat status, plus full state and time fixed effects. $^{***}p < 0.01$;** $p < 0.05$;* $p < 0.10$.

Table B14: Univariate regressions of ln(MMR) on mechanism Variables

Variable	Point Estimate	Standard Error
Antenatal Care	-0.040	0.005
Attended Births	-0.044	0.002
Modern Contraceptives	-0.063	0.004
log(Fertility Rates)	0.847	0.032
Teen Pregnancy	0.028	0.001
Women's Schooling	-0.444	0.031
Health Expenditure	-0.265	0.048
DAH Maternal Health	1.408	0.389
log(GDP p.c.)	-1.181	0.042
Birth Spacing	-0.013	0.008

Each line presents the point estimate and standard error from a projection of ln(MMR) on the variable indicated. Standard errors are clustered by country, but no additional controls are included.

Table B15: Mechanisms: impacts of gender quotas on intermediate outcomes (part 1)

	Antenatal Care			Attended Births			Modern Contraceptive Usage		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full sample, no controls									
Reserved Seats	4.699*	6.551**	12.107***	5.793**	7.394***	6.802**	1.669	1.957	2.406*
	[2.771]	[2.810]	[3.059]	[2.434]	[2.482]	[2.956]	[1.172]	[1.195]	[1.263]
Mean of Dep. Var.	84.210	84.229	84.229	83.726	83.679	83.679	29.913	29.673	29.673
Observations	678	651	651	1237	1210	1210	4182	4131	4131
Number of Countries	155	153	153	169	167	167	172	170	170
R-Squared	0.432	0.431	0.674	0.306	0.307	0.501	0.599	0.599	0.627
Panel B: Full sample, time-varying controls									
Reserved Seats	3.621	7.557**	10.850***	4.968	7.849***	7.479***	1.459	1.738	2.364**
	[3.823]	[3.185]	[2.312]	[3.089]	[2.771]	[2.447]	[1.201]	[1.242]	[1.125]
Mean of Dep. Var.	82.673	82.641	82.641	83.024	82.964	82.964	30.140	29.869	29.869
Observations	526	500	500	996	970	970	3166	3121	3121
Number of Countries	134	132	132	149	147	147	154	152	152
R-Squared	0.502	0.519	0.731	0.353	0.362	0.685	0.610	0.609	0.634
Population Weights	N	N	Y	N	N	Y	N	N	Y

Two-way FE models of intermediate outcomes as a function of the passage of gender quotas are displayed. In each case, in panel A no time-varying controls are included, and in panel B we control for the log of PPP adjusted GDP per capita, and a democracy score. Antenatal care coverage and birth attendance are newly harmonized data available for 1990-2015, however only available in a sub-sample of years for each particular country. Modern contraceptive coverage refers to the proportion of all women aged 15–49 using modern contraceptives. Unweighted, and population weighted specifications are displayed. Within each outcome group the first column includes all observations and does not weight by population, the second column removes India and China without weighting, and the third column weights by population. When weighting, China and India are removed from the estimation sample, to avoid regression results being largely driven by these two countries with a population an order of magnitude larger than other countries. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table B16: Mechanisms: impacts of gender quotas on intermediate outcomes (part 2)

	ln(Fertility Rate)			Teenage Pregnancy			Birth Spacing		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full sample, no controls									
Reserved Seats	-0.061**	-0.071**	-0.161***	-1.609	-2.387	-11.156**	1.896	1.985	2.414
	[0.028]	[0.028]	[0.050]	[2.786]	[2.876]	[4.724]	[1.818]	[1.828]	[3.272]
Mean of Dep. Var.	1.040	1.043	1.043	62.366	62.693	62.693	35.491	35.566	35.566
Observations	4303	4252	4252	4309	4258	4258	1429	1403	1403
Number of Countries	177	175	175	177	175	175	67	66	66
R-Squared	0.504	0.503	0.467	0.541	0.543	0.561	0.555	0.552	0.616
Panel B: Full sample, time-varying controls									
Reserved Seats	-0.073**	-0.082***	-0.156***	-2.756	-3.566	-11.372**	2.123	2.212	2.236
	[0.029]	[0.030]	[0.047]	[2.755]	[2.870]	[4.508]	[1.880]	[1.896]	[3.129]
Mean of Dep. Var.	1.041	1.043	1.043	64.731	65.114	65.114	36.281	36.338	36.338
Observations	3212	3167	3167	3212	3167	3167	1196	1173	1173
Number of Countries	156	154	154	156	154	154	63	62	62
R-Squared	0.506	0.505	0.509	0.535	0.536	0.571	0.483	0.484	0.504
Population Weights	N	N	Y	N	N	Y	N	N	Y

Refer to notes to Table B15. Alternative regressions are shown, however now for the outcomes of total fertility rate, teen age pregnancies per 1,000 women, and birth spacing in months estimated from DHS fertility pseudo-panels. Full notes are available in Figure 8. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table B17: Mechanisms: impacts of gender quotas on intermediate outcomes (part 3)

	Health Expenditure			Development Assistance			ln(GDP per capita)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full sample, no controls									
Reserved Seats	0.894**	0.958**	-0.147	-0.009	-0.010	-0.101**	-0.005	-0.052	0.034
	[0.371]	[0.389]	[0.401]	[0.027]	[0.028]	[0.041]	[0.062]	[0.043]	[0.056]
Mean of Dep. Var.	6.235	6.257	6.257	0.089	0.089	0.089	8.903	8.912	8.912
Observations	3178	3140	3140	3338	3287	3287	4186	4135	4135
Number of Countries	176	174	174	147	145	145	175	173	173
R-Squared	0.192	0.194	0.400	0.098	0.098	0.189	0.472	0.471	0.650
Panel B: Full sample, time-varying controls									
Reserved Seats	0.867**	0.895**	-0.201	-0.022	-0.025	-0.101***	0.019	-0.026	0.032
	[0.415]	[0.448]	[0.397]	[0.030]	[0.032]	[0.038]	[0.061]	[0.043]	[0.051]
Mean of Dep. Var.	6.132	6.157	6.157	0.095	0.095	0.095	8.899	8.910	8.910
Observations	2586	2550	2550	2445	2400	2400	3212	3167	3167
Number of Countries	155	153	153	127	125	125	156	154	154
R-Squared	0.234	0.235	0.451	0.125	0.125	0.238	0.458	0.453	0.668
Population Weights	N	N	Y	N	N	Y	N	N	Y

Refer to notes to Table B15. Alternative regressions are shown, however now for the outcomes of health expenditure as a percent of GDP, the proportion of development assistance earmarked for maternal health, and the log of GDP per capita. In the case of log(GDP per capita), when including controls, only democracy fixed effects are included. Full notes are available in Figure 8. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table B18: Gender quotas: DD impacts on female and male education

	Female Education			Male Education			Female/Male Education		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full sample, no controls									
Reserved Seats	0.470*	0.468*	1.204***	0.144	0.105	0.477*	0.065**	0.072**	0.134***
	[0.250]	[0.264]	[0.460]	[0.220]	[0.231]	[0.271]	[0.030]	[0.031]	[0.027]
Mean of Dep. Var.	7.471	7.473	7.473	7.565	7.561	7.561	0.985	0.986	0.986
Observations	2794	2753	2753	2446	2405	2405	2446	2405	2405
Number of Countries	141	139	139	123	121	121	123	121	121
R-Squared	0.333	0.325	0.398	0.214	0.205	0.289	0.161	0.160	0.360
Panel B: Full sample, time-varying controls									
Reserved Seats	0.460	0.466	1.315***	0.148	0.112	0.533**	0.064*	0.073**	0.141***
	[0.284]	[0.305]	[0.462]	[0.241]	[0.259]	[0.266]	[0.033]	[0.036]	[0.026]
Mean of Dep. Var.	7.477	7.479	7.479	7.542	7.536	7.536	0.986	0.988	0.988
Observations	2511	2470	2470	2188	2147	2147	2188	2147	2147
Number of Countries	132	130	130	114	112	112	114	112	112
R-Squared	0.348	0.337	0.442	0.238	0.224	0.357	0.201	0.203	0.407
Population Weights	N	N	Y	N	N	Y	N	N	Y

Each column and panel presents estimates from a two-way FE specification following that in Table A2, with identical controls and fixed effects. We now examine estimates for impacts of gender quotas on female and male education, as well as their ratio. These data are drawn from Barro and Lee's education database, recording completed years of schooling of 15–19 year-olds in each country and are interpolated between quinquennial time period. Further notes are available in Table A2. Standard errors clustered by country are displayed in parentheses. * $p<0.10$; ** $p<0.05$; *** $p<0.01$.

Table B19: A back of the envelope calculation of impacts on total maternal deaths

Baseline		$\Delta MMR \| Fertility$	
Births = 34,735,750 Deaths = 92,928	MMR=267.5	Births = 34,735,750 Deaths = **84,843**	MMR=244.2
$\Delta Fertility \| MMR$		$\Delta Fertility, \Delta MMR$	
Births = 32,616,869 Deaths = **87,259**	MMR=267.5	Births = 32,616,869 Deaths = **79,668**	MMR=244.2

The top left quadrant calculates maternal mortality ratios based on actual births and maternal deaths in all quota adopting countries one year prior to the quota. The top right quadrant calculates total maternal deaths if the maternal mortality ratio declines by 8.7%, but births remain fixed. The bottom left quadrant calculates total maternal deaths if the fertility rate falls by 6.1%, but the maternal mortality remains fixed at pre-quota levels. Finally, the bottom right quadrant calculates the total reduction in expected maternal deaths given (a) the fall in fertility and (b) the fall in the maternal mortality ratio.

Table B20: Gender quotas: DD impacts on TB mortality and adult mortality

	inv sin(TB Mortality)			ln(Male Mortality)			ln(Female Mortality)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full sample, no controls									
Reserved Seats	0.118	0.159	0.040	-0.035	-0.033	-0.029	-0.038	-0.038	-0.091*
	[0.110]	[0.109]	[0.070]	[0.051]	[0.054]	[0.054]	[0.052]	[0.055]	[0.051]
Mean of Dep. Var.	2.622	2.612	2.612	5.360	5.363	5.363	4.906	4.907	4.907
Observations	4147	4098	4098	4249	4198	4198	4249	4198	4198
Number of Countries	178	176	176	177	175	175	177	175	175
R-Squared	0.356	0.353	0.492	0.528	0.525	0.553	0.559	0.555	0.606
Panel B: Full sample, time-varying controls									
Reserved Seats	0.088	0.122	0.018	-0.025	-0.024	-0.010	-0.042	-0.045	-0.079*
	[0.119]	[0.121]	[0.077]	[0.057]	[0.061]	[0.055]	[0.058]	[0.062]	[0.045]
Mean of Dep. Var.	2.691	2.680	2.680	5.391	5.394	5.394	4.925	4.926	4.926
Observations	3212	3167	3167	3208	3163	3163	3208	3163	3163
Number of Countries	156	154	154	156	154	154	156	154	154
R-Squared	0.410	0.404	0.544	0.496	0.491	0.588	0.490	0.484	0.622
Population Weights	N	N	Y	N	N	Y	N	N	Y

Difference in difference specification following that in Table A2, with identical controls and fixed effects. We now examine estimates for impacts of gender quotas on tuberculosis and male and female mortality rates. Tuberculosis is measured as incidence per 100,000 people, and mortality is measured as per 1,000 adults of ages 15-60. Since the TB data had the occasional zero, we use the inverse hyperbolic sine transformation rather than the log transformation for this outcome. Further notes are available in Table A2. Standard errors clustered by country are displayed in parentheses. * p<0.10; ** p<0.05; *** p<0.01.

Table B21: Gender quotas: DD impacts infant mortality

	ln(IMR)			ln(Female IMR)			ln(Male IMR)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full sample, no controls									
Reserved Seats	-0.091	-0.091	-0.014	-0.087	-0.088	0.007	-0.105	-0.105	-0.054
	[0.131]	[0.131]	[0.146]	[0.112]	[0.112]	[0.147]	[0.119]	[0.119]	[0.137]
Mean of Dep. Var.	4.345	4.344	4.344	4.259	4.257	4.257	4.414	4.414	4.414
Observations	1096	1079	1079	1088	1071	1071	1093	1076	1076
Number of Countries	68	67	67	68	67	67	68	67	67
R-Squared	0.396	0.394	0.496	0.398	0.396	0.513	0.361	0.359	0.394
Panel B: Full sample, time-varying controls									
Reserved Seats	-0.098	-0.100	-0.038	-0.122	-0.125	-0.025	-0.100	-0.103	-0.081
	[0.132]	[0.132]	[0.099]	[0.108]	[0.108]	[0.093]	[0.121]	[0.121]	[0.108]
Mean of Dep. Var.	4.337	4.336	4.336	4.253	4.251	4.251	4.403	4.403	4.403
Observations	989	972	972	981	964	964	987	970	970
Number of Countries	65	64	64	65	64	64	65	64	64
R-Squared	0.443	0.442	0.575	0.437	0.436	0.562	0.416	0.417	0.467
Population Weights	N	N	Y	N	N	Y	N	N	Y

Difference in difference specification following that in Table A2, with identical controls and fixed effects. We now examine estimates for impacts of gender quotas on the natural logarithm of infant mortality, where total infant mortality and infant mortality by sex is estimated from all publicly available DHS waves. Further notes are available in Table A2. Standard errors clustered by country are displayed in parentheses. * p<0.10; ** p<0.05; *** p<0.01.

A Data Appendix

The following table provides an exhaustive list of the variables used in the paper along with the original sources.

Variable (Source)	Description
Maternal Mortality, Cross Country (MMEIG)	We used recently released estimates of the maternal mortality ratio (MMR) per 100,000 live births produced by the Maternal Mortality Estimation Inter-Agency Group (MMEIG) and published in the World Bank World Development Indicators (WDI, indicator SH.STA.MMRT). These data were made available for the first time in the year 2016 and before that there were no reliable annual cross-country data on MMR. These estimates were available for 183 countries annually for the period 1990–2015. Maternal mortality is identified using ICD-10 codes O00-O99 (Pregnancy, childbirth and puerperium); the official definition is "the number of women who die from pregnancy-related causes while pregnant or within 42 days of pregnancy termination per 100,000 live births." These are widely considered the best MMR measures to date, as they address known measurement difficulties in survey and vital statistics data on maternal mortality using Bayesian methods applied to multiple, complementary data sources including vital statistics, special inquiries, surveillance sites, population-based household surveys and census files (Alkema et al., 2016, 2017). The world distribution of average MMR for the period of 1990–2015 is in Figure B2.
Maternal Mortality, Indian states (SRS)	State-level estimates of maternal mortality in India are gleaned from the Office of the Registrar General & Census Commissioner's Sample Registration System (SRS) Bulletins (RGI, 2006). These data are available for the 15 large states – Andhra Pradesh, Assam, Bihar (including Jharkhand), Gujarat, Haryana, Karnataka, Kerala, Madhya Pradesh (including Chattisgarh), Maharashtra, Odisha, Punjab, Rajasthan, Tamil Nadu, Uttar Pradesh (Uttarakhand), and West Bengal – for the period of 1997–2003. These are complemented by an aggregate (state-level) estimate from 1986, reported in Ram et al. (2006) covering the same large states.
Ethnic Compostion (HIEF)	Ethnic composition is measured by Ethnic Fractionalization (EF Index), which is the probability that two individuals who are chosen at random from a country belong to two different ethnic groups. Time-varying data on EF come from Historical Index of Ethnic Fractionalization Dataset (HIEF) V 2.0 (Dražanová, 2020), downloaded from the Harvard Dataverse (https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/4JQRCL). The dataset is available for the years 1945–2013.
Political Gender Quota Data, Cross-Country (IDEA)	We collated measures for each country of whether the country has a legislated and binding reserved seat quota for women, its year of implementation, and the size of the quota measured as number of seats divided by all seats in the uni- or bi-cameral chamber. To create the database, we started with measures provided by Dahlerup (2005) and completed the most recent years from Global Database of Quotas for Women database (available online at quotaproject.org), which is a repository developed and maintained by the International Institute for Democracy and Electoral Assistance (IDEA), the Inter-Parliamentary Union, and Stockholm University.
Political Gender Quota Data, India (Iyer et al. (2012))	State-level data on the reservation of seats for women in local councils come from Iyer et al. (2012) who provide a time-varying state-level indicator for when women were given political representation. "[T]he indicator equals one in the years following the first local government election that implemented the "not less than one-third" reservation scheme for women representatives" (Iyer et al., 2012) .
Women in Parliament Data (WDI, MDG, Paxton et al. (2008))	We used three distinct annual-level measures of women in parliament to construct a comprehensive panel of the percentage of women occupying seats in the national parliament. These were the WDI indicator SG.GEN.PARL.ZS ("Proportion of seats held by women in national parliaments (%)"), The UN Millennium Development Goals (MDG) Indicators ("Seats held by women in national parliament, percentage"), and the Inter university Consortium for Political and Social Research (ICPSR) dataset compiled by (Paxton et al., 2008) ("Women in Parliament, 1945–2003: Cross-National Dataset"). The first two of these datasets had partially-complete coverage for the years 1990, and then 1997–2015, while the latter had partially-complete yearly coverage for each year starting in 1945, and ending in 2003. In order to construct as comprehensive a series as possible, we began with the WDI data, and then imputed missing years where available from the MDG indicators, and Paxton et al. (2008) data. When a missing WDI year was available in both the MDG and the ICPSR dataset, we favored the MDG measure, which was estimated using the same sample and year. Figures B5 and B4 present the distribution of the proportion of women in parliament pre- and post-quota implementation in quota countries, as well as the full distribution of the proportion of women in parliament over the period under study.

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Variable (Source)	Description
Health Expenditure (WHO)	Health expenditure at the country-year level was taken from the World Health Organization the National Health Accounts (NHA) data series. These provide a measure of total health expenditure as a percent of GDP, and are available for the years 1995–2013.
Development assistance for health (IHME)	Development assistance for health (DAH) data are based on the Institute for Health Metrics and Evaluation (IHME) Development Assistance for Health Database (1990–2017). These data are available at the source country $\times$ receiver country $\times$ year level. We compute the proportion Development Assistance for Health to Maternal Health as: $\frac{\text{Development Assistance for Health to Maternal Health - All Program Areas (constant 2017 US dollars)}}{\text{Development Assistance for Health disbursed from all channels (constant 2017 US dollars)}}$.
Women's Protests (Bell et al. (2019))	We compute the proportion of women's protests using country-level panel data on women's protests and total protests from Bell et al. (2019). (We are grateful to the Sam Bell for sharing the data with us.)
Contraception Prevalence (UN)	This variable measures the percentage of Women of reproductive age (15-49 years) who are currently using any modern method of contraception. The annual data are from the "ESTIMATES AND PROJECTIONS OF FAMILY PLANNING INDICATORS 2020" dataset generated by the UN Department of Economics and Social Affairs, Population Division. Downloaded from https://www.un.org/development/desa/pd/data/family-planning-indicators on 03/02/2021
Adult Male & Female Mortality (World Bank)	Data on male/female mortality for adults are available in the World Bank Data Bank (indicators SP.DYN.AMRT.MA and SP.DYN.AMRT.FE), based on measures from the United Nations Population Division, World Population Prospect and University of California, Berkeley, and Max Planck Institute for Demographic Research. This is measured as mortality between the ages of 15–60, per 1,000 male adults, and captures the likelihood that a male/female of age 15 dies by the age of 60.
Tuberculosis Mortality (WHO)	This is measured as the number of deaths due to Tuberculosis among HIV negative people, and is measured per 100,000 population. The data are from the WHO and were downloaded from: http://apps.who.int/gho/data/view.main.57020ALL?lang=en, accessed on 17/03/2016.
Women's Rights (CIRI)	Measures of women's political, economic, and social rights data are based on the Cingranelli et al. (2013) data set available from http://www.humanrightsdata.com/. We use the three following variables: - *Women's Political Rights*: takes into account women's rights to vote, to run for political office, to hold elected and appointed government positions, to join political parties, and to petition government officials. This variable takes discrete values between 0 and 3, with 3 representing high rights and 0 representing low rights. This variable is available for the period of 1981–2011 for approximately 139 (in 1990) to 195 (in 2011) countries. - *Women's Economic Rights*: takes into account women's rights to equal pay for equal work, free choice of profession or employment without the need to obtain a husband or male relative's consent, the right to gainful employment without the need to obtain a husband or male relative's consent, equality in hiring and promotion practices, job security (maternity leave, unemployment benefits, no arbitrary firing or layoffs, etc.), non-discrimination by employers, be free from sexual harassment in the workplace, to work at night, to work in occupations classified as dangerous, to work in the military and the police force. This variable is available for the period of 1981–2011 for approximately 139 (in 1990) to 195 (in 2011) countries. - *Women's Social Rights*: takes into account women's rights to equal inheritance; enter into marriage on a basis of equality with men; travel abroad; obtain a passport; confer citizenship to children or a husband; initiate a divorce; own, acquire, manage, and retain property brought into marriage; participate in social, cultural, and community activities; education; and freedoms to choose a residence/domicile, and from female genital mutilation of children and of adults without their consent, and from forced sterilization. This variable is available for the period of 1981–2007 for approximately 139 (in 1990) to 193 (in 2007) countries.
Women's Rights (VDEM)	Measures of women's rights and measures of relative social standing are based on data collected in the Variety of Democracy dataset Version 10 (Coppedge et al., 2020; Pemstein et al., 2020) – VDEM-10 – downloaded from https://www.v-dem.net/en/data/data/v-dem-dataset/ on the 27/10/2020. When generating ratios of women to male outcomes 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. We specifically use the following variables:

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Variable (Source)	Description
	- *Women's Civil Liberties Index ($v2x\_gencl$)*: "The index is formed by taking the point estimates from a Bayesian factor analysis model of the indicators for freedom of domestic movement for women ($v2cldmovew$), freedom from forced labor for women ($v2clslavef$), property rights for women ($v2clprptyw$), and access to justice for women ($v2clacjstw$)" (Coppedge et al., 2020). - *Women's Political Participation Index ($v2x\_genpp$)*: "The index is formed by taking the average of the indicators for lower chamber female legislators ($v2lgfemleg$, standardized) and power distributed by gender ($v2pepwrgen$)" (Coppedge et al., 2020). - *Exclusion by Gender Index ($v2xpe\_exlgender$)*: "The index is formed by taking the point estimates from a Bayesian factor analysis model of the indicators power distributed by gender ($v2pepwgen$), equality in respect for civil liberties by gender ($v2clgencl$), access to public services by gender ($v2peapsgen$), access to state jobs by gender ($v2peasjgen$), and access to state business opportunities by gender ($v2peasbgen$)" (Coppedge et al., 2020). - *Relative Freedom of Movement for Women*: Ratio of $\frac{\text{freedom of domestic movement for women (v2cldmovew)}}{\text{freedom of domestic movement for women (v2cldmovem)}}$. The indicators specify "the extent to which all women / men are able to move freely, in daytime and nighttime, in public thoroughfares, across regions within a country, and to establish permanent residency where they wish" (Coppedge et al., 2020). - *Relative Access to Justice for Women*: Ratio of $\frac{\text{Access to justice for women (v2clacjstw)}}{\text{Access to justice for men (v2clacjstm)}}$. Access to justice "specifies the extent to which women/men can bring cases before the courts without risk to their personal safety, trials are fair, and women have effective ability to seek redress if public authorities violate their rights, including the rights to counsel, defense, and appeal" (Coppedge et al., 2020). - *Relative Freedom of Discussion for Women*: Ratio of $\frac{\text{Freedom of discussion for women (v2cldiscw)}}{\text{Freedom of discussion for men (C) (v2cldiscm)}}$. Freedom of discussion "specifies the extent to which men are able to engage in private discussions, particularly on political issues, in private homes and public spaces (restaurants, public transportation, sports events, work etc.) without fear of harassment by other members of the polity or the public authorities. We are interested in restrictions by the government and its agents but also cultural restrictions or customary laws that are enforced by other members of the polity, sometimes in informal ways" (Coppedge et al., 2020). - *Power distributed by gender ($v2pepwrgen$)*: Measures if political power distributed according to gender. Lower values indicate men have near-monopoly on political power, while higher values indicate more equality in power between women and men. - *Freedom from forced labor for women($v2clslavef$)*: measures whether adult women free from servitude and other kinds of forced labor. "Involuntary servitude occurs when an adult is unable to quit a job s/he desires to leave — not by reason of economic necessity but rather by reason of employer's coercion. This includes labor camps but not work or service which forms part of normal civic obligations such as conscription or employment in command economies" (Coppedge et al., 2020). - *Property rights for women ($v2clprptyw$)*: measures whether women enjoy the right to private property. "Private property includes the right to acquire, possess, inherit, and sell private property, including land. Limits on property rights may come from the state (which may legally limit rights or fail to enforce them); customary laws and practices; or religious or social norms. This question concerns the right to private property, not actual ownership of property." (Coppedge et al., 2020).
Women, Business and the Law Index (World Bank)	The WBL index is a recently released index covering 170 countries between 1970-2020, providing a measures of women's equality in development outcomes, labor force participation, vulnerable employment and political participation where scores are based on the average of each economy's scores for 8 distinct topics listed below. A higher score indicates more gender equal laws. Mobility – Incorporates answers to the following questions – Can a woman apply for a passport in the same way as a man? Can a woman travel outside the country in the same way as a man? Can a woman travel outside her home in the same way as a man? Can a woman choose where to live in the same way as a man? Workplace – Incorporates answers to the following questions – Can a woman get a job in the same way as a man? Does the law prohibit discrimination in employment based on gender? Is there legislation on sexual harassment in employment? Are there criminal penalties or civil remedies for sexual harassment in employment? Pay – Incorporates answers to the following questions – Does the law mandate equal remuneration for work of equal value? Can a woman work at night in the same way as a man? Can a woman work in a job deemed dangerous in the same way as a man? Can a woman work in an industrial job in the same way as a man?

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Variable (Source)	Description
	Marriage – Incorporates answers to the following questions – Is there no legal provision that requires a married woman to obey her husband? Can a woman be head of household in the same way as a man? Is there legislation specifically addressing domestic violence? Can a woman obtain a judgment of divorce in the same way as a man? Does a woman have the same rights to remarry as a man? Parenthood – Incorporates answers to the following questions – Is paid leave of at least 14 weeks available to mothers? Length of paid maternity leave? Does the government administer 100% of maternity leave benefits? Is there paid leave available to fathers? Length of paid paternity leave? Is there paid parental leave? Shared days? Days for the mother? Days for the father? Is dismissal of pregnant workers prohibited? Entrepreneurship – Incorporates answers to the following questions – Can a woman sign a contract in the same way as a man? Can a woman register a business in the same way as a man? Can a woman open a bank account in the same way as a man? Does the law prohibit discrimination in access to credit based on gender? Assets – Incorporates answers to the following questions – Do men and women have equal ownership rights to immovable property? Do sons and daughters have equal rights to inherit assets from their parents? Do male and female surviving spouses have equal rights to inherit assets? Does the law grant spouses equal administrative authority over assets during marriage? Does the law provide for the valuation of non-monetary contributions? Pension – Incorporates answers to the following questions – Is the age at which men and women can retire with full pension benefits the same? Is the age at which men and women can retire with partial pension benefits the same? Is the mandatory retirement age for men and women the same? Are periods of absence due to childcare accounted for in pension benefits?
Women's Rights (additional data)	We gleaned Women's rights data from several additional sources: - *Women Ministers*: Women Minister data is drawn from the Inter Parliamentary Union (IPU). These were compiled based on country year aggregates for 1994, 1998, 2000, 2005, 2008, 2010, 2012, 2014 and 2015. - *Female Labor Force Participation*: This is modelled by the International Labour Organization (provided in the ILOSTAT database) as the percent of women aged 15 and above who are economically active. It is available from 1990 to 2015. - *Abortion (Save Mother's Life)*: Measures of abortion availability, and in which circumstances abortion is legal is drawn from the data of Elías et al. (2017). - *Abortion (Fetal Impairment)*: As above.
‡*Maternal Care Inputs* (World Bank)	Recent data from the World Bank Data Bank allow us to examine the state of maternal health care in a subset of countries and years. These data are constructed and released by the World Bank using comparable measures from each country: specifically data from UNICEF, the State of the World's Children, Child Info, and the Demographic and Health Surveys (DHS). As such, these measures are only available in years and countries for which surveys were conducted, resulting in fewer observations than the yearly measures of maternal mortality. In our analysis we use the full set of data released in the World Bank Data Bank. We use the following two policy relevant indicators: - *Antenatal Care*: The percent of pregnant women receiving prenatal care (indicator SH.STA.ANVC.ZS). - *Skilled Birth Attendance*: the percent of all births attended by skilled health staff (indicator SH.STA.BRTC.ZS).
Fertility (World Bank)	Total fertility rates are expressed as the number of children expected to be born per woman based on current fertility rates, and are available as World Bank indicator SP.DYN.TFRT.IN.
‡*Women's Schooling, Average 15–19* (Barro and Lee (2013))	Women's schooling measures are taken from the Barro-Lee dataset (Barro and Lee, 2013) which gives average years of schooling for women aged 15–19 years. Barro-Lee is only available quin-quennially from 1950 to 2015. We use the sample from 1990–2015, and linearly interpolate by country between 5 year periods.
‡*Men's Schooling, Average 15–19* (Barro and Lee (2013))	As above.
Teen Pregnancy (World Bank)	The adolescent fertility rate expressed as the number of births per 1,000 women aged 15–19 is provided by the World Bank, WDI database, indicator SP.ADO.TFRT.
GDP (World Bank)	The log of GDP per capita is PPP adjusted and measured in 2011 international dollars. This is taken from the World Bank WDI database, indicator NY.GDP.PCAP.PP.KD.
Infant Mortality Rates (DHS)	We use male and female infant mortality rates based on the DHS data. Infant mortality is defined as the death of a child before reaching the age one.

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Variable (Source)	Description
Quota Predictors (IPI, World Bank, Beck et al. (2001))	We examined quota predictors as laid out in Krook (2010): - *Peacekeepers*: the number of peacekeepers in a country from The International Peace Institute, IPI Peacekeeping Database. - *Net Overseas Development Assistance*: World Bank Indicator DT.ODA.ODAT.CD - *Political Competition*: a series of measures of political competition and landscape from (Beck et al., 2001)
Democracy (Polity IV)	Our measure of democracy was gleaned from the Polity IV project database. This database records information on the political regime in 167 countries, between 1800 and 2014. The democracy indicator is available annually, and is a 0–10 scale based on measures of competitiveness of political participation, openness and competitiveness of executive recruitment and constraints on executive powers. Higher values reflect more open, democratic societies.
Population (UN)	We collect population covering the entire population, and just the population of women aged 15-64 years which are available by country and year over an extended period of time. These are drawn from the United Nations Population Division's World Population Prospects.
Maternal Mortality (DHS)	We construct country by year measures of maternal mortality based on microdata collected in the DHS. The DHS employs the sisterhood method, asking each woman to list all her sisters, whether they are surviving, and if not, the year of their death and whether this was related to maternal causes. The DHS maternal mortality module which employs this procedure is implemented in only a sub-sample of countries. We collect all DHS survey waves which have implemented the maternal mortality module, and use these to construct retrospective panels of all sisters listed by women, converted into pseudo-panels such that each sister has an observation for each year in which she is aged between 15–49 and is surviving. For each sister$\times$year observation we infer whether she dies for reasons related to childbirth based on the sisterhood surveys. Based on this pseudo-panel, we calculate a country by year observation for rates of maternal mortality.
Fertility, birth spacing and birth composition (DHS)	Country by year measures of age-specific fertility rates, total fertility, birth spacing and characteristics of mothers giving birth are calculated from DHS microdata based on retrospective fertility panels. Each surveyed woman is asked to report her full fertility history including months and years of all births. From these fertility histories we generate a pseudo-panel covering all women with a single observation for each of their ages between 15–49 (or up to their age at the time of the survey). In each year, we infer whether the woman gave birth, and if so, the birth spacing between that and the following birth, as well as the child's sex. Based on these pseudo-panels we aggregate data to country$\times$year cells calculating fertility rates in each quinquennial age group (15-19, 20-24, …, 40-44), as well as the average birth spacing for all births, the sex ratio of births, and the mean education and literacy status of mothers giving birth.
Infant Mortality by sex (DHS)	We calculate (total) infant mortality as well as infant mortality by sex from DHS microdata. To do so, we generate a panel of all children born reported by mothers in the DHS, their year of birth, their gender, and their survival status. If they are recorded as not surviving, we record their age at death. From these micro-level observations, we calculate the infant mortality rate as the total number of infant deaths in the country and year cell, divided by the total number of births. A similar calculation is conducted for male and female infant mortality rates (with the denominator of male and female births respectively).

Notes. ‡ For indicated time-varying variables, missing values are linearly interpolated. Where relevant, tests using original (non-interpolated) values are provided as part of the online appendix.

B Matched Controls using a Pooled Synthetic Control Procedure

In the main event study specification identification is drawn by comparing changes in countries which adopt quotas to all other countries which did not adopt quotas. Thus, 'control' units consist of all non-quota countries, with prevailing differences (in the pre-reform period) captured by country fixed effects. An alternative model consists of explicitly matching each treated country with a single aggregate or synthetic matched control country, and observing how outcomes evolve in the treated and synthetic control, inferring treatment effects based on differences in each case.

We conduct such a procedure, generating a synthetic control for each quota country. We then generate a full dynamic set of treatment effects by pooling lags across each post-quota period as laid out below. Consider country c which adopted quotas in year t. For this country, the pool of countries which can potentially generate a synthetic control consists of all 156 countries which never adopted a reserved seat quota. For country c generate the synthetic control following Abadie et al. (2010) by finding the vector of weights $\mathbf{W}^*$ which minimize: $||\mathbf{X_1} - \mathbf{W}\mathbf{X_0}||$ where $\mathbf{X_1}$ and $\mathbf{X_0}$ consist of a matrix of (identically defined) matching variables between the quota country and the pool of non-quota countries. In this particular implementation, the matching variables consist of rates of the outcome of interest (women in parliament or maternal mortality) in each of the pre-quota periods $t-10, t-9, \ldots, t-3$, as well as binary indicators for the country's sub-region based on UN sub-region classifications. Note that there are three particular features of this method that should be highlighted. The first is that countries will be explicitly matched based on pre-treatment outcomes such that pre-trends in country c and its synthetic control should be equal in levels (this is a test of the quality of the synthetic control). Second is that periods $t-2$ and $t-1$ are not mechanically matched in the synthetic control procedure, and thus can act as placebo periods such that impacts should not be observed prior to the reform's passage. And finally, note that the inclusion of sub-region binary indicators is a device which favors choosing matched units from within the same geographic areas (and potentially exposed to similar shocks), but that units from outside of these areas can be included if the difference in binary variables owing to sub-region variations is offset by the weighted quadratic difference in other matching variables.[42]

We conduct this procedure for each quota country which is amenable to generation of a synthetic control in this way. Note that of the 22 quota countries, only 15 can be adequately matched in synthetic control methods. Uganda adopts quotas prior to 1990 and so has no pre-adoption periods to match on. South Sudan is a new country, and so has insufficient periods to match on. Afghanistan, Burundi, Iraq, Djibouti and Saudi Arabia each have multiple missing measures for MMR in the years prior to quota adoption and as such have missing entries in the $\mathbf{X_1}$ matrix. Once each country has its own synthetic control generated, we estimate full dynamic treatment effects by averaging the difference between each post-treatment period and the synthetic control over all treatment-synthetic control pairs. Thus, the estimated treatment effect on the first lag is the mean of the difference between each country and its synthetic control in the first post-quota lag, with similar procedures conducted for each lead and lag term.

Finally, to conduct inference in this setting we implement a permutation method, which consists of randomly permuting the actual distribution of quota years across alternative countries. Thus, each permutation maintains the precise structure of the data, simply permuting treatment units. We conduct 500 permutations, and generate 95% confidence intervals for *each* lag and lead as the end points of percentile 2.5 and 97.5 of each permuted treatment effect.

[42] In other words, countries from outside of the sub-region can be included in the synthetic control if they are considerably better matches than countries within the same sub-region and can pay the penalty based on different sub-region indicators in the matching process.

C Description of Resampling Procedure for MMR Uncertainty

Publicly available MMR data published by the MMEIG consist of a point estimate and the upper and lower points of the 80% uncertainty interval. In describing the modeling procedure, the authors note “We computed 80% uncertainty intervals (UIs) for the MMR and all related outcomes using the 10th and 90th percentiles of the posterior distributions. …We report 80% UIs rather than 95% UIs because of the substantial uncertainty inherent in maternal mortality outcomes: intervals based on higher uncertainty levels quickly lose their ability to present meaningful summaries of a range of likely outcomes.” (Alkema et al., 2016, p. 1250). In order to estimate standard errors and p-values based on these data, we undertake the following procedure:

Resample Algorithm

1. Take a clustered bootstrap resample from the original data $b = 1, \ldots, B$, with $B = 500$
2. Generate a random vector of size N (where N is the sample in the regression) where each element is either a) a draw of a normal variable where 80% of the probability mass falls between -1 and 1 (a draw from $\mathcal{N}(0, 0.7803)$), or b) a draw from a triangular distribution in the interval [-1,1]. Below this is ε, and in each case, these integrate to 1.
3. Generate a resampled value of maternal mortality as: $MMR_t^{b*} = MMR_{ct} + \varepsilon \frac{MMR_{ct}^{UB} - MMR_{ct}^{LB}}{2}$, where MMR_{ct}^{LB} is the lower bound estimate and MMR_{ct}^{UB} is the upper bound estimate; ie take the original measure, and draw a value from the uncertainty interval centred around this measure.
4. Estimate the original regression using the resampled data, with the re-resampled MMR measure. This results in an estimate of interest $\widehat{\beta}^{b*}$
5. if $b < 500$ return to step 1. Else go to step 6
6. Calculate the standard error of $\widehat{\beta}$ as the standard deviation of $\{\widehat{\beta}^{1*}, \widehat{\beta}^{2*}, \ldots, \widehat{\beta}^{500*}\}$. This replaces the original naive standard error, and similarly a p-value can be calculated associated with the null hypothesis of a null impact, analogous to the p-value calculated based on a standard regression coefficient.

This procedure is only necessary when estimating impacts of quotas on maternal mortality, and not for women in parliament as we are only adjusting for uncertainty in the dependent variable in cases where maternal mortality is used. Note that in the above we are re-sampling maternal mortality to provide full coverage of the 80% uncertainty interval, or indeed, to provide greater than full coverage in the case of the normal draw, for each country year pair. In each case, the normal or triangular distribution places more weight on the likelihood of observing a value of maternal mortality close to the stated estimate, and less weight on the likelihood of observing a value in the tails of the distribution.

The above resampling procedure assumes that uncertainty in maternal mortality is independent between countries and years. It may also be the case that uncertainty is correlated across years within a country. When undertaking inference robust to uncertainty we thus present p-values associated with a range of cases as presented in Table A5. These are:

1. Bootstrap: The bootstrap analogue of the original p-value (ie no uncertainty in MMR)
2. Triangular Correction: Resamples from the MMR uncertainty range from the WHO data (80% coverage) with a triangular distribution whose minimum and maximum are at the end points of the uncertainty range, and whose center is at the estimate

3. Triangular Correction by Country: Resamples as above, however now instead of taking uncertainty draws by country and year, takes uncertainty draws only at the level of the country. This implies that uncertainty with regards to MMR is perfectly correlated within a country over time. It is the limit case of assuming correlation within a country in uncertainty in MMR measurement.

4. Normal Correction: Resamples from the MMR uncertainty range assuming a normal distribution, where draws are taken so that the $10^{th}/90^{th}$ quintile of the normal are at the upper and lower end points of the uncertainty range presented in the WHO data in each case. This allows for us to sample outside the 80% confidence bounds presented in the original data, and will be the most demanding of all corrections.

5. Normal Correction by Country: Resamples as above, however now instead of taking uncertainty draws by country and year, takes uncertainty draws only at the level of the country.

In results documented in Table A5 we first provide p-values associated with a standard clustered bootstrap prior to taking into account uncertainty in MMR measurements. Next we provide two sets of bootstrapped p-values computed assuming either a triangular distribution or a normal distribution for MMR uncertainty intervals. While the end points of the triangular distribution are at the ends of the uncertainty interval, the normal distribution provides coverage outside of the 80% UI. In each of the two types of distributions we allow for the possibility of either assuming full correlation in uncertainty by country or not. The corrections by country assume full correlation in uncertainty within a country over time. Where not by country, the estimator assumes no correlation within a country over time. The triangular corrections re-sample from MMR so that coverage respects the full 80% uncertainty interval suggested by the MMEIG. The normal corrections re-sample so that 80% of re-samples fall within the 80% uncertainty interval reported by the MMEIG. Both of these inference procedures are implemented assuming no correlation by country, and then assuming full correlation in uncertainty by country.