The Illicit Benefits of Local Party Alignment in National Elections

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Abstract

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 through, inter alia, electoral fraud. A regression discontinuity design with Romanian local elections and a president impeachment referendum in 2012 uncovers higher referendum turnouts in localities aligned with the government coalition - the impeachment initiators. Electoral forensics tests present abnormal vote count distributions across polling stations, consistent with ballot stuffing. The alignment effect, driven by rural localities, may explain the clientelistic government transfers found in this context and documented worldwide.

Keywords: Political economy, elections, electoral fraud, partisan alignment

JEL codes: D72, H70

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 systematically manipulated (e.g. Aidt et al., 2019, Cruz et al., 2016, Collier and Vicente, 2014, Finan and Schechter, 2012,).[1] A popular tactic of voter persuasion 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 and is in itself not illicit. However, to date little is known about whether and how local partisan alignment produces the expected electoral advantages. The growing concern is that it drives local politicians to unorthodox practices like electoral fraud to deliver electoral returns to their patrons. This quid-pro-quo leads to the erosion of democratic institutions, with harmful welfare consequences in the long run.

This paper documents an alignment effect in the context of Romanian national

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[1]For instance, according to the 2014/2015 Afrobarometer and 2009 Latinobarometer, over 40% of respondents believed the elections were 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.

elections and it provides extensive evidence that this effect is partly underpinned 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; 2) the patterns in the electoral data are consistent with electoral fraud (partly ballot stuffing and potentially also misreporting or vote buying), explaining a part of the alignment effect uncovered. Using data on locality revenues after the elections, I also show suggestive evidence that the efforts of local politicians were subsequently rewarded. To get around the endogeneity of mayor alignment, I use a regression discontinuity design with multiple stacked thresholds (see Cattaneo et al., 2016) and compare national polls outcomes in localities where an aligned candidate narrowly won and lost the mayoral race. Local party alignment was decided in the June 2012 local elections, and my main outcomes are drawn from a nationwide referendum on impeaching the president, held in July 2012.

The electoral setting in this paper is particularly suited to detecting alignment effects and electoral fraud. 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: the president's all-time low popularity indicated a safe win for the government coalition, upon meeting the quorum. However, any effort to gain votes, be it legitimate campaigning, vote buying or rigging, called for allies in local government to access the necessary networks and logistics and control the proceedings inside polling stations. Thus, local party alignment is expected to have played a critical role in reducing the cost

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

of persuading voters, as well as electoral fraud or turnout buying.

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, rising up to 6.2 percentage points. This is in line with recent studies identifying higher social pressures, stronger clientelistic networks, and a higher prevalence of vote buying in rural areas (e.g. Funk, 2010; Vicente, 2014; Volintiru, 2012).

To unpack the alignment effect, I run a battery of diagnostic tests popular in electoral forensics for detecting abnormal statistical patterns in electoral returns, consistent with electoral fraud.[3] Following the novel methodologies from Beber and Scacco (2012) and Hicken and Mebane (2017), 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 should be uniformly distributed (10% average frequency for each digits). I find significantly different last digit frequencies in the turnout counts (and above 20% frequency in the incidence of 0 or 5), but not in the valid vote counts. I then examine their difference - null votes, and find that government-aligned localities have higher incidence of null votes, particularly those where turnout was below the nationally required 50% threshold. This is consistent with invalid ballots stuffing and accounts for around 10% of the effort to boost turnout. The data corroborates trial evidence against local and central politicians sentenced and indicates that rigging the referendum was systematic rather than a few isolated cases.

The growing literature on the role of local party alignment in intergovernmen-

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[3]Electoral forensics tests combine data visualisation and vote distribution tests to identify patterns consistent with electoral fraud. Mebane (2006) developed a methodology based on the distribution of the second digit in vote counts at polling stations. Contrasting vote counts in fair and suspicious elections, one can detect patterns consistent with human bias in the generation of vote distributions.

tal transfers has revealed a pattern of clientelistic allocations from higher to lower tier governments.[4] However, the evidence of a reverse transfer of political support from local to higher tier politicians is 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 (see (e.g. Zudenkova 2011; Persico et al. 2011). 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.[5] Migueis (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 on 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

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[4]The earlier studies revealed correlations between federal transfers and political affiliation of local governments with the party in central government (Grossman 1994; Snyder and Levitt 1995;Worthington and Dollery 1998). More recent studies estimate the causal link between alignment and transfers (Solé-Ollé and Sorribas-Navarro 2008 in Spain; Brollo and Nannicini 2012 in Brazil; Migueis 2013 in Portugal; Bracco et al. 2015 in Italy; Dey and Sen 2016 in India)

[5]These coattail effects occur when two elections are on the same day, due to lower cognitive costs for voters in choosing the same party. Other studies that have investigated coattail effects are: Cohen et al. (2000); Mattei and Glasgow (2005); Gélineau and Remmer (2006); Golder (2006); Ames (1994); Samuels (2000a); Samuels (2000b); Broockman (2009).

allocated a social protection program to aligned constituencies also received significant electoral bonuses in 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 consolidating the power of central politicians. The analysis presented here begins to unpack some of the methods local partymen use on the ground to deliver votes, showing hard evidence of electoral fraud through invalid ballot stuffing, a low-cost, low-risk device. Furthermore, subsequent increases in revenues for aligned localities suggest that local politicians may receive incentives to serve their parties this way. The findings suggest that this political alignment quid-pro-quo may engender an illicit type of political capital in local governments whenever the incentives are high and cost of corruption is low. This is particularly pernicious in countries with new institutions and weak monitoring and law enforcement, 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 through diagnostic tests, and further specifications; section 6 concludes.

2 Institutional Setting

2.1 The Romanian Electoral System and Politics in 2012

Romania is a young semi-presidential democracy, ruled by the president and a government accountable to the Parliament. The president is directly elected every five years in a runoff majority vote, while local and parliamentary elections are held every four years.[6] A multiparty system has been in place since 1990, with 39 parties and alliances registered for the 2012 parliamentary elections.

The two main rival parties in 2012 were the *Liberal Democratic Party* (PDL) and the *Social-Liberal Union* (USL). The incumbent president in 2012, Traian Basescu, although *de jure* politically unaffiliated, enjoyed strong support from the centrist PDL, whose leader he had been. USL was a coalition of the former communist Social Democratic Party (PSD), the National Liberal Party (PNL) and the Conservative Party (PC), who had long been in the opposition, while the PDL was in government.

USL was formed in February 2011, presided by the PSD and PNL leaders Victor Ponta and Crin Antonescu; their first priority, as per the USL Founding Document (5 February 2011), was to overthrow the PDL government and president Basescu. The electoral year 2012 had local polls on June 10 and parliamentary elections on December 9. With large stakes in taking power, USL gained momentum in April 2012, when an unpopular PDL cabinet weakened by their own austerity measures was dismissed by Parliament through a motion of no confidence. Shortly after, Victor Ponta took office and formed the USL coalition government, which immediately initiated the legal procedures to suspend president Basescu

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[6]The legislative elections are based on a closed list proportional representation system, which implies that the first position on the list (typically a local candidate) is essentially directly elected.

from office. These entailed a quick succession of controversial institutional reforms, leading to a full-blown political crisis.[7] 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 call to impeach the president on grounds of unconstitutional conduct in office (e.g. interference in the judiciary). On 6th July, a Parliament majority voted for impeachment, with his recall from office to be decided in a national referendum. Basescu was immediately suspended from his attributions and Crin Antonescu, leader of the *liberals*, became interim president. The referendum was held on the 29th July, when Basescu's popularity was at an all-time low, owing to the draconian austerity measures in 2010. Opinion polls just ahead of the referendum anticipated his removal from office.

Importantly, the referendum law No.3/2000 stipulated a *quorum* rule: a minimum turnout of 50% was necessary to validate the referendum. The question on the ballots was "Do you agree with the dismissal of the president Traian Basescu?". In the referendum an overwhelming 87.52% voted "YES". However, only 46.24% of the 18 million registered voters cast their ballot, and therefore the referendum was ruled invalid by the constitutional court. The president resumed his duties shortly.[8] Figure 1 shows a brief chronology of political and electoral events leading

[7]Constitutional court processes the national ombudsman were changed, clearing the road for the subsequent government ordinances for the president's impeachment. Ponta's initiatives and the June 2012 political crisis are described in Politeanu (2012) and in the media: http://www.economist.com/blogs/easternapproaches/2012/06/romanian-politics-0 (in English)

[8]President Basescu had withstood an impeachment referendum before, in 2007,

up to the referendum.

Figure 1 goes here

2.3 The local government and the corruption environment

Romania's local administration is organized into 42 counties subdivided into 423 urban localities (cities and towns) and 2859 rural localities (communes and villages). The local administration of each locality, urban and rural, falls in the remit of mayors, who hold most of the local political power, and local councils. In 2012, the mayors were directly elected in a first-past-the-post system.[9] Once elected, the mayors took office almost immediately.[10] In 2012 mayoral candidates could run for a separate party, a coalition, or as independent candidates. Hence, 42.39% of the mayoral seats were won by USL, but an additional 11.9% 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).

Heading the local administration, mayors enjoy the highest status in the local public servants' hierarchy and in the community. The 2012 switch to a simple majority for securing electoral victory in local elections and the closed-list system for

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.

[9]Article 66 of Law No. 215/2001 Art. stipulates that "the mayor is the head of public local administration and of the locality-specific public administration apparatus, which he/she manages and controls ". Before 2012, mayors used to be selected in a majority two-round election.

[10]Mayors' mandate is validated in court within just 20 days of the local elections, as stipulated in article 63, Law No. 215/2001, following which they take office immediately.

legislative elections (where top-list candidates are essentially directly elected) have invested local politicians with greater powers and influence in their parties' hierarchy. Mayors often emerge as corrupt local barons, who command administrative, financial and electoral resources vital for their parties in national elections; this is particularly characteristic for the power structure inside the *social-democratic party* (Brett, 2015). On the other hand, career concerns around climbing the ranks to parliamentary seats, and the preponderantly clientelistic allocation of government funds to local councils provides mayors of the party in power with strong incentives to serve their political overlords.[11] Hence, national parties are deeply invested in local elections, as the mayor's political alignment is typically seen as a predictor of the parties' performance in national elections (Buti, 2012; IRES, 2012).[12] Mayors play a key role in higher tier elections, using their networks and monopoly on resources to secure votes by: 1) pork-barrel spending (e.g. Pop-Eleches and Pop-Eleches 2012 investigate the EURO 200 program for supplying computers to deprived families; EFOR 2013 document the clientelist allocation of infrastructure funds); 2) campaigning and "get-out-the-vote" strategies including the use of local media or in-church propaganda, particularly in rural areas, (Seceleanu, 2009); 3) electoral corruption (vote buying and ballot fraud), using their authority and local infrastructure to enable or even coordinate the vote-rigging apparatus.

Vote buying and vote rigging was commonplace in Romania at the time of the referendum, particularly in rural undereducated communities (Volintiru 2012). Comsa and Postelnicu (2014) show that 19%-24% of voters reported vote-buying

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[11]See "Voting to survive: Romanians elect mayors despite corruption record " retrieved from Reuters UK (https://reut.rs/2DNB7UX); accessed on 16 May 2018 (In English).

[12]In the 2000 legislative and local elections, the local and legislative vote shares' correlation was 40% (Klašnja, 2015).

at the 2014 presidential elections. The referendum itself was fraught with allegations of electoral fraud (Freedom House, 2013). An undercover journalist revealed mayors' confessions hinting at unorthodox practices for boosting and depressing turnout "You give them [the voters] a snaps, a sandwich", or "Evening after evening we took the people in taverns. We'll have 75% [turnout]" (Biro, 2012).

Criminal investigations in the years following the referendum revealed the governing party's attempts to coordinate a large-scale top-down operation outside of the law. A corruption trial concluded with the sentencing of the *social-democrat* secretary general and campaign manager for electoral corruption aiming at a 60% turnout target (National Anticorruption Directorate Press Release 2013). Several mayors and local campaigners were indicted in that trial, which revealed a network coordinated from the top by email and SMS orders for a range of fraudulent practices: county and local councils were to organise events and handout prizes exclusively for voters; local campaign managers were to create lists of absent voters and accompany them to the station; ballot-stuffing was also organized (votes attributed to emigrants or deceased persons). Other misdeeds included threats to get or to impede votes and mobile ballot box misuse. The information transmission was done through central-county-local offices, but also directly from central to local offices.[13]

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[13]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). The organization of the vote rigging operation is explained here: https://bit.ly/2YGlsRT, published 18 February 2017 (in Romanian). See also the article by Princeton political scientist Grigore Pop-Eleches "Post-Election Report: Romanian's Presidential Impeachment Referendum, and a Request for Help in Identifying Potential Fraud" (http://themonkeycage.org/, 9th August 2012). Some PDL mayors were also charged with electoral misconduct: http://bit.ly/1Or9UCg,

Overall, the rich anecdotal evidence reveals the critical role of local allies in national polls. The formal analysis below first documents the overall impact of local politicians in the national referendum. The paper then unpacks one of the methods through which politicians operate on the ground, namely electoral fraud. The data suggests that bull ballot stuffing was one of the methods of fraud of choice (potentially alongside misreporting on return sheets and turnout buying). Finally, it provides suggestive evidence of the rewards flowing back from central to local governments, completing the *quid-pro-quo* circle.

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: the parties' vote shares in mayoral races (the running variable); the referendum turnout (share of ballots cast of total registered voters), the 'YES' vote share (percentage votes for dismissing the president). I also use the polling station - level total, valid and null vote counts and percentages for the electoral fraud diagnostics. Control variables include the 2007 impeachment referendum turnout, the number of mayoral candidates at the 2012 local elections, and the joint vote share of the top two mayoral candidates. Subsidiary analyses in the appendix use parliamentary election turnout and party vote shares by locality.

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

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Romania Libera, published 30 July 2012 (in Romanian).

size, the share of voting-age population, the share aged over 65, the share of Roma-
nians, of males, and of high school and university educated, and the unemployment
rate.[14]

iii) Fiscal data from 2011. This data from the Ministry of Regional Develop-
ment is available at locality level, including: total income (own taxes, intergov-
ernmental transfers, subsidies), total and split public expenditure (on education,
health, and public services). The above controls are standard in the electoral RD
literature (e.g. Pettersson-Lidbom, 2008; Ferreira and Gyourko, 2009). Similar
fiscal data for 2012 and 2013 is used for further outcomes.

3.1   Treatment definition

A locality is treated if the candidate aligned with the *governing coalition* won
the mayoral race. 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, according to USL's manifesto.

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 oc-
curred from disagreements over local administration offices. In terms of their
loyalty to the coalition, the *social-democrats* remained the main drivers of the ref-
erendum, while the separate *liberals* and *conservatives* had ambiguous incentives.[15]

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

[15]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.

If dissident *liberals/conservatives* were included in the treatment groups, the alignment effect may be biased downwards. I therefore place *liberal/conservative* localities in the control group in the main specifications. However, in section 5.3 I also report the estimates based on their inclusion in the treatment group, which are fully consistent with the main specifications.

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

3.2   The working sample and descriptive statistics

Firstly, of all 3182 localities I exclude those 11.4% where a candidate's vote share was above 80%.[16] Secondly, most local elections are multiparty races, where a candidate can win a tight race with a vote share lower than 51%, depending on how many parties compete and what is the strongest opponent's vote share (see e.g. Eggers et al., 2015, Klašnja and Titiunik, 2017). The working sample includes only those localities where a G-candidate was either winner or runner-up, regardless of how many parties competed (2386 races in total, of which 2116 are rural). Compared to two-party races, the effect in multiparty races is identified from multiple thresholds (see section 4), which has advantages both for external validity and for statistical power. In the 2012 local elections there were few two-

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[16]This is standard practice, since the identification relies on close electoral races. Moreover, their inclusion does not change the results qualitatively, since the RDD picks up effects close to the cut-off. 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).

party races (154, or 6.4%, with only 9 races with vote margins within 5 percentage points of the threshold). These were also in places significantly different from the main sample of narrow races, as online appendix A1 shows: two-candidates race localities have significantly lower population, higher per capita revenue, lower education levels and expenditures, suggesting a risk of selection bias from these localities.

In Table 1, I compare statistics from narrow (5 percentage points) races in all localities, and the subsample of rural localities, separately by G-alignment status.[17] Turnout in G-aligned localities is larger than in non-aligned localities (significantly so in rural areas). All differences in pre-treatment covariates between G-aligned and unaligned localities are insignificant, except for the slightly larger share of people in higher education in G-aligned rural localities. Appendix Table A3 shows the mean difference in baseline covariates in the much smaller urban sample. Many of the covariates differ significantly, which likely invalidates the RDD in the urban sample. In support for the validity of the RDD in the working sample, I test for discontinuities in pre-treatment characteristics in section 4.2. I also include these covariates in the main specifications.

<center>Table 1 goes here</center>

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[17]Appendix Table A2 displays these comparative statistics for all the races in the sample. Referendum turnout is markedly larger where the G-aligned candidates won (58.3%) than where they lost (47.2%). This difference is larger in rural localities (12 percentage points) and stands in contrast with the 2007 impeachment referendum turnout, which was slightly lower in G-aligned localities.

4   Identification Strategy

4.1   Estimation

Whether or not a locality is aligned with the government coalition depends on voter preferences and locality characteristics, which also likely affect national election outcomes. To identify the causal impact of mayors' alignment on referendum outcomes I use a sharp RDD, comparing referendum outcomes in localities where a G-candidate narrowly won and narrowly lost the mayoral race. The vote margin between the aligned and the strongest unaligned candidate is the running variable (dG), and the alignment treatment (henceforth G_wins) is assigned to localities where G-candidates' victory margins exceed 0 ($G\_wins = 1\,[dG \geq 0]$).

I follow the continuity-based approach for identifying the RD estimate of alignment on referendum outcomes (Hahn et al., 2001, discussed in Skovron and Titiunik, 2015 and Sekhon and Titiunik, 2016). The idea is that if potential outcomes are continuous functions of the running variable at the victory cut-off, then the average treatment effect is identified from the difference in the limits of the aligned and unaligned localities' mean observed outcomes, as dG approaches the cut-off 0. Thus, causal identification is possible even if potential referendum outcomes are not independent of vote margins in local elections. Estimation requires approximating, based on observed outcomes, two distinct regression functions of the potential outcomes across the cut-off, and taking the functions' limits difference at the cut-off. The empirical model is:

$$y_{ic} = \alpha + \beta \cdot G\_wins_{ic} + f(dG_i) + G\_wins_{ic} \cdot g(dG_i) + \gamma' X_{ic} + \theta_c + \varepsilon_{ic}, \quad (1)$$

where y_{ic} is the referendum turnout, or the share of "YES" votes in locality i and county c; $f(dG_i)$ and $g(dG_i)$ are control functions accounting for voters' preferences away from the cut-off, with different parameters on the two sides of the

cut-off; X_{ic} is a vector of locality covariates; I include county fixed effects θ_c to mitigate chance differences in the baseline levels of the outcomes (the regression intercept) which could bias the treatment effect (for a similar approach see e.g. Pinotti, 2017);[18] I report standard errors clustered at county level (all specifications, including those based on a narrow neighbourhood around the threshold, include at least 40 clusters).

The coefficient $\hat{\beta}$ is the estimated ATE of partisan alignment. If local officials supported their parties by means of campaigning or electoral fraud, then one can expect a positive and significant alignment effect on the referendum turnout. The effect on "YES" vote share is ambiguous as this margin was secondary to turnout and the president had low approval ratings at the time. Campaigning by opposing parties to dismiss/keep the president, or ballot stuffing with expressed partisan choices could produce a split in regression functions at the cut-off. On the other hand, turnout buying or artificial inflation by means of null ballot stuffing or made-up figures on return sheets would do nothing to change the vote balance, and thus the RDD estimate on this margin could be insignificantly different from zero.

I estimate the treatment effect using spline polynomials for the control functions (on the entire working sample) and local polynomial approximation within a data-driven optimal bandwidth that minimizes mean-squared-error (MSE) with the robust confidence intervals developed by Calonico et al. (2014). I also report OLS estimates on a small window around the threshold (up to [-5; +5] percentage points), although the results do not hinge on random assignment of localities into alignment at the threshold.

Because elections are disputed by many parties, the strongest candidates could

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[18]This issue is known as the Yule-Simpson Paradox, where effects in the overall sample appear opposite to effects in subsamples, due to differences in baseline characteristics of the subsamples which have not been accounted for.

narrowly win with fewer than 51% of the votes (e.g. winner has 38% and runner-up has 37% vote share). This gives rise to multiple winning thresholds. The causal treatment effect can be identified from normalising to 0 and pooling these thresholds, if standard assumptions hold for all thresholds (Cattaneo et al., 2016). The overall treatment effect is a weighted average over all average differences occurring at each threshold, where the weights are larger for thresholds that occur more often. In terms of external validity, the identification from multiple thresholds (essentially from more places in the aligned candidate's vote share distribution) potentially offers a more global interpretation of the RD estimates than the two-party settings in earlier studies, where the effect is identified only from the 50% winning threshold (occurring in a likely selected sample of localities). However, just like the design with one cut-off in two-party races, the RDD may underestimate the alignment effects if these are larger away from the victory threshold.

4.2 Validity analysis

There are several potential sources of endogeneity of partisan alignment which could undermine the RDD estimates' validity. Firstly, there is a risk of reverse causality if in localities with close elections voters that are more politically active are also more likely to elect mayors from the coalition in government. Furthermore, stark differences between aligned and unaligned close-race localities in terms of electoral behaviour and demographic characteristics would raise concerns that the observed effect is driven by those differences rather than actions of the elected officials at the referendum. Secondly, one might wonder if more corrupt localities or candidates, supported by political machines, manipulate their chances to win the local race and thereafter the referendum results. Manipulation based on corruption in local elections would be consistent with the hypothesis of this paper that local officials provide illicit political capital for their parties. However, it would bias the

RDD results.

Formally, 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); 2) potential outcomes are a continuous function in the running variable at the threshold (Hahn et al., 2001); 3) the density of the running variable is continuous at the threshold.[19] For the RD with multiple thresholds, these assumptions need to hold for all thresholds (Cattaneo et al., 2016).

Although assumption 2) cannot be tested, continuous covariates at the threshold provide support for it. I therefore estimate the RD model with each covariate as the dependent variable, for all and for rural localities. Figures A1 and A2 in the online appendix show separate RD plots for each baseline covariate (in all and in rural localities). These, together with the insignificant RD coefficients suggest that the observable characteristics should not confound the treatment effects (see Table A4 in the online appendix). The two significant differences (in share highly educated and health expenditure) would not occur if p-values were adjusted for multiple testing.

Assumption 3) requires that mayoral candidates (or the political machines that support them) do not have complete control over their realized vote share in local elections. Perfect manipulation is far from guaranteed, given the size of polling stations (even rural localities have an average population over 3000), electoral ob-

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[19]The extensively used electoral RDD has been recently criticised on validity grounds (e.g. Caughey and Sekhon (2011) warned that stronger competitors in the U.S. are more likely to win by a narrow margin). Eggers et al. (2015) surveyed a large number of electoral contexts, refuting the incumbency advantage and concluding that RDD remains sound if these main assumptions withstand thorough testing.

servers, voters and the presence of regulatory bodies and political competition. Some electoral fraud in local elections certainly occurred, but tests show that it did not generate a significant migration of G-aligned candidates over the victory threshold. Appendix Figure A3 histograms of the G-aligned vote margins show a small difference in the percentage of localities with a G-aligned candidate just below and above the threshold, particularly in rural races. However, the corresponding McCrary tests using polynomial approximations of the probability density functions around the 0 cut-off (obtained by pooling the multiple normalised thresholds) present an insignificant discontinuity, as seen in the overlapping confidence intervals around the threshold and the test statistics (t=1.03 for all, 1.21 for rural localities).

There remains a possibility that in some areas political machines mobilised voters in both polls in legitimate or illicit ways, which could bias the RDD estimates upwards. Therefore, after presenting the RD alignment effects in the next section, in section 5.2 I provide evidence of electoral fraud at the referendum based on diagnostic tests which do not require the assumption of no perfect manipulation in local elections. I also present further results in Section 5.3.1, which are robust to excluding regions with famous political machines.

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).[20] 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). By contrast, there is no perceptible change in vote shares around the threshold, and vote shares are fairly stable over the entire range of G' s vote margin.

Figure 2 goes here

The main referendum estimation results are displayed in Table 2: Panel A - all localities, Panel B - rural localities. I present three RD specifications: i) OLS estimations in a tight interval of [-5; +5] percentage points around the cut-off (columns 1 and 4); ii) Robust bias-corrected local linear approximations with the optimal bin selection, adjusting for covariates as in Calonico et al. (2018) (columns 2 and 5); iii) 3rd order polynomial approximations with full samples, allowing the polynomial parameters to differ across the victory threshold (columns 3 and 6). All specifications include county fixed effects to mitigate chance differences in the baseline levels of the outcomes in different regions (the regression intercept) which could bias the treatment effect. The second and third specifications also include locality baseline covariates.[21]

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[20]The regression functions, 95% and 99.5% confidence intervals (reported as per recent calls for enhancing research replicability, e.g. Benjamin et al., 2018) are approximated using third-order polynomials in the vote margin allowed to differ across the victory threshold. The outcome variables are residuals from regressions with county fixed effects.

[21]The estimation results with varying polynomial orders are similar (See Table 5). Note also that a smaller sample with additional covariates (share illiterate, share migrants, share working in agriculture and in public administration) pro-

Table 2 goes here

In Table 2, Panel A, the narrowest bandwidth estimate indicates that G-aligned localities have 4.5 percentage points higher turnout that unaligned localities. Increasing the bandwidth lowers the estimate to 4.3 percentage points (column 2) and using the full sample increases it (5.4 percentage points in column 3). The RD effect on turnout is significant at 0.5% level in all specifications (the highest p-value is 0.002). By contrast, all RD estimates of the alignment-driven difference in the "YES" vote share are close to zero and insignificant.

Because aligned urban localities have a lower turnout than unaligned and not fully comparable localities (Table A3 in the online appendix), these overall sample estimates may be downward biased. I therefore estimate the RD effects separately in rural localities in Panel B. Indeed, the alignment effects are larger for referendum turnout. All RD estimates are significant at 0.5% level (the highest p-value is 0.001), around 5.1 percentage points in the tightest interval around the threshold and 6.2 percentage points in the most complete specification.[22]

Taken together, these estimates show a marked positive impact of partisan alignment on turnout in the referendum, driven by rural localities.[23] Given the

duces similar results (estimates not reported).

[22]The inclusion of controls in the last specification does little to change the estimates. G-localities have proportionally slightly more people in higher education; however, lower education typically correlates with a more manipulable electorate. This implies at worst an underestimation of the RD estimate without controls. Note also that RD estimates from urban races are negative and imprecisely estimated, albeit entirely explained by municipality or county characteristics (estimates not reported).

[23]I estimate the same RD models for parliamentary election turnout at locality level and for the share of votes for G in Senate. All RD estimates are insignificant, refuting a direct alignment effect on these elections (see Appendix B).

lack of popular support for the president in 2012, meeting the 50% participation target would have led the *governing coalition* to a certain victory. The referendum alignment effect is therefore consistent with the interests of the *governing coalition* to obtain the validating quorum.[24] The question that follows is how this mobilization was achieved. Below I conduct some diagnostic tests to highlight the role of electoral fraud 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) better get-out-the-vote campaigning by the government coalition; 3) electoral fraud (vote buying and vote rigging). Regarding the first channel, the time window between new mayors taking office and the referendum was just over a month, ruling out pork-barrel spending between the two polls, at least in terms of large infrastructure projects. Whether such projects deployed ahead of local polls to secure re-election also brought votes in the referendum remains an open question. Nevertheless, the RD design picks up an alignment effect net of such pre-election investments, as long as they are similar or change in a continuous fashion across the local victory cut-off. This is confirmed in Table 1 and Appendix Figure A2, which show that 2011 local expenditures change seamlessly in narrow races.

In terms of legitimate efforts to influence turnout, differential campaigning

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[24]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 A5 shows that the president's party close winners have lower turnout than close runner-ups, also in rural localities. The discontinuity estimates are similar in magnitude to their counterparts in Table 2. The usual validity checks hold for the P-alignment treatment.

efforts cannot be ruled out, as locality-level data on campaign spending is not available. The only figures available on aggregate expenditures suggest the social-democrats and liberals spent the equivalent of 1 million dollars, while the president and his party spent around 800 thousand dollars (Romanian Electoral Authority Referendum Report, 2012, p.37). It is likely that this channel contributed to the gap in turnout between G and non-G localities.

However, anecdotal evidence points also to electoral corruption. In Romania, like in many young democracies, such practice is commonplace and local networks and their monopoly on political and administrative resources 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 target is the turnout (which is observable and enforceable) and thus vote buying would pay off. To my knowledge, there doesn't exist any comprehensive survey at locality level on electoral bribes at the 2012 referendum. I therefore rely on official voting data to identify the symptoms and extent of vote rigging and deduce how much of the alignment effect can be attributed to the alternative channels. Below I present the analysis of electoral fraud.

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. A court investigation of 74 officials for fraud in the referendum concluded in April 2016 with the sentencing of the leader of the *social democrats* (at the time

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of the referendum a minister in the Ponta cabinet and campaign manager). He was convicted for ordering and coordinating referendum fraud on a large scale by various means, including vote buying and fake ballots (e.g. for deceased persons, or people working abroad at the time).

To further substantiate the evidence on vote rigging I carry out a series of diagnostic tests in line with the most recent developments in electoral forensics: 1) I investigate anomalies in the distributions of the last digit of the referendum vote counts across polling stations; 2) I test for differences in the prevalence of null votes between aligned and unaligned localities; 3) I estimate finite mixture models inspired by the methods consecrated by Mebane(2016); 4) I investigate discontinuities in the density of polling stations across the referendum turnout distribution.

First, 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 Beber and Scacco (2012), adapted after Mebane (2006) 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). Since humans cannot perfectly reproduce randomness, when return sheets are tampered with, last digit frequencies deviate from the uniform distribution (often displaying a higher prevalence of "0"). Beber and Scacco (2012) contrast Sweden's fair elections' uniform digit distributions with Nigeria's 2003 fraud-ridden election digit frequencies; in the latter the digit "0" had a significantly higher than 10% frequency. Weidmann and Callen (2013) also find abnormal digit counts in the 2009 Afghan presidential elections, consistent with alternative evidence of electoral fraud. Hence, if the last digit distribution displays frequencies significantly different from 10%, there are reasons to suspect vote rigging. Figure 3 shows side-by-side the last digit dis-

tributions for the total vote count (voter turnout) and for the valid votes count reported by polling stations. Of the c. 17,000 polling stations, over 85% have 3- or 4- digit long vote counts. The average polling station size was 970 registered voters (S.D. 528) and the average station turnout was 450 voters (S.D. 267). The distributions are plotted for all localities (Figure 3a) and rural localities (Figure 3b) and 95% confidence bands are also displayed.

Figure 3 goes here

Figure 4 goes here

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" is particularly evident in rural localities), while the digit "7" appears less often than expected. The *chi-square* test of the hypothesis that the distribution of the last digit in turnout is uniform returns a p-value of 0.003 (all localities) and 0.021 (rural localities), suggesting there is evidence that the digit frequencies deviate from 10% (significant at 0.5% level for all localities, suggestive for rural localities; however, a two-sample Kolmogorov-Smirnov test for equality of distribution functions of last digits across rural and urban localities returns a p-value of 0.943, meaning that these distributions are not significantly different). By contrast, 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.

Recent developments in electoral forensics (see Hicken and Mebane, 2017) enable further investigation of the nature of this fraud. Looking at rounded vote

and turnout proportions, one may detect an intention of individual polling stations to signal their effort to commit fraud. For instance, in Russian elections it has been shown that the last digit of the rounded turnout and vote proportion was more often than expected 0 or 5. Therefore, in Table 3 I display the relative frequency of 0 and 5 in the last digit of both turnout and valid vote counts (C05) and that of rounded turnout and "YES" vote proportions (P05). In fair elections, C05 and P05 are expected to be 0.2. Consistent with the Beber and Scacco (2012) tests, the last digit of turnout count is 0 or 5 significantly more often than 20% of the time, as the p-values from t-tests show. However, neither valid vote counts, nor rounded percentages (P05) display abnormal last digit frequencies. There is some suggestive evidence of fraud, but no clear evidence of signalling manipulation efforts by individual polling stations.

Table 3 goes here

Thus, total vote counts present anomalies that valid vote counts do not. Given that their difference consists of null votes (votes deemed invalid, e.g. due to the misplacement of the stamp), these null votes could be one method to boost turnout on the return sheets. In rural localities, those controlled by G display a mean of around 22 null votes (6.6 per polling station), significantly higher at 0.5% than the 16.5 null votes (4.9 per polling station) in unaligned localities. These averages are unlikely to result from perfect coordination across polling stations. Instead, most stations have a small number of null votes (50% under 4 null votes in rural G-aligned localities, fairly uniformly distributed across 1-5 votes, and 50% under 3 null votes in other rural localities). However, the entire distribution of null vote counts in G-aligned localities is slightly shifted to the right, and these localities display a few stations with very high numbers of reported null votes: 38 stations with higher than 50 null votes, with a maximum of 249 null votes, compared to only 9 stations with over 50 null votes, with a maximum of 100 in the other localities.

These differences are very small and unlikely to explain the entire alignment effect detected in the RD, but perhaps a part of it.

To test this formally, in Table 4 I display estimates from the standard RD regressions (the counterpart of specifications 1 and 3 in Table 2), where the dependent variable is the polling station average number of null votes (for rural localities). I choose the station average as an outcome because the return sheets and any ballot stuffing would have occurred at the stations. The results are similar when estimating the locality number of null votes, because rural localities typically have between one and three polling stations. The estimates from a narrow region around the victory threshold are positive, yet very imprecise. The RD estimate from the polynomial approximation over the full sample (column (2)) is positive and significant at 5% level, suggesting higher prevalence of null ballots in G-controlled localities. Moreover, the same estimate from localities with turnout below the 50% quorum are significant and double those from the subsample of localities with turnout above the 50% threshold. G-aligned localities with low turnout have an additional 2 null ballots per station compared to G-unaligned localities with low turnout. The difference in high-turnout localities is statistically insignificant. This is suggestive evidence that efforts to inflate turnout artificially by means of stuffing spoilt ballots may have been encouraged in low-turnout areas. In terms of magnitude, this effect is small compared to the overall alignment effect. With an average of 346 voters per station turning up to vote in rural localities that were not aligned with G, the additional 2 null votes in G localities inflated the turnout by nearly 0.6%, which is only a tenth of the overall alignment effect in rural localities. The remainder is likely the result of either legitimate campaigning efforts or other forms of fraud, such as turnout buying or manipulating the return sheets.

Table 4 goes here

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Experts in electoral forensics have also developed tools to assess more precisely the "amount " or the probability of fraud in elections. Departing from the idea that the turnout percentage and winner vote share distributions should be normally distributed in no-frauds elections, (Mebane, 2016) expanded a model from (Klimek et al., 2012) using finite-mixture likelihood models to detect when these electoral outcomes come from different distributions. The idea is that some fraud converts votes from the opponent or from the pool of eligible voters into votes for the winner, distorting the vote percentage and turnout distributions. Using the finite-mixture model on polling station data, the author retrieves the probabilities that individual units pertain to one of three classes: no fraud, incremental fraud or extreme fraud. Throughout the analysis in the present paper, it was found consistently that turnout displays abnormal patterns, whereas vote shares do not. Thus, in this context we do not expect frauds that fit the Mebane(2016) framework. However, in a wider sense, one can fit a finite-mixture model on polling station data to see whether polling stations can be assigned into different classes in terms of the alignment effect detected in the RD analysis. Appendix Table A6 displays the classic RD estimates on all polling stations in the country (only races against G) and the counterpart estimates from a finite-mixture model with two components, corresponding to latent characteristics possibly consistent with fraud. Columns (1)-(3) display results for referendum turnout, and columns (4)-(6) for the share of "YES " votes at polling stations). The two classes show qualitatively different RD estimates for turnout exclusively. In column (2) we see a G-alignment effect (the effect of the polling station being located in a G locality on polling station outcome) of -0.587, statistically insignificant. In column (3), the alignment estimate is 4.007, significant at 1% level (p-value=0.007). The fact that some polling stations display some alignment effects and some do not, further substantiates the evidence of frauds in a share of stations. The probability π of the second class is 46.3%, meaning that nearly half of polling stations display a

suspicious behaviour.

As a complementary analysis, in Figure 5 I display the McCrary density discontinuity plots adapted to examine the density of polling stations in terms of their reported referendum turnout, for all localities (Figure 5a) and for rural localities (Figure 5b). I further separate the graphs for localities controlled by G and non-G, respectively. The plots provide tests of density discontinuity around the 50% referendum turnout threshold. While this was a national target, one can assume this was also a natural lower threshold for the polling stations, if only based on mental accounting (in fact, this was corroborated in court trial evidence). A significant positive discontinuity in density to the right of the 50% mark would indicate an abnormal concentration of polling stations above the quorum requirement, and hence a manipulation of turnout figures. 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). Varying the threshold between 40% and 70% to account for other potential targets at the polling station level does not reveal any density discontinuities. This is an additional evidence of artificial increases in turnout, in line with the main results in Table 2 and the above diagnostic tests.

Figure 5 goes here

Taken together, these tests strengthen the evidence that electoral fraud underpins at least in part the referendum result differences between G-aligned and unaligned localities. The evidence points towards an exclusive focus on turnout manipulation in line with G's incentives to boost turnout, but also some turnout manipulation by other parties in non-G localities. There is mild evidence of fraud by means of null ballot stuffing (but this accounts for a small fraction of the alignment effect), and the last digit tests suggest the manipulation of numbers on return sheets, but not in a way that signalled the polling stations' fraud efforts.

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Note that there is no evidence that the turnout was artificially inflated/reduced by deliberate reduction/increase in the number of registered voters. A comparison of local and referendum registered voters shows a small overall reduction from June to July polls, but this is similar across G-aligned and unaligned localities (also confirmed by RDD regressions with the numbers of registered voters and their difference as dependent variables).

5.3   Alternative specifications and further results

In this section I present some sensitivity tests with alternative treatment definition and different order polynomials, and further outcomes in terms of government transfers. I focus on the rural sample, where the main alignment effect was identified.

5.3.1   Alternative specifications

Firstly, I report the baseline RD estimates including control functions of varying polynomial orders, and the estimates in Table 5 are consistent across all the different specifications. The estimates are slightly lower in magnitude when fitting fourth order polynomials, but they remain statistically significant across all specifications. Moreover, including covariates leaves the estimates largely unchanged. OLS regression estimates in very narrow bandwidths (1-4 percentage points) display consistent effects, up to 6.2% and 6.7% in all and rural localities in the narrowest window, albeit with higher standard errors (Appendix Table A7). I also perform an additional check to rule out the concern that excess vote shares for G in local elections may have mechanically generated a larger turnout if G supporters are politically more active: 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 unaffected (results

available on request).

Table 5 goes here

Secondly, I revisit the treatment definition, where I now include the independent *liberals/conservatives* in the treatment rather than the control group. This automatically excludes from the sample the narrow races between *social-democrats* and *liberals/conservatives*. If dissident *liberals/conservatives* did fully align with the interests of the governing coalition, we would expect lower RD estimates with the treatment thus redefined. Table 6 shows slightly smaller effects on referendum turnout, albeit significant at 0.5% in specifications (2) and (3) (the RD estimate of G-alignment in rural localities is between 4.6 and 5.9 percentage points).[25]

Thirdly, considering the presence of political machines, I rerun the estimations excluding the most infamous (a large share of which were Southern county barons in the top-PSD clique). I identified the county council presidents with criminal convictions for corruption or abuse of power in the post-2012 years, who headed the regional party networks, and I excluded their counties from the sample. The results displayed in appendix table A8 show magnitudes similar to the main results (4.8-6.8%), significant at 0.5% level. This suggests the vote rigging operations were orchestrated everywhere in the country, with the help of local partymen, not driven by a handful of powerful networks.

Finally, one could reasonably expect that localities where G-mayors were incumbents before 2012 had more time to organise the referendum campaign or fraud. The RDD regressions split by localities with a G-aligned or a different mayor prior to 2012 show inconclusive differences in estimates in the two subsam-

[25]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 6.

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ples, with some significant estimates at 1-10% in localities with non-G mayors in previous elections (estimates available upon request). The results do not suggest a disadvantage of new incoming mayors in boosting referendum turnout.

Table 6 goes here

5.3.2 Further results: suggestive evidence from government transfers

The hypothesized political *quid-pro-quo* would entail a return on the local politicians' efforts to boost referendum turnout. An increasing number of studies have shown that transfers to local governments unequivocally follow the party in power. I investigate whether there are effects of G alignment on government transfers to localities, focusing on the rural regions.

I estimate the differences in per capita locality revenues in the years after the referendum, within the standard RD specifications. In Table 7 the RD alignment estimates are positive, but significant at 10% (p-values 0.014 and 0.093 in specifications 3 and 4, respectively) only for total locality revenue per capita in 2013, which is larger than in 2012. This is unsurprising given the reorganisation of the government, which happened mid-year in 2012. The results are in stark contrast to total revenue in 2011, the pre-treatment year, when they were lower for localities won over by G in later local elections (see appendix Table A2). Results on local tax revenues show no significant discontinuities, hence I zoom in on subsidies from the central government. The two potentially most visible to voters are roads and other subsidies. These do not display significant alignment effects at any conventional level (although at least in average terms, the balance of road subsidies has changed in favour of aligned localities relative to 2011).

Table 7 goes here

Going one step further, I estimate the 2013 revenue differences by the locality turnout achieved at the referendum. The question is whether larger turnouts

were rewarded with larger revenues. Table 8 reports RD estimates from OLS and non-parametric specifications for (columns 1-4), and difference-in-discontinuities estimates (columns 5-6). The former report side-by-side the discontinuities in localities with turnout above and below 50%; the latter take the average difference in alignment effects on revenues between localities which reached over 50% turnout and under 50% turnout. The assumption here is that the local politicians exerted efforts to achieve the national threshold at locality level. The diff-in-disc specifications are similar to the RD specifications, but in addition include interactions between the alignment treatment (and control function, where present) and a dummy for whether referendum turnout exceeded 50%; the diff-in-disc estimator is the coefficient on the treatment (G wins) interacted with the dummy for turnout above 50% (for a discussion and application of this technique see Grembi et al., 2012).

Table 8 goes here

The results in Table 8 are only suggestive of rewarded efforts to boost turnout: narrowly won G localities display larger per capita revenues and road subsidies where they achieved above quorum turnouts than where they fell short (the p-values are below 5%, but above 0.5%, in specifications 2 or 4, panels A and B). The differences in these rewards do not appear significant in the diff-in-disc specifications. This is perhaps due to the fact that the RD estimates' standard errors in localities with low turnout are quite large, suggesting that some of these also received large transfers. In fact, the 50% threshold is not the only relevant target if rewards were channelled to places which performed well relative to expectations, even if absolute turnout was less than the quorum. It thus transpires that some reciprocal transfers to "deserving" local G politicians may have taken place. However, the 50% turnout distinction may not be able to capture the full extent.

To sum up, the results on total revenue per capita in Table 7 suggest a gradual

reversal of the previous distribution of government funds, favouring G-aligned localities from 2012 onwards. The results in Table 8 suggest that higher turnout may have been on average rewarded with slightly higher transfers. Note also that these estimates might underestimate the alignment bonuses if transfers are strategic, and higher in the years just before new local elections, i.e. 2014-2015.

6   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 may give rise to electoral fraud and electoral manipulation. I present evidence consistent with electoral fraud from a regression discontinuity design in which I compare an impeachment referendum's outcomes following narrow local elections in Romania in 2012, combined with electoral forensics tests that display vote count and null ballot anomalies across polling stations.

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 central politicians the access and logistics to stuff null ballots or buy participation.

This, however, is not the tale of one referendum, but the illustration of a low-cost/high-return setting for electoral fraud, particularly pernicious in closely-disputed elections. African elections are fraught with vote buying and electoral violence despite intense international monitoring. Similar practices regularly make headlines in Latin America and South-East Asia. Ironically, in Russia's most recent elections the vote-rigging practices were caught live on webcams installed at polling stations to increase transparency. From the traditional vote buying in

African elections to the sophisticated voter manipulation in US and UK elections through social media profiling, efforts to detect and prevent such practices are only now emerging, and thus it is crucial to understand how political actors bend the rules to secure electoral success. Governments in young and fragile democracies prove time and time again an extraordinary ability to disguise the autocratic legacy of vicious self-serving manipulation as legitimate democratic exercises. 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 are fated never to succeed. The challenge is to educate and arm voters with the tools to keep the politicians' prerogatives in check. The ensuing themes for future research are voter information, political participation and technology as means to strengthen fragile democracies.

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Figures and Tables

Figure 1 Timeline of electoral events
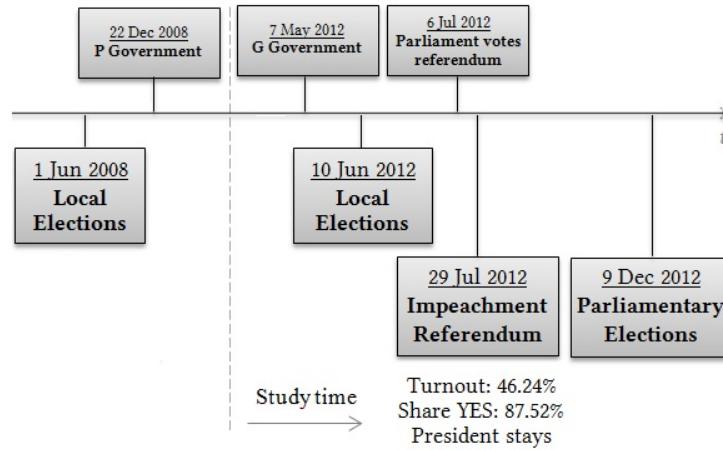
Note: The P Government was formed by the president's party (*the democrat-liberals*). The G Government was formed by the *social-liberal union*, the main opponents to the president's party rule.

Figure 2: G-alignment and referendum outcomes

(a) All localities
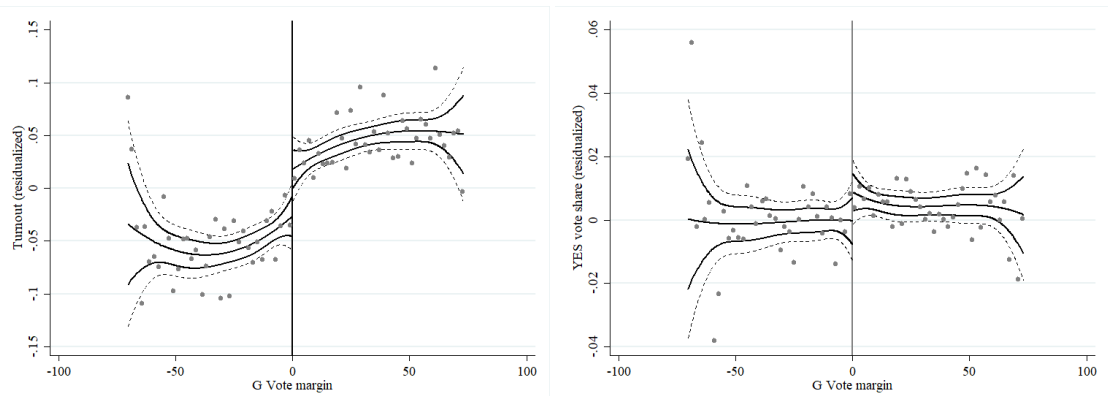
(b) Rural localities

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). 95% confidence intervals displayed in solid lines, 99.5% confidence intervals displayed in dashed lines

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
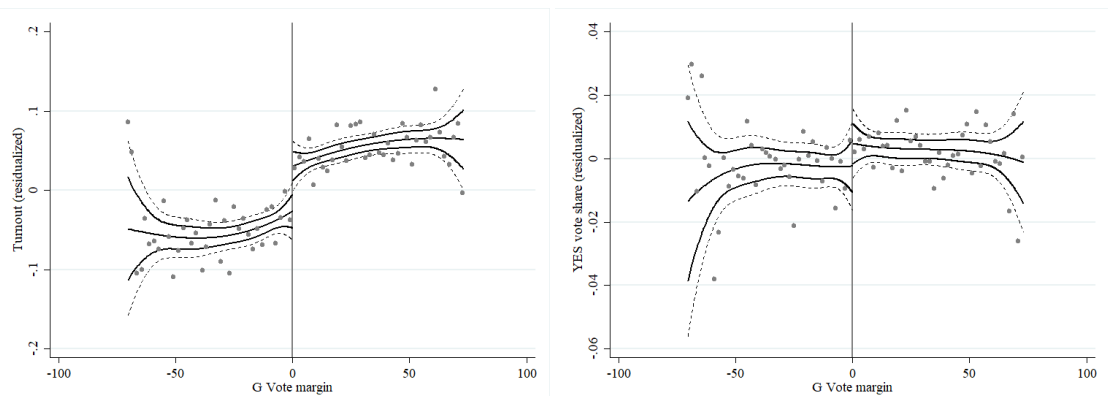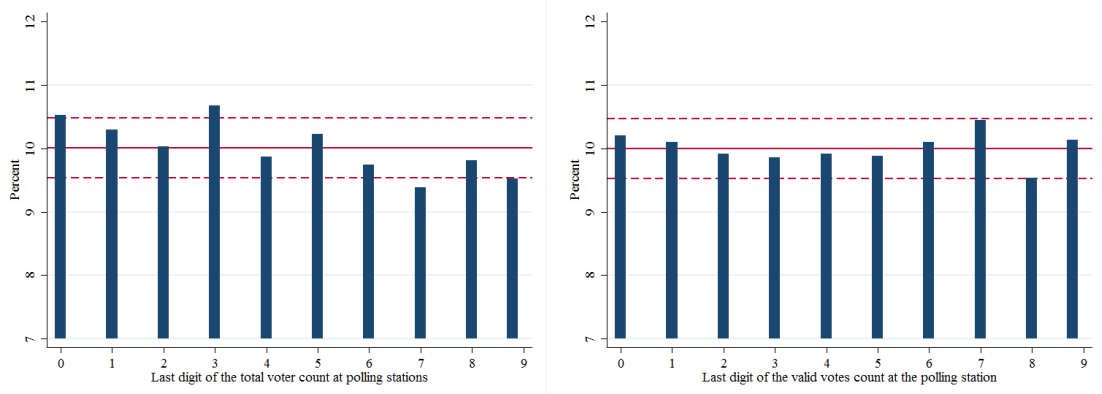

Note: The figure displays the distribution of the last digit of the referendum turnout (left) and the valid votes count (right) across polling stations. This distribution is based on the sample of polling stations with a 3- or 4- digit vote count (which make up more than 85% of the c. 17,000 stations). The last digit distributions are displayed for all polling stations in Figure 3(a) and for polling stations in rural localities in Figure 3(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. Chi-square p-values for the test that the distributions are uniform are 0.484 and 0.546 (valid vote counts digits, all and rural localities), and 0.003 and 0.021 (turnout digits, all and rural localities), suggesting manipulation only in turnout digits.
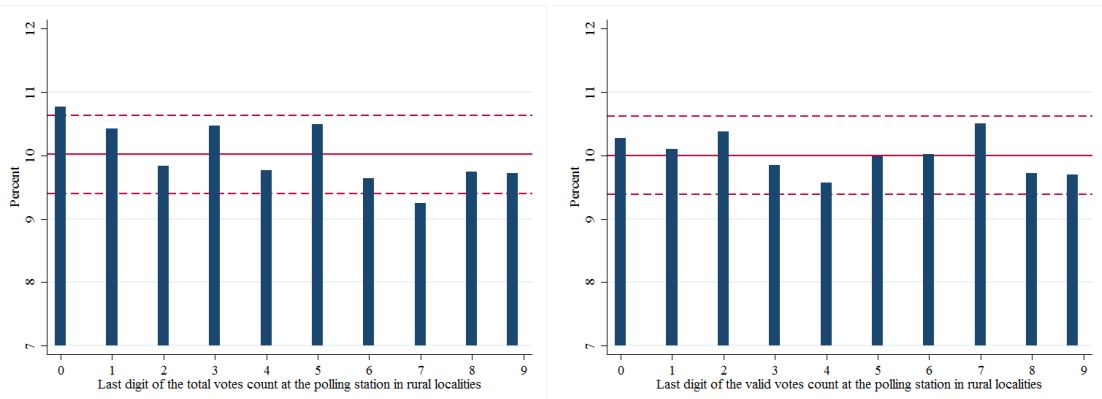
Figure 4: Distributions of the last digit in the total vote count (turnout) and the valid vote count by polling stations in G and non-G localities
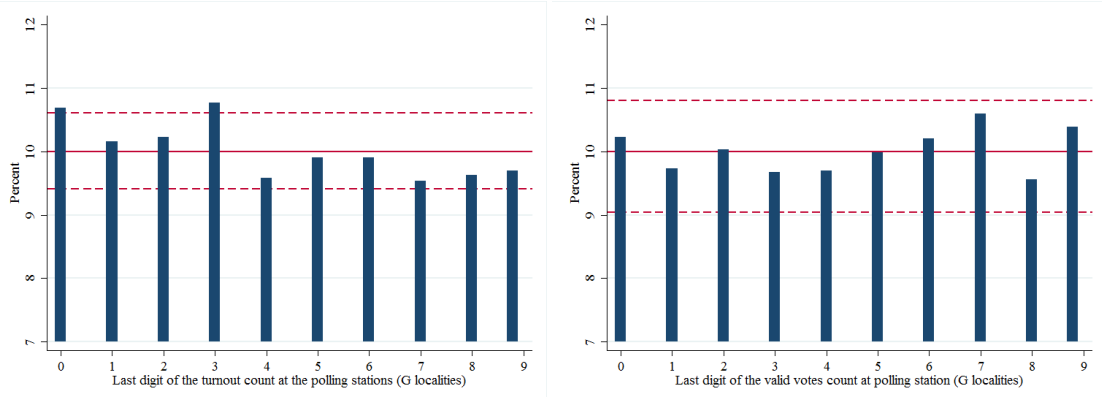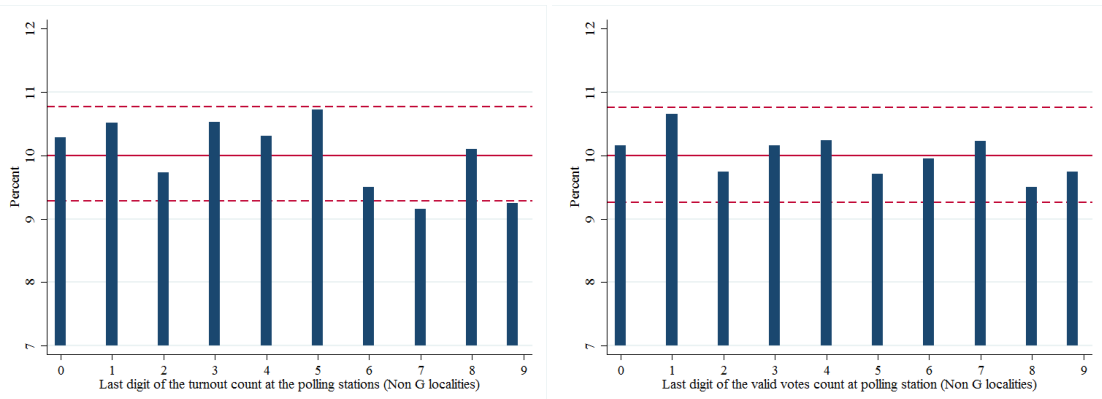
(a) G localities

(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 c. 17,000 stations). 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. Chi-square p-values for the test that the distributions are uniform are 0.343 and 0.679 (valid vote counts digits, G and non-G localities), and 0.051 and 0.000 (turnout digits, G and non-G localities localities), suggesting manipulation only in turnout digits.

Figure 5: Density discontinuity tests for turnout reported at polling stations
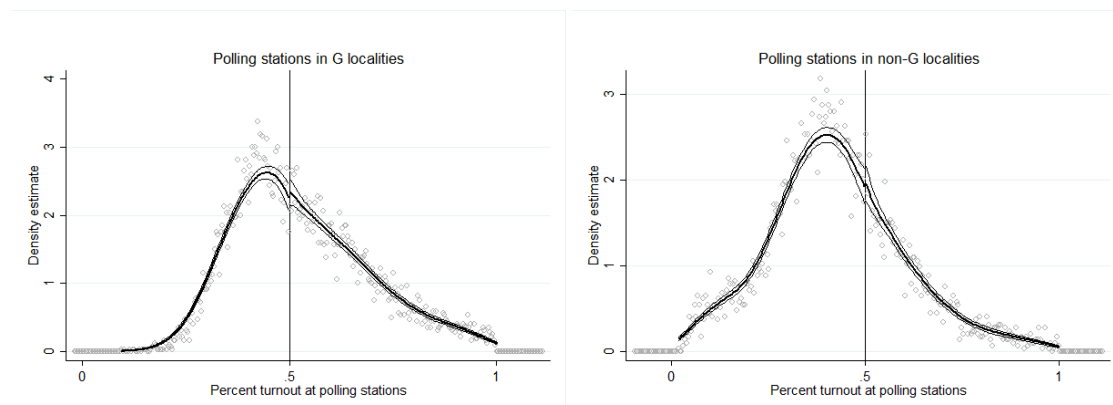
(a) All localities

(b) Rural localities
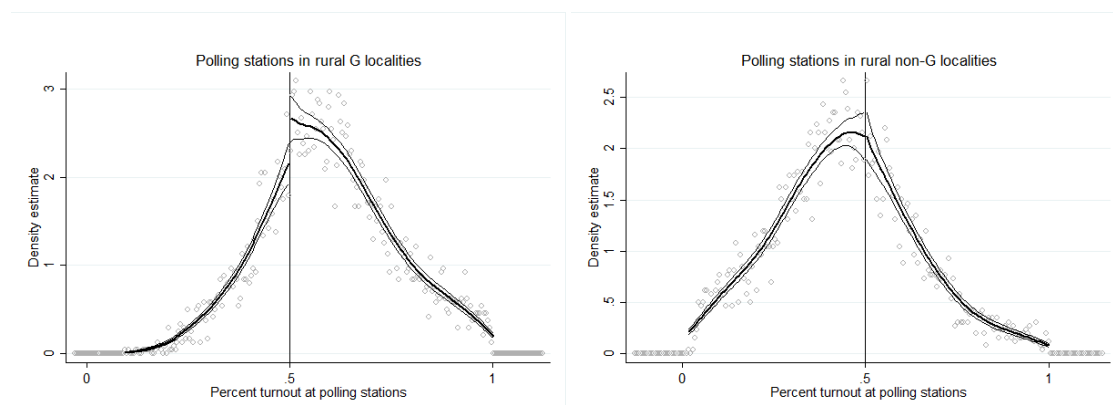
Note: The figure displays the density discontinuity test developed by Mc-Crary(2008), adapted here to polling stations in terms of their reported referendum turnout. Turnout is represented on the X-axis and the corresponding density on the Y-axis. The vertical line intersecting the X-axis at 0.5 marks the national target turnout for the referendum to be validated. A discontinuity in density around 0.5 (i.e 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 5b, 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% (2% of all polling stations).

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

	All localities			Rural localities		
	Non-G	G	p-value	Non-G	G	p-value
Panel A: Outcomes						
Referendum Turnout	0.528	0.553	0.176	0.531	0.566	0.088
Share YES	0.868	0.864	0.463	0.866	0.863	0.619
Parliamentary Elections Turnout	0.430	0.435	0.698	0.429	0.437	0.524
G Vote Share Senate	0.613	0.611	0.895	0.614	0.609	0.780
Panel B: Baseline Covariates						
No. candidates in local elections	5.244	5.222	0.916	5.076	5.036	0.847
Sum % votes top 2 local candidates	79.669	79.985	0.852	79.723	80.210	0.796
Turnout referendum 2007	44.478	44.782	0.793	43.974	44.751	0.546
Log population	8.177	8.152	0.820	7.943	7.926	0.824
Share adults	0.772	0.768	0.520	0.768	0.767	0.867
Share over 65	0.209	0.203	0.401	0.219	0.213	0.475
Share males	0.495	0.496	0.886	0.497	0.497	0.934
Share high education	0.048	0.050	0.777	0.034	0.040	0.064
Share high school	0.160	0.163	0.664	0.141	0.149	0.258
Share Romanian	0.880	0.883	0.884	0.889	0.885	0.847
Share unemployed	0.053	0.051	0.730	0.056	0.054	0.743
Per capita revenue	1405.986	1390.731	0.891	1338.739	1400.887	0.627
Per capita local revenue	455.977	483.665	0.452	409.855	447.069	0.323
Per capita road funds	14.989	11.005	0.532	16.919	11.980	0.517
Per capita subsidies	149.042	113.401	0.342	109.617	125.291	0.685
Per capita expenditure	1346.232	1281.949	0.519	1267.771	1276.639	0.937
Per capita education expenditure	375.396	364.864	0.642	345.181	337.243	0.727
Per capita health expenditure	12.802	7.884	0.130	10.699	6.278	0.227
Per capita public expenditure	162.911	137.353	0.319	137.195	138.022	0.975
Observations	127	162		105	138	

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 and for rural localities within 5 percentage points victory margin. The p-values for the differences in means tests are reported.

Table 2: Alignment and referendum outcomes. RD Estimates

	Referendum Turnout			**Referendum Yes Vote Share**		
	(1) OLS	(2) Local LR	(3) Polynomial	(4) OLS	(5) Local LR	(6) Polynomial
			Panel A: All localities			
G wins	0.045	0.043	0.054	-0.002	-0.002	0.006
	(0.013)	(0.014)	(0.014)	(0.006)	(0.005)	(0.007)
Obs.	289	949	2,386	289	899	2,386
Specification	[+/-5]	CCT Opt. h [+/-17.05]	3rd Ord.	[+/-5]	CCT Opt. h [+/-15.99]	3rd Ord.
			Panel B: Rural localities			
G wins	0.051	0.056	0.062	-0.001	0.003	0.006
	(0.014)	(0.015)	(0.015)	(0.008)	(0.006)	(0.008)
Obs.	243	898	2,116	243	1,002	2,116
Specification	[+/-5]	CCT Opt. h [+/-18.41]	3rd Ord.	[+/-5]	CCT Opt. h [+/-21.63]	3rd Ord.
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	No	No	Yes

Notes: The table displays RD estimates of the effect of *government coalition* (G) alignment on referendum turnout (columns 1-3) and the "YES" vote share (columns 4-6). Results are based on all localities (Panel A) and rural localities (Panel B). Estimates from OLS regression in a small interval around the cut-off in columns (1) and (4). Estimates from local linear regression using the Calonico et al. (2014) robust bias-corrected confidence intervals in columns (2) and (5). Estimates using polynomial approximations on the full sample including baseline covariates in columns (3) and (6). The covariates include demographic, labor market, fiscal and electoral characteristics of the localities (see Table 1). All columns include county fixed effects. Standard Errors clustered at county level in parentheses.

Table 3: Suggestive evidence: The frequency of 0 or 5 in the last digit of vote counts and percentages at polling stations

Statistic	Obs	Mean	P-value
Panel A All localities			
Referendum Turnout C05	15572	0.206	**0.041**
Referendum Valid Votes C05	15572	0.200	0.959
Referendum Turnout P05	15572	0.200	0.819
Referendum Share YES P05	15572	0.203	0.454
Panel B Rural localities			
Referendum Turnout C05	9039	0.211	**0.012**
Referendum Valid Votes C05	9039	0.202	0.597
Referendum Turnout P05	9039	0.203	0.477
Referendum Share YES P05	9039	0.205	0.201

Notes: The table displays the frequency of 0 or 5 in the last digit of referendum turnout and vote counts (C05) and referendum turnout and vote percentages (P05) at polling stations. P-values for t-tests of $H0 : C05 = 0.2$ and $H0 : P05 = 0.2$ are reported in the last column. Hicken and Mebane (2017) point out that the implied value of C05 and P05 with no fraud (last digits follow a uniform distribution) is 0.2. Polling stations may wish to signal their efforts to commit fraud by displaying numbers ending in 0 or 5. Thus, rejecting $H0$ is an indication of electoral fraud.

Table 4: Suggestive evidence: alignment and null votes (rural localities)

	Average number of null votes per station					
	(1) OLS	(2) Polynomial	(3) OLS	(4) Polynomial	(5) OLS	(6) Polynomial
	Overall		**Turnout < 50%**		**Turnout >= 50%**	
G wins	0.773	1.282	0.906	2.093	0.797	0.981
	(0.484)	(0.581)	(0.710)	(0.877)	(0.784)	(0.791)
Observations	243	2,116	94	840	149	1,276
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Specification	[+/-5]	3rd Ord. Poly.	[+/-5]	3rd Ord. Poly.	[+/-5]	3rd Ord. Poly.

Notes: The table displays RD estimates of the effect of *governing coalition* (G) alignment on average number of null votes per polling station in rural localities, in: all rural localities (columns 1-2); rural localities with referendum turnout below 50% (columns 3-4); rural localities with referendum turnout equal to or above 50%. Estimates from OLS regressions in a small interval around the cut-off in columns (1),(3) and (5). Estimates using polynomial approximations on the full sample, with covariates and county fixed effects in columns (2),(4) and (6). All specifications include county fixed effects. Standard Errors clustered at county level in parentheses.

Table 5: Alignment and referendum turnout. Alternative polynomials

	Turnout					
	(1)	(2)	(3)	(4)	(5)	(6)
			Poly. Approx. Full sample			
			Panel A: All localities			
G wins	0.065	0.066	0.042	0.050	0.040	0.041
	(0.008)	(0.008)	(0.012)	(0.010)	(0.018)	(0.016)
Obs.	2,386	2,386	2,386	2,386	2,386	2,386
			Panel B: Rural localities			
G wins	0.058	0.062	0.058	0.062	0.050	0.050
	(0.016)	(0.015)	(0.016)	(0.015)	(0.018)	(0.017)
Observations	2,116	2,116	2,116	2,116	2,116	2,116
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Specification	1st Ord.	1st Ord.	2nd Ord.	2nd Ord.	4th Ord.	4th Ord.
	Poly.	Poly.	Poly.	Poly.	Poly.	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 and with baseline controls. All specifications include county fixed effects. The controls include demographic, labor market, fiscal and electoral characteristics of the localities. Standard Errors clustered at county level in parentheses.

Table 6: Alignment and referendum outcomes - alternative treatment. RD Estimates

	Referendum Turnout			Referendum Yes Vote Share		
	(1) OLS	(2) Local LR	(3) Polynomial	(4) OLS	(5) Local LR	(6) Polynomial
Panel A: All localities						
G or L/C wins	0.037 (0.014)	0.046 (0.013)	0.050 (0.013)	0.002 (0.007)	-0.002 (0.005)	0.010 (0.007)
Obs.	276	980	2,203	276	738	2,203
Specification	[+/-5]	CCT Opt. h [+/-19.16]	3rd Ord.	[+/-5]	CCT Opt. h [+/-14.23]	3rd Ord.
Panel B: Rural localities						
G or L/C wins	0.046 (0.015)	0.059 (0.014)	0.057 (0.014)	0.003 (0.008)	0.002 (0.006)	0.009 (0.008)
Obs.	238	854	1,961	238	730	1,961
Specification	[+/-5]	CCT Opt. h [+/-18.76]	3rd Ord.	[+/-5]	CCT Opt. h [+/- 15.80]	3rd Ord.
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	No	No	Yes

Notes: The table displays RD estimates of the effect of *governing coalition or associated parties* (G or *Liberals/Conservatives*) alignment on referendum turnout (columns 1-3) and the "YES" vote share (columns 4-6). Results are based on all localities (Panel A) and rural localities (Panel B). Estimates from OLS regressions in a small interval around the cut-off in columns (1) and (4). Estimates from local linear regression using the Calonico et al. (2014) robust bias-corrected confidence intervals in columns (2) and (5). Estimates using polynomial approximations on the full sample including baseline covariates in columns (3) and (6). The covariates include demographic, labor market, fiscal and electoral characteristics of the localities (see Table 1). All columns include county fixed effects. Standard Errors clustered at county level in parentheses.

Table 7: Alignment and locality revenues in 2012-2013 (rural). RD Estimates

	Locality Fiscal Revenues			
	(1)	(2)	(3)	(4)
	OLS	Poly. Approx. Full sample	OLS	Poly. Approx. Full sample
	Panel A: Total per capita income			
	2012		2013	
G wins	142.525	108.883	247.277	222.262
	(127.787)	(110.568)	(96.338)	(129.179)
Obs.	243	2,116	243	2,116
	Panel B: Per capita road subsidies			
	2012		2013	
G wins	6.814	2.533	3.833	2.158
	(4.630)	(3.680)	(2.465)	(1.955)
Obs.	243	2,116	243	2,116
	Panel C: Per capita other subsidies			
	2012		2013	
G wins	41.994	32.947	49.605	15.901
	(27.902)	(24.470)	(22.096)	(22.926)
Obs.	243	2,116	243	2,116
County FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Specification	[+/-5]	3rd Ord. Poly.	[+/-5]	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)-(2) and from 2013 in columns (3)-(4). Estimates from OLS regressions in a small interval around the cut-off in columns (1) and (3). Estimates using polynomial approximations on the full sample, with controls and with county fixed effects in columns (2) and (4). The controls include demographic, labor market, fiscal and electoral characteristics of the localities before 2012. Standard Errors clustered at county level in parentheses.

Table 8: Alignment and locality revenues in 2013 by turnout (rural). RD Estimates and Difference-in-Discontinuities

	(1)	(2)	(3)	(4)	(5)	(6)
	Locality Fiscal Revenues					
	Turnout% <50	Turnout% >50	Turnout% <50	Turnout% >50	Diff-in-Disc	Diff-in-Disc
		Panel A: Total per capita income 2013				
G wins	177.937	302.358	153.975	203.715	161.612	238.650
	(176.487)	(139.012)	(205.823)	(149.336)	(149.299)	(199.345)
G wins x Turnout>50%					116.075	-41.470
					(180.199)	(219.431)
Obs.	94	149	840	1,276	243	2,116
		Panel B: Per capita road subsidies 2013				
G wins	6.243	2.917	0.365	3.669	4.875	0.147
	(8.630)	(1.399)	(4.443)	(1.775)	(6.103)	(2.980)
G wins x Turnout>50%					-2.017	3.935
					(6.201)	(3.838)
Obs.	94	149	840	1,276	243	2,116
		Panel C: Per capita other subsidies 2013				
G wins	42.333	52.306	21.665	5.267	52.277	31.981
	(47.068)	(23.337)	(31.886)	(30.306)	(39.996)	(33.345)
G wins x Turnout>50%					-2.122	-27.426
					(40.390)	(41.444)
Obs.	94	149	840	1,276	243	2,116
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	No	Yes
Specification	[+/-5]	[+/-5]	3rd Ord. Poly.	3rd Ord. Poly.	[+/-5]	3rd Ord. Poly.

Notes: The table displays RD estimates of the effect of *governing coalition* (G) alignment on local revenues in 2013 in rural localities. RD estimates in columns (1)-(4). Difference-in-discontinuities estimates in columns (5) and (6). Estimates from OLS regressions in a small interval around the cut-off in columns (1)-(2) and (5). Estimates using polynomial approximations on the full sample, with controls and with county fixed effects in columns (3)-(4) and (6). The controls include demographic, labor market, fiscal and electoral characteristics of the localities before 2012. Standard Errors clustered at county level in parentheses.