Appendix 1: Construction of the Data

We need to match the electoral data and the census data for the two considered elections: 2003 and 2009. The challenge is that, due to redistricting, the municipalities in each dataset do not always coincide. This section explains how we have merged our data.

The electoral data correspond to the official results of the elections provided by the ministry of interior.[1] The data consist of the number of seats obtained by each party in each municipality.

The key explanatory variable in our analysis is the electoral system in each municipality. For 2009, we can derive it from the electoral results released. Using the number of electoral districts in the municipality, we code municipalities with one electoral district as proportional and the rest as majoritarian (the lowest number of council seats in majoritarian municipalities was 11). For 2003 there is no information on the number of electoral districts. However, for reasons explained below, we feel confident that our population data is exactly the one used when applying the electoral system so that, in practice, we can use the population variable as an indicator for proportional/ majoritarian: Municipalities with population higher than 25,000 are coded as proportional and those below 25,000 as majoritarian.

The population and demographic data come from the "Haut Commisariat du Plan", the official statistical agency of Morocco. There have been censuses in Morocco in 1994 and 2004. For 2004, the data aggregated at the municipal level is publicly available.[2] The data includes the population of the municipality as well as demographic variables such as, in each municipality, the percent of literate individuals, or of mobile phones, or of the public sector employees.

The key challenge when merging population and electoral data is redistricting. Due to redistricting, boundaries of municipalities have changed so that the municipalities in the 1994 census (the census presumably used to determine majoritarian/proportionality status in 2003) need not correspond to the municipalities in the 2003 election data and similarly with the census of 2004 and the 2009 elections.

For the 2003 election, the problem has been overcome by an official document that lists municipalities *according to the 2004 demarcation*, with their population in 2004 *and* in 1994 (Décret 2-05-189 2nd December 2005). For the 2003 analysis, we use the 1994 population from this document merged with the 2003 electoral data.[3] The dataset is then merged with the demographic variables from the 2004 census.

The 2003 merged data appear satisfactory. In the merger, we lose only 18 out of 1,509 observations. Most importantly, it appears that our 1994 population variable has been precisely the one used to implement the electoral law. While, as mentioned above, we do not have explicit information on whether the electoral system in one municipality was proportional or majoritarian, we do have official information on another electoral variable: the number of councilors. The Official Bulletin 5096 from 03/04/2003 published the official mapping between population and number of councilors in the 2003 elections (for ex. municipalities with population between 15,000 and 25,000 ought to have 23 councilors).

[1] http://www.elections2003.ma for 2003 and http://www.elections2009.gov.ma for 2009.

[2] The data can be found at http://www.hcp.ma/.

[3] A few municipalities are dropped in the merger due to the difference between administrative and electoral local units. These correspond to neighborhoods within some big cities that are considered separately in the census, but not in elections. For our analysis this is not problematic, for these are very large units, way above the population threshold where electoral system changes from majoritarian to proportional.

Our merged data replicates this mapping almost perfectly.[4] This strongly suggests that mapping from population to proportional/majoritarian system has also been done on the basis of the data we use so that the lack of an explicit 2003 proportional/majoritarian variable is not problematic in our analysis.

For 2009, we merge election data with the 2004 population and demographic data from the census. The 2009 election data do not contain names of municipalities, but do include a municipal code that is also present in the 2003 election data. Thus, we merge the 2009 election results with the demographic data via the 2003 election data. This implies that that our 2009 data naturally includes the 2003 election results. Another implication is that slightly more observations are lost in the merger, although still a rather insignificant amount (35 out 1,510).

Table 1 shows some descriptive statistics of our sample, for all municipalities as well as separately for the towns with proportional and majoritarian systems. We display information on selected demographic and electoral variables. As demographic variables, besides population, we consider variables that have been shown to be potentially relevant for the success of different parties in Morocco (see Pellicer and Wegner 2012): an indicator of whether the municipality is urban or rural, literacy rates, percent of public employees and of owners of mobile phones. As electoral variables, we display the percent of seats of the considered parties. We also show information on our key outcome variables: the average seats of the three types of parties (clientelistic, programmatic and "in between"), as explained above.

(Table 1 around here)

The table shows that, overall, most Moroccan municipalities are rural and relatively small (90% rural and with mean population of around 10,000 inhabitants). Average literacy rates are, at 42%, comparatively low. The table also shows average seats of the different parties in the 2003 and 2009 elections. The results show the considerable fragmentation of the Moroccan political system, with four/five parties having between 10% and 20% of seats on average and no party much bigger than that. The parties we have selected for our analysis are the biggest in the country but do not completely exhaust the overall seats (a total of 66% of the seats on average in 2003 and 90% in 2009).

Appendix 2: Preferred regression discontinuity specifications

In order to choose our specifications, we start by following the standard approach: to use a linear model with bandwidth chosen on the basis of some performance criterion, such as the cross validation criterion.[5] In our case, the choice is not straightforward, for two reasons. First, because we have three outcome variables in each year; since we want to compare the treatment effect on each outcome, we prefer to use the same bandwidth for each type of party to ensure that the difference in results by type of party that may arise are not driven by differences in bandwidth. A second difficulty with bandwidth choice

[4] Only 9 out of the 1,491 do not match, out of which 5 are "Mechouars", neighborhoods with special status for containing a royal palace (we erase those from the analysis in both election years)

[5] The cross validation criterion essentially tries to choose the bandwidth that leads to best predictions of observations close to the threshold, see Imbens and Lemieux 2008 and Lee and Lemieux 2009 for details. We restrict the prediction of observations to those within a window of 10,000 at either side of the threshold.

is that the right and left side of the thresholds differ substantially (ex. in sample size). Figure 1 shows the Cross-Validation value for the right and left side of the threshold for the different types of parties.[6] The optimal bandwidth for each party is the population value in the horizontal axis that minimizes the function. It is clear that the optimal bandwidth differs by party, as well as by side of threshold (right vs. left). While the plots cannot give us a clear-cut answer on the choice of bandwidth, they are informative. For the left side, minima tend to be between 10,000 and 15,000. For the right side, clientelistic type of parties attain their minimum between 15,000 and 20,000, whereas the other two attain it at higher levels, although the decrease at levels higher than 20,000 is very mild. In any case, the graphs do show that windows smaller than 10,000, and specially smaller than 5,000, are likely not to be very efficient.

(Figure 1 around here)

On the basis of this, our pragmatic choice of specifications are the following. Our preferred specifications will be a linear spline with bandwidth of 15,000 (as a compromise emerging from Figure 1) and a fifth order polynomial with no spline using the full sample (to acknowledge the uncertainty surrounding the 15,000 bandwidth and the potential benefit from expanding it).

For 2009 we once again use the cross validation criterion. Figure 2 shows the Cross-Validation function for the three types of party to the left and right of the threshold. Again, results are not clear-cut. As for 2003, very small windows are likely to be quite inefficient (particularly below 10,000). On the left side, minima are attained between 20,000 and 25,000. On the right side, the minima are between 45,000 and 50,000. However, already at 25,000 values are relatively low, with very mild decreases thereafter; actually there are local minima between 25,000 and 30,000 not far off the global minima.

(Figure 2 around here)

On the basis of this, our preferred specifications for 2009 will be a linear spline with window 25,000, and a polynomial of order five using the full sample.

Appendix 3: Regression discontinuity validity checks

The identification strategy of the regression discontinuity (RD) design hinges on the continuity of potential outcomes around the discontinuity threshold. There are two types of problems that may render this assumption invalid. First, it could be that other variables important for outcomes also experience a discontinuity at the threshold. Second, knowing of the implications of falling at either side of the threshold, some agents may manipulate the forcing variable so as to fall into the side that is most convenient to them. This would lead to a systematic difference between observations at either side of the threshold in addition to the causal variable of interest.

We believe the manipulation concern can be readily ruled out in our case. For 2003, the forcing variable is the 1994 population. It is virtually impossible that 1994 population figures would have been purposefully manipulated with an eye on the 2003 elections. For 2009, as mentioned above, we believe there might have indeed been manipulation. However, this manipulation would have taken place via redistricting in 2008, not via the original population figures of 2004, which are the ones we use.

[6] The cross validation value has been normalized in order to make comparisons across types of parties easier.

We thus turn to the possibility that some variables other than the electoral system (and the number of councilors we already control for) also jump at the threshold. A priori there is no reason why any variable should jump at the relevant threshold, either for 2003 or for 2009. We consider the full set of 29 demographic variables in our 2004 census data. Table 2 provides estimates of the coefficient for the jump using each variable as dependent variable, for 2003 and 2009. for each year, we use the two preferred specifications, plus the specification with a small window for robustness. Most estimates are statistically indistinguishable from zero, although there are naturally some exceptions. The important message of the table is that no coefficient is robustly significant across specifications. Thus, it appears quite safe to rule out that other covariates jump at the threshold. To a certain extent this was already expected, given that, as we saw above, the introduction of covariates does not affect much the coefficients in the discontinuity regressions in the main text of the article.

(Table 2 around here)

A final potential concern for the validity of the RD design is that the outcome variable is generally "jumpy", so that the possible discontinuity at the threshold is just a reflection of the general behavior of the variable. In our case, there are two mechanisms that could generate discontinuities at different thresholds. First, as mentioned above, the number of councilors does change at specific population thresholds. Second, some parties, particularly those with limited capacity, could potentially use the population of the municipality as a variable to determine where to invest resources or even where to run. In order to assess whether this appears to be the case in our data, we perform placebo tests as recommended in Imbens and Lemieux (2008). These essentially consist on running the standard discontinuity regressions, but considering an arbitrary threshold instead of the one where the electoral rule changes. In order not to have results contaminated by the jump at the true threshold, regressions are run using observations only on the corresponding side of the threshold. We run our two preferred specifications plus the specification with the small window.[7] We use thresholds every 5,000 inhabitants, as well as the population thresholds where the number of councilors changes. Tables 3 and 4 show the results. Each row corresponds to a different threshold used, whereas each column corresponds to a particular model, with a given outcome variable and a given polynomial/window specification. Most of the coefficients are not statistically significant, although again, there are exceptions. Importantly, the population threshold where the number of councilors experiences by far the biggest jump (15,000 for both 2003 and 2009) is generally not significant. Several of the exceptions where the coefficients are significant correspond to situations where large coefficients of a given sign are followed by equally large coefficients of the opposite sign. These type of cases occur particularly at large thresholds, where there are very few data points around the threshold (ex. threshold 40,000 for the programmatic party in 2003). These cases seem to reflect outliers close to the threshold and do not appear very concerning. Besides these, two cases appear potentially concerning, both in 2003: threshold 10,000 for clientelistic parties and threshold 20,000 for "In-Between" parties.

(Table 3 around here)

(Table 4 around here)

[7] In several specifications, the number of councilors is not included because of collinearity.

In order to assess how concerning these cases are, we report the graph of the discontinuity for each case in Figure 3. We use the specification with the large sample and polynomial of degree 5. Overall, we believe the graphs are reassuring, in the sense that the jumps they pick up do not appear to be a source of great concern. In the first panel, the sequence of points could be equally well considered smoothly decreasing upon crossing the (fake) threshold. The second panel seems to reveal a clear outlier on the right and close to the threshold. The third panel is an example of the type of case mentioned above, with large jumps and small sample size around the threshold. Again, a set of outliers appear evident on the right close to the threshold. Overall, these pictures appear to represent more aberrant behavior than genuine jumps.

(Figure 3 around here)

Appendix 4:Analysis with alternative measure of clientelism

In order to assess whether our results are indeed driven by clientelism, it is be useful to have some additional and unrelated way of grouping parties into clientelistic/ programmatic and check if the results are robust to this alternative grouping. This requires a new proxy for the degree of clientelistim of a party. Our approach is as follows. According to the literature (Stokes 2007b, Pellicer 2009), among the clearest correlates of clientelism are poverty and illiteracy. Citizens with least resources and worst prospects are those most willing to trade off ideology for immediate material gains and to engage in clientelistic relations. Thus, clientelistim tends to thrive, not in middle or upper class districts, but rather in disadvantaged ones, in terms of poverty and illiteracy. On the basis of this, we consider a party that consistently does better in poorer and more illiterate places than in middle class ones, to be rather clientelistic. This would probably not be reasonable in polities where the programmatic logic dominates, but it appears reasonable in clientelistic polities, such as Morocco. In such countries, programmatic parties tend to be more successful in wealthier, middle class districts (Pellicer and Wegner 2012).

To compute an indicator of programaticness for each party, we thus run a regression, for each party, of the votes obtained by the party in a district on the percent of literacy/ wealth in the district. These regressions are performed using results from 2002 and 2007 national elections.[8] To check for robustness, we run several models, using as regressors literacy rates, percent of satellite dishes, and percent of mobile phones in the district.[9] The coefficients of these regressions are then our measures of how programmatic the party is: Parties that do better in wealthy/ literate districts would display a positive coefficient and would thus be coded as programmatic.[10]

[8]Using a different dataset to estimate the outcome and the explanatory variables helps making the two sets of estimates independent from each other. Moreover, the national level data have the advantage of possessing information on turnout and allow us to measure party support as votes per number of registered voters, a more accurate measure of support than the percent of seats (see Pellicer and Wegner 2012 for an elaboration of this point and for a description of the data).

[9]Notice that we do not aim to capture the "true" causal effect of literacy, or of mobile phones, on the success of different parties. Rather we try to capture broad correlations; i.e. to obtain a measure of which parties tend to perform better in districts that are overall better off. Our coefficients are thus just simple regressions with either literacy, mobile phones or satellite dishes as the only regressor.

[10]To facilitate interpretation, we standardize these coefficients, by subtracting the mean value of the

We then regress the estimated causal effect of proportionality for each party (i.e. our RD estimate of the jump at the proportionality threshold using our two preferred polynomial specifications) on this measure of programmaticness.[11] Our hypothesis states that the coefficient of interest in this regression should be positive (programmatic parties ought to gain from a proportional system). In addition to this, since theory strongly suggests that party size is likely to be relevant for explaining whether a party benefits from a proportional system, we include party size in the regression as well[12].

The results of these models are presented in Table 5. Columns 1 and 2 show the benchmark results. The coefficient of interest is the one of the variable "Literacy". To reiterate, this coefficient tells how much more programmatic parties benefit from a proportional system, when programmaticness is measured by how much better a party fares in literate as opposed to illiterate districts. For the two polynomial specifications, the coefficient is clearly positive (and highly significant), both for 2003 and for 2009. Moreover, the magnitudes are similar in the two years. The results thus indicate that more programmatic parties benefit more from a proportional system. These results hold using different proxies for the wealth of a district (satellite dishes or mobile phones) instead of literacy (Columns 3 to 6).[13] The coefficient for party size has the expected behavior. In 2003, it is strongly negative and significant: as theory would predict, bigger parties lose from a proportional system. For 2009, however, the effect disappears, presumably because of the increase in electoral thresholds from 3% to 6% in the proportional system: with the increased threshold, small parties are as much disadvantaged in the proportional as in the majoritarian system.

(Table 5 around here)

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coefficient across parties and dividing by its standard deviation.

[11]The 2003 (2009) estimate of the effect of proportionality is regressed on the 2002 (2007) measure of programmaticness.

[12]This variable is measured as the average percent of votes obtained in the relevant national election.

[13]In unreported regressions, we check for the robustness of these results further by running the regressions excluding the PJD, a clear outlier in terms of programmaticness according to our indicator, and by using local election results (instead of national ones) to compute our indicator of programmaticness. In both cases, results remain qualitatively similar, although significance levels drop.

Tables and Figures

Table 1: Descriptive Statistics

	All		Majoritarian		Proportional	
	Mean	SD	Mean	SD	Mean	SD
Pop94	11678.4	10221.0	9728.5	5118.5	42322.9	18403.2
Pop04	13244.8	13280.0	10761.5	6848.7	52271.5	24416.2
Urban	11.8	32.2	8.5	27.9	62.5	48.7
Literacy	42.6	13.1	41.7	12.5	57.4	13.6
Mobile.Phones	44.6	17.3	43.5	16.9	62.2	13.6
Public.Employees	6.5	9.2	6.0	8.8	14.5	11.5
2003						
RNI	12.4	21.0	12.7	21.5	7.7	8.6
MP	10.0	18.6	10.4	19.0	5.2	7.3
UC	4.4	13.7	4.4	14.0	4.8	8.1
PI	17.3	20.4	17.6	20.9	11.1	9.9
USFP	15.6	20.2	15.9	20.6	9.8	9.6
PPS	5.3	13.0	5.3	13.3	5.1	7.7
PJD	1.5	4.9	1.0	4.0	9.3	9.3
Clientelistic	9.0	9.2	9.2	9.4	5.9	4.0
In between	12.7	9.6	13.0	9.8	8.6	5.3
Programmatic	1.5	4.9	1.0	4.0	9.3	9.3
2009						
PAM	22.6	25.0	23.0	25.3	14.7	16.9
RNI	15.5	22.7	15.8	23.1	8.8	11.3
MP	7.8	16.4	7.8	16.5	7.6	15.6
UC	4.0	11.8	3.8	11.8	7.2	11.3
PI	19.8	23.7	20.0	23.9	14.6	15.3
USFP	12.6	19.2	12.8	19.4	9.2	12.3
PPS	4.1	12.1	4.2	12.3	3.2	7.6
PJD	3.5	8.7	3.1	8.2	12.2	13.7
Clientelistic	12.5	7.6	12.6	7.6	9.6	6.2
In between	12.2	9.6	12.3	9.7	9.0	6.7
Programmatic	3.5	8.7	3.1	8.2	12.2	13.7

For municipal characteristics, Majoritarian and Proportional refer to the 2003 elections. Figures are in percentage terms.

Table 2: Discontinuities in other variables

Outcome vars	2003 All	2003 $\pm 15K$	2003 $\pm 5K$	2009 All	2009 $\pm 25K$	2009 $\pm 10K$
Urban	0.09	0.12	-0.10	0.11	0.25	0.14
	(0.10)	(0.11)	(0.17)	(0.18)	(0.14)	(0.30)
Literacy	-4.05	0.25	-5.50	3.10	2.08	1.66
	(3.04)	(3.27)	(5.25)	(5.16)	(4.13)	(8.17)
Age.less.than.6	0.34	-0.59	-0.53	-0.16	-0.20	0.04
	(0.42)	(0.42)	(0.69)	(0.72)	(0.57)	(1.12)
Age.from.6.to.14	0.62	-0.56	0.27	-0.63	-0.45	0.11
	(0.55)	(0.54)	(0.86)	(0.79)	(0.63)	(1.31)
Age.from.14.to.59	-1.61	1.02	-0.48	0.69	0.21	0.21
	(0.85)	(0.79)	(1.33)	(1.10)	(0.87)	(1.59)
Age.more.than.60	0.39	-0.13	0.74	0.20	0.46	-0.34
	(0.41)	(0.40)	(0.64)	(0.64)	(0.52)	(0.95)
Single	-2.46	1.11	1.52	-1.28	-1.60	-2.35
	(0.97)	(0.89)	(1.48)	(1.49)	(1.15)	(2.39)
Married	2.31	-1.36	-1.68	0.80	0.84	2.63
	(0.96)	(0.85)	(1.45)	(1.65)	(1.27)	(2.77)
Fertility	-0.11	-0.37	-0.20	-0.27	-0.27	-0.15
	(0.18)	(0.18)	(0.31)	(0.27)	(0.22)	(0.40)
Amazig	6.61	8.76	28.45	-6.00	3.14	28.52
	(8.09)	(8.85)	(12.72)	(14.41)	(11.60)	(18.60)
Arab	-0.76	-0.77	-1.36	0.35	-0.18	1.51
	(0.90)	(0.87)	(1.31)	(1.67)	(1.37)	(2.56)
Arab.and.French	-2.14	0.94	-3.27	2.15	1.23	-0.11
	(1.98)	(2.14)	(3.19)	(3.29)	(2.63)	(5.15)
Primary	-2.93	0.45	-5.32	2.40	0.85	-0.20
	(2.68)	(2.87)	(4.57)	(4.44)	(3.62)	(7.03)
College	-0.67	-0.21	-0.51	0.31	0.52	-0.04
	(0.48)	(0.54)	(0.87)	(0.96)	(0.74)	(1.56)
Active	-94.36	-85.10	-160.66	-4.30	4.51	-2.34
	(100.17)	(91.93)	(163.81)	(119.09)	(6.05)	(2.27)
Public.Employees	-5.21	-0.02	-2.02	1.24	2.38	0.28
	(2.31)	(2.38)	(3.22)	(4.34)	(3.29)	(6.01)
Private.Employees	1.54	3.63	-0.86	-1.42	-4.26	0.11
	(4.12)	(4.48)	(7.64)	(6.12)	(5.12)	(8.11)
Number.of.Households	214.27	687.79	-440.77	295.31	616.41	394.01
	(495.15)	(520.80)	(868.05)	(589.12)	(441.72)	(857.50)
Slums	2.59	5.16	-8.33	4.43	3.11	-3.76
	(3.73)	(4.07)	(5.19)	(6.33)	(5.20)	(8.18)
HouseOwners	3.99	-4.05	1.21	-4.16	-4.52	-7.31
	(3.41)	(3.55)	(5.85)	(5.60)	(4.43)	(7.67)

Table 2: ***(continued)***

Outcome vars	2003 All	2003 $\pm 15K$	2003 $\pm 5K$	2009 All	2009 $\pm 25K$	2009 $\pm 10K$
Renting	-1.87	1.04	-3.67	2.35	4.49	11.13
	(2.40)	(2.51)	(4.14)	(4.69)	(3.68)	(6.65)
Houses.less.than.10.years	-4.71	1.50	-7.83	3.78	-1.01	14.17
	(2.97)	(2.78)	(4.48)	(5.04)	(4.17)	(9.01)
Houses.more.than.50.years	2.58	-5.07	1.61	-3.10	-0.10	-5.18
	(3.54)	(3.51)	(5.66)	(4.09)	(3.41)	(5.68)
Water	3.33	9.62	9.16	9.91	9.71	15.73
	(7.55)	(8.38)	(13.61)	(12.30)	(9.64)	(19.00)
Electricity	-4.03	2.98	-3.86	10.63	7.13	11.43
	(6.08)	(6.65)	(11.41)	(8.01)	(6.62)	(12.71)
Sewer	-6.91	-0.54	-4.70	15.52	15.37	15.37
	(6.77)	(7.54)	(12.12)	(12.83)	(10.37)	(19.57)
Septic.Tank	6.36	3.79	-6.75	-10.49	-11.16	-17.10
	(5.29)	(5.51)	(8.65)	(10.20)	(8.29)	(15.19)
Fixed.Phone	-3.17	-0.05	-0.57	1.00	0.79	0.13
	(1.48)	(1.55)	(2.51)	(2.58)	(1.99)	(3.99)
Mobile.Phones	2.14	4.35	-0.14	1.87	0.70	4.83
	(3.61)	(3.86)	(6.36)	(5.03)	(4.17)	(7.63)

Table 3: Discontinuities of party seats at other thresholds for 2003

Threshold	Clientelistic	Clientelistic	Clientelistic	In between	In between	In between	Programmatic	Programmatic	Programmatic
7.5	1.76	-0.08	1.79	-1.50	-0.56	-1.93	0.16	-0.07	0.16
	(1.29)	(0.88)	(1.14)	(1.34)	(0.94)	(1.19)	(0.59)	(0.40)	(0.52)
10	-1.96	-1.43	-1.98	-0.10	0.70	0.40	-1.40	-0.56	-1.27
	(1.30)	(0.85)	(1.16)	(1.40)	(0.90)	(1.26)	(0.59)	(0.36)	(0.50)
12.5	-0.16	0.10	0.56	2.07	1.80	1.72	1.01	0.71	1.00
	(1.40)	(1.07)	(1.39)	(1.49)	(1.13)	(1.44)	(0.62)	(0.57)	(0.60)
15	-1.88	0.009	0.89	0.88	1.31	-3.49	-2.16	-1.08	-1.41
	(3.91)	(3.488)	(1.85)	(3.51)	(3.07)	(1.85)	(1.54)	(1.33)	(0.76)
20	2.26	1.43	1.10	-4.91	-3.14	-3.70	-1.01	-0.83	-1.01
	(2.61)	(2.32)	(2.59)	(2.19)	(1.78)	(2.08)	(0.84)	(0.58)	(0.78)
30	-0.11	0.79	-2.68	3.64	2.97	3.98	-3.34	-5.01	-1.63
	(1.71)	(1.73)	(2.02)	(3.20)	(3.02)	(3.46)	(3.75)	(3.83)	(4.74)
35	-0.46	0.20	-2.67	1.66	1.61	1.57	2.59	3.32	-1.53
	(3.28)	(2.70)	(4.50)	(2.95)	(2.75)	(3.91)	(4.61)	(4.17)	(4.90)
40	-4.50	-1.47	-2.79	-2.12	-2.46	-2.89	13.20	10.66	8.79
	(3.09)	(2.60)	(4.24)	(3.63)	(3.16)	(2.89)	(7.35)	(6.37)	(7.95)
45	1.72	0.61	3.64	-5.09	-4.92	-8.74	-16.19	-13.30	-19.50
	(3.49)	(3.31)	(10.83)	(3.57)	(3.08)	(9.24)	(9.06)	(7.91)	(33.83)
50	-2.03	-3.77	-0.96	5.57	6.88	0.87	-2.68	-1.81	6.68
	(2.94)	(3.06)	(4.45)	(3.08)	(3.19)	(3.80)	(6.93)	(7.42)	(6.93)
window	All	$\pm 15K$	$\pm 5K$	All	$\pm 15K$	$\pm 5K$	All	$\pm 15K$	$\pm 5K$
poly.order	5	1	1	5	1	1	5	1	1
spline		x	x		x	x		x	x

Table 4: Validity checks: Discontinuities of party seats at other thresholds for 2009

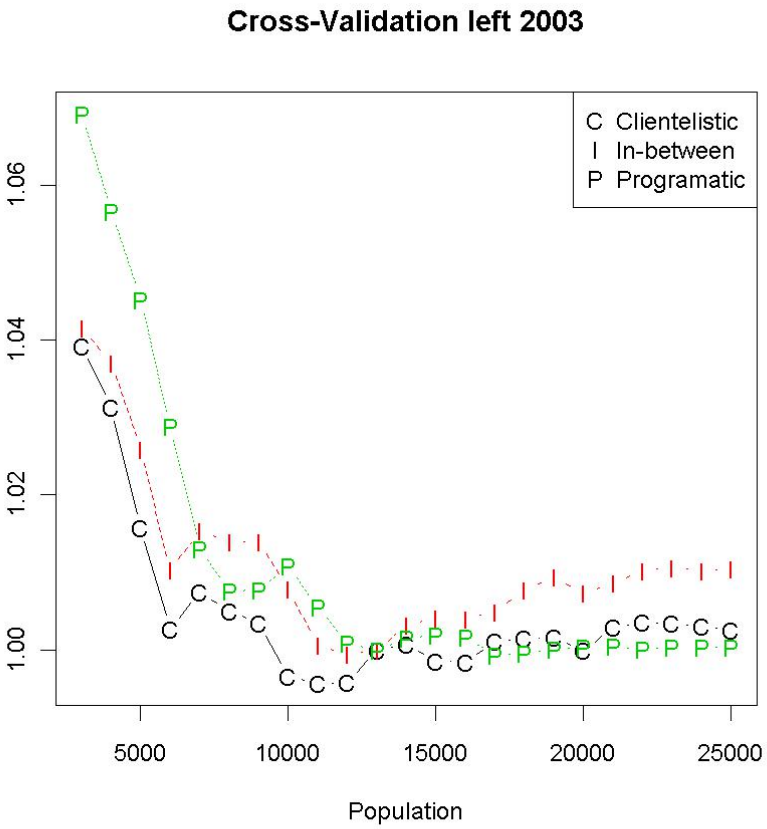
Threshold	Clientelistic	Clientelistic	Clientelistic	In between	In between	In between	Programmatic	Programmatic	Programmatic
7.5	-0.05	-1.53	-1.11	0.49	1.96	1.45	-0.10	0.37	0.63
	(0.99)	(0.72)	(0.80)	(1.25)	(0.93)	(1.02)	(1.10)	(0.85)	(0.92)
10	0.62	-0.63	-0.57	-0.60	0.33	0.32	-0.82	-0.44	-0.30
	(0.96)	(0.69)	(0.79)	(1.23)	(0.88)	(1.01)	(1.28)	(0.84)	(0.98)
15	-0.86	-0.62	-0.63	3.36	1.41	2.04	-0.33	-0.50	-0.28
	(1.41)	(0.82)	(0.98)	(1.87)	(1.08)	(1.28)	(1.15)	(0.77)	(0.87)
20	-1.61	0.73	0.78	0.90	-1.48	-2.20	0.40	-1.16	-0.27
	(1.64)	(1.12)	(1.40)	(2.00)	(1.35)	(1.69)	(1.63)	(1.14)	(1.32)
25	3.11	0.30	-1.38	-2.52	0.90	4.07	-0.79	0.17	0.41
	(2.52)	(1.69)	(1.94)	(3.03)	(2.05)	(2.34)	(5.08)	(3.34)	(3.58)
30	-0.16	-1.14	-1.10	-3.37	1.16	-0.67	4.16	3.60	3.95
	(3.32)	(2.98)	(3.33)	(4.12)	(3.59)	(4.00)	(5.23)	(5.50)	(5.66)
40	4.80	3.17	5.90	-5.55	-5.88	-5.57	4.41	6.91	5.23
	(3.88)	(3.04)	(3.84)	(3.60)	(2.65)	(3.52)	(5.31)	(3.63)	(5.12)
45	-6.64	-3.98	-6.16	-1.83	-2.06	-4.53	1.16	7.09	1.50
	(3.73)	(3.09)	(3.39)	(3.52)	(3.60)	(3.67)	(6.53)	(5.47)	(6.25)
50	6.42	2.53	7.67	2.19	0.31	-2.96	4.10	9.09	9.22
	(4.34)	(3.35)	(4.26)	(5.60)	(3.99)	(5.47)	(7.99)	(5.93)	(7.71)
55	3.54	1.94	1.45	-1.65	-0.13	-4.39	-14.20	-7.36	-15.32
	(4.79)	(3.72)	(5.84)	(5.92)	(3.91)	(6.31)	(6.45)	(6.19)	(7.67)
window	All	$\pm 25K$	$\pm 10K$	All	$\pm 25K$	$\pm 10K$	All	$\pm 25K$	$\pm 10K$
poly.order	5	1	1	5	1	1	5	1	1
spline		x	x		x	x		x	x

Table 5: Regressions of the effect of proportionality on measures of programaticness

	P1	P5	P1.1	P5.1	P1.2	P5.2
2003						
Size	-2.499***	-1.945***	-2.438***	-1.884**	-2.475***	-1.927***
	(0.496)	(0.562)	(0.562)	(0.695)	(0.525)	(0.580)
Literacy	1.039**	1.327***				
	(0.331)	(0.317)				
Satellite			0.671	0.943**		
			(0.425)	(0.290)		
MobPhones					0.961**	1.300***
					(0.330)	(0.329)
N	22	22	22	22	22	22
2009						
Size	-0.240	-0.325	-0.555	-0.735	-0.041	-0.044
	(0.522)	(0.819)	(0.620)	(0.948)	(0.566)	(0.917)
Literacy	1.162***	1.676***				
	(0.346)	(0.501)				
Satellite			1.242**	1.669*		
			(0.463)	(0.665)		
MobPhones					0.789**	1.072*
					(0.306)	(0.468)
N	24	24	24	24	24	24

P1 and P5 refer to the specification used for the discontinuity regressions. P1 stands for a specification with a linear polynomial with spline and a window of $\pm 15,000$ around the threshold. P5 stands for a specification that uses the full sample, a polynomial of order 5 and no spline.

Figure 1: Cross validation function for 2003 parties



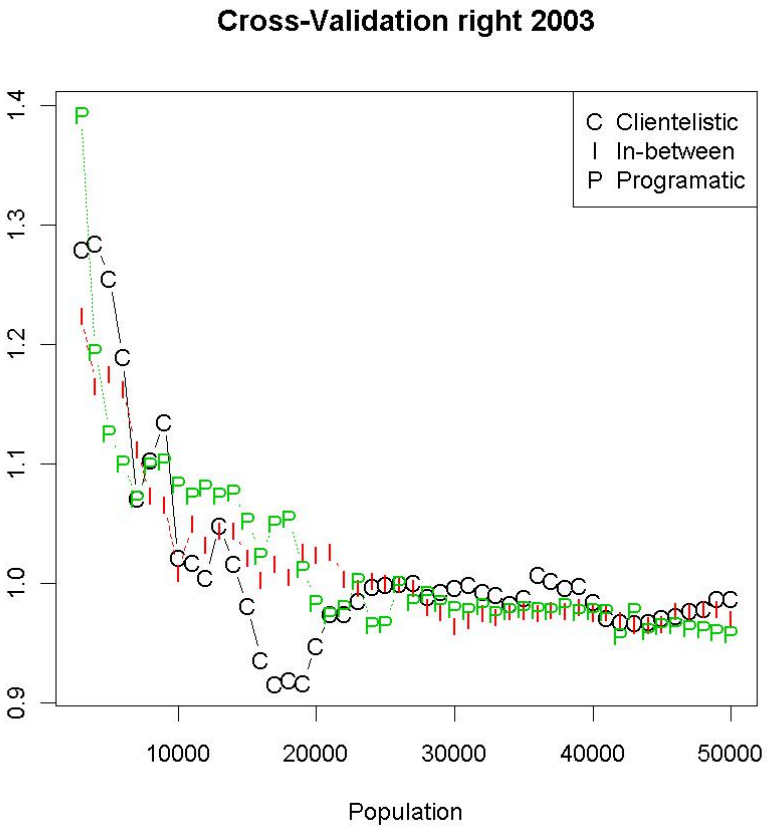
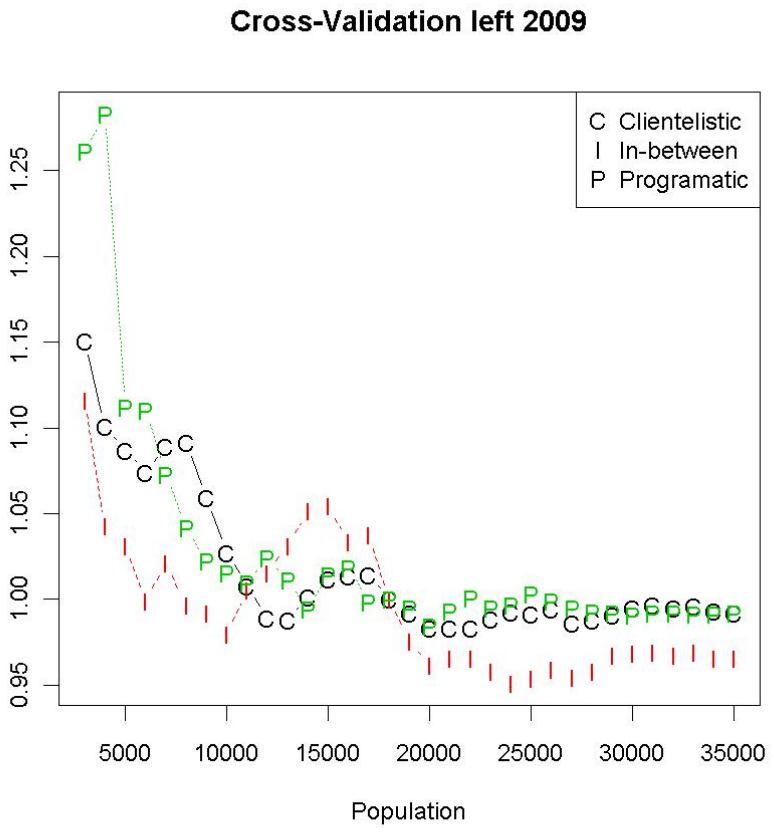


Figure 2: Cross validation function for 2009 parties



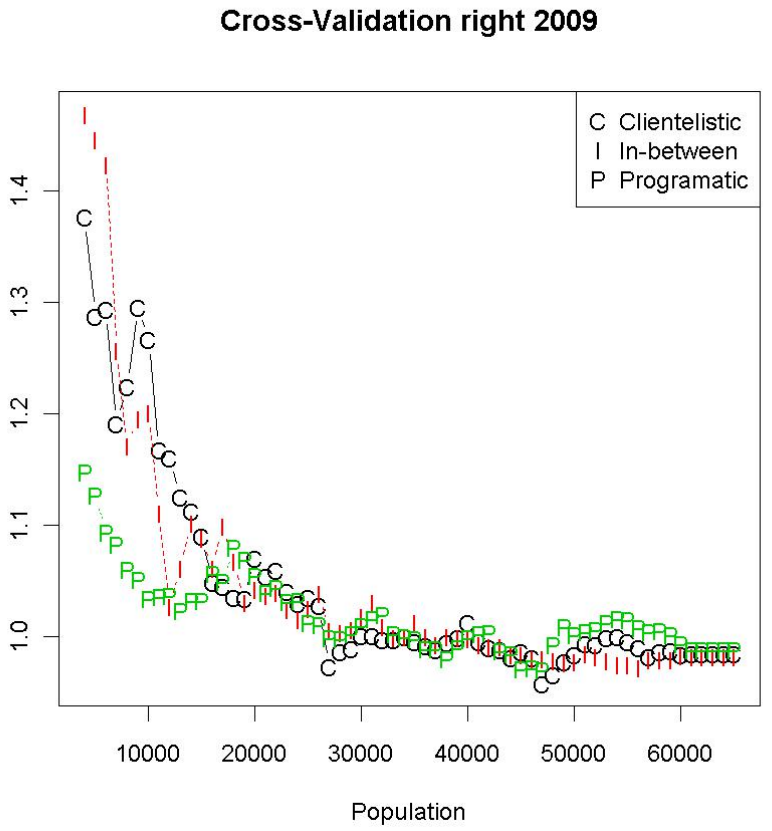


Figure 3: Discontinuity in seats for the significant cases in the placebo test