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

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October 20, 2018

Determining whether the link between district magnitude and women's representation is causal is problematic because 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: 3,816

*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 and the 2018 GEL-ALACIP meeting. We thank Olle Folke, Emilia Simison, Özge Kemahlıoğlu, Francisco Cantú, Pablo Pinto, Tiffany Barnes, Mark P. Jones, Ernesto Calvo, 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; Schwindt-Bayer 2009, 2010; Piscopo 2015; Rosen 2017; though see Reynolds 1999 for a more skeptical view) and promoted (O'Brien and Rickne 2016).[1] 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 (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 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. Districts that elect more representatives tend to be more urbanized and more socially diverse (Monroe and Rose 2002; Gerring et al. 2015; Kedar, Harsgor 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; Roberts, Seawright and Cyr 2013). Comparing elections to different chambers within the same polity (Jones 2009; Roberts, Seawright and Cyr 2013) does not solve the problem because voting behavior across multiple tiers is probably correlated (Fiva and Folke 2016), and electoral rules other than district magnitude may also vary between tiers.

[1] 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.

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-offs. 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. The implication is that as district magnitude increases, the average number of seats obtained by a party goes up, and thus lower-ranked (women) candidates 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. 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; furthermore, in Argentina a unit increase in magnitude sharply increases the probability that at least one woman will be elected. These results are clearly 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 legislature") between 1985 and 2015. For the former, we combined electoral data from Tow (N.d.) with our own dataset of candidates to the Chamber. For Buenos Aires, the provincial Electoral Court reports both electoral results and the names of successful candidates since 1983.[2]

We estimate difference-in-differences models of the form

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

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; $\mathit{Magnitude}_{d,t}$ is the total number of seats elected in district d in year t;[3] and μ_d and δ_t are district and year fixed effects, respectively.

[2] http://www.juntaelectoral.gba.gov.ar/mapa-provincia-bsas.php.

[3] 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).

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. 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 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).[4]

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). 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.

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 within districts – such as voters' attitudes to women candidates – are not worrisome because our treatment is switched on or off repeatedly within each district. Nonetheless, the fact that executive officials – presidents, governors and mayors – are elected every four years[5] means that some districts have larger

[4] 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.

[5] The president was originally elected for a six-year term, but since 1995 presidential elections take place in concurrent years.

magnitudes in years with executive elections ("concurrent years"), while in others *Magnitude* is larger in midterm elections ("midterm years").[6] If all districts had larger magnitudes in concurrent (midterm) years, this would violate the parallels 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 coat-tail 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 of four ensuring that exactly half of the upper and the lower chamber would be renewed every two years. A random draw then determined which of the two groups would elect provincial deputies rather than senators in 1985.[7] 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.

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). 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

[6] 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*.

[7] Personal interview with Pascual Cappelleri, who presided the lower chamber of the provincial legislature during 1983-1987.

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.[8] 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; it has also been successful at increasing women's representation, though less dramatically than in Argentina (see Table A3 and Barnes 2016, ch. 1).

Results

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 employing 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).[9]

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. 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

[8] We thank Name for bringing this possibility to our attention.

[9] We calculated these with the `clusterSEs` package in R (Esarey and Menger forthcoming), using 999 bootstrap replications.

enforcement mechanisms reported by Rosen (2017). 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 nontrivial effect considering that even after 1997, just 25% of elected legislators were women (see Table A3c). 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.[10]

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.[11] 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.

[10] 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).

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

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).

Mechanisms. The previous results show a positive but not entirely reliable effect of *Magnitude* on the election of women. 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. 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 sen in the last two models of Table 2b, this results in an overall reduction in the % of women in the first two position 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 east 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

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

	Women elected (%)		*Women elected (%) (log)*‡		*Woman elected (0/100)*	
(a) *Party magnitude* (1): Full sample						
CDE of *Magnitude*†	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) *Party magnitude* (2): Pre-quota (ARG: 1985-1991; PBA: 1985-1995)						
CDE of *Magnitude*†	-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) *Party magnitude* (3): Post-quota (ARG: 1993-2015; PBA: 1997-2015)						
CDE of *Magnitude*†	-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) *Women First (%)* (1995-2015)						
CDE of *Magnitude*†	2.15		1.49		13.83	
	[-2.94:7.25]		[0.27:2.72]		[3.81:23.84]	
num. obs	209		209		209	
(e) *Women First (%, weighted)* (1995-2015)						
CDE of *Magnitude*†	1.06		1.44		12.39	
	[-3.75:5.87]		[0.27:2.61]		[2.64:22.14]	
num. obs	209		209		209	
(f) *Women Second (%)* (1995-2015)						
CDE of *Magnitude*†	1.96		1.52		13.45	
	[-3.24:7.15]		[0.28:2.76]		[3.53:23.37]	
num. obs	209		209		209	
(g) *Women Second (%, weighted)* (1995-2015)						
CDE of *Magnitude*†	1.17		1.44		12.22	
	[-3.97:6.32]		[0.20:2.68]		[2.29:22.14]	
num. obs	209		209		209	
(h) *Women First Two (%)* (1995-2015)						
CDE of *Magnitude*†	2.18		1.52		13.63	
	[-3.07:7.42]		[0.27:2.78]		[3.54:23.72]	
num. obs	209		209		209	
(i) *Women First Two (%, weighted)* (1995-2015)						
CDE of *Magnitude*†	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.

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. Panels (a) to (c) look at the CDE of *Magnitude* net of the effect of *Party magnitude*. 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:[12] 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. 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.

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 revenues, the number of public employees, or the unemployment and infant mortality rates.[13] In line with

[12] In this case, we took the natural logarithm of the outcome (+ 1), of *Magnitude*, and of *Party magnitude*.

[13] Time-varying data of this kind is only available for Argentina.

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 exploited 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. 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* that are know to be short-term 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 between elections. Somewhat surprisingly, the effects are weaker for the province of Buenos Aires, where changes in district magnitude are much larger. Part of the reason, we suspect, is that district magnitudes tend to be larger across the board, and district magnitude is likely to have diminishing effects (Schwindt-Bayer 2005).

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 an 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. 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 women candidates in the first two positions of the party list has not increased much.[14] 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.

[14] Results not shown but available upon request from the authors.

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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 San Luis Santa Cruz	Yes	5	3	2
Chubut Formosa La Rioja Río Negro Tierra del Fuego*	Yes	5	2	3
Jujuy San Juan	No	6	3	3
Chaco	Yes	7	4	3
Corrientes† Misiones Salta Santiago del Estero‡	Yes	7	3	4
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.

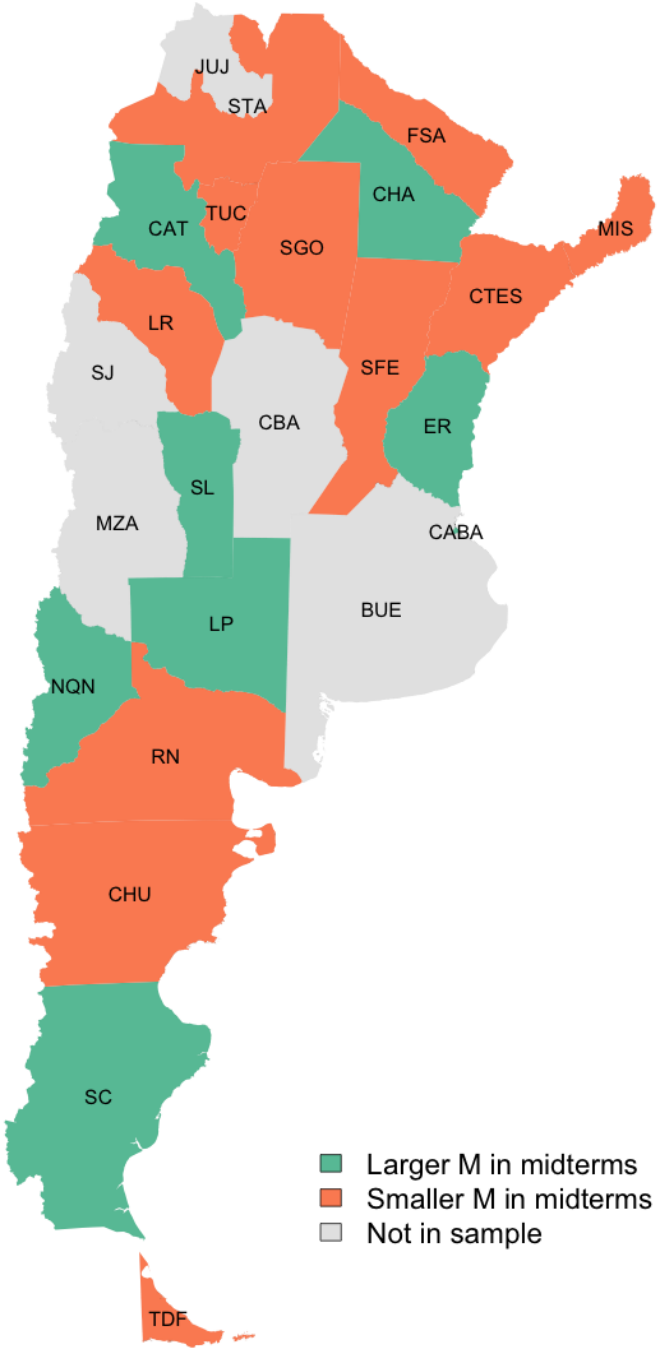


Figure A1: The electoral calendar for the Argentine Chamber of Deputies, 1985-2015. See Table A1 for further details.

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

district	in sample?	delegation size	magnitude (midterm)	magnitude (concurrent)
sección VIII	Yes	3 (upper) + 6 (lower)	6	3
sección VII	Yes	3 (upper) + 6 (lower)	3	6
sección II	Yes	5 (upper) + 11 (lower)	11	5
sección V	Yes	5 (upper) + 11 (lower)	5	11
sección VI	Yes	6 (upper) + 11 (lower)	11	6
sección IV	Yes	7 (upper) + 14 (lower)	7	14
sección I	Yes	8 (upper) + 15 (lower)	8	15
sección II	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.

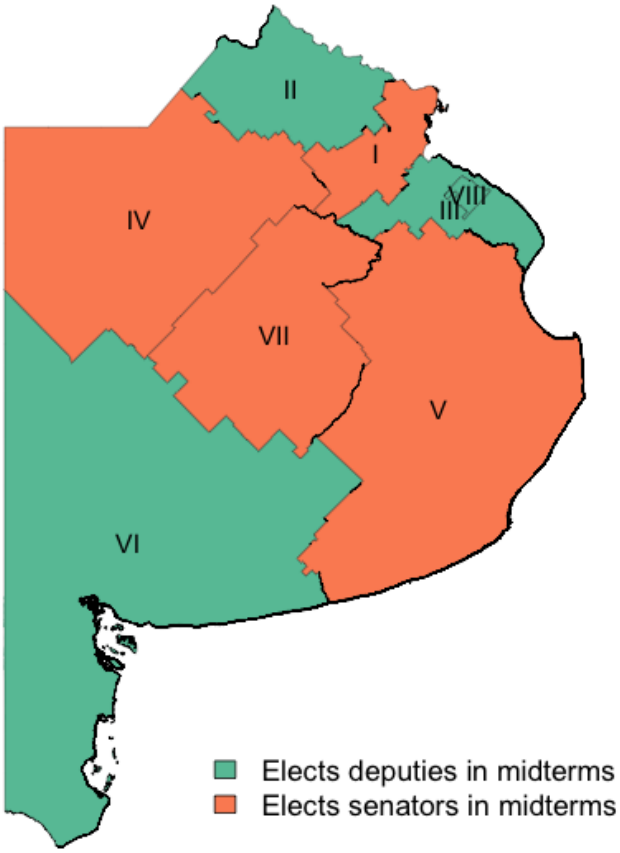


Figure A2: The electoral calendar for the legislature of the province of Buenos Aires, 1985-2015. See Table A2 for further details.

A2 Descriptive statistics

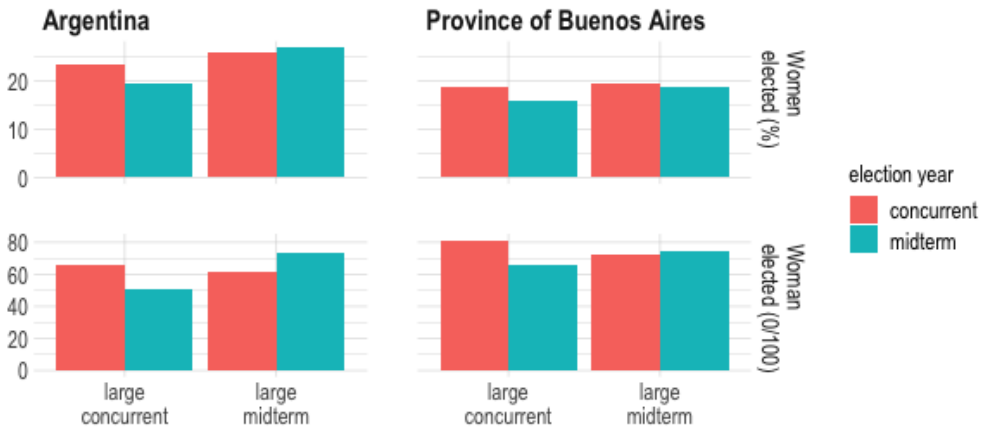
Table A3 presents the descriptive statistics for the main variables of interest. Figure A3 plots the average values of the outcome variables conditional on (a) whether a district's magnitude is larger in concurrent or midterm elections; and (b) whether an election falls on a concurrent or a midterm year. For both the Argentine Chamber of Deputies and the legislature of Buenos Aires, the % of women elected and the dummy indicating whether at least one woman was elected show a spike in election years featuring larger magnitudes, which is especially marked after the introduction of gender quotas.

Table A3: Descriptive statistics

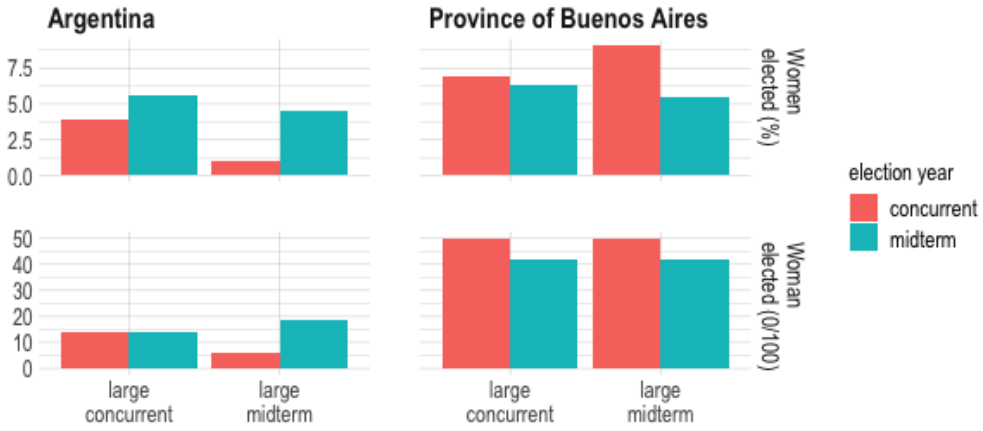
	Argentina					Province of Buenos Aires				
(a) Full sample (1985-2015)	*N*	mean	sd.*	min.	max.	*N*	mean	sd.*	min.	max.
Magnitude	302	3.9	2.6	2.0	13.0	128	8.6	4.3	3.0	18.0
Women elected (%)	302	23.9	20.8	0.0	100.0	128	18.3	13.6	0.0	50.0
Woman elected (0/100)	302	62.9	48.4	0.0	100.0	128	73.4	44.3	0.0	100.0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

(a) Full sample



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



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

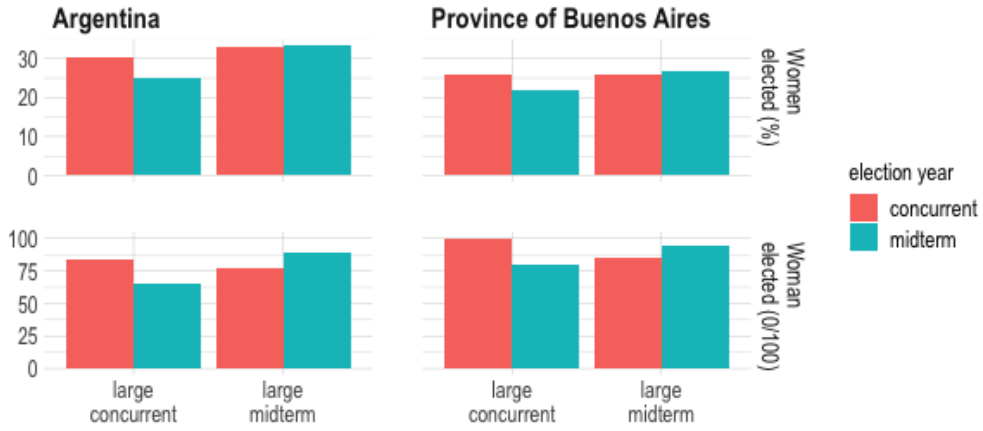


Figure A3: Average values of the outcome variables, conditional on (a) whether a district's magnitude is larger in concurrent or midterm elections; and (b) whether an election took place in a concurrent or a midterm year.

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[1] should not differ systematically from those that have higher magnitudes in concurrent years.[2] 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;[3] (b) the intervening variables used to estimate the controlled direct effects of *Magnitude*;[4] (c) the pseudo-outcomes reported in the placebo analysis (see Table A6), again measured in 1983;[5] (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;[6] (e) several demographic variables, such as population (density);[7] (f)

[1] 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).

[2] 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).

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

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

[5] 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.

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

[7] 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;[8] and (g) several measures of a district's electoral (over-)representation in 1983.[9]

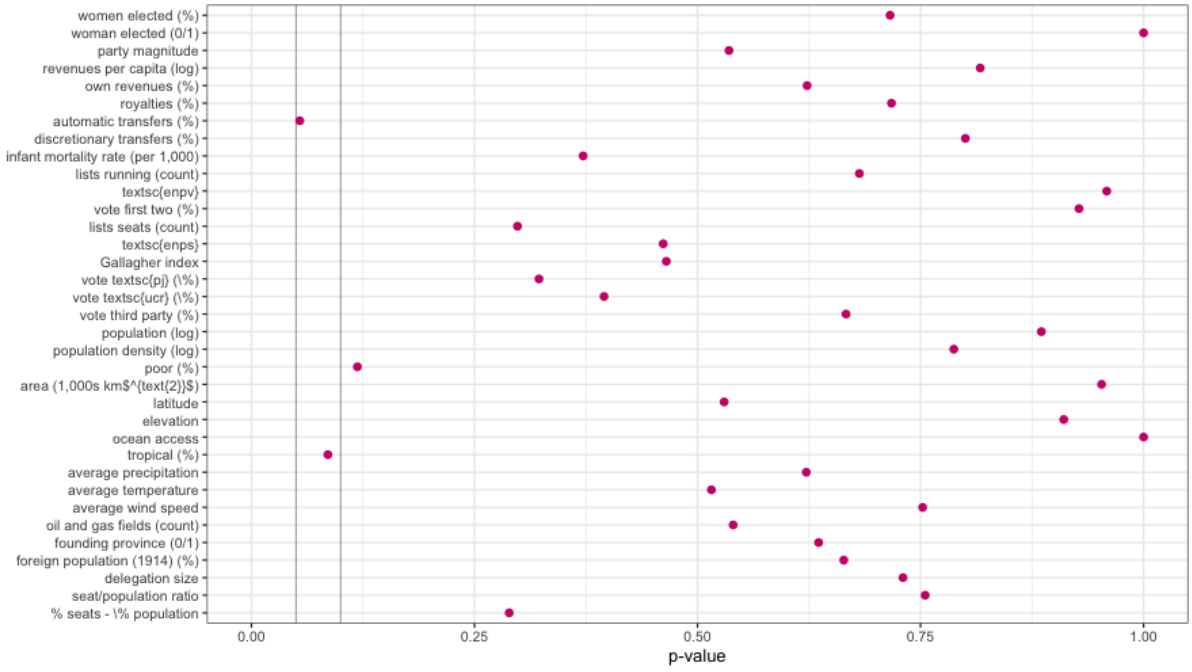
Tables A4 and A5 display the results for the 19 Argentine provinces included in our sample and the province of Buenos Aires, respectively. In each table we report the means for both groups of districts, as well as the difference between the two and the exact p-values for the sharp null hypothesis that having a larger magnitude in midterm years has no effect for any district, which are also displayed in Figure A4. 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[10] (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 A4 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.

[8] Sources: INDEC and Mitton (2016). We are thankful to Todd Mitton for kindly sharing this data.

[9] 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.

[10] 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* ($N = 8$)

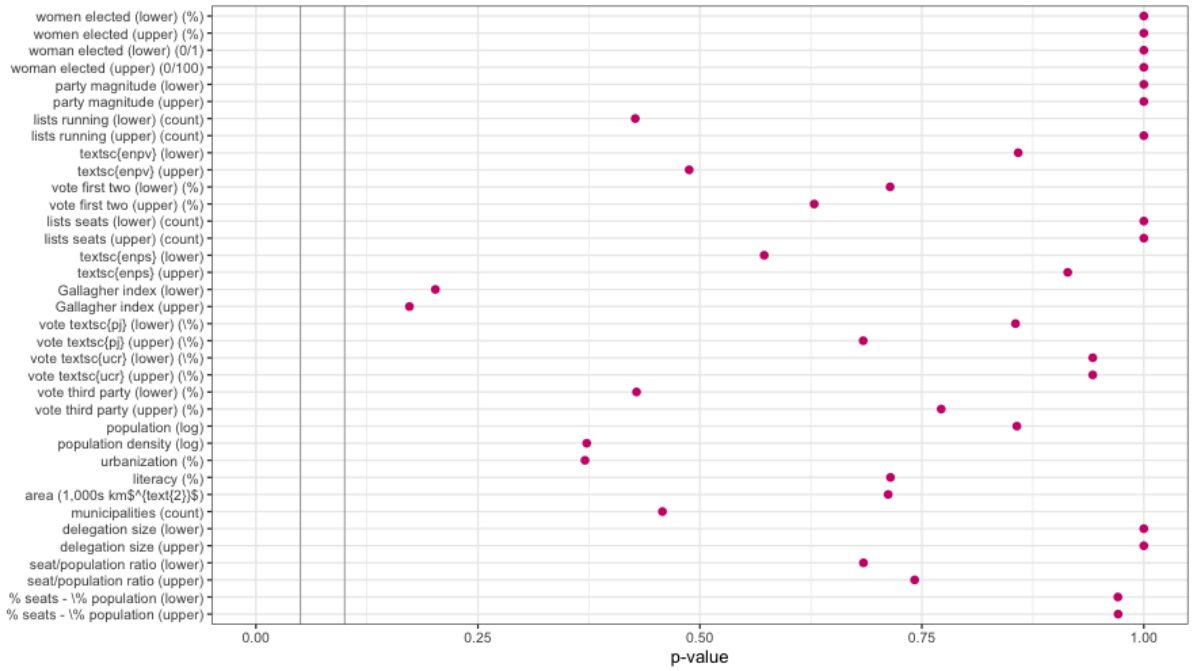


Figure A4: Checking covariate balance. The dots report the exact p-values for the sharp null hypothesis that having a higher magnitude in midterm years has no effect on any district. See Online Appendix A3 for details.

Table A4: Covariate balance (1): Argentina

(a) *Outcome variables (1983)*	large midterm mean	large concurrent mean	difference	*p*-value
women elected (%)	2.29	3.12	-0.83	0.72
woman elected (0/1)	0.25	0.18	0.07	1.00
(b) *Intervening variables (1983)*				
party magnitude	2.69	3.36	-0.68	0.54
(c) *Pseudo-outcomes (1983)*				
revenues per capita (log)	7.16	7.09	0.07	0.82
own revenues (%)	19.56	14.80	4.76	0.62
royalties (%)	12.78	9.68	3.10	0.72
automatic transfers (%)	28.50	33.99	-5.50	0.05
discretionary transfers (%)	38.83	41.00	-2.17	0.80
infant mortality rate (per 1,000)	35.50	39.52	-4.02	0.37
(d) *Electoral outcomes (1983)*				
lists running (count)	11.50	12.00	-0.50	0.68
ENPV	2.69	2.71	-0.02	0.96
vote first two (%)	84.49	84.99	-0.50	0.93
lists seats (count)	2.62	2.18	0.44	0.30
ENPS	2.22	2.07	0.14	0.46
Gallagher index	7.97	9.38	-1.41	0.47
vote PJ (%)	38.70	43.01	-4.31	0.32
vote UCR (%)	44.19	41.98	2.21	0.40
vote third party (%)	10.05	8.06	1.99	0.67
(e) *Demographics (1980)*				
population (log)	12.93	12.99	-0.07	0.89
population density (log)	2.10	1.69	0.41	0.79
poor (%)	31.00	39.81	-8.81	0.12
(f) *Geography and history*				
area (1,000s km²)	104.93	106.92	-1.99	0.95
latitude	35.11	32.52	2.58	0.53
elevation	6.20	6.17	0.03	0.91
ocean access	0.38	0.27	0.10	1.00
tropical (%)	20.11	52.64	-32.53	0.09
average precipitation	55.57	63.80	-8.22	0.62
average temperature	15.02	16.57	-1.55	0.52
average wind speed	3.53	3.39	0.14	0.75
oil and gas fields (count)	33.25	19.00	14.25	0.54
founding province (0/1)	0.38	0.55	-0.17	0.64
foreign population (1914) (%)	31.06	26.68	4.38	0.66
(g) *Political representation (1983)*				
delegation size	8.25	7.09	1.16	0.73
seat/population ratio	2.18	1.97	0.22	0.76
% seats - % population	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
women elected (lower) (%)	2.78	1.67	1.11	1.00
women elected (upper) (%)	2.78	3.12	-0.35	1.00
woman elected (lower) (0/1)	0.25	0.25	0.00	1.00
woman elected (upper) (0/100)	0.25	0.25	0.00	1.00
(b) *Intervening variables (1983)*				
party magnitude (lower)	5.50	5.75	-0.25	1.00
party magnitude (upper)	2.88	2.88	0.00	1.00
(c) *Electoral outcomes (1983)*				
lists running (lower) (count)	14.25	15.00	-0.75	0.43
lists running (upper) (count)	13.75	14.00	-0.25	1.00
ENPV (lower)	2.38	2.37	0.01	0.86
ENPV (upper)	2.29	2.34	-0.05	0.49
vote first two (lower) (%)	89.21	89.80	-0.58	0.71
vote first two (upper) (%)	91.04	90.38	0.66	0.63
lists seats (lower) (count)	2.25	2.00	0.25	1.00
lists seats (upper) (count)	2.00	2.00	0.00	1.00
ENPS (lower)	1.97	1.89	0.08	0.57
ENPS (upper)	1.88	1.89	-0.02	0.91
Gallagher index (lower)	6.18	7.44	-1.25	0.20
Gallagher index (upper)	6.55	7.77	-1.22	0.17
vote PJ (lower) (%)	36.38	36.07	0.30	0.85
vote PJ (upper) (%)	36.91	36.09	0.83	0.68
vote UCR (lower) (%)	52.84	53.72	-0.89	0.94
vote UCR (upper) (%)	54.13	54.29	-0.16	0.94
vote third party (lower) (%)	4.12	3.34	0.78	0.43
vote third party (upper) (%)	2.80	3.27	-0.47	0.77
population (log)	13.69	13.62	0.07	0.86
population density (log)	4.33	3.16	1.17	0.37
(d) *Demographics (1980)*				
urbanization (%)	89.57	81.86	7.71	0.37
literacy (%)	96.33	95.98	0.35	0.71
area (1,000s km²)	32.88	44.02	-11.14	0.71
municipalities (count)	13.25	18.00	-4.75	0.46
(e) *Geography and history*				
delegation size (lower)	11.50	11.50	0.00	1.00
delegation size (upper)	5.75	5.75	0.00	1.00
(f) *Political representation (1983)*				
seat/population ratio (lower)	1.64	1.94	-0.29	0.68
seat/population ratio (upper)	1.63	1.92	-0.28	0.74
% seats - % population (lower)	-0.13	0.13	-0.27	0.97
% seats - % population (upper)	-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

(a) Pooled models	*revenues per capita (log)*	*% own revenues*	*% royalties*	*% automatic transfers*	*% discretionary transfers*	*public employees (per 1,000)*	*unemployment rate (%)*	*infant mortality (per 1,000)*
Magnitude	0.01	0.45	0.23	-0.72	0.18	-0.69	-0.04	-0.42
	[-0.13:0.15]	[-4.46:5.35]	[-3.36:3.81]	[-6.17:4.74]	[-1.82:2.18]	[-6.47:5.08]	[-1.16:1.07]	[-2.46:1.62]
(b) FE models								
Magnitude	-0.00	-0.42	0.02	0.25	0.37	0.04	-0.01	-0.24
	[-0.03:0.02]	[-1.44:0.60]	[-1.27:1.31]	[-1.19:1.68]	[-0.70:1.44]	[-1.57:1.65]	[-0.53:0.52]	[-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.