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The Effects of Gender Quotas in Latin American National Elections

Andreas Kotsadam[a] and Måns Nerman[b]

Abstract

This study investigates the effects of gender quotas in national elections on political participation, public policy, and corruption in Latin America. We are able to replicate the findings from previous research that women in politics do affect these outcomes, but only when we treat the number of women in parliament as exogenous. We argue, however, that the introduction of gender quotas caused an – in this context – exogenous increase in women's representation, and while we find that quotas in Latin America increased the number of women in parliament, we find no substantial effects beyond mere representation. The mechanisms for these findings are scrutinized, and we find no indications that quota women are more marginalized than other elected women in Latin American parliaments. Hence, increasing women's representation by means of gender quotas may not result in the same outcomes as an increased representation in non-quota elections.

Keywords: Gender quotas, Latin America, Women in Parliament

JEL: H50, D72, Z18

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1 Introduction

Women account for half of the world's population, but hold just short of one-fifth of the parliamentarian seats worldwide. Fifteen years ago the same figure stood at just eleven percent (IPU statistical archive). Over these years, affirmative actions have been introduced in more than 100 countries around the world in order to increase the number of women in politics, either by governments or by political parties of own accord. In terms of legal electoral quotas, Latin America has been in the forefront; the first democratic country to adopt a gender quota in national elections was Argentina in 1991, and throughout that decade many Latin American countries followed suit (Gray 2003). As of today, eleven of the eighteen countries in Latin America have introduced gender quotas in national elections, making Latin America the region in the world with the highest share of quota countries.

The introduction of quotas to increase women's participation raises the question of what effects we can expect from an increased political power for women. The arguments usually put forward for quotas are based on justice, women's experiences, women's interests, and the importance of female politicians as role models (Dahlerup 2003). In particular, it is argued that women have different biological or socially constructed experiences than men, or even that men and women have conflicting interests, making it likely that they also inhibit different preferences, and empirical research confirms this (e.g., Lott and Kenny 1999; Edlund and Pande 2002; Edlund et al. 2005; Funk and Gathmann 2008; Miller 2008; Finseraas et al. 2012). If preferences are different, it is also possible that they may not be fully taken account of without proper female representation. Again, empirical evidence seems to support this; results from cross-sectional comparisons show that increased political representation of women is correlated with different spending priorities (e.g., Thomas 1991; Besley and Case 2003) and that there is a correlation between increased female representation and less corruption (Dollar et al. 2001; Swamy et al. 2001).

These results rarely imply causation though. One obvious concern is that women may be better represented in areas where voters have specific political preferences or that less corrupt societies elect more women. A few studies have used econometric techniques that help identify causal effects of women in politics: Clots-Figueras (2011) studies close elections between women and men in India and finds that elected women invest more in education, Rehavi (2007) finds that increasing the number of women in politics led to increased public welfare expenditure in the U.S. during the 1990s, and Svaleryd (2002) uses longitudinal data and finds that Swedish municipal boards with increased female representation tend to increase spending on child care relative to spending on the elderly.

However, few studies have utilized the introduction of gender quotas in politics to look at the effects of policies. Chattopadhay and Duflo (2004) use the randomized introduction of a gender quota at the local level in India, and find that policies on issues closer to the preferences of women were implemented more often in villages with female chiefs, and Beaman et al. (2009), using the same setting as Chattopadhay and Duflo (2004), find changing gender norms following the quota. Hence, there are claims of various effects of women's representation. However, given that many gender norms and roles are highly context specific, one can hardly expect to always find the same effects in different societies. Furthermore, it is likely that different kinds of quotas at different levels of governance also produce differing results, as decision making procedures, policy responsibilities, and closeness to the electorate may differ.

In the present article, we aim to identify the impacts of the increased number of female politicians in national parliaments caused by the extensive introduction of quotas for women in Latin American countries. Specifically, we look at the impacts on a group of outcomes that previous research has suggested that women in politics should affect, including corruption, women's political participation, and policies on education and health. By using national-level rather than more local-level quotas this study will broaden the knowledge of the effects of quotas. Furthermore, the social context in the Latin American countries likely differs from that in other countries previously investigated. Given the potential impact and political sensitivity of the introduction of quotas in these countries, studying their effects is of great importance.

Our results show that while the quotas substantially increased the number of women in parliament, they had no measurable effects on policy, political participation, or corruption. However, we find that estimations not utilizing the quota introduction, instead looking at variation in the share of women in parliament not caused by quota measures, often show correlations between female representation in parliament and the mentioned outcomes. This seems to imply either that women elected through quotas differ from other elected women, or that the correlations between women in parliament and our outcomes are spurious. In either case, it suggests that quotas have been ineffective with respect to these issues in Latin America. Further analysis also shows that the quotas did increase the share of women in ministerial positions, suggesting that quota parliamentarians are not more marginalized than other elected women.

2 Theory and Expectations

This section presents theory on how quotas may be effective in changing policy, how and why quotas were introduced in Latin America, and whether one can consider them exogenous to other developments. Based on these discussions and the existing literature, we then discuss what effects of quotas we can expect to find in the Latin American context.

2.1 Theory on Quota Effects

The motivations for implementing gender quotas point to several mechanisms through which quotas are thought to have effects beyond representation. Dahlerup (2003) presents four arguments: a *justice argument*, implying that women have the right to half of the representative seats as they constitute half of the population; an *experience argument*, implying that the gender-specific experiences of women, whether biological or socially constructed, need to be represented; an *interest group argument*, arguing that men and women may have conflicting interests and, consequently, men cannot represent women; and a *role model argument*, where the existence of female politicians is thought to help other women engage in politics. Araújo and García (2006) add the argument that a higher female representation may lend further legitimacy to the democratic system and its institutions.

While the justice argument is more concerned with representation per se, having different experiences or interests implies that men and women may have different preferences over policy. However, despite differences in preferences, the identity of the policymaker need not affect policy. This is maybe most famously formulated in Downs' (1957) median voter theorem, which predicts that as long as candidates can commit to policies, political competition leads to an allocation that is preferred by the median voter irrespective of the identity of the politician. There is nevertheless ample evidence that the identity of the politician can indeed have an effect on policy: Pande (2003) and Besley et al. (2004) find that political reservations in local governments for disadvantaged castes in India affected the provision of public goods, and Chattopadhyay and Duflo (2004) and Clots-Figueras (2011) present similar findings for reservations for women. These effects can be better explained in alternative models such as the citizen candidate model, where political candidates cannot completely commit to a policy platform, and if they get elected they try to implement their own political agenda (e.g., Besley and Coate 1997; Chattopadhyay and Duflo 2004).

So when should we expect to find effects of gender quotas on policy? Duflo (2005b) presents three necessary conditions for gender quotas to affect the provision of public goods. Firstly, the quotas need to be effective in raising the number of elected women. If not, they have not had an effect on the decision making bodies. Secondly, policy preferences must differ between men and women, as there would otherwise be no reason to expect female politicians to behave differently than their male colleagues. Thirdly, the identity of a policymaker must affect his or her decision, or women's differential preferences would already have been represented (albeit by male politicians).

Moving away from public goods, the identity of a politician may be even more important if the goods that he or she allocates are low spillover goods, i.e., goods that mainly benefit their direct beneficiaries, since it may increase the probability of receiving such goods for the group that the politician identifies with (Besley et al. 2004). This argument has been put forward with respect to ethnic or geographically clustered groups that may benefit from, e.g., a well, yet when it comes to gender, the degree of spillover is not as clear. In fact, viewing the household as one decision maker or a unit that maximizes total household welfare and where the partners can commit to cast their votes in a particular way, it is unclear whether there should be a difference in political preferences between the sexes. However, the household as one unit often does not seem to adequately describe reality, since a household usually does not behave as an efficient unit (e.g., Duflo and Udry 2004; Duflo 2005a). Furthermore, studies of political preferences systematically do find differences between the sexes (e.g. Lott and Kenny 1999; Edlund and Pande 2001).

2.2 Quotas in Latin America and Expected Effects

Latin America is the region in the world where electoral quotas have been implemented most extensively. Argentina was the first democratic country in the world to have a national electoral quota in 1991 and many countries followed suite in the late 1990s. Most countries introduced their first quotas in 1996 or 1997, and the spread of quotas in the region indicates a strong contagion effect (Escobar-Lemmon and Robinson 2005). Table 1 shows the years of first quota introduction in Latin American countries.

2.2.1 What Determined Quota Introductions in Latin America?

Utilizing the introduction of gender quotas in order to identify effects of women in politics raises the question of what determined the timings of quota introductions in Latin America. This is important if one considers to what extent the quota introductions were exogenous to changing gender equity norms in society. Previous authors have argued that recommendations from international organizations, such as the UN, the EU, the Inter-Parliamentary Union (IPU), and the OSCE have been highly influential in the introduction of quotas in Latin America (e.g., Dahlerup 2003), and so have international agreements, in particular the Platform for Action in 1995 at the fourth Women's World Conference in Beijing (Htun and Jones 2001; Schwindt-Bayer 2009; True and Mintrom 2001). This platform urged governments to ensure equal access and full participation in political decision making, and the introduction of gender quotas was suggested as a specific measure. This is the reason, Peschard (2003) argues, why so many Latin American countries introduced quotas in 1996 and 1997. In fact, the only countries to pass laws on quotas before the Beijing conference were Argentina and Bolivia. Lubertino (2003) argues that a crucial factor for the implementation of quotas in Argentina was the participation of Argentinean women in the UN Women's Conference in Nairobi in 1985.

But why have only some Latin American countries introduced quotas? To answer this question, it is insightful to look at the countries that currently do not have any quotas. For instance, gender quotas have been deemed unconstitutional in some countries. This happened in both Uruguay in 1988 and in Colombia in 2000, and in Venezuela the quota law introduced in 1997 was declared unconstitutional in 2000 and subsequently removed (quotaproject.org). However, Uruguay has now passed a new electoral law stipulating a 33 percent quota that will go into effect in the 2014 elections. In Chile, the former president Michelle Bachelet proposed a gender quota bill that did not pass in the parliament, and in Guatemala a quota bill did not reach the required two-thirds majority needed for implementation. Nicaragua has a law on "the promotion of the necessary measures, in conformity with the Law of the matter, in order to establish a proportional percentage of women and men to the positions on the electoral lists of the national /…/ Parliament" (quotaproject.org), but no binding quota law. In El Salvador, women's movements have lobbied for a quota law but without success. Hence, it seems that most of the countries that have not introduced quota laws have come close to doing so, lending support to the idea of the pool of Latin American non-quota countries being a potential control group for the Latin American quota countries. We take this discussion a bit further in the empirical strategy section below, where we propose some quantitative tests for the exogeneity of the quota introduction.

2.2.2 What Effects Can We Expect from Quotas?

There is a lack of agreement in the literature with respect to which outcome variables are most likely affected by gender quotas (Wängnerud 2009). We therefore focus on a wide set of outcomes, and in order to identify key areas that we think may have been affected by the quotas we will rely on both theory and previous empirical literature. In addition, we will use survey data to help us get a picture of the Latin American context regarding gender and preferences. Below, we motivate the three main areas covered: government policies, women's political participation, and corruption.

2.2.2.1 Government Policy

The previous literature is full of suggestions about the effects of women in politics on government policy. Besley and Case (2003) report that male and female politicians in the US behave differently with regard to spending priorities, with women putting more focus on education and support for families and children. Clots-Figueras (2011) studies elections in India where either a man or a woman closely beat an opponent of the other sex, and similarly finds that women invest more in education. Rehavi (2007) finds that increasing the number of women in politics led to increased public welfare expenditure in the U.S. in the 1990s, and Svaleryd (2002) shows that more females in Swedish municipalities increase childcare expenditure relative to spending on the elderly. Although not dealing with female politicians per se, studies from the US have also found that granting suffrage to women increased both the size of the state (Lott and Kenny 1999) and health spending (Miller 2008), and in Switzerland female suffrage led to a small increase in the size of government and a larger shift in the scope of government toward social expenditures (Funk and Gathmann 2008). As mentioned earlier, Chattopadhay and Duflo (2004) find that policies on issues closer to the preferences of women were implemented more often in Indian villages with female chiefs (randomly allocated by a quota). On the other hand, Campa (2011), using a regression discontinuity design, finds no effects of a gender quota on spending in Spanish municipalities.

When investigating the effects of quotas it is important to contextualize the expected effects, as gender roles are social products and differ widely across the globe (Wängnerud 2009). This implies that gender gaps in political preferences are by no means fixed across time and space. For instance, in the OECD countries, women had more conservative political preferences than men and tended to vote for bourgeois parties in the 1960s (Campbell et al. 1960). Since then, women's political preferences have gradually shifted leftwards all over the OECD area, and in Scandinavia women are now more left-leaning than men (Inglehart and Norris 2000). A range of studies have revealed that gender gaps in political preferences are prevalent across the world and regarding a broad range of policies (e.g., Svallfors 1997; Alesina and La Ferrara 2005; Alvarez and McCaffery 2003; Lott and Kenny 1999; Aidt and Dallal 2008; Chattopadhyay and Duflo 2004), but they also show differences across countries and time periods. In Latin America, women are more likely than men to vote for conservative parties (Escobar-Lemmon and Taylor-Robinson 2005).

Therefore, we also expect the effects of quotas to be context dependent. Some differences between men and women are, however, universal in direction (although not in degree), such as women conducting more paid and unpaid care work than men and also bearing greater responsibility for childcare. An indisputable fact is also the difference between the sexes in reproductive capacity; i.e., women bear children and thus for example are more vulnerable to health risks associated with birth. One may therefore expect that policies that concern women more than men are more likely to change as a result of higher female political representation. Investigating the effects of the gender quota in Argentina, Franceschet and Piscopo (2008) find that it seems to have induced an increase in parliamentary bills concerning women's issues such as reproductive health and violence against women, but also that this increased attention did not carry over to changes in policy outcomes.

In less obvious cases, however, it is important not to take differences in political preferences between men and women for granted. As we expect that policies on issues closer to the preferences of women are more likely to be affected, we use the Latinobarometer survey (described in the data section) to get a picture of preference differences between men and women in Latin America[1]. We find that women are more inclined than men to think that the state is responsible for health care and for education. However, compared to men, women also to a greater extent perceive that taxes are too high. Hence, while spending priorities may very well differ depending on women's representation, we should not automatically expect the results from the US of a larger state to automatically carry over to Latin America.

2.2.2.2 Women's Political Participation

One part of the motivation of gender quotas in politics is that female politicians may act as role models and pave the way for other women in politics. Empirical evidence from India supports this view: Beaman et al. (2009) show that people's gender stereotypes weaken and that attitudes toward women as policymakers become less biased once they have been exposed to female leaders, supplying robust evidence that women's quotas may be effectively used to promote gender equality by reducing the gender bias of societal norms. An important question is whether this result can be generalized to other contexts and to national-level quotas.

Again consulting the Latinobarometer data[2], we find clear differences between men and women in political participation in Latin America prior to the quotas. In particular, women were less likely to participate in demonstrations and to vote, and they were more likely to state that politics is complicated, that they were not interested in politics, and that they did not engage in politics in general. Given these apparent inequalities, it is important to investigate whether more women in parliaments has affected women's political interests and/or ability to participate in the political sphere. Using the Latinobarometer data from 2005, Zetterberg (2009) shows that political participation is not higher in countries that have introduced quotas. While he also controls for lagged (from 1996) control variables, he does not fully exploit the data to include all years and does not make use of country fixed effects or control for trends in the variables of interest. As will be explained in the empirical strategy, we control for time trends and employ difference in differences (and also triple differences by exploiting men as another control group), and hence we regard our results as more credible in terms of identifying causal effects.

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[1] These results are available upon request.

[2] Again, the results are available upon request.

2.2.2.3 Corruption

Finally, we move on to investigate the effects on corruption, which is an area where previous studies have shown clear correlations between female representation and less corruption and have argued for a causal interpretation of these correlations as effects of women in parliament. Based on micro studies showing that women behave more altruistically and honestly than men, Dollar et al. (2001) put forward the hypothesis that more women in parliaments should lead to less corruption. Using data from the corruption index from the International Country Risk Guide (ICRG), they find support for their hypothesis in cross-country regressions and conclude that increasing the number of women in parliament is likely to reduce corruption. In a similar fashion, Swamy et al. (2001) first analyze data from the World Values Surveys and show that women on average have a more negative attitude toward bribe-taking. They then conduct a cross-country study using the Corruption Perception Index from Transparency International and also find a negative coefficient of women in parliament on corruption.

An inherent problem in these previous studies is that their identification strategies are vulnerable to omitted variables bias, a critique offered by, e.g., Goetz (2007). For instance, having elections that are more democratic, a higher level of transparency in the democratic institutions or a higher degree of political participation may lead to both less corruption and more women in parliaments at the same time. The previous studies acknowledge this problem and do include control variables (Dollar et al. 2001 control for civil liberties and Swamy et al. 2001 for political rights, both measures collected by Freedom House). In the present article, we instead exploit the introduction of quotas to investigate the effects of women in politics on corruption in Latin America.

Context is likely to be important also with respect to corruption. The previous studies pooled together around 100 countries and found a correlation between women in politics and corruption. While the causality of the relationship can clearly be contested, it is also likely that it masks heterogeneity across country groups. Alatas et al. (2011) make this point evident by conducting an economic experiment in Australia, India, Indonesia, and Singapore. They show that while women are less tolerant of corruption than men in Australia, there are no gender differences in the propensities to engage in and punish corrupt behavior in the other countries.

3 Data and Methodology

3.1 Data

The dependent variables are intended to cover our three main areas of investigation, namely policies, political participation, and corruption. The descriptions and sources of these variables, as well as of our independent variables of main interest, are presented in Table 2, and we discuss them below.

Using data from the Global Database of Quotas for Women (quotaproject.org) and the country-specific references therein, we classify a country as a quota country if it introduced some legal quota for women in its national parliament during our period of investigation, i.e., 1985-2009. We also create two variables representing the scope of the quotas in the lower (or only) house (*Q_size_lo*) and upper house of parliament (Q_*size_up*), measuring the share of candidates that are required to be women. They range from 0 for periods and countries with no quotas to 1 in the hypothetical case of a quota that requires all candidates to be women. The data on the share of women in parliament is taken from the *PARLINE database on national parliaments* from the Inter-Parliamentary Union (www.ipu.org). Again, we separate this measure into women in the lower house (*WiP_lo*) and upper house (*WiP_up*) of parliament. The data on quotas and women in parliament is available for all years of investigation. We also use data on the share of women ministers (*female_ministers*) as used by Escobar-Lemmon and Taylor-Robinson (2005). This data includes all our countries for all years of democracy from 1980 to 2003.

To operationalize the effects of quotas on policies, we first look at government consumption as a share of GDP. This measure, as well as the total tax revenues, should be a good proxy for the size of the state. We also investigate effects on spending on certain sectors of the economy, namely on education, health, social security, and social spending in general (which is the sum of the former three plus housing expenses). For these spending measures, we have data since at least 1990 from the World Development Indicators (WDI) and the Economic Commission for Latin America and the Caribbean (ECLAC).

To investigate the effects of quotas on political participation, we mainly use data from the Latinobarometer. The Latinobarometer is an annual survey (with a gap in 1999) that started in 1995 and now includes 18 countries in Latin America, with about 20,000 respondents per wave. In 1995, only 8 countries were included in the survey, but already in 1996 the number was increased to 17[3]. In total, we have 216,998 observations from 18 countries from the years 1995 to 2007. A disadvantage of this survey material is that for the group of countries that passed quota laws around 1997, there is only data for a few years prior to the first quota election. The variables we focus on in assessing the effects on political participation are frequency of talking about political issues with friends, political

[3] The only country under study not included in 1996 is the Dominican Republic, which was included for the first time in 2004.

interest, and whether the respondent would vote if there were an election the Sunday after the interview. We also look at confidence in the national congress and satisfaction with democracy, which are thought to reflect to what extent men and women are satisfied with the democratic system and its institutions. For all variables from the Latinobarometer, we use the (weighted) average for women in each country and year. Using micro-level data, it is also possible to create measures on the gender gap in participation, which in turn enables us to use the "difference in difference in differences" approach further elaborated on in the empirical strategy section below. All gap measures are created as the average value for men less the average value for women in the same year and country. As a complementary measure on political participation, we also include official figures on electoral turnout from the IDEA voter turnout database. Unfortunately, this data is not measured by gender, so we can only observe the aggregate development of men and women before and after quotas.

To measure corruption we use the corruption component of the International Country Risk Group's index on political risk (ICRG), which is a module that assesses corruption in the political system. It may sound a bit too wide in scope to be connected to the number of women in parliament, but is in fact the measure used in the previously discussed study by Dollar et al. (2001).

3.2 Empirical Strategy

In order to investigate the effects of quotas on the outcomes described above, and to assess the actual mechanism of increased women in parliaments, we proceed in several steps in our empirical investigation, as described below. Since a vital part of this study relies on quotas being effective in raising the number of women in national parliaments and on their introduction being exogenous in settings where the share of women in parliament is not, we will start by discussing how to assess these assumptions. We then move on to describe our strategy for estimating the effects of gender quotas on other variables.

3.2.1 The Effect of Quotas on the Number of Women in Parliament

Though investigated in previous literature, it is not evident to what extent gender quotas in general have actually increased the number of women in parliaments in Latin America. Whereas some countries in other parts of the world have introduced gender quotas in the form of reserved seats guaranteeing women a specific share of the seats in parliament, the quota countries in Latin America have all introduced so-called list quotas where a specified share of all party candidates must be women. This makes the actual outcome in terms of representation uncertain. Moreover, the countries within the region also differ in terms of whether voters are free to choose which candidates on a party's list to vote for or whether the orders of candidates are fixed (open vs. closed lists), whether there is a placement mandate implying that parties have to place women on certain (more electable) positions on the lists, and to what degree there are sanctions against parties that do not comply with the quota law. Jones (2009), looking at data at the election district level in Latin America, shows that in Latin America the effects of national election quotas on representation vary depending on both quota and election rules, as well as district size. The main conclusion is that as long as there is enforcement, both open and closed list quotas are effective although closed lists are slightly more so. As dividing quotas into categories may create almost as many categories as there are elections, we keep the analysis simple by defining only the size of the legal quota irrespective of the rules surrounding it.

Hence, in order to see to what extent quotas have been effective, we start by estimating the effect of quotas on the share of women in national parliaments, which will serve several purposes: firstly, it will help us understand to what extent quotas in general have been effective in Latin America and hence guide us on how to interpret later results on quota effects; secondly, it will give us an empirical understanding of what the processes of quota implementations looked like; and thirdly, it will serve as a benchmark test of whether significant effects of quotas can be identified within our sample. The last point is important since if we cannot find significant effects of quotas on women in parliament we can not reasonably expect to find such effects on other variables either. This may be either because quotas have not been effective in raising the share of women in parliament (which would make the theoretical mechanism redundant), or because our sample is too small to find significant effects.

As our data is limited to the 18 countries in Latin America and to elections held since 1985, we try to keep the empirical model fairly simple. Still, at least two factors seem important to account for: that the countries start off with quite different shares of women in parliament before any quotas were introduced, and that the effects of the quotas have taken some time to come about in many countries. The latter is because laws were often not properly enforced or not designed sharply enough to have an effect initially. Over time, adjustments were made and laws were complied with to a larger extent.

To account for these factors, we use fixed effects estimations and hold the possibility open to include lagged values of the quota variable. Our benchmark estimation for the share of women in parliament for country *c* at time *t* thus takes the form:

$$WiP_{ct} = \alpha_c + f(t) + \lambda_1 \cdot Q\_size_{ct} + \lambda_2 \cdot Q\_size_{c,t-1} + \vartheta_{ct},$$

where *WiP* stands for the share of women in parliament and *Q_size* measures the scope of the quotas. In this setup, the underlying assumption of our identification depends on how we define the counterfactual time trend (i.e., the functional form of $f(t)$). We will use two alternative versions. First we will allow for year fixed effects $(f(t)=\theta_t)$, hence introducing year dummies common to all countries. This is a typical "difference-in-differences" (DiD) setup, where the underlying assumption is that all countries would follow the same path (albeit from different starting points) in the absence of quotas. Then we will also test our results by using country-specific time trends $(f(t)=\theta_c \cdot t)$, which implies that there is a linear time trend for each country and that the introduction of quotas will lead to deviations from that trend. This last setup allows for differences in development between countries even without quotas, but has the disadvantages of possibly introducing "too much" flexibility in a model applied to such a short panel as ours, and of being vulnerable to non-linearities in the time trends[4].

As a further robustness check, we will also try the alternative model

$$WiP_{ct} = \alpha_c + f(t) + \lambda_1 \cdot Q\_size_{ct} + \lambda_2 \cdot Q\_size_{ct} \cdot T_{ct} + \vartheta_{ct},$$

where T_{ct} is a time variable that starts at 1 the year after the quota introduction. This setup basically does the same as before but allows for a new time trend after the quota introduction, rather than just introducing a lag.

3.2.2 On the Exogeneity of Quota Introduction

If gender quotas affect our outcome variables of interest by increasing the number of women in parliament, then why do we not just observe the share of women in parliament? The answer is of course that the share of women in parliament in different countries is likely correlated with other variables such as what parties and politics the voters have opted for in the last election, or cross-country differences in gender attitudes. This makes the share of women in parliament an endogenous variable in most settings. Contrary to this, based on discussions in the previous literature, we have argued that the introduction of gender quotas is potentially exogenous in Latin America. To some extent this has to be taken on faith, although we are able to perform some tests of the endogeneity of the quota introductions.

[4] This last point may be serious as quotas are always implemented towards the end of the panel, making them prone to pick up effects that really should be attributed to a convex or concave time trend. However, the introduction of more flexible functional forms (such as entering a country-specific squared time term) would introduce a high level of collinearity between the time controls and the quota variables.

First, there may be reason to believe that there were different attitudes to women in politics in the quota and non-quota countries before the introductions of quotas, which would seriously question our identifying assumptions. We test this assumption by checking for any pre-quota differences in the number of women in parliament. This is done by regressing the number of women in parliament on a common time trend and its square[5], and a dummy, *Q_country,* for being a quota country (i.e., a country introducing quotas at some point in time). We only use observations on elections prior to any quota introductions.

$$WiP_{ct} = \alpha + \theta_1 \cdot t + \theta_2 \cdot t^2 + \beta \cdot Q\_country_c + \varepsilon_{ct}$$

If our estimation of β is statistically significantly different from zero, then the quota introductions were correlated with pre-quota levels of women in parliament and the assumption of exogeneity seems less reasonable.

Moreover, we exploit the fact that some of the quota countries are bicameral. If the introduction of quotas in the lower houses came about due to changes in the underlying factors that made ”women in parliament” endogenous in the first place, then the introduction of lower house quotas should also be correlated with the share of women in parliament in the upper houses. Hence, we are able to perform a placebo test by regressing the share of women in the upper house on the introduction of gender quotas in the lower house. Failing to find such correlations (given that we can find such correlations using the actual upper house quotas of course) would greatly strengthen the exogeneity assumption of the quota introductions.

3.2.3 The Effects of Quotas on Policy, Participation, and Corruption

Turning to our estimations of the effects of quotas on policy, political participation, and corruption, our empirical strategy differs a bit from that for the effects on women’s representation, as we now almost always have yearly data. We want to capture the long-term effects of quotas since they may not be immediate, the influence of new parliamentarians may increase with experience, and many of our explanatory variables may change only slowly. Hence, for any dependent variable y_{ct}, we estimate the following fixed effects model:

$$y_{ct} = \alpha_c + g(t) + \lambda_1 \cdot Q\_size_{ct} + \lambda_2 Q\_size_{ct} \cdot T + \varepsilon_{ct},$$

where *T* is a time variable that equals 0 in non-quota years and starts at 1 the year after the quota introduction. Hence, λ_1 will give us an estimate of the immediate effect of the quota introduction,

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[5] Note that in this case the time trend is common to all countries and the *Q_country* variable is stable over time, which makes controlling for a non-linear trend much less problematic than before.

while the interaction term allows the quota to give rise to a new trend in the outcome variable. Again, we start with a standard DiD setup with year fixed effects, so that $g(t) = \theta_t$. As discussed before, this setup is robust to common non-linearities in the trend and assumes the countries to have similar developments in the absence of quotas. As we now typically have more observations than in the estimations of women in parliament, we conduct a robustness check by allowing for both year fixed effects and country-specific time trends, so that $g(t) = \theta_t + \mu_c \cdot t$. The result of the latter setup is that we no longer rely on the outcomes of non-quota countries as counterfactuals, which makes effects harder to find but also less reliant on the similar trends assumption. Finally, for the variables based on micro-level data from the Latinobarometer (*Notvoting*, *Donttalkpolitics*, *Notinterestedpol*, *Noconf.congress*, and *Satisfieddemocracy*), we create gap measures (the average of males minus the average of females) in order to exploit yet another difference, thereby creating DDD (difference-in-difference-in-differences) estimators. Hence, in these cases the estimates of λ_1 and λ_2 will show us whether the quota caused women's development to differ from that of men.

3.2.4 Further Econometric Issues

Kezdi (2003) and Bertrand et al. (2004) caution against trusting estimates from DiD regressions in finite samples – estimations much like the ones described in previous sections – if there is reason to believe that there is serial correlation in variables and error terms. Given that our panel is fairly narrow (consisting of 18 countries), clustering of the standard errors at the country level to reduce this bias may perform rather badly. Instead, we use the Newey-West estimator for standard errors, assuming heteroskedastic error terms and serial correlation up to two periods back. As a robustness check, we also clustered the standard errors at the country level (results are available upon request), yet the interpretations of our results remained unchanged.

4 Results

4.1 Quota Effects on the Share of Women in Parliament

Table 3 presents results from regressions to establish the effect of introducing women quotas on the actual election outcomes in terms of the share of women in parliament. Columns 1 and 2 show a regression of the share of women in parliament (henceforth WiP) on a time trend common to all countries, and a quota dummy and the scope of the quota, respectively. We do not take into account the type of quota introduced, such as whether there were any placement mandates or whether there were open or closed lists, as we try to keep the specification as simple as possible due to the limited number of elections in the Latin American countries since 1985. In Column 1 we can see that, on average, there is a small yet positive and statistically significant time trend, implying that the share of women in parliaments increases by about 0.5 percentage points per annum in the absence of gender quotas. Imposing a gender quota, however, yields an increase of 0.22 of the value of that quota (Column 2). That is, a quota stipulating that at least 30 percent of the candidates in an election ought to be women raises the share of women in parliament by about 7 percentage points on average.

At a first glance, this may seem like a rather small figure. However, one should remember three things: that countries already had some women in parliament prior to the quota, that in many cases there is no placement mandate of candidates, and that voters may often choose whom to vote for among the candidates on the lists. Moreover, if one compares the effect of the quota to the time trend, the result of a 30 percent quota is equivalent to an increase of the share of women in parliament that would have taken more than 14 years to achieve without it.

Column 3 introduces year dummies rather than a linear time trend, making the parameter somewhat larger, while Column 4 introduces country-specific time trends, resulting in a decrease of the quota scope parameter to about half its size. However, it is difficult to say to what extent the time parameters pick up gradual increases of WiP in quota countries, as the model assumes an immediate and pertaining effect of the quota. This becomes evident in Columns 5 and 6, where we add a lagged value of the scope of the quota (i.e., the last election's quota) in order to take into account the fact that many countries revised and sharpened the rules of the quota to make sure that parties conformed to it. Indeed, the aggregated effect of the quota introduction is now a little larger than it was in the simpler estimations, and from the size of the parameters it is evident that the effect of a quota becomes much larger in time for the second election. Hence, it is important to recognize that the introductions of quotas to some extent have gradual effects over time. In Columns 7 and 8, we interact the quota variable with a time variable rather than using a one period lag, and obtain comparable results.

To save space, the results from identical estimations on the effects of quotas in the upper house are not shown, yet are available upon request. The results are the same as for the lower house, except that in many countries the upper house quotas were introduced at a later stage than for the lower house, so the design of the quotas had already been refined. Hence, lags and time interactions are less important. The total effects of the quotas are very similar to those of the lower houses though.

4.1.1 On the Exogeneity of Quotas

We have already discussed the qualitative evidence pointing to the fact that quota introductions in Latin America were relatively exogenous processes with a lot of international influence. Following our empirical strategy, Table 4 presents further quantitative evidence of this. First, in Column 1 we regress the share of women in the lower (or only) houses of parliament on a common time trend and a dummy for being a quota country, and include only data from elections where no quota was introduced (i.e., all elections in non-quota countries and only pre-quota elections in quota countries). The *Q_country* dummy then shows the difference between quota and non-quota countries prior to quota introductions. Column 2 does the same but includes a squared time trend. In both cases, the very small and statistically insignificant parameters of *Q_country* tell us that in terms of WiP, there are no measurable differences between the quota and non-quota countries prior to the quota introductions. Columns 3-6 do the same for the upper chambers and yield equivalent results.

As argued, it could still be that quotas were introduced in some countries because of some other underlying changes in these countries, and that these would have yielded changes in the share of women in parliament anyway. In order to test for this, we run placebo regressions by estimating the share of women in parliament in the upper houses on the quotas in the lower houses. If there are omitted variables that affect WiP in the lower house, they should arguably do so in the upper house as well, whereas an exogenous quota introduced in the lower house should have no effect in the upper house.

As is evident from these regressions in Columns 7-8, the introductions of quotas in the lower houses have no measurable correlation with WiP in the upper house. This holds even when not controlling for any upper house quotas (Column 7). Hence, if there are any changes in unobserved variables simultaneous with quota introductions, they do not seem to affect the number of women in parliament. Of course, this is no guarantee that any such simultaneous changes in unobserved variables do not affect other variables such as the dependent variables analyzed in the next section.

4.2 Quota Effects on Policy, Participation, and Corruption

This section comments on our results from the reduced form regressions of policy, political participation, and corruption.

4.2.1 Policy

Table 5 shows the results of regressing government spending on health, education, and social security as well as total social spending, all expressed in percent of GDP, on the scope of quotas in the lower (or only) house of parliament. All the policy tables follow the same pattern: each variable is regressed on the quota scope and the scope interacted with time (this time variable is 0 for all non-quota years and starts at 1 the year after the quota). The first column is the DiD setup and the second adds country-specific time trends. As is evident, we find very few significant results here. Health spending in the DiD setup has one significant parameter, but only at the 10 percent level.

Table 6 shows regressions of the size of government, measured as government consumption and tax revenue as shares of GDP. Here we see a few significant parameters, although they are quite sensitive to changes in the model specification. In the DiD setup, there is no statistically significant effect of quotas on government consumption, but the effect becomes negative and statistically significant over time when controlling for country-specific time trends. For tax revenue we note a statistically significant positive trend after the introduction of the quotas in the DiD setup, but this effect disappears when we add country-specific time trends. Whether one should trust the DiD or the country-specific time trend estimations is unclear a priori, but given the increases of the R squares, the country-specific time trends seem to add a lot of information. It can also be noted that the significant trend parameters mentioned point in the opposite directions of the (statistically insignificant) direct effects of the quota introductions.

4.2.2 Political Participation

Columns 1-2 of Table 7 show the results from regressing the share of women who report that they would not vote if there were elections today on the quota scope. There is no statistically significant effect on women's voting, and the same goes for the male-female gap of the same variable (Columns 3-4). A perhaps more reliable but also in a sense more crude variable is the actual voter turnout in elections (Columns 5-6). This is the aggregate effect on both men and women (i.e., it could be zero even if there are significant effects in different directions between the sexes). We find no statistically significant effect here either.

Table 8 shows the effects of quotas on variables related to self-reported political interest. The only statistically significant result is that of women becoming less and less interested in politics over time after a quota introduction (Column 6), which is contrary to expectations. However, the result does not hold in the DiD estimation (Column 5). Furthermore, the trend is not different from that of men (Columns 7-8), so if there is an effect of quotas it does not seem to differ between men and women.

Turning to satisfaction with democratic institutions, Column 2 of Table 9 seems to indicate that women lose confidence in congress over time after the quota introduction (although the initial effect is positive), and Column 4 indicates that they may be doing so at a faster rate than men. Perhaps this is indicative of disappointment over non-appearance of quota effects. In line with this, Column 5 also seems to indicate that women become less and less satisfied with democracy, yet this effect becomes positive (and the immediate jump is statistically significant) when controlling for country-specific time trends.

All in all, we do not seem to find any support for a boost in women's political participation or interest in or satisfaction with democratic institutions as a result of quota introductions. This may be our most surprising result.

4.2.3 Corruption

Table 7 also includes two regressions of corruption (Columns 7 and 8). Whereas Column 8 shows no significant effect, the DiD estimation in Column 7 shows a statistically significant trend of *more* corruption after the introduction of quotas (a low level of corruption corresponds to a high value of the index). This is in stark contrast to previous research, which has argued for women in parliaments leading to less corruption. Moreover, since the quota introductions led to a significant number of parliamentarians being replaced with new ones, one could argue that corruption should be decreased by this fact alone. In this light, the lack of a positive effect of a quota is even more striking.

5 Discussion of Potential Mechanisms

So far we have found that quotas are effective in raising the number of women in parliaments, but that they have not had any clear statistically significant effects on policy, attitudes, or political institutions. In this section we discuss possible mechanisms for these results. In particular, we will try to test whether the share of women in parliament in general has any effect on our outcome variables, and whether the effects of women in parliament differ depending on whether or not they were elected with the help of quotas. Any such difference could be due to either quota women being different from non-quota women, or that they become marginalized by the incumbent male elite.

Previous research has argued that the introductions of quotas were intended to break a male-dominated structure, and that it would be likely that the incumbent elite reacts to this (Childs and Krook 2006; Dahlerup 2006; Grey 2006; Zetterberg 2008). One proposed strategy for the maintenance of status quo would be to marginalize women into positions with less power. It is not clear, however, that such attempts would merely be reactions to increasing women in parliaments via quotas. Investigating the effects of quotas on women's political power in Mexican state legislatures, Zetterberg (2008) does not find women elected in a quota state to face greater obstacles than women elected in a no-quota state. Instead, he finds that *all* elected women face severe constraints. Fréchette et al. (2008), on the other hand, argue that the male incumbency advantage can increase following quotas, given that there is a bias among the voters for male candidates. This is so since the incumbent men will not have to compete with other men to the same extent after a quota introduction. Similarly, Heath et al. (2005) use data from six Latin American countries and find a negative correlation between share of elected women and share of women in high status committees. Hence, quotas may even have a negative effect on the substantive (as opposed to the numerical) representation of women in politics.

In order to test whether the quota introduction has led to increased political power for women, we investigate the effects of quotas on the share of women in ministerial positions. Arguably, if women have increased their real political power, rather than having been marginalized, this should show up also at the ministerial level, which in itself is unaffected by the quota. We have data on the share of women in ministerial positions from 1980, i.e., at a point before any country had introduced quotas, to 2003, when almost all quota countries had their quotas in effect. This enables us to conduct a convincing test of the effects of quotas on the share of female ministers in the same way as we have already done for WiP. If quotas for women do not lead to a higher share of women in ministerial positions, this can be taken as evidence of marginalization of female politicians. Table 10 shows that the direct effect of introducing quotas is not statistically significant. There are indications, however, that quotas increased the share of female ministers over time. In particular, Column 6 shows a statistically and substantially significant effect of having had quotas in the prior election, and Columns 7 and 8 similarly show a positive trend of the share of female ministers after quota introductions. Hence, it does not seem likely that the driving force behind our insignificant results is simply marginalization of elected women.

To further investigate the absence of significant effects of gender quotas on the outcomes discussed in the results section, we contrast these results with those from similar estimations by regressing our outcome variables on the share of women in parliament directly, and only using observations from years in which there was no legal quota in effect[6]. In doing this, we are interested in finding the correlation between our outcome variables and the share of women in parliament that stems from the ordinary election process. If we find correlations when using the share of women in parliament but not when using the quotas, this could indicate either that the share of women in parliament is an endogenous variable spuriously correlated with the outcome variables or that women elected through gender quotas do not have the same effects on policy, political participation, and corruption as do women elected in non-quota elections, or a combination of the two.

In order to keep these estimations as similar as possible to those performed for the quota, for each dependent variable we estimate models of the form:

$$y_{ct} = \alpha_c + h(t) + \lambda \cdot WiP\_lo_{ct} + \varepsilon_{ct},$$

[6] This is essential – if we were to include quota years, the quota effects would make up the lion part of the variation in the share of women in parliament.

where we allow for year fixed effects and add country-specific time trends by first using $h(t) = \theta_t$ and then $h(t) = \theta_t + \mu_c \cdot t$. Hence, the first of these is again a DiD estimation with country fixed effects and time dummies, and the second adds country-specific time trends. It should be noted here that as these estimations include country fixed effects, they should reduce the (potential) endogeneity of women in lower house of parliament (*WiP_lo*) substantially by controlling for all unobserved time-invariant country characteristics. As in the quota estimations, we again use Newey-West standard errors with a two period serial correlation. The estimated coefficients of *WiP_lo* from these estimations are presented in Table 11.

Looking at public expenditure, we now see increases of public expenditure (as share of GDP) on health and, in one of the estimations, education being correlated with more women in parliament, while there is a statistically insignificant negative correlation between spending on social security and women in parliament. Furthermore, both total social spending and government consumption have sizeable positive parameters, although the statistical significance of these varies. In all, there seems to be some evidence of a correlation between the share of women in parliament and both spending priorities and the size of government.

Turning to women's political interest and participation, there is also some evidence that the higher the share of women in parliament, the more prone women are to talk about politics, to vote, and to have higher confidence in the congress. Furthermore, the correlation with the likelihood to vote seems to be significantly stronger for women than for men, judging by the positive parameter for the gap between men and women. Likewise, although there is no statistically significant evidence of women being differently interested in politics following the quotas, it seems that the gap between men's and women's interest decreases with female representation in parliament.

Finally, the parameter for corruption is large (*ICRG*, the dependent variable, takes on values from 0 for very corrupt to 5 for little corruption), positive, and statistically significant at the 10 percent level in the DiD model, indicating a positive correlation between a low level of corruption and high female representation. This is in line with the previous literature, but in stark contrast to our findings regarding the effects of the gender quota on corruption. The statistical significance is sensitive to model specification though.

Hence, when using the share of women in parliament directly, we find quite a bit of support for correlations between higher female representation and policy, increases in women's political interest and participation, increased confidence in congress, and lower corruption. The fact that these correlations can not be found when looking at the introduction of legal quotas for women's representation seems to indicate that female representation that stems from gender quotas is different from that of an ordinary election process. This could be for several reasons. For instance, one suggestion is that women elected with the help of quotas are marginalized and hold little real power, yet our evidence of increased shares of women ministers seems to contradict this. However, there may be other differences between women elected via quotas and women elected via regular electoral competition that reduce the potential, ability, or willingness for quota women to change existing policies. For instance, it is still the political parties that nominate the candidates, which may result in women close to the incumbent (male) elites being chosen to fill out the quotas. In line with this, Franceschet and Piscopo (2008) find that the perception of many politicians in Argentina is that women elected by quotas have been placed there because they are loyal to the party or tied to a male politician. Whether it is true or not, such perceptions may affect the power or willingness of women in parliament to change policy.

However, it could also be that correlations between women in parliament and the outcome variables are spurious in that higher female representation and other outcomes are all driven by unobserved factors, e.g., by some broader sense of gender equality in society, which in turn is not correlated with the introduction of gender quotas. If the former is true, it seems that using gender quotas as a "fast track" to gender-equal representation may not – at least over the time periods spanned here – be very successful other than in terms of nominal representation. If the latter is true, it seems that female representation, while likely desirable for other reasons, may not be very important for actual policy, women's political interest and participation, or corruption.

6 Conclusion

Previous research has shown that quotas for women in politics affect the distribution of public goods, attitudes toward women, and the probability of women being elected even after the quotas are withdrawn. These findings are, however, based on different types of quotas, for different levels of government and in different countries, than in the present study. In this article, we assess the effects of quotas in Latin American national parliaments on a wide range of outcomes.

Although we find substantial and statistically significant effects of quotas on the number of women in parliament, we find very few statistically significant results on political participation, policies, and corruption. If anything, it seems that the political interest and participation have gone down among women after quota introductions. In stark contrast to earlier research, we also find that gender quotas have possibly raised corruption levels. All these effects of quotas, apart from the direct effect on the share of women in parliament, are sensitive to model specification though. The lack of significant effects indicates that the increased share of women in national parliaments resulting from quotas has had close to no substantial measurable effects beyond numerical representation in Latin America. This conclusion is also strengthened by the fact that when using the share of women in parliament directly, we find correlations with policies, political participation, and corruption much more in line with expectations. This indicates that to the extent that these correlations are causal effects, they do not seem to carry over to an increased share of women in parliament following a quota.

This finding contrasts some of the previous studies investigating the effects of quotas. Chattopadhay and Duflo (2004) and Beaman et al. (2009) found that quotas at the lowest level of governance (village councils) in India affected the distribution of public goods in favor of women's preferences and also reduced negative stereotypes toward women. Clots-Figueras (2011) found that quotas for women in Indian states increase spending on education, whereas Campa (2011) found no effects of quotas on spending outcomes in Spanish local governments.

It is obvious that results of the same rules may be different in different contexts, and looking at the preferences of women in Latin America, it is far from evident that one should expect a larger government or more spending on health or education as has been proposed before. This also holds true with respect to corruption as shown by Alatas et al. (2011). Nonetheless, not taking quota introductions into account, we are able to reproduce statistically significant effects of women in parliament on, e.g., corruption and spending on health care, which may indicate either that the correlation between women in parliament and our outcomes is spuriously driven by other time-variant factors or that increasing women in parliaments via quotas has some other adverse effects. However, it is also the case that the quotas studied in previous research are at a lower level of governance, which is also likely to be of importance. For instance, it is possible that at the national level, the women who are placed on the party lists are those closest to male-leaning party elites. Hence, quotas in national elections may not be effective in increasing the effective representation of women's preferences, but only in increasing the number of women in parliament.

Finally, the types of quotas studied here are also different from those in the previous literature. The quotas we investigate are most similar to those investigated by Campa (2011), as the quotas in both cases imply that a certain proportion of women must be on the electoral lists, as opposed to reserved seats whereby a certain share of the actual representatives must be women. Furthermore, the quotas in Latin America do not say anything about the role of the women once they have been elected, which is in strong contrast to the quotas at the Indian village level, which allocate chief positions, including agenda setting power, to women. Although we show that the quotas in Latin

America did lead to increased power for women in terms of ministerial positions, it remains unclear whether they substantially enhanced women's political power beyond raising the number of female ministers. Based on existing levels of knowledge, it is too early to draw conclusions about the importance of agenda-setting power, level of governance, or context more generally, and we therefore urge future studies to further investigate the causal effects of different types of quotas in other settings.

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8 Figures and Tables

Table 1. Quotas. Years of quota introductions.

Country	*Year of first quota law*	*Year of first quota election*
Argentina	1991	1993
Bolivia	1993	1997
Brazil	1996	1998
Chile	-	-
Colombia	-	-
Costa Rica	1997	1998
Dominican Rep.	1997	1998
Ecuador	1997	1998
Guatemala	-	-
Honduras	2000	2001
Mexico	2002	2003
Nicaragua	-	-
Panama	1997	1999
Peru	1997	2000
Paraguay	1996	1998
El Salvador	-	-
Uruguay	-	-
Venezuela	1997	1998

- indicates that no quota was introduced during the analyzed period.

Table 2. Variables. Description of variables

Variables	***Description***	***Source***
Independent variables of main interest		
Q_ineffect	Dummy =1 if quota is in effect	
Q_size_lo	Scope of legal quota (share of candidates that must be women, 0 implies no quota and 1 implies 100 percent quota) for elections in lower chamber.	Quotaproject
Q_size_hi	Same as q_scope_lo but for upper parliament.	Quotaproject
Q_country	=1 for countries that at some point 1990-2009 introduced some lega gender quota for national elections; 0 otherwise.	Quotaproject
Dependent variables		
Women in Politics		
WiP_lo	Share of women in the lower chamber of parliament.	IPU
WiP_hi	Share of women in the upper chamber of parliament.	IPU
female_ministers	Share of women in ministerial positions.	Escobar-Lemmon and Taylor-Robinson (2005)
Policy		
Govcons	General government final consumption expenditure as a percentage of GDP.	WDI
taxrevenue_gdpshare	Total tax revenue as a percentage of GDP.	ECLAC
educspending_gdpshare	Public expenditure on education as percentage of GDP.	ECLAC
healthspending_gdpshare	Public expenditure on health as percentage of GDP.	ECLAC
socsecspending_gdpshare	Public expenditure on social security as percentage of GDP.	ECLAC
soctotspending_gdpshare	Public expenditure on total social spending as percentage of GDP.	ECLAC
Political Participation		
Donttalkpolitics	=1 if the respondent never or almost never talks about politics with friends; 0 otherwise. Country and year averages.	Latino-barometer
Notinterestedpol	=1 if the respondent is not very interested or not interested at all in politics; 0 otherwise. Country and year averages.	Latino-barometer
Notvoting	=1 if respondent answer would not vote next Sunday; 0 otherwise. (No response and do not know coded as missing.) Country and year averages.	Latino-barometer
election_turnout_lo	The total number of votes cast in lower house elections as a percent of the voting age population. Country and year averages.	IDEA voter turnout database
Noconfidencecongress	=1 if the respondent has little or no confidence in the National Congress; 0 otherwise. Country and year averages.	Latino-barometer
Satisfieddemocracy	=1 if the respondent is very satisfied or somewhat satisfied with the way democracy works in the country X; 0 if not satisfied or not at all satisfied. Country and year averages.	Latino-barometer
*_gap	All measures from the Latinobarometer allow us to create the gender gap of each country and year. These are created as the average for men less that of women.	Latino-barometer
Corruption		
ICRG	International Country Risk Group's index of perceived corruption. Ranging from 0 (very much corruption) to 5 (no corruption).	ICRG

Table 3. WiP lower. Fixed effects estimations of the share of women in the lower (or only) house of parliament.

	-1	-2	-3	-4	-5	-6	-7	-8
VARIABLES	t	t	i.t	c*t	i.t	c*t	i.t	c*t
Q_ineffect_lo	0.058***							
	(0.014)							
Q_size_lo		0.216***	0.249***	0.105**	0.111*	0.086**	0.115**	0.130***
		(0.045)	(0.052)	(0.046)	(0.057)	(0.042)	(0.051)	(0.042)
L.Q_size_lo				0.249***	0.162***			
					(0.059)	(0.040)		
Q_size_lo * T							0.040***	0.027***
							(0.008)	(0.007)
t	0.005***	0.005***						
	(0.001)	(0.001)						
Year dummies			yes		yes		yes	
Country spec. time trends				yes		yes		yes
Constant	0.049***	0.050***	0.061***	0.045***	0.060***	0.054***	0.059***	0.059***
	(0.009)	(0.009)	(0.017)	(0.007)	(0.015)	(0.006)	(0.014)	(0.007)
N	115	115	115	115	115	115	115	115
R-squared	0.623	0.641	0.741	0.842	0.792	0.870	0.812	0.868
Number of countryid	18	18	18	18	18	18	18	18

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Some tests. Tests of pre-quota differences in female representation between quota and non-quota countries (Columns 1-6), and placebo estimations of the effect of lower house quota introductions on upper house representation.

Dependent:	wip_lo		wip_up					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Q_country	-0.00926	-0.00869	0.0161	0.0160				
	(0.00810)	(0.00807)	(0.0107)	(0.0108)				
Q_country					0.0181	0.0177		
					(0.0119)	(0.0122)		
Q_sizee_lo							0.0206	-0.160
							(0.0968)	(0.104)
Q_sizee_up								0.368***
								(0.112)
t	0.00338***	0.00254	0.00294***	0.00434	0.00304***	0.00412	0.00460***	0.00422***
	(0.000676)	(0.00234)	(0.000918)	(0.00352)	(0.000961)	(0.00353)	(0.00149)	(0.00136)
t2		3.45e-05		-5.59e-05		-4.30e-05		
		(9.08e-05)		(0.000139)		(0.000145)		
Constant	0.0585***	0.0617***	0.0213	0.0155	0.0215*	0.0172	0.0250	0.0268*
	(0.0107)	(0.0145)	(0.0128)	(0.0184)	(0.0126)	(0.0178)	(0.0164)	(0.0150)
N	88	88	50	50	50	50	62	62
R-squared	0.345	0.346	0.213	0.216	0.222	0.224	0.303	0.430
N countries							11	11

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Country fixed effects included in Column 8.

Table 5. Gov size 1. Fixed effects, reduced form effect of quota introduction.

Dependent:	Health spend (% GDP)		Educ. spend (% GDP)		Soc. Sec spend (% GDP)		Tot Soc. spend (% GDP)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Q_size_lo	0.276	0.676*	0.0397	0.160	0.721	0.483	0.177	1.083
	(0.320)	(0.348)	(0.584)	(0.384)	(0.708)	(0.595)	(1.087)	(0.991)
Q_size*T	-0.0681	0.0389	-0.112	-0.0463	-0.109	0.0281	-0.359	0.406
	(0.0604)	(0.126)	(0.101)	(0.145)	(0.171)	(0.190)	(0.241)	(0.356)
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes
Country spec. time trends		yes		yes		yes		yes
Constant	2.839***	3.404***	5.136***	4.128***	5.119***	-1.512	13.46***	10.42***
	(0.234)	(0.595)	(0.305)	(0.484)	(0.600)	(1.695)	(0.783)	(1.152)
N	322	322	322	322	300	300	322	322
R-squared	0.257	0.422	0.479	0.664	0.302	0.702	0.503	0.687
N countries	18	18	18	18	17	17	18	18

Newey-West standard errors with two period serial correlation in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6. Gov size 2. Fixed effects, reduced form effect of quota introduction.

Dependent:	Govmnt Cons. (% GDP)		Tax Revenue (% GDP)	
	(1)	(2)	(3)	(4)
Q_size_lo	4.337	2.716	-2.072	-1.602
	(2.671)	(2.370)	(1.454)	(1.100)
Q_size*T	0.402	-1.438**	0.666***	-0.431
	(0.413)	(0.627)	(0.246)	(0.326)
Year dummies	yes	yes	yes	yes
Country spec. time trends		yes		yes
Constant	12.72***	4.012*	16.73***	7.492***
	(1.257)	(2.063)	(0.527)	(1.976)
N	448	448	360	360
R-squared	0.078	0.597	0.494	0.785
N countries	18	18	18	18

Newey-West standard errors with two period serial correlation in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Political participation. Fixed effects, reduced form effect of quota introduction.

Dependent:	Female Notvoting		Notvoting_gap		Election_turnout_lo		Corruption	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Q_size_lo	-0.138	-0.125	0.0628	0.0178	11.00	11.34	0.0516	0.114
	(0.105)	(0.150)	(0.0403)	(0.0497)	(13.24)	(13.80)	(0.640)	(0.634)
Q_size*T	0.0143	0.0985	0.00449	-0.0321	-1.512	-4.738	-0.329**	-0.148
	(0.0188)	(0.0762)	(0.00574)	(0.0203)	(2.616)	(3.789)	(0.153)	(0.210)
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes
Country spec. time trends		yes		yes		yes		yes
Constant	0.270***	-0.865	-0.0632***	0.521	75.24***	83.11***	3.108***	2.675***
	(0.0519)	(0)	(0.0128)	(0)	(5.476)	(7.193)	(0.414)	(0.156)
N	182	182	182	182	122	122	378	378
R-squared	0.191	0.309	0.208	0.311	0.172	0.512	0.234	0.543
N countries	18	18	18	18	18	18	18	18

Newey-West standard errors with two period serial correlation in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8. Political interest. Fixed effects, reduced form effect of quota introduction.

Dependent:	Fem. donttalkpolitics		Donttalkpolitics_gap		Fem. Notinterestedpol		Notinterestedpol_gap	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
q_scope_lo	0.0226	-0.0151	-0.00768	0.0124	-0.0472	0.0196	-0.0304	-0.0622
	(0.0415)	(0.0454)	(0.0276)	(0.0341)	(0.0751)	(0.0862)	(0.0366)	(0.0457)
Q_size*T	-0.00272	-0.0128	0.00812*	0.00916	0.0174	0.105***	-0.00358	-0.0277
	(0.00787)	(0.0259)	(0.00454)	(0.0154)	(0.0145)	(0.0396)	(0.00639)	(0.0185)
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes
Country spec. time trends		yes		yes		yes		yes
Constant	0.918***	0.858	-0.116***	-0.904	0.611***	0.656	-0.0594**	0.283
	(0.0279)	(0)	(0.0113)	(0)	(0.0414)	(0)	(0.0229)	(0)
N	147	147	147	147	164	164	164	164
R-squared	0.116	0.324	0.117	0.303	0.278	0.524	0.207	0.309
N countries	18	18	18	18	18	18	18	18

Newey-West standard errors with two period serial correlation in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 9. Political satisfaction. Fixed effects, reduced form effect of quota introduction.

Dependent:	Fem. Noconf.congress		Noconf.congr_gap		Fem. satisfieddemocr.		Satisfieddemocracy_gap	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Q_size_lo	-0.0779	-0.187**	-0.0159	-0.0204	0.0520	0.246**	0.00195	-0.0282
	(0.0796)	(0.0831)	(0.0337)	(0.0502)	(0.0808)	(0.0984)	(0.0304)	(0.0425)
Q_size*T	0.0233	0.0746**	0.00371	-0.0394**	-0.0403**	0.00379	-0.00431	0.00501
	(0.0165)	(0.0329)	(0.00507)	(0.0173)	(0.0159)	(0.0543)	(0.00604)	(0.0191)
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes
Country spec. time trends		yes		yes		yes		yes
Constant	0.529***	-0.615	0.00319	0.511	0.559***	0.298	0.0407**	0.394
	(0.0581)	(0)	(0.0143)	(0)	(0.0520)	(0)	(0.0172)	(0)
N	199	199	199	199	199	199	199	199
R-squared	0.296	0.592	0.151	0.267	0.287	0.404	0.110	0.242
N countries	18	18	18	18	18	18	18	18

Newey-West standard errors with two period serial correlation in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 10. Female ministers. Fixed effects estimations of the share of female ministers.

	-1	-2	-3	-4	-5	-6	-7	-8
VARIABLES	t	t	i.t	c*t	i.t	c*t	i.t	c*t
Q_ineffect_lo	0.020							
	(0.020)							
Q_size_lo		0.095	0.063	0.040	0.062	0.032	0.031	0.020
		(0.066)	(0.083)	(0.080)	(0.087)	(0.078)	(0.082)	(0.074)
L.Q_size_lo				0.008	0.175**			
					(0.108)	(0.086)		
Q_size*T						0.046**	0.060***	
							(0.023)	(0.019)
t	0.007***	0.007***						
	(0.001)	(0.001)						
year dummies			yes		yes		yes	
Country spec. time trends				yes		yes		yes
Constant	0.017	0.019	0.040*	0.015	0.040*	0.022*	0.042**	0.025**
	(0.013)	(0.013)	(0.021)	(0.012)	(0.021)	(0.012)	(0.020)	(0.012)
Observations	115	115	115	115	115	115	115	115
R-squared	0.623	0.641	0.741	0.842	0.792	0.870	0.812	0.868
Number of countryid	18	18	18	18	18	18	18	18

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 11. Fixed effects estimations of various outcomes on the share of women in parliament (non-quota years only).

Dependent	Model	Coefficient	Std. Err.	N
Policy				
healthspending_gdpshare	FE, year dummies	2.858*	(1.728)	224
	FE, Country*t	3.622**	(1.485)	224
educspending_gdpshare	FE, year dummies	1.513	(2.165)	224
	FE, Country*t	3.285*	(1.700)	224
socsecspending_gdpshare	FE, year dummies	-4.467	(3.755)	204
	FE, Country*t	-2.074	(1.455)	204
soctotspending_gdpshare	FE, year dummies	3.214	(4.850)	224
	FE, Country*t	11.41***	(4.041)	224
govcons	FE, year dummies	30.20*	(17.42)	308
	FE, Country*t	16.61	(12.22)	308
taxrevenue_gdpshare	FE, year dummies	-6.226	(5.261)	242
	FE, Country*t	3.380	(4.344)	242
Political participation and interest				
f_donttalkpolitics	FE, year dummies	-0.752***	(0.232)	92
	FE, Country*t	-0.203	(0.330)	92
donttalkpolitics_gap	FE, year dummies	-0.0126	(0.112)	92
	FE, Country*t	-0.0725	(0.227)	92
f_notinterestedpol	FE, year dummies	-0.641	(0.443)	101
	FE, Country*t	-0.481	(0.330)	101
notinterestedpol_gap	FE, year dummies	0.252**	(0.124)	101
	FE, Country*t	0.342*	(0.194)	101
f_noconfidencecongress	FE, year dummies	-0.980**	(0.476)	116
	FE, Country*t	-0.763	(0.484)	116
noconfidencecongress_gap	FE, year dummies	0.0554	(0.121)	116
	FE, Country*t	0.0115	(0.163)	116
f_satisfieddemocracy	FE, year dummies	0.526	(0.489)	116
	FE, Country*t	0.170	(0.725)	116
satisfieddemocracy_gap	FE, year dummies	0.220*	(0.126)	116
	FE, Country*t	0.0597	(0.185)	116
f_notvoting	FE, year dummies	-0.934	(0.603)	101
	FE, Country*t	-1.769**	(0.731)	101
notvoting_gap	FE, year dummies	0.379**	(0.165)	101
	FE, Country*t	0.440***	(0.161)	101
election_turnout_lo	FE, year dummies	-49.79	(79.94)	83
	FE, Country*t	-21.77	(83.19)	83
Corruption				
icrg	FE, year dummies	4.790*	(2.517)	281
	FE, Country*t	3.475	(2.358)	281

Note: Coefficients of wip_lo, the share of women in the lower or only house of parliament, from fixed effects estimations of various dependents. Observations from years when there was a legal gender quota in effect have been removed. For each dependent, the wip_lo coefficients from two models are presented: one with country fixed effects and year dummies, and one that also adds country specific time trends. Newey-West standard errors with two period serial correlation in parentheses. *** p<0.01, ** p<0.05, * p<0.1