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Quota Shocks:

Electoral Gender Quotas and Government Spending Priorities Worldwide

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Abstract: The rapid expansion of electoral gender quotas in the past few decades has been met with considerable scholarly and public attention. Despite this, there has been little empirical work examining the global legislative consequences of gender quotas over time. Developing a unique time-series cross-sectional dataset from 139 states during the peak period of quota adoption and implementation (1995 - 2012), we test whether and how quotas are associated with subsequent changes in government spending priorities. We find that substantial quota shocks - those associated with a large increase in women's parliamentary representation - are followed by increased government expenditures towards public health. Further, we find that increases in health spending are offset by relative decreases in military spending and other spending categories. Our findings provide strong evidence that quota policies influence government priorities in historically feminized policy areas, but principally when they are complied with and have substantial numerical consequences.

Keywords: electoral gender quotas, government spending, substantive representation, budget priorities, legislative politics

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Introduction

One of the most significant institutional developments in representative democracy in the last twenty years is the recent, rapid, and global diffusion of electoral quotas for women in politics. Over 70 countries have implemented legally mandated parliamentary gender quotas, either reserving seats for women in legislative houses or requiring parties to have a certain percentage of women candidates on party lists. Over 60 political parties in more than 30 additional countries have also voluntarily put in place quota measures to increase women's inclusion (International IDEA, 2017). Taken together, these policies have deeply transformed the composition of legislative bodies worldwide. Indeed, women's average share of parliamentary membership doubled between 1995 and 2015, moving from 11.3 percent to 22.6 percent during this time (IPU, 2015). Responding to this phenomenon, studies related to the causes and consequences of electoral gender quotas have become one of the fastest growing subfields in gender and politics research (Krook, 2009). Yet despite what has developed into a rich and substantial literature, still relatively little work has attempted to ascertain the substantive effects of quotas from a comparative perspective.¹ This lack of cross-national research is a result, in part, of the difficulty in comparing if and how quota policies change decision-making outcomes in a way that is comparable across cases and over time.

Here we make at least three contributions to this literature: first, developing a unique time series cross-sectional dataset (between 1995 and 2012) from a global sample of countries, we present the most comprehensive comparative analysis to date of the substantive effects of gender quotas worldwide. Second, we bring together disparate literatures as we build an innovative theory to investigate when in the policy making

¹ With substantive effects, we refer to those effects that are related to policy-makers' activities in the legislature. Throughout the analysis, we will use "substantive effects", "legislative effects", and "policy-related effects" synonymously.

process quotas affect legislative decision-making. We hypothesize that quotas may affect decision-making outcomes when quotas are first adopted as policy, when they are first implemented (i.e. used for the first time as new electoral rules), and/or when they result in significant increases in women's descriptive (numeric) representation. During the adoption or implementation stages, quotas may send signals or cues about the importance of particular policy areas to existing or incoming legislators. In this scenario, quotas act as a heuristic device about changing legislative priorities to both quota and non-quota-elected representatives. Alternatively, quotas may affect decision-making outcomes when they result in substantial increases in the number of newly elected women representatives. Quota policies have had varying levels of effectiveness in their intended outcome to increase women's numeric representation (Paxton & Hughes, 2015; Tripp & Kang, 2008). Indeed, the descriptive changes in women's representation following quotas – what we call here *quota shocks* – can range from essentially non-existent (very marginal increases in women's numeric representation usually due to limited quota compliance) to dramatic (women's numbers increasing several fold).

Taking advantage of variation in the dates of quota adoption and quota implementation also allows us to address important endogeneity concerns when estimating quota effects. We theorize that if quotas are endogenous to a national sentiment or political climate that is also conducive to certain political outcomes, we are most likely to observe the legislative effects of quotas following quota adoption – and not necessarily following implementation (often several years later) or as a function of the number of women who gain office through quotas. To our knowledge, this is the first study of quota outcomes that has made this distinction explicit.

As a final contribution, we examine a set of outcome variables rarely used in the quota literature: changes in government spending priorities. Most studies that focus on potential substantive effects of quotas look for changes in policy outcomes, specifically

whether quotas are followed by greater legislative attention to policy areas concerning women's rights or women's welfare (see e.g. Franceschet, Krook, & Piscopo, 2012; Franceschet & Piscopo, 2008; Goetz & Hassim, 2003). These types of outcomes are inherently difficult to study comparatively, however, because their occurrence is not particularly frequent across cases. Examining changes in governments' budgetary priorities has several advantages. First, budget setting is an essential government function that all states must accomplish each year, rather than a rare event. Additionally, there are relatively consistent and comparable annual data available worldwide during the period in which the majority of quota adoptions and implementations have occurred. Finally, budgetary allocations represent the distribution of somewhat finite resources, and therefore represent trade-offs that, at least to some degree, reflect politicians' priorities in government investments. Examining changes in government expenditures represents a new approach in the quota literature – and, at the same time, examining the ascriptive composition of legislatures is a fairly unexplored dimension in research on the determinants of government spending (see however Bolzendahl, 2009).

Our empirical approach takes advantage of the rich temporal and spatial variation in gender quota adoption in the post-1995 period to examine how quota policies are followed by subsequent changes in government spending priorities. To preview our results, we find that the depth of quota shocks – that is, the degree to which quotas change the gender composition of legislatures – is associated with subsequent increases in government spending in one policy area: health. We do not, however, find significant changes in the two other sectors we examine: education or military spending. Using a modeling approach to explicitly examine trade-offs *between* budget categories, we find that increases in health spending are offset by *relative* decreases in military spending as well as other residual spending categories – but not by changes in education spending. Taken together, our findings provide strong evidence that quota policies influence government spending in an

area that previous research suggests is generally prioritized by women, both citizens and elites. But, quotas appear most meaningful in this regard when they have their intended numerical consequences, and thus substantially disrupt previous patterns of male overrepresentation. Putting the findings in a broader context, our analysis contributes to research on the role of the legislator identity – and other reforms targeting historically underrepresented groups – in reshaping government spending priorities.

1. Gender Quotas and Legislative Priorities: Existing Research

1.1 Quotas and Women's Substantive Representation

Empirical research on the legislative effects of gender quotas is usually situated within the framework of women's substantive representation (e.g. Franceschet et al., 2012). Within this literature, most scholars have examined whether quotas have been followed by subsequent changes in gender-related public policy. For instance, in the first edited volume examining the policy-related impacts of quotas, Franceschet et al (2012, pp., 12) refer to work on quotas and women's substantive representation as studies “seeking to establish whether quota introduction increases the number of policies proposed, debated, and passed on behalf of women as a group.” Just like research on the process of representation more generally, research on whether quotas have been associated with advances in policy passages have, to date, been mixed. Some work has found that women legislators are more likely to sponsor or co-sponsor bills related to women's rights (Barnes, 2012; Franceschet & Piscopo, 2008; Kerevel & Atkeson, 2013) and that quota-elected women have been instrumental in the successful passage of these bills (Kerevel & Atkeson, 2013). Other work, however, reports mixed findings (Devlin & Elgie, 2008) or a general lack of success in advancements in gender-egalitarian legislation following quota implementation (Htun, Lacalle, & Micozzi, 2013).

1.2 Measuring Outcomes and Defining Women's Interests

Just like research on gender and political representation in general, work investigating if and how quota policies have affected outcomes related to women's substantive representation often build on the expectation that men and women citizens have different social experiences, which lead to divergent political interests, priorities, and preferences (Sapiro, 1981; see also Young, 2000). However, the extant literature has not reached an agreement on how to conceptualize and measure these differences. Some studies that connect gendered differences in political priorities to the legislative activities of women politicians first specify women's interests through predefined issue areas that objectively and disproportionately affect women's welfare, such as maternal health or combating domestic violence (Clayton, Josefsson, & Wang, 2016; Schwindt-Bayer, 2006). Other work measures women's interests in areas that may also indirectly affect women given traditional gender roles, such as investments in child health (Miller, 2008).

Rather than examining pre-specified interests, yet another group of scholars has attempted to first ascertain gendered differences in citizens' policy priorities, and then measure the extent to which women's presence in political bodies affects attention to these priorities. Importantly, this work reveals gender differences in legislative and spending priorities are not uniform. Even in the same setting, there are disparate claims. For instance, while both examining evidence from India, Chattopadhyay and Duflo (2004) find that men citizens prioritize investments in education, whereas Clots-Figueras (2012) find that education is a women-preferred policy area.

1.3 Quota Effects on State Spending

Here, we focus on government spending priorities, a highly relevant but less researched outcome in the literature on the substantive effects of quotas. Although there is variation across countries, spending priorities reflect, at least to some degree, MPs' preferences and influence. Despite the potential budgetary implications of electoral gender quotas, most

work on women legislators' influence on spending priorities has either excluded the role of quotas (Bolzendahl, 2009; Swiss, Fallon, & Burgos, 2012) or included quotas as a methodological tool to address issues of endogeneity (e.g. Chattopadhyay & Duflo, 2004; Chen, 2010). Further, research on the global determinants of government spending typically prioritize explanations related more to broader political contexts, such as the depth of democracy (Albertus & Menaldo, 2014) and constituent preferences and issue definition (Soroka & Wlezien, 2005) or to macro-economic variables, such as the effects of globalization (Wibbels, 2006), foreign direct investment (Avelino, Brown, & Hunter, 2005), and trade (Wibbels & Ahlquist, 2011), and less to ascriptive characteristics within legislatures.

As a final relevant literature, we also draw on work that examines the micro-determinants of legislators' ability to affect budgetary decisions as they work within different institutional constraints. A rich body of work examines how political institutions affect the size, priorities, and changing levels of government spending (see, e.g., Besley & Case, 2003), related to, for instance, legislature and ruling coalition nature and size (Martin & Vanberg, 2013), level of bureaucracy (Ting, 2012), and executive control in presidential versus parliamentary systems (Lienert, 2005). Although our empirical strategy holds time invariant county-specific variables constant, and thus holds political contexts relatively fixed, an extension of this work would seek to understand how quotas are associated with budgetary outcomes when they are embedded within different types of political systems.

2. How Quotas Affect Budgetary Priorities: Theory and Hypotheses

Here we develop a theory to test when in the policy-making process quotas affect MP preferences and behavior, both at the individual level and in the aggregate. Whereas the sequential model we develop is likely applicable across a range of policy-making processes, we focus our discussion here to the ways in which gender quotas may affect

budgetary decisions made within national legislatures. In creating our hypotheses, we make the assumption that legislatures – and legislators – affect how governments allocate public spending to different policy areas. Jones et al. (2009:856) provide a useful definition on this point, noting, “Budgets quantify collective political decisions made in response to incoming information, the preference of decision makers, and the institutions that structure how decisions are made.” By analyzing changes within countries, we keep the third component of this process relatively fixed and focus on the ways quotas may affect the first two dimensions: incoming information and collective legislator preference.

We propose that quotas have the potential to change legislative funding priorities in at least two broad ways. First, quotas may affect priorities when they are first adopted or implemented (i.e. used for the first time as new electoral rules) by sending specific policy-generated cues to officeholders, prompting changes in legislative behavior among both quota and non-quota elected MPs alike (c.f. Franceschet et al., 2012). Second, by altering the gender composition of legislative bodies (in some cases dramatically) quotas may also influence governments’ budgetary priorities by bringing more women into legislatures, which may change aggregate legislator preferences, as well as increase women’s ability to collectively affect legislative decisions.

We begin by developing the argument of quotas as a potential signaling mechanism in either the adoption or implementation phases. In such instances, the very act of quota adoption or implementation may provide legislators new information about changing legislative priorities. First, during the policy adoption process, quotas may send policy cues to both men and women policy-makers about women’s policy priorities (c.f. Zetterberg, 2009). All legislators that have made decisions about adopting gender quotas have been exposed to, and participated in, quota debates, often with women’s organizations, women party activists, and other quota advocates. As a consequence, the introduction of quota policies may draw attention to gender equality issues more broadly and thus to issues

prioritized by women citizens. Increased awareness about gender-related issues may generate a reorientation of all MPs' priorities toward areas that they believe better reflect women's preferences (c.f. Miller, 2008). If the propositions about policy-generated cues and awareness raising of gender equality issues are true, we should find that the mere adoption of quotas into constitutional or electoral law or the voluntary adoption by political parties causes a reorientation of spending priorities among both men and women MPs.

In addition, quotas may be adopted in times of general progressive policy change, particularly in post-conflict settings. In this scenario, parliaments may adopt quotas as part of a bundle of progressive measures meant to remedy previous social grievances, of which a reprioritization of public finances may also be one. Further, when parties adopt quotas voluntarily, they may be doing so as part of a larger party strategy to appeal to women voters, which may also include commitments to historically feminized issue areas. We note that this raises the issue of attempting to disentangle the impact of quotas from a scenario in which changing attitudes towards spending priorities in general (for either historic or strategic reasons) only correspond with, and are not caused by, the introduction of quotas. In such instances, we expect to observe subsequent changes in budgetary priorities following quota adoption, when the policy is most directly a part of public discourse and most likely to reflect public sentiment. If either of these competing scenarios is at play (quota adoption as a policy cue or quotas as a part of a host of progressive reforms), we hypothesize **(H1)** that quota policies will be followed by changes in government spending priorities following their initial adoption.

Quotas may also send policy cues when they are first implemented during elections and a new cohort of legislators, including the newly quota-elected representatives, take office. Previous work suggests that quotas can change dynamics within parliaments and affect legislators' priorities and behavior – regardless of the number of women who achieve office through the policy. Under the condition that *any* women enter parliament once

quotas are applied in an election, quota implementation may politicize gender in politics, and women legislators may perceive an obligation to act on behalf of women (Franceschet & Piscopo, 2008). In this scenario, a perceived mandate for gender equality may prompt changes in women MPs' priorities regardless of whether the quota policy generates a large leap in the number of women legislators. Additionally, even small numbers of quota-elected women may serve as critical actors (Childs & Krook, 2009) and work through formal channels or engage in informal discussions and negotiations to affect budget decisions. These expectations lead us to hypothesize **(H2)** that quota policies will be followed by changes in government spending priorities once they are first implemented regardless of the number of women they bring into office.

Gender quotas may also affect public spending if they bring in women representatives who, for personal or constituency-related reasons, prioritize and pursue different issues than current parliamentarians. In this argument, quotas must not only be adopted and implemented during elections, but they must also be effective in their intended aim of increasing women's numbers. Experiences from a large number of countries show that quotas do not uniformly result in this outcome. For instance, when legally mandated quotas target candidate lists in proportional systems, party gatekeepers have commonly chosen not to comply with the quota law unless they are sanctioned for not doing so, or parties comply with the letter of the law but choose to put women in non-electable positions (Bjarnegård & Zetterberg, 2016; Hinojosa, 2012).

Exactly how women "make a difference" is still a pending issue in the literature on gender and political representation (Childs & Krook, 2008). Here, we theorize that a quota-related increase in the number of women in parliament may serve to change both formal and informal parliamentary institutions and thus affect public spending patterns during at least three different stages of the budgetary process. First, quota-elected legislators may affect government spending priorities when budget proposals are first formulated, through

(informal) negotiations and bargaining with other legislators or members of the executive – particularly if quotas increase the likelihood that women gain leadership positions within their parties (see O’Brien & Rickne, 2016). If MPs’ policy preferences are gendered, we expect women’s bargaining power in informal lobbying scenarios to increase as their numbers grow. Second, women legislators may influence government spending priorities during the legislature’s review of the budget proposal. This work usually takes place within committees; and as recent analyses suggest, as more women enter parliament through quotas, the likelihood that women are able to enter relevant sectoral (e.g. education, labor, health, defense) (Kerevel & Atkeson, 2013) or budget-setting committees (Barnes, 2014; Heath, Schwindt-Bayer, & Taylor-Robinson, 2005) is likely to increase. Finally, quota-elected representatives may influence the budget during chamber wide legislative procedures to approve, reject, or amend the executive’s budget proposal. Again, assuming men and women MPs – for personal or constituency-related reasons – have different legislative priorities, women’s increased presence in political bodies will cause an aggregate preference shift in the legislature and, *ceteris paribus*, will affect collective legislative decision-making outcomes.

If our expectations about women’s numerical representation are true, we should find that an increased number of women legislators following quota implementation is associated with subsequent changes in government spending priorities. Whereas in this work we are not able to test at which stage or combination of stages quota-elected representatives exert the most influence in budgetary allocations, if women newcomers act in any of the above ways, we expect that the budgetary effects of gender quotas will be most visible when quotas generate significant leaps in the number of women elected to legislative bodies. This leads to our final hypothesis (**H3**): Quota policies will affect within-country changes in government spending priorities by increasing women’s presence in legislative decision-making.

We have yet to specify *which* policy areas we expect quota policies to affect. Here we rely on previous literatures and make no strong *a priori* expectations that might risk essentializing women's interests or priorities. We select three areas of the budget where we expect gender gaps in political priorities to vary: health, education, and military spending. Although of course not all topics related to health are exclusively of women's interest (see e.g. Murray, 2014), we do see strong evidence that women citizens and women MPs support increased funding for public health, particularly maternal and child health (Bhalotra & Clots-Figueras, 2014; Miller, 2008; Schwindt-Bayer, 2006; Swiss et al., 2012; Westfall & Chantiles, 2016). As for education, we see less evidence of a consistent gender gap (Chattopadhyay & Duflo, 2004; Clots-Figueras, 2012; Gottlieb et al., 2015); and there is some evidence that military spending is associated with male dominance in parliaments (Koch & Fulton, 2011). We therefore expect budgetary changes following quotas to be most associated with increased public health expenditures, unrelated to education spending, and marginally associated with decreased military spending.

Finally, it is theoretically important to understand whether any changes in government spending priorities are (mainly) the result of a redistribution of funds from one area of the budget to another, or whether quota policies contribute to a recognition of new policy priorities by (also) adding funds and increasing total spending (c.f. Fraser, 1998). Our expectations here are mixed. Some research suggests that women's legislative presence mainly affects the redirection of funds between budgetary categories rather than changes to total spending (Bolzendahl 2009). However, work on gender differences in citizens' political preferences in advanced industrialized settings indicates that women favor larger public service sectors and more redistributive spending than men in general, and thus presumably tolerate greater levels of government spending (Catalano Weeks, 2015; Inglehart & Norris, 2003; Iversen & Rosenbluth, 2010). We therefore are agnostic

as to whether quotas will affect state budgets mainly by reorienting spending priorities within existing constraints or also by increasing total spending levels.

3. Data and Methodology

3.1 Dependent Variables: Government Spending Priorities

We use time-series cross-sectional data from 139 countries between 1995 and 2012 to measure how gender quotas relate to subsequent changes in government spending priorities.² Aside from relative data completeness, the post-1995 period is ideal for testing the potential impacts of quotas across a range of outcomes, as the vast majority of quota adoptions and implementations have occurred during this period.³ We drop the ten cases in which countries adopted quotas prior to this time period, given that our models exploit variation in the timing of quota policies to assess subsequent changes in spending priorities. Our main dependent variables consist of expenditure data by sector for health, education, and military spending.⁴ Using data from the World Bank's World Development Indicator (WDI) dataset, we measure sectoral spending as the percentage each category contributes to total government expenditures.

3.2 Key Independent Variables: Operationalizing Quotas

² Our sample includes most countries with a population over one million. We drop the few cases with a population over one million for which there is complete missingness across a wide range of our covariates, such as Somalia and South Sudan. The ten cases we drop that adopted quotas before 1995 are: Argentina, Austria, Canada, Germany, Mozambique, Norway, Netherlands, South Africa, Sweden, and Uganda.

³ This is due, in large part, to the momentum gained in global quota adoption after the United Nation's 4th World Conference on Women held in Beijing in 1995.

⁴ In addition to being of theoretical interest, these are the only spending categories for which there are consistent longitudinal data.

Across models, we run three primary specifications each operationalizing different quota policy dimensions. These are all labeled as *Quota* in the regression tables that follow and operationalization is indicated in each model's column. To test H1, we use a simple dichotomous measure of quota *adoption*, coded zero for each country-year before quota adoption (in the single or lower house of parliament), and one for the adoption year and each year subsequent. Whereas relatively accurate and up-to-date data on quota policies by country are publically available by International IDEA through the Quota Project, current data on years of quota adoption and implementation are not widely available (but see Hughes, Paxton, Clayton, & Zetterberg, Forthcoming). Building on information from the Quota Project, in addition to data compiled by Krook (2006), Bush (2011), Hughes, Krook, and Paxton (2015) and other secondary sources, including academic books and articles, expert interviews, and news coverage of quota adoption in specific cases, we construct our own coding of quota adoption by year. This measure includes all quota laws that specify gender requirements for positions on candidate lists or reserve parliamentary seats for women. We also include voluntary party quotas, but only those that meet a fairly strict criterion: the total number of adopting parties must hold at least thirty percent of seats in the lower parliamentary house at the time of adoption. Our data include 15 cases of reserved seat quota adoption, 43 cases of candidate list quota adoption, and 7 cases of voluntary political party quota adoption.

To test H2, we model quota policy *implementation* as a second specification of our key independent variable. As with our adoption specification, we code implementation dichotomously, coded zero for each country-year before quota implementation and one for the year in which a functioning quota was first used during a parliamentary election and each year subsequent. In most instances, quota implementation does not occur until the subsequent electoral cycle, typically a year or more after adoption. In other instances, countries will adopt gender quotas, but will take several electoral cycles to properly enforce

the policy in elections (e.g. Armenia), will fail to hold proper elections at all (e.g. Eritrea), or will implement the policy after our 2012 end date (e.g. Ireland and Saudi Arabia). We code quota implementation as involving at least minimal compliance to the letter and spirit of the law, which often requires enforcement mechanisms such as minimum sanctions for noncompliance or placement mandates for women candidates on party lists. For instance, we code Brazil as having adopted a candidate list quota in 1997, but never as functionally implementing the law. We code twelve countries that adopted but did not implement quotas during the 1995 - 2012 period. Figure 1 maps our cases; we include our complete coding of quota adoption and implementation dates in the Online Appendix.

Figure 1 Here

Finally, to test H3, we construct a variable that measures quota policy depth. Here we measure the level of change in women's descriptive representation that was initiated by the initial quota shock. Similar to our coding of adoption and implementation, we code quota shocks as zero for each country-year before quota implementation and as the percentage point change in women's representation induced by the quota for the year of implementation and each year subsequent.⁵ Quotas are intended to increase the numeric representation of women in politics and there are varying degrees of the strength of this treatment across countries. The greatest quota shock in our data is Angola's candidate list quota in 2008, which resulted in a 27.8 percentage point increase in women's numeric representation; moving from 9.5 percent to 37.3 percent in one election cycle. Kyrgyzstan, Algeria, Senegal, Afghanistan, Rwanda and Burundi all also experienced quota shocks that increased women's parliamentary representation by over 20 percentage points. On the other end of this spectrum, some states have adopted quotas that impose requirements that

⁵ Across specifications, all non-quota adopting countries are coded zero for each country-year.

are similar to current levels of women's parliamentary representation. For instance, when China enforced a reserved seat quota in 2007, women's representation in parliament only increased from 20.6 to 21.3 percent. The mean quota shock in our data is a 9.9 percentage point increase. Women's mean parliamentary representation in the year preceding a quota shock in our data is 10.9 percent, meaning that, on average quotas nearly double women's parliamentary representation in the first election cycle.

Figure 2 plots women's parliamentary representation centered on the year of quota implementation for each of the 53 quota-implementing cases included in our sample, and reveals the degree to which quota shocks have transformed the composition of parliamentary bodies worldwide in the past two decades. We label these changes in women's representation following quota implementation as *shocks* in large part because they represent clear breaks in women's numbers between the pre- and post-quota implementation periods. Figure 2 shows the semi-exogenous nature of quota shocks; the strong breaking pattern around the pre- and post-quota periods indicates that these dramatic jumps in women's numbers would not have occurred without specific policy interventions. It is also important to note that whereas some quotas are adopted by party elites for strategic reasons – for instance, to appeal to women voters – quotas are often adopted as a result of pressure from international organizations (Bush, 2011) or transnational feminist activists (Hughes, Krook, & Paxton, 2015), in which case the sudden increase in women's numbers following quota implementation occurs in spite of – rather than because of – domestic conditions.⁶

⁶ The significant results we present below maintain statistical significance (of at least $p < 0.10$) when we exclude the seven cases of voluntary political party quotas (i.e. the cases where we have

Figure 2 Here

3.3 Control Variables

As with all observational analyses, we cannot completely rule out the possibility of an endogenous relationship between quota adoption and subsequent changes in government spending priorities. We attempt to address this possibility through sequentially modeling different stages and features of the quota adoption and implementation process. Further, in the OLS regressions that follow, we include country fixed effects to account for time-invariant country-specific features, and we include a host of political, social, and economic covariates that vary within countries over time that previous literatures suggest may affect both government spending choices and gender quota adoption. We include a dichotomous measure of democracy (a score of six or higher) and autocracy (a score of -6 or lower) from the Polity IV data.⁷ We also include an index of cumulative democracy, measured as the total number of years a country has experienced democracy from 1945 to the observation year. We include the most recent Beck et al. (2001) data on political institutions to record whether a country currently has a leftist ruling party, as well as a measure of the cumulative years of leftist party rule in the post-War period. This is a particularly relevant potential confounder, as leftist rule may be a crucial determinant of both gender quota adoption (particularly for voluntary party quotas) and patterns of social spending (see Dahlerup, 2007).

less reason to believe that quotas are of a semi-exogenous nature). Appendix Table OA3 replicates the results presented in Table 1 excluding these cases.

⁷ Our results are robust to inclusion of the standard polity score as well as different transformations of this indicator. Our results also hold when we include a slate of demographic controls such as the percentage of the population that is elderly (over 65) and young (under 15) – but we exclude these variables from our main models as they are unlikely related to gender quota adoption / implementation.

We also include a host of economic variables from the World Bank's World Development Indicators (WDI) database, which previous research has indicated affect different dimensions of government spending: official development assistance (ODA), oil rents, total trade (sum of exports and imports of goods and services), and foreign direct investment (FDI) (all variables are logged, lagged one year, and taken as a percentage of GDP). These variables also indicate the extent to which states are embedded within global networks, likely making them more receptive to global norms of gender equality and more receptive to pressure from international organizations to adopt gender quotas (see Bush 2011). Thus, we consider them important potential confounders of our relationship of interest. Finally, to address the possibility that a potential reduction in military spending is a function of an ending conflict rather than from the role of quotas adopted during post-conflict transitions, we include two measures of conflict in our models of military expenditures: the number of battle deaths and the presence of a peacekeeping mission in each country-year.

3.4 Descriptive Statistics

Figures 3 and 4 display descriptive country-level spending patterns over the 1995 – 2012 period among quota adopting countries centered on the year of adoption (each budget sector is measured as a percentage of total government expenditures).⁸ Whereas there do not appear to be great differences before and after quota adoption related to education spending (Figure 3, left panel), we see a downward trend related to military spending (Figure 3, right panel), although this appears, in part, to be driven by a handful of high expending countries prior to quota adoption, likely relating to cases in which quotas were adopted in post-conflict transitions. Figure 4 (left panel) indicates that health spending is

⁸ We center on adoption here rather than implementation to include the broader of the two measures and to speak to the first hypothesis we theorized above.

increasing across cases before quota adoption, and the intercept and rate of change appear to increase slightly in the post-adoption period. Figure 4 (right panel) descriptively previews our main finding and displays health-spending trends centered on quota adoption within the sample of high quota shock cases, those with a mean shock above a 9.9 percentage point increase in women's parliamentary representation. Among the high quota shock cases, the average rate of change in the percentage of government expenditures dedicated to public health increases substantially in the post-quota period.

Figures 3 & 4 Here

3.5 Empirical Strategy

In the models that follow, we employ a difference-in-difference approach. To use the causal language often associated with this strategy, countries which adopt quotas (or implement quotas, depending on the specification) during the 1995 – 2012 period are our treatment group, whereas non-quota-adopting countries make up the control group. The inclusion of country and year fixed effects allows for the difference-in-difference approach. The resulting coefficient estimates, then, measure how quotas are associated with within-country changes in government spending relative to the non-quota control countries. Because health spending may also increase in non-quota adopting countries during our period of observation, using the non-quota countries as a counterfactual set of cases allows us to compare the difference in quota-adopting countries with non-quota cases over the same years. The inclusion of country and year fixed effects means that only trend changes in spending that coincide precisely with the timing of quota adoption (or implementation) are captured by our *Quota* variable

4. Results

4.1 Quotas and Changes in Government Spending Patterns

We begin with an examination of how quota policies are associated with changes in the percentage of government spending dedicated to public health. Both previous literature and the descriptive patterns presented in Figures 3 and 4 suggest this is an area likely affected by quotas. Table 1 shows the first series of model results. Starting with our test of H1, Model 1 indicates that, controlling for potential confounding variables, quota adoption is associated with increased health spending in adopting countries relative to non-adopting countries in the same period. The unstandardized coefficient associated with this model (0.718) can be interpreted as the average percentage point increase in health spending in adopting countries in the post-quota period relative to non-adopters.⁹ Related to H2, on the other hand, Model 2 suggests that the implementation of a functioning quota in a parliamentary election is not associated with subsequent changes in health spending. The regression coefficient is positive, but the relationship is not statistically significant. However, when we test H3 in Model 3 and operationalize the *Quota* variable as the initial depth of the quota shock (i.e. the change in women's numeric representation after quotas are first applied in an election), we detect a statistically significant increase in subsequent health spending. The coefficient in Model 3 (0.056) is the percentage point change in health spending associated with each percentage point increase in women's representation following the initial quota shock. That is, a country which sees women's representation increase by 10 percentage points following quota implementation, is followed, on average, by a 0.56 percentage point increase in health spending in the post-quota period compared to non-quota implementing countries.¹⁰

⁹ Across all cases and country-years, the average amount spent on public health is 10.8 percent of total government expenditures with a standard deviation of 4.3 percentage points.

¹⁰ The results from Model 1 include the 12 cases in which countries adopted but did not implement quotas during the 1995 – 2012 period. When we exclude these cases, the coefficient is reduced in

Table 1 Here

These findings provide support for H1 and H3 but not for H2. Related to H1, we hypothesized that significant changes in spending priorities following quota adoption would indicate that adoption sends cues to legislators about changing national priorities and/or that our findings are driven by the general tendency for quota adoption to occur during times of general progressive policy change. Our concurrent finding in support of H3 – that health spending increases as the number of quota-elected women increases – suggests that solely the latter interpretation is less likely: if quotas were *only* endogenous to a national sentiment that is conducive to certain political outcomes, we would not expect substantial increases in women’s numbers to also be associated with increased health spending. Further, our finding that increased health spending does not also follow quota implementation suggests that the political climates that may have surrounded quota adoption seem to dissipate by the time quotas are implemented in elections. This strengthens our confidence in H3: increases in health spending are driven by an increase in women legislators who enter through quotas.¹¹

To further understand the relationships between the different quota policy dimensions, we construct a measure that splits the quota adoption cases into those that resulted in high quota shocks (those greater than the mean, 9.9 percentage points) and those that resulted in low quota shocks (those below the mean). The results of these additional analyses are presented in Model 4 and Model 5 in Table 1. They indicate that the high

magnitude. This provides additional evidence of adoption effects that are distinct from our results related to quota shocks, which we discuss below.

¹¹ In the Online Appendix, we also include an analysis which uses women’s parliamentary representation, rather than quota features, as the key independent variable. Our results are very robust to this specification (see Appendix Table OA4).

quota shock subset of quota adopters remains statistically significant with a much larger coefficient size than that found in Model 1 (see Model 4 in Table 1), whereas the low quota shock subset of adopting countries loses statistical significance (see Model 5 in Table 1). These findings suggest that the positive association between quota adoption and subsequent increases in health spending (as shown in Model 1) is being driven in part by the cases in which women's representation increased substantially following quota adoption. The findings provide additional support to H3. Substantively, the coefficient associated with high quota shock cases in Model 5 (1.13) indicates that countries which implement high-magnitude quotas increase the percentage of government expenditures dedicated to public health by over one percentage point in the post-quota period relative to non-quota-adopting countries over the same period of time. As the mean across all country-years is 10.8 percentage points, this represents a non-trivial increase in the post-quota period for this subset of high-quota shock countries.

Our findings indicate that quotas which substantially increase women's parliamentary representation are followed by significant increases in the percentage of government expenditures dedicated to public health. We now turn to modeling how our three main quota measures are associated with subsequent changes in education and military spending (again measured as a percentage of total government spending). When controlling for potential confounders, the analysis indicates that quota adoption, quota implementation, and quota shocks are all positively associated with education expenditures but negatively related with military spending (as percent of government spending); however, these results – which we present in Online Appendix Table OA1 – are statistically insignificant across all model specifications.¹² As a consequence, we conclude that gender

¹² We have a smaller sample size for the models on education and military spending. To address the possibility that the difference in significance is due to sample size, we replicate our health spending finding using only the subset of country-years included in the education and military

quotas are unrelated to education spending and military spending, respectively. Thus, in line with our expectations, the increase in government spending that we identify following quotas is only targeting public health, i.e. a policy area that appears to be more prioritized by women, both citizens and elites, than by men.

4.2 Where Do Increases in Health Spending Come From?

So far, we have found evidence that governments that adopt high-impact quotas (those which lead to significant increases in women's representation) subsequently dedicate a larger proportion of their national budgets to public health. This provides evidence that quotas displace funds from other policy areas and redirect them towards the health sector. These results, however, do not tell us where these funds are being redirected from. Nor do they preclude the possibility that governments *also* dedicate more funding to public health by increasing the size of the national budget. In this section, we qualify our main finding about quota shocks and subsequent increases in health spending by addressing these issues.

To examine which areas of the budget are depleted as the percentage of government expenditures dedicated to public health increases, we examine how quota shocks are associated with changes in health spending *relative* to other sectoral spending. Specifically, we model how our measure of quota shocks is associated with changes in the ratio of health spending as compared to military, education, and other residual spending categories, respectively. We do this through a series of seemingly unrelated regression (SUR) models. More technically, each SUR model takes as the relevant dependent variable the logged ratio of the percentages of government expenditures dedicated to each spending category pair (see Philips, Rutherford, & Whitten, 2016).¹³ By varying the numerator in each pair

spending models, respectively. The coefficient for our quota depth specification slightly increases in magnitude, and retains significance below the 0.10 level.

¹³ We use STATA's Clarify package (Tomz, Whittenberg, & King, 2003)

but maintaining the base category (i.e. health spending), we examine how quota shocks are associated with the changing composition of health spending relative to each other category. For instance, if a country decreases its military expenditures from 15 percent of the national budget to 10 percent and at the same time increases health spending from 10 percent to 20 percent, the change in ratios associated with these spending choices moves from 1.5 (15/10) to 0.5 (10/20). An additional advantage of the SUR framework is that it allows us to run all three models simultaneously (education, military, and other spending relative to health spending) to account for the interdependent nature of spending choices. This strategy explicitly allows for the assumption that the choices governments make on military spending (relative to health), for instance, are almost certainly not independent from the choices regarding education spending (relative to health).¹⁴

The results of the SUR analyses are shown in Table 2. They reveal that increases in quota shocks are significantly associated with decreases in military spending and other spending categories *relative* to health spending, but not with relative decreases in education spending. The findings here are different from those concerning military spending as a percentage of total government expenditures (in Online Appendix, Table OA1). To understand this difference and to reconcile the two findings, consider that the latter operationalization of the dependent variable includes a stable denominator (the percent of military spending out of 100 percent of government expenditures) whereas the SUR-related operationalization has a changing denominator – the percent of government expenditures dedicated to health spending – which increases on average after quota adoption. In combination, our results reveal that increases in health spending are offset by simultaneous

¹⁴ Data on military and education spending contain about 60 percent of the observations available for health spending. To compare across budgetary categories, we choose to use Amelia II for R to impute missing values rather than list-wise delete observations with existing health spending values.

decreases in military spending – but that these decreases are not substantial enough to achieve statistical significance when measured relative to total government expenditures.

Table 2 about here

Next, we test, albeit indirectly, the possibility that governments also dedicate more funding to public health by increasing the size of their national budgets. More specifically we examine whether quota shocks are both associated with increases in total health expenditures and followed by increases in total government expenditures (both measured as a percentage of GDP). If quotas also increase government spending on public health by adding to total spending, we should observe that quotas are both followed by increased health spending and by increased total spending. However, if quotas only increase health expenditures by reallocating funds to health and away from military spending and other sectors, we should not observe a change in total spending following the adoption of high-impact quotas. Table OA2 in the Online Appendix displays these results. We find that quota depth is associated with increases in total health spending and with increases in total government spending. As such, we cannot rule out the possibility that quotas both serve to increase health spending through the reprioritization of public finances towards health *and* through an increase in total spending.

5. Discussion

Our analysis of electoral gender quotas and government spending priorities worldwide provides a consistent main result: governments that adopt quotas that lead to significant increases in women's representation subsequently dedicate a larger proportion of their national budgets to public health. This finding suggests that quotas are most effective when they most disrupt previous patterns of male overrepresentation, implying that quota-elected women can act as important agents of political reform. In line with our expectations, quotas

are most effective in the area of public health, a policy area that has been historically prioritized by women.

We qualify our main finding in important ways: Increases in health spending following high impact quotas appear to be the result of a relative decrease in military spending and other spending categories (but not in education spending) and may *also* be the result of an increase in total spending. Our finding that quotas redistribute finite financial resources in largely zero-sum budgetary decisions has important theoretical implications. This result suggests that quotas, by diversifying parliaments, can disrupt established power structures in essential legislative functions and redirect funds from an area generally more prioritized by men (defense) to a sector more commonly prioritized by women (public health).

Our work also points to several outstanding questions that we hope will prompt future research. First, our findings support previous work that has connected improvements in health – and increased health spending – to gender equality reforms such as women’s enfranchisement (Miller, 2008) and increased gender equality in parliaments (e.g. Bhalotra & Clots-Figueras, 2014; Chen, 2010; Swiss et al., 2012). However, there is less consensus regarding *why* these patterns consistently emerge. It is possible that our results stem from funds that are being redistributed to areas within the health sector that predominately affect women – such as women’s maternal or reproductive health – in which case, our findings would fit well into the framework of women’s substantive representation (see e.g. Westfall & Chantiles, 2016). Another possibility is that women legislators prioritize health because it is a relatively uncontentious policy area; one they can safely claim as political newcomers. Both of these possibilities deserve further investigation.

Second and somewhat relatedly, although our findings support the claim that MPs’ priorities are gendered, our analysis is not able to pinpoint at which point in the representative process these preferences are formed and expressed. Previous work on this

subject has largely attributed women citizens' prioritization of health to their disproportionate role worldwide as caretakers, both within their households and larger communities (see also Bhalotra & Clots-Figueras, 2014; Gottlieb, Grossman, & Robinson, 2016). An important extension of our work, then, might involve mapping gender differences in MP priorities to gender differences within their electorates, both related to health as well as other potentially gendered priorities.

A third puzzle revealed by our findings relates to the specific mechanisms through which quota-elected representatives are able to influence national budgets. We theorized that women MPs might affect spending decisions during informal negotiations with key decision-makers, through the increased supply of women MPs eligible for relevant committees, and/or during chamber wide budgetary votes. Our finding that quotas matter for health spending, however, does not tell us where during this process quota-elected representatives have the most impact. Quantitative data on committee membership or cabinet appointments in parliamentary systems before and after quota implementation, or process tracing of budgetary procedures in high quota shock cases, would certainly provide insight here. In addition, an analysis of mechanisms would also benefit from a focus on the ways men MPs' preferences and priorities may be reoriented by large leaps in women's representation. Quota shocks may "feminize" parliaments making men better at representing women or by enabling men MPs to speak about issues related to men's social experiences, such as mental health, which were previously discouraged by overly masculinized legislative cultures (Lovenduski, 2005; Murray, 2014).

Finally, our analysis calls for more theoretical and empirical work on how the rich institutional variation among quotas may moderate their potential legislative consequences. For instance, candidate quotas and voluntary party quotas are typically adopted in proportional representation systems, meaning that quota-elected MPs are not identifiable vis-à-vis other legislators when they are elected through candidate lists. This may grant

women in these systems more influence and less stigmatization than “quota women” in reserved seat systems typically adopted in first-past-the-post electoral systems (see Clayton, 2015). Alternatively, however, women in reserved seats who are directly elected by constituencies may have more legislative autonomy to act outside the wishes of party leaders than women elected through party lists. These countering expectations have yet to be thoroughly examined in the quota literature.¹⁵ Further, other institutional rules and constraints in which quotas are embedded – such as the level of democracy or the strength and nature of the ruling party – almost certainly affect the ability of quota-elected MPs to influence legislative decisions. Our findings here call for more comparative work on the circumstances in which quotas affect parliamentary outcomes.

6. Conclusion

In political debates across the globe, and within the normative literature on gender and political representation, gender quotas are often justified as means to effect policy change by including women’s interests and priorities in the policy process (Bacchi, 2006). Here we have provided the most comprehensive study to date to theorize and test the legislative consequences of the recent, rapid, and global expansion of electoral gender quotas. Our results suggest that electoral engineering through quota policies may be an effective tool not only to diversify parliaments but also, at least to some extent, to change political priorities in areas favored by historically marginalized groups.

By showing that the identity of legislators is important for governments’ spending priorities, our theoretical framework and findings are potentially important for reform

¹⁵ Because the sample size for each quota type is relatively small in our data, when we run the specifications presented in Table 1 on each type separately, no quota type achieves statistical significance on its own.

processes targeting other historically underrepresented groups, such as ethnic, racial, and sexual minorities. Quotas may be one tool of many to this end and other efforts, such as electoral financing to support underrepresented groups, may have similar effects if they are able to substantially increase the presence of these groups in legislative bodies. Finally, our analysis has important implications for understanding public health outcomes. Research suggests that below a certain threshold of health expenditures, countries are unable to meet basic health needs (Ghobarah, Huth, & Russett, 2004). The implications of women's representation, then, may extend beyond the political sphere, as greater inclusion of women's preferences in political decision-making has the potential to improve other aspects of human development.

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Associated Tables and Figures

Quotas Adoptions: 1995 - 2012

Figure 1: Cases of countries which adopted quotas between 1995 and 2012. Reserved seat quotas in black, candidate list quotas in dark grey, and voluntary political party quotas in light grey.

Figure 2: Quota shocks: increase in women's parliamentary representation before and after the quota policy's implementation

Figure 3: Education and military spending as percentage of the total government expenditures among quota cases, centered on adoption year

Figure 4: Health spending as percentage of total government expenditures among all quota cases and high quota shock cases, centered on adoption year

Table 1: Effects of quota timing on subsequent health spending (1995 - 2012).

Quota IV Measure	Adopt	Implement	Shock	High Shock	Low Shock
	(1)	(2)	(3)	(4)	(5)
Quota	0.718*	0.215	0.0564*	1.163*	0.258
	(0.345)	(0.394)	(0.027)	(0.549)	(0.344)
Democracy	0.347	0.364	0.339	0.391	0.366
	(0.451)	(0.460)	(0.456)	(0.459)	(0.462)
Agg. Democracy	0.0827 ⁺	0.0812 ⁺	0.0854 ⁺	0.0923 ⁺	0.0810 ⁺
	(0.046)	(0.046)	(0.046)	(0.047)	(0.047)
Autocracy	-0.378	-0.467	-0.410	-0.402	-0.472
	(0.372)	(0.379)	(0.364)	(0.361)	(0.388)
Leftist Ruling Party	0.0367	0.0117	0.00595	0.0339	0.0218
	(0.317)	(0.319)	(0.320)	(0.316)	(0.314)
Agg. Leftist Party	-0.0224	-0.0225	-0.0173	-0.0228	-0.0242
	(0.055)	(0.055)	(0.055)	(0.054)	(0.055)
log(ODA)	0.0649	0.0748	0.0698	0.0737	0.0731
	(0.050)	(0.050)	(0.050)	(0.050)	(0.049)
log(Oil Rents)	-0.118 ⁺	-0.123 ⁺	-0.122 ⁺	-0.117 ⁺	-0.122 ⁺
	(0.062)	(0.065)	(0.065)	(0.066)	(0.064)
log(Trade)	0.0845	0.0883	0.0903	0.0727	0.0814
	(0.233)	(0.240)	(0.234)	(0.234)	(0.240)
log(FDI)	0.0241	0.0335	0.0263	0.0141	0.0366
	(0.080)	(0.081)	(0.079)	(0.080)	(0.078)
Intercept	-20.86	-34.35	-25.73	-23.87	-34.90
	(44.314)	(44.190)	(43.942)	(43.804)	(43.395)
Country & Year FE's	✓	✓	✓	✓	✓
Nonlinear Time Trend	✓	✓	✓	✓	✓
<i>N</i>	2227	2227	2227	2227	2227
Number of Countries	139	139	139	139	139
<i>R</i> ²	0.772	0.771	0.772	0.773	0.771
<i>BIC</i>	9722.3	9735.3	9722.8	9717.5	9735.5

Notes: OLS Regression Results. Health spending is measured as the percent of total government expenditures. Standard errors clustered by country. All covariates lagged one year.

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 2: Effects of quota shock depth on subsequent compositional spending (1995 - 2012).

Variable	Coefficient	(Std. Err.)
(1) DV: log(% Edu. Spending / % Health Spending)		
Quota	0.003	(0.002)
Democracy	-0.017	(0.030)
Agg. Democracy	-0.001	(0.003)
Autocracy	0.037	(0.040)
Leftist Ruling Party	-0.010	(0.024)
Agg. Leftist Party	0.004	(0.003)
log(ODA)	-0.002	(0.005)
log(Oil Rents)	0.012*	(0.005)
log(Trade)	-0.031	(0.024)
log(FDI)	-0.009	(0.007)
UN Peacekeeping	-0.214**	(0.060)
log(Battle Deaths)	0.000**	(0.000)
(2) DV: log(% Mil. Spending / % Health Spending)		
Quota	-0.026**	(0.005)
Democracy	0.031	(0.068)
Agg. Democracy	-0.012 [†]	(0.006)
Autocracy	-0.041	(0.090)
Leftist Ruling Party	-0.060	(0.055)
Agg. Leftist Party	0.001	(0.008)
log(ODA)	-0.005	(0.011)
log(Oil Rents)	0.017	(0.012)
log(Trade)	0.163**	(0.054)
log(FDI)	-0.012	(0.016)
UN Peacekeeping	0.086	(0.136)
log(Battle Deaths)	0.000*	(0.000)
(3) DV: log(% Other Spending / % Health Spending)		
Quota	-0.005*	(0.002)
Democracy	-0.043	(0.030)
Agg. Democracy	-0.012**	(0.003)
Autocracy	0.073 [†]	(0.040)
Leftist Ruling Party	0.020	(0.024)
Agg. Leftist Party	0.002	(0.003)
log(ODA)	-0.013*	(0.005)
log(Oil Rents)	0.005	(0.005)
log(Trade)	-0.062**	(0.024)
log(FDI)	-0.006	(0.007)
UN Peacekeeping	-0.077	(0.060)
log(Battle Deaths)	0.000**	(0.000)
N	2206	

Notes: SUR Regression Results. Spending categories are measured as the percentage of total government expenditures. All covariates lagged one year. Intercepts, country and year fixed effects, and non-linear time trend calculated but not reported.

[†] $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table OA1: Effects of quota timing on subsequent military and education expenditures (1995 - 2012).

Spending area Quota IV Measure	Military Adopt (1)	Military Implement (2)	Military Shock (3)	Edu. Adopt (4)	Edu. Implement (5)	Edu. Shock (6)
Quota	-5.979 (4.915)	-0.745 (0.915)	-0.0968 (0.097)	0.384 (0.652)	0.0675 (0.694)	0.0561 (0.059)
Democracy	-5.606 (6.224)	-5.395 (6.189)	-5.445 (6.163)	-0.127 (0.668)	-0.131 (0.670)	-0.123 (0.661)
Agg. Democracy	-0.0666 (0.196)	0.0143 (0.148)	-0.00600 (0.138)	0.0334 (0.075)	0.0318 (0.076)	0.0406 (0.073)
Autocracy	-1.520 (3.029)	-0.611 (2.098)	-0.699 (2.116)	-0.712 (0.991)	-0.774 (0.998)	-0.688 (0.996)
Leftist Ruling Party	0.764 (1.171)	0.611 (0.975)	0.618 (0.992)	-0.149 (0.254)	-0.158 (0.252)	-0.173 (0.250)
Agg. Leftist Party	0.183 (0.232)	0.150 (0.220)	0.149 (0.217)	0.0887 (0.087)	0.0870 (0.088)	0.0939 (0.087)
log(ODA)	0.273 (0.204)	0.152 (0.126)	0.158 (0.129)	0.134** (0.051)	0.138** (0.051)	0.131** (0.048)
log(Oil Rents)	0.883 (0.824)	0.947 (0.883)	0.948 (0.881)	0.160 (0.136)	0.155 (0.136)	0.161 (0.136)
log(Trade)	-7.362 (8.214)	-7.588 (8.454)	-7.504 (8.470)	-0.346 (0.881)	-0.343 (0.889)	-0.425 (0.855)
log(FDI)	0.270 (0.238)	0.261 (0.255)	0.248 (0.253)	-0.109 (0.131)	-0.101 (0.129)	-0.117 (0.130)
UN Peacekeeping	-15.60 (11.919)	-15.44 (12.190)	-15.47 (12.186)			
Battle Deaths	0.001** (0.001)	0.001* (0.001)	0.001* (0.001)			
Intercept	-102.9 (301.742)	90.22 (147.243)	74.81 (142.558)	58.87 (86.287)	50.51 (87.697)	66.52 (87.220)
Country & Year FE's	✓	✓	✓	✓	✓	
Nonlinear Time Trend	✓	✓	✓	✓	✓	
<i>N</i>	1373	1373	1373	1318	1318	1318
Number of Countries	139	139	139	139	139	139
<i>R</i> ²	0.792	0.789	0.789	0.787	0.786	0.787
<i>BIC</i>	10967.0	10981.6	10981.3	6099.7	6101.2	6095.8

Notes: OLS Regression Results. Education and military expenditures are measured as the percent of total government expenditures. Standard errors clustered by country. All covariates lagged one year. ⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table OA2: Effects of quota shock depth on subsequent total health expenditures and total government expenditures (1995 - 2012).

Spending DV (as % of GDP)	Health	log(Total)
Quota IV Measure	Quota Shock Depth	Quota Shock Shock
	(1)	(2)
Quota	0.395** (0.012)	0.005* (0.002)
Democracy	0.009 (0.115)	0.081+ (0.046)
Agg. Democracy	0.028+ (0.015)	0.011** (0.004)
Autocracy	-0.199 (0.192)	-0.065 (0.042)
Leftist Ruling Party	0.009 (0.020)	-0.038+ (0.020)
Agg. Leftist Party	0.015 (0.004)	0.004 (0.004)
log(ODA)	0.032* (0.014)	0.010* (0.005)
log(Oil Rents)	-0.019 (0.018)	0.006 (0.008)
log(Trade)	0.046 (0.079)	0.110* (0.051)
log(FDI)	-0.010 (0.025)	0.016* (0.008)
Intercept	-27.037* (12.187)	12.409** (4.033)
Country & Year FE's	✓	✓
Nonlinear Time Trends	✓	✓
<i>N</i>	2233	2233
Number of Countries	139	139
<i>R</i> ²	0.907	0.820
<i>BIC</i>	4138.059	-1148.995

Notes: OLS Regression Results. Health expenditures and total government expenditures are measured as a percent of GDP. Standard errors clustered by country. All covariates lagged one year. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table OA3: Effects of quota timing on subsequent health spending (1995 - 2012) excluding voluntary political party quotas

Quota IV Measure	Adopt	Implement	Shock	High Shock	Low Shock
	(1)	(2)	(3)	(4)	(5)
Quota	0.637 ⁺ (0.337)	0.0552 (0.399)	0.0470 ⁺ (0.025)	0.896 ⁺ (0.492)	0.273 (0.378)
Democracy	0.337 (0.462)	0.364 (0.472)	0.349 (0.467)	0.375 (0.470)	0.355 (0.473)
Agg. Democracy	0.110* (0.051)	0.109* (0.051)	0.112* (0.051)	0.116* (0.052)	0.108* (0.051)
Autocracy	-0.495 (0.366)	-0.595 (0.374)	-0.525 (0.359)	-0.526 (0.357)	-0.576 (0.386)
Leftist Ruling Party	0.0192 (0.343)	-0.00336 (0.345)	-0.00562 (0.345)	0.0107 (0.342)	0.00704 (0.341)
Agg. Leftist Party	-0.0167 (0.056)	-0.0172 (0.056)	-0.0128 (0.056)	-0.0178 (0.055)	-0.0180 (0.056)
log(ODA)	0.0618 (0.050)	0.0714 (0.050)	0.0667 (0.050)	0.0700 (0.050)	0.0682 (0.049)
log(Oil Rents)	-0.118 ⁺ (0.063)	-0.122 ⁺ (0.066)	-0.121 ⁺ (0.066)	-0.117 ⁺ (0.067)	-0.121 ⁺ (0.065)
log(Trade)	0.0411 (0.225)	0.0411 (0.229)	0.0468 (0.225)	0.0354 (0.227)	0.0363 (0.229)
log(FDI)	0.0490 (0.080)	0.0595 (0.081)	0.0504 (0.080)	0.0410 (0.080)	0.0611 (0.079)
Intercept	1.603 (45.592)	-12.23 (45.275)	-3.630 (45.172)	-3.201 (45.275)	-9.600 (43.158)
Country & Year FE's	✓	✓	✓	✓	✓
Nonlinear Time Trend	✓	✓	✓	✓	✓
<i>N</i>	2115	2115	2115	2115	2115
Number of Countries	132	132	132	132	132
<i>R</i> ²	0.769	0.768	0.769	0.769	0.768
<i>BIC</i>	9269.9	9279.9	9271.2	9269.5	9278.9

OLS Regression Results. Health spending is measured as the percent of total government expenditures. Standard errors clustered by country. All covariates lagged one year.

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

As an additional test, we run a series of models which include women's parliamentary representation as the key independent variable rather than the difference-in-difference approach we employ with quota adoption and implementation above. Using an OLS specification with fixed effects and a lagged dependent variable, we find a very strong and robust association between increases in women's parliamentary representation and subsequent increasing in public health spending. See Appendix Table OA4 below. Although not as well identified as our approach in the main text, this finding provides additional evidence to support H3; that is, that women's increased presence is driving spending patterns rather than quota adoption *per se*. When we remove the country and year fixed effects and interact quota adoption or quota implementation with women's descriptive representation, the interaction terms are not significant – providing further evidence that quotas mainly function through increasing women's presence in political decision making rather than through a separate institutional effect (tables available on request).

Table OA4: Effects of women's parliamentary representation on subsequent health spending (1995 - 2012).

Health Expenditures	% of GDP	log(% of GDP)	% of Total Expenditures
	(1)	(2)	(3)
Lagged DV	0.697** (0.029)	0.629** (0.018)	0.424** (0.159)
% Women	0.008** (0.003)	0.003** (0.001)	0.001** (0.001)
Democracy	-0.042 (0.055)	-0.016 (0.017)	0.004 (0.008)
Agg. Democracy	-0.003 (0.058)	-0.005** (0.002)	0.001 (0.001)
Autocracy	0.058 (0.047)	-0.026 (0.034)	-0.005 (0.009)
Leftist Party	0.026 (0.032)	0.018 [†] (0.011)	0.005 (0.006)
Agg. Left Party	0.015** (0.007)	0.003 (0.003)	0.001 (0.001)
log(GDP)	0.003 (0.063)	-0.076 (0.092)	-0.006 (0.009)
log(ODA)	0.007 (0.006)	0.002 (0.003)	0.001 (0.001)
log(Oil Rents)	0.009 (0.008)	0.001 (0.003)	-0.001 (0.001)
log(Trade)	-0.033 (0.047)	-0.057 [†] (0.032)	0.014 (0.011)
log(FDI)	-0.018 (0.014)	-0.010 (0.032)	-0.001 (0.001)
Country & Year FE's	✓	✓	✓
Nonlinear Time Trends	✓	✓	✓
N	1778	1778	1778
R ²	0.966	0.942	0.898

Notes: OLS Regression Results. Standard errors clustered by country. All covariates lagged one year.

⁺ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$