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The Illicit Benefits of Local Party Alignment in National Elections

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JEL classification codes
D72, D73, H7, K16, K42

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How do central politicians in young democracies secure electoral support at grass-roots level? I show that alignment with local governments is instrumental in swaying national elections via electoral fraud. Using a regression discontinuity design with Romanian local elections and a president impeachment referendum in 2012, I find higher referendum turnouts in localities aligned with the government coalition - the impeachment initiators. A variety of electoral forensics tests uncover abnormal vote count distributions across polling stations, consistent with null ballot stuffing and possibly vote buying. The alignment effect is driven by rural localities, those with weaker opposition party presence, and higher vote buying incidence in past elections. This illicit transfer from local to national government may explain the reverse clientelistic grants found in the intergovernmental transfers literature.

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1 Introduction

The past decade has seen democracy across the globe relapse into stagnation or even decline. While the young democracies in Eastern Europe, Asia, Africa and Latin America hold increasingly frequent elections and their politicians extensively use a democratic rhetoric, their political processes are fundamentally flawed with their electorates being

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systematically manipulated (e.g. Cruz et al., 2016, Collier and Vicente, 2014, Finan and Schechter, 2012,).[1] A popular form of manipulation is targeted spending and clientelistic allocation of government transfers to more responsive voters (e.g., Pop-Eleches and Pop-Eleches, 2012 Manacorda et al., 2011, Drazen and Eslava, 2010). Recent studies have shown that more funds are being channelled to constituencies controlled by the parties in government, particularly in developing countries and nations in transition (e.g. Bracco et al., 2015; Brollo and Nannicini, 2012; Solé-Ollé and Sorribas-Navarro, 2008). This so-called "alignment effect" is assumed to increase the electoral success of incumbent politicians. However, to date little is known about whether and how local partisan alignment produces the expected electoral advantages. The growing concern is that reaping electoral rewards from aligned constituencies by means of manipulation and electoral fraud, leads to the erosion of democratic institutions, with harmful welfare consequences in the long run.

This paper documents an alignment effect in Romanian national elections and it provides extensive evidence that this effect is driven by electoral fraud. I combine municipality and polling station data in a quasi-experimental setting and a series of diagnostic tests to show that: 1) the ruling coalition had better electoral outcomes in localities where the local government (the mayor) was affiliated with the ruling coalition than other localities; 2) the patterns in the electoral data are consistent with ballot stuffing, which together with vote buying and intimidation account for the observed alignment effect. To get around the endogeneity of mayor alignment, I use a regression discontinuity design, and compare national polls outcomes in localities where an aligned candidate narrowly won and narrowly lost the mayoral race. Local party alignment was decided in the June 2012 local elections, and my main outcomes are drawn from the July 2012 nationwide referendum, which I contrast with the subsequent parliamentary elections.

One of the ongoing puzzles in the vast literatures on targeted spending and vote buying is why it would be rational for politicians to disburse more money to certain constituen-

[1]For instance, according to the 2014/2015 Afrobarometer and 2009 Latinobarometer, over 40% of respondents believed the elections are rigged. In 2006-2013, between 16% and 18% of African voters declared they were offered an electoral gift in the past elections. In former Soviet countries and Russia, the OSCE and Freedom House continue to observe widespread vote-buying and ballot stuffing.

cies, since, by virtue of the secret ballot, voters cannot credibly commit to a certain vote (see Robinson and Verdier, 2013). This could lead to an underestimation of the incidence of vote buying and its associated electoral returns, despite it being widespread in most nations in transition and developing countries. The electoral setting in this paper circumvents the voter commitment problem. The July 2012 referendum was launched by the coalition in government to decide the impeachment of the president, who was affiliated with the opposition party.[2] Importantly, a quorum requirement of 50% voter presence made the referendum turnout the key outcome. Since turnout is easily observed, paying voters for or against casting their ballot had a guaranteed return. Moreover, alongside vote buying, tampering with the ballots at the polling stations is another common form of electoral fraud, one which requires access to the vote rigging machinery. For the national politicians with a stake in the referendum, having an aligned party in local government would have meant that the right networks were in place to facilitate that access. Thus, local party alignment would make tampering with the ballot count less costly. In the referendum setting, the focus on turnout is expected to have brought this cost further down.

I document an excess turnout of up to 5.4 percentage points in localities with mayors from the governing coalition compared to other localities. The effect is driven by rural localities, where the electoral alignment bonus is up to 6.2 percentage points. This is in line with recent studies on other countries and the abundant Romanian contextual evidence that identify higher social pressures, more tightly-knit clientelistic networks, and a higher prevalence of vote buying in rural areas (e.g. Funk, 2010; Vicente, 2014; Volintiru, 2012).

Possible mechanisms behind the alignment effect are: pork-barrel spending ahead of the referendum; voters' bias towards the incumbent local party; turnout buying and vote rigging. Pork-barrel spending is a less likely channel, seen as the time window between new mayors taking office and the referendum was just over a month. I find no significant alignment effects in terms of the referendum vote shares, nor in terms of the voters'

[2]For details surrounding the referendum see http://www.theguardian.com/world/2012/jul/29/romanians-unlikely-impeach-president-traian-basescu (in English)

revealed preferences in the outcomes in the legislative elections in December the same year, which is at odds with the second mechanism above.

I then run a battery of diagnostic tests popular in electoral forensics for their straightforward use of statistics to uncover abnormal patterns in electoral returns, consistent with electoral fraud.[3] Specifically, following the innovative methodology developed by Beber and Scacco (2012), I contrast the distributions of the last digits in turnout counts with those of the valid vote counts at the polling stations. The authors demonstrate that in fair elections, the last digit of the vote counts with more than three digits in length should be uniformly distributed. Thus, each digit 0-9 should appear on average in 10% of the polling stations. I find significantly higher or lower last digit frequencies in the turnout counts, but not in the valid vote counts. Since their difference is made up of null votes, I examine graphically the concentration of null votes at different turnout levels. I find that government-aligned localities have both more polling stations with null votes and higher shares of null votes around and above the 50% threshold. This is consistent with ballot stuffing using invalid votes by the governing coalition, in an effort to boost turnout. While the last digit tests suggest some efforts of the opposing party to depress turnout, turnout density discontinuity tests show clear signs of turnout manipulation around the 50% threshold occurring only in aligned localities. This evidence corroborates the accounts of electoral misconduct revealed in a series of trials which had high party officials and local politicians convicted for rigging the referendum. The methods described in the investigations included vote buying and fraudulent ballot stuffing in emigrants' names. I show additional evidence of these channels from heterogeneity tests which reveal larger alignment effects in counties with higher perceived incidence of vote buying and in counties with higher shares of out-migrants.

Furthermore, I explore the results in narrow races between different parties with varying interests in the referendum. The alignment bonus is insignificant in races between

[3]Electoral forensics tests combine data visualisation and vote distribution tests to identify patterns consistent with electoral fraud from publicly available electoral data. Mebane (2006) developed a new methodology which inspected the distribution of the second digit in vote counts at polling stations. The idea is to contrast vote counts data in fair and suspicious elections, the latter of which are expected to display patterns consistent with human bias in the generation of vote distributions.

the governing coalition and the president's party - the two opponents with the strongest conflicting interests in the referendum. By contrast, there are very large effects (up to 14 percentage points) in races between the governing coalition and its allies (the latter having little interest in monitoring the incumbents). This points to the role of party competition in ensuring the right checks and balances are in place for free and fair elections.

Finally, I show some post-referendum alignment benefits in terms of locality revenues in 2013. Moreover, under stronger assumptions, I also use the alignment in the RD setting as an instrument for referendum turnout, and I show that the latter generated significantly higher turnout and incumbent vote shares at parliamentary turnout. This suggests that the referendum served as a mobilization exercise.

The growing literature on the role of local party alignment in intergovernmental transfers has revealed a pattern of clientelistic allocations from the higher tier to lower tier governments.[4] However, the evidence of a reverse transfer of political support from local to higher tier politicians is both scant and mixed. Local politicians are assumed to provide important political capital for their parties, partly for ensuring policy congruence, partly because they can mobilize voters and pass along electoral rewards to higher tier politicians.[5] One of the few studies to test this hypothesis found that Brazil's president's party may have had a small (albeit not robust) electoral gain in national elections from gifting their mayors with larger grants (Brollo and Nannicini, 2012). Ade and Freier (2013) found that winning parties in mayoral elections did better in simultaneous municipality council elections, but not in subsequent European or German parliament elections.[6] Migueis

[4]The earlier studies revealed some correlations between political affiliation of local governments and the incumbent party in central government (Grossman, 1994 and Snyder and Levitt, 1995 for the U.S. and Worthington and Dollery, 1998, for Australia). More recent studies used research designs such as difference-in-differences and regression discontinuity to estimate the causal link betwen alignment and transfers (Solé-Ollé and Sorribas-Navarro, 2008, provide evidence from Spain; Brollo and Nannicini, 2012, reveal partisan bias in transfers ahead of local elections in Brazil; Migueis, 2013, shows evidence for Portugal; Bracco et al., 2015, for Italy; Dey and Sen, 2016, for India)

[5]This is an explicit assumption in theoretical studies that explain partisan transfers (e.g. Zudenkova, 2011 and Persico et al., 2011), but also in the empirical literature.

[6]Their results come from coattail effects, where certain politicians have higher election chances due to their parties' success in a different election. Their explanation is that when two elections are on the same day, voters incur smaller cognitive costs in choosing the same party, otherwise they update their choices to ensure a power balance through divided government. Other studies have documented presidential coattail effects in the U.S., see Cohen et al. (2000), Mattei and Glasgow (2005), Gélineau and Remmer (2006) and Golder (2006)), and have also explored reverse coattail effects (i.e. electoral success propagating from lower to higher tier politicians), showing just mixed correlational evidence (Ames, 1994, Samuels, 2000a,

(2013) also found no effects of local party alignment in Portuguese legislative elections, despite a clear pattern of preferential government transfers.

Two papers present evidence in support of the local officials' role in higher tier elections, both in developing countries. Dey and Sen (2016) use a fuzzy regression discontinuity design to show that the party in Indian village councils that preferentially allocated a social protection program to aligned constituencies also received significant electoral bonuses subsequent elections. Martinez-Bravo (2014) presents a theoretical model and data from the democratization years in Indonesia, suggesting that appointed local officials had incentives to invest effort into delivering votes for the district mayoral elections (hypothesizing that vote buying may have been an important channel).

I contribute to this literature with new evidence on the role of local alignment in the electoral success of central politicians, and also on the mechanisms for boosting electoral success. The patterns consistent with electoral fraud suggest that the political alignment quid-pro-quo may engender an illicit type of political capital in local governments, which politicians can exploit at the expense of democratic values and institutions. This is particularly pernicious in fragile countries with new institutions, as the broader literature on political processes in young and vulnerable democracies shows (Keefer and Vlaicu, 2008, Finan and Schechter, 2012, Martinez-Bravo, 2014).

The paper is structured as follows: section 2 describes the institutional setting; section 3 discusses the data; section 4 outlines the identification strategy; section 5 displays the main results, the mechanisms though diagnostics and heterogeneity tests, and the robustness checks; section 6 presents further alignment results from local revenues and parliamentary elections; section 7 concludes.

Samuels, 2000b; Broockman, 2009).

2 Institutional Setting

2.1 The Romanian Electoral System and Politics in 2012

Romania is a young semi-presidential democracy, ruled by a government accountable to the Parliament. As head of state, the president is an active player in internal politics: he oversees the balance of powers, has the right to appoint the prime minister and to veto laws. The president is directly elected every five years in a runoff majority vote, while local and parliamentary elections are held every four years.[7] A multiparty system has been in place since 1990, with 39 parties and alliances registered for the 2012 parliamentary elections. However, the most prominent parties in 2012 were the former communist Social Democratic Party (PSD), *the liberals* - National Liberal Party (PNL) and the *centrist* Liberal Democratic Party (PDL).

The incumbent president in 2012, Traian Basescu, although *de jure* politically unaffiliated, enjoyed strong support from the centrist PDL and was seen as its unofficial leader.[8] From December 2008 until April 2012, all the cabinets represented coalitions with PDL. For most of this time, PSD and PNL were in the opposition. In February 2011, these forged a coalition - the *Social-Liberal Union* (USL), joined by the Conservative Party (PC). Presided by the PSD and PNL leaders Victor Ponta and Crin Antonescu, USL made its first priority to remove the PDL government and president Basescu from office.[9]

The year 2012 was an electoral year: local elections were held on June 10, and parliamentary elections on December 9. In light of these electoral events, the two rivals, PDL and USL, had large stakes from staying in power and taking power, respectively. The coalition gained momentum in April 2012, when a newly formed PDL cabinet fell

[7]The legislative elections run on a list system of independent candidatures, based on a closed list proportional representation system, with a minimum vote share of 5% required for any party to get seats. In the proportional representation system, legislative seats get allocated to each party in proportion to the number of votes the party receives. The closed-list system means that each party has an internal method for deciding the candidates put forth for elections, and each voter effectively casts only one vote for the party in each chamber, for the candidate decided *a priori* by the party.

[8]Article 84 in the Romanian Constitution stipulates that the president cannot be a member of any political party while in office. However, before taking office in 2004, Basescu had been been the hailed president of one of the parties that merged in 2007 to found PDL.

[9]USL Founding Document issued on 5 February 2011 outlines the alliance's political agenda. The first objective reads: 'To remove from power, in a democratic manner, the current clientelistic, corrupt and inefficient regime'.

short of majority support in the Parliament and was dismissed through a motion of no confidence.[10] Shortly after, Victor Ponta took office and formed the USL coalition government, which immediately initiated the legal procedures to suspend president Basescu from office. These entailed a quick succession of controversial institutional reforms, leading to a full-blown political crisis.[11] The impeachment of the president was to be decided at the national referendum in July 2012.

2.2 The 2012 Impeachment Referendum

On 4th July, the *social-liberal union* submitted to the Parliament an official request to impeach the president on grounds of unconstitutional conduct in office.[12] On 6th July, a Parliament majority voted in favor of impeachment, with his recall from office to be decided in a national referendum.[13] The referendum was held on the 29th July, when Basescu's popularity was at an all-time low, owing to the draconian austerity cuts by at least 25% in public sector wages in 2010. Opinion polls just ahead of the referendum anticipated his removal from office.[14]

Importantly, the referendum law in Romania stipulated a *quorum* rule: a minimum turnout of 50% was necessary to validate the referendum.[15] On the day of the referendum, a staggering 87.52% voted "YES" for having Basescu removed.[16] However, only 46.24% of the 18 million registered voters cast their ballot, and therefore the referendum was ruled

[10] In February 2012 the prime minister who, at that time, was a favourite of the president, was forced by popular protests to step down from office. His centrist government had lost popular support following the austerity measures in 2010 (e.g. 25% public sector wage cuts).

[11] Constitutional court processes were changed and a new national ombudsman had ties to the *social-liberals*. This facilitated the subsequent government ordinances for the president's impeachment. Ponta's initiatives and the June 2012 political crisis are described in Politeanu (2012) and some online sources:
http://www.economist.com/blogs/easternapproaches/2012/06/romanian-politics-0 (in English)
http://www.theguardian.com/world/2012/jul/29/romanians-unlikely-impeach-president-traian-basescu (in English)

[12] Basescu was accused of having taken over too many of the government's attributions and to have attempted influencing the justice courts. The official document's title is 'Solicitare privind suspendarea din functie a presedintelui Romaniei, Traian Basescu'

[13] Basescu was immediately suspended from his attributions and Crin Antonescu, leader of the *liberals*, became interim president.

[14] Around 67-70% of respondents would vote him out, and just over 50% would cast their vote (53% in rural and 52% in urban areas) according to a survey conducted by the Group for Social-Behavioral Studies "Avangarde": "Operations Research" Survey 23-25th July; See http://bit.ly/1O8r35T (In Romanian)

[15] Law No.3/2000, article 5(2).

[16] The exact question on the ballots was "Do you agree with the dismissal of the president Traian Basescu?"

invalid by the constitutional court. The president resumed his duties shortly.[17] Figure 1 shows a brief chronology of political and electoral events leading up to the referendum.

2.3 The local government and the corruption environment

Romania's local administration is organized into 42 counties subdivided into urban localities (cities and towns) and rural localities (communes and villages). The local administration falls in the remit of mayors and local councils. As head of the local public administration, the mayor enjoys the highest status in the civil servants hierarchy and in the community (particularly in rural areas).[18] In 2012, the mayor were directly elected in a first-past-the-post system.[19] Once elected, the mayors took office almost immediately.[20]

Importantly, mayoral candidates can run for a separate party, a coalition, or as independent candidates. In the 2012 local elections, 42.39% of the seats were won by USL coalition, but an additional 11.9% of the seats were taken by candidates representing *the social-democrats* separately, and 8.3% by separate *liberal* candidates. The local competition between parties *within the governing coalition* was allegedly borne by quarrels over local offices. This has implications for the treatment definition (see section 3.1). The mayor's political alignment typically flags the locality's political leaning, and is thought to predict the parties' local performance in national elections (Buti, 2012; IRES, 2012).[21]

Mayors play an important part in higher tier elections, where they use their networks to mobilize voters through: 1) campaigning and "get-out-the-vote" strategies (Seceleanu, 2009); 2) pork-barrel spending (e.g. Pop-Eleches and Pop-Eleches, 2012 investigate the EURO 200 program for supplying the needy with computers; EFOR, 2013 document

[17]President Basescu had withstood an impeachment referendum before, in 2007, when 74.48% of voters agreed to keep him in office. The turnout in the first referendum was 44.45%, but no quorum rule was in place at the time.

[18]Article 66 of Law No. 215/2001 Art. stipulates that "the mayor has a position of public authority. He/She is the head of public local administration and of the locality-specific public administration apparatus, which he/she manages and controls"

[19]Before then, mayors were selected in a majority two-round election. The Law No. 129/2011 changed this to a first-past-the-post-system. Local councillors are also directly elected on the same day as the mayor, up to a number of seats determined by the population size.

[20]Mayors' mandate is validated in court in maximum 20 days after the local elections, as stipulated in article 63, Law No. 215/2001. Mayors can assume office right after taking the oath of duty, as soon as their mandate is deemed valid by the court.

[21]In the 2000 legislative and local elections, the vote share correlation was 40% (Klasnja, 2014).

the clientelist allocation of infrastructure funds); 3) local media and church propaganda (particularly in rural areas); 4) coordinating vote-buying and electoral fraud operations, have easier access to the vote-rigging apparatus as heads of the local administration.

Vote buying is a common practice in Romania, particularly in rural, less educated communities. Votes are bought for petty cash or goods, such as eggs and buckets, and even expensive gifts, such as mobile phones and livestock, and the incidence is high (Volintiru, 2012).[22] For instance, at the 2014 presidential elections, 19%-24% of voters reported vote-buying, and 16%-26% voter intimidation (Comsa and Postelnicu, 2014).[23] The 2012 impeachment referendum was fraught with allegations of electoral fraud (Freedom House, 2013). An undercover journalist passing for a central party representative published his inquiries to small town mayors, just ahead of the polls (Biro, 2012). The *governing coalition's* mayors hinted at unorthodox practices to boost turnout: "You give them [the voters] a snaps, a sandwich (...). We'd rather you [the party] sent us money.", or "Evening after evening we took the people in taverns. We'll have 75% [turnout]". Conversely, the *president's party's* mayors organized distractions to turn voters away from the polls.

Additionally, several mayors were indicted in a controversial referendum corruption trial which convicted a senior minister from the *the social democrats* for electoral corruption aiming at a 60% turnout target (National Anticorruption Directorate Press Release 2013).[24] Court files revealed a variety of fraudulent practices: ballot-stuffing (votes attributed to emigrants or deceased persons), threats to get or to impede votes, double-counting and misusing the mobile ballot box.[25] Most evidence highlights the governing coalition's misconduct, but the president himself manipulated voters through the media to

[22]Volintiru (2012) documented these practices in face-to-face interviews with local officials and party members.

[23]The authors carried out a series of four telephone surveys, each on a sample of 1300-2500 voters, using the Crosswise technique, an established method which preserves response anonymity in sensitive questions about fraud, thereby producing reliable distributions of the variables of interests based on underlying truthful reporting.

[24]See "Romanian minister found guilty of vote-rigging in referendum" retrieved from http://www.reuters.com/article/us-romania-corruption-idUSKBN0O00J820150515, accessed 31 August 2016 (in English).

[25]NYU Politics professor Joshua Tucker describes the context of the referendum and the fraud means on U.S. political science blog http://themonkeycage.org/ in the article "Post-Election Report: Romanian's Presidential Impeachment Referendum, and a Request for Help in Identifying Potential Fraud" posted on 9th August 2012.

refrain from voting, and several PDL mayors were also indicted for electoral misconduct.[26]

Overall, the rich anecdotal evidence reveals the illicit influence of local officials in national polls. The formal analysis below sets out to test whether and how this created a significant alignment bonus in the referendum and subsequent elections.

3 Data

To estimate the mayors' partisan alignment effect on the referendum, I use locality and polling station data:

i) Electoral data from 2012 local elections, the national impeachment referendum and legislative elections, available at polling station level. This is public data from the Romanian Electoral Authority (AEP). For the RDD I aggregate this data at locality level, obtaining: the vote shares for each party's mayoral candidate (the running variable); the referendum turnouts (i.e. the share of ballots cast from the total number of registered voters), the 'YES' vote share (the percentage of votes in favour of dismissing the incumbent president). I also use the polling station - level total and valid vote counts, as well as null vote counts for the electoral fraud diagnostics. For further outcomes I use parliamentary election turnouts and party vote shares by locality. In my control variables I include the turnout in a 2007 impeachment referendum, the number of mayoral candidates at the 2012 local elections, and the added vote share of the top two mayoral candidates as electoral controls in the regressions.

ii) Census data from 2011 and 2002 Population Censuses from Statistics Romania for a set of pre-treatment locality-level covariates: the locality log population size, the share of voting-age population, the share aged over 65, the share of Romanians, of males, and of high school and university educated, and the unemployment rate.[27]

iii) Fiscal data from 2011. This data from the Ministry of Regional Development is available at locality level, including: total income (own taxes, intergovernmental transfers,

[26] See e.g. http://bit.ly/1Or9UCg, Romania Libera, published 30 July 2012 (in Romanian). (In Romanian)

[27] For a slightly smaller sample, I also have a number of additional covariates: the shares of illiterate, the share working in agriculture and in public administration, the proportion of migrants working abroad.

subsidies), total and split public expenditure (on education, health, and public services). These form the second set of covariates.

3.1 Treatment definition

A locality is treated if the newly elected mayor is aligned with the *governing coalition*. Alignment here refers to *de facto* support for the *social-liberal union*'s manifesto requiring: interest in ousting the president and representing the coalition in all elections.

In the local races, 1979 top-two mayoral candidates represented the *governing coalition*, while some ran separately for a party within it: 706 for *the social-democrats*, 554 for the *liberals*, and 47 for the *conservatives*. These divisions occurred from disagreements over local administration offices. In terms of their loyalty to the coalition, the *social-democrats* remained the main drivers of the referendum, while the separate *liberals* and *conservatives* had ambiguous incentives.[28] To compare their engagement in the referendum, the online appendix Table A1 presents the turnout between the different parties having won mayoral seats in narrow races, including separate *liberals* and *conservatives*. The only significant difference in turnout appears in races between *social-democrats* (mean turnout was 64.6%) and *liberals/conservatives* (mean turnout was 56.2%). I therefore place *liberal/conservative* localities in the control group in the main specifications, and move them to the treatment group in the robustness section 5.3.[29]

In sum, a locality is aligned with the *governing coalition* (henceforth G-aligned) if the mayor ran for governing coalition or the *social-democrats*, and G-unaligned otherwise.

3.2 The working sample and descriptive statistics

The complete dataset contains all 3181 localities except the capital, of which I exclude localities with a candidate with vote share above 80%, which is unproblematic since the

[28]The *social-democrats* were the strongest party in politics in 2012, having won alone 11.9% seats in the local elections, while the other two combined secured 9.65% of seats. Moreover, PSD leader Victor Ponta was head of the cabinet and also the main proponent of the reforms facilitating the impeachment.

[29]In alternative specifications, I also control for races against *liberals* and *conservatives*, and I also exclude these races from the sample.

identification relies on close electoral races. These account for 11.4% of all races.[30]

Secondly, I restrict the sample to those localities where a G-candidate was either winner or runner-up, irrespective of the number of candidates running in that locality. This leaves out 421 races, or 13.2% of all localities.[31] The caveats are: i) sample selection based on popular support for the aligned candidate; ii) Narrow races within localities need not imply close comparability across localities: I might compare a 47% share G-winner with 37% share G-runner up (depending on how many candidates split the votes in local elections), a threat to internal validity.

The alternative is a more clear-cut identification from two- or three- candidates races (e.g. Brollo and Nannicini, 2012). However, this entails a different sample selection and lower statistical power. As Romanian elections are traditionally disputed amongst many parties (up to fifteen in this case), this sample includes a mere 154 (6.4%) two-candidate races and 479 (20%) three-candidate races against G. The question is essentially how to resolve this trade-off between internal validity and precision.

To resolve this trade-off between internal validity and estimation precision, first, I examine the selection of two-candidate races and second, I run a series of robustness checks. Online appendix A2 shows comparative statistics between the races with a victory margin below 5 percentage points and all the two-candidates races. The latter have significantly lower population, higher per capita revenue, and, importantly, lower education levels and expenditures. Thus, most observables, except the higher per capita revenue, suggest two-candidate localities could be more susceptible to manipulation, potentially giving rise to an upward selection bias the RD estimations. Nevertheless, while the main identification strategy uses the entire sample, in section 5.3 I present estimations based on two- and three- candidates samples. Furthermore, in my main estimations control for the number of candidates and the joint vote share of the top-two candidates.

The RD working sample contains 2386 localities with mayoral races against a G-

[30]However, their inclusion does not change the results qualitatively, since the RDD picks up effects close to the cut-off, as explained in detail in the next section. Ade and Freier (2013), for instance, exclude races where the victory margin is larger than 60%. This is also useful because estimates using higher order polynomial control functions in the RDD are sensitive to extreme values of the assignment variable, and may therefore be biased (Gelman and Imbens, 2014).

[31]In addition, 9 observations are dropped due to missing data.

aligned candidate, of 2116 small rural localities (mean population - 3263), and 270 are large urban localities. Appendix Table A3 displays the comparative statistics in national elections outcomes and locality characteristics in this sample, for all, and for rural and urban localities. Referendum turnout is markedly larger in localities where the G-aligned candidates won (58.3%) than where they lost (47.2%). This difference is larger in rural localities (12 percentage points). Interestingly, this stands in contrast with the 2007 impeachment referendum turnout, which was significantly lower in G-aligned localities. The share of votes for impeaching the president is very similar in all groups, around 86%, while turnout and G-vote shares in parliamentary elections are higher in G-localities. Table A3 also displays largely similar pre-treatment characteristics between G-aligned and unaligned localities, with some differences in ethnic composition and fiscal revenues.

Such differences could cause endogeneity concerns, unless these covariates are continuous around the victory threshold. Therefore, in Table 3, I zero in on races within 5 percentage points of the victory threshold. The only significant electoral difference remains in referendum turnout in rural areas. Almost all differences in pre-treatment covariates between G-aligned and unaligned rural localities vanish, except in the share of people in higher education, which is larger in G-aligned localities. Given that a few differences in revenues and expenditures persist in the urban sample, which is much smaller, it is unlikely that the RD estimates in this subsample would be unbiased. To rule out that these few small differences confound the RDD estimates in the overall and rural sample, I test formally the discontinuities in pre-treatment characteristics in the validity section 4.2. I also including these covariates in the main specifications in the results section.

4 Identification Strategy

I identify the impact of mayors' *government coalition*-alignment on referendum outcomes from closely-contested mayoral elections. Using the sharp regression discontinuity design as in Lee (2008), I compare referendum outcomes in localities where a G-candidate narrowly won and narrowly lost the mayoral race. To reiterate, because Romanian elections are traditionally disputed among many parties, I focus only on the top two candidates'

parties. The vote margin between the aligned and unaligned candidate is the running variable, and the alignment treatment is assigned to localities where G-candidates' victory margins exceed 0. The victory margin is determined by: 1) the localities' characteristics (including voters' preferences); 2) pure chance. Lee (2008) showed that, as long as the probability density function of the running variable (conditional on locality characteristics) is continuous in a small region around the victory threshold, the variation in treatment status can be considered to be due to chance, as in a randomized experiment. This assumption should hold in closely-contested elections, owing to the inherent uncertainty about the final vote count, which, in principle, makes it difficult for the candidates (or voters) to perfectly control the vote margin. If the partisan alignment treatment is as good as randomly assigned, the average treatment effect is the expected difference in referendum outcomes between treated and non-treated localities around the threshold. Below I expand on the econometric model and I also provide treatment validity tests.

4.1 Econometric specifications

Let dG_i be the vote share difference in locality i between the G-aligned and unaligned candidates at the mayor elections. This vote margin is the *running variable*: the G-aligned candidates wins when dG_i is positive and is the runner-up when dG_i is negative (i.e. the victory threshold is $dG_i^* = 0$). The Average Treatment Effect (ATE) of partisan alignment is gauged from the discontinuity in observed outcomes at the cut-off. This can be estimated using the following simple linear regression model in a tight region around the cut-off:

$$y_i = \alpha + \beta \cdot G\_wins_i + \varepsilon_i, \tag{1}$$

$$\text{with} \;\; G\_wins_i = 1\left[dG_i \geq 0\right] \text{ and } E\left[\varepsilon_i | dG_i\right] = 0,$$

where the dependent variable y_i is: 1) referendum turnout and 2) "YES" vote share, in locality i. $\hat{\beta}$ is the estimated ATE of partisan alignment and $1\left[.\right]$ is the identity function. I restrict the sample to the vote margin intervals [-2.5; +2.5] and [-5; +5] percentage points. These yield quite small samples, trading off precision for validity.

An alternative, using the entire sample, is a spline polynomial approximation with different parameters on the left and right of the threshold:

$$y_{ic} = \alpha + \beta_0 \cdot G\_wins_{ic} + \sum_{k=1}^{p} \delta_k \cdot dG_{ic}^k + G\_wins_{ic} \sum_{k=1}^{p} \beta_k \cdot dG_{ic}^k + \gamma' X_{ic} + \theta_c + \varepsilon_{ic}, \quad (2)$$

where y_{ic} is the referendum turnout, or the share of "YES" votes in locality i and county c; X_{ic} is a vector of locality covariates and θ_c are county fixed effects, included in some regressions to account for county-specific victory thresholds;[32] p is the order of the victory margin polynomial(control function), which accounts for voters' preferences away from the threshold. Following the literature, the locality covariate include: 1) demographic characteristics including the locality (log) population size, the age, gender, ethnicity, education and unemployment rate by locality; 2) fiscal pre-treatment characteristics, including locality per capita revenues and expenditures; 3) pre-treatment electoral controls: the number of candidates in the 2012 local elections, the joint vote share of the top-two mayoral candidates, and the turnout in the previous impeachment referendum in 2007 (see also the Data section).[33] Standard errors are clustered at county level.

4.2 Validity analysis

The coefficient $\hat{\beta}$ provides the unbiased estimate of the impact of partisan alignment on referendum outcome y if: 1) there is a discontinuity in treatment at the zero cut-off (plausibly satisfied in this case because all winning candidates took office immediately after elections);[34] 2) potential outcomes are a continuous function in the running variable at the threshold (Hahn et al., 2001).[35] While the latter assumption is untestable, Lee

[32]Moreover, they account for the fact that counties have traditionally supported preponderantly certain political parties, and for the political alignment of the elected county council president.

[33]The electoral RD literature includes similar controls. E.g. Pettersson-Lidbom (2008) estimates the impact of left- vs. right- wing party control on economic outcomes, controlling for a number of predetermined characteristics e.g. income, population size, proportion of people below 15, and proportion of people above 65. Ferreira and Gyourko (2009) also use a RDD to estimate the difference in economic outcomes between localities with a Republican or a Democrat mayor. Their covariates include percentage white households, percentage with a college degree or more, household income, as well as predetermined fiscal outcomes in year t-1: total revenues per capita, total taxes per capita, total current expenditures per capita, total full-time employees per 1000 residents.

[34]In very few cases, mayors have given up their seat well into their mandate, so this should not affect the identification, because the outcomes are realized shortly after the elections.

[35]The RDD in elections has been used extensively in the literature, most notably to investigate incumbency advantages. A few recent studies have raised concerns with respect to the validity of the design,

(2008) has shown that this can be replaced by the milder assumption that the probability density function of the running variable should be continuous at the cut-off, implying:

i) Locality unobservables do not vary discontinuously at the cut-off $dG_i^* = 0$. However, only the continuity of the observable covariates at the threshold can be tested.

ii) The assignment mechanism is perfectly followed. This requires that candidates cannot perfectly manipulate the vote share so as to win elections.

Assumption i) requires smooth locality covariates around the threshold, which can be inferred from estimating the RD model (1) with each covariate as the dependent variable. I run a condensed version of this test: first, I predict the referendum outcome using only locality covariates as explanatory variables; then I estimate the RD model with the predicted instead of the realized outcomes on the left-hand side. The insignificant RD estimates in the online appendix Table A4 suggest we cannot reject that covariates vary continuously around the cut-off.[36] Nearly all individual RD estimates for each covariate in rural localities also support the continuity hypothesis (Table A5 in the online appendix). However, the significant differences in the share in higher education and health expenditure dispute the assumption that these races are narrow enough to ensure a good counterfactual. Nonetheless, the inclusion of these covariates in the main regressions in section 5 does not change the estimated treatment effect much. In addition, the two-candidate races estimations should provide complementary evidence to mitigate this concern.

Assumption ii) requires that mayoral candidates do not have complete control over their realized vote share in local elections. This is plausible, as the average rural locality in Romania has a population of over 3000, making perfect foresight about the precise vote counts unlikely. While some cases of electoral fraud in these elections were recorded,

particularly in the U.S. close elections, where, e.g. Caughey and Sekhon (2011) have shown that stronger competitors are more likely to win elections by a narrow margin. Eggers et al. (2015) have surveyed a large number of electoral contexts and refuted the incumbency advantage in various countries, concluding that the U.S. case is an exception, and that the RDD is a sound approach in electoral analysis, provided that these main assumptions withstand thorough testing.

[36] I present the estimates from model (1) (using a bandwidth of 5 percentage points in columns 1, 4 and 7) and model (2), without and with fixed effects (columns 2, 5, 8 and 3, 6, 9, respectively). The outcomes are predicted by including successively: i) the demographic covariates (columns 1-3); an additional subset of fiscal covariates (columns 4-6); iii) an additional subset of electoral covariates (columns 7-9). The insignificant RD coefficients across all, rural and urban localities suggest that the observable characteristics should not confound the treatment effects.

they need not invalidate the alignment treatment if the winner was to a certain extent randomly decided. I explore this using the test developed by McCrary (2008) to ascertain the continuity of the probability density function of the running variable around the cut-off.[37] A significant bunching of localities just above the victory threshold would be indicative of manipulation. Appendix Figure A1 histograms of the G-aligned vote margins show a perceptible difference in the percentage of localities with a G-aligned candidate just below and above the threshold, particularly in rural races. Figure A1 also displays the corresponding McCrary polynomial approximations of the probability density functions. The slight discontinuity is not significant, as the overlapping confidence intervals around the threshold and the test statistics suggest (t=1.03 for all, 1.21 for rural, 0.96 for urban localities). While this is reassuring, there remains some indeterminacy about whether the RD effects estimates in the next section capture electoral fraud or other unobserved differences between localities. Therefore, in section 6.2 I provide further evidence of electoral fraud at the referendum based on diagnostic tests which do not require the assumption of no perfect manipulation at the local elections.

5 Results

5.1 Main estimates

This section presents the main estimations of the impact of mayors' party alignment on outcomes in the July 2012 national referendum for the president's impeachment, identified from the closely contested local elections in June 2012.

Figure 2 illustrates the essence of my findings. The figure plots regression function approximations for the referendum outcomes (turnout and vote shares) in races against G-aligned candidates (i.e. candidates representing the government coalition). The approximations are based on the data-driven selection of bins in Calonico et al. (2015).[38]

[37] McCrary obtained smooth approximations of the running variable probability density functions to the right and to the left of the cut-off. The estimate of the discontinuity of probability density functions at the threshold can be used to test the null hypothesis of zero discontinuity.

[38] The Stata command from the robust data-driven regression discontinuity package developed by Calonico et al. (2015) is *rdbinselect*, using the evenly-spaced bins method.

The figure displays a discontinuous increase in referendum turnout above the zero vote margin threshold, in races where a G-aligned candidate narrowly wins (Figure 2a, left). The sharp positive break in turnout is even larger in rural localities - around 5 percentage points (Figure 2b, left). In urban areas, however, turnout is lower in narrowly won G-aligned localities.[39] By contrast, there is no perceptible change in vote shares around the threshold, and vote shares are fairly stable at above 85% (in favor of dismissing the president) over the entire range of G' s vote margin.

The online appendix Figure A2 displays analogous graphs for the legislative elections in December the same year. Expectedly, no discontinuity is detected in locality-level turnout, as total vote counts were irrelevant in these polls. The vote shares for G in senate elections seem to be almost smooth around the cut-off.[40] The figures point to fundamental differences in incentives to mobilize votes at the two elections, which I explore later.

The main referendum estimation results are displayed in Table 2: Panel A - all localities, Panel B - rural, and Panel C - urban. I present six RD specifications: i) OLS estimations in the tight intervals [-2.5; +2.5] and ii) [-5; +5] percentage points around the cut-off (columns 1-2 and 7-8); iii) Robust local linear approximations with the optimal bin selection as in Calonico et al. (2014) (columns 3 and 9); iv) 3rd order polynomial approximations with full samples, allowing the polynomial parameters to differ across the cut-off (columns 4-6 and 10-12); v) the latter including locality covariates (columns 5-6 and 11-12) and vi) the latter adding county fixed effects (columns 6 and 12).[41]

In Table 2, Panel A, the narrowest bandwidth estimate indicates that G-aligned localities have 3.1 percentage points higher turnout that unaligned localities. Increasing the bandwidth lowers it to a statistically insignificant 2 percentage points (column 2). The effect of mayor alignment on turnout is large and significant in the specifications that include covariates and county fixed effects (5.4 percentage points in column 6). By

[39]However, note that there is a lower density of winning G-aligned candidates in cities, and the locality covariates in Table 1 differ significantly between G-aligned and unaligned localities.

[40]The same graphs with vote shares for G in the Lower Chamber are very similar.

[41]The estimation results with varying polynomial orders are similar, and are displayed in Table 6. Note also that introducing an additional set of covariates (share illiterate, share migrants, share working in agriculture and in public administration) makes the sample slightly smaller, but the results from that sample do not change when including these covariates (available upon request).

contrast, all RD estimates of the alignment-driven difference in the"YES" vote share are close to zero and insignificant.

Panel B displays estimates from rural localities, showing larger alignment effects for referendum turnout. All RD estimates are significant, around 5.3 percentage points in the tightest interval around the threshold and 6.2 percentage points in the most complete specification. The inclusion of covariates changes the estimate slightly from 4 to 5 percentage points, and including county fixed effects brings it to 6.2 percentage points.[42] While these are probably not significant differences, the question of comparability of aligned and unaligned localities arises, since the all races where G was one of the top-two candidates are included in the sample, not just those with two competitors. Most characteristics in the rural sample in Table 1 are the same in G-aligned and unaligned localities, except the share in high education and the health expenditures (online appendix Table A5). Lower education is typically associated with less informed voters, and since G-localities have proportionally more people in higher education, implying the RD estimate without controls is likely underestimated, as confirmed in Table 2. Nevertheless, I investigate the locality comparability issue further using two-candidates races results in section 5.3.

The RD estimates from urban races in panel C show a negative and imprecisely estimated, turnout premium for G-aligned localities. However, the difference is entirely explained by municipality or county characteristics in columns (5)-(6). As anticipated by the significant differences in predetermined characteristics in Table 1, the validity assumptions do not hold in the urban sub-sample. As in the full and rural samples, the RD estimates for the share of "YES" votes are not significant.

Taken together, these estimates show a marked positive impact of partisan alignment on turnout in the referendum, driven by rural localities. This alignment effect is found neither for parliamentary elections turnout, nor for parliamentary vote shares (online appendix Table A6).[43] Given the lack of popular support for the president in 2012,

[42]The increase from column 5 to 6 is almost entirely driven by three counties. Once these counties are excluded from the sample, the estimates are more consistent across specifications and the inclusion of county fixed effects does not alter the RD estimate size. The results excluding these three counties are available upon request. These counties do not seem to differ from the rest in terms of average number of local competitors, the vote share of top two parties in local elections, nor composition of races.

[43]These polls used closed lists, meaning that in each chamber and college (part of a city and/or a group

meeting the 50% participation target would have led the *governing coalition* to a certain victory. The referendum alignment effect is therefore not surprising, being consistent with the interests of the *governing coalition* to obtain the validating quorum.[44] The more intriguing question is how this mobilization was achieved. Below I conduct some heterogeneity and diagnostic tests to highlight the role of electoral fraud and local checks and balances in the election results.

5.2 Mechanisms

A number of channels could conceivably generate the estimated turnout inflation: 1) pork-barrel spending ahead of the referendum; 2) a bias of voters in favor of the incumbent party in the locality; 3) vote buying and vote-rigging. The time window between new mayors taking office and the referendum was just over a month, lending little credibility to the first channel. With respect to the second channel, if the results were driven entirely by voters' preferences, this should have been reflected in the referendum vote shares and the legislative elections outcomes. However, the results presented in section 5.1 prove otherwise.

It is therefore plausible that electoral fraud accounts for the results. In Romania, like in many young democracies, such practice is commonplace and local networks are very important. Therefore, the assumption is that having an aligned party in local government ensures the access to the networks that operate the vote-rigging machinery. This facilitates manipulation either by vote buying or by ballot fraud. One might be sceptical about vote buying as an effective manipulation tool, since politicians and voters cannot contract on the actual vote (see Robinson and Verdier, 2013). In this setting, however, the referendum circumvents this problem, since the turnout (which is observable and enforceable) is the

of villages), the voter had one vote for the candidate from his preferred party. Hence, the voting decisions were arguably more complex than in the referendum. Since there is no minimum participation rule in legislative elections, the absence of an alignment premium for turnout is not surprising

[44]Since the president's party had all the reasons to keep the president in power, they would have had incentives to lower turnout. Treatment can thus also be defined as P-alignment. Appendix Table A7 shows that the president's party close winners have lower turnout than close runner-ups, also in rural localities. The discontinuity estimates are slightly smaller than their counterparts in Table 2 and only significant when controlling for covariates and county fixed effects. However, in this sample, most close races are between P and G, a competition which can result in lower alignment benefits, as discussed further in section 5.2. The usual validity checks hold for the P-alignment treatment.

main objective.[45] Below I investigate the evidence for this channel.

5.2.1 Evidence of electoral fraud

Most high-stakes Romanian elections are fraught with allegations of electoral fraud (see also section 2.3). National surveys and international observers report a high prevalence of vote buying and manipulation at the ballot box. For instance, Comsa and Postelnicu (2014) report an incidence of vote-buying of up to 24% in the 2014 elections. Following a 2012 impeachment referendum court investigation of 74 officials, in April 2016, the leader of the *social democrats* and senior minister Liviu Dragnea was convicted for referendum fraud by various means, including vote buying and fake ballots (e.g. for deceased persons, or people working abroad at the time).

To further substantiate this evidence I carry out a series of diagnostic tests in line with the most recent developments in electoral forensics: 1) I exploit differences between narrow referendum races in terms of demographics and norms correlated with electoral fraud; 2) I investigate anomalies in the distributions of the last digit of the referendum vote counts across polling stations; 3) I analyse the distribution of null votes; 4) I investigate discontinuities in the density of referendum turnout.

First, to explore differences in the correlates of corruption, in Table 5 I conduct a heterogeneity analysis based on: the pre-treatment share of people who worked abroad (Panel A), pre-existing stated vote-buying norms (Panel B); party incumbency (Panel C). For space considerations, I focus on the only referendum outcome that showed patterns consistent with manipulation in Table 2: referendum turnout in rural localities. The results in panel A suggest that localities with migrant shares above the median 5% display larger and significant alignment effects in the expected directions in two out of three specifications.[46] The same holds for localities with more widespread vote buying practices. I use the 2011 Romanian Electoral Surveys data which asked individuals about electoral practices, including vote-buying. Not all localities are represented, hence I construct a

[45] See also (Nichter, 2008) for a similar discussion on why turnout buying is more likely to occur than vote buying.

[46] Note that the results are opposite to what one would expect in the absence of manipulation: an additional migrant is one less vote, so turnout should naturally be smaller.

county level measure of vote-buying norms. The share of people who report vote buying ranges from 6.25% to 87% with a standard deviation of 21%, which offers a fair amount of variation. I divide counties by the median share of voters who admitted to have been asked to sell their vote (25%). The alignment premium seems higher in counties where vote buying is perceived as a more common practice (and insignificant in two specifications in low-vote buying prevalence counties). In panel C, the RD estimate is consistently larger in localities where G was not in office in the previous mandate.[47] This goes against a mechanisms where voters' reactions to past pork-barrel spending of former G mayors would explain the turnout results. One potential explanation for this result is that the new G leadership needs to prove their loyalty to the party, which could attract future grants that boost their re-election chances. This may also flag a tendency to target swing voters (à la Lindbeck and Weibull, 1987), rather than strongly supportive localities where G is incumbent.

Second, I carry out diagnostic tests based on the frequency of occurrence of every possible last digit in vote counts at polling stations. These tests have been developed by Bebber and Scacco(2012), adapted after Mebane(2006, 2008) and have become a popular tool in electoral forensics. The intuition is that in fair elections, the occurrence of the last digit in the vote count at each polling station should be random. Thus, each digit 0-9 should be uniformly distributed with a mean frequency of 10% (provided the vote count is more than two-digits long). Psychology research shows that humans cannot perfectly reproduce randomness. Hence, if the last digit distribution displays frequencies significantly different from 10%, there are reasons to suspect vote tampering has taken place. Figure 3 shows side-by-side the last digit distributions for the total vote count (voter turnout) and for the valid votes count reported by polling stations. There are around 17,000 polling stations in my sample, and over 85% of them have 3- or 4- digit long vote counts. These distributions are plotted for all localities (Figure 3a), rural and urban localities (Figures 3b and 3c) and 95% confidence bands are also displayed.

[47]To capture the interaction between G's victory and G incumbency, here the treatment is 1 if G wins the race and the locality had a G mayor before 2012, and 0 if G loses the race regardless of party incumbency in their locality; in the case of non-incumbency, the treatment is 1 if G wins and the locality had a mayor from a different party before 2012.

Interestingly, there appear to be anomalies in the last digit distribution only for the case of total turnout by polling station. The digits "0" and "3" appear significantly more often than expected ("0" in rural localities and "3" in urban ones), while the digits "7" and "9" appear less often than expected. There are no significant outlier frequencies for the last digits in the count of valid votes (which are generally closer to the expected value of 10%). In Figure 4 I also look at these distributions separately by localities with G mayors (Figure 4a) and localities with other mayors (Figure 4b). The high frequencies for low digits are representative of the former, while the low frequencies for high digits occur in the latter. This is consistent with some degree of ballot rigging in both G and non-G held localities. This could also be consistent with the incentives for G to boost turnout (hence the scope for stuffing ballots to reach round numbers), and for non-G to reduce turnout (hence destroy ballots when the count is high, which a high last digit would indicate).

Thus, total vote counts present anomalies that valid vote counts do not. Given that their difference is made up of null votes (votes considered invalid, e.g. due to the misplacement of the stamp), these null votes may have been an important manipulation tool. In Figure 5a I present scatter plots of all the polling stations, with the proportion of null votes (out of the total vote counts) on the Y-axis and turnout on the X-axis, separately for G and non-G localities. If ballot boxes were stuffed with null votes to boost turnout, there should be a higher proportion of null votes associated with high turnouts for the interested party (G). Indeed, there is a higher concentration of polling stations with high shares of null votes around and above the 50% turnout threshold in G localities (going up to a sizeable 30% null vote share). The latter also have a higher density of polling stations with null votes to the right of the threshold, compared to non-G localities. This is also reflected in the null vote scatters in rural localities. The pattern is consistent with a systematic increase in turnout by G, through the introduction of null votes in the ballot box.

Lastly, in Figure 6 I display the McCrary density discontinuity plots for the referendum turnout at polling station level, for all localities (Figure 6a) and for rural localities

(Figure 6b). I further separate the graphs for localities controlled by G and non-G, respectively.[48] The plots provide tests of density discontinuity around the 50% referendum turnout threshold. While this was a national target, it was arguably passed down to the polling stations (as the evidence uncovered in the post-referendum court trial suggests). A significant discontinuity in density around the 50% mark would indicate sorting around the threshold, and hence electoral fraud. The only statistically significant discontinuity is found in the turnout in G-controlled rural localities (the discontinuity statistic is 2.59, larger than the critical value of the 95% confidence level). This is an additional evidence of artificial increases in turnout, in line with the main results in Table 3 and the above diagnostic tests.

Taken together, these tests strengthen the evidence for electoral fraud underlying the referendum result differences between G-aligned and unaligned localities. Most evidence points to electoral manipulation by G, in line with their incentives to boost turnout, but it does not rule out some manipulation by other parties in non-G localities (as the last digit tests show). The digit tests and the null votes plots suggest that the most conspicuous mechanisms for electoral manipulation was ballot stuffing with invalid papers. However, according to the heterogeneity analysis, vote buying/intimidation as an alternative way to boost or depress turnout remains a possibility.

5.2.2 The role of checks and balances

Electoral fraud requires that the party in office has access to the vote rigging apparatus. However, the other parties with conflicting interests, particularly the challengers in the mayoral race, may block this access. This points to the role of local competition, ensuring checks and balances in the electoral process. Exploring the varying degrees of interest in the referendum from the different parties, in Table 4 I estimate the baseline RD model in sub-samples of mayoral election races between: i) G and P (panel A); ii) G and any parties except P (panel B); iii) G and *liberals/conservatives* (panel C). To reiterate, P had high incentives to monitor G, and vice-versa, while *the liberals/conservatives* may have

[48]The sample of excludes the 2% of roughly 17000 polling stations which have above 100% turnout, mostly in localities which are tourist resorts, and therefore receive more voters than are registered.

been indifferent or mildly interested in helping G (as evident in appendix Table A1).

The results in the three panels are fairly different: in races between G and P the effects are very small and insignificant (except in the specification with county fixed effects). By contrast, when G wins over parties unrelated to P, in particular the *liberals/conservatives*, G obtains a significant turnout advantage. The alignment bonus is 10 percentage points in narrow races between G and *liberals/conservatives* (panel C, column 1). This is in line with the strong incentives of P, and weak incentives for other parties, especially the allied ones, to keep the governing coalition in check. While these results do not have a causal interpretation they highlight the role of checks and balances and competition in vote delivery.[49]

5.3 Robustness Checks

In this section I return to the concerns about sample selection and treatment definition enunciated in section 3. The main estimates are based on a sample of all races where a G-aligned candidate came first or second, regardless of how many other candidates there are. This choice reflects a trade-off between internal validity (as I risk inducing sample selection based on voter preferences) and precision of the estimates (larger sample). The risk, however, is that a narrowly won and narrowly lost race by G candidates may actually be quite different in unobserved characteristics (e.g. G wins with 47% to 45% votes and loses with 35% to 37%; the overall difference in support for G in the two races is 12 percentage points).

In what follows I describe various sensitivity and robustness checks, all focused mainly on the rural samples at the referendum, where the main alignment estimate was identified.

Firstly, I run the RD regressions in a restricted sample of rural localities where only two (or up to three) candidates competed (an approach used in Brollo and Nannicini, 2012). The results shown in Table 5 confirm that the estimates are qualitatively similar to the baseline results, although imprecisely estimated due to the small sample size. These small sample estimates are generally larger in magnitude than the main estimates in Table

[49]By design, the electoral RDD picks up effects in close competitions. Hence, the RDD may underestimate the alignment effects, which might be even larger away from the victory threshold.

2, except for the county fixed effects specification (6) in Panel A. This, along with the descriptives in the online appendix Table A2 displaying lower education levels in the small sample, raise concerns that the two-candidate races might themselves form a selected sample. Thus these estimates might be (potentially upward) biased. A complementary approach to use even more of the original sample is to also include races where a G-aligned candidate came third (an approach used in Migueis, 2013). Indeed, this reclaims roughly 200 additional observations compared to the baseline sample and the estimates are similar, only slightly larger than those in Table 2 (Table 5, panel C).

Secondly, I report the baseline RD estimates including control functions of varying polynomial orders, and the estimates in Table 6 are consistent across all the different specifications. In addition, there is a concern that excess vote shares for G in local elections may have automatically generated a larger turnout if G supporters are politically more active. The parliamentary election turnout results suggest this is not the case, but as an additional test to dismiss a mechanical effect I proceed as follows: I predict referendum turnout using the local election vote shares, and I introduce this predicted variable as a control in the baseline RD regressions. The RD alignment estimate is reassuringly unaffected.

Finally, I revisit the treatment definition, where I now include the winning independent *liberals/conservatives* in the treatment rather than the control group. This automatically excludes narrow races between *social-democrats* and *liberals/conservatives*. As shown in the appendix Table A1 and also in the heterogeneity tests, precisely these races displayed a large difference in turnout between winning and losing G localities. Hence, the redefined treatment is expected to deliver lower RD estimates than the baseline specifications. Table 7 results show smaller effects on referendum turnout, significant only upon inclusion of controls and county fixed effects.[50] Panels B and C in Table 7 look at the redefined treatment effect on parliamentary outcomes, which remains insignificant. Overall these results again suggest that races between the parties within the governing coalition are the

[50]I also redo the estimations with the original treatment, but only for localities where no candidate represented the coalition parties separately (perfect alignment between local and national parties). The results are very similar to those in Table 7 and are available upon request.

main drivers of the alignment effect.

6 Persistence of alignment: government transfers and following elections

6.1 Government transfers

The electoral advantages of alignment might fuel the political *quid pro quo* invoked in the intergovernmental transfers literature. An increasing number of studies have shown that transfers to local governments unequivocally follow the party in power. Accordingly, in this section I investigate the alignment effect on local government revenues after the 2012 local elections.[51]

In Table 8 I report RD estimates from the baseline specifications for rural localities, where the dependent variables are: Total locality per capita revenue (panel A); Locality-level per capita revenue from local taxation (own income, panel B); Locality-level per capita transfers for road infrastructure (panel C); Locality-level subsidies per capita (panel D). The data from 2012 is reported in columns (1)-(3) and from 2013 in columns (4)-(6). The RD alignment estimates are almost all positive, but significant only for total locality revenue per capita in 2013, which, interestingly, is much larger than in 2012. This is in stark contrast to total revenue in 2011, the pre-treatment year, when if anything, they were lower for localities won over by G in later local elections (see appendix Table A3). This is not driven by an increase in local tax revenue, which is not significantly larger in aligned localities. The difference can only be explained by government subsidies and discretionary transfers. Since I have only local budget data on subsidies, I look at the two potentially most visible to voters: roads and other subsidies. These do not display significant alignment effects (although at least in average terms, the balance of road subsidies has changed in favour of aligned localities relative to 2011). It is perhaps not surprising that the alignment premium for total revenue is smaller in 2012 than in 2013,

[51]Data on discretionary government transfers is not available for 2012-2013, therefore I use certain categories of local revenues, which are redistributive and discretionary in nature.

since the G government only took office in May and the reshuffling of local administration seats only in June.

These results, at least on total revenue per capita, suggest a gradual reversal of the previous distribution of government funds, favouring G-aligned localities from 2012 onwards. Note also that these might underestimate the alignment bonus if transfers are strategic, and higher in the years just before new local elections, i.e. after 2013.[52]

6.2 Parliamentary elections

The core result in this paper is that local officials deliver votes for their parties in national polls by means of electoral fraud. This effect is not detected in legislative elections, where contracting the voter's decision would be crucial to such a strategy for securing support. However, to the extent that the referendum turnout was inflated by buying the voters' participation, this could indirectly impact the results in subsequent elections. This indirect effect might follow from activating voters, inducing habit-formation in the decision to come to the polls, thereby anchoring turnout (Melton, 2014; Górecki, 2013).

OLS regressions in appendix Table A8 indicate that the larger average turnouts in aligned localities in legislative elections vanish when controlling for referendum turnout. However, Table A6 showed insignificant direct alignment effect on parliamentary elections outcomes identified from a RDD. Therefore, I investigate whether the referendum turnout, instrumented by the alignment treatment in narrow local races (i.e. the RD in Table 2), significantly influenced parliamentary turnout. Hence, I estimate a two stage least squares model as in Van Der Klaauw (2002), where the baseline RD is the first stage, and the predicted referendum turnout $E[Turnout_i|dG_i] = \beta G\_wins_i + f(dG_i)$ is then used as the explanatory variable in the second stage:

$$Yparl_i = \gamma_0 + \gamma_1 E[Turnout_i|dG_i] + k(dG_i) + \varepsilon_i, \tag{3}$$

where $Yparl_i$ is the parliamentary election turnout or the vote share for G in locality i, $k(dG_i)$ is a control function for voters' preferences away from the local election victory

[52] This data is currently unavailable.

threshold.[53] The assumptions for the narrowly-won alignment to be a good instrument are: i) instrument relevance (this assumption holds, as seen in the first stage reported in Table 2); ii) instrument validity; i.e. alignment effects work exclusively through their impact on referendum turnout. The insignificant RD results in Table A6 are reassuring in this respect, although the alignment with local party in government may also create a comparative advantage in elections, for instance through more effective campaigning. The IV estimations will yield an unbiased estimate only if the exclusion restriction conditional on locality characteristics holds.

Table 9, columns (4)-(6), displays the 2SLS estimates of the effect of referendum turnout on parliamentary election turnout and vote shares. The estimates are all significant, with 0.27 percentage points excess turnout and 0.4 percentage points increase in G vote share for 1 percentage point additional referendum turnout. Overall, the results support the persistence, or "stickiness" of referendum turnout. The implication is that the referendum may have been a successful mobilization exercise for G, whose excess voters shifted the G vote share in the legislative elections, albeit by a small margin.

7 Concluding Remarks

This paper set off to examine the means available to central politicians in young and fragile democracies, to secure grass-roots support in elections. My findings suggest that party alignment between local and central government is key in facilitating electoral fraud to manipulate the elections. This is evident from a regression discontinuity design in which I compare a president dismissal referendum outcome in narrow local elections in Romania in 2012, combined with electoral forensics tests that display vote counts anomalies across polling stations distributions.

In the case of the impeachment referendum, the minimum 50% turnout requirement and low popularity of the president under scrutiny offered a particularly low-cost route to manipulating the election outcomes. Working in tandem with local officials granted the

[53]Van Der Klaauw (2002) examine the impact of college financial aid offers on students' decisions to enrol into university, exploring financial aid offer discontinuities along a calculated college aid score.

central politicians the access and logistics to effectively stuff the ballots with null votes. These findings raise concerns about the legitimacy of the political capital that local officials provide for the national parties, particularly in young inexperienced democracies. In this context, this practice nearly invalidated one of the most direct forms of democracy.

But this is not the tale of one referendum. This is just an illustrative setting where the politicians can credibly buy the voters' support or circumvent it altogether at a very low cost. A setting such as this makes it easier to detect electoral fraud, but it does not rule it out in other contexts. Governments in young and fragile democracies across the globe prove time and time again an extraordinary ability to disguise the autocratic legacy of vicious self-serving habits as legitimate democratic exercises. African elections are closely monitored by international observers, but their electorates are systematically manipulated in the pre-election period. Incumbent politicians in South-East Asia spare no vote buying efforts in protecting their incumbency advantage. Ironically, in Russia's most recent parliamentary election the vote-rigging practices were caught live on webcams installed at polling stations in the name of transparency. And where everything else fails, politicians resort to reinventing the rules. Following the unsuccessful attempt to oust the president in the 2012 referendum, the Romanian parliament voted to reduce the future referenda participation requirement to 30%.

This does not imply that all attempts at upholding democracy in such difficult environments are fated never to succeed. The challenge, however, is to educate and arm voters with the will and the tools to limit the politicians' prerogatives. This opens new paths to a vibrant research agenda on voter information, political participation and the use of science and technology to facilitate and lend them the much-needed efficiency in abolishing the status-quo.

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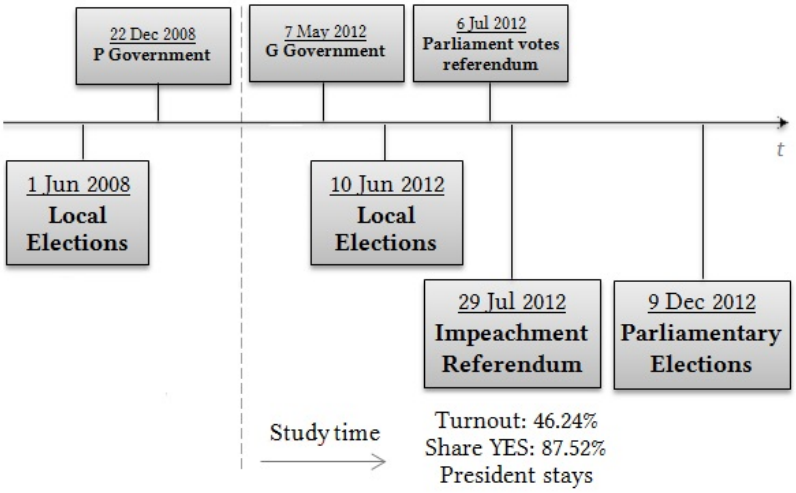
Figures and Tables

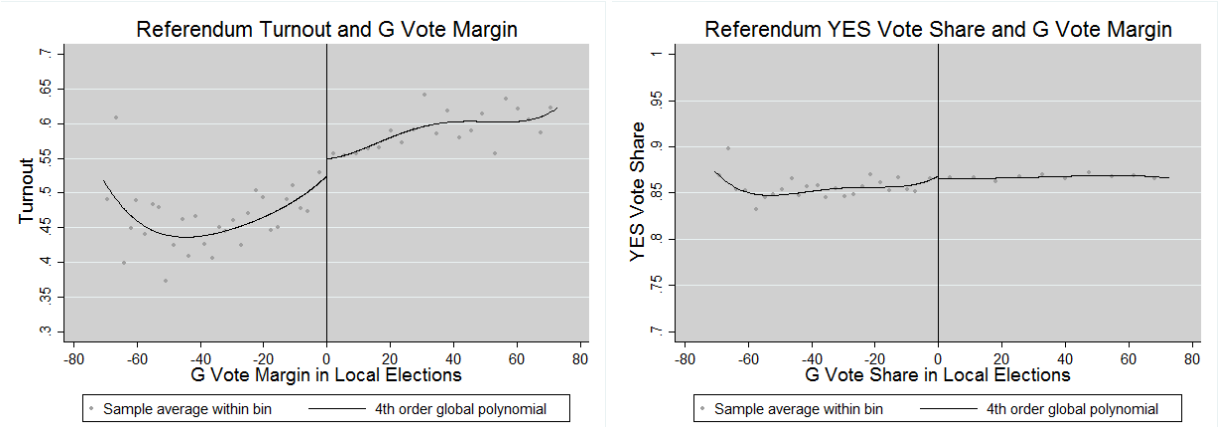
Figure 1 Timeline of electoral events



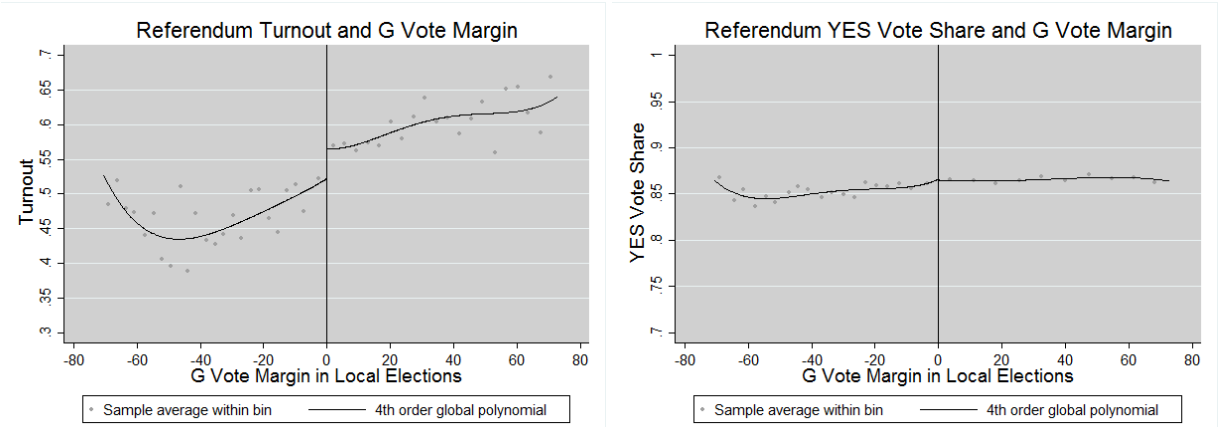
Note: The P Government was formed by the president's party (*the democrat-liberals*). The G Government was formed by the *social-liberal union*, the main opponents to the president's party rule.

Figure 2: G-alignment and referendum outcomes

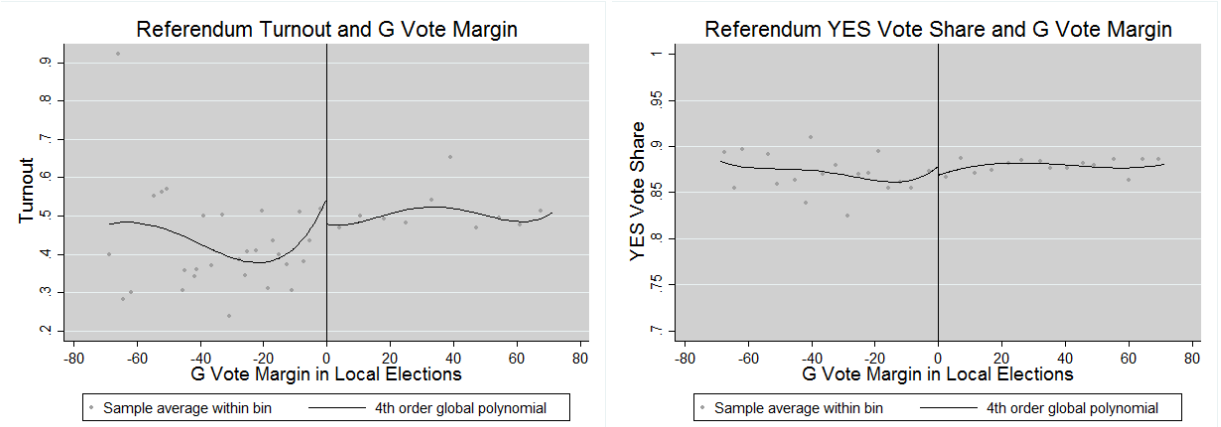
(a) All localities



(b) Rural localities



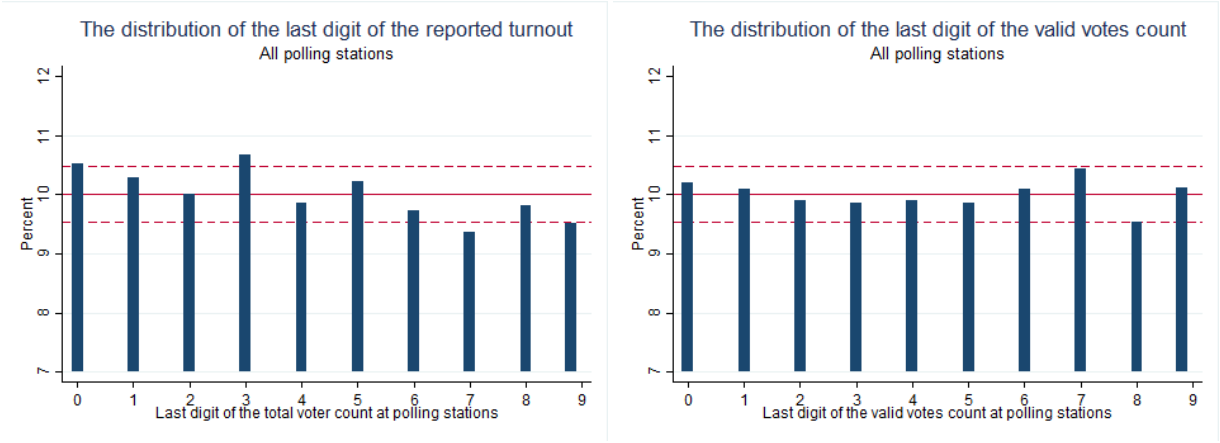
(c) Urban localities



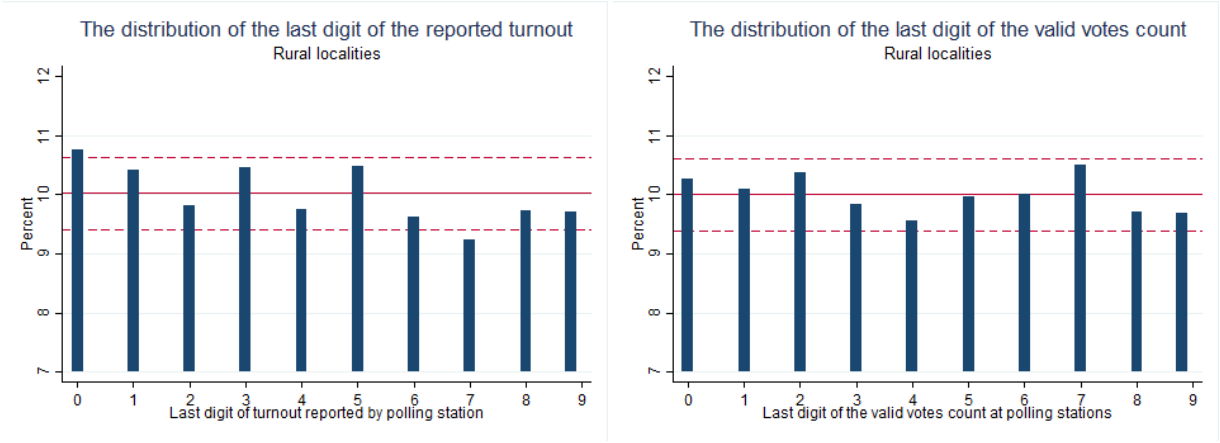
Note: The figure displays the polynomial approximations of the referendum turnout (left) and "YES" vote share (right) plotted against the running variable on the X-axis (Victory Margin for G in races against G).

Figure 3: Distributions of the last digit in the total vote count (turnout) and the valid vote count by polling stations

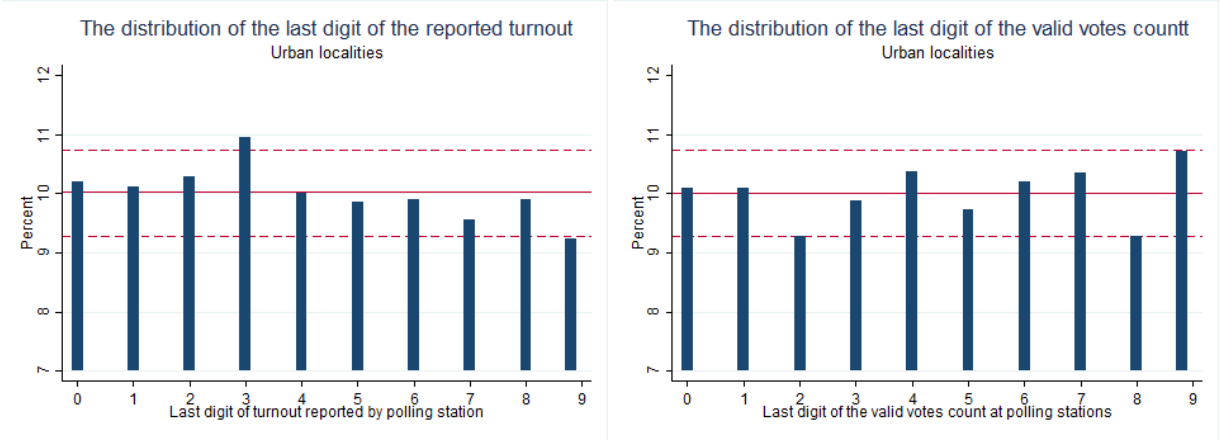
(a) All localities



(b) Rural localities



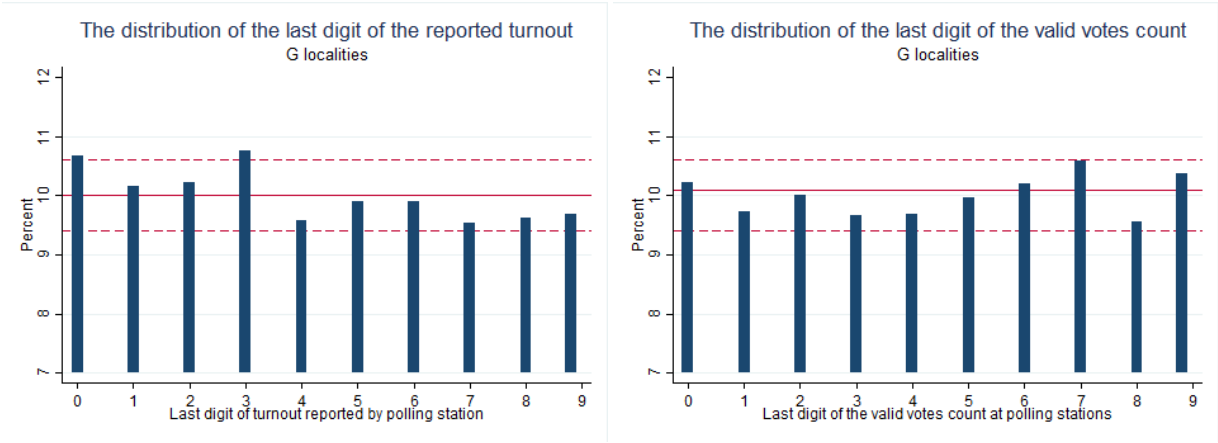
(c) Urban localities



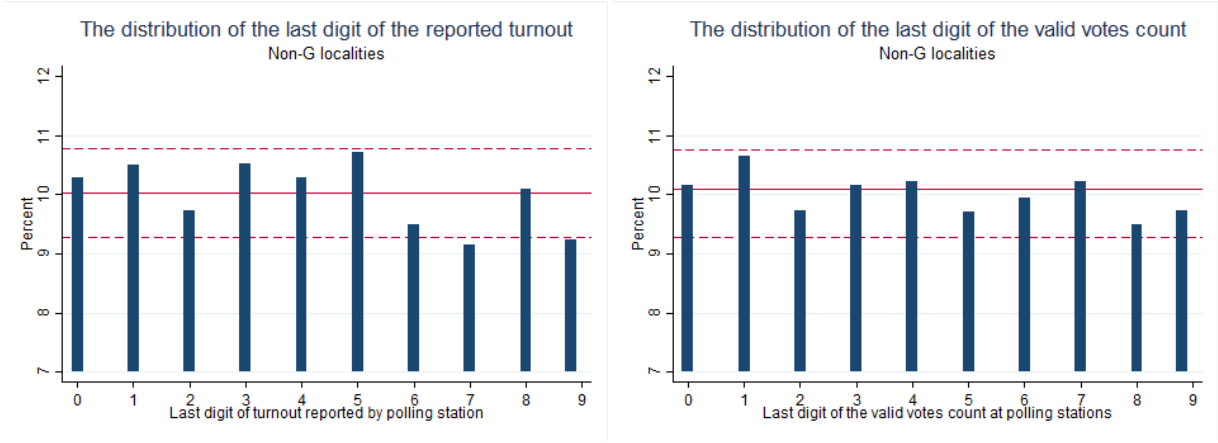
Note: The figure displays the distribution of the last digit of the referendum turnout (left) and the valid votes count (right) across polling stations. This distribution is based on the sample of polling stations with a 3- or 4- digit vote count (which make up more than 85% of the population). The last digit distributions are displayed for the entire sample in Figure 3(a), for polling stations in rural localities in Figure 3(b), and urban localities in Figure 3(c). The solid horizontal line is the expected frequency for each digit. The dashed horizontal lines mark the 95% confidence interval. In normal elections the distribution of the last digit of vote counts should be uniform with an average of 10% frequency for each of the digits 0-9. Digit frequencies outside of the confidence interval indicate the presence of vote count manipulation.

Figure 4: Distributions of the last digit in the total vote count (turnout) and the valid vote count by polling stations in G and non-G localities

(a) G localities



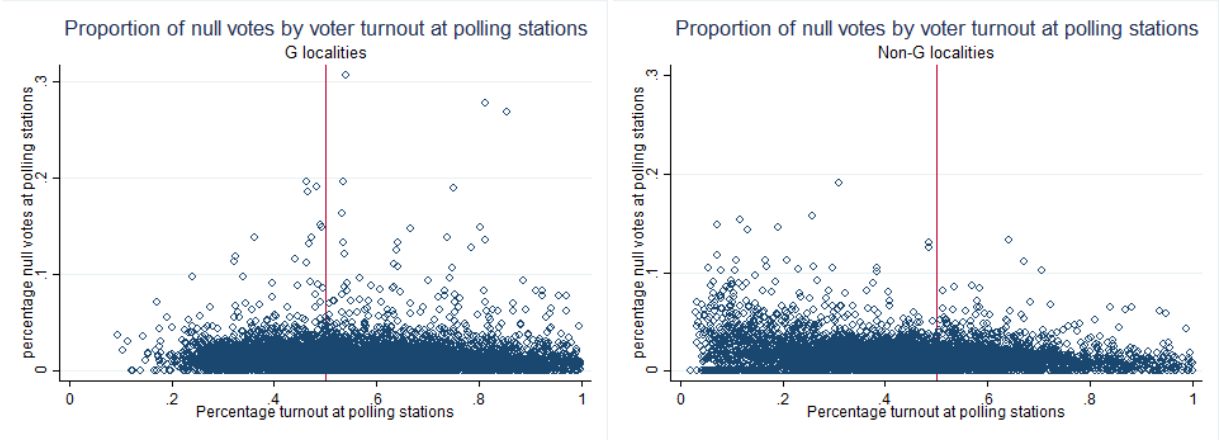
(b) Non-G localities



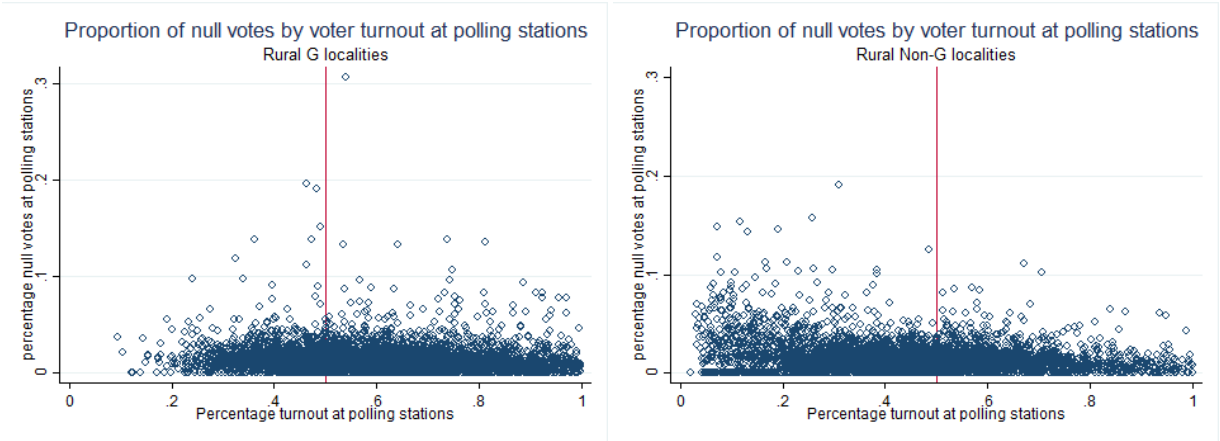
Note: The figure displays the distribution of the last digit of the referendum turnout (left) and the valid votes count (right) across polling stations. This distribution is based on the sample of polling stations with a 3- or 4- digit vote count (which make up more than 85% of the population). The last digit distributions are displayed for the entire sample of polling stations in localities with a G mayor in Figure 4(a) and for polling stations in localities with a non-G mayor in Figure 4(b). The solid horizontal line is the expected frequency for each digit. The dashed horizontal lines mark the 95% confidence interval. In normal elections the distribution of the last digit of vote counts should be uniform with an average of 10% frequency for each of the digits 0-9. Digit frequencies outside of the confidence interval indicate the presence of vote count manipulation.

Figure 5: Distribution of null votes by percentage turnout reported at polling stations

(a) All localities



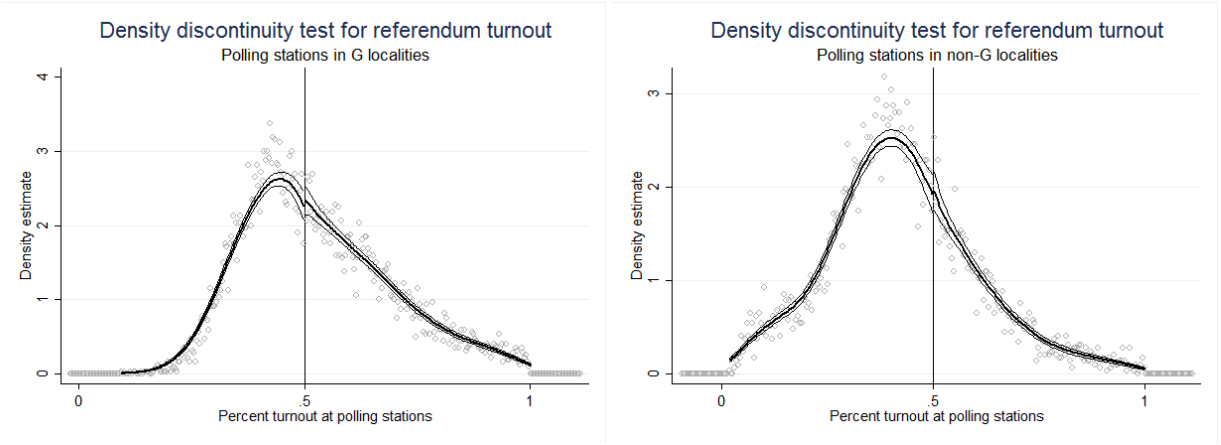
(b) Rural localities



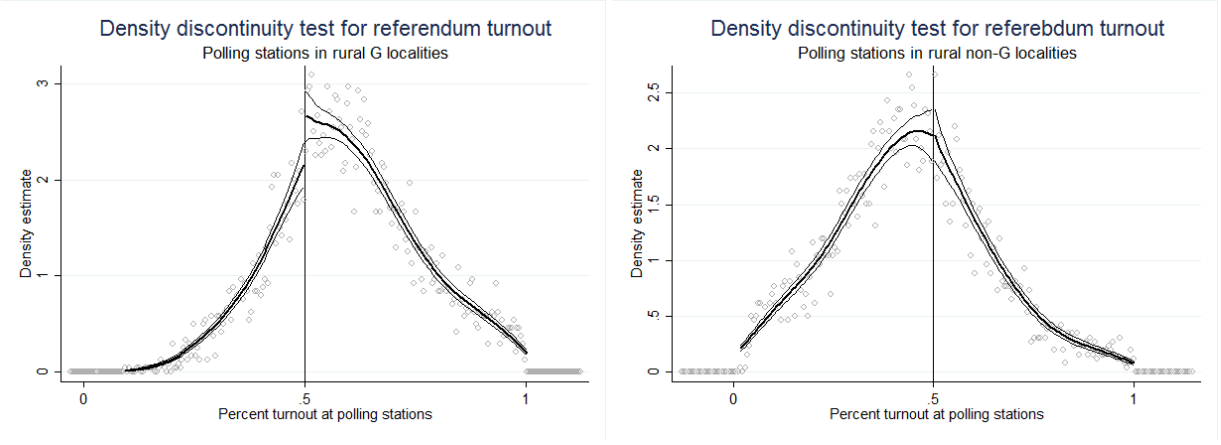
Note: The figure displays a scatter of null vote (expressed as percentage of total votes, on Y-axis) against percentage turnout (on the X-axis) at polling stations in localities with a G-mayor (right) and non-G mayor (left). The vertical line intersecting the X-axis at 0.5 marks the national target turnout for the referendum to be valid. For display purposes, the sample excludes polling stations with turnout larger than 100% (these represent 2% of all polling stations, and are mostly tourist resorts).

Figure 6: Distribution of null votes by percentage turnout reported at polling stations

(a) All localities



(b) Rural localities



Note: The figure displays the density discontinuity test developed by McCrary(2008), applied here to referendum turnout reported at each polling station. Turnout is represented on the X-axis and the corresponding density on the Y-axis. The vertical line intersecting the X-axis at 0.5 marks the national target turnout for the referendum to be validated. A discontinuity in density around at the 50% turnout mark is indicative of sorting around the threshold, and hence consistent with electoral manipulation. All discontinuity estimates around the threshold are insignificant, except for the discontinuity estimate in rural G localities (Figure 6b, left), which is significant at 5% significance level (discontinuity estimate 0.2, standard error 0.077, yielding a test statistic of 2.59). For display purposes, the sample excludes polling stations with turnout larger than 100% (these represent 2% of all polling stations, and are mostly tourist resorts).

Table 1: Descriptive statistics. Samples of races against G within 5% margins around cut-off

	All sample			Rural			Urban		
	G	**non-G**	**P-value**	**G**	**non-G**	**P-value**	**G**	**non-G**	**P-value**
Panel A: Outcomes									
Turnout	0.553	0.528	0.176	0.566	0.531	**0.088**	0.479	0.514	0.403
Share YES	0.864	0.868	0.457	0.863	0.866	0.613	0.868	0.877	0.412
Turnout Parliamentary Elections	0.435	0.430	0.694	0.437	0.429	0.514	0.423	0.438	0.526
G Vote Share in Senate	0.611	0.613	0.892	0.609	0.614	0.775	0.624	0.612	0.677
Panel B: Covariates									
No. candidates in local elections	5.222	5.244	0.917	5.036	5.076	0.850	6.292	6.045	0.700
Sum % votes top 2 local candidates	79.985	79.669	0.851	80.210	79.722	0.795	78.688	79.413	0.855
Turnout referendum 2007	44.783	44.478	0.529	44.751	43.974	0.544	44.965	46.882	0.450
Log Population	8.152	8.177	0.819	7.926	7.943	0.826	9.453	9.293	0.657
Share adult population	0.768	0.772	0.523	0.767	0.768	0.869	0.773	0.788	0.191
Share over 65	0.203	0.209	0.400	0.213	0.219	0.472	0.145	0.163	**0.045**
Share males	0.496	0.495	0.884	0.497	0.497	0.933	0.488	0.488	0.827
Share high education	0.050	0.048	0.775	0.040	0.034	**0.082**	0.106	0.117	0.586
Share high school	0.163	0.160	0.663	0.149	0.141	0.261	0.247	0.251	0.795
Share Romanians	0.883	0.880	0.884	0.885	0.889	0.850	0.868	0.840	0.464
Unemployment rate	0.051	0.053	0.730	0.054	0.056	0.743	0.033	0.037	0.510
Per capita revenue	1390.731	1405.986	0.890	1400.887	1338.739	0.624	1332.330	1726.939	**0.022**
Per capita own revenue	483.665	455.977	0.443	447.069	409.855	0.313	694.090	676.106	0.850
Per capita roads funds	11.005	14.989	0.506	11.980	16.919	0.485	5.396	5.780	0.932
Per capita subsidies	113.401	149.042	0.335	125.291	109.617	0.690	45.035	337.206	**0.004**
Per capita expenditures	1281.949	1346.232	0.505	1276.639	1267.771	0.935	1312.485	1720.706	**0.019**
Per capita expenditures education	364.864	375.396	0.631	337.243	345.181	0.715	523.681	519.604	0.945
Per capita health expenditures	7.884	12.802	0.101	6.278	10.699	0.184	17.117	22.839	0.358
Per capita public expenditures	137.354	162.911	0.304	138.022	137.195	0.975	133.511	285.646	**0.025**
N	162	127		138	105		24	22	

Note: The table displays the comparison in means in outcome variables (Panel A) and locality characteristics (Panel B) between localities aligned and unaligned with G, for all, rural and urban localities within 5 percentage points victory margins. The p-values for the differences in means tests are reported (significant differences marked in bold).

Table 2: Alignment and referendum outcomes. RD Estimates

	Referendum Turnout						**Share YES**					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	OLS	OLS	Local LR	Poly.	Approx.	Full sample	OLS	OLS	Local LR	Poly.	Approx.	Full sample
						Panel A: All localities						
G wins	0.031	0.025	0.020	0.021	0.037***	0.054***	-0.005	-0.005	-0.006	0.002	0.003	0.006
	(0.021)	(0.015)	(0.021)	(0.020)	(0.013)	(0.014)	(0.008)	(0.006)	(0.007)	(0.007)	(0.008)	(0.007)
Obs.	143	289	1,057	2,386	2,386	2,386	143	289	921	2,386	2,386	2,386
						Panel B: Rural localities						
G wins	0.053**	0.035*	0.041*	0.040*	0.050***	0.062***	-0.002	-0.004	-0.002	0.003	0.004	0.006
	(0.024)	(0.018)	(0.022)	(0.022)	(0.016)	(0.015)	(0.009)	(0.007)	(0.008)	(0.008)	(0.009)	(0.008)
Obs.	118	243	949	2,116	2,116	2,116	118	243	942	2,116	2,116	2,116
						Panel C: Urban localities						
G wins	-0.078	-0.034	-0.106**	-0.079	-0.029	0.017	-0.016	-0.009	-0.015	-0.007	-0.005	-0.001
	(0.051)	(0.040)	(0.053)	(0.049)	(0.054)	(0.035)	(0.012)	(0.011)	(0.013)	(0.013)	(0.011)	(0.011)
Obs.	25	46	113	270	270	270	25	46	119	270	270	270
County FE	No	No	No	No	No	Yes	No	No	No	No	No	Yes
Controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Specification	[-2.5; +2.5]	[-5; +5]	CCT Opt. h	3rd Ord. Poly.	3rd Ord. Poly.	3rd Ord. Poly.	[-2.5; +2.5]	[-5; +5]	CCT Opt. h	3rd Ord. Poly.	3rd Ord. Poly.	3rd Ord. Poly.

Notes: The table displays RD estimates of the effect of *governing coalition* (G) alignment on referendum turnout (columns 1-6) and the "YES" vote share (columns 7-12). Results are based on all localities (Panel A), rural (Panel B) and urban (Panel C). Estimates from simple linear regression in a small interval around the cut-off in columns (1)-(2) and (7)-(8). Estimates from local linear regression using the Calonico et al. (2014) robust bias-corrected confidence intervals in columns (3) and (9). Estimates using polynomial approximations on the full sample, without controls, with controls and with county fixed effects in columns (4)-(6) and (10)-(12). The controls in columns 5-6 and 11-12 include demographic, labor market, fiscal and electoral characteristics of the localities. Standard Errors clustered at county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Heterogeneity: migration, vote buying and incumbency. G races in rural localities

	Turnout					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Poly.	Approx. Full sample	OLS	Poly.	Approx. Full sample
	Panel A: Migration					
	Share work abroad <5%			Share work abroad >= 5%		
G wins	-0.008	0.031	0.070**	0.051**	0.040	0.061***
	(0.027)	(0.035)	(0.029)	(0.025)	(0.028)	(0.020)
Obs.	98	906	906	145	1,210	1,210
	Panel B: Vote Buying Norms					
	County share vote buying <= 25%			County share vote buying > 25%		
G wins	0.025	0.012	0.050**	0.052***	0.068**	0.073***
	(0.026)	(0.031)	(0.020)	(0.015)	(0.025)	(0.023)
Obs.	155	1,283	1,283	88	833	833
	Panel C: Incumbency					
	G mayor in 2008			Other mayor in 2008		
G wins	0.014	0.008	0.043*	0.040*	0.056**	0.066***
	(0.028)	(0.035)	(0.022)	(0.022)	(0.025)	(0.017)
Obs.	152	1,371	1,371	180	1,401	1,401
Controls	No	No	Yes	No	No	Yes
County FE	No	No	Yes	No	No	Yes
Specification	[-5; +5]	3rd Ord. Poly.	3rd Ord. Poly.	[-5; +5]	3rd Ord. Poly.	3rd Ord. Poly.

Notes: The table displays RD estimates of the effect of *governing coalition* (G) alignment on referendum turnout in rural localities, by: the share of migrants abroad (Panel A) below median in columns (1)-(3) and above median columns (4)-(6); the share who report vote buying (Panel B) below median in columns (1)-(3) and above median columns (4)-(6); whether G was in office before 2012, in columns (1)-(3), or not, in columns (4)-(6). Estimates from simple linear regression in a small interval around the cut-off in columns (1) and (4). Estimates using polynomial approximations on the full sample, without controls, with controls and with county fixed effects in columns (2)-(3) and (5)-(6). The measure of perceived vote prevalence is computed based on the Romanian Electoral Surveys 2011 questions on people's observations or perceived incidence of vote buying. Standard Errors clustered at county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Heterogeneity: different parties' races (rural)

	Turnout		
	(1)	(2)	(3)
	OLS	Poly. Approx.	Full sample
	Panel A: Races between G and P		
G wins	0.028	0.007	0.052***
	(0.022)	(0.027)	(0.016)
Obs.	161	1,359	1,359
	Panel B: Races between G and non-P		
G wins	0.044	0.081***	0.058**
	(0.031)	(0.028)	(0.025)
Obs.	82	757	757
	Panel C: Races between G and L-C		
G wins	0.100*	0.144**	0.039
	(0.049)	(0.057)	(0.030)
Obs.	39	322	322
Controls	No	No	Yes
County FE	No	No	Yes
Specification	[-5; +5]	3rd Order Poly.	3rd Order Poly.

Notes: The table displays RD estimates of the effect of *governing coalition* (G) alignment on referendum turnout in rural localities in different close races: between G and P (Panel A), between G and other parties except P (Panel B) and between G and the *liberals/conservatives* (Panel C). Estimates from simple linear regression in a small interval around the cut-off in column (1). Estimates using polynomial approximations on the full sample, without controls, with controls and with county fixed effects in columns (2)-(3). Standard Errors clustered at county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Alignment and referendum turnout in rural races. Robustness to race structure

	Turnout					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	Local LR	Poly.	Approx.	Full sample
	Panel A: Two-candidates races in rural localities					
G wins	0.125*	0.106*	0.062	0.100	0.127	0.018
	(0.052)	(0.049)	(0.069)	(0.091)	(0.081)	(0.093)
Obs.	6	9	63	150	150	150
	Panel B: Two- and three-candidates races in rural localities					
G wins	0.017	-0.016	0.056	0.043	0.088**	0.104**
	(0.068)	(0.057)	(0.059)	(0.053)	(0.040)	(0.047)
Obs.	18	34	230	604	604	604
	Panel C: Races where G is in top three candidates in rural localities					
G wins	0.048**	0.034*	0.047**	0.044**	0.058***	0.068***
	(0.023)	(0.017)	(0.020)	(0.021)	(0.015)	(0.015)
Obs.	120	247	934	2,301	2,301	2,301
County FE	No	No	No	No	No	Yes
Controls	No	No	No	No	Yes	Yes
Specification	[-2.5; +2.5]	[-5; +5]	CCT Opt. h	3rd Ord. Poly.	3rd Ord. Poly.	3rd Ord. Poly.

Notes: The table displays RD estimates of the effect of *governing coalition* (G) alignment on referendum turnout in rural localities, in different races: two-candidate races (Panel A); three-candidate races (Panel B); nearly all G races (including those where G came third, Panel C). Estimates from simple linear regression in a small interval around the cut-off in columns (1)-(2). Estimates from local linear regression using the Calonico et al. (2014) robust bias-corrected confidence intervals in column (3). Estimates using polynomial approximations on the full sample, without controls, with controls and with county fixed effects in columns (4)-(6). The controls in columns 5-6 and 11-12 include demographic, labor market, fiscal and electoral characteristics of the localities. Standard Errors clustered at county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Alignment and referendum turnout. Alternative polynomials

	Turnout					
	(1)	(2)	(3)	(4)	(5)	(6)
	Poly. Approx. Full sample					
	Panel A: All localities					
G wins	0.055***	0.066***	0.016	0.050***	0.025	0.041**
	(0.011)	(0.008)	(0.017)	(0.010)	(0.020)	(0.016)
Obs.	2,386	2,386	2,386	2,386	2,386	2,386
	Panel B: Rural localities					
G wins	0.058***	0.072***	0.028	0.062***	0.044*	0.050***
	(0.011)	(0.008)	(0.017)	(0.010)	(0.024)	(0.017)
Obs.	2,116	2,116	2,116	2,116	2,116	2,116
County FE	No	Yes	No	Yes	No	Yes
Controls	No	Yes	No	Yes	No	Yes
Specification	1st Ord. Poly.	1st Ord. Poly.	2nd Ord. Poly.	2nd Ord. Poly.	4th Ord. Poly.	4th Ord. Poly.

Notes: The table displays RD estimates of the effect of *governing coalition* (G) alignment on referendum turnout in rural localities, using alternative polynomials orders. All localities (Panel A) and rural localities (Panel B). Estimates using polynomial approximations of first order in columns (1)-(2), second order in columns (3)-(4) and fourth order in columns (5)-(6), without controls, with controls and with county fixed effects. The controls include demographic, labor market, fiscal and electoral characteristics of the localities. Standard Errors clustered at county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Alignment, referendum and parliamentary outcomes. Alternative treatment

	Referendum and Parliamentary Outcomes					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	Local LR	Poly.	Approx.	Full sample
	Panel A: Referendum Turnout rural					
G or L-C wins	0.021	0.021	0.011	0.009	0.035**	0.057***
	(0.028)	(0.020)	(0.022)	(0.026)	(0.016)	(0.014)
Obs.	114	238	1,002	1,961	1,961	1,961
	Panel B: Parliamentary Turnout rural					
G or L-C wins	0.001	0.008	0.002	0.003	0.013	0.020*
	(0.014)	(0.012)	(0.015)	(0.014)	(0.011)	(0.011)
Obs.	114	238	855	1,961	1,961	1,961
	Panel C: Senate G Vote shares rural					
G or L-C wins	0.017	0.013	-0.008	-0.015	-0.005	0.008
	(0.021)	(0.015)	(0.018)	(0.018)	(0.015)	(0.014)
Obs.	114	238	825	1,961	1,961	1,961
County FE	No	No	No	No	No	Yes
Controls	No	No	No	No	Yes	Yes
Specification	[-2.5; +2.5]	[-5; +5]	CCT Opt. h	3rd Ord. Poly.	3rd Ord. Poly.	3rd Ord. Poly.

Notes: The table displays RD estimates of the effect of *governing coalition* (G, including independently running *liberals/conservatives*) alignment on referendum (Panel A) and parliamentary outcomes (Panels B and C) in rural localities. Estimates from simple linear regression in a small interval around the cut-off in columns (1)-(2). Estimates from local linear regression using the Calonico et al. (2014) robust bias-corrected confidence intervals in column (3). Estimates using polynomial approximations on the full sample, without controls, with controls and with county fixed effects in columns (4)-(6). The controls in columns (5)-(6) and (11)-(12) include demographic, labor market, fiscal and electoral characteristics of the localities. Standard Errors clustered at county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Alignment and locality revenues in 2012-2013 (rural). RD Estimates

	Locality Fiscal Revenues					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Poly.	Approx.	OLS	Poly.	Approx.
			Full sample			Full sample
	Panel A: Total per capita income					
	2012			2013		
G wins	166.063	85.920	108.883	301.383***	228.842	222.262*
	(111.149)	(135.762)	(110.568)	(105.005)	(141.873)	(129.179)
Obs.	243	2,116	2,116	243	2,116	2,116
	Panel B: Local tax per capita revenue					
	2012			2013		
G wins	43.751*	38.782	10.930	60.135*	27.611	-9.310
	(25.823)	(29.835)	(26.446)	(32.027)	(35.162)	(31.781)
Obs.	243	2,116	2,116	243	2,116	2,116
	Panel C: Per capita road subsidies					
	2012			2013		
G wins	4.478	0.665	2.533	2.962*	1.059	2.158
	(2.762)	(3.291)	(3.680)	(1.634)	(2.004)	(1.955)
Obs.	243	2,116	2,116	243	2,116	2,116
	Panel D: Per capita other subsidies					
	2012			2013		
G wins	32.176	46.623	32.947	38.521*	25.499	15.901
	(19.290)	(28.030)	(24.470)	(19.388)	(26.597)	(22.926)
Obs.	243	2,116	2,116	243	2,116	2,116
Controls	No	No	Yes	No	No	Yes
County FE	No	No	Yes	No	No	Yes
Specification	[-5; +5]	3rd Ord. Poly.	3rd Ord. Poly.	[-5; +5]	3rd Ord. Poly.	3rd Ord. Poly.

Notes: The table displays RD estimates of the effect of *governing coalition* (G) alignment on local revenues and government transfers after the referendum in rural localities. Revenues from 2012 in columns (1)-(3) and from 2013 in columns (4)-(6). Estimates from simple linear regression in a small interval around the cut-off in columns (1)-(2). Estimates from local linear regression using the Calonico et al. (2014) robust bias-corrected confidence intervals in column (3). Estimates using polynomial approximations on the full sample, without controls, with controls and with county fixed effects in columns (4)-(6). The controls in columns (5)-(6) and (11)-(12) include demographic, labor market, fiscal and electoral characteristics of the localities. Standard Errors clustered at county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 9: The referendum turnout impact on parliament elections outcomes. RD and IV estimates.

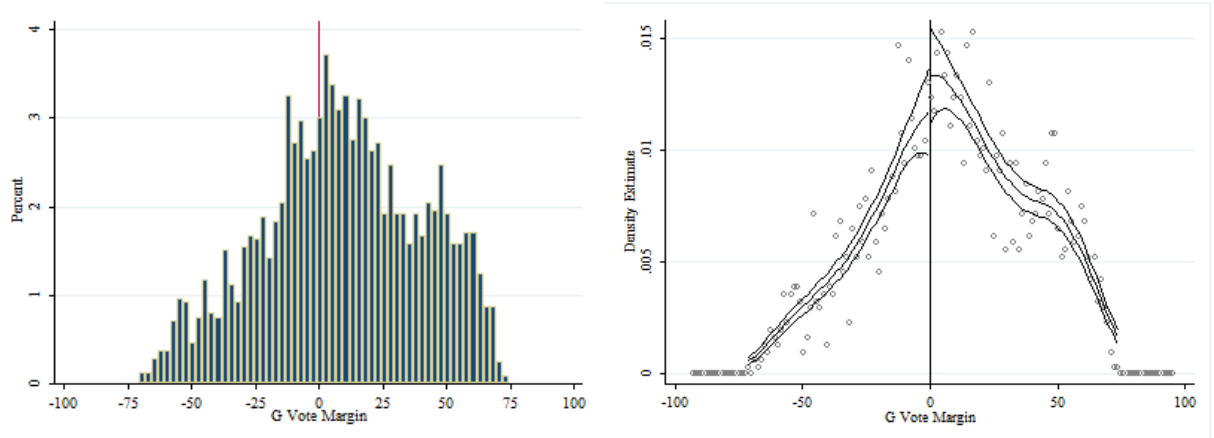
	Electoral Outcomes					
	(1)	(2)	(3)	(4)	(5)	(6)
	Reduced form RD			2SLS		
	Poly. Approx.		Full sample	Poly. Approx.		Full sample
	Panel A: Parliamentary Turnout			Parliamentary Turnout		
G wins	0.023***	0.014	0.011			
	(0.005)	(0.010)	(0.011)			
Referendum turnout				0.308***	0.331***	0.273**
				(0.068)	(0.068)	(0.111)
Obs.	2,116	2,116	2,116	2,116	2,116	2,116
R-squared	0.474	0.475	0.475	0.544	0.542	0.546
	Panel B: Parliamentary Turnoutt			Senate G Vote Share		
G wins	0.040***	0.016	0.003			
	(0.008)	(0.012)	(0.017)			
Referendum turnout				0.579***	0.577***	0.403***
				(0.088)	(0.092)	(0.122)
Obs.	2,116	2,116	2,116	2,116	2,116	2,116
R-squared	0.483	0.485	0.486	0.544	0.545	0.565
	Panel C: Parliamentary Turnout			Lower Chamber G Vote Share		
G wins	0.037***	0.019	0.015			
	(0.008)	(0.012)	(0.018)			
Referendum turnout				0.535***	0.525***	0.377***
				(0.089)	(0.095)	(0.126)
Obs.	2,116	2,116	2,116	2,116	2,116	2,116
R-squared	0.480	0.482	0.482	0.549	0.550	0.560
First stage F	-	-	-	38.680	29.820	17.290
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Specification	1st Ord. Poly.	2nd Ord. Poly.	3rd Ord. Poly.	1st Ord. Poly.	2nd Ord. Poly.	3rd Ord. Poly.

Notes: The table displays OLS estimates of the impact of G alignment on parliamentary outcomes (columns 1-3) and two-stage least squares estimates of the impact of referendum turnout on parliamentary elections turnout (columns 4-6) in rural localities. All estimations use polynomial approximations based on the full sample, with varying polynomial orders (first order in columns 1 and 4, second order in columns 2 and 5, third order in columns 3 and 6). All regressions include locality controls and county fixed effects. Standard Errors clustered at county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

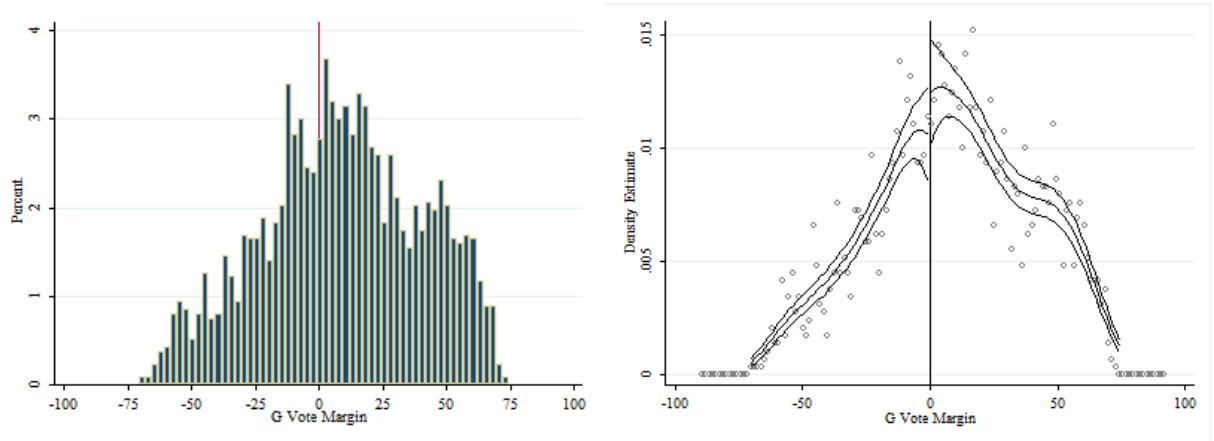
A Appendix

Figure A1: G Vote Margin Distributions and McCrary Density Tests

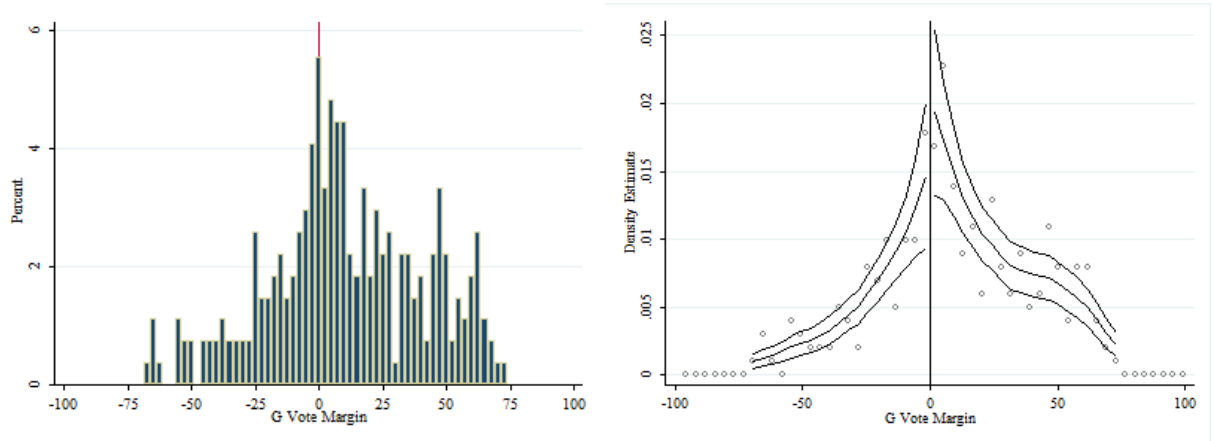
(a) All localities



(b) Rural localities



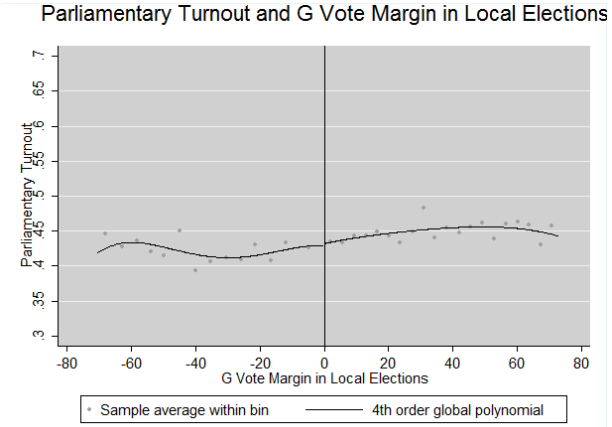
(c) Urban localities

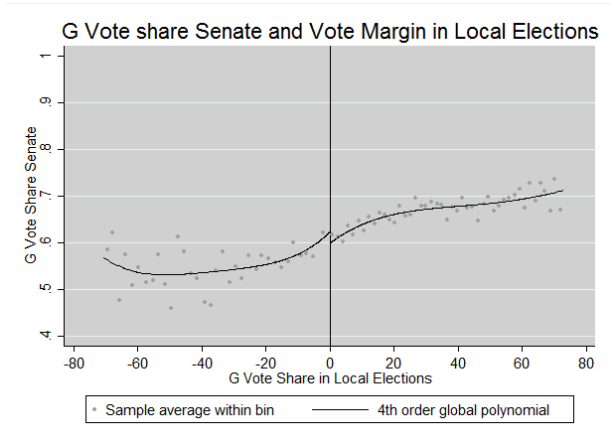


Note: The figure displays the histograms (left) and McCrary density plots (right) for the running variable (Victory Margin) in races against G. All localities: discontinuity estimate 0.128, standard error 0.124. Rural localities: discontinuity estimate 0.177, standard error 0.146. Urban localities: discontinuity estimate 0.260, standard error 0.277.

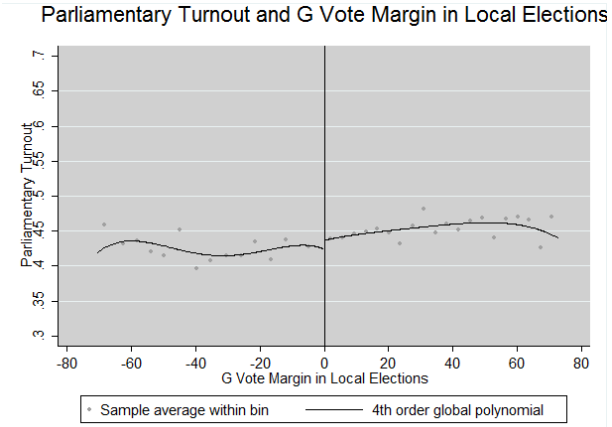
Figure A2: G-alignment and Parliamentary election outcomes

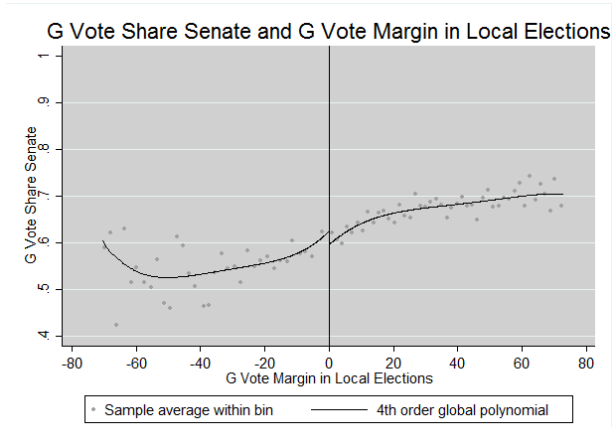
(a) All localities



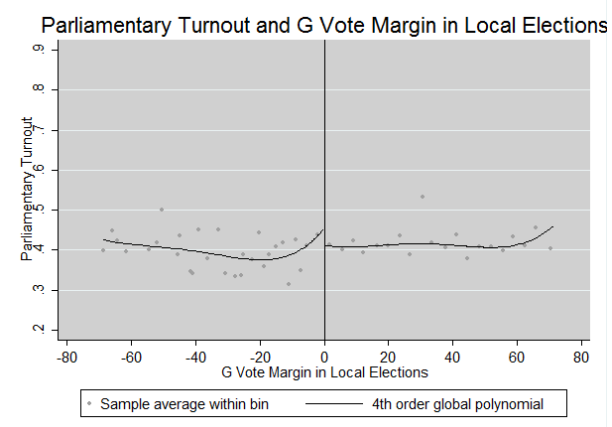


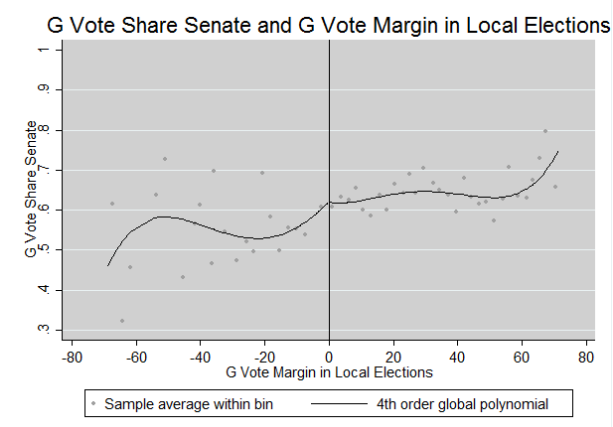
(b) Rural localities





(c) Urban localities





Note: The figure displays the polynomial approximations of the referendum turnout (left) and "YES" vote share (right) plotted against the running variable on the X-axis (Victory Margin for G in races against G).

Table A1: Referendum Turnout in races within 5 percentage points from the victory cut-off

Race (Top 2 parties)	G wins	SD wins	P wins	L/C wins	Difference
Panel A: Races between G and P, SD and P, P and L/C, SD and L/C					
G(=SD+L/C) vs P	0.547		0.528		0.019
	(0.152)		(0.159)		
	n=82		n=68		
SD vs P		0.541	0.507		0.034
		(0.118)	(0.179)		
		n=20	n=17		
P vs L/C			0.451	0.453	-0.002
			(0.191)	(0.117)	
			n=12	n=13	
SD vs L/C		0.649		0.562	**0.087***
		(0.152)		(0.133)	
		n=25		n=17	
Panel B: Main G treatment= party SD or coalition SD+L/C					
G(=SD or SD+L/C) vs P	0.546		0.524		0.022
	(0.145)		(0.162)		
	n=102		n=85		
G(=SD or SD+L/C) vs L/C	0.650			0.551	**0.101****
	(0.149)			(0.139)	
	n=26			n=18	

Note: The table displays the comparison in means in referendum turnout between localities for different *narrow* races. SD stands for *social-democrats*, L/C is *liberals/conservatives*, P stands for *the president's party*. Standard deviations are reported in parantheses. The differences in means are reported for each type of race in the last column. *** p<0.01, ** p<0.05, * p<0.1

Table A2: Descriptive statistics. Races within 5% victory margin vs. two-candidate races.

	Races against G		
	5% margin	**2-cand**	**p-value**
Panel A: Outcomes			
Turnout	0.543	0.519	0.161
Share YES	0.867	0.850	**0.003**
Turnout Parliamentary Elections	0.433	0.450	**0.102**
G Vote Share in Senate	0.612	0.612	0.993
Panel B: Covariates			
No. candidates in local elections	5.336	2.000	**0.001**
Turnout Local Elections	44.554	47.181	**0.000**
Sum % first 2 candidates	79.199	1.000	**0.009**
Log Population	8.170	7.769	**0.000**
Share adult population	0.770	0.771	0.915
Share over 65	0.206	0.206	0.955
Share males	0.496	0.497	0.390
Share high education	0.050	0.038	**0.004**
Share high school	0.163	0.143	**0.001**
Share Romanians	0.883	0.879	0.784
Unemployment rate	0.052	0.049	0.510
Per capita revenue	1,405.645	1,594.426	**0.069**
Per capita own revenue	473.531	490.976	0.582
Per capita roads funds	12.749	10.798	0.706
Per capita subsidies	130.420	101.823	0.299
Per capita expenditures	1,317.394	1,429.516	0.183
Per capita expenditures education	369.123	333.459	**0.031**
Per capita health expenditures	10.294	11.179	0.771
Per capita public expenditures	148.956	193.998	**0.078**
N max	280.000	152.000	

Note: The table displays the comparison in means in outcome variables (Panel A) and locality characteristics (Panel B) between the sample of close races (under 5 percentage points victory margin) and two-candidate races sample, for races against G. The p-values for the differences in means are reported (significant differences marked in bold).

Table A3: Descriptive statistics. Full samples of races against G

	All sample			Rural			Urban		
	G	**non-G**	**P-value**	**G**	**non-G**	**P-value**	**G**	**non-G**	**P-value**
Panel A: Outcomes									
Turnout	0.583	0.472	**0.000**	0.595	0.476	**0.000**	0.497	0.439	**0.003**
Share YES	0.867	0.857	**0.000**	0.866	0.855	**0.000**	0.878	0.869	**0.049**
Turnout Parliamentary Elections	0.447	0.422	**0.000**	0.452	0.425	**0.000**	0.412	0.402	0.243
G Vote Share in Senate	0.661	0.562	**0.000**	0.665	0.562	**0.000**	0.636	0.561	**0.000**
Panel B: Covariates									
No. candidates in local elections	4.650	4.517	**0.055**	4.443	4.344	0.128	6.208	5.969	0.387
Sum % votes top 2 local candidates	85.139	86.297	**0.011**	85.718	86.802	**0.023**	80.783	82.061	0.363
Turnout referendum 2007	44.746	45.213	**0.000**	44.943	45.273	0.471	43.265	44.706	0.186
Log Population	8.157	8.115	0.227	7.957	7.957	0.987	9.660	9.444	0.102
Share adult population	0.771	0.769	0.439	0.770	0.768	0.340	0.779	0.783	0.435
Share over 65	0.204	0.203	0.833	0.212	0.209	0.296	0.141	0.152	**0.005**
Share males	0.495	0.496	0.458	0.497	0.497	0.703	0.486	0.487	0.449
Share high education	0.046	0.045	0.398	0.037	0.037	0.903	0.116	0.110	0.379
Share high school	0.159	0.156	0.155	0.147	0.146	0.491	0.250	0.239	**0.097**
Share Romanians	0.893	0.875	**0.002**	0.898	0.881	**0.005**	0.854	0.824	**0.061**
Unemployment rate	0.049	0.049	0.844	0.051	0.051	0.963	0.033	0.035	0.380
Per capita revenue	1366.051	1465.576	**0.010**	1335.907	1443.941	**0.009**	1592.916	1647.126	0.628
Per capita own revenue	485.207	492.102	0.646	439.949	463.897	**0.079**	825.824	728.785	0.156
Per capita roads funds	11.227	11.603	0.799	12.228	12.559	0.841	3.694	3.585	0.932
Per capita subsidies	98.412	140.310	**0.000**	95.892	127.683	**0.003**	117.373	246.270	**0.001**
Per capita expenditures	1306.659	1396.277	**0.012**	1270.828	1365.327	**0.010**	1576.322	1655.995	0.504
Per capita expenditures education	357.241	368.331	0.137	331.726	350.470	**0.003**	549.264	518.215	0.394
Per capita health expenditures	9.582	9.316	0.804	6.837	7.594	0.433	30.240	23.769	0.211
Per capita public expenditures	175.527	200.354	**0.077**	170.342	192.366	0.148	214.552	267.392	0.119
N	1475	911		1302	814		173	97	

Note: The table displays the comparison in means in outcome variables (Panel A) and locality characteristics (Panel B) between localities aligned and unaligned with G, for all races against G, and separately for rural and urban localities. The p-values for the differences in means are reported (significant differences marked in bold).

Table A4: Validity Tests of Covariates Discontinuity. Turnout predicted by pretreatment covariates. RD estimates

	Predicted Turnout 1			**Predicted Turnout 2**			**Predicted Turnout 3**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	Poly.	Approx.	OLS	Poly.	Approx.	OLS	Poly.	Approx.
				Panel A: All localities					
G wins	-0.003	-0.018	-0.009	-0.002	-0.015	-0.006	-0.002	-0.015	-0.008
	(0.011)	(0.012)	(0.010)	(0.011)	(0.012)	(0.010)	(0.011)	(0.014)	(0.011)
Obs.	289	2,386	2,386	289	2,386	2,386	289	2,386	2,386
				Panel B: Rural localities					
G wins	-0.004	-0.015	-0.008	-0.002	-0.012	-0.004	-0.002	-0.012	-0.007
	(0.013)	(0.015)	(0.012)	(0.013)	(0.015)	(0.012)	(0.013)	(0.016)	(0.013)
Obs.	243	2,116	2,116	243	2,116	2,116	243	2,116	2,116
				Panel C: Urban localities					
G wins	-0.009	-0.026	-0.021	-0.012	-0.026	-0.024	-0.009	-0.024	-0.024
	(0.021)	(0.024)	(0.028)	(0.019)	(0.022)	(0.027)	(0.019)	(0.023)	(0.027)
Obs.	46	270	270	46	270	270	46	270	270
County FE	No	No	Yes	No	No	Yes	No	No	Yes
Specification	[-5; +5]	3rd Order Poly.	3rd Order Poly.	[-5; +5]	3rd Order Poly.	3rd Order Poly.	[-5; +5]	3rd Order Poly.	3rd Order Poly.

Notes: The table displays RD tests of discontinuity in pretreatment characteristics around the G victory threshold. Instead of testing individual characteristics, I predict referendum turnout with subsets of all covariates: in columns (1)-(3) demographic characteristics (population, age, gender, education) and labor market characteristics (unemployment rate), fiscal covariates are added in columns (4)-(6), and local elections characteristics (number of mayoral candidates and turnout at 2012 local elections) are further included in columns (7)-(9). Standard Errors clustered at county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A5: The continuity of observable characteristics at the victory threshold.

	Rural localities		
Dependent variable			
	(1)	(2)	(3)
Log Population	-0.017	0.031	0.047
	(0.074)	(0.087)	(0.077)
Share adult population	-0.001	-0.003	0.000
	(0.006)	(0.007)	(0.006)
Share over 65	-0.006	-0.009	-0.002
	(0.008)	(0.009)	(0.008)
Share males	-0.000	-0.000	-0.001
	(0.001)	(0.002)	(0.002)
Share high education	0.006**	0.005	0.005*
	(0.003)	(0.003)	(0.003)
Share high school	0.008	0.008	0.007
	(0.006)	(0.007)	(0.006)
Share Romanians	-0.003	-0.022	-0.014
	(0.019)	(0.023)	(0.021)
Unemployment rate	-0.002	-0.004	-0.006
	(0.005)	(0.006)	(0.005)
Per capita revenue	62.148	-41.792	-97.802
	(132.769)	(131.854)	(119.491)
Per capita own revenue	37.214	32.446	22.186
	(32.706)	(35.631)	(30.034)
Per capita roads funds	-4.939	-4.708	-1.727
	(6.482)	(4.130)	(3.737)
Per capita subsidies	15.674	32.145	23.867
	(42.222)	(38.168)	(38.181)
Per capita expenditures	8.868	-102.439	-153.092
	(109.500)	(113.434)	(101.119)
Per capita expenditures education	-7.938	-17.967	-24.555
	(21.392)	(20.310)	(18.783)
Per capita health expenditures	-4.422	-6.910*	-7.269*
	(3.576)	(4.056)	(3.980)
Per capita public expenditures	0.827	2.182	-7.197
	(26.317)	(35.064)	(36.894)
No. candidates in local elections	-0.040	0.050	-0.008
	(0.216)	(0.230)	(0.228)
Turnout referendum 2007	0.776	-0.749	-1.088
	(1.392)	(1.708)	(1.416)
Sum % votes first 2 candidates	0.487	0.432	0.774
	(1.665)	(1.417)	(1.487)
County FE	No	No	Yes
Specification	[-5; +5]	3rd poly.	3rd poly.

Notes: The table displays RD estimates from regressions with covariates as the dependent variable and G wins as the main independent variable, exclusively in rural areas.

Table A6: Alignment and parliamentary election outcomes. RD Estimates

	Parliamentary Turnout						G Share Votes Senate					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	OLS	OLS	Local LR	Poly.	Approx.	Full sample	OLS	OLS	Local LR	Poly.	Approx.	Full sample
					Panel A: All localities							
G wins	0.006	0.004	0.000	-0.002	0.004	0.006	-0.003	-0.002	-0.021	-0.017	-0.009	0.002
	(0.012)	(0.010)	(0.013)	(0.012)	(0.011)	(0.009)	(0.019)	(0.013)	(0.016)	(0.017)	(0.016)	(0.015)
Obs.	143	289	1,048	2,386	2,386	2,386	143	289	999	2,386	2,386	2,386
					Panel B: Rural localities							
G wins	0.016	0.008	0.009	0.007	0.010	0.011	-0.002	-0.004	-0.023	-0.015	-0.007	0.003
	(0.015)	(0.012)	(0.015)	(0.014)	(0.013)	(0.011)	(0.022)	(0.014)	(0.018)	(0.020)	(0.018)	(0.017)
Obs.	118	243	897	2,116	2,116	2,116	118	243	888	2,116	2,116	2,116
					Panel C: Urban localities							
G wins	-0.040	-0.015	-0.048	-0.045*	-0.013	0.017	-0.013	0.012	-0.002	-0.026	-0.028	-0.011
	(0.036)	(0.021)	(0.033)	(0.025)	(0.023)	(0.022)	(0.036)	(0.027)	(0.035)	(0.030)	(0.032)	(0.032)
Observations	25	46	121	270	270	270	25	46	134	270	270	270
County FE	No	No	No	No	No	Yes	No	No	No	No	No	Yes
Controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Specification	[-2.5; +2.5]	[-5; +5]	CCT Opt. h	3rd Ord. Poly.	3rd Ord. Poly.	3rd Ord. Poly.	[-2.5; +2.5]	[-5; +5]	CCT Opt. h	3rd Ord. Poly.	3rd Ord. Poly.	3rd Ord. Poly.

Notes: The table displays RD estimates of the effect of *governing coalition* (G) alignment on parliamentary elections turnout (columns 1-6) and the senate G vote share (columns 7-12). Results are based on all localities (Panel A), rural (Panel B) and urban (Panel C). Estimates from simple linear regression in a small interval around the cut-off in columns (1)-(2) and (7)-(8). Estimates from local linear regression using the Calonico et al. (2014) robust bias-corrected confidence intervals in columns (3) and (9). Estimates using polynomial approximations on the full sample, without controls, with controls and with county fixed effects in columns (4)-(6) and (10)-(12). The controls in columns 5-6 and 11-12 include demographic, labor market, fiscal and electoral characteristics of the localities. Standard Errors clustered at county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A7: President's party (P) alignment and referendum outcomes. RD Estimates

	Turnout						**Share YES**					
	(1) OLS	(2) OLS	(3) Local LR	(4) Poly.	(5) Approx.	(6) Full sample	(7) OLS	(8) OLS	(9) Local LR	(10) Poly.	(11) Approx.	(12) Full sample
					Panel A: All localities							
P wins	-0.022	-0.032*	-0.011	-0.019	-0.043**	-0.055***	0.010	0.005	0.010	0.003	0.001	-0.004
	(0.030)	(0.019)	(0.025)	(0.023)	(0.017)	(0.016)	(0.009)	(0.006)	(0.008)	(0.007)	(0.007)	(0.007)
Obs.	114	233	732	1,770	1,770	1,770	114	233	782	1,770	1,770	1,770
					Panel B: Rural localities							
P wins	-0.020	-0.037	-0.018	-0.021	-0.046**	-0.065***	0.011	0.005	0.010	0.005	0.003	-0.003
	(0.038)	(0.022)	(0.026)	(0.027)	(0.019)	(0.017) (0.010)		(0.006)	(0.009)	(0.008)	(0.008)	(0.008)
Obs.	98	201	704	1,585	1,585	1,585	98	201	740	1,585	1,585	1,585
					Panel C: Urban localities							
P wins	-0.013	0.004	0.054	0.003	-0.045	0.018	0.001	0.004	-0.006	-0.012	-0.013	-0.016
	(0.063)	(0.047)	(0.078)	(0.062)	(0.055)	(0.050)	(0.013)	(0.008)	(0.013)	(0.012)	(0.010)	(0.012)
Obs.	16	32	77	185	185	185	16	32	102	185	185	185
County FE	No	No	No	No	No	Yes	No	No	No	No	No	Yes
Controls	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Specification	[-2.5; +2.5]	[-5; +5]	CCT Opt. h	3rd Ord. Poly.	3rd Ord. Poly.	3rd Ord. Poly.	[-2.5; +2.5]	[-5; +5]	CCT Opt. h	3rd Ord. Poly.	3rd Ord. Poly.	3rd Ord. Poly.

Notes: The table displays RD estimates of the effect of *the president's party* (P) alignment on referendum turnout (columns 1-6) and the "YES" vote share (columns 7-12). Results are based on all localities (Panel A), rural (Panel B) and urban (Panel C). Estimates from simple linear regression in a small interval around the cut-off in columns (1)-(2) and (7)-(8). Estimates from local linear regression using the Calonico et al. (2014) robust bias-corrected confidence intervals in columns (3) and (9). Estimates using polynomial approximations on the full sample, without controls, with controls and with county fixed effects in columns (4)-(6) and (10)-(12). The controls in columns 5-6 and 11-12 include demographic, labor market, fiscal and electoral characteristics of the localities. Standard Errors clustered at county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A8: The impact of local alignment and referendum turnout on parliament elections turnout. OLS estimates

	Outcomes in Parliamentary Elections		
	(1)	(2)	(3)
	Panel A: Turnout		
G wins	0.028***	-0.016***	-0.001
	(0.005)	(0.005)	(0.005)
Referendum turnout		0.365***	0.266***
		(0.028)	(0.031)
Obs.	2,116	2,116	2,116
R-squared	0.019	0.386	0.546
	Panel B: G Vote Share Senate		
G wins	0.103***	0.058***	0.051***
	(0.010)	(0.008)	(0.006)
Referendum turnout		0.374***	0.404***
		(0.059)	(0.043)
Obs.	2,116	2,116	2,116
R-squared	0.141	0.350	0.553
	Panel C: G Vote Share Lower Chamber		
G wins	0.101***	0.054***	0.050***
	(0.010)	(0.008)	(0.007)
Referendum turnout		0.392***	0.411***
		(0.058)	(0.048)
Obs.	2,116	2,116	2,116
R-squared	0.129	0.346	0.548
Controls	No	No	Yes
County FE	No	No	Yes

Notes: The table displays OLS estimates of the effect of G alignment and referendum turnout on parliamentary outcomes. Standard Errors clustered at county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1