The Effect of District Magnitude on Electoral Outcomes
Evidence from Two Natural Experiments in Argentina

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Abstract

How does district magnitude affect electoral outcomes? This paper addresses this question by exploiting a combination of two natural experiments in Argentina between 1985 and 2015. Argentine provinces elect half of their congressional delegation every two years, and thus districts with an odd number of representatives have different magnitudes in different election years. Furthermore, whether a province elects more representatives in midterm or concurrent years was decided by lottery in 1983. The results indicate that district magnitude (a) increases electoral support for small parties, (b) increases the (effective) number of parties getting seats, and (c) reduces electoral disproportionality. The last two results are driven by the mechanical rather than the psychological effect of electoral rules.

Keywords: electoral systems – district magnitude – mechanical and psychological effects – natural experiment – Argentina

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Do electoral rules affect electoral outcomes? If so, what are the mechanisms that drive this process? Electoral rules have long captured the attention of political scientists, as they dictate how votes are translated into seats and thus can determine how many parties (and which ones) gain legislative representation; keep party systems in place; or even bias the entire political system to the right or the left.[1] Furthermore, since electoral rules can be more easily manipulated than other features of the political system — like the party system or the structure of executive authority, to say nothing of more informal components such as political culture —, understanding how small change sin such rules can affect electoral outcomes constitutes a central research question within the discipline.

In practice, however, understanding when and why electoral rules do make a difference is complicated by two factors. Since the work of Duverger[2] it has been known that electoral rules may operate through two mechanisms. The *mechanical effect* refers to the fact that different electoral rules may translate the same vote distribution into different distributions of seats. This effect is "mechanical" in the sense that, once a vote distribution is given, the corresponding seat distribution is determined by a mathematical algorithm, independently of human volition. But of course, strategic players — such as candidates, voters and party elites — can anticipate these effects and adjust their behavior accordingly, thus modifying the underlying vote distribution. Duverger called this phenomenon the *psychological effect* of electoral rules.

Yet even though this distinction is well understood, disentangling the relative contribution of these mechanisms to electoral outcomes is complicated by the fact that they may interact with each other in multiple ways. For example, imagine a change in electoral rules that is expected to benefit small parties (mechanical effect): to the extent that this makes voters more likely to support small parties (psychological effect), the final vote distribution will

[1]Cox 1997; Kedar, Harsgor and Sheinerman 2016; Lijphart 1994; Monroe and Rose 2002; Rodden 2009; Taagepera and Shugart 1989.

[2]Duverger 1951/1967.

be more fragmented, which may end up benefitting large parties. In other words, a reform intended to boost the vote share of small parties may nevertheless leave large parties with as many seats as before (though with fewer votes). This may create the impression that the new rules have no effect at all, when actually the mechanical and psychological effect are working against each other. Alternatively, a change in rules may have no effect on the distribution of votes, either because players anticipate (correctly) that the mechanical effect is trivial, or because they are imperfectly informed (or imperfectly rational) and thus fail to adjust their behavior. It may also occur that players do adjust their behavior in response to a change in electoral rules, but this is not enough to change the distribution of seats above and beyond what the new rules would warrant. For example, in a context of three-party competition, increasing the number of seats from two to three will likely result in one more party winning representation; given this change, the psychological effect cannot only make a difference unless the vote share of the first-, second- or fourth-placed party increases substantially.

These examples do not pretend to be exhaustive; rather, their goal is to illustrate that understanding the effect of electoral rules requires looking at three different sets of outcomes. First, the fact that strategic players anticipate the mechanical effect means that electoral rules may affect how voters and elites *coordinate* their behavior before seats are distributed: how many parties enter the race, how many votes they receive, or whether voters tend to favor large parties over small ones. Second, electoral rules shape the distribution of seats: how many parties gain representation, how seats are distributed between them, and how (dis)proportional is their allocation. Finally, the effect of electoral rules on the distribution of seats may be driven by the mechanical effect, the psychological effect, or some combination of the two. The point is that finding that electoral rules do not matter for some outcomes is consistent with finding strong effects for others; for example, a change in rules may have no effect on the final distribution of seats, but only because the mechanical and psychological effect are canceling each other out. The bottom line is that electoral rules can only be deemed inconsequential when they fail to have an effect on any of these outcomes — if their

are, and are perceived as, irrelevant, and thus fail to alter players' behavior and the translation of votes into seats.

The second problem for assessing the effect of electoral rules is that despite abundant observational evidence of an association between electoral rules and political outcomes,[3] showing that this relationship is causal has proved elusive. One possibility is that the relationship may reflect reverse causality, i.e. political parties may choose those rules that are more likely to keep them in office. Alternatively, changes in electoral rules and electoral outcomes may result from a common cause, such as a shock in voters' preferences. Districts that elect more representatives tend to be more urbanized and more socially diverse, which may affect voters' willingness to support certain kinds of parties.[4] Comparing elections for different offices within the same polity — i.e., lower- and upper-house elections that follow the same district boundaries —[5] is problematic because behavior in both tiers may be correlated, for example if citizens cast a straight-party vote, or if small parties systematically nominate their best candidates in the more competitive tier.[6]

To address these issues, in this paper I exploit two natural experiments determining the composition of the Argentine Chamber of Deputies. First, the Argentine lower house is elected by closed-list PR in 24 multi-member districts that are coterminous with the country's provinces;[7] however, the Chamber is renewed by halves every two years, and thus the nineteen provinces that have an odd delegation size elect a different number of representatives in concurrent and midterm election years.[8] Second, the choice of which provinces

[3] See Benoit 2001; Clark and Golder 2006; Crisp, Olivella and Potter 2012; Ferrara and Herron 2005; Herron and Nishikawa 2001; Lijphart 1994; Singer and Stephenson 2009; Singer 2013; Taagepera and Shugart 1989.

[4] Gerring et al. 2015; Kedar, Harsgor and Sheinerman 2016; Monroe and Rose 2002.

[5] Blais et al. 2011; Cox 1997, ch. 2; Lago and Martínez 2007; Lago 2012.

[6] Fiva and Folke 2016; Lago and Montero 2009.

[7] For this reason, throughout this paper I use the expressions "district(s)" and "province(s)" interchangeably.

[8] The number of deputies per province has remained almost constant since 1983; the only exception is Tierra del Fuego, whose delegation size increased from two to five in 1991, after it acquired provincial status.

would elect a larger number of representatives in concurrent or midterm years was done by lottery in 1983, when half of the deputies elected in that year's election were randomly chosen to receive a shortened two-year mandate instead of a four-year one.[9] In other words, in Argentina the number of seats elected in a given province varies periodically, while several potentially confounding factors — such as history, social diversity, or the structure of the party system — remain constant. Furthermore, whether a province elects more or less representatives in a given year is not systematically associated with midterm or concurrent elections. Taken together, these considerations provide an ideal design for identifying the effect of district magnitude on (a) electoral coordination; (b) the distribution of seats; and (c) how the mechanical and psychological effect contribute to the latter. District magnitude — the number of seats elected in a given district in a given election — is one of the most fundamental elements of an electoral system: it influences how many parties enter the race and how voters choose between them, as well as determining how proportional is the translation of votes into seats.[10] Some authors claim that this effect is conditional on the underlying number of social cleavages[11] or the extent to which legislators can seek a personal vote,[12] but none of them denies that district magnitude is one of the most relevant components of an electoral system.

In line with these expectations, the results show that district magnitude increases the effective number of parties receiving votes and decreases the vote share of the two largest parties, though neither effect is entirely reliable. Magnitude also has a strong positive effect on the (effective) number of lists gaining representation, and a large negative one on electoral disproportionality. In substantive terms, these results imply that simplifying Ar-

[9] Dal Bó and Rossi 2011.

[10] Cox 1997; Rae 1967; Riker 1982; Shugart 1985.

[11] Amorim Neto and Cox 1997; Clark and Golder 2006; Cox 1997; Ordeshook and Shvetsova 1994; Potter forthcoming.

[12] Carey and Shugart 1995.

gentina's electoral calendar so that all provinces elected their entire delegation simultaneously would decrease electoral support for the two largest parties by 6.3 percentage points, increase the number of lists gaining representation by 17, or cut disproportionality in half. These effects are somewhat stronger for small provinces as well as those that had a larger regional party in 1983, though the difference with the rest of the sample is not very large. Further inspection reveals that the effect of magnitude on the distribution of seats is almost entirely driven by the mechanical effect: although higher magnitudes do increase the vote share of small parties, the fact that many Argentine provinces elect few representatives means that this effect cannot compete with having an additional seat to distribute.

This paper contributes to a rapidly growing literature on the causal effect of electoral rules.[13] In this regard, it is worth noting that while none of its three major elements — the explanatory variable, the main outcome variables, or the identification strategy — is unique, their combination is novel. To begin with, I focus on the effect of electoral rules on the distribution of both votes[14] and seats,[15] rather than either of them separately. Unlike Singer, I focus on variation in district magnitude driven by the electoral calendar rather than exogenous reapportionment changes.[16] In this regard, my identification strategy is very similar to that of Crisp and Demirkaya,[17] though these authors examine the combined effect of magnitude and electoral formula simultaneously. To disentangle the contribution of the mechanical and psychological effects to the distribution of seats, I adopt the framework proposed by Fiva and Folke,[18] but looking at the role of district magnitude rather than the electoral for-

[13] Bordignon, Nannicini and Tabellini 2016; Crisp, Potter and Lee 2012; Crisp and Demirkaya 2016; Fiva and Folke 2016; Fujiwara 2011; Shugart 1985; Singer 2015.

[14] Bordignon, Nannicini and Tabellini 2016; Crisp, Potter and Lee 2012; Crisp and Demirkaya 2016; Fujiwara 2011; Singer 2015.

[15] Fiva and Folke 2016; Singer 2015.

[16] Singer 2015.

[17] Crisp and Demirkaya 2016.

[18] Fiva and Folke 2016.

mula. Finally, the fact that not all Argentine provinces have higher magnitudes in the same election years allows me to control for national-level waves — unlike the cases of Norway or Brazil, where national waves are contemporaneous with changes in electoral rules.[19]

Research design and data

Expectations. The literature has made four main claims about the effect of district magnitude on electoral outcomes. First, as long as a PR formula is employed, higher magnitudes should increase the number of parties winning seats.[20] Second, this should increase both the number of parties running and the vote share of small parties. In small-magnitude districts only large parties can expect to receive a seat, which induces voters to withdraw support from small parties and discourages them from entering the race in the first place. As district magnitude increases, the opposite effect holds: voters become more likely to cast a ballot for small parties, thus increasing incentives for such parties to field candidates.[21] Third, these effects should be stronger in more heterogeneous districts. Intuitively, the previous mechanism should only operate when voters *would like* to support small parties but are wary of "wasting" their votes on hopeless candidates; if voters have a strong preference for large parties to begin with, increasing district magnitude should not increase electoral support for small parties.[22] Finally, higher magnitudes should induce a more proportional translation of votes into seats: with more seats to distribute, it is more likely to find an allocation that will roughly reflect the vote shares obtained by different parties.[23] However, this relationship may apply in very small districts. To see why, note that if two parties obtain roughly 50

[19] Crisp and Demirkaya 2016; Fiva and Folke 2016.

[20] Lijphart 1990, 1994; Rae 1967; Shugart 1985; Taagepera and Shugart 1989.

[21] Cox 1997.

[22] Amorim Neto and Cox 1997; Cox 1997; Clark and Golder 2006; Golder 2006; Duverger 1951/1967, 1952; Ordeshook and Shvetsova 1994; Potter forthcoming.

[23] Lijphart 1990, 1994; Rae 1967; Shugart 1985; Taagepera and Shugart 1989.

percent of the vote, a magnitude of two will result in a more proportional allocation of seats than a magnitude of three. Since small magnitudes are common in Argentina (see Table 1), this is a relevant consideration.

The Argentine electoral calendar. I examine these claims with district-level data for elections to the Argentine Chamber of Deputies between 1985 and 2015. The electoral rules governing the composition of this body provide two natural experiments with which to identify the effect of district magnitude on electoral outcomes. First, the use of a scattered electoral calendar means that district magnitude varies regularly within provinces (see Table 1). Specifically, the chamber is elected by closed-list PR in 24 multi-member districts that are coterminous with the country's provinces.[24] Within each district, seats are distributed according to the d'Hondt formula, with a legal threshold of 3 percent of registered voters.[25] Deputies last four years in office, but according to the 1853 constitution — which the outgoing military government reinstated in 1983 — the Chamber is renewed by halves every two years, with each province electing half of its representatives in each electoral turn. Thus, the 19 provinces with an odd number of representatives have higher district magnitudes in some years than in others (see Table 1).

The number of seats per province has remained almost constant since 1983. In that year, the outgoing military government established that each province would receive one seat per 161,000 population (or fraction larger than 80,500), but added three additional provisions. First, each province would receive three additional seats regardless of population. Second, no province could have less than five deputies. And thirdly, no province could have fewer

[24] Strictly speaking, Argentina is divided into 23 provinces and one autonomous city, but the later can be considered as an additional province for seat allocation purposes.

[25] This makes little difference in practice because mandatory voting ensures that turnout is relatively high — values lower than 60 percent are uncommon — and low magnitudes mean that parties that do not reach the threshold would not have obtained representation anyway. The threshold is only relevant in the province of Buenos Aires (magnitude = 35), which is not included in the analysis because it has an even number of representatives.

Table 1: *Delegation size and district magnitude 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 median		10.7 6.5	5.3 3.0	5.4 3.0

Note: Midterm years are 1985, 1989, 1993, 1997, 2001, 2005, 2009 and 2013. Concurrent years are 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.

deputies than it had at the moment of the military coup of 24 March 1976. The initial allocation of seats was based on the 1980 census; Congress was supposed to reapportion the number of seats per province in subsequent censuses (1991, 2001 and 2010), but it has not done so. The only district to gain representation since 1983 was Tierra del Fuego, which elected two deputies until it became a province in 1990, and five afterwards. Thus, the Chamber had 254 members between 1983 and 1991, and 257 since 10 December 1991.

The second natural experiment is that whether a province has a higher magnitude in years with concurrent executive elections was decided randomly in 1983. Since executive officials — presidents, governors and mayors — are elected every four years,[26] some provinces have a higher magnitude in years with executive elections ("concurrent years"), while others have a higher magnitude during midterms ("midterm years").[27] To the extent that these provisions are systematically different — for example, if higher magnitudes coincide with concurrent elections in large provinces —, disentangling the effect of magnitude from that of concurrency would be impossible. This is a serious consideration, both due to coattail effects[28] and because nomination and entry decisions are unlikely to be independent across offices, which may affect the pool of candidates. For example, national legislators often run for governor or mayor at the end of their mandate,[29] and parties that agree to support a com-

[26] The president was originally elected for a six-year term, but the 1994 constitutional reform reduced it to four years. Thus, since 1995 all presidential elections took place in concurrent years.

[27] Concurrent years are 1983, 1987, 1991, 1995, 1999, 2003, 2007, 2011 and 2015. Midterm years are 1985, 1989, 1993, 1997, 2001, 2005, 2009 and 2013. In Corrientes and Santiago del Estero the electoral calendar was displaced by two years due to political turmoil. Thus, beginning in 1993 and 2005 respectively, concurrent years in these provinces correspond to midterm years in others, and vice versa. Note that I speak of concurrent *years* rather than concurrent *elections* because even if executive and legislative elections take place in the same year, they need not take place in the same day: in some provinces the constitution bars concurrent elections explicitly, while in others the governor can set the date of provincial elections on the basis of short-term political considerations.

[28] Jones 1997.

[29] Micozzi 2013, 2014*a*,*b*.

mon candidate for a given office may also support a common list of candidates for a different office.

Fortunately, whether a province ended up electing more representatives in midterm or concurrent years was decided by lot in 1983. That year, every province elected its entire congressional delegation, but subsequently half of each district's representatives received a two-year mandate instead of a four-year one. To decide which legislators would receive a full term, each party-province-delegation had to divide its members into two groups of equal size, group #1 and group #2.[30] Party-province-delegations that had an odd number of representatives had to coordinate with another provincial delegation from the same party that also had an odd number of representatives. A random draw then determined that legislators belonging to group #1 would receive a four-year mandate, implicitly deciding which provinces would elect a larger number of representatives in concurrent and midterm years.[31]

Specification. The structure of Argentina's electoral calendar suggests adopting a difference-in-differences approach in which the treatment of interest — having a higher district magnitude — is switched on and off every two years within each province. Identification using a difference-in-differences design depends on the parallels-paths assumption, i.e. the treatment and control groups would have followed parallel paths in the absence of treatment.[32] The fact that district magnitude varies periodically within provinces with an odd number of representatives is reassuring in this regard, as it ensures that the results cannot be attributed to the fact that a change in magnitude in a province happened to coincide with some secular demographic change or a major realignment of that province's party system. In particular, the fact that the Argentine political system changed substantially after 2003[33] is not problematic because the treatment of interest continued to vary in a regular fashion afterwards.

[30] The two representatives from Tierra del Fuego were placed in group #2.

[31] Dal Bó and Rossi 2011:1243-4.

[32] Angrist and Pischke 2009, ch. 5.

[33] Calvo and Escolar 2005; Torre 2005.

Nonetheless, the parallels paths assumption would be violated if higher magnitudes coincided with concurrent (or midterm) years in all provinces, because in that case having a higher magnitude would be perfectly collinear with (non-)concurrency. It is here that the second natural experiment kicks in: since higher magnitudes coincide with concurrent years in some provinces but not in others, and since a province's electoral calendar was randomly determined, the effect of concurrency will cancel out in the aggregate. To put it differently, while simply comparing a province with itself at different moments in time would violate the parallel paths assumption — because, within provinces, higher magnitudes are always (or never) collinear with (non-)concurrency —, provinces that have a higher magnitude in midterm years are, as a group, comparable to those that have a higher magnitude in concurrent years, and thus the parallel paths assumption is reasonable.

These considerations suggest fitting models of the form

$$Y_{it} = \gamma \cdot magnitude_{it} + \mu_i + \delta_t + \varepsilon_{it}, \quad (1)$$

where Y_{it} is the outcome, $magnitude_{it}$ is the district magnitude of province i in year t, μ_i and δ_t are province and year fixed effects, and ε_{it} is the error term. The sample is restricted to provinces with an odd number of representatives. Since the model includes province fixed effects and *magnitude* only varies by increments of one within provinces, this is equivalent to including a dummy indicating whether a province had a higher magnitude in a given year.

I also report two additional sets of results. Since a unit change in *magnitude* should be more relevant in small provinces, I report separate results for the subsample of provinces that have a delegation size of 5 (see Table 1). To examine whether the effect of *magnitude* is heterogeneous, in some specifications I interact it with *vote third party*, the average percentage of the vote for president, national deputies, governor and provincial deputies obtained by the largest party other than the PJ or the UCR in 1983. This variable captures the capacity of regional elites to sustain a viable provincial party, no small feat in a heavily nationalized

election like that of 1983, when the UCR and the PJ captured 92 percent of the presidential vote, 94 percent of national legislative seats, most provincial legislative seats, and 19 of 22 governorships.[34] Arguably, *vote third party* is different from the number of social cleavages in a province as commonly measured by the literature. In practice, however, this distinction is more apparent than real. On the one hand, the opposition between center and periphery, which often leads to the creation of regional parties — third parties in Argentina have rarely crossed provincial boundaries —, has long been recognized as a distinctive social cleavage.[35] Moreover, the theoretical argument about the heterogeneous effects of district magnitude is not about the underlying number of social cleavages *per se*, but rather about the effect of electoral rules when voters demand, and elites supply, multiple electoral alternatives. As long as multiple parties can claim substantial electoral support, it does not matter whether those parties represent "real" social cleavages or rather elites' capacity to develop and sustain viable party organizations; rather, the point is that voters faced at least three viable choices in the ballot, leading to more serious coordination problems than in other districts.

Data. I employ six outcome variables. To measure electoral coordination, I look at *# lists running*, a count of the number of lists participating in the election; a weighted average of the number of lists contesting, the effective number of parties in votes or *ENPV*;[36] and *vote first two*, the combined vote percentage of the two most voted lists. Higher values of the first two variables indicate a more fragmented playing field, while *vote first two* measures the extent to which voters tend to favor large parties. Thus, the effect of *magnitude* should be positive in the first two cases and negative in the third. To examine the final distribution of seats, I look at how many parties received at least one seat, *# list seats*; the effective number of parties in seats, *ENPS*; and the *Gallagher index*, a measure of the disproportionality in the translation

[34] Tierra del Fuego did not elect a governor until it became a province in 1990. The City of Buenos Aires elected its first executive in 1996, after it had become an autonomous district.

[35] Brancati 2007; Lipset and Rokkan 1967.

[36] Formally, $ENPV = \frac{1}{\sum_{i=1}^{N} v_i^2}$, where v_i indicates party i's vote share (Laakso and Taagepera 1979).

Table 2: *Descriptive statistics*

	Full sample (provinces: 19; n = 302)				**Small provinces** (provinces: 10; n = 158)			
(a) Explanatory variables	mean	sd.*	min.	max.	mean	sd.*	min.	max.
magnitude	3.88	0.52	2.00	13.00	2.50	0.52	2.00	3.00
vote third party	10.96	11.04	1.76	42.81	12.68	11.54	1.97	42.81
(b) Dependent variables (1): Electoral coordination								
# lists running	8.36	2.96	2.00	33.00	7.03	2.38	2.00	14.00
ENPV	3.04	0.77	1.49	10.06	2.89	0.66	1.49	9.96
vote first two	77.89	9.99	29.28	100.00	80.01	8.89	31.53	100.00
(c) Dependent variables (2): Seat distribution								
# lists seats	2.21	0.57	1.00	7.00	1.88	0.49	1.00	3.00
ENPS	2.02	0.52	1.00	6.00	1.81	0.48	1.00	3.00
Gallagher index	15.91	7.48	2.31	52.18	18.80	9.40	2.31	52.18

Note: (*) Within-province standard deviation (except for *vote third party*, which does not vary over time).

of votes into seats.[37] Again, the expectation is that *magnitude* should have a positive effect on the first two variables and a negative one on the third. Data for constructing these variables comes from Andy Tow's Electoral Atlas, a website that provides district-level information on electoral returns in Argentina.[38] Table 2 presents the descriptive statistics, distinguishing between the main sample and the subset of ten provinces with a delegation size of 5.

To determine how the mechanical and psychological effects contribute to the final distribution of seats, I follow Fiva and Folke and employ a district's actual vote distribution in year t as a counterfactual for that district's vote distribution at $t + 1$, and vice versa.[39] Then,

[37] Formally, $I_{\text{Gall}} = \sqrt{\frac{1}{2}\sum_{i=1}^{N}(s_i - v_i)^2}$, where s_i and v_i are the seat and vote shares of party i, respectively. For ease of interpretation, I multiply the index by 100: a value of 0 indicates perfect proportionality, while 100 means that one party received all seats with no votes, while another got all votes and no seats.

[38] `http://andytow.com/atlas/totalpais/`. The Atlas aggregates information from both Argentina's Interior Ministry and provincial electoral authorities.

[39] Fiva and Folke 2016.

(a) Higher magnitude in concurrent years[†]

	Outcome	Magnitude (midterm years*)		Magnitude (concurrent years**)	
Vote distr. (midterm years*)	*# lists seats*	A	2.07	B	2.32
	ENPS		1.95		2.09
	Gall. index		17.71		13.54
		mechanical		psychological	
		total effect			
Vote distr. (concurrent years**)	*# lists seats*	C	2.00	D	2.28
	ENPS		1.87		2.07
	Gall. index		19.38		14.73

(b) Higher magnitude in midterm years[‡]

	Outcome	Magnitude (midterm years*)		Magnitude (concurrent years**)	
Vote distr. (midterm years*)	*# lists seats*	A	2.34	B	2.16
	ENPS		2.10		2.03
	Gall. index		14.65		16.25
		mechanical		psychological	
		total effect			
Vote distr. (concurrent years**)	*# lists seats*	C	2.39	D	2.16
	ENPS		2.07		1.98
	Gall. index		14.36		16.44

Figure 1: Disaggregating the contribution of the mechanical and psychological effects to the final distribution of seats. A and D report the actual values of the outcome variables, while B and C indicate the values that would have resulted from counterfactual vote distributions. (*) 1985, 1989, 1993, 1997, 2001, 2005, 2009 and 2013. (**) 1987, 1991, 1995, 1999, 2003, 2007, 2011 and 2015. (†) Chubut, Corrientes, Formosa, La Rioja, Misiones, Río Negro, Salta, Santa Fe, Santiago del Estero, Tierra del Fuego (since 1991) and Tucumán. (‡) Catamarca, Chaco, Ciudad de Buenos Aires, Entre Ríos, La Pampa, Neuquén, San Luis and Santa Cruz.

I calculate both the actual and counterfactual distribution of seats at t and $t + 1$; since there are multiple changes in district magnitude, I repeat the process for all subsequent pairs of years (i.e., $t + 2$ and $t + 3$; $t + 4$ and $t + 5$; and so on).

More specifically, consider Figure 1, which closely mirrors Fiva and Folke's Figure 3. In each panel, the top row indicates the mean values of the outcome variables based on the actual vote distributions from midterm years, while the bottom row does the same for concurrent years. In turn, the columns indicate whether district magnitude corresponded to that of midterm or concurrent years. Thus, each panel is divided into four sectors: A and D indicate the actual values observed in midterm and concurrent years, respectively, while B reports the values that would have resulted from employing the vote distribution from midterm years to calculate the seat distribution in concurrent years, and the opposite is true for C. Figure 1a reports the values corresponding to those provinces that have a higher magnitude in concurrent years, while Figure 1b focuses on provinces where district magnitude is higher in midterm years (see Table 1).

The total effect of the electoral rules on the distribution of seats is the difference between A and D, i.e. that between the actual values observed in midterm and concurrent years. As noted by Fiva and Folke, this effect can be then decomposed into several subcomponents. The mechanical effect indicates what would happen if the vote distribution remained constant, but district magnitude changed; i.e., it is the difference between actual outcome A and counterfactual outcome B.[40] The psychological effect is estimated as the change in outcomes that result from keeping the electoral rules constant, but updating the vote distribution — i.e., the move from B to D. Note that this should not be interpreted as the effect of the electoral rules on the distribution of votes — what I call electoral coordination —, but rather as *the extent to which a change in the distribution of seats can be attributed to a change in the distribution of votes*. The distinction is relevant because even a substantial change in the distribution of votes may not bring about a change in the distribution of seats. To see why, suppose that district magnitude increases from 2 to 3 in a scenario of three-party competition. If the distribution of votes does not change, this alone will guarantee the third-placed list a seat. And since three lists are receiving one seat each, the seat distribution can only change if the most voted list grabs an additional seat from the third-placed one, which requires either (a) a large increase in the vote share of the first- or second-placed lists; or (b) a massive defection from the third-placed list in favor of lower-placed ones. The bottom line is, if the three most voted lists are close to each other, even a large change in the vote distribution will not alter the distribution of seats.

Finally, the psychological effect can be divided into two subcomponents. On the one hand, higher magnitudes may increase electoral support for small parties so much that these parties would have won representation even if district magnitude had remained constant. This effect is estimated as the difference between A and C. For the same reasons discussed

[40] To estimate this effect, I employ the specification presented in equation 1, but defining Y_{it} as the actual values of the outcome variable in midterm years, and the counterfactual values in concurrent years — in both cases on the basis of the actual vote distribution from midterm years.

above, however, this is unlikely to be an issue when magnitudes are very low. On the other, there is what Taagepera and Shugart called the "law of conservation of disproportionality:" to the extent that the psychological effect increases support for small parties, the mechanical effect will be stronger than what would otherwise be the case.[41] In terms of Figure 1, this is the difference between moving from C to D — i.e., keeping the vote distribution from concurrent elections constant, while changing the number of seats to distribute — minus the mechanical effect; formally, $[C \rightarrow D] - [A \rightarrow B]$.[42] Since this effect cannot be estimated directly, I estimated each of its two components separately, and calculated the standard errors by bootstrapping.[43]

Results

Balance check. For the identification strategy to be valid, provinces that received a higher magnitude in midterm or concurrent years should not be systematically different in terms of their pre-treatment characteristics. Table 1 already showed that the electoral calendar does vary between provinces that elect a similar number of representatives: districts with a delegation size of 5 or 9 are evenly divided; the two largest provinces have a higher magnitude in opposite years (concurrent in Santa Fe, midterms in the City of Buenos Aires); and among provinces with a delegation size of 7, one has a higher magnitude in midterm years and the other four in concurrent years. More systematically, Figure 2 shows that whether a province was assigned to have a higher magnitude in midterm or concurrent years is not systematically associated with other provincial characteristics. Specifically, the figure reports the exact p-values for the sharp null hypothesis that receiving a higher magnitude in midterm

[41] Taagepera and Shugart 1989:120-5.

[42] See Fiva and Folke 2016:271-3 for an extended discussion.

[43] Specifically, I sampled with replacement from the set of provinces, estimated the $C \rightarrow D$ and $A \rightarrow B$ separately, and recorded the difference between the two. I repeated this process 999 times, using the 2.5th and 97.5th quantiles to construct the 95% confidence intervals.

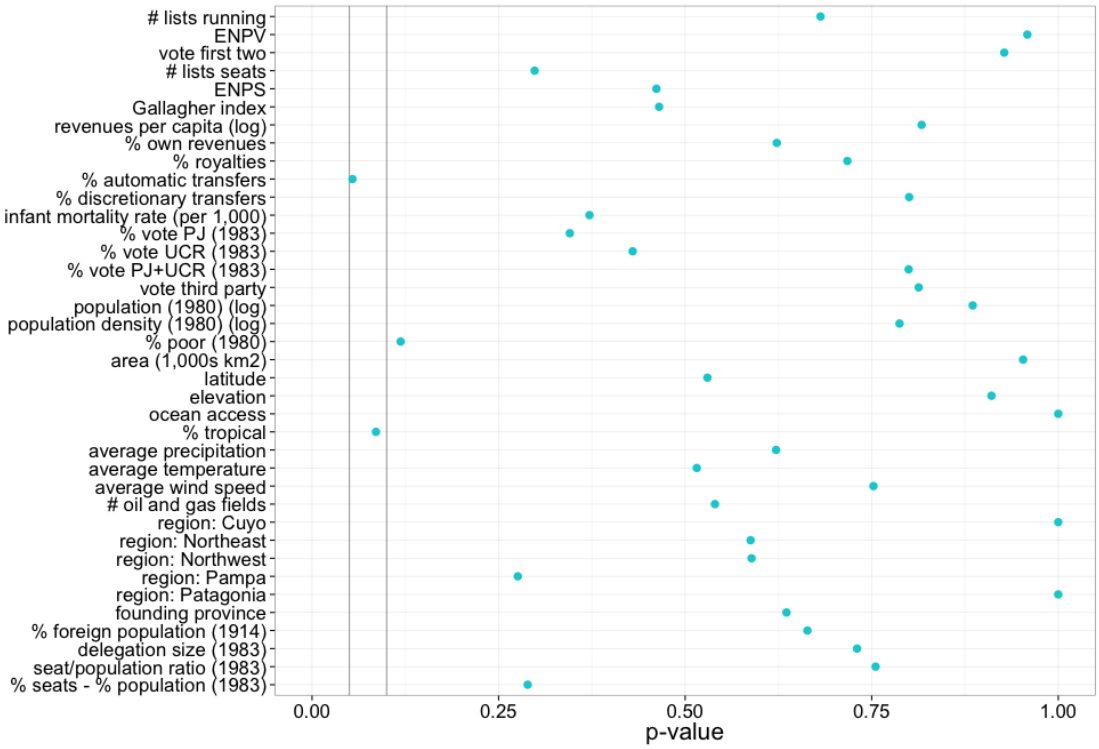

Figure 2: Checking covariate balance. The dots report the exact p-values for the sharp null hypothesis that having a higher magnitude in midterm years has no effect on any province. See the online appendix for further details.

years had no effect on the distribution of 38 pre-treatment covariates for any province.[44] Consistent with the claim that assignment to either group was randomly determined, only two differences are statistically significant at the 0.10 level: the percentage of a province's land area covered by (sub)tropical biomes and the percentage of 1983 provincial revenues that came from automatic transfers from the national government. This is unlikely to be an artifact of the small sample size: most p-values are quite large, and Table A1 in the online appendix shows that the substantive difference in means between both groups is quite small.

Electoral coordination. To facilitate interpretation I present the results graphically, relegating all tables to the online appendix. Figure 3 reports the point estimates and 95% confidence intervals of the marginal effect of *magnitude* on different measures of electoral coordination.

[44] All 38 covariates were measured before 1985. See the online appendix for further details.

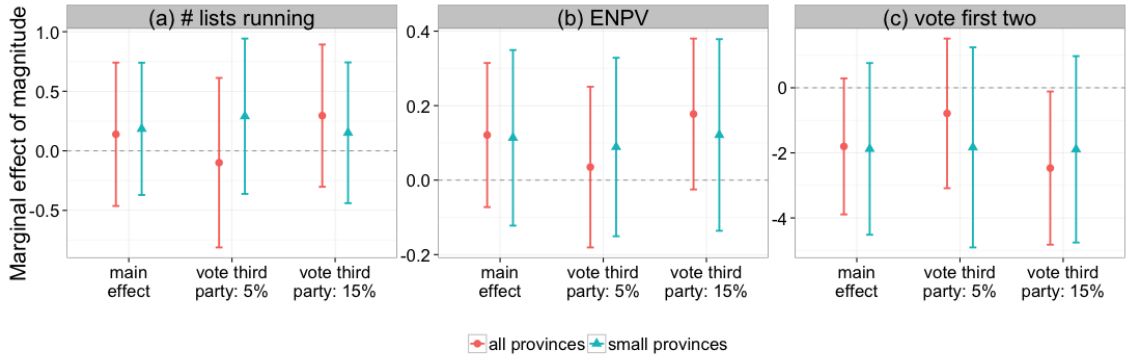

Figure 3: Point estimates and 95% confidence intervals for the effect of district magnitude on electoral coordination in Argentina, 1985-2015. All values are based on the results reported in Table A2 in the online appendix.

The confidence intervals are calculated using robust standard errors clustered by province, and adjusted on the basis of t-scores from a Student distribution with 18 (or 9) degrees of freedom rather than the usual z-scores. I present the results for both the unconditional effect of *magnitude*, and the conditional effect when a party other than the PJ or the UCR obtained 5 or 15 percent of the vote in 1983, respectively. I also report separate estimates for the full sample and the subset of provinces with a delegation size of 5.

Figure 3a shows that district magnitude makes little effect on the number of lists competing in a race: the estimated effect is positive but substantively small, and the confidence intervals are very wide. This likely reflects the combination of two forces. Since establishing a party that has a realistic chance of winning seats involves a long-term investment, strategic politicians should not be overtly responsive to changes in magnitude that are known to be short-term. Moreover, the fact that hopeless lists are pretty common in Argentina suggests that for many parties the decision to run is not motivated by the prospect of winning a seat.[45]

The next two panels of Figure 3 show that higher magnitudes increase the effective number of parties in votes and reduces the vote share of the two most voted parties, though neither effect is entirely reliable. Specifically, a unit increase in *magnitude* increases the effective

[45] Blais et al. (2011) make a similar point about Japan.

number of parties in votes by 0.12, a value comparable to that reported by other authors.[46] To put this number in perspective, consider what would happen if Argentina simplified its electoral calendar, holding legislative elections every four years instead of two. This would increase median district magnitude from 3 to 6.5 (see Table 1), which would translate into 0.42 more effective parties — a 14 percent increase over the average number of parties in the sample, and more than half the within-province standard deviation for this variable (see Table 2). The effect is even stronger when *vote third party* ≥ 15%, though neither of these estimates is statistically significant at conventional levels, and the second result is only reliable at the 0.10 level. There is little difference between the full sample and small provinces.

Figure 3c shows that a unit increase in *magnitude* decreases the percentage of the vote obtained by the two largest parties by 1.8 percentage points. In concrete terms, this means that simplifying Argentina's electoral calendar would increase electoral support for small parties by 6.3 percentage points, a substantial effect considering that *vote third party* averaged 22 percent during the period under study (see Table 2). The effect is only statistically significant at the 0.10 level, though the estimate is larger (and reliable at conventional levels) when a third party obtained 15 percent of the vote or more in 1983. Again, there is little variation by district size.

Distribution of seats. Figure 4a shows that a unit increase in *magnitude* translates into 0.2-0.3 parties gaining representation in the national legislature, an effect comparable to changing the electoral formula in municipal elections in Norway.[47] The implication is that if Argentina simplified its electoral calendar, the number of lists gaining representation would increase by 0.7 per province, which would translate into $0.70 \times 24 \approx 17$ additional lists in the Chamber. The results are stronger for small provinces as well as for those districts where a

[46] The estimate is nearly 2.5 times larger than the one reported by Fiva and Folke 2016. Singer and Stephenson 2009 and Singer 2015 report effect sizes of 0.45 and 0.75 for the effect of log (*magnitude*) on *ENPV*; a similar specification yields an estimate of 0.35 (results available upon request).

[47] Fiva and Folke 2016 report an estimate of 0.20-0.22 for this variable.

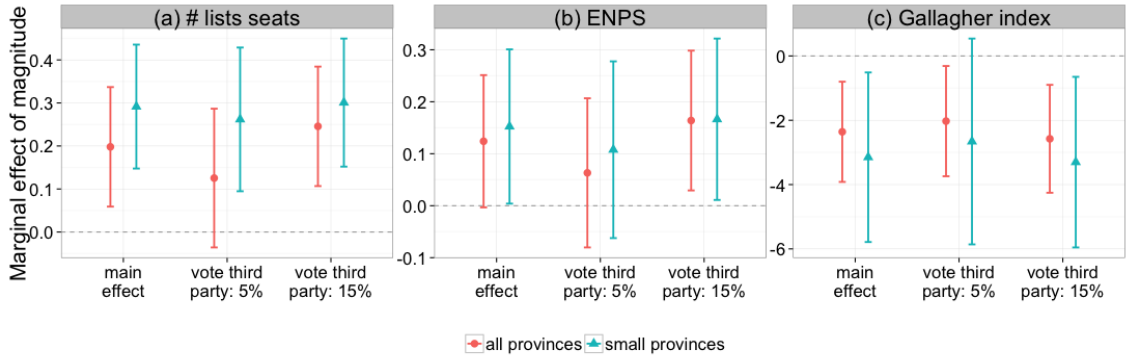

Figure 4: Point estimates and 95% confidence intervals for the effect of district magnitude on the distribution of seats in Argentine lower house elections, 1985-2015. All values are based on the results reported in Table A3 in the online appendix.

third party did better in 1983, though the corresponding estimates are not statistically distinguishable from the main effects for the full sample.

Figure 4b shows that a unit increase in *magnitude* increases the effective number of parties gaining representation by 0.12-0.15. This effect is roughly half in size to that found by other authors in Spain or Norway,[48] and implies that if the Argentine electoral calendar were simplified, the average *ENPS* would increase by about 0.40 — four-fifths of the within-province standard deviation (see Table 2). Again, the effect is stronger for small provinces and those where a third party did better in 1983 — indeed, the estimate is no longer reliable when *vote third party* equals 5 percent —, though the magnitude of the difference is modest.

The last panel of Figure 4 shows that higher magnitudes are also associated with a more proportional translation of votes into seats. The size of the effect is four to six times larger than that reported by Fiva and Folke, though admittedly the baseline level of disproportionality was much lower in Norway. To put these numbers in perspective, an increase in *magnitude* from 3 to 6.5 would cut disproportionality by more than half, from 15.9 to 7.7. The effect is somewhat stronger for small provinces, contradicting Taagepera and Shugart's suggestion that increasing district magnitude from 2 to 3 may result in more dispropor-

[48] Fiva and Folke 2016; Singer 2015.

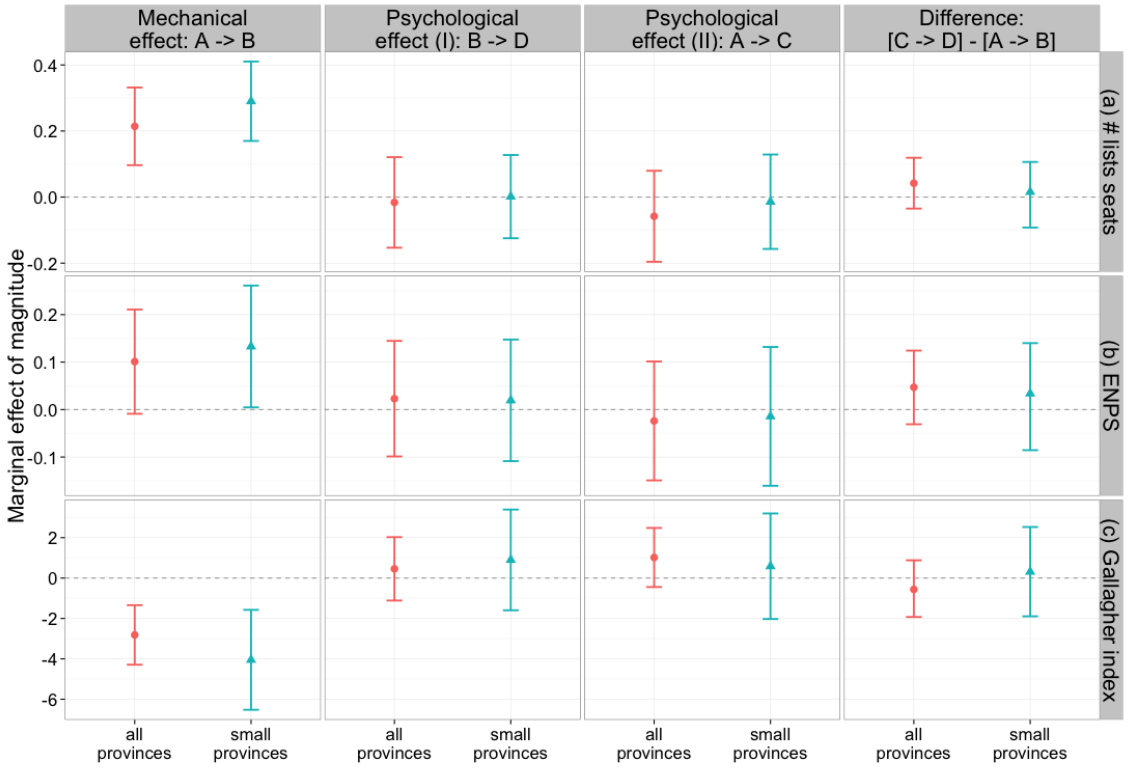

Figure 5: Point estimates and 95% confidence intervals for the contribution of the mechanical and the psychological effects to the distribution of seats in Argentina, 1985-2015. All values are based on the results reported in Table A4 in the online appendix.

tional outcomes.[49] Nonetheless, the fact that the confidence intervals for these provinces are comparatively wider than before suggests that there are some instances in which increasing *magnitude* from 2 to 3 did result in more disproportional outcomes.

Decomposing the mechanical and psychological effects. Figure 5 shows that these results are almost entirely driven by the mechanical effect. Both for the full sample and the subset of small provinces, the estimate for the mechanical effect is almost identical in size to the main effects reported in Figure 4, while estimates of the psychological effect are generally centered around zero. As mentioned above, this does not mean that there is no psychological effect in the sense that an increase in district magnitude does not prompt a change in the distribution of *votes*, but rather that whatever changes there are in the distribution of votes, they are

[49] Taagepera and Shugart 1989:114.

not strong enough to bring about a change in the distribution of *seats*. In particular, when *magnitude* equals 2 or 3 — two thirds of the sample —, competition between three main parties[50] will result in a 1-1 or 1-1-1 distribution of seats. In other words, a unit increase in *magnitude* will increase the (effective) number of lists receiving seats by one, and all by virtue of the mechanical effect. For the psychological effect to make a difference, voters should become either much more willing to support the two largest parties or to desert the third-placed one so that the seat distribution becomes 2-1-0. Alternatively, when there are only two strong parties,[51] an increase in *magnitude* from 2 to 3 will change the seat distribution from 1-1 to 2-1-0; the psychological effect can only make a difference if the vote share of the third-placed list increases substantially.

Of course, these results may also be due to the fact that district magnitude has no effect on the distribution of votes. The fact that the results reported in Figure 3 are not always statistically significant at conventional levels gives credence to this interpretation. However, this argument is subject to two objections. One is that while the estimates of Figure 3 go in the expected direction but are not entirely reliable, the estimated psychological effects are very close to zero. The other is that the psychological effects need not be associated with equivalent changes in the distribution of votes: for example, the 2003 electoral reform in Norway did not change the effective number of parties in votes but had a sizable psychological effect.[52]

Robustness and placebo tests. These results are robust to several specification changes. One potential concern is that the confidence intervals are too narrow because they do not take into account the fact that there are very few provinces. This is unlikely to be an issue, as the intervals are already adjusted using a Student distribution with 9-18 degrees of freedom, and

[50] Given the d'Hondt formula employed in Argentina, this means that the most voted list must not double the second-placed list, and not triple the third-placed one.

[51] Technically, the top-voted list does not double the second but triples the third.

[52] Fiva and Folke 2016.

the distribution of the explanatory variable is identical for all provinces. Nonetheless, Tables A2 and A3 in the online appendix show that calculating the 95% confidence intervals on the basis of the wild bootstrap procedure proposed by Cameron and coauthors[53] produces identical results. Another potential objection is that the results are being driven by a handful of districts in which the effect of *magnitude* is particularly large. To account for this possibility, I replaced the outcome variables with their rank-based versions — i.e., I assigned the value of 1 to the observation with the lowest value within a province, 2 to the second-lowest, and so on until 16 —, thus ensuring that the outcome variables have the same distribution for all provinces. The results remain the same, with the exception of the effective number of parties in seats. Closer inspection show that this is due to the fact that in districts that combine two-party competition with a delegation size of five, increasing *magnitude* from two to three will change the distribution of seats from 1-1 to 2-1-0, thus *reducing* the *ENPS* from 2 to 1.8.[54] While modest in absolute terms, this effect wreaks havoc among the rank-based variables, which weight all increases or decreases equally.

Finally, Figure 6 reports 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 — including several measures of provincial revenues, the number of public employees, or the unemployment and infant mortality rates.[55] Consistent 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.[56]

[53] Cameron, Gelbach and Miller 2008; Cameron and Miller 2015.

[54] Indeed, the effect is much stronger for the subset of small provinces (see Table A3c).

[55] I thank an anonymous reviewer for suggesting this check.

[56] This is especially relevant because it is well documented that small provinces receive systematically more transfers from the center (Galiani, Torre and Torrens 2016; Gervasoni 2010; Gibson and Calvo 2000), and indeed removing the province fixed effects shows an extremely strong association between *magnitude* and the provincial revenues measures (results available upon request). There are no reasons to expect these revenues to fluctuate with short-term changes in district magnitude, however.

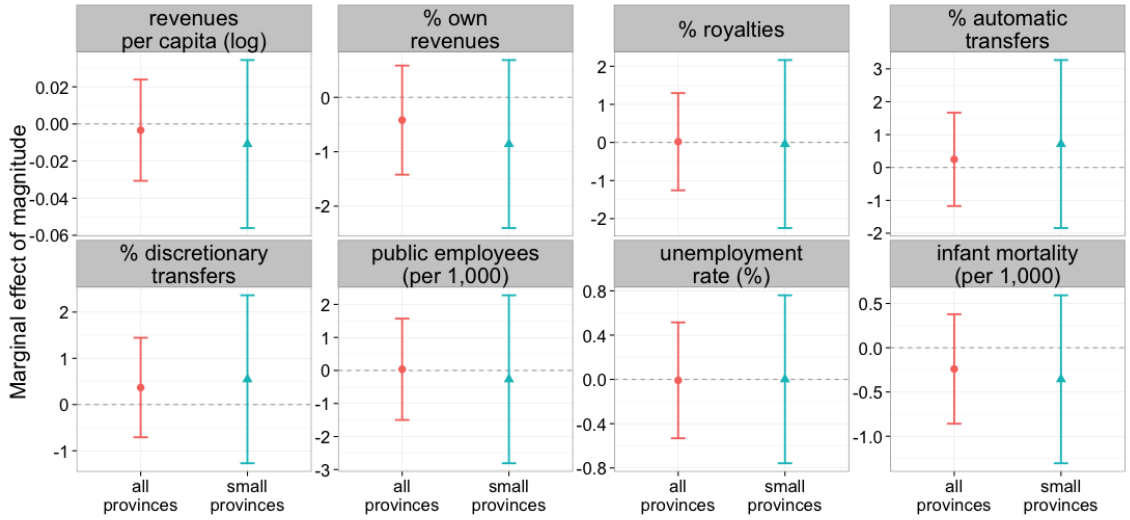

Figure 6: Point estimates and 95% confidence intervals for the effect of district magnitude on some time-varying pseudo-outcomes that should not be affected by the electoral system. All values are based on the results reported in Table A5 in the online appendix.

Discussion and conclusion

A decade ago, Matthew Shugart celebrated the maturity of the literature on electoral systems while lamenting the scarcity of "crucial experiments" that could isolate the effects of electoral rules from that of other factors that shape electoral outcomes.[57] Political scientists responded to this appeal by devising ingenious designs to find evidence of contamination effects in mixed-member systems,[58] identifying the effect of double-ballot rules on electoral coordination,[59] determining how the mechanical and psychological effect shape the distribution of seats,[60] examining the impact of open-list systems on voter turnout,[61] determining

[57] Shugart 2005.

[58] Crisp, Potter and Lee 2012.

[59] Bordignon, Nannicini and Tabellini 2016; Fujiwara 2011.

[60] Fiva and Folke 2016.

[61] Sanz forthcoming.

whether district magnitude increases the number of parties,[62] or comparing the effects of the single-member plurality and the multiple non-transferable votes systems.[63]

By exploiting the peculiar nature of Argentina's electoral calendar to identify the effect of district magnitude on electoral coordination and the distribution of seats, this paper clearly inscribes itself within this "credibility revolution" in the study of electoral systems. In so doing, it extends this literature in two ways. First, it focuses on the effect of district magnitude[64] rather than the electoral formula[65] or the combination of the two.[66] Second, while existing studies have focused on either the distribution of votes before seats are counted,[67] or the relative contribution of the mechanical and psychological effect to the final distribution of seats,[68] this study examines both sets of outcomes. The analysis underscores four main findings. First, higher magnitudes do not increase party entry, though they do have an impact on voters' propensity to support small parties. Second, changes in magnitude have a strong effect on the distribution of seats, sharply increasing the (effective) number of lists that gain legislative representation and decreasing the disproportionality in the translation of votes into seats. Third, this second effect is driven by the mechanical rather than the psychological effect of electoral rules. Finally, there is some evidence that district magnitude makes more of a difference (a) in small districts, and (b) among those provinces that had a stronger third party in 1983.

Of course, these findings are more relevant if they can be extrapolated beyond the Argentine case. In this regard, it is worth mentioning that the external validity of the results is

[62] Singer 2015.

[63] Crisp and Demirkaya 2016.

[64] See also Crisp, Olivella and Potter 2012; Singer 2015.

[65] Bordignon, Nannicini and Tabellini 2016; Fiva and Folke 2016; Fujiwara 2011

[66] Crisp and Demirkaya 2016.

[67] Bordignon, Nannicini and Tabellini 2016; Crisp, Olivella and Potter 2012; Crisp and Demirkaya 2016; Fujiwara 2011; Singer 2015.

[68] Blais et al. 2011; Fiva and Folke 2016.

strengthened by two factors. First, these findings are consistent with previous studies: while several authors have shown that electoral rules affect voters' support for small parties,[69] the evidence that the electoral system also affects party entry has been mixed.[70] Similarly, while Fiva and Folk find that the psychological effect matters for the distribution of seats in Norwegian municipalities, their estimates of the mechanical effect are much larger in size.[71] Second, the results are consistent with theoretical expectations: higher magnitudes increase support for small parties, make them more likely to win seats, and reduces the disproportionality in the translation of votes into seats; and the effects are stronger in small provinces and those where a third party did better in 1983. The point is that had the results been more *ad hoc*, it would be harder to claim that they are valid in other contexts. Given that this is not the case, the claim that they reflect universal features of electoral rules rather than the peculiarities of the Argentine case gains in credibility.

That said, the structure of Argentina's electoral calendar poses some limitations on the generalizability of the findings. Since district magnitude only changes by increments of one, the results may offer a poor guide to what we could expect following a dramatic increase (or decrease) in district magnitude. Furthermore, the fact that magnitude oscillates in a predictable way means that the results can only identify the effect of short-term changes that are known to be short-term. This may be one of the reasons why the results show no effect for party entry: if party elites anticipate that district magnitude is going to oscillate, they will be less likely to invest in a new party organization based on short-term considerations. Whether this means that the strategic considerations of these politicians are driven by the lowest or highest values of district magnitude within a province is unclear, however: perhaps strategic elites decide not to run even when magnitude increases because they anticipate that they will

[69] Crisp, Olivella and Potter 2012; Fujiwara 2011; Singer 2015.

[70] Bordignon, Nannicini and Tabellini 2016; Crisp and Demirkaya 2016; Fiva and Folke 2016 provide strong evidence that electoral rules affect party entry, but Crisp, Olivella and Potter 2012; Fujiwara 2011; Singer 2015 report null findings.

[71] Fiva and Folke 2016.

do badly two years later; alternatively, the lure of winning office in higher-magnitude years may increase the propensity to field candidates in low-magnitude elections as well. On the bright side, exploiting the oscillation of the electoral calendar over a 30-year period rather than looking at what happens just before or after an electoral reform ensures that the results are not being driven by a handful of elections.

Finally, this paper joins a large literature that has taken advantage of the exogenous variation provided by a staggered electoral calendar to study a variety of political phenomena, including legislative careers and behavior,[72] electoral fraud,[73] coattail effects,[74] or mobilization and turnout.[75] Along with the work of Crisp and Demirkaya, who take advantage of a similar design to study senatorial elections in Brazil,[76] this paper shows how staggering legislative elections may provide a unique opportunity for identifying the causal effect of electoral rules. Given that such calendars are relatively common,[77] one hopes that additional opportunities of this kind await to be discovered.

[72] Fukumoto and Matsuo 2015; Goetz et al. 2014; Hicken and Ravanilla 2015; Rosas and Langston 2011; Shepsle et al. 2009.

[73] Fukumoto and Horiuchi 2011.

[74] Magar 2012.

[75] Fukumoto and Horiuchi forthcoming.

[76] Crisp and Demirkaya 2016.

[77] See Fukumoto and Matsuo 2015, Table 1.

A Balance check (online)

If the decision of which provinces would elect more deputies in midterm than in concurrent years was decided randomly, provinces that have a higher magnitude in midterm years[78] should not differ systematically from those that elect more deputies in concurrent years.[79] To check whether this is the case, I collected data on 38 pre-treatment covariates and examined the difference in means between both groups of provinces.

These covariates include (a) the dependent variables, as measured in the 1983 election;[80] (b) the pseudo-outcomes reported in the robustness checks, again measured in 1983;[81] (c) a host of electoral outcomes measured in 1983, including the (combined) vote share of the PJ and the UCR and the share of the vote received by the largest third party;[82] (d) several demographic variables, such as population (density), taken from the 1980 census; (e) a wide array of geographic and historical variables, including area, average latitude, elevation, precipitation, etc, as well as dummies for the country's main geographic regions;[83] and (f) several measured of provinces' political (over-)representation in the national Congress in 1983.[84]

Table A1 displays the means for both groups of provinces, as well as the difference between the two and the exact p-values for the sharp null hypothesis of no effect for any province. Given that the randomization had to respect some restrictions — notably, the

[78] Catamarca, La Pampa, Neuquén, San Luis, Santa Cruz, Chaco, Entre Ríos and the Ciudad de Buenos Aires (see Table 1).

[79] Chubut, Formosa, La Rioja, Río Negro, Tierra del Fuego, Corrientes, Misiones, Salta, Santiago del Estero, Tucumán and Santa Fe.

[80] Source: Andy Tow's Electoral Atlas (`http://andytow.com/atlas/totalpais/`).

[81] 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.

[82] Source: Andy Tow.

[83] Sources: INDEC and Mitton (2016). I am thankful to Todd Mitton for kindly sharing this data.

[84] Sources: Andy Tow and INDEC.

number of deputies elected in concurrent and midterm years had to be equal —,[85] I calculated the p-values using simulations. First, I 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.[86] Each of these vectors represents a different random allocation of the provinces into two groups. Second, for every random draw I calculated the difference in means for each of the variables, and saved these values. Third, I calculated 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. These are the p-values reported in Table A1 and Figure 2. For example, the p-value of 0.89 for the log of population in 1980 indicates that approximately 89, 000 simulations produced a difference in means that was equal to or larger in size than the one observed in the sample.

[85] Dal Bó and Rossi 2011.

[86] 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. 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, and thus it became a member of the former group.

Table A1: *Checking covariate balance*

(a) *Outcome variables (1983)*	large midterm mean	large concurrent mean	difference	p-value
# lists running	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	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
(b) *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
(c) *Electoral outcomes (1983)*				
% vote PJ (1983)	39.02	43.26	-4.24	0.34
% vote UCR (1983)	42.93	40.10	2.83	0.43
% vote PJ+UCR (1983)	81.95	83.36	-1.41	0.80
vote third party	11.81	10.65	1.16	0.81
(d) *Demographics (1980)*				
population (1980) (log)	12.93	12.99	-0.07	0.89
population density (1980) (log)	2.10	1.69	0.41	0.79
% poor (1980)	31.00	39.81	-8.81	0.12
(e) *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	33.25	19.00	14.25	0.54
region: Cuyo	0.12	0.09	0.03	1.00
region: Northeast	0.12	0.27	-0.15	0.59
region: Northwest	0.12	0.27	-0.15	0.59
region: Pampa	0.38	0.09	0.28	0.28
region: Patagonia	0.25	0.27	-0.02	1.00
founding province	0.38	0.55	-0.17	0.64
% foreign population (1914)	31.06	26.68	4.38	0.66
(f) *Political representation (1983)*				
delegation size (1983)	8.25	7.09	1.16	0.73
seat/population ratio (1983)	2.18	1.97	0.22	0.76
% seats - % population (1983)	0.81	0.46	0.35	0.29

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

B Full results and robustness checks (online)

This section presents the tables with the main results and the robustness checks:

(1) Table A2 presents the results for the effect of *magnitude* on electoral coordination reported in Figure 3, as well as the robustness checks for the rank-ordered versions of the dependent variables.

(2) Table A3 presents the results for the effect of *magnitude* on the final distribution of seats reported in Figure 4, as well as the robustness checks for the rank-ordered versions of the dependent variables.

(3) Table A4 presents the results for the contribution of the mechanical and psychological effects to the distribution of seats reported in Figure 5.

(4) Table A5 presents the results of the placebo tests reported in Figure 6.

Table A2: *District magnitude and electoral coordination in Argentina, 1985-2015*

	# lists running		***ENPV***		***vote first two***	
(a) Main effect	full sample	small provinces	full sample	small provinces	full sample	small provinces
magnitude	0.14	0.18	0.12	0.11	-1.80	-1.88
	[-0.46:0.74]	[-0.33:0.70]	[-0.07:0.32]	[-0.10:0.33]	[-3.89:0.29]	[-4.33:0.57]
	[-0.38:0.65]	[-0.34:0.71]	[-0.09:0.33]	[-0.06:0.29]	[-4.42:0.81]	[-4.32:0.56]
(b) Heterogeneous effect						
magnitude	-0.30	0.36	-0.04	0.07	0.05	-1.80
	[-1.16:0.57]	[-0.41:1.13]	[-0.30:0.22]	[-0.20:0.34]	[-2.87:2.97]	[-5.51:1.91]
magnitude	0.04	-0.01	0.01	0.00	-0.17	-0.01
× *vote third party*	[-0.01:0.09]	[-0.07:0.04]	[-0.00:0.03]	[-0.02:0.02]	[-0.38:0.04]	[-0.27:0.25]
(c) Main effect (rank)						
magnitude	0.34	0.31	0.96	0.88	-0.88	-0.93
	[-0.45:1.12]	[-0.69:1.31]	[-0.06:1.98]	[-0.44:2.19]	[-1.87:0.10]	[-2.20:0.34]
(b) Heterogeneous effect (rank)						
magnitude	-0.17	0.53	0.05	1.34	-0.01	-0.92
	[-1.30:0.96]	[-1.13:2.19]	[-1.39:1.49]	[-0.66:3.33]	[-1.42:1.39]	[-2.91:1.08]
magnitude	0.05	-0.02	0.08	-0.04	-0.08	-0.00
× *vote third party*	[-0.03:0.12]	[-0.13:0.10]	[-0.02:0.18]	[-0.17:0.10]	[-0.18:0.02]	[-0.14:0.14]
num. obs	302	158	302	158	302	158
provinces	19	10	19	10	19	10
elections	16	16	16	16	16	16

Note: OLS regression estimates. Values in square brackets report 95% confidence intervals based on robust standard errors (HC3) clustered by province, and assuming a Student distribution with degrees of freedom equal to the number of provinces minus 1. In the third row of panel (a), values in square brackets report the 95% confidence intervals based on the wild bootstrap procedure proposed by Cameron, Gelbach and Miller 2008 and Cameron and Miller 2015. All specifications include province and year fixed effects.

Table A3: *District magnitude and the distribution of seats in Argentina, 1985-2015*

	# lists seats		***ENPS***		***Gallagher index***	
(a) Main effect	full sample	small provinces	full sample	small provinces	full sample	small provinces
magnitude	0.20	0.29	0.12	0.15	-2.36	-3.15
	[0.06:0.34]	[0.16:0.42]	[-0.00:0.25]	[0.01:0.29]	[-3.92:-0.80]	[-5.60:-0.70]
	[0.02:0.38]	[0.12:0.46]	[-0.01:0.25]	[-0.02:0.32]	[-4.10:-0.61]	[-6.35:0.05]
(b) Heterogeneous effect						
magnitude	0.07	0.24	0.01	0.08	-1.75	-2.34
	[-0.13:0.26]	[0.05:0.43]	[-0.16:0.19]	[-0.12:0.27]	[-3.84:0.34]	[-5.95:1.27]
magnitude	0.01	0.00	0.01	0.01	-0.06	-0.06
× *vote third party*	[0.00:0.02]	[-0.01:0.01]	[-0.00:0.02]	[-0.01:0.02]	[-0.19:0.08]	[-0.26:0.13]
(c) Main effect (rank)						
magnitude	1.45	2.11	-0.65	-1.88	-1.10	-1.02
	[0.61:2.28]	[1.12:3.10]	[-1.67:0.36]	[-3.14:-0.62]	[-2.05:-0.15]	[-2.25:0.21]
(b) Heterogeneous effect (rank)						
magnitude	0.65	1.86	-1.01	-2.27	-0.75	-0.50
	[-0.52:1.82]	[0.45:3.26]	[-2.46:0.43]	[-4.13:-0.41]	[-2.09:0.58]	[-2.29:1.29]
magnitude	0.07	0.02	0.03	0.03	-0.03	-0.04
× *vote third party*	[0.00:0.14]	[-0.06:0.10]	[-0.06:0.12]	[-0.08:0.14]	[-0.12:0.06]	[-0.15:0.06]
num. obs	302	158	302	158	302	158
provinces	19	10	19	10	19	10
elections	16	16	16	16	16	16

Note: OLS regression estimates. Values in square brackets report 95% confidence intervals based on robust standard errors (HC3) clustered by province, and assuming a Student distribution with degrees of freedom equal to the number of provinces minus 1. In the third row of panel (a), values in square brackets report the 95% confidence intervals based on the wild bootstrap procedure proposed by Cameron, Gelbach and Miller 2008 and Cameron and Miller 2015. All specifications include province and year fixed effects.

Table A4: *Contribution of the mechanical and psychological effects to the distribution of seats in Argentina, 1985-2015*

	# lists seats		***ENPS***		***Gallagher index***	
(a) Main effect	full sample	small provinces	full sample	small provinces	full sample	small provinces
Mechanical effect: A → B	0.21	0.29	0.10	0.13	-2.82	-4.05
	[0.10:0.33]	[0.18:0.40]	[-0.01:0.21]	[0.01:0.25]	[-4.29:-1.34]	[-6.35:-1.75]
Psychological effect (I): B → D	-0.02	0.00	0.02	0.02	0.46	0.90
	[-0.15:0.12]	[-0.12:0.12]	[-0.10:0.14]	[-0.10:0.14]	[-1.11:2.03]	[-1.42:3.21]
Psychological effect (II): A → C	-0.06	-0.01	-0.02	-0.01	1.02	0.59
	[-0.20:0.08]	[-0.15:0.12]	[-0.15:0.10]	[-0.15:0.12]	[-0.44:2.48]	[-1.84:3.01]
Difference: [C → D] - [A → B]	0.04	0.01	0.05	0.03	-0.56	0.31
	[-0.04:0.12]	[-0.09:0.11]	[-0.03:0.12]	[-0.09:0.14]	[-1.93:0.88]	[-1.89:2.53]
(b) Heterogeneous effect						
Mechanical effect: A → B	0.16	0.17	0.04	-0.01	-1.88	-2.19
	[-0.01:0.32]	[0.01:0.33]	[-0.11:0.19]	[-0.18:0.15]	[-3.91:0.15]	[-5.67:1.29]
Mechanical effect: A → B	0.01	0.01	0.01	0.01	-0.09	-0.15
× *vote third party*	[-0.00:0.01]	[0.00:0.02]	[-0.00:0.01]	[0.00:0.02]	[-0.23:0.06]	[-0.34:0.05]
Psychological effect (I): B → D	-0.09	0.07	-0.03	0.09	0.12	-0.15
	[-0.29:0.10]	[-0.10:0.24]	[-0.20:0.14]	[-0.09:0.26]	[-1.92:2.17]	[-3.71:3.41]
Psychological effect (I): B → D	0.01	-0.01	0.01	-0.01	0.03	0.08
× *vote third party*	[-0.00:0.02]	[-0.01:0.00]	[-0.01:0.01]	[-0.01:0.00]	[-0.10:0.16]	[-0.12:0.29]
Psychological effect (II): A → C	-0.10	0.00	-0.06	-0.01	0.70	0.69
	[-0.30:0.09]	[-0.20:0.20]	[-0.23:0.11]	[-0.21:0.19]	[-1.33:2.73]	[-2.83:4.21]
Psychological effect (II): A → C	0.00	-0.00	0.00	-0.00	0.03	-0.01
× *vote third party*	[-0.01:0.01]	[-0.01:0.01]	[-0.01:0.01]	[-0.01:0.01]	[-0.10:0.16]	[-0.19:0.17]
num. obs	302	158	302	158	302	158
provinces	19	10	19	10	19	10
elections	16	16	16	16	16	16

Note: OLS regression estimates. Values in square brackets report 95% confidence intervals based on robust standard errors (HC3) clustered by province, and employing a Student distribution with degrees of freedom equal to the number of provinces minus 1. All specifications include province and year fixed effects.

Table A5: *Placebo tests. The effect of district magnitude on time-varying pseudo-outcomes in Argentina, 1985-2011*

(a) Main sample	***revenues per capita (100's)***	***% own revenues***	***% royalties***	***% automatic transfers***	***% discretionary transfers***	***public employees (per 1,000)***	***unemployment rate (%)***	***infant mortality (per 1,000)***
magnitude	-0.00	-0.42	0.02	0.25	0.37	0.04	-0.01	-0.24
	[-0.03:0.02]	[-1.42:0.58]	[-1.26:1.30]	[-1.17:1.67]	[-0.71:1.44]	[-1.50:1.57]	[-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
(b) Small provinces								
magnitude	-0.01	-0.86	-0.04	0.71	0.55	-0.27	0.00	-0.36
	[-0.05:0.03]	[-2.29:0.57]	[-2.09:2.01]	[-1.66:3.08]	[-1.14:2.23]	[-2.63:2.10]	[-0.70:0.71]	[-1.24:0.53]
num. obs	128	128	128	128	128	112	137	117
provinces	10	10	10	10	10	10	10	10
elections	13	13	13	13	13	12	14	12

Note: OLS regression estimates. Values in square brackets report 95% confidence intervals based on robust standard errors (HC3) clustered by province, and employing a Student distribution with degrees of freedom equal to the number of provinces minus 1. All specifications include province and year fixed effects.

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