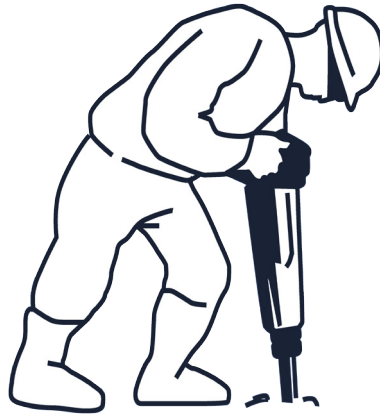


Southern Africa Labour and Development Research Unit



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by

Miquel Pellicer and Eva Wegner

About the Author(s) and Acknowledgments

We would like to thank Herbert Kitschelt for sharing his data on Moroccan parties from the “Political Accountability in Democratic Party Competition and Economic Governance” Project. We would also like to thank the participants of the ECPR (2011) workshop on Voting Experiments, seminar participants at the University of Cape Town and participants of the course Views of Institutional and Behavioural Economics for very helpful comments and suggestions. All remaining errors are ours.

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Electoral Rules and Clientelistic Parties: A Regression Discontinuity Approach*

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December 10, 2011

Abstract

This paper studies the causal effect of electoral systems on the performance of clientelistic vs. programmatic parties. We argue that, contrary to majoritarian systems, proportional systems disfavor clientelistic parties as voters can hardly be pivotal for electing their local patron. We test this insight using data from local elections in Morocco from 2003 and 2009. We use a regression discontinuity approach exploiting the fact that the law stipulates a population threshold below which the system is majoritarian and above which it is proportional. Results show a differential causal effect of proportional systems on programmatic and clientelistic parties: Clientelistic parties halve their seats and the programmatic party doubles them when crossing the threshold of proportionality. An important caveat is that the sample size around the threshold being relatively small, some coefficients are estimated relatively imprecisely. Fixed effects estimates exploiting a change in threshold from 2003 to 2009 yield qualitatively similar results.

1 Introduction

In many developing countries, clientelistic parties are dominant in electoral contests. Such parties have persisted in spite of modernization and democratization processes, both in the form of so-called party machines that rely on brokers to mobilize clients and of more caucus style parties that rely on all sorts of notables for this task (Piattoni 2001, Roniger and Gunes-Ayata 1994). How clientelistic parties operate, gain support and monitor the compliance of their voters has received increasing interest in recent years from political scientists and economists alike (see, for instance, Bardhan et al. 2008, Finan and Schechter forthcoming, Hicken 2011, Manacorda et al. 2011, Kitschelt and

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Wilkinson 2007b, Lindberg and Morrison 2008, Stokes 2005, Vicente and Wantchekon 2009, Wantchekon 2003).¹ The type of linkage these parties establish with voters - where especially poorer voters exchange their say in the political process for some form of particularistic good - is conflicting with the spirit of democratic elections (Stokes 2007a). Additionally, and to some extent as a result of the distortion of democratic representation, clientelism is also associated with bad economic outcomes, such as inequality persistence, the inefficient allocation of public resources, and worse public services for the poor (see Pellicer 2009, Bardhan and Mookerjee 2011, Keefer and Khemani 2005).

In spite of the strong research interest that clientelism has generated in the last decade, there is still relatively little knowledge about its determinants. Low levels of economic development as well as poverty are generally associated with clientelism (Brusco et al 2004, Calvo and Murillo 2004, Kitschelt and Wilkinson 2007a). Poorer voters are thought to give more value to immediate spoils from clientelistic exchanges relative to programmatic benefits in the future and are thus cheaper to buy. Several authors have also argued that robust political competition benefits programmatic parties (e.g. Kitschelt and Wilkinson 2007a, Grzymala-Busse 2007) although this is not uncontested (e.g. Lindberg and Morrison 2008).

Little is known regarding the impact of political institutions, such as electoral rules, on clientelism. Electoral rules are likely to be relevant, since they affect the strategic calculations of parties and voters (see Duverger 1954, Riker 1982, Cox 1997, Taagepera and Shugart 1989, 1993, among many others in a vast literature). Among other things, electoral rules affect the relative electoral value of emphasizing individual characteristics of politicians vs. party platforms/labels. Systems that pit individual candidates against each other, such as majoritarian systems and open list Proportional Representation (PR), provide incentives to cultivate a 'personal vote' as opposed to party labels (Carey and Shugart 1995, Lancaster 1986, Shugart et al. 2005). On the basis of this type of insight, Piattoni (2008) and Kitschelt (2000) articulate the intuition that these types of electoral systems benefit clientelism. However, even if this is plausible, it is difficult to ascertain empirically. The main reason is that electoral rules typically vary only at the cross-country level, and many factors that are difficult to control for could be behind any observed correlation. More generally, as stated by Hicken (2011) in his review article on clientelism, institutional choice is likely to be endogenous to preferences regarding clientelism so that an independent role of electoral institutions is particularly difficult to identify.

In this paper, we study the effect of electoral rules on the success of clientelistic vs. programmatic parties in Morocco in the local elections of 2003 and 2009. Our analysis uses what we believe is a credible identification strategy to estimate the effects of electoral rules. We exploit the fact that for the local elections there is a given population threshold that separates municipalities with majoritarian and proportional systems. This institutional feature allows us to employ a regression discontinuity approach to uncover the causal effect of the electoral system on the success of different parties. In particular, for the 2003 election, towns with less than 25,000 inhabitants had a majoritarian system whereas towns with more than 25,000 had a proportional system. The scheme was maintained for the local elections of 2009, while the population threshold increased to

¹See also the large-scale empirical research project by Herbert Kitschelt and others at Duke University that investigates party-citizen linkages, among others clientelistic ones, around the world. The survey design and some of the findings are reported in Kitschelt 2011, Kitschelt and Freeze 2010, Kitschelt and Kselman 2010, Kitschelt et al. 2009.

35,000. Precisely because of the electoral law, majoritarian towns in Morocco are by default smaller than proportional ones and thus may differ in many respects, such as degree of urbanization, literacy, and so on. Thus simply comparing the performance of a party in majoritarian vs. PR towns would yield results that confound the effect of the electoral system with that of urbanization, literacy, etc. The regression discontinuity approach solves this problem by estimating and comparing the performance of each party in towns at either side of, and very close to, the threshold. The idea is that towns with around 24,000 inhabitants are likely to be very similar to towns with around 26,000 inhabitants, except for the fact that, in 2003, the former had a majoritarian system and the latter had a proportional one. If party performance differs across these types of town, this difference can be quite safely attributed to the electoral system.

We provide a simple argument, complementary to the insight mentioned above, for why majoritarian systems may encourage clientelism. Whereas majoritarian systems allow clients to reward their patrons with their vote directly, proportional closed list systems make it impossible for clients to select “their” patron and thereby reward her for her services. Thus, in proportional closed list systems, voters are more likely to follow their programmatic inclinations. This insight differs slightly from the one suggested in the literature, in that it focuses on the strategic behaviour of *voters* instead of that of *politicians*. The argument actually comes from a local politician in Morocco, whose town had been switched to PR. He complained about PR, saying that in the previous single member districts, he “didn’t even have to campaign” because “everybody knew him” and was “grateful” for the “services he had provided”. Now, he was buried in a list of 25 people for the whole town where people could not select him personally any longer and “could not be sure that he would be elected”.²

To test this insight, we construct two municipal level datasets, one for 2003 and one for 2009, merging the respective election results with population data and some municipal characteristics. The election data contains the number of seats won by each party in each municipality (no information on votes is available). The population data come from the 1994 and 2004 censuses and municipal characteristics come from the 2004 census. It is clear that our 1994 population data is precisely the one that was used to implement the law allocating the electoral system. Thus, for 2003, our regression discontinuity has a “sharp” design. For the 2009 elections, substantial redistricting in 2008 implies that the 2004 municipalities do not correspond exactly to the 2009 ones: a few towns that according to 2004 population data should be majoritarian but are instead proportional and vice versa. This, in turn, leads to a “fuzzy” design in 2009.

We classify the major Moroccan parties into three levels of clientelism: first, fully clientelistic parties, second, parties in-between, and third, programmatic parties. This classification is achieved in a quite straightforward way, by differentiating between parties that were created from within the regime and parties that were created by the opposition. Regime parties have access to power and resources, use these to mobilize voters and are thus classified as clientelistic parties. Opposition parties lack this access to resources and have mobilized voters on the basis of calls for democratic reforms. We have thus classified them as programmatic parties. The in-between group of semi-clientelistic parties consists of parties that were created by the opposition after independence in 1956 but have been co-opted into government since the late 1990s. As a result, they have increasingly adopted clientelistic practices while keeping some characteristics of programmatic parties. Our classification is consistent with data recently compiled by the “Political Accountability”

²Authors’ interview with a municipal councilor in Boujaad, Morocco, 26 November 2008.

project by Herbert Kitschelt and others at Duke University. Using various indicators from their database as well as indices established by Kitschelt and Freeze (2010), our group of clientelistic parties consistently displays higher values on clientelistic indicators followed by the in-between group and followed by the programmatic group. For programmatic indicators, the relationship is exactly reversed.

We estimate the causal effect of electoral rules on the average percent of seats of clientelistic, in-between and programmatic parties. We do so using regressions with different specifications: polynomial of different orders, with/ without splines, and restricting the sample to different windows around the corresponding threshold. We also run specifications that include different sets of controls. For 2009, we follow the fuzzy design by instrumenting the proportional system by an indicator of whether population is higher than the threshold. The instrument proves to be quite strong. All our estimates use robust standard errors.

Our results show that there is indeed a causal effect of proportional systems on the success of different types of parties in line with our hypothesis. In the two years, parties we identify as clientelistic loose, on average, around 5pp. of seats from the proportional system. The “in-between” parties are indeed in between, losing around 2pp. in 2003 and essentially zero in 2009. Our programmatic party, in turn, benefits from a 2pp. increase in seats in 2003 and around 6pp. in 2009. These magnitudes are large in a political system as fragmented as the Moroccan. The proportional system essentially halves the seats of the clientelistic parties and doubles the seats of the programmatic one.

An important caveat is that some coefficients are estimated relatively imprecisely, especially for 2009, where the number of municipalities around the threshold is particularly small. Whereas p-values for the clientelistic parties in 2003 are very low, they hover around 0.1 for the programmatic party in the 2003 elections, as well as for the clientelistic parties in 2009. Coefficients for the programmatic party in 2009 are never significant, even at the 10% level. Two factors, however, lend credence to our results. First, for all parties, coefficients are remarkably similar across specifications. Coefficients change little when using different polynomials, different windows, and different sets of controls. Second, using Seemingly Unrelated Regressions, we test whether the coefficients for the clientelistic and programmatic parties are the same. This test is a more accurate test of our hypothesis than whether each coefficient is different from zero. The hypothesis is clearly rejected for 2003 and displays p-values of around 0.1 (slightly higher or lower depending on the specification) for 2009.

We perform the usual robustness checks to assess the validity of the regression discontinuity design. Threshold manipulation can be ruled out by the very fact that our population data predates the elections for so many years: it seems implausible that the 1994 population figures would have been manipulated with an eye towards the 2003 elections and similarly with the 2004 population data and the 2009 elections. Actually, there may have been manipulation for the 2009 election via the 2008 redistricting, but by using the 2004 population data, our approach should be immune to it. We also find no concerning evidence of other covariates jumping at the threshold or of jumps of our outcome variables at other thresholds.

In addition to regression discontinuity, we use fixed effects as an alternative identification strategy. The change in threshold from 2003 and 2009 together with heterogeneous population growth implies that some towns that were majoritarian in 2003 became proportional in 2009 and vice versa. We can exploit this variation over time to control for municipal fixed effects when attempting to explain party results. This identification

strategy is less credible than the regression discontinuity one, because changes in electoral system are correlated with population growth, and this may bias results (towns that changed from proportional to majoritarian are precisely those whose population stagnated, from above 25,000 in 1994 to below 35,000 in 2009). However, the exercise is valuable in that the identification assumptions are completely unrelated to those of regression discontinuity. The fixed effect results are very similar to the regression discontinuity ones: accounting for fixed municipal effects clientelistic parties suffer a loss of almost 4pp from a proportional system, whereas the programmatic party gains around 2pp. The only difference between the two sets of results is that, in the fixed effects results, the “in-between” parties suffer a loss from proportionality almost as large as the clientelistic ones. All results are statistically significant, even with only 36 municipalities where the electoral system changed.

We discuss our interpretation of the results, assessing whether our results can be driven by factors other than clientelism. Most notably, we focus on differences in the size of parties, a variable known to be relevant for the effect of electoral rules on party performance. We argue that since clientelist and programmatic parties in our sample have similar support around the threshold, party size cannot drive the results. Moreover, we try to tackle the question in a more thorough way by constructing an alternative measure of clientelism/ programmaticness for each party and relating this measure to the causal effect of proportionality on each party. Our alternative measure of programmaticness is the coefficient from regressing election results on demographic characteristics of the municipality, for each party. Since poor and illiterate people are more susceptible to clientelism, clientelistic parties ought to obtain relatively more support in districts with high levels of illiteracy and poverty and vice versa. The coefficient from regressing party votes on literacy reflects precisely how much better a party does in middle class districts than in poor ones and thus provides a measure of how programmatic the party is. While this is by no means a perfect measure of clientelism/ programmaticness, it serves as a useful robustness check as it is a measure unrelated to our qualitative classification. We find that parties that appear more programmatic according to our measure experience a more positive gain from a proportional system. The result holds in a variety of specifications, although it is not always statistically significant.

The paper is organized as follows. Section 2 provides our conceptual framework for establishing a link between electoral rules and the success of clientelistic parties. Section 3 describes the Moroccan electoral law for the local elections of 2003 and 2009 and establishes the classification of Moroccan parties into clientelistic/ programmatic types. Section 4 describes the data and its construction. Section 5 presents our empirical approach. Section 6 shows the results from the regression discontinuity approach as well as the fixed effects estimation. Section 7 provides some validity checks of the regression discontinuity approach. Section 8 discusses the interpretation of the results. Section 9 concludes.

2 Conceptual Framework

We are interested in understanding the effect of electoral rules on the success/ prevalence of clientelism. Electoral rules and in particular district magnitude potentially affects the behavior of parties and voters in a way that may promote or discourage clientelism. The works of Carey and Shugart(1995), Lancaster (1986), and Shugart et al. (2005) point

out that majoritarian systems/low district magnitudes as well as proportional open list systems give incentives to politicians to emphasize a personal vote over party platforms. In electoral systems that pit specific candidates against each other instead of parties, the value of promoting a party label diminishes. This can be interpreted as encouraging clientelism and patronage from the politicians side (Kitschelt 2000, Piattoni 2008).

We argue that electoral rules also can affect the provision of clientelism via strategic considerations of voters. Here, we consider strategic voting as a broad concept whereby citizens take into account what other voters do when casting their vote. In particular, we argue that clientelistic parties, all else being equal, will be more successful the lower the district magnitude (i.e. in majoritarian systems as opposed to proportional (closed list) ones).

Our argument is simple. Consider a polity where there are two parties, a programmatic and a clientelistic one. The clientelistic party is just a collection of local patrons, with no program, and thus no value as a party. Candidates of this party have value as individuals, but only for the inhabitants of their respective neighborhood/ district. If elected, the programmatic party will implement some program which could be thought as providing some public good. The relevant trade off for voters in such a polity is essentially between the local patron and the public goods that the programmatic party could provide. Voters will obviously differ in the value they attach to each of them. Some voters will be core voters, either unconditional to their patron or to the programmatic party. Others will be more opportunistic swing-voters, acknowledging the benefits of the public goods provided by the programmatic party but possibly favoring the clientelistic party if this has a fair chance of having their local patron elected. We believe this setting captures well the political choices faced by citizens in many developing contexts.

In such a setting, electoral systems can have a strong impact on the voting behavior of the opportunists. In majoritarian systems, the choice voters face is directly and unambiguously between the benefits provided by programmatic party and the benefits provided by their local patron. To the extent that opportunists value the benefits of their local patron more than that of the public good provided by the programmatic party, they will vote for the clientelistic party. In closed list proportional systems, instead, their vote can go to the programmatic party for strategic reasons. In proportional systems where the vote is over a list, the order of the candidates is crucial. Opportunists supporting patrons in end-of list neighborhoods will realize that their local patron has little chance of being elected and will therefore rather vote for the programmatic party. Without the support of these opportunists, the chances of patrons positioned just above the bottom of the list decrease, making their opportunistic clients less inclined to vote for the clientelistic party. In this way, a “cascade effect” can be set into motion that may end swelling considerably the votes of the programmatic party.³

Theoretically, a simple way to counteract this drain of the opportunists’ vote under PR would be for clientelistic parties to promise supporters of bottom of the list patrons rewards for their vote even if their patron did not get elected. Rewarding voters would thus become a party, instead of a individual patron’s, business. This path would, however, require stronger organizations than many clientelistic parties appear to have (see Kitschelt

³There is a substantial debate if such clientelistic exchanges need to be sustained by monitoring (e.g. Stokes 2005, Kitschelt and Wilkinson 2007a) or if norms of reciprocity are sufficient (e.g. Finan and Schechter forthcoming). Our approach is compatible with both views and we do have anecdotal evidence that more explicit monitoring (taking a picture of the ballot with a mobile phone) as well as norms of reciprocity (where clients feel a moral obligation) are at play in Morocco.

and Kselman (2011). In practice, and as we will discuss in more detail in the next section, the instability of decisions and political personnel of clientelistic parties in Morocco - and possibly elsewhere - does not enable them to make credible commitments to voters on a party level.

3 Electoral System and Political Parties in Morocco

Morocco's political system combines authoritarian and democratic institutions. The king decides on core policies but the elected government has a say. Multi-party elections have been held since independence in 1956. Until the mid-1990s, outright electoral fraud served to ensure docile parliaments dominated by pro-palace parties; since then, political liberalization measures have included the cleaning of voters' rolls and an increase in the transparency of elections.⁴

Morocco's current electoral legislation is the regime's response to opposition demands for a PR system (Bendourou 2001, Ferrié 2002). Up to the 2000s, both national and municipal elections in Morocco were held under a simple first-past-the-post system which was argued to benefit the monarchy (Lust-Okar and Jamal 2002) and to favour vote buying (Willis 2004). In 2002, the law was changed into closed list PR for parliamentary elections.⁵ For municipal elections, a mixed system was adopted. In the 2003 municipal elections, a single member plurality system was maintained only for towns with less than 25,000 inhabitants. In towns with more than 25,000 inhabitants, councilors were elected under a proportional, closed list, system. Lists needed to obtain at least 3% of the votes locally to gain seats. The minimum district magnitude under PR was 25 seats. For the 2009 elections, the population threshold was increased to 35,000 inhabitants, the minimum threshold of votes to 6%, and the minimum district magnitude under PR to 29 seats.⁶

Political parties are well-established although only few of them are institutionalized (Axtmann 2003, Santucci 2001, Willis 2002). There are frequent party splits, partly encouraged by the monarchy as a means to stay in power, partly as an outcome of leadership disputes. The party system is highly fragmented with a large number of parties competing in elections and winning small amounts of seats.⁷

The Moroccan case has the advantage of allowing for a quite straightforward identification of clientelistic and programmatic parties. In Morocco, the line that divides clientelistic and programmatic parties is a party's origin, that is whether it was created from within the monarchy or from the ranks of the opposition. The country's monarch is considered to be the "ultimate patron" (Willis 2002, p. 15) of a vast clientelistic network the late King Hassan II established to govern the country after independence. This network was politically organized by means of parties created on the initiative of the

⁴Other important components were constitutional reforms that upgraded the prerogatives of representative institutions, an increase in press freedom and the freeing of political prisoners. Generally, Morocco became a much less repressive state. For a history of political development in Morocco and especially the political liberalization process, see Storm 2007.

⁵The maximum mean district magnitudes are very low (five seats, and 3.3 respectively). Santucci (2005, p.5) argues in this context that the reformed voting system looked like a "disguised majority single-ballot system" and thus shows the regime's unwillingness to adopt a genuinely proportional system.

⁶Seats are distributed according to a largest remainder formula that favours small parties, see Gallagher (1992).

⁷In the parliamentary elections of 2007, for instance, 20 parties won seats in the national assembly, out of which four won only one seat, and another five only five seats or less.

monarchy or its allies (Leveau 1976, Lust-Okar 2005, Moore 1993). These clientelistic parties did not invest in a political program as their core appeal to voters - and to politicians joining them - was their access to power structures and the concurrent ability to gain and distribute favors. programmatic parties, in contrast were created by opposition figures with no or little access to this type of resources. Instead, their electoral mobilization and appeal was tied to demands for democratic reforms (Santucci 2001). In short, the key dividing line between clientelistic and programmatic parties in Morocco is akin to Shefter's (1977, p.415) distinction between "internally" and "externally mobilized" parties.

Clientelistic parties and practices in Morocco resemble those in many developing countries. In rural, more isolated areas, parties rely on traditional notables and tribal leaders who provide all sorts of favours throughout the legislative period (emergency relief, payment for funerals, sheep for religious occasions, etc.) (Leveau 1976, Liddell 2010). In towns, notables sought out by clientelist parties can also include other types of locally relevant figures such as trade union leaders or well-connected individuals (Liddell 2010). In larger cities, especially in districts that include shanty towns wealthy businessmen engage either in outright vote-buying via brokers or may even provide club goods such as sewerage on their own account (Catusse 2002, Zaki 2007).

Two characteristics of this type of clientelistic parties are important to note as they address the question why clientelistic parties are unable to credibly promise rewards to opportunist supporters of bottom of the list patrons under PR. First, and most importantly, party organizations are weak and play no or only a small role in selecting candidates and leaders and in legitimizing and enforcing decisions (Willis 2002). Instead, they are collections of the variety of the mentioned notables.⁸ Second, notables - whether rural or urban - are non-partisan. In Morocco, this is shown by ubiquitous floor-crossing, typically when another party offers a better deal (Willis 2002, pp.15-16). The combination of these two factors implies that party decisions and political personnel are unstable. Thus, parties cannot make credible commitments to voters who are unlikely to trust that promises of rewards are enforced if their local patron is not elected.

The most important clientelistic parties in Morocco are the *Mouvement Populaire* (MP) founded in 1958 by Mohand Laenser to organize the support of the rural notables for the regime, the *Rassemblement National des Indépendents* (RNI), founded in 1977 by Ahmed Osman, a brother in law of King Hassan II out of 'candidates without party affiliation' after these had won the largest share of seats in the 1977 parliamentary elections, and the *Union Constitutionnelle* (UC), a split off the RNI in 1983. Since 2007, the *Parti Authenticité et Modernité* (PAM) has been a powerful addition to this group. It was founded by Fouad Ali el Himma, who has a well advertised close friendship with King Mohammed VI (Liddell 2010). Since its creation, the PAM has absorbed many politicians of other clientelistic parties and has gained the largest number of seats in the 2009 municipal elections.⁹ These parties, MP, RNI, UC, and PAM (in the 2009 analysis) form the group of clientelistic parties in our study.

Programmatic parties were founded by opposition figures or movements, the key pro-

⁸Weak organizations as a characteristic of clientelistic parties is not confined to Morocco. Kitschelt and Kselman (2011) find that informal organizations, that "delegate" their voter interaction to notables are particularly associated with clientelistic practices, whereas formal organizations are associated with programmatic linkages.

⁹The PAM did not contest the 2007 parliamentary elections but floor-crossing allowed it to be the largest group in parliament by 2010 (Koehler 2010).

grammatic point being the reform of the monarchy. In the past, the most important of these parties were the *Istiqlal* (Independence) Party (PI), the *Union Socialiste des Forces Populaires* (USFP), and the smaller *Parti du Progrès et du Socialisme* (PPS) that have their roots in the struggle for independence (Moore 1993). Since the late 1990s, all three parties have, however, been continuously co-opted into national government. With the increase in closeness to the power center, they have attracted more opportunistic candidates. They have visibly abandoned former demands for democratization and become less distinctive from the clientelistic parties. In contrast to the latter they do have organizations and some rank-and-file, which, however has become increasingly alienated from the leadership that hangs on to the spoils of national office (Wegner and Pellicer forthcoming). The PI, USFP, and PPS will thus be considered as ambiguous, “in-between” parties.

In the last decade, the most important remaining clear-cut programmatic party has been the Islamist Party of Justice and Development (PJD). Since its creation in the early 1990s it has kept its distance to the monarchy and has up to 2003 never held office, even locally.¹⁰ The party has invested heavily into programmatic linkages regarding both the development of increasingly precise policies and mobilization structures to publicize the platform and make it credible.¹¹ The party organization is an important tool for legitimizing leaders and candidates and for linking with voters. Party officials exert relatively tight control over the MPs and municipal councilors and enforce voting discipline as their reputation is important. They have avoided filling their lists with notables and have a strong preference for party candidates who are elected by the local rank-and-file.¹² The PJD will be the programmatic party in this study.

The classification of key Moroccan parties into clientelistic vs. programmatic parties is based on a historical legacy of political parties in Morocco that makes their belonging in either one of the camps easy to identify. Our confidence in this ranking is further enhanced by its strong correlation with data collected by the “Political Accountability in Democratic Party Competition and Economic Governance” project of Herbert Kitschelt and others. In this project an expert survey (academics and journalists) targeted the type of linkages parties were establishing with voters and how much effort parties were making in establishing these linkages. In Morocco, 13 experts completed the survey. Most of the parties considered in this paper were included, the exceptions being the PPS, the UC, and the PAM.¹³

Table 1 exhibits the mean scores for our “programmatic”, “in-between”, and “clientelistic” groups according to the Political Accountability project findings, divided in programmatic and clientelistic linkages. For all the indicators, higher scores imply more programmatic/ clientelistic linkages, respectively. The scores for programmatic and clientelistic effort are the answers to two general questions asking directly about the extent to which parties seek to mobilize electoral support by emphasizing the attractiveness of the party’s positions on policy issues (programmatic effort) or the capacity of the party to deliver targeted material benefits (clientelistic effort). As shown in Table 1, the programmatic score is highest for the party we labeled as programmatic, followed by the ambiguous “in-between” group and the clientelistic group, and vice-versa for the clien-

¹⁰After the 2003 municipal elections, the PJD governed 13 towns, all in coalitions.

¹¹For a detailed study of the Party of Justice and Development, see Wegner 2011.

¹²See Wegner 2011, chapter 3 on party organization.

¹³The survey questionnaire and information about the project is available at <http://www.duke.edu/web/democracy>.

telistic effort score.

[Table 1 around here]

Regarding programmatic linkages, Kitschelt and Freeze (2010) have established two indices that measure issue cohesion, salience and distinctiveness, cosalpo3econ and cosalpo4 . For clientelistic linkages, the indicator is an additive index of answers regarding how much efforts parties make in providing different types of particularistic goods to voters, such as gifts and access to social policy entitlements. The scores of our parties for these indices show the same pattern as the ones for the general questions: the PJD consistently gets the highest score on programmatic linkages and the lowest on clientelistic linkages and the opposite is true for the RNI and MP. In all cases, the ambiguous parties (Istiqlal and USFP) lie indeed in-between the other two groups.

4 Data and descriptive statistics

Our aim is to understand the effect of the electoral system on the success of different types of parties. The main identification strategy we use is a regression discontinuity approach, using the fact that the electoral system in Moroccan local elections is designed to change from majoritarian to proportional at a given population threshold. Our approach thus requires a dataset that contains, at the municipal level: 1. electoral results, 2. the population, (hopefully as used by the administration to assign the electoral system) and, 3. some average demographics to serve as control and for validity checks.

We construct two datasets, one for 2003 and one for 2009. The details of the construction are in the Data Appendix. The 2003 dataset contains the 2003 election results from the ministry of interior, population from the 1994 census and demographic variables from the 2004 census. The 2009 data contains the 2009 election results merged with population and demographic variables from the 2004 census. Moreover, we merge the two datasets to obtain a panel structure that we exploit later in the analysis. Overall, the mergers are quite successful, and only around 2% of observations are lost. Electoral results have information only on seats, not on votes. Thus, we measure party success as the proportion of seats won by the party in each municipality. Municipal characteristics from the census data include, in addition to population, variables such as literacy rates, percent of mobile phones, rates of public and private employment, age structure, etc.

For 2003 we feel confident that the 1994 population data we use is precisely the one used by the administration to implement the electoral law. Thus, for 2003 we use a sharp regression discontinuity design. For 2009, in contrast, the mapping between the electoral system and our 2004 population variable is not sharp. This is likely to be because there was a substantial redistricting in 2008, just before the 2009 elections. Since we have no information on the specifics of the redistricting, we are forced to use the 2004 population data as the forcing variable in the regression discontinuity analysis. This turns out to have both a disadvantage and an advantage. The disadvantage is that this may introduce additional noise in our estimation, as the population measure used does not correspond to the true population at election time. The advantage is that, if the redistricting happened for political reasons, using the “true population” in 2009 could threaten our identification strategy: potentially, governing parties would put pressure to have their population changed to end up at the side of the threshold more beneficial

to them; municipalities at different sides of the threshold *on the basis of the 2009 true population data* would end up being different. The 2004 population data, which pre-dates the process, avoids this problem.¹⁴ We will return to this issue when considering the validity of our regression discontinuity approach.

Our empirical approach relies heavily on the municipalities around the threshold of proportionality (25,000 in 2003 and 35,000 in 2009). Thus, we restrict ourselves to municipalities below 100,000 inhabitants. After merging and cutting these large municipalities we end up with sample sizes of 1471 for 2003 and 1449 for 2009. Figure 1 shows the distribution of the population across municipalities. The figure shows that most of the Moroccan municipalities are small, way below the corresponding thresholds, particularly for 2009. For 2003, there are 95 municipalities in a $\pm 5,000$ window around the threshold and 266 within a $\pm 10,000$ window. For 2009, the figures are only 36 and 92, respectively.

(Figure 1 around here)

Table 2 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 2011 forthcoming): 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 parties we consider. 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 2 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 comparatively low, with only 42% on average. 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 votes (a total of 66% of the seats on average in 2003 and 90% in 2009).

The table also shows differences between proportional (large) municipalities and majoritarian (small) ones. The two types of municipality differ significantly along all dimensions: Smaller towns with majoritarian systems display lower literacy rates, more poverty as measured by ownership of mobile phones and a lower percent of public employees. They also differ substantially in electoral results: The parties we label as clientelistic tend to perform better in smaller villages with majoritarian systems whereas the programmatic party performs substantially better in larger towns with a proportional system. These results could be taken as first (naive) evidence that clientelistic parties do indeed benefit from a majoritarian system, as we hypothesize. However, the very fact that majoritarian

¹⁴Actually, we do have casual evidence that the redistricting was indeed politically motivated. The town of Larache, where we have conducted extensive fieldwork, and where the opposition had been gaining support, saw the annexation of two small villages for the 2009 elections, both of which predominantly are supportive of clientelistic parties.

municipalities are also smaller cautions against such interpretation at this stage. majoritarian municipalities, as just shown, tend to be poorer and more illiterate and it may well be that it is precisely because of this (and not because of the electoral system) that clientelistic parties succeed in these municipalities. In order to obtain credible evidence on the causal effect of the electoral system, we exploit the discontinuity at the relevant population threshold using a regression discontinuity approach. We explain in more detail the approach in the next section.

5 Empirical Approach

To identify the causal effect of electoral rules on the success of different types of party we rely mainly on a regression discontinuity approach (RD), exploiting the discontinuity of the electoral system at a particular population threshold. A growing literature in economics and political science uses this approach to uncover the causal effect of electoral rules on different types of variables (see Gagliarducci et al. 2011, Eggers 2010 and Petterson-Lidbom and Tyrefors 2007). To the extent that other potentially relevant variables are continuous at the respective threshold, the jump in seat shares at the threshold represents the causal effect of the electoral system on the performance of the different parties (See Imbens and Lemieux (2008) and Angrist and Pischke (2009) for details on regression discontinuity designs).

For the reasons explained in the previous section, for 2003 we use a “sharp” design, considering that 1994 population perfectly determines the electoral system. We estimate the following model, using simple OLS with robust standard errors:

$$y_i^j = \beta_0 + f(p_i) + \gamma T_i + \beta_1 C_i + \beta_2 X_i + \varepsilon_i \quad (1)$$

$$T_i = \begin{cases} 1 & \text{if } p_i > \tilde{p} \\ 0 & \text{if } p_i \leq \tilde{p} \end{cases}$$

where i indexes municipalities and j indexes type of party (clientelistic, programmatic and in-between). Thus, y_i^j is the average share of seats in municipality i of the parties we have included in category j . $f(p_i)$ is some possibly non-linear but continuous function of population that captures the effect of population on the success of different types of parties. We allow for different polynomial specifications of $f(p_i)$ with and without spline. T_i is an indicator function that measures treatment status, whether the electoral system is proportional as opposed to majoritarian, as given by the population threshold \tilde{p} , which in 2003 equals 25,000. C_i is the number of councillors in municipality i . Some specifications will include X_i as a vector of controls that includes a variety of demographics of the municipality or a full set of provincial dummies. Finally, ε_i is an error term.

The key identification assumption of the RD approach is that $f(p_i)$ is continuous around \tilde{p} : i.e. conditional on the number of councillors (and other controls if applicable), nothing potentially relevant jumps at the threshold. In that case, γ captures the jump in the dependent variable at the threshold \tilde{p} over and above the smooth change captured by $f(p_i)$. Thus, γ is the local causal effect of interest, where local refers to the type of municipality with population around the threshold. Intuitively, if nothing relevant jumps at the threshold and we observe a jump in our dependent variable, this jump ought to be caused by the change in treatment status at the threshold.

In our case, the number of councillors *does jump* at the threshold \tilde{p} . However, since it jumps at many other thresholds (7,500, 12,500, 15,000 and 50,000), its effect can be netted out from our treatment effect of interest by conditioning. Moreover, the jump in number of councillors at \tilde{p} is small, from 23 to 25 (as opposed to, for instance, from 15 to 23 at the 15,000 threshold) so that it is highly unlikely that it would drive the results we obtain. Besides the jump in the number of councillors, there is no reason to expect any relevant variable to have a discontinuity at the given thresholds. We tackle this issue below with a variety of validity checks.

From an inference point of view, our key limitation is the small sample size around the threshold. This implies that coefficients are often not precisely estimated. To partially address this, we use several specifications for $f(p_i)$, estimating a variety of polynomial models using different windows around the discontinuity point and controlling for several sets of variables. While this does not make estimates more precise it allows to verify whether the coefficient of interest is stable under different 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 (Imbens and Lemieux 2008).¹⁵ 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 is that the right and left side of the thresholds differ substantially (ex. in sample size). Figure 2 shows the Cross-Validation value for the right and left side of the threshold for the different types of parties.¹⁶ 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 2 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 2) 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). In addition, to check the robustness of the results to observations close to the threshold, we will also use a linear spline with a window of 5,000.¹⁷ Finally, the linear spline with 15,000 window will be augmented with a variety

¹⁵The 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.

¹⁶The Cross Validation value has been normalized in order to make comparisons across types of parties easier.

¹⁷In the specification with the 5,000 window, we cannot control for number of councillors as this variable becomes collinear with the treatment.

of controls.¹⁸

For 2009, due to imperfect compliance, it is necessary to use fuzzy RD. This amounts to estimating equation 1, where the treatment T_i is now to have a proportional as opposed to a majoritarian system. This treatment is then instrumented by the assignment variable Z_i , which is simply the indicator function for municipalities of population higher than the relevant 2009 threshold \tilde{p} . We estimate the models by TSLS, using robust standard errors. The first stage is of the form:

$$T_i = \alpha_0 + g(p_i) + \rho Z_i + \alpha_1 C_i + \alpha_2 X_i + \xi_i \quad (2)$$

$$Z_i = \begin{cases} 1 & \text{if } p_i > \tilde{p} \\ 0 & \text{if } p_i \leq \tilde{p} \end{cases}$$

For 2009 we run the similar specifications as for 2003, with and without interactions, and with full as well as restricted samples. In the specifications with interactions, all polynomial terms multiplying the treatment variable in the second stage are instrumented, with first stages analogous to (2) but including all polynomial terms interacted with Z_i .

In order to choose our specifications for 2009 we once again use the Cross Validation criterion. Figure 3 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.

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. We will also use a linear spline with window 10,000 and augment the 25,000 linear spline with demographic and provincial controls.¹⁹ In addition, we will report models that control as well for the percent seats won in 2003 and for the electoral system in 2003 in the corresponding municipality.

When using instrument variables, it is important to assess the strength of the instruments, as the instrumental variable estimator may be problematic in small samples when the instruments are weak. In our case, the instruments are quite strong. In particular, only two municipalities below the threshold are in reality proportional while 8 of those above the threshold are in reality majoritarian.²⁰ The results of the relevant first stages using our preferred specifications are shown in Table 3. The coefficient for the assignment variable is close to 0.8 in the two specifications, implying that the reduced form coefficients will be around four fifths the value of the IV values. The F statistic for the significance of the excluded instruments is sufficiently large, with values way above the “rule of thumb” value of 10 suggested by Stock, Wright and Yogo (2002).

(Table 3 around here)

¹⁸In the models with spline, we center the population variable around \tilde{p} in order for the OLS coefficient estimates to yield the jump at the discontinuity point.

¹⁹As for 2003, the specification with the 10,000 window cannot control for number of councillors.

²⁰This implies the extent of non-compliance is 4 out of 36 in a $\pm 5,000$ window and 7 out of 92 in a $\pm 10,000$ window.

In addition to regression discontinuity, we use a second unrelated identification strategy: to control for municipal fixed effects. The change in population threshold from 2003 to 2009 together with population growth implies that the electoral system of some municipalities changed from 2003 to 2009. Municipalities with population less than 25,000 in 2003 but more than 35,000 in 2009 would have shifted from majoritarian to proportional while those that had population higher than 25,000 in 2003 but lower than 35,000 in 2009 would have shifted from proportional to majoritarian. This allows us to control for municipality fixed effects when estimating the effect of the electoral system on the performance of the different types of party. We estimate the following equation:

$$y_{it}^j = \alpha_i + \lambda_t + \gamma T_{it} + \varepsilon_{it} \quad (3)$$

where α_i are municipality fixed effects, λ_t are time fixed effects, T_{it} is the treatment (proportional system) and ε_{it} is the error term. The parameter of interest is γ .

This model exploits the time variation in treatment status to estimate the treatment effect. The key identification assumption is that changes in treatment status are not correlated with the error term, i.e. with the implicit support for the given type of party. It is not clear whether this assumption holds in practice or not. There are two types of potential violations. First, changes in treatment status, by the very structure of the data, are related to population growth. Stagnating towns are more likely to become majoritarian whereas fast growing towns are likely to become proportional. The features of a municipality that makes its population grow may be linked to the success of different types of party, hence generating bias in our estimate. Another potential source of bias is gerrymandering. Some (selected) municipalities may have been subject to redistricting precisely in order to prevent them from becoming proportional. There is nothing we can do to address the first type of bias. For the second type we will consider the “intention to treat” effect. We will rerun the analysis replacing the treatment variable T_{it} by assignment-to-treatment variable (the indicator of whether population is higher than the corresponding threshold). To the extent that our population data has not been manipulated for the elections, this approach will deal with the bias potentially induced by gerrymandering.

Because we believe that the fixed effects identification strategy is less convincing than the one behind the RD, we view the fixed effects results primarily as robustness checks to the RD results. What is important is that the identification assumptions of the fixed effects model are completely different to those of the RD. Thus, we believe that results are considerably strengthened to the extent that the two sets of estimates agree.

6 Results

6.1 Regression Discontinuity Results

We consider first the results for 2003. Following the custom in studies using regression discontinuity (RD) designs, we present first graphical evidence of the relation between population and the average seats of the different types of parties around the threshold where the electoral system changes. We provide two types of figures, corresponding to our two preferred specifications (one with the full sample and 5 degree polynomial with no spline and the other with a linear spline and a 15,000 window around the threshold), presented in Figures 4 and 5. The dots in the figures show the average seats of the

respective type of party at intervals of 2,500 population windows (1,250 for the figures with the restricted sample). These are arguably the key figures in the paper. They show, for 2003, a strong and clear negative jump at the threshold for the clientelistic parties; a negative yet smaller and less clear jump for the parties “in-between”; and a strong and clear positive jump for the programmatic party.

(Figure 4 around here)

(Figure 5 around here)

The sizes of the effects are important. Clientelistic parties essentially halve their share of seats upon crossing the threshold of proportionality, from an average of around 10% to around 5%. The programmatic party moves up from a very small percentage (around 2%) to almost 5%.

Table 4 provides the estimates of the models specified above for 2003. All numbers refer to the estimate of γ , the treatment effect of interest, for the three different types of party under the different specifications. Columns 1, 2 and 3 show the specifications with different polynomials and windows around the threshold. The rest of the columns add different sets of controls to the linear specification with 15,000 window. The coefficients are roughly stable. For the clientelistic parties, the numbers are always negative, with values from -6 to -4. For the programmatic party, the numbers are always positive, with values always around 2. For the parties “in between”, the coefficients are indeed in between, negative but typically less so than the clientelistic ones, with values between -3 and -1.

(Table 4 around here)

For the clientelistic parties, the standard errors are reasonably low compared to the coefficient estimates: under most specifications, the coefficients are significant at a 1% level. For the programmatic party, p-values hover around 0.1, so that coefficients are sometimes significant at a 10% level and sometimes not. However, and importantly, for both programmatic and clientelistic parties, the size of the coefficients remain similar even when samples are restricted to observations very close to the threshold (5,000 window). Also noticeable is the fact that coefficients barely change even when adding a set of around 30 demographic controls (such as literacy, occupational and age structure, etc.), or the full set of 59 provincial dummies (columns 4 and 5).

For 2009, the discontinuities are shown again using the full sample (Figure 6) and the restricted, 25,000 window sample (Figure 7). Notice, also that since there is no perfect compliance in 2009, the pictures refer to the “reduced form” where the dependent variable is plotted directly against the instrument. On the basis of the estimates of the first stage provided above, the jumps observed in the pictures will be around four fifths of the true effect.

(Figure 6 around here)

(Figure 7 around here)

Qualitatively, however, the pictures are useful and the message they deliver is the same as for 2003: clientelistic parties lose and the programmatic party gains upon crossing the proportionality threshold; parties “in between” experience essentially no effect. An important difference with 2003 is the higher variability in the “dots”, particularly around the threshold. This is probably mainly due to the smaller sample sizes in 2009. The

measurement error of the population variable induced by the 2008 redistricting might compound the problem. As we will presently see, this is reflected in less precise estimates below.

Table 5 shows the coefficient estimates of our different 2009 specifications (different polynomials and windows and different sets of controls). All numbers refer to the IV estimate of the effect of having a proportional system for each type of party in each specification. The estimates are relatively similar to those in 2003. Estimates range from -3 to -6 for the clientelistic parties. For the programmatic party they are somewhat larger and more variable, from 4 to 9. Notice, however, that the programmatic party obtained better results in 2009 so that, in relative terms, the coefficients reflect similar effects (i.e. the programmatic party doubles its share of seats upon crossing the threshold). For the parties "in between" coefficients are around zero, although they turn rather negative in the specifications with controls.

(Table 5 around here)

While the coefficients are similar to those in 2003, the standard errors are considerably larger, making the estimates imprecise and rarely statistically significant. P-values for the clientelistic parties hover around 0.1, so that coefficients are sometimes significant at the 10% level and sometimes not. The coefficients for the programmatic party, in turn, are never significant, even at a 10% level. Again, the fact that coefficients remain quite stable across specifications is remarkable. For 2009, coefficients are not only robust to a small window (10,000), to demographic variables, and to provincial dummies. They are also robust to including as control past results of the corresponding type of party in the given municipality (column 6). This important variable is unsurprisingly always very significant in the regressions. We believe that the fact that coefficients are robust to the introduction of this control (i.e. that even accounting for past results that the parties had in 2003, results in 2009 jump upon crossing the threshold in the expected ways) gives a degree of confidence in our estimates. Moreover, coefficients remain robust also to the inclusion of the 2003 electoral system as control (column 7). This implies that the coefficients we observe for 2009 are not just a noisy reflection of the 2003 effects.

In order to probe further the statistical significance of our results, we perform tests of equality of coefficients estimating the equations for different types of party as a system of Seemingly Unrelated Regressions (SUR). Our hypothesis holds that clientelistic parties ought to suffer from a proportional system relative to programmatic ones. Thus, strictly speaking, our hypothesis is *not* that effects should be different from zero for the two parties. Rather, the hypothesis is that the coefficient for clientelistic parties should be smaller than that for the programmatic one. To ascertain whether the coefficients for the different types of party are different, we estimate our equations as a SUR system, which allows for the (realistic) possibility that unexplained electoral results across types of party are correlated (good results of a party will typically imply worse results for the others). We estimate equations using our two preferred specifications, both with robust standard errors. The test results clearly reject that coefficients for the clientelistic and programmatic parties are the same in 2003 (p-values less than 0.001). For 2009, rejections are less clear although arguably sufficiently so: the linear spline with 25,000 window yields a p-value of 0.108 whereas the full sample specification yields a p-value of 0.068. Besides these, In-between parties cannot be distinguished from the other parties in 2009, although they do appear different than the programmatic in 2003 (p-values less than 0.021).

6.2 Fixed Effects Results

The results of the fixed effects estimation of equation 3 are presented in Table 6. Column 1 shows the coefficient of having a proportional system, controlling for municipal and time fixed effects, for each type of party. The figures are broadly consistent with the RD results: Clientelistic parties *lose* 3.7 seats upon becoming proportional, whereas the programmatic party *gains* 2.4 percent. The “In between” parties, are still in between, although with a negative coefficient close to that of the clientelistic parties. Notice that all coefficients are statistically significant (at the 10% level), even with only 36 observations where the electoral system changes.

(Table 6 around here)

As mentioned above, these estimates may be biased. Column 2 presents the “intention-to-treat” effects that deals with the potential gerrymandering bias, where the indicator of proportionality is replaced by the indicator of whether the municipality has population higher than the corresponding threshold. The results are virtually the same as those in column 1: electoral manipulation does not appear to be driving the results.

These results can be interpreted as the effect of a *change* in electoral system on results of different parties. In the regressions, moves from majoritarian to proportional are treated symmetrically as those from proportional to majoritarian. Column 3 relaxes this assumption and allows these effects to differ. These are the results from regressing the change in seats from 2003 to 2009, on two dummy variables: one being an indicator of whether the municipality was majoritarian and became proportional and the other if it was proportional and became majoritarian. There are reasons why the the two effects might no be a mirror of each other. Prior to 2003, the electoral system was majoritarian in all municipalities. The municipalities that became proportional in 2009 experienced their first change in electoral system, whereas those that became majoritarian in 2009 experienced their second change (from majoritarian to proportional and back). To the extent that parties learn and adapt their strategies over time, one would expect to see less effects the second time around, when the electoral system becoming majoritarian again. The absolute value differences in the coefficients can give indication of the persistence of our hypothesized effects. Still, it needs to be emphasized that these results are merely suggestive, both because of the potential biases mentioned above and because of the small sample sizes involved.

With this caveat in mind, results in column 3 show that (re-)becoming majoritarian tends to yield weaker effects than becoming proportional, suggesting some degree of adaptation. However, there is also some evidence that adaptation is not complete: clientelistic parties, for instance, loose upon becoming proportional for the first time, but then recover most of the gains when re-becoming majoritarian. Interestingly, the difference in absolute values of the coefficients is strongest for the programmatic party: whereas becoming proportional for the first time entails a substantial increase in seats, reverting to majoritarian implies a much smaller (and statistically insignificant) loss. This is consistent with a situation where programmatic parties may turn to clientelism upon gaining access to power and resources: the change to a proportional system enables programmatic parties to obtain more power, but once in power, the party engages in clientelistic practices that make it not loose support when the system reverts to majoritarian.

Overall, even if the fixed effects results need to be taken with caution, they do provide evidence in support of our hypothesis in line with, and in addition to, the RD results presented above. Moreover, they suggest that the effects of electoral rules do not disappear

after the first election with a different system. It appears that especially the clientelistic parties find it difficult to adapt successfully to a proportional system. The apparent inability to adapt is thus consistent with our contention that clientelistic parties in Morocco do not manage to coordinate successfully the provision of particularistic goods.

7 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. Still, the apparent absence of manipulation can be verified in the histograms of Figure 1 mentioned above that show the distribution of population across municipalities. If there was manipulation one would expect “bunching” of density at some side of the threshold.²¹ The figures show no evidence of such “bunching”.

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 7 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 coefficient for the discontinuity in tables 4 and 5.

(Table 7 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 behaviour of the variable. In our case, there is one mechanism that could generate discontinuities at different thresholds: 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

²¹Presumably, one would expect bunching on the left side of the threshold, as municipalities with access to power at the national level (i.e. governed by clientelistic parties) would have attempted to remain majoritarian.

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.²² We use thresholds every 5,000 inhabitants. Tables 8 and 9 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. Several of the exceptions 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 8 around here)

(Table 9 around here)

In order to assess how concerning these cases are, we report the graph of the discontinuity for each case in Figure 8. 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 behaviour than genuine jumps.

(Figure 8 around here)

8 Discussion: Is it really clientelism?

Our empirical approach allows us to claim to have identified the causal effect of electoral rules on the share of seats obtained by a party or a set of parties. Even if the effects are not always statistically significant, we believe the combination of all pieces of evidence we provide make our estimates reasonably credible. Thus we feel quite confident that there is a differential effect of electoral rules on different types of party. Moreover, on the basis of criteria relating to the historical legacy of Moroccan political parties (and supported by Political Accountability project indicators), we can claim that our party groupings do differ in terms of clientelism. We thus interpret our results as saying that, according to our data, majoritarian systems (as opposed to proportional ones) benefit clientelistic parties and harm programmatic ones.

²²In several specifications, the number of councilors is not included because of collinearity.

However, even if our estimates are credible and if the grouping of parties we make maps well into the clientelistic/programmatic dimension, it is important to notice that there could potentially be other interpretations for our results. Most notably, the already mentioned literature on electoral rules and number of parties suggests that small parties suffer from majoritarian systems. Our results could be driven by this. More generally, our party grouping could correlate with other factors that for some reason generate heterogeneous treatment effects of the type we observe.

There are several reasons why we believe our estimates do capture differences in the clientelistic/ programmatic dimension. First, we consider three categories of parties, not only two, that we order from low to high level of clientelism; and the results follow the same ordering as expected for the two years. Moreover, these parties do not exhaust the vote share, so that what happens to one is not automatically offset by the others.

Second, size does not seem to drive the results. Pellicer and Wegner 2011 verify that while in 2003 there is indeed an important causal effect of the electoral system on the number of parties (fewer parties in the majoritarian system), for 2009, this effect is absent due to the high electoral threshold in the proportional system (6%). Size thus should not be driving results in 2009. More importantly, the grouping of parties we use does not correlate with size: The clientelistic and the programmatic parties actually have a relatively similar degree of support in both years around the threshold (around 5% in 2003 and 10% in 2009 on the proportional side of the threshold, see figures 4 and 6). Thus, the fact that clientelistic parties benefit when the system becomes majoritarian and the programmatic party suffers, cannot be attributed to the clientelistic parties being bigger.

However, it would 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 clientelism 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, clientelism 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.

To implement this approach, we need an estimate of the effect of illiteracy/ poverty on the results of each party, and an estimate of the causal effect of proportionality for each party. The latter is estimated as usual, using our municipal data and estimating our two preferred specifications for 2003 and 2009. The effects of illiteracy and poverty on party results are estimated using results from 2002 and 2007 national elections. Using a different dataset to estimate the effect of proportionality and the effect of illiteracy/ poverty 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 (forthcoming) for an elaboration of this point and for a description of the data).²³ We thus run a regression

²³Information on turnout in our context is relevant because clientelistic strategies often take the form of encouraging turnout (Nichter 2008). A party that obtains a few votes in a low turnout middle class district and many votes in a high turnout slum could appear (misleadingly) more successful in the middle

for each party, of the percent of votes obtained by the party on the percent of illiteracy/ poverty in the circumscription. This coefficient is then our measure of clientelism of the party. In practice, we run several models, using as regressors literacy rates, percent of satellite dishes, and percent of mobile phones in the circumscription.²⁴ These are indicators of wealth, so that the coefficients we obtain are rather a measure of how programmatic the party is.²⁵ We then regress the estimated causal effect of proportionality for each party on this measure of programmaticness. The 2003 (2009) estimate of the effect of proportionality is regressed on the 2002 (2007) measure of programmaticness. Our hypothesis states that the coefficient of interest in the latter regression should be positive (programmatic parties ought to gain from a proportional system). In addition to this, since theory strongly suggests that size of the party is likely to be relevant for explaining whether a party benefits from a proportional system, we include party size in the regression as well. This variable is measured as the average percent of votes obtained in the relevant national election.

Due to the uncertainty surrounding our estimated measures of clientelism/ programmaticness, we view the results that follow as merely suggestive and do not place much emphasis on inference. Rather, we run a variety of models to estimate our measure of programmaticness in different ways: Using different proxies for wealth, erasing clear outliers and using alternative datasets to check if results remain qualitatively similar. Our benchmark specification uses literacy as proxy for wealth, although we also try as proxies the percent of satellite dishes and the percent of mobile phones. Additionally, upon examination of the data, it becomes clear that the estimated degree of programmaticness is much higher for the PJD than for any other party. This is in accordance to our qualitative evidence, but precisely for that reason, risks undermining the purpose of the exercise, which is to use an alternative indicator of clientelism. Thus we rerun the analysis dropping the PJD. Finally, to make sure that the results are not driven by the use of national data, we rerun the analysis using the municipal data. We estimate regressions such as those in equation 1, using literacy as control, and use the coefficient for literacy as the indicator for programmaticness. In these regressions, we also compute the size of the party as the percent of seats at the threshold on the proportional side. Regarding the estimation of the effect of proportionality on each party, these are estimated in the usual manner, using our two preferred polynomial models (polynomial 1 with restricted sample and polynomial 5 with full sample).

The results of all these models are presented in Table 10. 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 the sensitivity of party results to a district’s literacy. 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, as measured by how much better they fare in literate districts, benefit more from a proportional system. The coefficient for the party size has the expected behaviour. In 2003,

class district if support is measured as percent of seats instead of as votes out of registered voters.

²⁴Notice that we don’t 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.

²⁵In order make interpretation easier, we standardize these coefficients, by subtracting the mean value of the coefficient across parties and dividing by its standard deviation.

it is strongly negative and significant: as theory would predict, bigger parties loose 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 10 around here)

All these results are remarkably robust across specifications. Focusing on our coefficient of interest, for the two years, coefficients are similar when we use satellite dishes or mobile phones instead of literacy as proxies for wealth (Columns 3 to 6). Even when we drop the PJD, results remain similar, although significance levels drop (Columns 7 and 8). When we use the municipal data instead of national one to estimate our measure of programmaticness, the size of the coefficient drops and becomes insignificant; still, results remain qualitatively similar (Columns 9 and 10).

9 Concluding Remarks

In this paper we argue that electoral rules matter for the success of clientelistic parties. We hypothesize that, when clientelistic parties are just collections of local “notables”, these parties are more successful in majoritarian systems than in proportional ones. In proportional systems with higher district magnitudes, voters are hardly pivotal in electing their local patron and so they have more incentives to vote for the programmatic ones.

We tackle the question empirically using two local elections in Morocco, in 2003 and 2009. Morocco is particularly suited for studying the topic for two reasons. First, because its peculiar party history makes it very straightforward to distinguish clientelistic parties from programmatic ones. Second, and more importantly, because its electoral system is based on a population threshold that can be exploited to uncover causal effects, using a regression discontinuity design.

We find that electoral systems have a strong effect on the success of different parties. Clientelistic parties loose an average of around 4-5pp of seats upon crossing the proportionality threshold in both years. This effects are sizable in the Moroccan context, corresponding to the clientelist parties halving their seat share. We believe such differences can have strong implications for governance and thus for the evolution of inequality and the development paths of different localities. To obtain evidence of these implications is left for further research. The analysis of consequences for governance needs to await a political climate were “sensitive” information such as municipal budgets become available. The analysis of socioeconomic implications needs to await the next census in 2014. Still, we believe that the present paper provides a relevant piece for understanding the causal determinants of clientelism in particular and voting behavior in developing countries more generally.

Data Appendix

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.²⁶ 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.²⁷ 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.²⁸ 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 councillors. The Official Bulletin 5096 from 03/04/2003 published the official mapping between population and number of councillors in the 2003 elections (for ex. municipalities with population between 15,000 and 25,000 ought to have 23 councillors).

²⁶<http://www.elections2003.ma> for 2003 and <http://www.elections2009.gov.ma> for 2009.

²⁷The data can be found at <http://www.hcp.ma/>.

²⁸A few municipalities are dropped in the merger due to the difference between administrative and electoral local units. These correspond to neighbourhoods 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.²⁹ 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).

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²⁹Only 9 out of the 1,491 do not match, out of which 5 are “Mechouars”, neighbourhoods with special status for containing a royal palace (we erase those from the analysis in both election years)

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Table 1: Indicators of clientelism and programmaticness, by type of party

		Programmatic	In.Between	Clientelistic
		PJD	USFP Istiqlal	RNI MP
Programmatic effort		3.6	3	2.3
Programmatic indices	cosalpo3econ	0.16	0.13	0.07
	cosalpo4	0.13	0.11	0.1
Clientelistic effort		2.7	3.1	3.5
Clientelism index		12	13.6	14.7

Source: Moroccan parties data from 'Political Accountability in Democratic Party Competition and Economic Governance' Project, Duke University. Higher scores imply higher degrees of programmatic and clientelistic linkage strategies, respectively. Programmatic Indices established by Kitschelt and Freeze (2010) and measure the issue coherence, salience and distinctiveness of party platforms, cosalpo3econ addresses economic issues only, cosalpo4 includes non-economic issues. Clientelistic index addresses different types of particularistic goods provided by parties.

Table 2: 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
Client	9.0	9.2	9.2	9.4	5.9	4.0
InBtw	12.7	9.6	13.0	9.8	8.6	5.3
Program	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
Client	12.5	7.6	12.6	7.6	9.6	6.2
InBtw	12.2	9.6	12.3	9.7	9.0	6.7
Program	3.5	8.7	3.1	8.2	12.2	13.7

Average characteristics of municipalities, average electoral results of selected parties and their standard deviations. For municipal characteristics, Majoritarian and Proportional refer to the 2003 elections. Figures are in percentage terms.

Table 3: First stage for the 2009 elections

Outcome vars	1	2
Prop09	0.75 (0.10)***	0.83 (0.08)***
window	All	$\pm 25K$
poly.order	5	1
spline		x
N.below	1379	634
N.above	70	49
Fstat.instruments	54	103

Standard errors in parenthesis. Signif. codes: 0.01 ‘***’ 0.05 ‘**’ 0.1 ‘*’. Coefficients from regressions of a given outcome variable on an indicator for population higher than the proportionality threshold using different specifications. The outcome variable is the indicator of whether the municipality had a proportional system. The different specifications are given in the rows below the line. Column 1 uses the full sample, a polynomial of order 5 and no spline. Column 2 uses a linear polynomial with spline and a window of $\pm 25,000$ around the threshold. N.below and N.above refer to the amount of observations above and below the threshold. Fstat.instruments refers to the F statistic of the hypothesis that the excluded instruments (i.e. variables including the indicator for population higher than the threshold) equal zero.

Table 4: Discontinuity regressions for the 2003 elections

Outcome vars	1	2	3	4	5
Client	-4.19 (1.59)***	-5.48 (1.50)***	-4.71 (2.81)*	-5.89 (1.69)***	-5.55 (1.75)***
InBtw	-2.95 (1.60)*	-2.97 (1.65)*	-2.10 (2.59)	-2.58 (1.83)	-1.06 (1.99)
Program	2.48 (1.32)*	2.13 (1.41)	2.24 (2.00)	2.41 (1.38)*	1.92 (1.46)
window	All	$\pm 15K$	$\pm 5K$	$\pm 15K$	$\pm 15K$
poly.order	5	1	1	1	1
spline		x	x	x	x
controls				Demogr	Provs
N.below	1383	581	64	581	581
N.above	88	54	29	54	54

Standard errors in parenthesis. Signif. codes: 0.01 ‘***’ 0.05 ‘**’ 0.1 ‘*’. Coefficients from regressions of a given outcome variable on an indicator for population higher than the proportionality threshold using different specifications. Each row corresponds to a different outcome variable: Percent of seats of clientelistic, in-between and programmatic parties, respectively. The different specifications are given in the rows below the line. Column 1 uses the full sample, a polynomial of order 5 and no spline. Column 2 uses a linear polynomial with spline and a window of $\pm 15,000$ around the threshold. Column 3 uses a linear polynomial with spline and a window of $\pm 5,000$ around the threshold. Columns 4 and 5 use the specification of column 2 adding different sets of controls: A set of 29 municipal characteristics (see table 7 for a list) and a set of provincial dummies, respectively. N.below and N.above refer to the amount of observations above and below the threshold.

Table 5: Discontinuity IV regressions for the 2009 elections

Outcome vars	1	2	3	4	5	6	7	8
Client09	-5.03 (2.84)*	-3.51 (2.25)	-6.15 (4.26)	-3.54 (2.28)	-4.12 (2.42)*	-3.42 (2.20)	-3.66 (2.35)	-4.38 (2.39)*
InBtw09	-0.01 (3.09)	-0.02 (2.48)	0.85 (4.53)	-0.33 (2.55)	-0.39 (2.85)	-0.28 (2.27)	-0.74 (2.66)	-1.70 (2.53)
Program09	8.40 (6.17)	5.30 (4.64)	9.17 (9.78)	5.92 (4.93)	4.27 (5.10)	3.83 (3.86)	5.98 (5.00)	5.74 (4.48)
window	All	$\pm 25K$	$\pm 10K$	$\pm 25K$	$\pm 25K$	$\pm 25K$	$\pm 25K$	$\pm 25K$
poly.order	5	1	1	1	1	1	1	1
spline		x	x	x	x	x	x	x
controls				Demogr	Provs	PastSeats	PastES	D+PS+PES
N.below	1379	634	59	634	634	634	634	634
N.above	70	49	33	49	49	49	49	49

Standard errors in parenthesis. Signif. codes: 0.01 ‘***’ 0.05 ‘**’ 0.1 ‘*’. Coefficients from IV regressions of a given outcome variable on an indicator for proportionality, using as instrument(s) an indicator of population higher than the proportionality threshold possibly interacted with population, under different specifications. Each row corresponds to a different outcome variable: Percent of seats of clientelistic, in-between and programmatic parties, respectively. The different specifications are given in the rows below the line. Column 1 uses the full sample, a polynomial of order 5 and no spline. Column 2 uses a linear polynomial with spline and a window of $\pm 25,000$ around the threshold. Column 3 uses a linear polynomial with spline and a window of $\pm 10,000$ around the threshold. Columns 4 to 8 use the specification of column 2 adding different sets of controls: A set of 29 municipal characteristics (see table 7 for a list), a set of provincial dummies, the percent of seats obtained by the respective type of party in the 2003 elections, an indicator of whether the electoral system was proportional in the 2003 elections, and the demographic, past electoral seats and past electoral system variables simultaneously, respectively. N.below and N.above refer to the amount of observations above and below the threshold.

Table 6: Fixed effect regressions

Outcome vars		1	2	3
Client	Proportional	-3.75 (1.26)***		
	Assigned.to.Prop		-4.30 (1.41)***	
	Become.Prop			-4.615 (3.355)
	Become.Maj			3.458 (1.263)**
InBtw	Proportional	-3.69 (1.32)***		
	Assigned.to.Prop		-3.54 (1.49)**	
	Become.Prop			-5.753 (1.597)**
	Become.Maj			2.982 (1.685)*
Program	Proportional	2.40 (1.39)*		
	Assigned.to.Prop		3.69 (1.72)**	
	Become.Prop			6.646 (2.395)**
	Become.Maj			-0.943 (1.601)
N		1449	1449	1449
N.varying		36	32	36
N.to.Prop				9
N.to.Maj				27

Standard errors in parenthesis. Signif. codes: 0.01 ‘***’ 0.05 ‘**’ 0.1 ‘*’. Coefficients from regressions of a given outcome variable on variables related to having a proportional system using municipal fixed effects and year fixed effects. The outcome variables are the percent of seats of clientelistic, in-between and programmatic parties, respectively. Column 1 uses as main explanatory variable an indicator of proportionality. Column 2 uses as main explanatory variable the ‘assignment to proportionality’ variable: an indicator of population being higher than the proportionality threshold. Column 3 corresponds to a regression of the change in seats of each type of party from 2003 to 2009 on changes in electoral system. The regression distinguishes whether change was from majoritarian to proportional or from proportional to majoritarian. N refers to the total amount of observations. N.varying refers to the amount of observations that experience a change in electoral system from 2003 to 2009. N.to.Prop refers to the amount of municipalities changing from majoritarian to proportional and N.to.Maj to the amount changing from proportional to majoritarian

Table 7: Validity checks: Discontinuities in other variables

Outcome vars	2003	2003	2003	2009	2009	2009
	All	$\pm 15K$	$\pm 5K$	All	$\pm 25K$	$\pm 10K$
Urban	0.09 (0.10)	0.12 (0.11)	-0.10 (0.17)	0.11 (0.18)	0.25 (0.14)*	0.14 (0.30)
Literacy	-4.05 (3.04)	0.25 (3.27)	-5.50 (5.25)	3.10 (5.16)	2.08 (4.13)	1.66 (8.17)
Mobile.Phones	2.14 (3.61)	4.35 (3.86)	-0.14 (6.36)	1.87 (5.03)	0.70 (4.17)	4.83 (7.63)
Public.Employees	-5.21 (2.31)**	-0.02 (2.38)	-2.02 (3.22)	1.24 (4.34)	2.38 (3.29)	0.28 (6.01)
Age.less.than.6	0.34 (0.42)	-0.59 (0.42)	-0.53 (0.69)	-0.16 (0.72)	-0.20 (0.57)	0.04 (1.12)
Age.from.6.to.14	0.62 (0.55)	-0.56 (0.54)	0.27 (0.86)	-0.63 (0.79)	-0.45 (0.63)	0.11 (1.31)
Age.from.14.to.59	-1.61 (0.85)*	1.02 (0.79)	-0.48 (1.33)	0.69 (1.10)	0.21 (0.87)	0.21 (1.59)
Age.more.than.60	0.39 (0.41)	-0.13 (0.40)	0.74 (0.64)	0.20 (0.64)	0.46 (0.52)	-0.34 (0.95)
Single	-2.46 (0.97)**	1.11 (0.89)	1.52 (1.48)	-1.28 (1.49)	-1.60 (1.15)	-2.35 (2.39)
Married	2.31 (0.96)**	-1.36 (0.85)	-1.68 (1.45)	0.80 (1.65)	0.84 (1.27)	2.63 (2.77)
Fertility	-0.11 (0.18)	-0.37 (0.18)**	-0.20 (0.31)	-0.27 (0.27)	-0.27 (0.22)	-0.15 (0.40)
Amazig	6.61 (8.09)	8.76 (8.85)	28.45 (12.72)**	-6.00 (14.41)	3.14 (11.60)	28.52 (18.60)
Arab	-0.76 (0.90)	-0.77 (0.87)	-1.36 (1.31)	0.35 (1.67)	-0.18 (1.37)	1.51 (2.56)
Arab.and.French	-2.14 (1.98)	0.94 (2.14)	-3.27 (3.19)	2.15 (3.29)	1.23 (2.63)	-0.11 (5.15)
Primary	-2.93 (2.68)	0.45 (2.87)	-5.32 (4.57)	2.40 (4.44)	0.85 (3.62)	-0.20 (7.03)
College	-0.67 (0.48)	-0.21 (0.54)	-0.51 (0.87)	0.31 (0.96)	0.52 (0.74)	-0.04 (1.56)
Active	-94.36 (100.17)	-85.10 (91.93)	-160.66 (163.81)	-4.30 (119.09)	4.51 (6.05)	-2.34 (2.27)
Private.Employees	1.54 (4.12)	3.63 (4.48)	-0.86 (7.64)	-1.42 (6.12)	-4.26 (5.12)	0.11 (8.11)
Number.of.Households	214.27 (495.15)	687.79 (520.80)	-440.77 (868.05)	295.31 (589.12)	616.41 (441.72)	394.01 (857.50)
Slums	2.59 (3.73)	5.16 (4.07)	-8.33 (5.19)	4.43 (6.33)	3.11 (5.20)	-3.76 (8.18)

Standard errors in parenthesis. Signif. codes: 0.01 ‘***’ 0.05 ‘**’ 0.1 ‘*’. Coefficients from regressions of a given outcome variable on an indicator for proportionality using different specifications. Columns 1 to 3 refer to 2003. Columns 4 to 6 refer to 2009 and correspond to IV regressions using as instrument(s) an indicator of population higher than the proportionality threshold possibly interacted with population. The outcome variables are given in each row and correspond to various municipal characteristics. For each year, the first column uses the full sample, a polynomial of order 5 and no spline; the second column uses a linear polynomial with spline and a window of $\pm 15,000$ around the threshold ($\pm 25,000$ in 2009); the third column uses a linear polynomial with spline and a window of $\pm 5,000$ around the threshold ($\pm 10,000$ in 2009).

Table 7: (continued)

Outcome vars	2003	2003	2003	2009	2009	2009
	All	$\pm 15K$	$\pm 5K$	All	$\pm 25K$	$\pm 10K$
HouseOwners	3.99 (3.41)	-4.05 (3.55)	1.21 (5.85)	-4.16 (5.60)	-4.52 (4.43)	-7.31 (7.67)
Renting	-1.87 (2.40)	1.04 (2.51)	-3.67 (4.14)	2.35 (4.69)	4.49 (3.68)	11.13 (6.65)*
Houses.less.than.10.years	-4.71 (2.97)	1.50 (2.78)	-7.83 (4.48)*	3.78 (5.04)	-1.01 (4.17)	14.17 (9.01)
Houses.more.than.50.years	2.58 (3.54)	-5.07 (3.51)	1.61 (5.66)	-3.10 (4.09)	-0.10 (3.41)	-5.18 (5.68)
Water	3.33 (7.55)	9.62 (8.38)	9.16 (13.61)	9.91 (12.30)	9.71 (9.64)	15.73 (19.00)
Electricity	-4.03 (6.08)	2.98 (6.65)	-3.86 (11.41)	10.63 (8.01)	7.13 (6.62)	11.43 (12.71)
Sewer	-6.91 (6.77)	-0.54 (7.54)	-4.70 (12.12)	15.52 (12.83)	15.37 (10.37)	15.37 (19.57)
Septic.Tank	6.36 (5.29)	3.79 (5.51)	-6.75 (8.65)	-10.49 (10.20)	-11.16 (8.29)	-17.10 (15.19)
Fixed.Phone	-3.17 (1.48)**	-0.05 (1.55)	-0.57 (2.51)	1.00 (2.58)	0.79 (1.99)	0.13 (3.99)

Standard errors in parenthesis. Signif. codes: 0.01 ‘***’ 0.05 ‘**’ 0.1 ‘*’. Coefficients from regressions of a given outcome variable on an indicator for proportionality using different specifications. Columns 1 to 3 refer to 2003. Columns 4 to 6 refer to 2009 and correspond to IV regressions using as instrument(s) an indicator of population higher than the proportionality threshold possibly interacted with population. The outcome variables are given in each row and correspond to various municipal characteristics. For each year, the first column uses the full sample, a polynomial of order 5 and no spline; the second column uses a linear polynomial with spline and a window of $\pm 15,000$ around the threshold ($\pm 25,000$ in 2009); the third column uses a linear polynomial with spline and a window of $\pm 5,000$ around the threshold ($\pm 10,000$ in 2009).

Table 8: Validity checks: Discontinuities of party seats at other thresholds for 2003

Threshold	Client	Client	Client	InBtw	InBtw	InBtw	InBtw	Program	Program	Program	Program
5	-1.22 (1.46)	-1.36 (1.21)	-2.06 (1.35)	0.63 (1.54)	-0.09 (1.27)	0.71 (1.42)	-0.28 (0.52)	-0.35 (0.47)	-0.51 (0.52)		
10	-1.96 (1.30)	-1.43 (0.85)*	-1.98 (1.16)*	-0.10 (1.40)	0.70 (0.90)	0.40 (1.26)	-1.40 (0.59)**	-0.56 (0.36)	-1.27 (0.50)**		
15	-1.88 (3.91)	0.01 (3.49)	0.89 (1.85)	0.88 (3.51)	1.31 (3.07)	-3.49 (1.85)*	-2.16 (1.54)	-1.08 (1.33)	-1.41 (0.76)*		
20	2.26 (2.61)	1.43 (2.32)	1.10 (2.59)	-4.91 (2.19)**	-3.14 (1.78)*	-3.70 (2.08)*	-1.01 (0.84)	-0.83 (0.58)	-1.01 (0.78)		
30	-0.11 (1.71)	0.79 (1.73)	-2.68 (2.02)	3.64 (3.20)	2.97 (3.02)	3.98 (3.46)	-3.34 (3.75)	-5.01 (3.83)	-1.63 (4.74)		
35	-0.46 (3.28)	0.20 (2.70)	-2.67 (4.50)	1.66 (2.95)	1.61 (2.75)	1.57 (3.91)	2.59 (4.61)	3.32 (4.17)	-1.53 (4.90)		
40	-4.50 (3.09)	-1.47 (2.60)	-2.79 (4.24)	-2.12 (3.63)	-2.46 (3.16)	-2.89 (2.89)	13.20 (7.35)*	10.66 (6.37)*	8.79 (7.95)		
45	1.72 (3.49)	0.61 (3.31)	3.64 (10.83)	-5.09 (3.57)	-4.92 (3.08)	-8.74 (9.24)	-16.19 (9.06)*	-13.30 (7.91)	-19.50 (33.83)		
50	-2.03 (2.94)	-3.77 (3.06)	-0.96 (4.45)	5.57 (3.08)*	6.88 (3.19)**	0.87 (3.80)	-2.68 (6.93)	-1.81 (7.42)	6.68 (6.93)		
55	2.91 (1.43)**	0.07 (1.75)	0.24 (1.05)	2.91 (3.32)	2.66 (3.47)	2.68 (6.64)	-7.67 (5.08)	-2.94 (5.65)	-9.72 (8.22)		
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		

Standard errors in parenthesis. Signif. codes: 0.01 '***' 0.05 '**' 0.1 '*'. Coefficients from regressions of a given outcome variable on an indicator for population being higher than a given threshold, using different specifications. Columns correspond to different outcome variables and different specifications. Rows correspond to different thresholds. Columns 1 to 3 refer clientelistic parties, columns 4 to 6 to in-between parties and columns 7 to 9 to the programmatic party. For each type of party, three specifications are used, one in each of the 3 columns corresponding to each party. The first specification uses the full sample, a polynomial of order 5 and no spline; the second uses a linear polynomial with spline and a window of $\pm 15,000$ around the threshold below the true proportionality threshold of 25,000. Regressions using these include observations of population below 25,000. The first four rows correspond to thresholds below the true proportionality threshold of 25,000. Regressions using these include observations of population higher than 25,000. The reverse applies to the remaining rows, that use only observations with population higher than 25,000.

Table 9: Validity checks: Discontinuities of party seats at other thresholds for 2009

Threshold	Client09	Client09	Client09	InBtw09	InBtw09	InBtw09	InBtw09	Program09	Program09	Program09
10	0.62 (0.96)	-0.63 (0.69)	-0.57 (0.79)	-0.60 (1.23)	0.33 (0.88)	0.32 (1.01)	-0.82 (1.28)	-0.44 (0.84)	-0.30 (0.98)	
15	-0.86 (1.41)	-0.62 (0.82)	-0.63 (0.98)	3.36 (1.87)*	1.41 (1.08)	2.04 (1.28)	-0.33 (1.15)	-0.50 (0.77)	-0.28 (0.87)	
20	-1.61 (1.64)	0.73 (1.12)	0.78 (1.40)	0.90 (2.00)	-1.48 (1.35)	-2.20 (1.69)	0.40 (1.63)	-1.16 (1.14)	-0.27 (1.32)	
25	3.11 (2.52)	0.30 (1.69)	-1.38 (1.94)	-2.52 (3.03)	0.90 (2.05)	4.07 (2.34)*	-0.79 (5.08)	0.17 (3.34)	0.41 (3.58)	
30	-0.16 (3.32)	-1.14 (2.98)	-1.10 (3.33)	-3.37 (4.12)	1.16 (3.59)	-0.67 (4.00)	4.16 (5.23)	3.60 (5.50)	3.95 (5.66)	
40	4.80 (3.88)	3.17 (3.04)	5.90 (3.84)	-5.55 (3.60)	-5.88 (2.65)**	-5.57 (3.52)	4.41 (5.31)	6.91 (3.63)*	5.23 (5.12)	
45	-6.64 (3.73)*	-3.98 (3.09)	-6.16 (3.39)*	-1.83 (3.52)	-2.06 (3.60)	-4.53 (3.67)	1.16 (6.53)	7.09 (5.47)	1.50 (6.25)	
50	6.42 (4.34)	2.53 (3.35)	7.67 (4.26)*	2.19 (5.60)	0.31 (3.99)	-2.96 (5.47)	4.10 (7.99)	9.09 (5.93)	9.22 (7.71)	
55	3.54 (4.79)	1.94 (3.72)	1.45 (5.84)	-1.65 (5.92)	-0.13 (3.91)	-4.39 (6.31)	-14.20 (6.45)**	-7.36 (6.19)	-15.32 (7.67)*	
60	0.35 (6.83)	-0.14 (4.40)	5.00 (8.23)	-5.68 (5.43)	1.19 (3.99)	-6.00 (6.05)	-4.55 (9.77)	-9.87 (7.90)	-3.91 (8.50)	
window	All	±25K	±10K	All	±25K	±10K	All	±25K	±10K	
poly.order	5	1	1	5	1	1	5	1	1	
spline		x	x		x	x		x	x	

Standard errors in parenthesis. Signif. codes: 0.01 '***', 0.05 '**', 0.1 '*'. Coefficients from regressions of a given outcome variable on an indicator for population being higher than a given threshold, using different specifications. Notice that the regressions correspond to the 'reduced form', where outcome variables are regressed directly on the instrument. Columns correspond to different outcome variables and different specifications. Rows correspond to different thresholds. Columns 1 to 3 refer clientelistic parties, columns 4 to 6 to in-between parties and columns 7 to 9 to the programmatic party. For each type of party, three specifications are used, one in each of the 3 columns corresponding to each party. The first specification uses the full sample, a polynomial of order 5 and no spline; the second uses a linear polynomial with spline and a window of ±25,000 around the threshold the third column uses a linear polynomial with spline and a window of ±10,000 around the threshold. The first four rows correspond to thresholds below the true proportionality threshold of 35,000. Regressions using these include observations of population below 35,000. The reverse applies to the remaining rows, that use only observations with population higher than 35,000.

Table 10: Regressions of the effect of proportionality on measures of programmaticness

	P1	P5	P1.1	P5.1	P1.2	P5.2	P1.NoPJD	P5.NoPJD	P1Munic	P5Munic
2003										
Size	-2.499*** (0.496)	-1.945** (0.562)	-2.438*** (0.562)	-1.884* (0.695)	-2.475*** (0.525)	-1.927** (0.580)	-2.573*** (0.457)	-1.816** (0.496)	-0.186* (0.066)	-0.176* (0.070)
Literacy	1.039** (0.331)	1.327*** (0.317)					0.616 (0.834)	2.071** (0.585)	0.280 (0.238)	0.187 (0.195)
Satellite.Dishes			0.671 (0.425)	0.943** (0.290)						
Mobile.Phones					0.961** (0.330)	1.300*** (0.329)				
N	22	22	22	22	22	22	21	21	23	23
2009										
Size	-0.240 (0.522)	-0.325 (0.819)	-0.555 (0.620)	-0.735 (0.948)	-0.041 (0.566)	-0.044 (0.917)	-0.200 (0.534)	-0.256 (0.880)	-0.007 (0.046)	0.021 (0.030)
Literacy	1.162** (0.346)	1.676** (0.501)					1.293 (0.663)	1.903 (1.091)	0.198 (0.164)	0.315 (0.267)
Satellite.Dishes			1.242* (0.463)	1.669* (0.665)						
Mobile.Phones					0.789* (0.306)	1.072* (0.468)				
N	24	24	24	24	24	24	23	23	28	28

Standard errors in parenthesis. Signif. codes: 0.01 '***' 0.05 '**' 0.1 '*'. The outcome variable in the regressions is the regression discontinuity estimate of the effect of proportionality on each party. The explanatory variables are size for the party and different measures of programmaticness. For each measure of programmaticness, the effect of proportionality for each party is estimated using two models, denoted P1 and P2. 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. The measures of programmaticness come from regressing the respective results in the national elections of 2002 (for 2003) and 2007 (for 2009), on different indicators of wealth of the district. The first six columns consider different proxies for wealth for the programmaticness measure: Literacy, satellite dishes and mobile phones. The following two columns subtract the PJD from the analysis. The last two specifications use the municipal data to estimate the measure of programmaticness. The discontinuity regressions are estimated adding literacy as control. The literacy coefficient becomes the measure of programmaticness of each party. The size variable is the estimated percent of seats of each party at the threshold on the proportionality side.

Figure 1: Distribution of Population in 2003 and 2009 elections

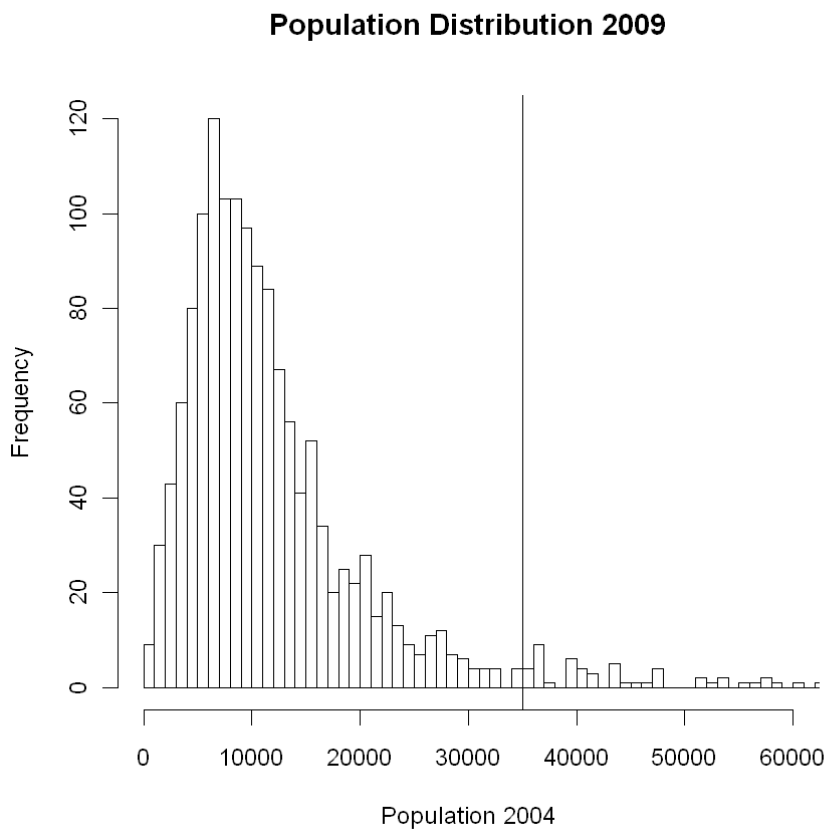
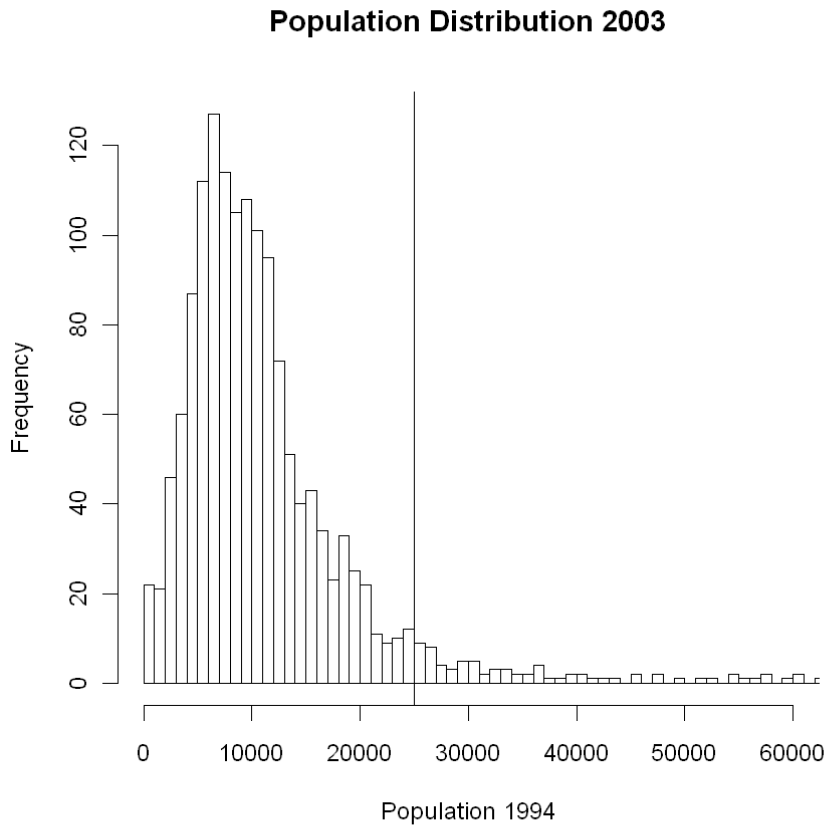
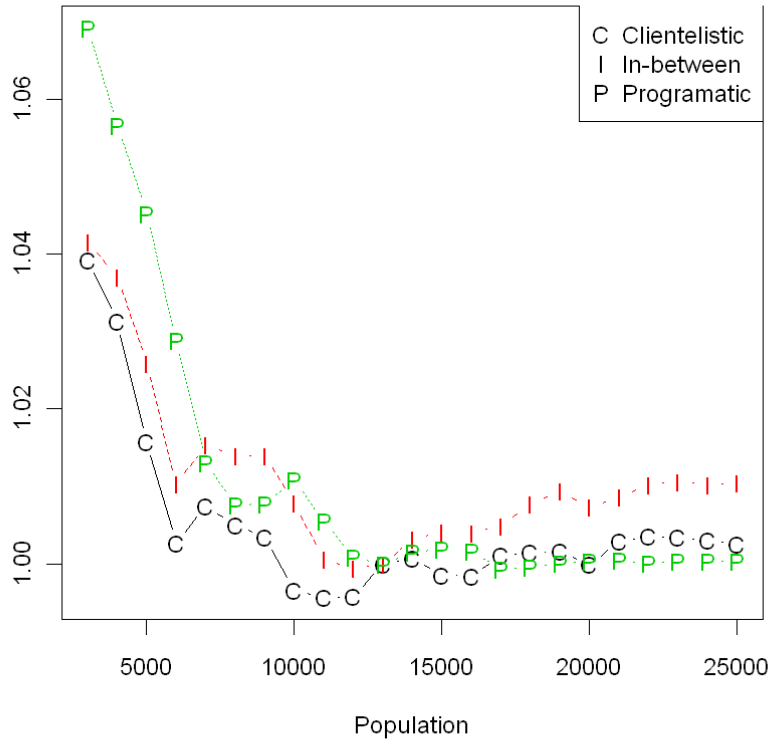


Figure 2: Cross Validation function for 2003 parties

Cross-Validation left 2003



Cross-Validation right 2003

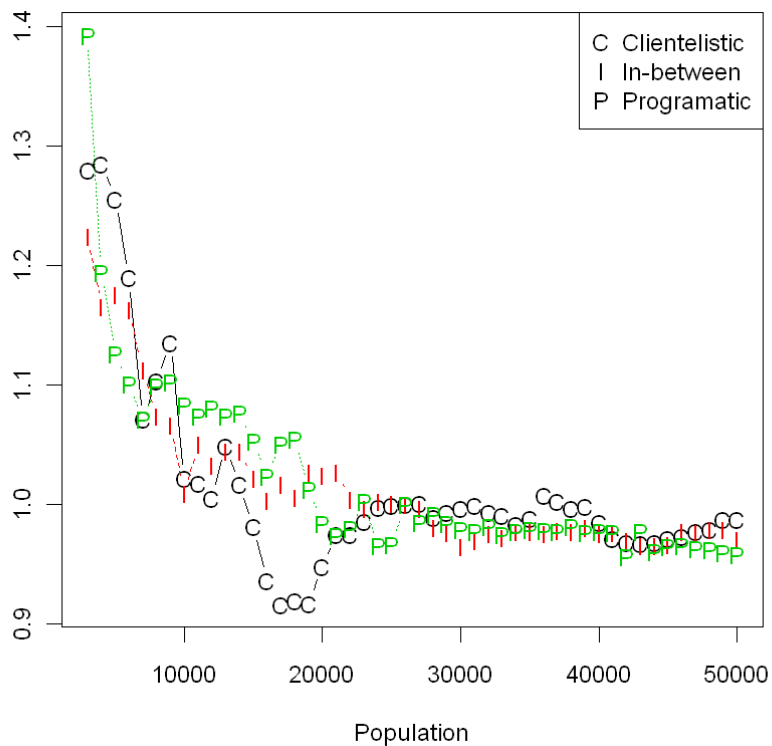
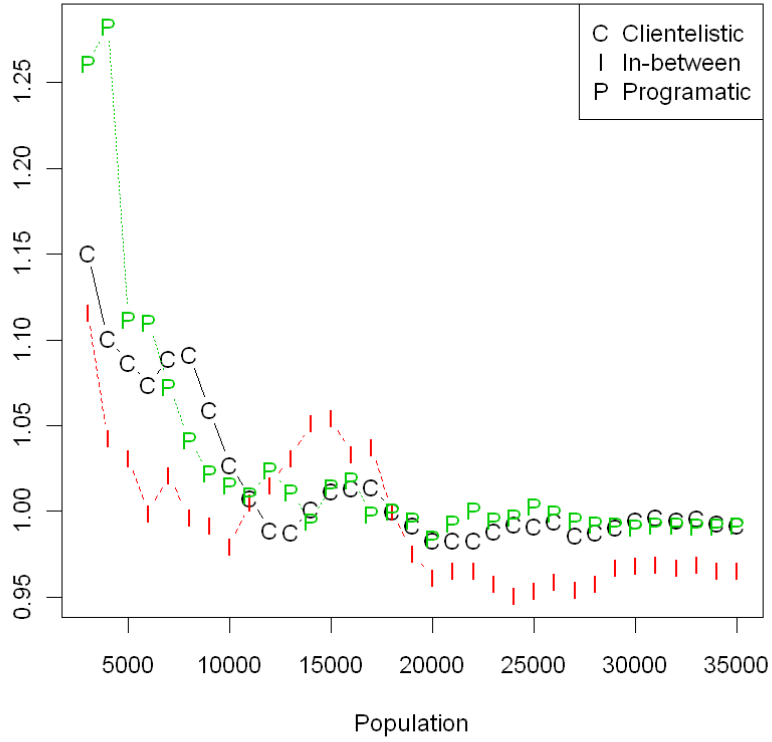


Figure 3: Cross Validation function for 2009 parties

Cross-Validation left 2009



Cross-Validation right 2009

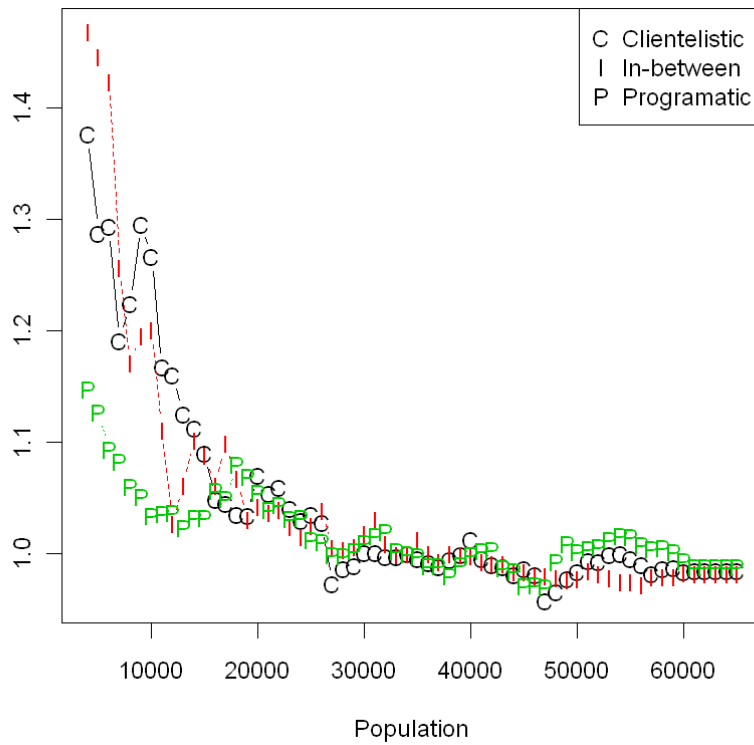


Figure 4: 2003 Discontinuity in seats for different types of party: Full sample

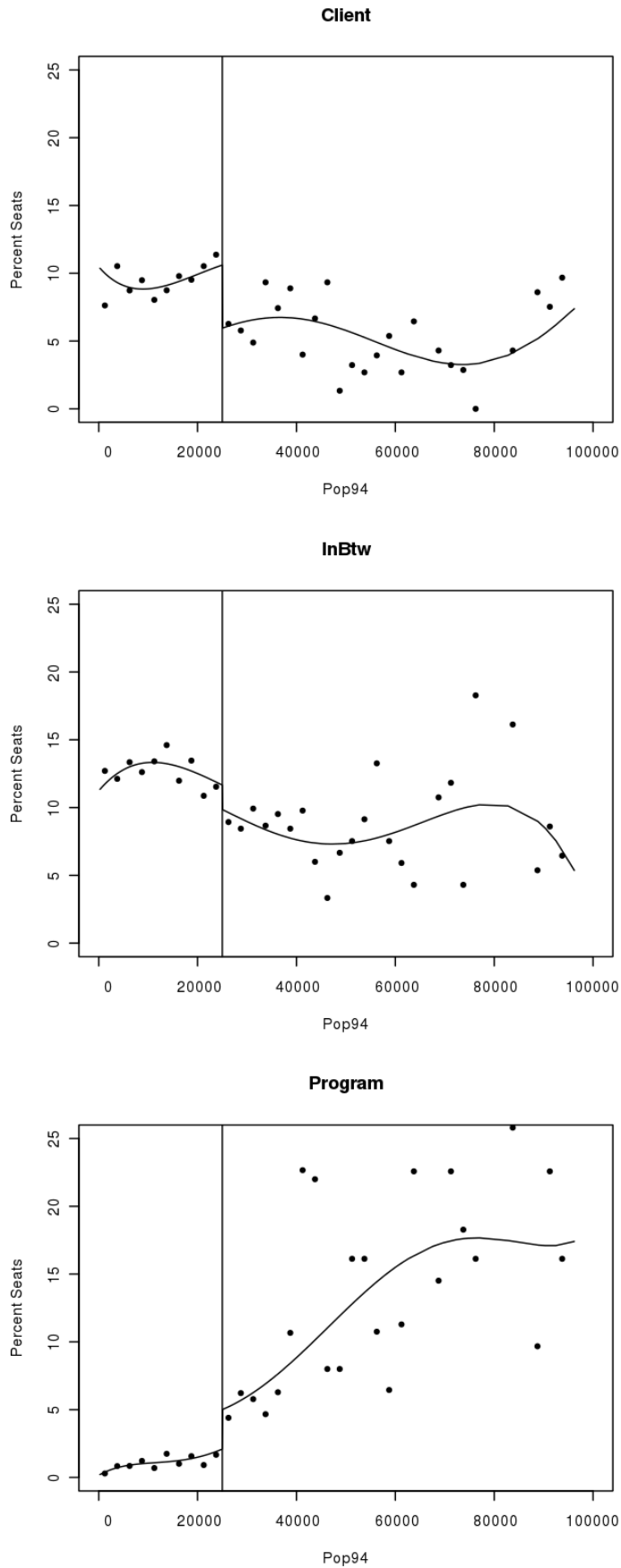


Figure 5: 2003 Discontinuity in seats for different types of party: Restricted sample

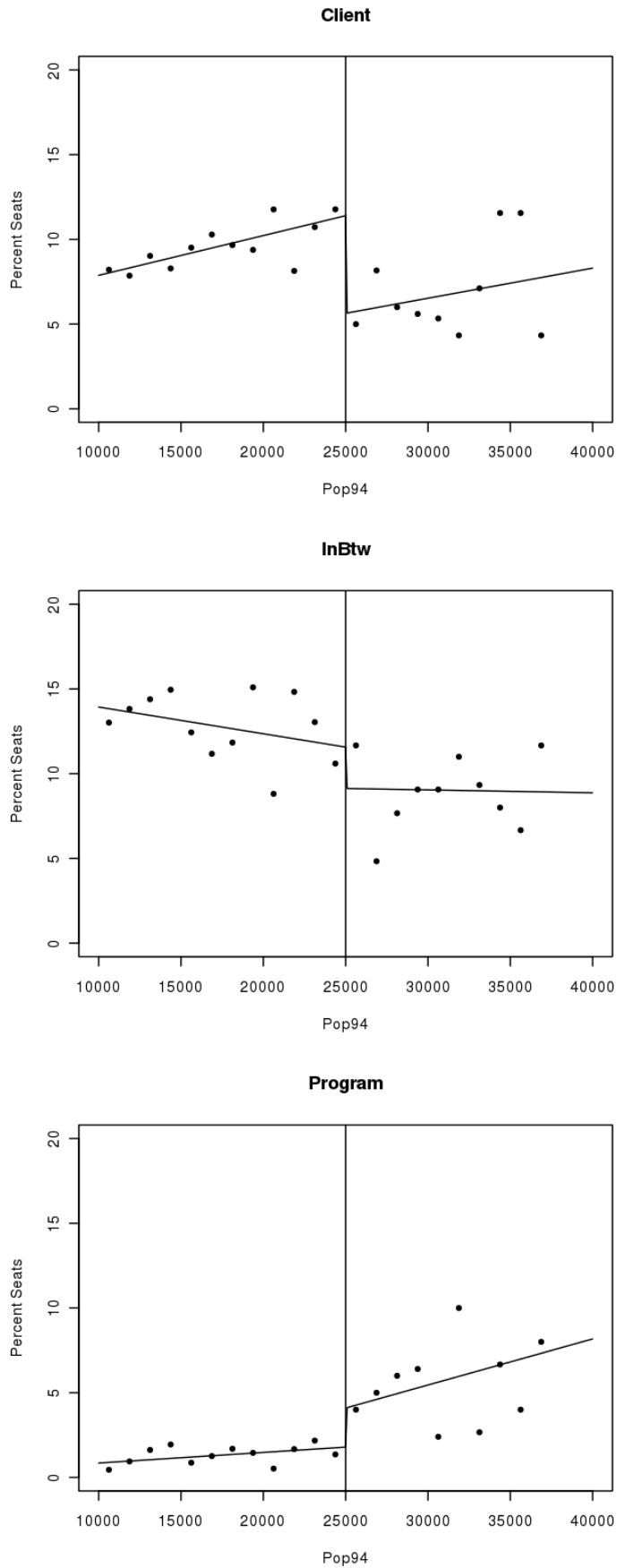


Figure 6: 2009 Discontinuity in seats for different types of party: Full sample

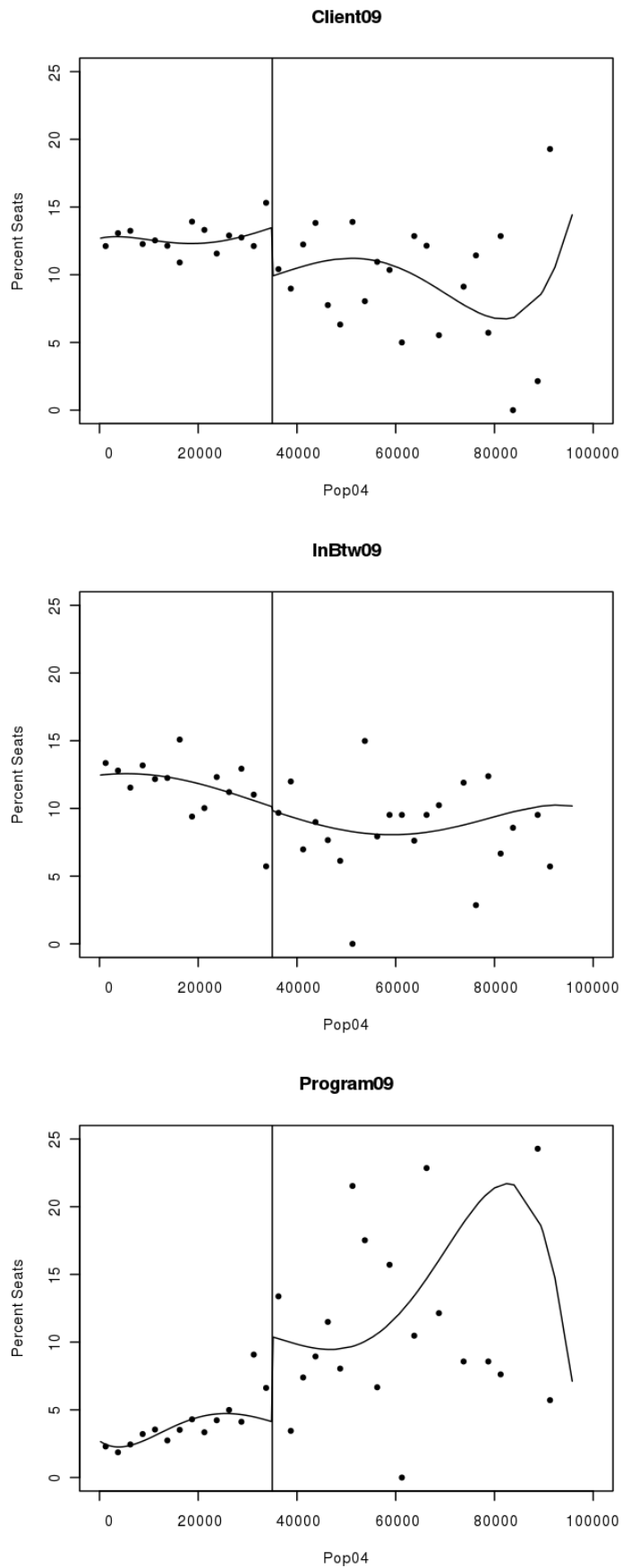


Figure 7: 2009 Discontinuity in seats for different types of party: Restricted sample

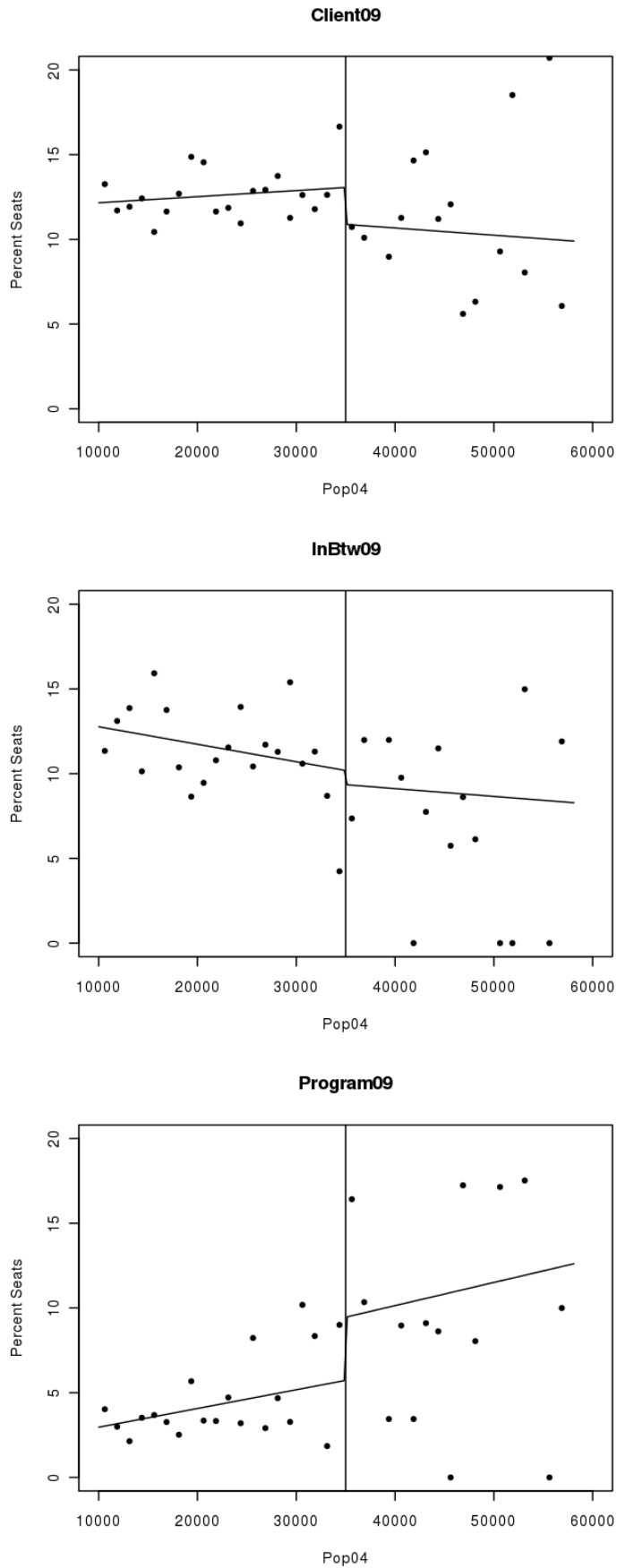
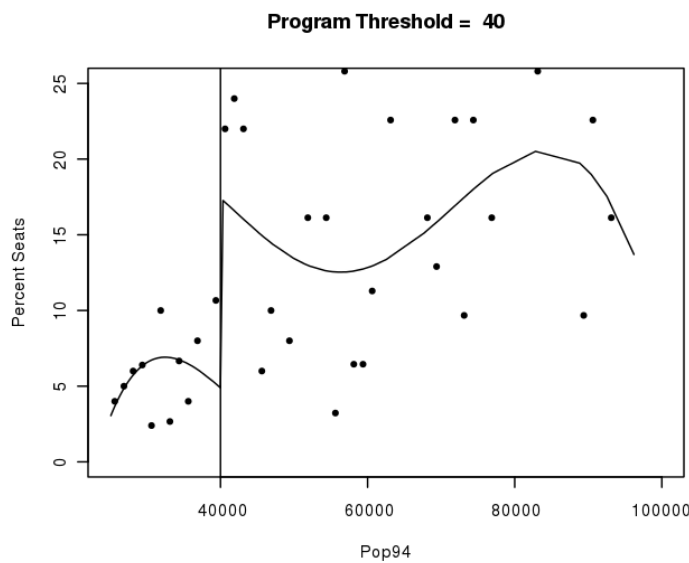
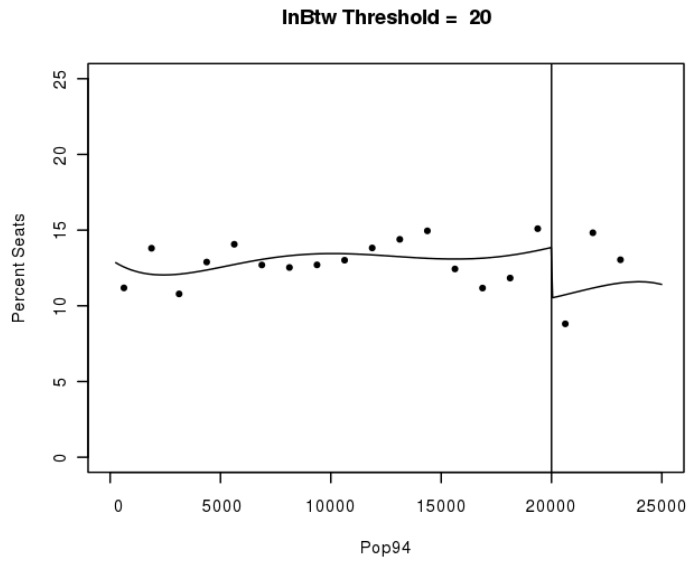
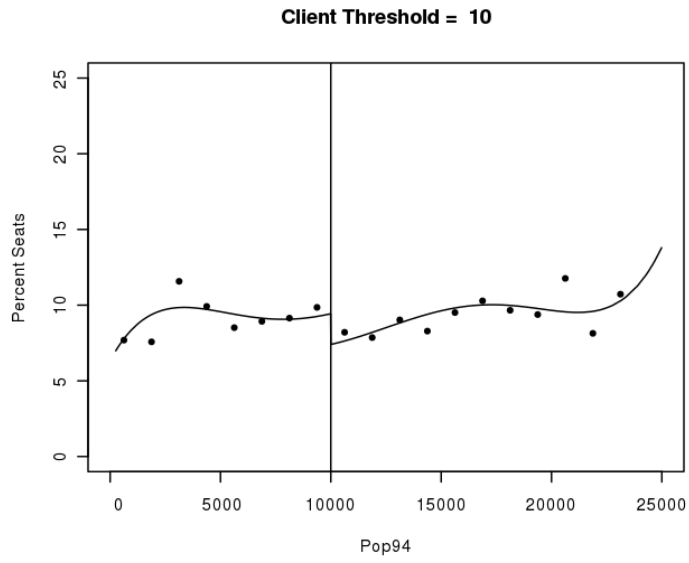


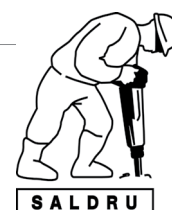
Figure 8: Discontinuity in seats for the significant cases in the placebo test



southern africa labour and development research unit

The Southern Africa Labour and Development Research Unit (SALDRU) conducts research directed at improving the well-being of South Africa's poor. It was established in 1975. Over the next two decades the unit's research played a central role in documenting the human costs of apartheid. Key projects from this period included the Farm Labour Conference (1976), the Economics of Health Care Conference (1978), and the Second Carnegie Enquiry into Poverty and Development in South Africa (1983-86). At the urging of the African National Congress, from 1992-1994 SALDRU and the World Bank coordinated the Project for Statistics on Living Standards and Development (PSLSD). This project provide baseline data for the implementation of post-apartheid socio-economic policies through South Africa's first non-racial national sample survey.

In the post-apartheid period, SALDRU has continued to gather data and conduct research directed at informing and assessing anti-poverty policy. In line with its historical contribution, SALDRU's researchers continue to conduct research detailing changing patterns of well-being in South Africa and assessing the impact of government policy on the poor. Current research work falls into the following research themes: post-apartheid poverty; employment and migration dynamics; family support structures in an era of rapid social change; public works and public infrastructure programmes, financial strategies of the poor; common property resources and the poor. Key survey projects include the Langeberg Integrated Family Survey (1999), the Khayelitsha/Mitchell's Plain Survey (2000), the ongoing Cape Area Panel Study (2001-) and the Financial Diaries Project.



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