

# District Magnitude and Women's Representation Evidence from Natural Experiments in Argentina and the Province of Buenos Aires\*

Adrián Lucardi  
Department of Political Science  
ITAM  
[adrian.lucardi@itam.mx](mailto:adrian.lucardi@itam.mx)

Juan Pablo Micozzi  
Department of Political Science  
ITAM  
[juan.micozzi@itam.mx](mailto:juan.micozzi@itam.mx)

January 30, 2019

While it is widely accepted that closed-list proportional representation with large magnitudes promote the election of women, determining whether the relationship is causal is problematic because district magnitude often covaries with other factors that may also facilitate the election of women. We address this issue with electoral data for the Argentine Chamber of Deputies and the legislature of the province of Buenos Aires between 1985 and 2015. For identification, to exploit the fact that both bodies elect half of their members every two years, which results in some districts having varying magnitudes in concurrent and midterm elections. We find that larger district magnitudes modestly augment the proportion of women elected (in log-log specifications), as well as the probability that at least one woman will be elected (in Argentina). Consistent with the claim that compliance with gender quotas is minimalist, additional results show that the effect is mediated by party magnitude, and district magnitude only makes a difference when quotas are in place.

Keywords: women's representation – electoral systems – district magnitude – natural experiment – Argentina – Buenos Aires

Word count: 4,500

---

\*We thank the Asociación Mexicana de Cultura A.C. for financial support. Paula Tussie provided excellent research assistance. Ernesto Calvo and Todd Mitton kindly shared their candidate and geographical data on Argentina, respectively. Previous versions of this paper were presented at the 2017 MPSA annual meeting, the Hobby School of Public Affairs at the University of Houston, the 2018 GEL-ALACIP meeting, the 2018 Latin America PolMeth Meeting, and the department of Political Science at the University of CEMA. We thank Olle Folke, Emilia Simison, Özge Kemahloğlu, Francisco Cantú, Pablo Pinto, Tiffany Barnes, Mark P. Jones, Ernesto Calvo, John Londregan, Chris Tausanovitch, Sofia Vera, Michelle Torres, Guillermo Rosas, Jake Bowers, Carmen Le Foulon, Luis Maldonado, Sybil Rhodes, Jorge Streb, Jorge Szeinfeld and Pascual Cappelleri for their helpful comments. All remaining mistakes are our responsibility.

Can political institutions improve the representation of women? If so, which ones and through which mechanisms? There is widespread consensus that gender quotas increase the number of women elected (Htun 2004; Jones 2009; Jones, Alles and Tchintian 2012; Schmidt and Saunders 2004; Krook 2007, 2018; Franceschet and Piscopo 2008; Schwindt-Bayer 2009, 2010; Piscopo 2015; Rosen 2017; though see Reynolds 1999 for a more skeptical view) and promoted (O'Brien and Rickne 2016).<sup>1</sup> To be effective, however, quotas have to be sufficiently generous (Schmidt and Saunders 2004; Schwindt-Bayer 2009; Rosen 2017) and provide placement mandates (Jones 2009; Jones, Alles and Tchintian 2012; Schwindt-Bayer 2009; Rosen 2017) that are actively enforced (Htun and Jones 2002; Dahlerup and Freidenvall 2005; Larsrud and Taphorn 2007; Tripp and Kang 2008; Schwindt-Bayer 2009; Rosen 2017). Their effectiveness also depends on their interaction with other electoral rules, such as closed-list versus open-list proportional representation (Jones 2009; Schwindt-Bayer 2009; Thames and Williams 2010; González-Eiras and Sanz 2018).

In this paper we explore how district magnitude – the number of candidates elected in a district in an election – affects women's representation. While the positive association between district magnitude and the election of women is widely documented (Matland 1993; Matland and Taylor 1997; Reynolds 1999; Salmond 2006; Schwindt-Bayer 2010; Thames and Williams 2010; Krook 2018; though see Schmidt and Saunders 2004; Schmidt 2009 for a more skeptical view), two issues remain unanswered. The first is whether this association reflects a *causal* relationship. Most existing studies rely on cross-sectional comparisons at a single moment in time, which make them vulnerable to omitted variable bias. Simply put, districts that elect more representatives tend to be more urbanized and more socially diverse (Monroe and Rose 2002; Gerring et al. 2015; Kedar, Harshgor and Sheinerman 2016), which may affect women's labor market opportunities, voters' attitudes toward them, or party leaders' willingness to place them in electable positions (Salmond 2006; Schmidt 2009; Roberts, Seawright and Cyr 2013). Comparing elections to different chambers within the same polity – either explicitly (Roberts, Seawright and Cyr 2013),

---

<sup>1</sup>Gender quotas can also increase turnout (De Paola, Scoppa and De Benedetto 2014), improve some parties' electoral chances (Casas-Arce and Saiz 2015) and improve the quality of male candidates (Besley et al. 2017), at least at the local level.

or implicitly by including district fixed effects (Jones 2009) – does not solve the problem because voting behavior across multiple tiers is probably correlated (Fiva and Folke 2016). For example, citizens may cast a straight-party vote, or placing a woman at the top of the Senate list may make a party more likely to place a man at the top of the House ticket, or vice versa.

The second question concerns the *mechanisms* linking district magnitude and the election of women. According to the *balancing* argument, (some) party leaders prefer to nominate a diverse pool of candidates in order to maximize their party's electoral appeal (Matland 1993; Salmond 2006; Casas-Arce and Saiz 2015; Krook 2018; Meserve, Pemstein and Bernhard forthcoming), and larger magnitudes provide them with the opportunity to do so. Intuitively, there is a trade-off between putting weightier politicians – usually men – in the list, and making the latter more representative; the larger the number of candidates that can be nominated, the easier it is to balance this trade-off. This predicts that increasing district magnitude should improve women's representation *even in the absence of quotas*. Furthermore, parties should have little incentives to send women to lower-ranked positions in party lists when magnitude increases.

In contrast, the *minimal compliance* mechanism posits that party leaders only nominate women when compelled by gender quotas, and thus place them in the lowest-ranked position they are legally allowed to. Intuitively, as district magnitude increases, the average number of seats obtained by a party goes up, and thus lower-ranked (women) candidates – who are more likely to be women – are more likely to get elected (Jones 1998, 2009; Jones, Alles and Tchintian 2012; Schmidt and Saunders 2004; Esteve-Volart and Bagues 2012). This suggests that quotas and large magnitudes should reinforce each other: the former guarantees that there will be enough women in electable positions, while the latter ensures that parties will receive enough seats to get lower-ranked women candidates elected. In addition, the effect of *district* magnitude should be mediated by that of *party* magnitude – the average number of seats elected by a party –: once the effect of this variable is accounted for, the effect of district magnitude should disappear.

In this paper we offer a pioneer attempt at tackling these issues simultaneously by combining data from the Argentine Chamber of Deputies and the legislature of the province of Buenos Aires between 1985 and 2015. To obtain an exogenous source of variation in district magnitude, we exploit the fact that

these two bodies are renewed by halves every two years, and thus the same district may elect a different number of representatives in concurrent or midterm elections. In Argentina, each province elects half of its congressional delegation every two years; since most provinces have an odd number of representatives, the number of seats elected in a province varies by increment of one between concurrent and midterm elections. In the province of Buenos Aires, half of the electoral districts elect all their representatives to the lower chamber in midterm years and all their upper chamber delegation in concurrent years, while the other half follow the opposite pattern. Since the lower chamber is twice as large as the upper, district magnitude varies by a factor of two within the same district every two years. Furthermore, since both polities introduced a gender quota during the 1990s, we can examine whether the effect of district magnitude is independent of, or conditioned by, the presence of quotas.

We find a positive but modest effect of district magnitude on women's representation. The effect in levels are generally statistically insignificant, even if they go in the expected direction, but the log-log specifications show a strong positive effect of district magnitude on women's representation. Additionally, in Argentina a unit increase in magnitude sharply increases the probability that at least one woman will be elected; the implication is that if all provinces elected their entire delegation simultaneously, all districts would elect at least one woman in every election. Further analyses show that these results are driven by the minimal compliance mechanism. First, the effects are conditional on the presence of gender quotas. Second, a unit increase in district magnitude induces large parties – the ones that can legally do so – to displace women candidates *away* from the second place in the list. Thirdly, a controlled direct effects analysis (Acharya, Blackwell and Sen 2016) indicates that the effect of district magnitude is mediated by *party* magnitude rather than by women's positions in party lists.

## Research design

We examine the effect of district magnitude on women's representation in elections to the Argentine Chamber of Deputies ("the Chamber") and the legislature of the province of Buenos Aires ("the legis-

lature”) between 1985 and 2015. For the former, we combined electoral data from Tow (N.d.) with our own dataset of candidates to the Chamber.<sup>2</sup> For Buenos Aires, the provincial Electoral Court reports both electoral results and the names of successful candidates since 1983.<sup>3</sup>

We estimate difference-in-differences models of the form

$$y_{d,t} = \beta \cdot \text{Magnitude}_{d,t} + \mu_d + \delta_t + \varepsilon_{d,t}, \quad (1)$$

where  $y_{d,t}$  is either the proportion of women elected in district  $d$  in election year  $t$ , or a 0/100 dummy indicating that at least one woman had been elected;  $\text{Magnitude}_{d,t}$  is the total number of seats elected in district  $d$  in year  $t$ ; <sup>4</sup> and  $\mu_d$  and  $\delta_t$  are district and year fixed effects, respectively.

To identify the effect of *Magnitude*, we exploit systematic but exogenous variation in the electoral calendar. In Argentina, the lower chamber is elected by closed-list PR in 24 multi-member districts that are coterminous with the country’s 23 provinces plus its federal capital. Within each district, seats are distributed employing the d’Hondt formula, with a legal threshold of 3 percent of registered voters.<sup>5</sup> Candidate nominations are in the hands of provincial party branches; competitive primaries are sometimes held, but these are rarer if a party controls the governorship (De Luca, Jones and Tula 2002). Deputies serve four-year terms, but the Chamber is renewed by halves every two years, and thus the 19 provinces with an odd number of representatives elect a different number of deputies in concurrent and midterm

---

<sup>2</sup>We have the name of all successful candidates, but complete data on unsuccessful candidates is only available since 1995.

<sup>3</sup><http://www.juntaelectoral.gba.gov.ar/mapa-provincia-bsas.php>. Information on unsuccessful candidates is unavailable.

<sup>4</sup>District fixed effects mean that we focus on within-district changes in magnitude; in the Argentine sample, this means one-unit changes between elections, but in Buenos Aires these changes range between 3 and 9 (see Tables A1 and A2).

<sup>5</sup>This makes little difference in practice because turnout is relatively high, and low magnitudes mean that parties that do not reach the threshold would not have obtained representation anyway.

years (see Table A1). We restrict the sample to the 19 provinces with an odd number of representatives, where *Magnitude* ranges between 2 and 13 (see Table A1).<sup>6</sup>

In the province of Buenos Aires, both legislative chambers are elected by closed-list PR in eight multi-member districts, called *secciones*, whose magnitudes range between 3 and 18 (see Table A2). Both the districts' boundaries and their magnitude have remained constant since 1983. There is a legal threshold equivalent to a full Hare quota; among lists surpassing this threshold, seats are distributed following the Hare formula, and any remaining seats go to the most voted list (provincial law No. 5109).<sup>7</sup> As in Argentina, candidate nominations are decided by provincial party leaders; in practice, this often means the governor (for the incumbent party) and powerful mayors of each *sección* (Lodola 2009; Caminotti, Rotman and Varetto 2011). Since the lower chamber is twice as large as the provincial Senate (92 vs. 46 members, respectively), variation in *Magnitude* is induced by the fact that in midterm years, four districts hold elections for the upper chamber, while the other four hold lower-chamber elections; two years later, the roles are reversed (see Table A2 and Figure A2). This means that we will be comparing elections for different bodies, but we do not regard this as overly problematic because both chambers have almost identical powers, and all provincial legislators are elected for a four-year term according to the same rules. In particular, note that unlike Lago and Martínez (2007); Jones (2009); Jones, Alles and Tchintian (2012) or Roberts, Seawright and Cyr (2013), we do not compare elections held on the same day for different offices, where candidate nominations may be interdependent or voters may cast a straight ballot for their preferred party; rather, our design resembles that of Crisp, Potter and Lee (2012), who look at the same districts in different elections held under different rules.

By comparing a district with itself at different moment in time, we ensure that all characteristics that remain constant within districts are balanced by definition. Time-varying characteristics that vary slowly

---

<sup>6</sup>With a minor exception – Tierra del Fuego elected two deputies until becoming a province in 1990, and five afterwards –, the number of representatives per province has remained constant since 1983.

<sup>7</sup>Since the Hare quota is calculated by dividing the number of valid votes by the number of seats, this means that district magnitude also affects the legal threshold. We thank Jorge Streb for bringing this point to our attention.

within districts – such as voters’ attitudes to women candidates – are not worrisome because our treatment is switched on or off repeatedly within each district. This lends credibility to the assumption that the treatment and control groups would have followed parallel paths in the absence of treatment (Angrist and Pischke 2009, ch. 5). Nonetheless, the fact that executive officials – presidents, governors and mayors – are elected every four years<sup>8</sup> means that some districts have larger magnitudes in years with executive elections (“concurrent years”), while in others *Magnitude* is larger in midterm elections (“midterm years”).<sup>9</sup> If all districts had larger magnitudes in concurrent (midterm) years, this would violate the parallel paths assumption, as a larger value of *Magnitude* would have been perfectly collinear with (non-)concurrency, and executive races may affect legislative ones, either via coattail effects (Jones 1997) or by affecting the pool of candidates (Lucardi and Micozzi 2016). Thus, it is important to note that roughly half of the districts in each sample elect a larger number of representatives in concurrent or midterm elections (see Appendix A1), and furthermore their identity was determined randomly. In Argentina, every province elected its entire congressional delegation in 1983, but subsequently half of each district’s representatives received a shortened two-year mandate instead of a four-year one. The decision of which legislators would receive a full term – and thus, implicitly, of which provinces would elect more representatives in concurrent or midterm years – was decided by lot shortly after the election (Dal Bó and Rossi 2011:1243-4). In Buenos Aires, the entire legislature was elected in 1983, but the following year the eight *secciones* were divided into two groups ensuring that exactly half of the upper and the lower chamber would be renewed every two years. A random draw decided which group would elect provincial deputies rather than senators in 1985.<sup>10</sup> Appendix A3 confirms that districts that ended up having a larger magnitude in concurrent and midterm years are well-balanced along a wide array of pre-treatment characteristics.

---

<sup>8</sup>The president was originally elected for a six-year term, but since 1995 presidential elections take place in concurrent years.

<sup>9</sup>Concurrent years are 1987, 1991, 1995, 1999, 2003, 2007, 2011 and 2015. Midterm years are 1985, 1989, 1993, 1997, 2001, 2005, 2009 and 2013 (see Tables A1 and A2). We speak of concurrent *years* rather than *elections* because executive and legislative elections taking place in the same year need not take place in the same *day*.

<sup>10</sup>Personal interview with Pascual Cappelleri, who presided the lower chamber of the provincial legislature during 1983-1987.

Both Argentina and Buenos Aires adopted a gender quota during the 1990s. Beginning in 1993, all lists running for the Argentine Chamber of Deputies must include one woman for every three positions (Jones 1998; Tula 2004). Female representation increased dramatically, from around 4% between 1983 and 1991 to 30% afterwards (Table A3; see also Franceschet and Piscopo 2008). However, since 2000 parties that are competing for the first time *or* are expected to renew one or two seats must place one women in the first two positions in the list (see decree No. 1246/2000 and Tula 2004). Since many districts have a magnitude of 2 and many parties elect no more than two seats even with magnitudes of 3 to 5, the link between district magnitude and women’s representation may have weakened since the 2001 election.<sup>11</sup> In Buenos Aires, a quota mandating a minimum of 30% of candidates of each gender has been in force since 1997, with placement mandates becoming more stringent over time; the quota also increased women’s representation, though less dramatically than in Argentina (see Table A3 and Barnes 2016, ch. 1).

## Results

*Graphical summary.* To show that the results are not an artifact of model specification, in Figure 1 we display the average values of the outcome variables conditional on (i) whether a district’s magnitude is larger in concurrent or midterm elections; and (ii) whether an election falls on a concurrent or a midterm year. If increasing district magnitude promotes women’s representation, districts with larger magnitudes in concurrent elections should elect more women in concurrent years, and the opposite should hold for districts that have a larger magnitude in midterm years. This is exactly what Figure 1a shows: for both the Argentine Chamber of Deputies and the legislature of Buenos Aires, the % of women elected and whether at least one woman was elected show a spike in election years featuring larger magnitudes. Panels (b) and (c) indicate that in both samples the effect only appeared after the adoption of gender quotas.

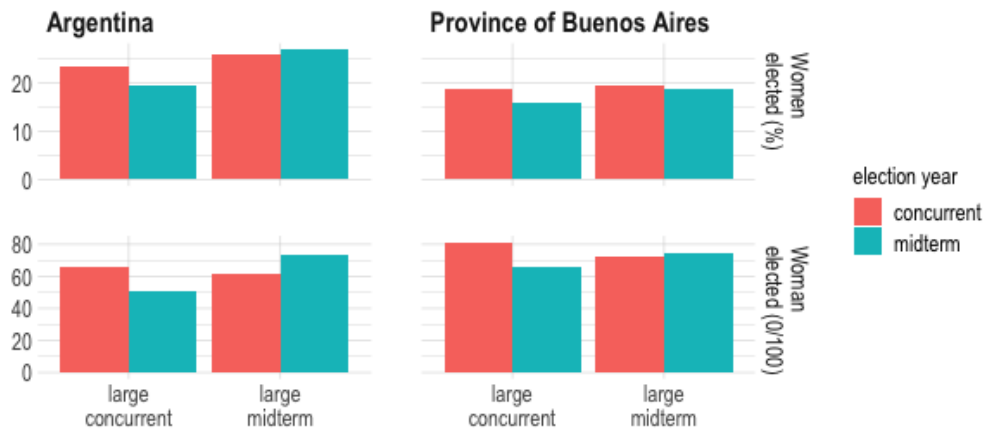
*Main results.* Table 1 reports the main results. Since there are just 19 provinces in Argentina and 8 *secciones* in Buenos Aires, below each estimate we report two alternative 95% confidence intervals (CIs): one em-

---

<sup>11</sup>We thank Mark P. Jones for bringing this possibility to our attention.



(a) Full sample



(b) Pre-quota (ARG:1985-1991; PBA:1985-1995)



(c) Post-quota (ARG:1993-2015; PBA:1997-2015)

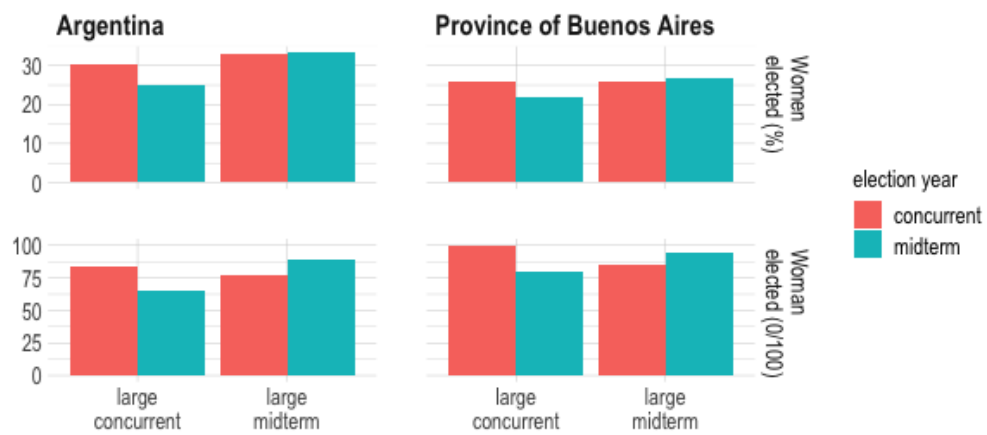


Figure 1: Average values of the outcome variables, conditional on (a) whether a district's magnitude is larger in concurrent or midterm elections; and (b) whether an election took place in a concurrent or a midterm year.

ploying standard errors clustered by district but adjusting the critical value of the  $t$ -statistic to account for the small number of clusters, and the other based on the wild-bootstrapped CIs proposed by Cameron and Miller (2015).<sup>12</sup>

The results are somewhat sensitive to the definition of the outcome variable. In the specifications in levels reported in columns (1)-(4), the point estimates of *Magnitude* are generally positive, as expected, but fall short of statistical significance at conventional levels. A look at the first three panels shows that the estimates for the full sample (1985-2015) shown in Table 1a are being driven by the post-quota era (1993-2015 for Argentina and 1997-2015 for Buenos Aires). For example, column (1) indicates that after the adoption of gender quotas, a unit increase in *Magnitude* in elections for the Argentine Chamber of Deputies increased the proportion of women elected in a district by 2.5 percentage points, when the pre-quota estimate was just 1 pp. This implies that if Argentina held legislative elections every four years instead of two – thus increasing median district magnitude from 3 to 6.5; see Table A1 –, the proportion of women elected to the Chamber would increase by almost 8.7 pp. This is in line with the effect of generous quotas ( $\geq 30\%$ ) with placement mandates and effective enforcement mechanisms reported by Rosen (2017). The effect is substantial, as just 24% of candidates elected in an average election are women (see Table A3). Table 1d shows that the effect was more than twice as large – 5.4 pp. – between 1993 and 1999, when the quota was in place but all parties could send women to the third position in the list (Jones 1998; Tula 2004). Still, neither of these estimates are statistically significant at the 5% level; nor are the somewhat larger tobit estimates reported in column (2).

In Buenos Aires the effects are much more modest in size. Column (3) indicates that a unit increase in *Magnitude* increases the number of elected women by a fifth of a percentage point, but the effect was essentially zero before the adoption of gender quotas in 1997 and jumped to 0.38 pp. afterwards. This value implies that increasing district magnitude by 5.75 – the median difference between the lower and upper chamber; see Table A2 – would raise the proportion of women elected by 2.2 pp., a small but non-trivial effect considering that even after 1997, just 25% of elected legislators were women (see Table A3c).

---

<sup>12</sup>We calculated these with the `clusterSEs` package in R (Esarey and Menger forthcoming), using 999 bootstrap replications.

The tobit estimates in column (4) are slightly larger, but the difference is modest. Again, none of the estimates is statistically significant at conventional levels. Despite the considerable reduction in sample size, it is worth noting that these estimates fall just short of statistical significance only due to the stringent criteria used to calculate the confidence intervals.<sup>13</sup>

The log-log estimates reported in columns (5)-(6) are substantially stronger, though again the effect only appears after the adoption of quotas. In Argentina, the statistically significant estimate reported in Table 1a means that every 1% increase in *Magnitude* increased the proportion of women elected by 1.4% – suggesting that marginal candidates tend to be women.<sup>14</sup> The size of the effect more than quadruples after the introduction of quotas, from 0.4 to 1.7 – reaching a whopping 3.1 between 1993 and 1999. In Buenos Aires, the point estimate of 0.66 for the post-quota era is also statistically significant, when before the adoption of quotas it was much closer to zero and negative.

In the last two columns of Table 1 the outcome is a dummy that takes the value of 100 when at least one woman was elected, and zero otherwise. Thus, the coefficients can be interpreted as percentage point changes. For the Argentine sample we see an impressive and reliably estimated effect of 13 pp. that is entirely driven by the post-quota period, reaching a whopping 24 pp. increase between 1993 and 1999. In practical terms, this means that simplifying Argentina's electoral calendar would ensure that at least one woman is elected in every province in every election (see Table A3). In contrast, we find no effect for the province of Buenos Aires, probably because few districts elected women before 1997, but almost all of them did so afterwards (see Table A3).

---

<sup>13</sup>The *t* statistic for the estimate reported in Table 1a is 2.15, but since there are just 8 districts, we multiply the clustered standard errors by 2.365 – the critical values from a Student's *t*-distribution with 7 degrees of freedom. The wild bootstrapped confidence intervals are also much more conservative than the usual ones (Cameron and Miller 2015).

<sup>14</sup>*Women elected (%)* may take the value of zero, so we added +1 to all observations.

Table 1: District magnitude and women's representation in Argentina and the province of Buenos Aires, 1985-2015

	Women elected (%)			Women elected (%) ( $\log^{\ddagger}$ )			Women elected (0/100)	
	(1)	(2)	(3)	(4)*	(5)	(6)	(7)	(8)
(a) Full sample								
<i>Magnitude</i> <sup>†</sup>	2.11 [-1.29;5.51] [-1.13;5.35]	4.76 [-0.92;10.43]	0.21 [-0.02;0.44] [-0.13;0.55]	0.29 [-0.47;1.05]	1.38 [0.31;2.45] [0.35;2.42]	0.35 [-0.14;0.84] [-0.17;0.87]	13.13 [2.54;23.73] [2.41;23.86]	1.28 [-1.29;3.85] [-1.24;3.79]
num. obs	302	302	128	128	302	128	302	128
(b) Pre-quota (ARG: 1985-1991; PBA: 1985-1995)								
<i>Magnitude</i> <sup>†</sup>	0.97 [-4.25;6.18] [-3.29;5.22]	16.41 [-17.33;50.16]	-0.07 [-1.04;0.91] [-1.45;1.32]		0.41 [-1.26;2.09] [-1.12;1.95]	-0.16 [-1.35;1.03] [-1.19;0.86]	6.38 [-9.29;22.04] [-6.27;19.03]	0.68 [-4.69;6.05] [-7.09;8.45]
num. obs	74	74	48		74	48	74	48
(c) Post-quota (ARG: 1993-2015; PBA: 1997-2015)								
<i>Magnitude</i> <sup>†</sup>	2.48 [-1.73;6.69] [-1.58;6.54]	4.48 [-1.21;10.18]	0.38 [-0.17;0.93] [-0.27;1.03]	0.45 [-0.43;1.33]	1.70 [0.41;2.99] [0.36;3.03]	0.66 [0.04;1.27] [-0.05;1.37]	15.34 [2.34;28.34] [3.32;27.37]	1.63 [-1.12;4.39] [-0.88;4.14]
num. obs	228	228	80	80	228	80	228	80
(d) Post-quota (pre-2001) (ARG: 1993-1999)								
<i>Magnitude</i> <sup>†</sup>	5.44 [-7.90;18.78] [-6.56;17.44]	11.17 [-0.57;22.92]			3.07 [-0.03;6.17] [0.58;5.56]		23.86 [-5.93;53.66] [-2.43;50.15]	
num. obs	76	76			76		76	
sample model	ARG OLS	ARG tobit	PBA OLS	PBA tobit	ARG OLS	PBA OLS	ARG OLS	PBA OLS

All specifications include district and year fixed effects. Values in square brackets report 95% CIs. Those reported on the second row of each panel are based on robust standard errors clustered by district, and assuming a Student's  $t$ -distribution with degrees of freedom equal to the number of districts minus one. The CIs reported in the third row of columns (1), (3) and (5)-(8) are based on the wild bootstrap procedure proposed by Cameron and Miller (2015). (\*) Values for the pre-quota period are not reported because the model did not converge. (†)  $\log(Magnitude)$  in columns (5)-(6). (‡) We added 1 to all observations before logging.

Table 2: District magnitude and intermediate outcomes in Argentina and Buenos Aires, 1985-2015

(a) Outcome: <i>Party magnitude</i>						
	Full sample		Pre-Quota		Post-quota	
<i>Magnitude</i>	0.33	0.16	0.29	0.22	0.35	0.12
	[0.18:0.48]	[-0.01:0.32]	[0.06:0.51]	[0.07:0.38]	[0.16:0.53]	[-0.07:0.30]
	[0.19:0.47]	[-0.17:0.48]	[0.10:0.47]	[-0.01:0.46]	[0.17:0.53]	[-0.17:0.40]
num. obs	302	128	74	48	228	80
sample	ARG	PBA	ARG	PBA	ARG	PBA
(b) Outcome: Women's position in list						
	Women First		Women Second		Women First Two	
	(%)	(%, wt.)	(%)	(%, wt.)	(%)	(%, wt.)
<i>Magnitude</i>	-0.13	3.34	-0.95	-7.34	-0.54	-2.00
	[-4.76:4.50]	[-3.02:9.71]	[-7.28:5.38]	[-13.78:-0.90]	[-2.98:1.90]	[-3.94:-0.06]
	[-4.62:4.36]	[-3.01:9.70]	[-7.19:5.30]	[-13.14:-1.55]	[-3.05:1.97]	[-3.83:-0.17]
num. obs	209	209	209	209	209	209
sample	ARG	ARG	ARG	ARG	ARG	ARG

OLS regression estimates. All specifications include district and year fixed effects. Values in square brackets report 95% CIs. Those reported on the second row of each panel are based on robust standard errors clustered by district, and assuming a Student's *t*-distribution with degrees of freedom equal to the number of districts minus one. The CIs reported in the third row of each panel are based on the wild bootstrap procedure proposed by Cameron and Miller (2015).

*Mechanisms.* The previous results show a positive but not entirely reliable effect of *Magnitude* on the election of women. The effect is already discernible in the raw data (see Figure 1), though consistent with the minimal compliance mechanism, the effect is nil before the introduction of gender quotas but becomes positive afterwards. In this section we extend the analysis in two steps. First, Table 2 examines how district magnitude affects two possible mediating variables. Panel (a) shows a sizable positive effect of *Party magnitude* – defined as the median number of representatives elected among all lists that received at least one seat –, especially in the Argentine sample. Furthermore, there is little difference between the pre- and post-quota periods, which is to be expected as gender quotas should not affect the size of party delegations. In turn, panel (b) looks at whether *Magnitude* influences parties' decisions to nominate women candidates to the top two positions in party lists, for which we only have data for Argentina after 1995. The first two columns indicate that larger magnitudes do not increase the proportion of women heading party lists, regardless of whether we weight them equally or by their vote shares. However, the next two columns show that increasing *Magnitude* by one *reduces* the proportion of women nominated

in the second place of the list by 7.3 percentage points, but only when lists are weighted by their vote shares. As seen in the last two models of Table 2b, this results in an overall reduction in the % of women in the first two positions of the list. In other words, some women candidates are being displaced to lower-ranked positions in the list, and it is large parties – those that can expect to elect at least three candidates and thus are legally allowed to nominate a woman in the third position of the list (Jones 1998; Tula 2004) – that are driving the effect. This is inconsistent with the balancing mechanism – why send women to a less attractive position when you can nominate more candidates? –, but fits nicely with the minimal compliance story: if parties are reluctant to nominate women unless legally mandated to do so, an increase in district magnitude allows them to comply by sending women to lower-ranked positions.

The next step is to examine whether district magnitude still has an effect on women's representation after accounting for the role of these mediating variables. To do so, we estimated the *controlled direct effect* (henceforth, CDE) of *Magnitude* on the outcomes of interest. The CDE of an explanatory variable  $X$  can be interpreted as the effect of  $X$  on some outcome variable  $Y$  while fixing the mediator  $M$  at some certain value  $m$  for all units in the population (Acharya, Blackwell and Sen 2016). Since the value of the mediator may be influenced by some confounding variable  $Z$  in addition to  $X$ , Acharya, Blackwell and Sen (2016) propose a two-step estimation procedure in which the outcome is first regressed on the treatment, the mediator and all potential confounders, and then the outcome minus the effect of the mediator is regressed on the treatment and the pre-treatment confounders.

Table 3 presents the results. Besides country and year fixed effects, in the Argentine sample we also included a set of dummies indicating concurrency with elections for the Senate, the governorship, or the provincial legislature; whether the governor was running for re-election or appeared in the ballot in another way (i.e., as a candidate for the Senate); and whether the incumbent governor was legally allowed to run for re-election.<sup>15</sup> Panels (a) to (c) look at the CDE of *Magnitude* net of the effect of *Party magnitude*.

---

<sup>15</sup>The logic behind these variables is that concurrent elections, an incumbent running or a governor who is not a lame duck may affect party magnitude or the position of women candidates in the party list independently of district magnitude by increasing the level of electoral support (Jones 1997) or the internal unity (De Luca, Jones and Tula 2002) of the governor's party, or by

Table 3: Controlled direct effects of district magnitude on women's representation, 1985-2015

	<i>Women elected (%)</i>		<i>Women elected (%) (log)<sup>‡</sup></i>		<i>Woman elected (0/100)</i>	
(a) <i>Party magnitude</i> (1): Full sample						
CDE of <i>Magnitude</i> <sup>†</sup>	0.02	-0.08	0.52	-0.04	8.40	0.88
	[-3.77:3.80]	[-0.66:0.49]	[-0.40:1.44]	[-0.65:0.58]	[0.17:16.62]	[-1.45:3.21]
num. obs	302	128	302	128	302	128
(b) <i>Party magnitude</i> (2): Pre-quota (ARG: 1985-1991; PBA: 1985-1995)						
CDE of <i>Magnitude</i> <sup>†</sup>	-0.87	0.09	-0.89	-0.05	-1.17	2.41
	[-5.68:3.93]	[-0.76:0.93]	[-2.37:0.58]	[-1.16:1.06]	[-15.81:13.46]	[-2.74:7.55]
num. obs	74	48	74	48	74	48
(c) <i>Party magnitude</i> (3): Post-quota (ARG: 1993-2015; PBA: 1997-2015)						
CDE of <i>Magnitude</i> <sup>†</sup>	-0.05	0.08	0.79	0.22	9.98	1.03
	[-4.76:4.65]	[-0.68:0.83]	[-0.29:1.88]	[-0.42:0.85]	[0.55:19.40]	[-0.79:2.86]
num. obs	228	80	228	80	228	80
(d) <i>Women First (%)</i> (1995-2015)						
CDE of <i>Magnitude</i> <sup>†</sup>	2.15		1.49		13.83	
	[-2.94:7.25]		[0.27:2.72]		[3.81:23.84]	
num. obs	209		209		209	
(e) <i>Women First (% , weighted)</i> (1995-2015)						
CDE of <i>Magnitude</i> <sup>†</sup>	1.06		1.44		12.39	
	[-3.75:5.87]		[0.27:2.61]		[2.64:22.14]	
num. obs	209		209		209	
(f) <i>Women Second (%)</i> (1995-2015)						
CDE of <i>Magnitude</i> <sup>†</sup>	1.96		1.52		13.45	
	[-3.24:7.15]		[0.28:2.76]		[3.53:23.37]	
num. obs	209		209		209	
(g) <i>Women Second (% , weighted)</i> (1995-2015)						
CDE of <i>Magnitude</i> <sup>†</sup>	1.17		1.44		12.22	
	[-3.97:6.32]		[0.20:2.68]		[2.29:22.14]	
num. obs	209		209		209	
(h) <i>Women First Two (%)</i> (1995-2015)						
CDE of <i>Magnitude</i> <sup>†</sup>	2.18		1.52		13.63	
	[-3.07:7.42]		[0.27:2.78]		[3.54:23.72]	
num. obs	209		209		209	
(i) <i>Women First Two (% , weighted)</i> (1995-2015)						
CDE of <i>Magnitude</i> <sup>†</sup>	3.04		1.57		14.64	
	[-2.08:8.16]		[0.32:2.82]		[4.51:24.78]	
num. obs	209		209		209	
sample	ARG	PBA	ARG	PBA	ARG	PBA

CDE estimates (Acharya, Blackwell and Sen 2016). All specifications include district and year fixed effects. Values in square brackets report 95% CIs based on standard errors adjusted to account for the uncertainty introduced by the two-step estimation procedure, and assuming a Student's *t*-distribution with degrees of freedom equal to the number of districts minus one. (†)  $\log(Magnitude)$  in columns (3)-(4). (‡) We added 1 to all observations before logging.

In contrast with the estimates of columns (1) and (3) of Table 1, which were positive even if not statistically significant, the CDEs are almost exactly zero, indicating that after accounting for the effect of *party* magnitude, *district* magnitude plays little role in the election of women. The next two columns show a similar story for the log-log model:<sup>16</sup> although some estimates remain positive, they are cut by more than half, and become far from statistically significant. The last two columns show that *Magnitude* seems to have a positive and statistically significant controlled direct effect on the probability of electing at least one woman, though the point estimates are cut by a third with respect to those reported in Table 1.

The following six panels replicate the analysis for the proportion of women in top positions in party lists, for which we only have data for Argentina since 1995. In contradiction with the balancing mechanism, there is little difference with respect to the estimates reported in Table 1c; some estimates are reduced, but only by a modest amount. Whatever effect district magnitude has on the election of women, it is driven by *Party magnitude*, as the minimal compliance mechanism predicts, rather than by women's relative position in party lists. To put it differently, district magnitude matters for women's representation chiefly because it makes some lists elect more representatives, and these are disproportionately likely to be women.

*Balance check and placebo tests.* To strengthen the credibility of these findings, we performed two additional tests. For the identification strategy to be valid, districts that have a larger magnitude in midterm or concurrent years should not be systematically different in terms of their pre-treatment characteristics. Tables A1 and A2 and the balance checks reported in Appendix A3 shows that this is indeed the case. Second, in Table A6 we report the results for a series of placebo tests in which the outcome is some time-varying covariate that should not be affected by periodic changes in district magnitude – such as provincial rev-

---

increasing the supply of experienced candidates (mostly men; see Franceschet and Piscopo 2014) in top list positions. We did not include similar variables in the Buenos Aires sample because they would be perfectly collinear with the year fixed effects.

We estimated the CDE using the `DirectEffects` package in R.

<sup>16</sup>In this case, we took the natural logarithm of the outcome ( $+ 1$ ), of *Magnitude*, and of *Party magnitude*.



enues, the number of public employees, or the unemployment and infant mortality rates.<sup>17</sup> In line with the claim that district magnitude should have no effect on these outcomes, the point estimates are not only statistically insignificant, but very close to zero in substantive terms.

## Discussion

Despite some important exceptions (De Paola, Scoppa and De Benedetto 2014; Casas-Arce and Saiz 2015; O’Brien and Rickne 2016; Besley et al. 2017; González-Eiras and Sanz 2018; John, Smith and Zack 2018), “crucial experiments” that can isolate the effects of electoral rules from that of other potentially confounding factors are still rare (Shugart 2005). In order to both identify the effect of district magnitude on women’s representation and to adjudicate between two alternative mechanisms that may be driving this effect, in this paper we took advantage of the staggered renewal rules employed in Argentina and the province of Buenos Aires, and combined it with the introduction of gender quotas in both polities during the 1990s. Our research design has the further advantage of allowing us to look at the oscillation of the electoral calendar over three decades; this way, we are confident that our results cannot be attributed to a handful of elections, and can interpret our results causally both before and after the introduction of gender quotas. We found a positive but modest effect of district magnitude on women’s representation, though only when quotas were in place. While these results are insignificant for the specifications in levels, the log-log models show a large and significant effect, and we also find that after 1993 a unit increase in *Magnitude* increases the probability that an Argentine province will elect at least one woman by a whopping 15 percentage points.

That said, the small number of observations (and clusters) reduces the reliability of our estimates. In addition, we can only focus on short-term changes in *Magnitude* rather than on more permanent increases. Extrapolating from the Argentine case to scenarios involving larger shifts in magnitude may be problematic insofar as the Argentine provinces only observe limited changes in district magnitude

---

<sup>17</sup>Time-varying data of this kind is only available for Argentina.

between elections. Changes in magnitude are much larger in the province of Buenos Aires, but somewhat surprisingly the results are weaker for this sample. A potential explanation is that in Buenos Aires the electoral threshold depends on the Hare quota, which in turn depends on district magnitude (see fn. 7). Smaller magnitudes imply higher thresholds: for example, when  $m = 3$ , a party needs 33% of valid votes to obtain representation, and even with  $m = 5$ , only parties getting at least 20% of the vote can win any seats. To the extent that this deprives small parties from representation, then the largest parties will receive enough seats for quotas to kick in, even in small-magnitude districts. The fact that district magnitudes likely have diminishing effects on women's representation (Schwindt-Bayer 2005) may also play a role.

It is also worth noting that the effects we find are relatively modest compared with that of quotas: between 1991 and 1993, the percentage of women elected to the Argentine Chamber of Deputies in the 19 provinces included in the sample jumped from 2.7% to 19.4% (it would reach 30.7% in 1995), while the Buenos Aires legislature witnessed an increase from 15.9% to 27% between 1995 and 1997. Still, our results indicate that district magnitude can be a useful aide to gender quotas. In contrast with the balancing mechanism, but in line with the minimal compliance story, we find that the effect of *Magnitude* only becomes positive *after* the introduction of gender quotas.<sup>18</sup> The results in Tables 2 and 3 confirm this fact: on the one hand, larger magnitudes induce large parties place women in lower-ranked positions in party lists – the opposite of what the balancing mechanism predicts. On the other, the controlled direct effect of *Magnitude* tends to zero when *Party magnitude* – but not different measures of women's position in party lists – is accounted for. In this regard, our results point to a somewhat bittersweet conclusion: quotas are working, but only because party elites are forced to comply with them. In contrast to previous work showing a positive effect of quotas on either reluctant compliers' vote shares (Casas-Arce and Saiz 2015) or the quality of political selection more generally (Besley et al. 2017), women politicians in Argentina do not seem to be making inroads besides being elected in larger numbers (see Esteve-Volart and Bagues 2012 for a similar observation with regard to Spain). After a jump in 2001, the proportion of

---

<sup>18</sup>To be sure, the number of observations is smaller for the pre-quota sample. However, this cannot explain the results, as the size of the estimates is much smaller for the pre-quota era.

women candidates in the first two positions of the party list has not increased much.<sup>19</sup> Perhaps for this reason, both Argentina and Buenos Aires have recently adopted a zipper quota mandating a 50-50 gender split; while this will increase the proportion of women elected, the results from this paper suggest that most parties will opt to place women candidates in the second, rather than the first, position in the list.

## References

- Acharya, Avidit, Matthew Blackwell and Maya Sen. 2016. "Explaining Causal Findings Without Bias: Detecting and Assessing Direct Effects." *American Political Science Review* 110(3):512–529.
- Angrist, Joshua and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics. An Empiricist's Companion*. Princeton: Princeton University Press.
- Barnes, Tiffany D. 2016. *Gendering Legislative Behavior: Institutional Constraints and Collaboration*. Cambridge: Cambridge University Press.
- Besley, Timothy, Olle Folke, Torsten Persson and Johanna Rickne. 2017. "Gender Quotas and the Crisis of the Mediocre Man: Theory and Evidence from Sweden." *American Economic Review* 107(8):2204–2242.
- Cameron, A. Colin and Douglas L. Miller. 2015. "A Practitioner's Guide to Cluster-Robust Inference." *Journal of Human Resources* 50(2):317–372.
- Caminotti, Mariana, Santiago Rotman and Carlos Varetto. 2011. "Carreras Políticas y Oportunidades "Generizadas" en la Provincia de Buenos Aires (1983-2007)." *PostDATA* 16(2):191–221.
- Casas-Arce, Pablo and Albert Saiz. 2015. "Women and Power: Unpopular, Unwilling, or Held Back?" *Journal of Political Economy* 123(3):641–669.
- Crisp, Brian F., Joshua D. Potter and John J. W. Lee. 2012. "Entry and Coordination in Mixed-Member Systems: A Controlled Comparison. Testing the Contamination Hypothesis." *The Journal of Politics* 74(2):571–583.
- Dahlerup, Drude and Lenita Freidenvall. 2005. "Quotas as a "Fast Track" to Equal Representation for Women: Why Scandinavia Is No Longer the Model." *International Feminist Journal of Politics* 7(1):26–48.
- Dal Bó, Ernesto and Martín A. Rossi. 2011. "Term Length and the Effort of Politicians." *Review of Economic Studies* 78(4):1237–1263.
- De Luca, Miguel, Mark P. Jones and María Inés Tula. 2002. "Back Rooms or Ballot Boxes? Candidate Nomination in Argentina." *Comparative Political Studies* 35(4):413–436.
- De Paola, Maria, Vincenzo Scoppa and Marco Alberto De Benedetto. 2014. "The Impact of Gender Quotas on Electoral Participation: Evidence from Italian Municipalities." *European Journal of Political Economy* 35:141–157.
- Esarey, Justin and Andrew Menger. forthcoming. "Practical and Effective Approaches to Dealing with Clustered Data." *Political Science Research and Methods*.
- Esteve-Volart, Berta and Manuel Bagues. 2012. "Are Women Pawns in the Political Game? Evidence from Elections to the Spanish Senate." *Journal of Public Economics* 96(3):387–399.
- Fiva, Jon H. and Olle Folke. 2016. "Mechanical and Psychological Effects of Electoral Reform." *British Journal of Political Science* 46(2):265–279.
- Franceschet, Susan and Jennifer M. Piscopo. 2008. "Gender Quotas and Women's Substantive Representation: Lessons from Argentina." *Politics & Gender* 4(3):393–425.
- Franceschet, Susan and Jennifer M. Piscopo. 2014. "Sustaining Gendered Practices? Power, Parties, and Elite Political Networks in Argentina." *Comparative Political Studies* 47(1):85–110.

<sup>19</sup>Results not shown but available upon request from the authors.

- Gerring, John, Maxwell Palmer, Jan Teorell and Dominic Zarecki. 2015. "Demography and Democracy: A Global, District-level Analysis of Electoral Contestation." *American Political Science Review* 109(3):574–591.
- González-Eiras, Martín and Carlos Sanz. 2018. "Women's Representation in Politics: The Importance of the Electoral System." Unpublished manuscript.
- Htun, Mala. 2004. "Is Gender like Ethnicity? The Political Representation of Identity Groups." *Perspectives on Politics* 2(03):439–458.
- Htun, Mala N. and Mark P. Jones. 2002. Engendering the Right to Participate in Decision-Making: Electoral Quotas and Women's Leadership in Latin America. In *Gender and the politics of rights and democracy in Latin America*, ed. Maxine Molyneux and N. Craske. London: Palgrave Macmillan pp. 32–56.
- John, Sarah, Haley Smith and Elizabeth Zack. 2018. "The Alternative Vote: Do Changes in Single-Member Voting Systems Affect Descriptive Representation of Women and Minorities?" *Electoral Studies* 54:90–102.
- Jones, Mark P. 1997. "Federalism and the Number of Parties in Argentine Congressional Elections." *The Journal of Politics* 59(2):538–549.
- Jones, Mark P. 1998. "Gender Quotas, Electoral Laws, and the Election of Women: Lessons from the Argentine Provinces." *Comparative Political Studies* 31(1):3–21.
- Jones, Mark P. 2009. "Gender Quotas, Electoral Laws, and the Election of Women Evidence From the Latin American Vanguard." *Comparative Political Studies* 42(1):56–81.
- Jones, Mark P, Santiago Alles and Carolina Tchintian. 2012. "Cuotas de Género, Leyes Electorales y Elección de Legisladoras en América Latina." *Revista de Ciencia Política (Santiago)* 32(2):331–357.
- Kedar, Orit, Liran Harshgor and Raz A. Sheiner. 2016. "Are Voters Equal under Proportional Representation?" *American Journal of Political Science* 60(3):676–691.
- Krook, Mona Lena. 2007. "Candidate Gender Quotas: A Framework for Analysis." *European Journal of Political Research* 46(3):367–394.
- Krook, Mona Lena. 2018. Electoral Systems and Women's Representation. In *The Oxford Handbook of Electoral Systems*, ed. Erik S. Herron, Robert J. Pekkanen and Matthew Søberg Shugart. Oxford: Oxford University Press pp. 175–192.
- Lago, Ignacio and Ferran Martínez. 2007. "The Importance of Electoral Rules: Comparing the Number of Parties in Spain's Lower and Upper Houses." *Electoral Studies* 26(2):381–391.
- Larsrud, Stina and Rita Taphorn. 2007. "Designing for Equality: Women's Quotas and Women's Political Participation." *Development* 50(1):36–42.
- Lodola, Germán. 2009. "La Estructura Subnacional de las Carreras Políticas en Argentina y Brasil." *Desarrollo Económico* 49(194):247–286.
- Lucardi, Adrián and Juan Pablo Micozzi. 2016. "The Effect of the Electoral Cycle on Legislators' Career Strategies. Evidence from Argentina, 1983–2007." *Legislative Studies Quarterly* 41(4):811–840.
- Lupu, Noam and Susan C. Stokes. 2009. "The Social Bases of Political Parties in Argentina, 1912–2003." *Latin American Research Review* 44(1):58–87.
- Matland, Richard E. 1993. "Institutional Variables Affecting Female Representation in National Legislatures: The Case of Norway." *The Journal of Politics* 55(3):737–755.
- Matland, Richard E. and Michelle M. Taylor. 1997. "Electoral System Effects on Women's Representation Theoretical Arguments and Evidence from Costa Rica." *Comparative Political Studies* 30(2):186–210.
- Meserve, Stephen A., Daniel Pemstein and William T. Bernhard. forthcoming. "Gender, Incumbency and Party List Nominations." *British Journal of Political Science*.
- Mitton, Todd. 2016. "The Wealth of Subnations: Geography, Institutions, and Within-Country Development." *Journal of Development Economics* 118:88–111.
- Monroe, Burt L. and Amanda G. Rose. 2002. "Electoral Systems and Unimagined Consequences: Partisan Effects of Districted Proportional Representation." *American Journal of Political Science* 46(1):67–89.
- O'Brien, Diana Z. and Johanna Rickne. 2016. "Gender Quotas and Women's Political Leadership." *American Political Science Review* 110(1):112–126.
- Piscopo, Jennifer M. 2015. "States as Gender Equality Activists: The Evolution of Quota Laws in Latin America." *Latin American Politics and Society* 57(3):27–49.

- Reynolds, Andrew. 1999. "Women in the Legislatures and Executives of the World: Knocking at the Highest Glass Ceiling." *World Politics* 51(4):547-572.
- Roberts, Andrew, Jason Seawright and Jennifer Cyr. 2013. "Do Electoral Laws Affect Women's Representation?" *Comparative Political Studies* 46(12):1555-1581.
- Rosen, Jennifer. 2017. "Gender Quotas for Women in National Politics: A Comparative Analysis across Development Thresholds." *Social Science Research* 66:82-101.
- Salmond, Rob. 2006. "Proportional Representation and Female Parliamentarians." *Legislative Studies Quarterly* 31(2):175-204.
- Schmidt, Gregory D. 2009. "The Election of Women in List PR Systems: Testing the Conventional Wisdom." *Electoral Studies* 28(2):190-203.
- Schmidt, Gregory D. and Kyle L. Saunders. 2004. "Effective Quotas, Relative Party Magnitude, and the Success of Female Candidates: Peruvian Municipal Elections In Comparative Perspective." *Comparative Political Studies* 37(6):704-734.
- Schwindt-Bayer, Leslie A. 2005. "The Incumbency Disadvantage and Women's Election to Legislative Office." *Electoral Studies* 24(2):227-244.
- Schwindt-Bayer, Leslie A. 2009. "Making Quotas Work: The Effect of Gender Quota Laws On the Election of Women." *Legislative Studies Quarterly* 34(1):5-28.
- Schwindt-Bayer, Leslie A. 2010. *Political Power and Women's Representation in Latin America*. Oxford University Press.
- Shugart, Matthew S. 2005. Comparative Electoral Systems Research: The Maturation of a Field and New Challenges Ahead. In *The Politics of Electoral Systems*, ed. Michael Gallagher and Paul Mitchell. Oxford: Oxford University Press pp. 25-55.
- Thames, Frank C. and Margaret S. Williams. 2010. "Incentives for Personal Votes and Women's Representation in Legislatures." *Comparative Political Studies* 43(12):1575-1600.
- Tow, Andrés. N.d. "Atlas Electoral de Andy Tow." <http://towsa.com/wordpress/>.
- Tripp, Aili Mari and Alice Kang. 2008. "The Global Impact of Quotas: On the Fast Track to Increased Female Legislative Representation." *Comparative Political Studies* 41(3):338-361.
- Tula, María Inés. 2004. La Ley de Cupos en la Argentina: Reforma Electoral y Representación Política. In *VI Jornadas de Sociología, Facultad de Ciencias Sociales, Universidad de Buenos Aires*. Buenos Aires: .

# Online Appendix

- (1) Section [A1](#) describes the electoral calendar of the Argentine Chamber of Deputies and the legislature of the province of Buenos Aires.
- (2) Section [A2](#) presents the descriptive statistics.
- (3) Section [A3](#) presents the results of the balance checks.
- (4) Section [A4](#) reports the placebo tests for Argentina.

# A1 Electoral calendar

This section describes in detail the electoral calendar for the lower chamber of the Argentine Congress and the legislature of the province of Buenos Aires. Table A1 lists all districts that elect representatives to the Argentine Chamber of Deputies and indicates their magnitude in midterm and concurrent election years. Figure A1 plots them in the map. Tables A2 and Figure A2 do the same for the legislature of the province of Buenos Aires.

Table A1: Delegation sizes and district magnitudes in Argentina, 1985-2015

province	in sample?	delegation size	magnitude (midterm)	magnitude (concurrent)
Catamarca				
La Pampa				
Neuquén	Yes	5	3	2
San Luis				
Santa Cruz				
Chubut				
Formosa				
La Rioja	Yes	5	2	3
Río Negro				
Tierra del Fuego*				
Jujuy	No	6	3	3
San Juan				
Chaco	Yes	7	4	3
Corrientes <sup>†</sup>				
Misiones	Yes	7	3	4
Salta				
Santiago del Estero <sup>‡</sup>				
Entre Ríos	Yes	9	5	4
Tucumán	Yes	9	4	5
Mendoza	No	10	5	5
Córdoba	No	18	9	9
Santa Fe	Yes	19	9	10
Ciudad de Buenos Aires	Yes	25	13	12
Buenos Aires	No	70	35	35
Total	19/24	257	127	130
mean		10.7	5.3	5.4
median		6.5	3.0	3.0

Midterm years: 1985, 1989, 1993, 1997, 2001, 2005, 2009 and 2013. Concurrent years: 1987, 1991, 1995, 1999, 2003, 2007, 2011 and 2015. (\*) Elected only 2 deputies before 1991 (in midterm years). (†) The ordering of midterm and concurrent elections is reversed after 1993, when the subnational electoral calendar changed. (‡) The ordering of midterm and concurrent elections is reversed after 2005, when the subnational electoral calendar changed.



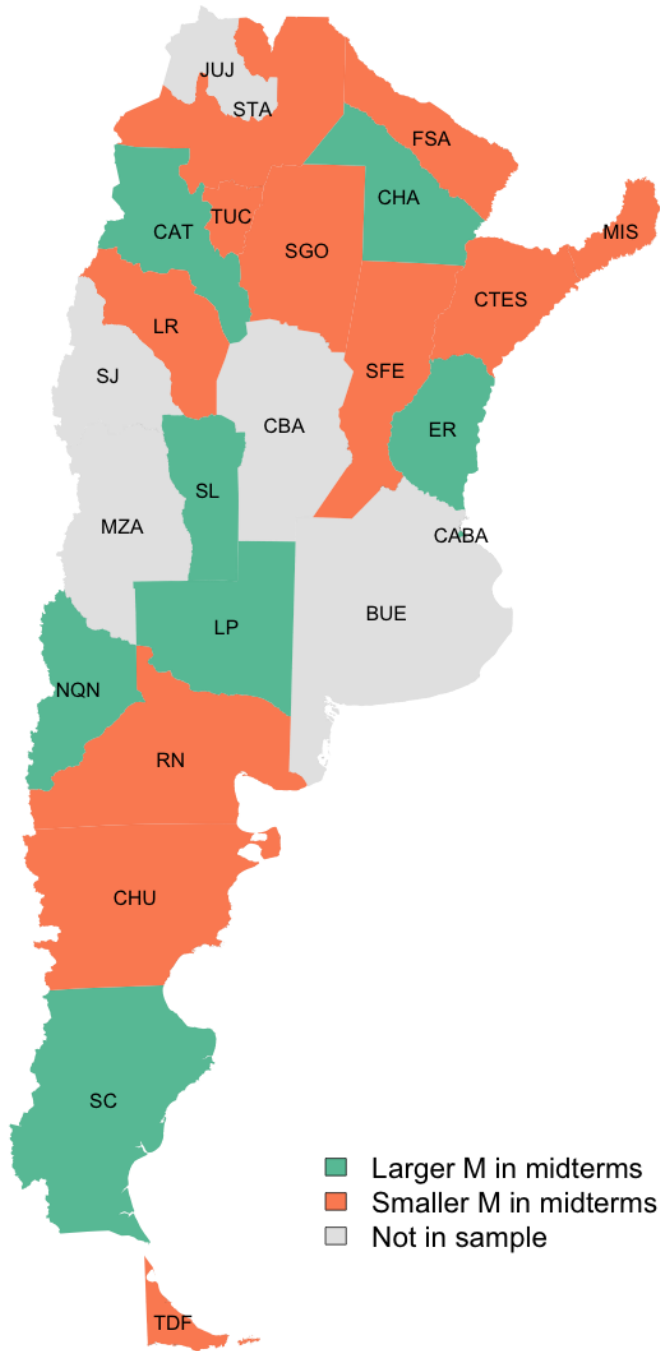


Figure A1: The electoral calendar for the Argentine Chamber of Deputies, 1985-2015. See Table A1 for further details.

Table A2: Delegation sizes and district magnitudes in Buenos Aires, 1985-2015

district	in sample?	delegation size	magnitude (midterm)	magnitude (concurrent)
<i>sección VIII</i>	Yes	3 (upper) + 6 (lower)	6	3
<i>sección VII</i>	Yes	3 (upper) + 6 (lower)	3	6
<i>sección II</i>	Yes	5 (upper) + 11 (lower)	11	5
<i>sección V</i>	Yes	5 (upper) + 11 (lower)	5	11
<i>sección VI</i>	Yes	6 (upper) + 11 (lower)	11	6
<i>sección IV</i>	Yes	7 (upper) + 14 (lower)	7	14
<i>sección I</i>	Yes	8 (upper) + 15 (lower)	8	15
<i>sección III</i>	Yes	9 (upper) + 18 (lower)	18	9
Total	8/8	46 (upper) + 92 (lower)	69	69
mean		5.8 (upper) + 11.5 (lower)	8.6	8.6
median		5.5 (upper) + 11 (lower)	7.5	7.5

Midterm years: 1985, 1989, 1993, 1997, 2001, 2005, 2009 and 2013. Concurrent years: 1987, 1991, 1995, 1999, 2003, 2007, 2011 and 2015.

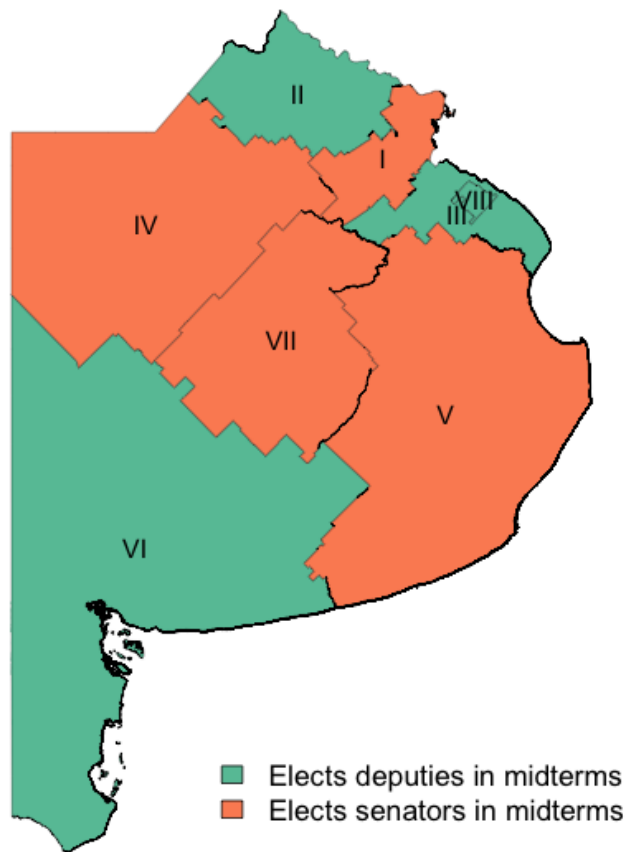


Figure A2: The electoral calendar for the legislature of the province of Buenos Aires, 1985-2015. See Table A2 for further details.

## A2 Descriptive statistics

Table A3: Descriptive statistics

(a) Full sample (1985-2015)	Argentina					Province of Buenos Aires				
	<i>N</i>	mean	sd.*	min.	max.	<i>N</i>	mean	sd.*	min.	max.
<i>Magnitude</i>	302	3.9	2.6	2.0	13.0	128	8.6	4.3	3.0	18.0
<i>Women elected (%)</i>	302	23.9	20.8	0.0	100.0	128	18.3	13.6	0.0	50.0
<i>Woman elected (0/100)</i>	302	62.9	48.4	0.0	100.0	128	73.4	44.3	0.0	100.0
<i>Party magnitude</i>	302	1.7	0.8	1.0	5.0	128	3.5	1.5	1.0	9.0
(b) Pre-quota (ARG: 1985-1991; PBA: 1985-1995)										
<i>Magnitude</i>	74	3.9	2.7	2.0	13.0	48	8.6	4.3	3.0	18.0
<i>Women elected (%)</i>	74	3.9	10.5	0.0	50.0	48	6.9	8.6	0.0	33.3
<i>Woman elected (0/100)</i>	74	13.5	34.4	0.0	100.0	48	45.8	50.4	0.0	100.0
<i>Party magnitude</i>	74	1.6	0.7	1.0	4.0	48	3.5	1.3	1.0	7.0
(c) Post-quota (ARG: 1993-2015; PBA: 1997-2015)										
<i>Magnitude</i>	228	3.9	2.6	2.0	13.0	80	8.6	4.3	3.0	18.0
<i>Women elected (%)</i>	228	30.4	19.0	0.0	100.0	80	25.2	11.2	0.0	50.0
<i>Woman elected (0/100)</i>	228	78.9	40.9	0.0	100.0	80	90.0	30.2	0.0	100.0
<i>Party magnitude</i>	228	1.8	0.8	1.0	5.0	80	3.5	1.7	1.5	9.0
<i>Women First (%)</i> †	209	16.9	15.6	0.0	75.0					
<i>Women First (% weighted)</i> †	209	16.7	23.0	0.0	82.2					
<i>Women Second (%)</i> †	209	74.4	23.2	0.0	100.0					
<i>Women Second (% weighted)</i> †	209	74.5	29.4	0.0	100.0					
<i>Women First Two (%)</i> †	209	45.6	10.6	0.0	66.7					
<i>Women First Two (% weighted)</i> †	209	45.6	12.0	0.0	82.0					
(d) Post-quota (pre-2001) (ARG: 1993-1999)										
<i>Magnitude</i>	76	3.9	2.6	2.0	13.0					
<i>Women elected (%)</i>	76	22.2	19.7	0.0	66.7					
<i>Woman elected (0/100)</i>	76	61.8	48.9	0.0	100.0					
<i>Party magnitude</i>	76	1.6	0.6	1.0	3.5					

(\*) Indicates the within-province standard deviation rather than the sample standard deviation. (†) Data for these variables is only available for the Argentine sample between 1995 and 2015.

## A3 Balance check

If the choice of which districts would elect more deputies in midterm than in concurrent years was decided randomly, districts that elect more representatives in midterm years<sup>1</sup> should not differ systematically from those that have higher magnitudes in concurrent years.<sup>2</sup> To check whether this is the case, we collected data on 35 (for Argentina) or 36 (for Buenos Aires) pre-treatment covariates and examined the difference in means between both groups of districts in each sample.

These covariates include (a) the outcome variables, as measured in the 1983 election;<sup>3</sup> (b) the intervening variables used to estimate the controlled direct effects of *Magnitude*;<sup>4</sup> (c) the pseudo-outcomes reported in the placebo analysis (see Table A6), again measured in 1983;<sup>5</sup> (d) a host of electoral outcomes measured in 1983, such as the effective number of parties in votes or seats, the number of seats running, and the vote share of the PJ and the UCR;<sup>6</sup> (e) several demographic variables, such as population (density);<sup>7</sup> (f)

---

<sup>1</sup>Catamarca, La Pampa, Neuquén, San Luis, Santa Cruz, Chaco, Entre Ríos and the Ciudad de Buenos Aires in Argentina (see Figure A1 and Table A1); *secciones* II, III, VI and VIII in the province of Buenos Aires (see Figure A2 and Table A2).

<sup>2</sup>Chubut, Formosa, La Rioja, Río Negro, Tierra del Fuego, Corrientes, Misiones, Salta, Santiago del Estero, Tucumán and Santa Fe in Argentina (see Figure A1 and Table A1); *secciones* I, IV, V and VII in the province of Buenos Aires (see Figure A2 and Table A2).

<sup>3</sup>Sources: Tow (N.d.) (for Argentina) and the provincial Electoral Court (<http://www.juntaelectoral.gba.gov.ar/mapa-provincia-bsas.php>) for Buenos Aires.

<sup>4</sup>Sources: Tow (N.d.) (for Argentina) and the provincial Electoral Court (<http://www.juntaelectoral.gba.gov.ar/mapa-provincia-bsas.php>) for Buenos Aires.

<sup>5</sup>Sources: BASECIAP (<http://www.econ.uba.ar/www/institutos/admin/ciap/baseciap/>) for the financial variables, and Argentina's statistical institute (INDEC; <http://www.indec.gob.ar/>) for infant mortality.

<sup>6</sup>Sources: Tow (N.d.) (for Argentina) and the provincial Electoral Court (<http://www.juntaelectoral.gba.gov.ar/mapa-provincia-bsas.php>) for Buenos Aires.

<sup>7</sup>Sources: 1980 census (from INDEC), plus Lupu and Stokes (2009) for urbanization and literacy in Buenos Aires.

an array of geographic and historical variables, including area, average latitude, elevation, precipitation, etc;<sup>8</sup> and (g) several measures of a district's electoral (over-)representation in 1983.<sup>9</sup>

Tables A4 and A5 display the results for the 19 Argentine provinces included in our sample and the province of Buenos Aires, respectively. In each table we report the means for both groups of districts, as well as the difference between the two and the exact  $p$ -values for the sharp null hypothesis that having a larger magnitude in midterm years has no effect for any district, which are also displayed in Figure A3. We calculated these using simulations. First, we sampled 100,000 vectors of eight 1's and ten 0's (or ten 1's and eight 0's), adding Tierra del Fuego to the ten-province group<sup>10</sup> (for Argentina) or four 1's and four 0's (for Buenos Aires). Each of these vectors represents a different random allocation of the districts into two groups. Second, for every draw we calculated the difference in means for each variable, and saved these values. The  $p$ -values reported in Tables A4 and A5 as well as Figure A3 indicate the proportion of draws in which the absolute value of the difference in means in the actual sample was smaller than the absolute value of the simulated differences in means. For example, the  $p$ -value of 0.89 for the log of population in the Argentine sample indicates that approximately 89,000 simulations produced a difference in means that was equal to or larger in size than the one we observe in the data.

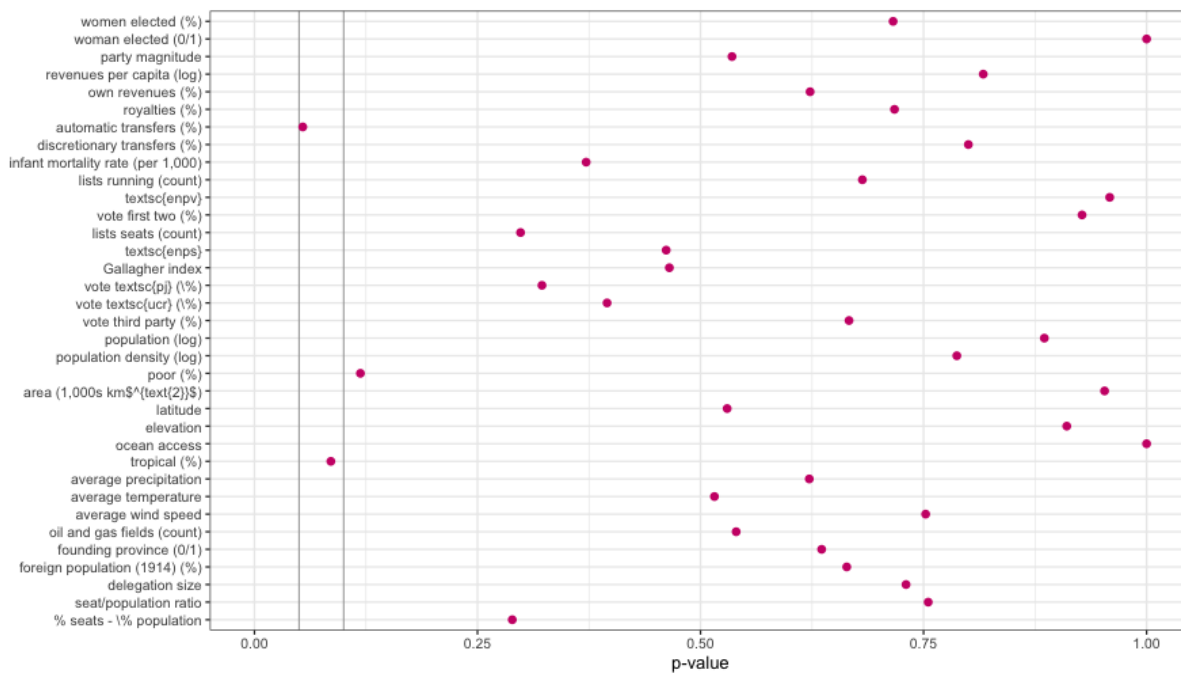
---

<sup>8</sup>Sources: INDEC and Mitton (2016). We are thankful to Todd Mitton for kindly sharing this data.

<sup>9</sup>Sources: Tow (N.d.), the 1980 census (from INDEC) and the provincial Electoral Court (<http://www.juntaelectoral.gba.gov.ar/mapa-provincia-bsas.php>) for Buenos Aires.

<sup>10</sup>This reflects the rules of the original draw that determined whether the deputies elected in 1983 would receive a two- or a four-year mandate: first, the number of deputies elected in concurrent and midterm years had to be equal; and second, the two deputies from Tierra del Fuego had to be elected simultaneously (see Dal Bó and Rossi 2011). That is, before Tierra del Fuego became a province there was a group of ten provinces with a higher magnitude in concurrent years, a group of eight with a higher magnitude in midterm years, and a district that elected its two only representatives in midterm years. Upon becoming a province, Tierra del Fuego began to elect three additional representatives in concurrent years, thus entering the former group.

(a) Argentine Chamber of Deputies ( $N = 19$ )



(b) Legislature of the Province of Buenos Aires ( $N = 8$ )

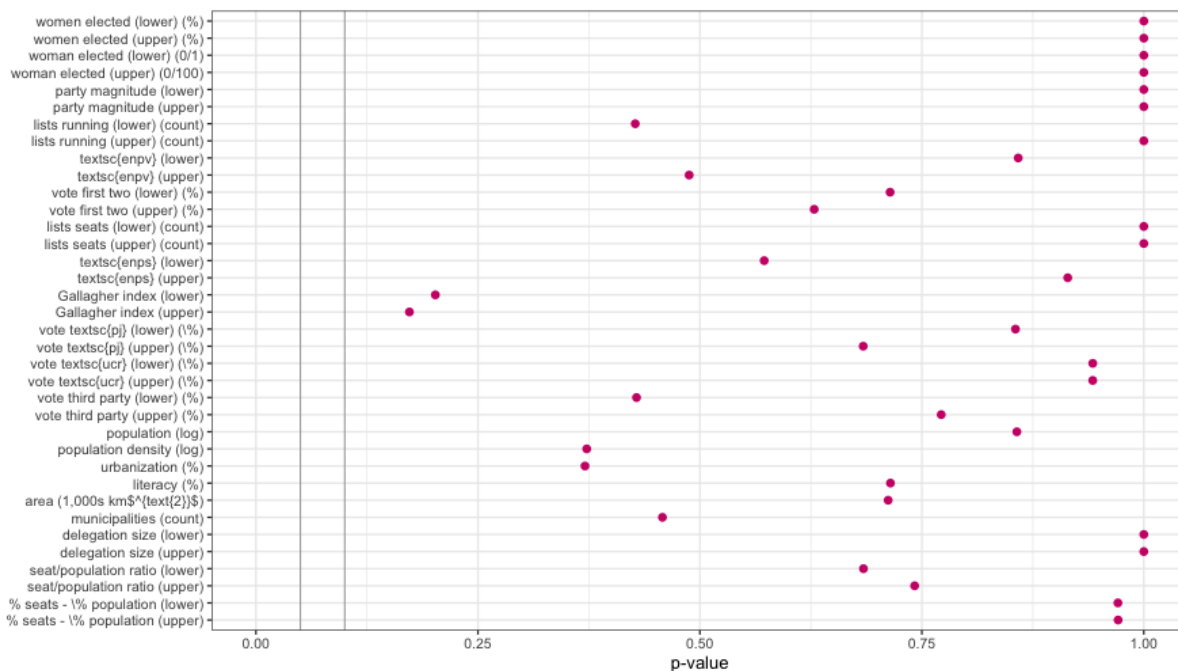


Figure A3: Checking covariate balance. The dots report the exact  $p$ -values for the sharp null hypothesis that having a higher magnitude in midterm years has no effect on any district. See Online Appendix A3 for details.

Table A4: Covariate balance (1): Argentina

(a) Outcome variables (1983)	large midterm mean	large concurrent mean	difference	p-value
<i>women elected (%)</i>	2.29	3.12	-0.83	0.72
<i>woman elected (0/1)</i>	0.25	0.18	0.07	1.00
(b) Intervening variables (1983)				
<i>party magnitude</i>	2.69	3.36	-0.68	0.54
(c) Pseudo-outcomes (1983)				
<i>revenues per capita (log)</i>	7.16	7.09	0.07	0.82
<i>own revenues (%)</i>	19.56	14.80	4.76	0.62
<i>royalties (%)</i>	12.78	9.68	3.10	0.72
<i>automatic transfers (%)</i>	28.50	33.99	-5.50	0.05
<i>discretionary transfers (%)</i>	38.83	41.00	-2.17	0.80
<i>infant mortality rate (per 1,000)</i>	35.50	39.52	-4.02	0.37
(d) Electoral outcomes (1983)				
<i>lists running (count)</i>	11.50	12.00	-0.50	0.68
<i>ENPV</i>	2.69	2.71	-0.02	0.96
<i>vote first two (%)</i>	84.49	84.99	-0.50	0.93
<i>lists seats (count)</i>	2.62	2.18	0.44	0.30
<i>ENPS</i>	2.22	2.07	0.14	0.46
<i>Gallagher index</i>	7.97	9.38	-1.41	0.47
<i>vote PJ (%)</i>	38.70	43.01	-4.31	0.32
<i>vote UCR (%)</i>	44.19	41.98	2.21	0.40
<i>vote third party (%)</i>	10.05	8.06	1.99	0.67
(e) Demographics (1980)				
<i>population (log)</i>	12.93	12.99	-0.07	0.89
<i>population density (log)</i>	2.10	1.69	0.41	0.79
<i>poor (%)</i>	31.00	39.81	-8.81	0.12
(f) Geography and history				
<i>area (1,000s km<sup>2</sup>)</i>	104.93	106.92	-1.99	0.95
<i>latitude</i>	35.11	32.52	2.58	0.53
<i>elevation</i>	6.20	6.17	0.03	0.91
<i>ocean access</i>	0.38	0.27	0.10	1.00
<i>tropical (%)</i>	20.11	52.64	-32.53	0.09
<i>average precipitation</i>	55.57	63.80	-8.22	0.62
<i>average temperature</i>	15.02	16.57	-1.55	0.52
<i>average wind speed</i>	3.53	3.39	0.14	0.75
<i>oil and gas fields (count)</i>	33.25	19.00	14.25	0.54
<i>founding province (0/1)</i>	0.38	0.55	-0.17	0.64
<i>foreign population (1914) (%)</i>	31.06	26.68	4.38	0.66
(g) Political representation (1983)				
<i>delegation size</i>	8.25	7.09	1.16	0.73
<i>seat/population ratio</i>	2.18	1.97	0.22	0.76
<i>% seats - % population</i>	0.81	0.46	0.35	0.29

Mean values of pre-treatment covariates for provinces that have a larger magnitude in midterm or concurrent years, respectively. The p-values correspond to the sharp null hypothesis that the effect of having a larger magnitude in midterm years is zero for all provinces.



Table A5: Covariate balance (2): province of Buenos Aires

(a) Outcome variables (1983)	large midterm mean	large concurrent mean	difference	p-value
<i>women elected (lower) (%)</i>	2.78	1.67	1.11	1.00
<i>women elected (upper) (%)</i>	2.78	3.12	-0.35	1.00
<i>woman elected (lower) (0/1)</i>	0.25	0.25	0.00	1.00
<i>woman elected (upper) (0/100)</i>	0.25	0.25	0.00	1.00
(b) Intervening variables (1983)				
<i>party magnitude (lower)</i>	5.50	5.75	-0.25	1.00
<i>party magnitude (upper)</i>	2.88	2.88	0.00	1.00
(c) Electoral outcomes (1983)				
<i>lists running (lower) (count)</i>	14.25	15.00	-0.75	0.43
<i>lists running (upper) (count)</i>	13.75	14.00	-0.25	1.00
<i>ENPV (lower)</i>	2.38	2.37	0.01	0.86
<i>ENPV (upper)</i>	2.29	2.34	-0.05	0.49
<i>vote first two (lower) (%)</i>	89.21	89.80	-0.58	0.71
<i>vote first two (upper) (%)</i>	91.04	90.38	0.66	0.63
<i>lists seats (lower) (count)</i>	2.25	2.00	0.25	1.00
<i>lists seats (upper) (count)</i>	2.00	2.00	0.00	1.00
<i>ENPS (lower)</i>	1.97	1.89	0.08	0.57
<i>ENPS (upper)</i>	1.88	1.89	-0.02	0.91
<i>Gallagher index (lower)</i>	6.18	7.44	-1.25	0.20
<i>Gallagher index (upper)</i>	6.55	7.77	-1.22	0.17
<i>vote PJ (lower) (%)</i>	36.38	36.07	0.30	0.85
<i>vote PJ (upper) (%)</i>	36.91	36.09	0.83	0.68
<i>vote UCR (lower) (%)</i>	52.84	53.72	-0.89	0.94
<i>vote UCR (upper) (%)</i>	54.13	54.29	-0.16	0.94
<i>vote third party (lower) (%)</i>	4.12	3.34	0.78	0.43
<i>vote third party (upper) (%)</i>	2.80	3.27	-0.47	0.77
<i>population (log)</i>	13.69	13.62	0.07	0.86
<i>population density (log)</i>	4.33	3.16	1.17	0.37
(d) Demographics (1980)				
<i>urbanization (%)</i>	89.57	81.86	7.71	0.37
<i>literacy (%)</i>	96.33	95.98	0.35	0.71
<i>area (1,000s km<sup>2</sup>)</i>	32.88	44.02	-11.14	0.71
<i>municipalities (count)</i>	13.25	18.00	-4.75	0.46
(e) Geography and history				
<i>delegation size (lower)</i>	11.50	11.50	0.00	1.00
<i>delegation size (upper)</i>	5.75	5.75	0.00	1.00
(f) Political representation (1983)				
<i>seat/population ratio (lower)</i>	1.64	1.94	-0.29	0.68
<i>seat/population ratio (upper)</i>	1.63	1.92	-0.28	0.74
<i>% seats - % population (lower)</i>	-0.13	0.13	-0.27	0.97
<i>% seats - % population (upper)</i>	-0.13	0.13	-0.27	0.97

Mean values of pre-treatment covariates for districts that have a larger magnitude in midterm or concurrent years, respectively. The p-values correspond to the sharp null hypothesis that the effect of having a larger magnitude in midterm years is zero for all districts.

## A4 Placebo tests for Argentina

To increase our confidence that the results are not an artifact of the data but rather reflect the actual effect of district magnitude on women's representation, Table A6 displays the effect of *Magnitude* on women's representation on a set of time-varying outcomes – such as provincial revenues per capita, the % of revenues coming from different sources, or outcomes such as the unemployment rate of infant mortality –, that should not be affected by short-term variations in district magnitude.

Table A6: Placebo tests. The effect of district magnitude on time-varying pseudo-outcomes in Argentina, 1985-2011

	<i>revenues per capita (log)</i>	<i>% own revenues</i>	<i>% royalties</i>	<i>% automatic transfers</i>	<i>% discretionary transfers</i>	<i>public employees (per 1,000)</i>	<i>unemployment rate (%)</i>	<i>infant mortality (per 1,000)</i>
(a) Pooled models								
<i>Magnitude</i>	0.01 [-0.13;0.15]	0.45 [-4.46;5.35]	0.23 [-3.36;3.81]	-0.72 [-6.17;4.74]	0.18 [-1.82;2.18]	-0.69 [-6.47;5.08]	-0.04 [-1.16;1.07]	-0.42 [-2.46;1.62]
(b) FE models								
<i>Magnitude</i>	-0.00 [-0.03;0.02]	-0.42 [-1.44;0.60]	0.02 [-1.27;1.31]	0.25 [-1.19;1.68]	0.37 [-0.70;1.44]	0.04 [-1.57;1.65]	-0.01 [-0.53;0.52]	-0.24 [-0.86;0.38]
num. obs	245	245	245	245	245	218	263	225
provinces	19	19	19	19	19	19	19	19
elections	13	13	13	13	13	12	14	12

OLS regression estimates. Values in square brackets report 95% confidence intervals based on robust standard errors clustered by province, and employing a Student's *t*-distribution with degrees of freedom equal to the number of provinces minus 1.