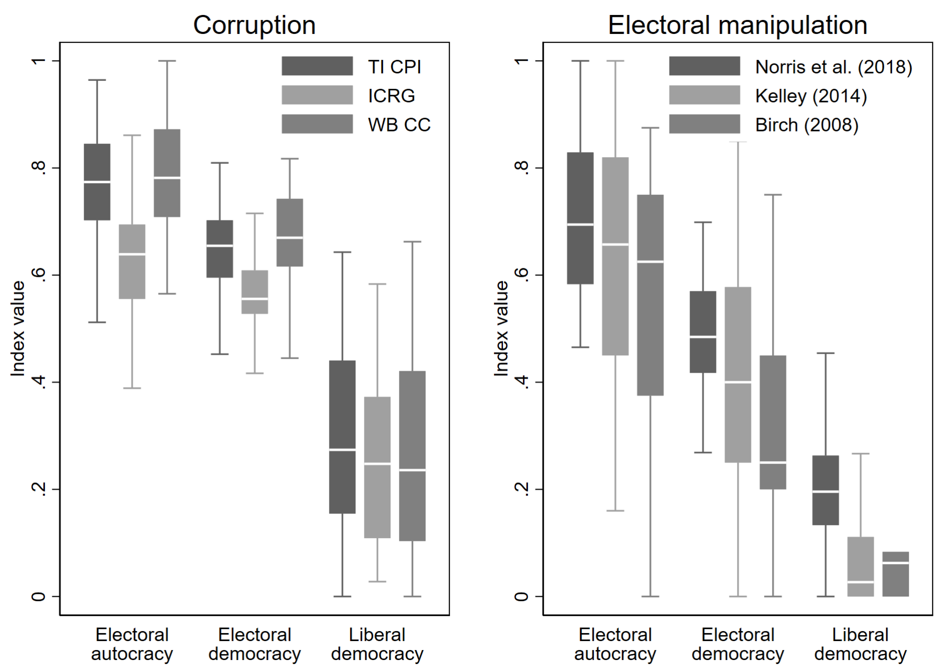
Supplementary Appendix to "Anti-Corruption Efforts and Electoral Manipulation in Democracies"

Contents

A1 Corruption and Electoral Manipulation Across Regime Types

Figure A1 shows boxplots of additional measures of corruption (left plot) and electoral manipulation (right plot) by regime type. Regime types are defined by variable `v2x_regime` in the V-Dem country-year dataset (Coppedge et al., 2018).[1] From left to right, the corruption measures are the Transparency International's Corruption Perception Index (Transparency International, 2017), the ICRG Quality of Governance Index (PRS Group and others, 2018), and the World Bank Control of Corruption Index (Kaufmann, Kraay, and Mastruzzi, 2009). For electoral manipulation, from left to right are the Perceptions of Electoral Integrity Index (Norris, Wynter, and Cameron, 2018), the Quality of Elections Index (Kelley, 2014), and the Index of Electoral Malpractice (Birch, 2008). All measures are for 2012 (or the most recent year available if before 2012), and have been rescaled from original scales to [0,1], with lower values indicating less corruption or electoral manipulation. The patterns confirm that the relative prevalence of corruption and electoral manipulation by regime type in Figure 1 in the paper is not driven by our choice of data source.

Figure A1: Corruption and electoral manipulation by regime type—additional measures



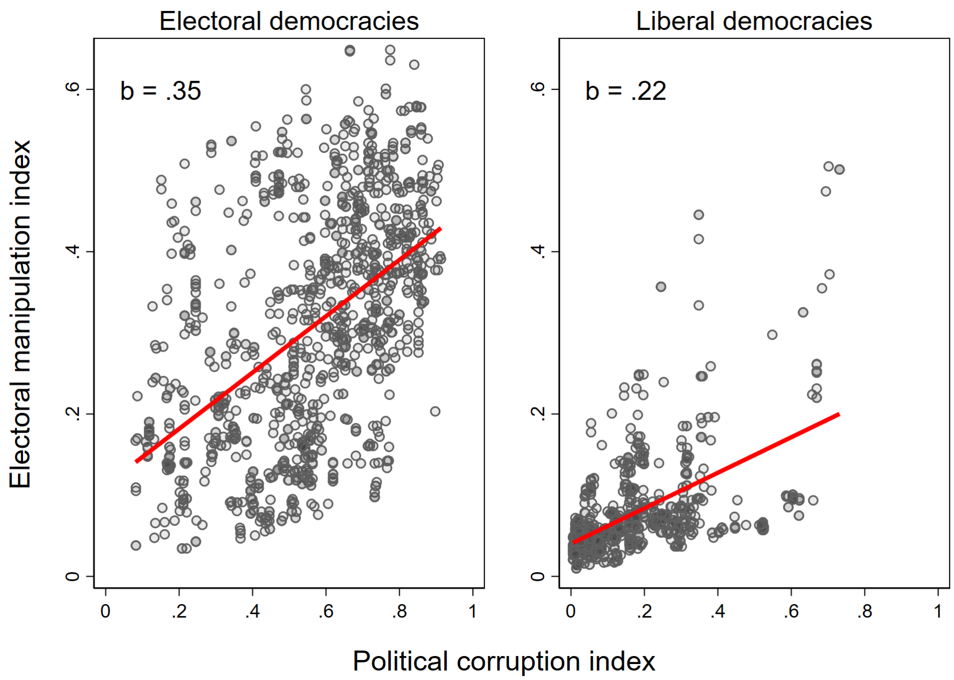
Note: Upper and lower lines on boxes are the 75th and 25th percentiles, respectively; the middle line is the median. Lines on whiskers are "adjacent" values (the 75th (25th) percentile plus 1.5 × interquartile range). Regime types are defined by variable `v2x_regime` from the V-Dem country-year dataset (Coppedge et al., 2018). Measures of corruption and electoral manipulation are described in the text. All measures have been rescaled from original scales to [0,1]. Higher values indicate more corruption or electoral manipulation.

Using the same V-Dem measures of political corruption and electoral manipulation as in Figure 1 in the paper, and the same classification of regime types, Figure A2 shows the association between

[1] The patterns are qualitatively unchanged if we use a regime classification based on Polity scores or Freedom House scores.

corruption and electoral manipulation in the post-Cold War period (1992-2017). The correlation for electoral democracies is shown in the left panel, and for liberal democracies in the right panel. While generally positive, the association is stronger for the former than for the latter countries.

Figure A2: Association between corruption and electoral manipulation by regime type



Note: Regime types are defined by variable `v2x_regime` from the V-Dem country-year dataset (Coppedge et al., 2018). From the same dataset, Political Corruption Index is the variable `v2x_corr`; the electoral manipulation index is based on the variable `v2xel_frefair`. Data are for the period 1992-2017. All measures have been rescaled from original scales to [0,1]. Higher values indicate more corruption or electoral manipulation.

A2 Background for Measures of Anti-Corruption Action and Local Corruption

A2.1 Characteristics of Anti-Corruption Agency's Caseload

Figure A3 shows a sample press release by the Romanian Anti-Corruption Directorate (DNA). We combed through more than 2,000 such press releases to create the dataset of geo-coded anti-corruption actions we use in the analysis.

Figure A3: Sample press release by the Romanian Anti-Corruption Direcotrate

Direcția Națională Anticorupție

acasă
sesizați-ne
despre noi
rezultatele noastre
mass media
informare publică
cooperare internațională

MINISTERUL PUBLIC

PARCHETUL
DE PE LÂNGĂ
ÎNALTA CURTE DE CASAȚIE ȘI JUSTIȚIE

IMPARȚIALITATE·INTEGRITATE·EFICIENȚĂ

COMUNICAT

Prin decizia penală nr. 586 din 28 mai 2012, Curtea de Apel Bacău a hotărât următoarele:

Condamnarea inculpatului **MANEA CIPRIAN - IONUȚ**, asociat și administrator la SC Rul Com SRL, la pedeapsa de **2 ani închisoare cu suspendare condiționată, pe un termen de încercare de 4 ani**, pentru săvârșirea următoarelor infracțiuni: **evaziune fiscală, în legătură directă cu infracțiunile de corupție** și **instigare la abuz în serviciu contra intereselor publice, dacă funcționarul a obținut pentru altul un avantaj patrimonial**.

Condamnarea inculpatului **GHERGHEL SORIN**, șeful Reprezentanței Registrului Auto Român (R.A.R.) Neamț, la pedeapsa de **2 ani închisoare cu suspendare condiționată, pe un termen de încercare de 4 ani**, pentru săvârșirea infracțiunii de **instigare la abuz în serviciu contra intereselor publice, dacă funcționarul a obținut pentru altul un avantaj patrimonial**.

Condamnarea inculpatului **GHILEA CONSTANTIN - VALENTIN**, director la Direcția Județeană de Accize și Operațiuni Vamale Neamț (D.J.A.O.V.), la pedeapsa de **2 ani închisoare cu suspendare condiționată, pe un termen de încercare de 4 ani**, pentru săvârșirea infracțiunii de **abuz în serviciu contra intereselor publice, dacă funcționarul a obținut pentru altul un avantaj patrimonial**.

Condamnarea inculpaților **SURDU GHEORGHE**, șef compartiment echipe mobile, **PREUTU MITICĂ**, controlor vamal și **CĂSĂMÂNDRĂ VASILE**, inspector vamal, la pedepse de câte **un an închisoare cu suspendare condiționată, pe un termen de încercare de 3 ani**, pentru săvârșirea infracțiunii de **abuz în serviciu contra intereselor publice, dacă funcționarul a obținut pentru altul un avantaj patrimonial**.

Ia act că partea vătămată Ministerul Finanțelor Publice, prin D.G.F.P. Neamț, nu s-a constituit parte civilă în cauză.
Obligă pe fiecare dintre inculpați să plătească statului câte 5500 lei cu titlu de cheltuieli judiciare.

DEFINITIVĂ

Inculpații au fost trimiși în judecată de procurorii Direcției Naționale Anticorupție la data de la data de 16 octombrie 2009 (comunicatul nr. 289/VIII/3 din data de 12 noiembrie 2009).

BIROUL DE INFORMARE ȘI RELAȚII PUBLICE

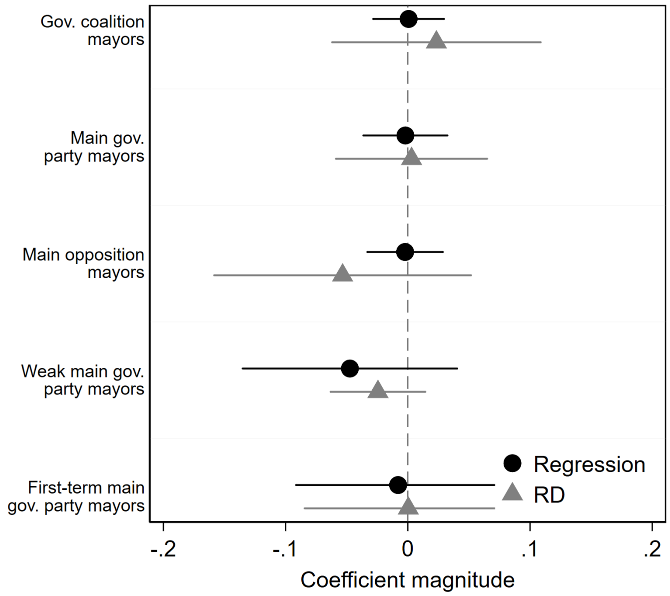
Figure A4 suggests no evidence that the DNA exhibited political bias in its caseload during the period we study (2010-2012). The figure shows two types of estimates: (a) from an OLS regression model (indicated with black circles in the graph), with control variables for administrative locality type,[2] population size, and the share of women, Hungarians, Roma, and university-educated inhabitants in each locality; (b) from a regression discontinuity model (indicated with gray triangles), with the running variable defined based on the criterion indicated on the y-axis.[3]

Going from top to bottom, there is no evidence that in the period we study the anti-corruption agency was more or less likely to press charges against public officials in localities under the control of: (a) the governing coalition parties; (b) the main governing coalition party (PSD); (c) the main opposition party (PDL); (d) vulnerable mayors from the main government party (in localities with the PSD's seat share in the local council of less than 20%); or (e) the first-time mayors from the main governing coalition party.

[2]Comunes, towns, and municipalities. Among other things, these administrative designations determine local tax rates, transfers from the central government, and the size of the local council.

[3]For example, for the top-most estimate, the running variable is based on the winning margin for the governing coalition in the 2008 local election (i.e. the election preceding the anti-corruption actions).

Figure A4: Patterns in anti-corruption agency's caseload, 2008-2012



Note: Figure shows two types of estimates: (a) from a regression model (black circles), and (b) a regression discontinuity model (gray triangles). The samples, indicated on the y-axis, and the model details are described in the text.

We further find no evidence of an uneven *geographical* distribution of anti-corruption cases, whether across Romania's regions or counties.[4]

A2.2 Measures of Local Corruption

Our measure of local corruption combines two indirect corruption risk indicators. The first is based on red flags in local public procurement, the second is based on a mismatch between infrastructure spending and infrastructure outcomes. We describe each measure in turn. These measures have been used in previous research, which contains more details and validity checks (Klašnja, 2015).

The procurement-based measure is an average of three corruption risk indicators: (1) frequency of use of a *less-transparent procurement procedure* (restricted auction, accelerated restricted auction, negotiation, accelerated negotiation, and negotiation without a participation notice) in place of the default, highest-transparency procedure (open auction); (2) *price per quantity* of regularized purchases procured in most localities, such as office or medical supplies;[5] (3) frequency of *single bidder contracts*, whereby a procurement tender is fulfilled with only one submitted bid.

Each indicator is standardized with respect to the relevant product market to mean zero and standard deviation one across all contracts for the period 2008-2012, and then averaged across all product markets.[6] These standardized values are then averaged by locality. While none of the three

[4]In both cases, we ran a regression model of the binary DNA charge indicator on dummies for geographic areas (regions or counties). The regions are: South, Transylvania/Banat, and Moldova. There are forty one counties.

[5]Unlike in some other countries, such as Italy, in the period we study, Romania did not have a centralized and standardized framework agreement that would encompass these purchases for all procurement contracts.

[6]The relevant market is determined by the first two digits of the Common Procurement Vocabulary (CPV) codes.

indicators alone represent proof of corruption in procurement, the expectation is that suspicious patterns on multiple measures are more indicative of corrupt practices.

The data on procurement contracts come from the Agency of Digital Agenda of Romania, which maintains a portal where contract-level data are available.[7] We focus on the two most common types of contracts: direct acquisitions (applied for small and regularized purchases of standard products), and public works/service contracts (used for the majority of other, more complex and expensive works or services).

The measure of "missing infrastructure" compares the change in the actual stock of infrastructure with the change in spending on the same infrastructure in the period 2008-2012, controlling for other factors. We focus on sewer and water pipes, because it includes key local infrastructure (beside roads and electricity provision), and because spending on this type of infrastructure is most clearly under the purview of local government. The annual data on the stock of sewer and water pipes (in km of length) come from the Romanian Statistical Office.[8] The data on sewer and water spending come from the Ministry of Finance and the Ministry of Regional Development and Public Administration.[9]

We define the missing infrastructure indicator as the difference between the predicted and the observed change (inverse residuals) in the physical stock of sewer and water pipes, based on a multi-level regression model of the change in the physical stock in sewer and water pipes on the change in spending on the same infrastructure, controlling for a variety of other factors at the local and county level.[10] Higher values indicate a greater mismatch between changes in the physical stock and changes in spending. Like the procurement risk measure, this indicator is standardized to mean zero and standard deviation one.

To simplify the analysis and reduce measurement error, we use a binary 'local corruption' measure that equals one if either of the two indicators is above the sample median, and zero otherwise.

A2.3 Relationship Between Measures of Anti-Corruption Action and Local Corruption

Table A1 shows the cross-tabulation between our binary measure of local corruption and the anti-corruption charges. Our local corruption measure is correlated with the presence of anti-corruption actions in the same locality; however, the correlation coefficient is relatively low, at .12 (significant at $p < .001$).

[7] `www.e-licitatie.ro`.

[8] Available for a fee at `www.insse.ro`

[9] `www.dpfbl.mdrap.ro` and `www.mfinante.ro`, respectively.

[10] The controls at the local level are: central government transfers to the local council for water, sanitation, and road maintenance; amount of repatriated income tax revenue; change in total locality expenditures and revenues; change in capital expenditures; tax collection effectiveness as calculated by the Ministry of Finance; exposure to floods during the period 1999-2007; mayoral co-partisanship with the central government; and the mayor's margin of victory in 2008. At the county level, the controls are: central government transfers to the county council for water, sanitation, and road maintenance; change in total county expenditures and revenues; change in capital expenditures; county GDP in constant 2008 lei; average road utilization in 2008 (vehicles/km); county president's co-partisanship with the central government; and the county president's margin of victory in 2008.

Table A1: Local corruption and anti-corruption actions

Local corruption	Anti-corruption action No	Anti-corruption action Yes	Total
No			
Frequency	1,061	25	1,086
Row %	97.70	2.30	100.00
Column %	44.15	18.66	42.81
Yes			
Frequency	1,342	109	1,451
Row %	92.49	7.51	100.00
Column %	55.85	81.34	57.19
Total			
Frequency	2,403	134	2,537
Row %	94.72	5.28	100.00
Column %	100.00	100.00	100.00

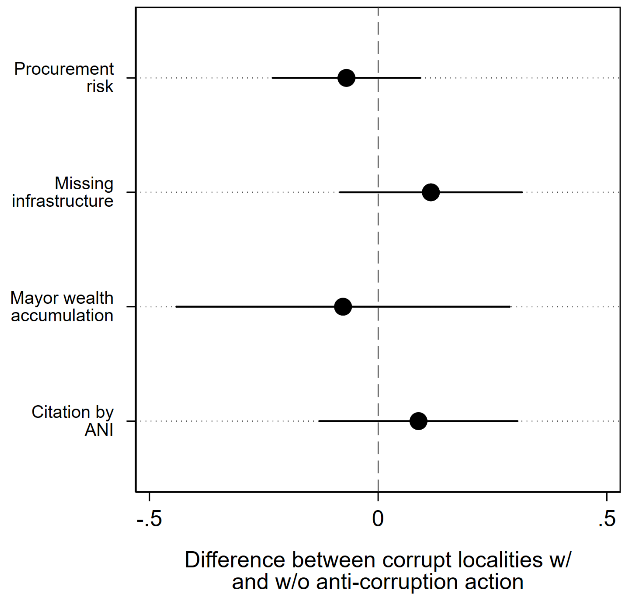
Note: The local corruption measure is described in Section A2.2. The anti-corruption action measure is described in Section in the paper.

As discussed in the paper, it is possible that despite the low correlation, the two measures are both noisy, related indicators of an unobserved true corruption level. In that case, the presence of both indicators in a locality may suggest a greater level of corruption than in localities indicated by only one of the measures.

One way to evaluate this possibility is to compare the extent of corruption, as measured by the continuous procurement and missing infrastructure indicators, in localities with the above-median corruption level—with and without the presence of DNA's anti-corruption actions. If localities with both DNA actions and above-median local corruption are indeed more corrupt than localities with just above-median corruption but no DNA actions, the average values on the procurement and infrastructure measures should be higher. If not, it is more plausible that the DNA measure and the local corruption measure are reasonably separate.

The first two estimates in Figure A5 show that we do *not* observe systematically greater procurement corruption risk or infrastructure misappropriation in corrupt localities with DNA anti-corruption actions than in corrupt localities without such actions. The figure shows the standardized coefficients from a regression of each variable on the presence of DNA actions, controlling for administrative locality type, population size, and county.

Figure A5: Difference in corruption in above-median corruption localities with and without anti-corruption actions



Note: Estimates are standardized coefficients from a regression of each variable (indicated on the y-axis) on the presence of anti-corruption actions, controlling for administrative locality type, population size, and county. Lines are the 95% confidence intervals based on robust standard errors. The first two estimates are for the continuous local corruption risk indicators described in Section A2.2. 'Mayor wealth accumulation' is the difference in the average wealth accumulation (between 2008 and 2012) of re-running mayors and their closest re-running challengers, calculated based on the information from wealth disclosures. 'Citation by ANI' is the presence of a citation of any local politician by the National Integrity Agency (ANI) for false information in wealth disclosures, or potential conflict of interest or incompatibility. See the text for more details.

The bottom two estimates examine differences in two other, more tentative proxies for corruption. The first is the difference in the average wealth accumulation between 2008 and 2012 of re-running mayors relative to the closest re-running challengers.[11] (Self-citation omitted) shows that a larger differential in wealth accumulation is correlated with corruption. The second proxy is the presence of a citation of any local politician by the National Integrity Agency (ANI) in the period 2008-2012. This agency is in charge of processing wealth and interest disclosures, and tracking any conflicts of interest and incompatibilities between officials' public and private roles.[12] As in the case of the procurement/infrastructure measures, we do not observe measurable differences in these proxies between the higher-corruption localities with and without anti-corruption actions. Overall, these results make it less plausible that localities with both higher local corruption risks and past DNA actions exhibit measurably greater corruption than corrupt localities without prior DNA actions.

[11] Mayors and many other elected officials are required to submit income and asset disclosures each year while in office, as well as when they run in elections. Based on hand-coded data from these disclosures for both 2008 and 2012, we calculate the rate of wealth accumulation, and standardize it to mean zero and standard deviation one.

[12] The agency issues citations whenever these issues apply, and can issue fines and refer individuals to local prosecutors if they are not remedied.

A3 Survey Questions on Perceptions of and Experiences with Manipulation

To create the individual-level measures of perceptions of and experience with electoral manipulation, we utilize a nationally representative face-to-face survey of 1,204 adult Romanian citizens, fielded between October 30 and November 10, 2012 by the University Babeş-Bolyai in Cluj-Napoca and TNS-CSOP, an independent survey company in Romania.

To measure perceptions of manipulation, we make use of the following survey question (percent of responses in each category are shown in parentheses):

> *In recent months, much has been discussed about possible electoral fraud in the July 29, 2012 referendum. Which of the following statements is closest to your opinion on the referendum:*
>
> 1. *The referendum proceeded correctly, without fraud* (27%).
> 2. *There was some local fraud, but it did not affect the general vote* (27%).
> 3. *There was an organized referendum fraud campaign, and the real turnout was considerably lower than the official result* (13%).
> 4. *Don't know* (32%).

In the analysis, we use a binary variable that treats respondents who chose the third response option as perceiving electoral manipulation.[13]

To measure respondents' experiences with manipulation, we included a list experiment in the survey that aimed to capture any experience with intimidation or attempts to induce an individual to turn out or not. The control group received the following question:

> *People decided whether or not to go to the referendum based on different reasons. On this list there are a few of the reasons that people have told us. Could you please tell me how many of these reasons influenced whether or not you went to the referendum this year. I am not interested in which these reasons are or whether or not you went to the referendum, but only how many of the three reasons influenced your decision to go or not to go:*
>
> 1. *What I saw on TV during the referendum campaign.*
> 2. *My personal opinion on President Băsescu.*
> 3. *Discussions with other people about the referendum.*

The treatment group saw the same question, but with a fourth, potentially sensitive, item added to the list: *4. Someone threatened you or gave you something.*

As is standard in list experiments, the dependent variable is the average difference in the number of items chosen between the treatment and the control group (Blair and Imai, 2012).[14] In

[13] We treat the "don't know" responses as missing. Results are qualitatively similar when these responses are multiply imputed.

[14] The randomization was successful, as the respondent background characteristics (gender, age, education, Hungarian, Roma, employment status, public sector employment, and vote for USL, PDL, or others), are all balanced. We also find no evidence for design or ceiling/floor effects (Blair and Imai, 2012).

the analysis, we examine this outcome in sub-samples defined by the anti-corruption actions and local corruption.[15]

A4 Model Specifications

Here, we provide more details on the model specifications for the results presented in the paper.
1. Opposition conversion rate. The OLS model specification for testing hypotheses H1 and H2 is:

$$\begin{aligned}\text{Turnout}_{i,j} &= \beta_0 + \beta_1\text{Vote}_{i,j} + \beta_2\text{USL}_j + \beta_3\text{DNA}_j + \beta_4\text{Corruption}_j \\ &+ \sum_k \gamma_k \mathbb{Int}\{\text{Vote}_{i,j}, \text{USL}_j, \text{DNA}_j, \text{Corruption}_j\} \\ &+ \sum_m \theta_m \textbf{Controls}_j + \delta_c + \epsilon_{i,j},\end{aligned}$$

for polling station i in locality j and county c; Turnout is the referendum turnout, Vote is the main opposition party's local election vote share,[16] USL is the indicator of whether the locality has a mayor from the governing coalition, DNA is the prior anti-corruption actions variable, Corruption is the local corruption variable, $\mathbb{Int}\{\text{Vote}_{i,j}, \text{USL}_j, \text{DNA}_j, \text{Corruption}_j\}$ is the full factorial set of k interaction terms,[17] **Controls** is the set of m locality-level controls (educational breakdowns, ethnic composition, administrative locality types, and tourist visits), δ_c are the county dummies. Standard errors are clustered by locality. The key independent variables are described in the article. Figure 2 plots the estimates for $\text{Vote}_{i,j}$ for governing coalition localities ($\text{USL}_j = 1$) for different combinations of variables $\{\text{DNA}_j, \text{Corruption}_j\}$.

The OLS model specification testing hypothesis H3 (results of which are shown in the left panel of Figure 4) is:

$$\begin{aligned}\text{Turnout}_{i,j} &= \beta_0 + \beta_1\text{Vote}_{i,j} + \beta_2\text{USL}_j + \beta_3\text{DNA-Corruption}_j + \beta_4\text{Opposition}_j \\ &+ \sum_k \gamma_k \mathbb{Int}\{\text{Vote}_{i,j}, \text{USL}_j, \text{DNA-Corruption}_j, \text{Opposition}_j\} \\ &+ \sum_m \theta_m \textbf{Controls}_j + \delta_c + \epsilon_{i,j},\end{aligned}$$

where:

$$\text{DNA-Corruption}_j = \begin{cases} 1 & \text{if} \quad \text{DNA}_j = 1 \ \& \ \text{Corruption}_j = 1 \\ 0 & \text{if} \quad \text{otherwise} \end{cases}$$

and Opposition is one of the opposition strength variable (as described in the article).

[15] The survey sampling frame targeted a nationally representative sample of individuals, not a representative sample in each locality included in the survey. However, we have no a priori reason to believe that the potential unrepresentativeness of within-locality samples should systematically bias our results in any specific direction.

[16] We utilize the main opposition party's vote share for the county council instead of the local council or mayor because the latter are more vulnerable to local idiosyncracies. These different vote shares are highly correlated, however.

[17] This set includes all the pairwise double and triple interaction terms, and the full quadruple interaction term between the four variables (i.e. $k = 11$).

The RD model specification testing hypothesis H3 (shown in the right panel of Figure 4) is:

$$
\begin{aligned}
\text{Turnout}_{i,j} = \beta_0 &+ \beta_1 \text{Vote}_{i,j} + \beta_2 \text{USL2008}_j + \beta_3 \text{USL2008 Margin}_j \\
&+ \beta_4 \text{USL2008}_j \times \text{Vote}_{i,j} + \beta_5 \text{USL2008}_j \times \text{USL2008 Margin}_j \\
&+ \sum_m \theta_m \textbf{Controls}_j + \delta_c + \epsilon_{i,j},
\end{aligned}
$$

where USL2008 Margin_j is the vote margin of the 2012 governing coalition's mayoral candidate in the 2008 local election in locality j, and $\text{USL2008}_j = 1(\text{USL2008 Margin}_j > 0)$. This specification is run separately in localities for which DNA-Corruption $= \{0, 1\}$ (i.e. in the two groups defined by the binary DNA-Corruption). The model is estimated with a weighted local linear regression using a triangular kernel, within the optimal bandwidth using the covariate-adjusted optimal bandwidth selector outlined in Calonico et al. (2019), and applying the matching weights from a propensity score model discussed in Section A5 below. Standard errors are clustered by locality. The figure plots the estimates for $\text{Vote}_{i,j}$ for the 2012 governing coalition localities when $\text{USL2009}_j = \{0, 1\}$.

2. Survey-based measures of electoral manipulation. The linear probability OLS model specification for the referendum fraud perception dependent variable is:

$$
\begin{aligned}
\text{Perception}_{i,j} = \beta_0 &+ \beta_1 \text{USL}_j + \beta_2 \text{DNA}_j + \beta_3 \text{Corruption}_j \\
&+ \sum_k \gamma_k \mathbb{Int}\{\text{USL}_j, \text{DNA}_j, \text{Corruption}_j\} \\
&+ \sum_m \theta_m \textbf{Controls}_i + \sum_n \theta_n \textbf{Controls}_j + \delta_r + \epsilon_{i,j},
\end{aligned}
$$

for individual i in locality j and region r; the perception variable is described in Section A3 above; $\textbf{Controls}_i$ is the set of m individual control variables (age, gender, education, ethnicity, employment status, and party vote in the local election), $\textbf{Controls}_j$ is the set of n locality control variables (size and administrative type of locality), δ_r are the region dummies. Standard errors are clustered by locality, and observations are weighted by the survey-provided population weights. The left panel of Figure 3 plots the average predicted probability of Perception $= 1$ for governing coalition localities ($\text{USL}_j = 1$) for different combinations of variables $\{\text{DNA}_j, \text{Corruption}_j\}$.

The OLS model specification for the list experiment-based measure of experience with manipulation is:

$$
\begin{aligned}
\text{ItemCount}_{i,j} = \beta_0 &+ \beta_1 \text{Treatment}_i + \beta_2 \text{USL}_j + \beta_2 \text{DNA}_j + \beta_3 \text{Corruption}_j \\
&+ \sum_k \gamma_k \mathbb{Int}\{\text{Treatment}_i, \text{USL}_j, \text{DNA}_j, \text{Corruption}_j\} + \epsilon_{i,j},
\end{aligned}
$$

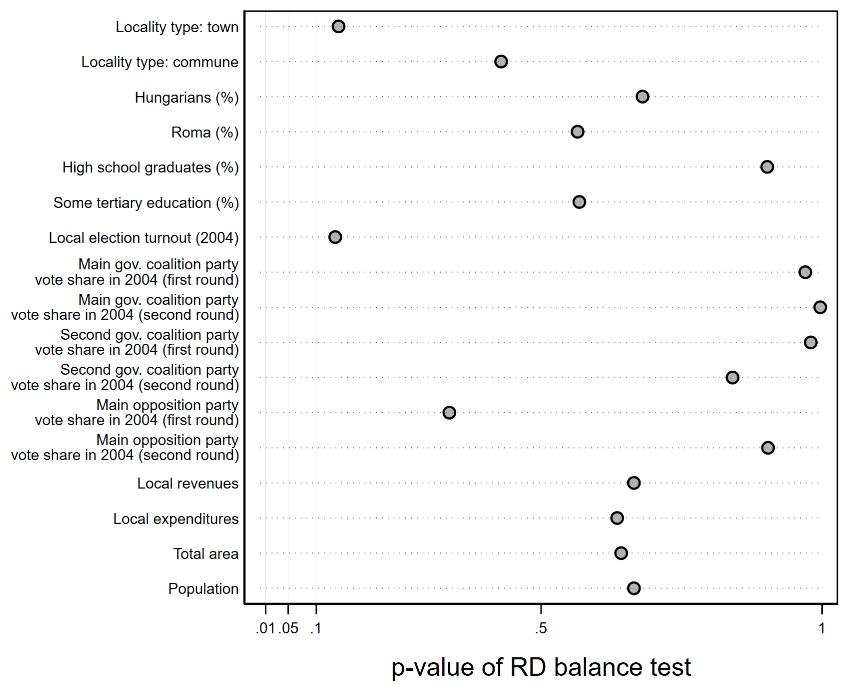
for individual i in locality j; ItemCount is the count of items in the list experiment a respondent chose, described in Section A3 above; Treatment is a binary indicator of whether a respondent was shown the sensitive item. The estimate for Treatment gives the estimated proportion of individuals choosing the sensitive item (i.e. those who have experienced referendum manipulation). Standard errors are clustered by locality, and observations are weighted by the survey-provided population weights. The right panel of Figure 3 plots the estimate for Treatment for governing coalition localities ($\text{USL}_j = 1$) for different combinations of variables $\{\text{DNA}_j, \text{Corruption}_j\}$.

A5 Validity of the Opposition Checks Regression Discontinuity Design

The key assumption in the electoral RDD we use to test hypothesis H3 (in the right panel of Figure 4) is that parties cannot precisely control their vote share in close elections. Two types of tests are usually undertaken to examine the validity of this assumption: to ascertain no RD effects on relevant predetermined variables (Caughey and Sekhon, 2011), and no disproportionately many close wins just above the winning threshold (McCrary, 2008).

Figures A6 and A7 show the results for the two validity tests, respectively. The running variable is the 2008 local election vote margin for the governing coalition (the governing coalition being that in 2012 rather than in 2008).

Figure A6: Balance in background characteristics of localities around the winning-margin cutoff, 2008

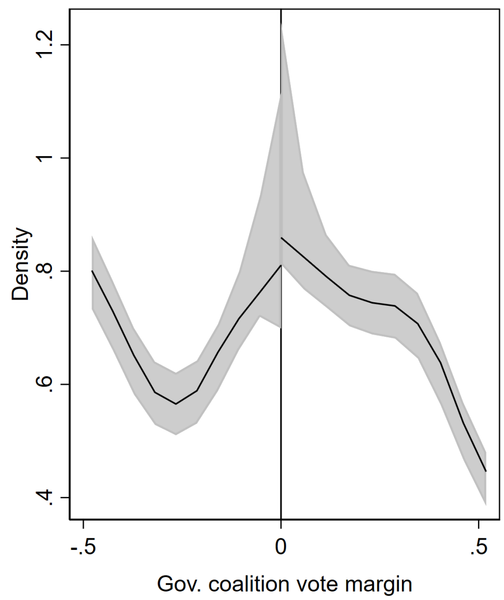


Note: Circles represent the p-values from RD models identical to those employed in the main analysis, but with background characteristics (listed on the y-axis) as outcome variables. The running variable is the vote margin for the (2012) governing coalition in the 2008 mayoral elections.

Figure A6 shows balance in a number of background characteristics of localities with close local elections involving the governing coalition in 2008. The figure shows the p-values from RD models with background characteristics as outcome variables (listed on the y-axis). Given the inclusion of covariates in our RD specification (administrative locality type, the share of Hungarians, Roma, and inhabitants with secondary or some tertiary education; see the paper and Section A4 above), another important assumption is that the RD treatment has no effect on these covariates as well (Calonico et al., 2019). Figure A6 verifies that is indeed the case.

Figure A7, further, suggests no evidence of sorting by the governing coalition mayors in 2008 around the winning cutoff. That is, mayoral candidates from parties comprising the governing coalition were on average no more likely to win than lose a closely-contested election in 2008.

Figure A7: Sorting around the winning-margin cutoff, 2008



Note: Figure shows the estimated density of the vote margin for the governing coalition in the 2008 mayoral elections (the running variable in the RD analysis) within small bins on the x-axis, and the associated point-wise 95% confidence intervals (in gray). The estimates are based on the procedure outlined in Cattaneo, Jansson, and Ma (2018).

We now further discuss the details of our RD approach, briefly mentioned in the paper. Rather than examining the RD effect of prior opposition control on electoral manipulation in the referendum (as measured through the opposition conversion rate), we are interested in the *heterogeneity* of this effect in two subsamples: localities with prior anti-corruption actions and above-median corruption risk vs. localities with no anti-corruption actions and/or low corruption risk. As discussed in Carril et al. (2018) (also Hsu and Shen 2019), the identification assumptions in the standard RD approach do not automatically apply when studying RD effect heterogeneity. In particular, the relationship between the running variable and the outcome may be different in the two subsamples; related, localities in the two subsamples may differ on other characteristics, even if they are all close to the cutoff. These issues may induce different RD effects in the two subsamples even when the true heterogeneity is null.

The results shown in Figure 4 in the paper follow Carril et al. (2018) in addressing these potential issues. First, we minimize any specification bias by running the RD analysis separately in the two subsamples. This amounts to augmenting our RD specification shown in Section A4 with interactions of all our variables (including our running variable, the winning cutoff dummy variable, and their interaction) with the dummy variable indicating our two samples (the DNA-Corruption variable). Second, to minimize differences in covariates in the two samples, we match localities on the covariates used throughout the analysis in the paper (administrative locality type, the share of

Hungarians, Roma, and inhabitants with secondary or some tertiary education).[18] Table A2 shows that the localities in the two subsamples are indeed imbalanced on a number of covariates (columns 1-4); however, matching successfully decreases these differences (column 5-8).

Table A2: Subsample balance in covariates in the regression discontinuity design

	Before matching				After matching			
	DNA/ high corr.	No DNA/ low corr.	Diff.	p-value	DNA/ high corr.	No DNA/ low corr.	Diff.	p-value
Hungarians (% of total pop.)	0.04	0.02	0.02	0.61	0.04	0.01	0.03	0.13
Roma (% of total pop.)	0.01	0.03	-0.02	0.00	0.02	0.02	-0.01	0.18
Locality type: Town	0.11	0.15	-0.05	0.41	0.07	0.05	0.02	0.63
Locality type: Commune	0.02	0.66	-0.64	0.00	0.03	0.03	-0.00	0.98
Some tertiary educ. (% of total pop.)	0.22	0.07	0.14	0.00	0.21	0.20	0.01	0.84
High school (% of total pop.)	0.29	0.18	0.11	0.00	0.28	0.27	0.01	0.14

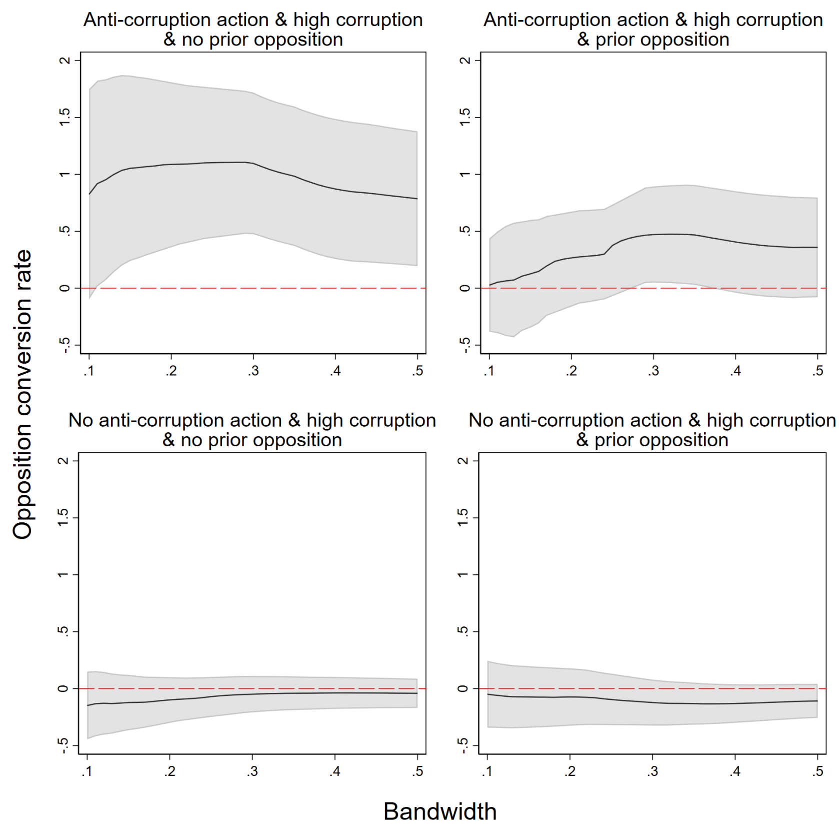
Note: Columns 1-4 show the balance tests before matching in localities close to the RD cutoff, in subsamples defined by anti-corruption actions and local corruption. Columns 5-8 show the same balance tests after propensity score matching on covariates included in the RD analysis (administrative locality type, the share of Hungarians, Roma, and inhabitants with secondary or some tertiary education). Matching is used based on the approach to heterogeneous RD effects outlined in Carril et al. (2018).

It is important to note that these results come from analyses utilizing the optimal bandwidth procedure designed for standard RD applications—without regard for the interaction between the running variable and the DNA-Corruption subsample indicator. We could not find a treatment of this issue in the literature, but it raises the possibility that the chosen bandwidth is not 'optimal' (in the sense of minimizing the properly chosen variance-bias trade-off). To guard against this concern, Figure A8 shows the same four estimates as in the right panel of Figure 4 in the paper, but across a number of bandwidths—for each percentage point from .1 to .5 (the bandwidth used in the analysis in the paper is .28).[19] While the estimates vary somewhat, the substantive takeaways are not affected by bandwidth choice.

[18]Carril et al. (2018) propose using inverse probability weighting (IPW) to make the two subsamples similar on covariates; however, IPW did not sufficiently decrease imbalances in our case. Instead, we employ propensity score matching.

[19]We do not go below .1, because that leaves less than 10 localities with both prior anti-corruption action and high corruption.

Figure A8: Prior opposition control, electoral manipulation, anti-corruption actions, and local corruption—multiple bandwidths



Note: Figure reproduces the four estimates shown in the right panel of Figure 4 in the paper, but across multiple bandwidths around the 2008 local election winning cutoff. Shaded areas are the 95% confidence intervals based on standard errors clustered by locality. The estimates are based on the RD model specifications shown in Section A4.

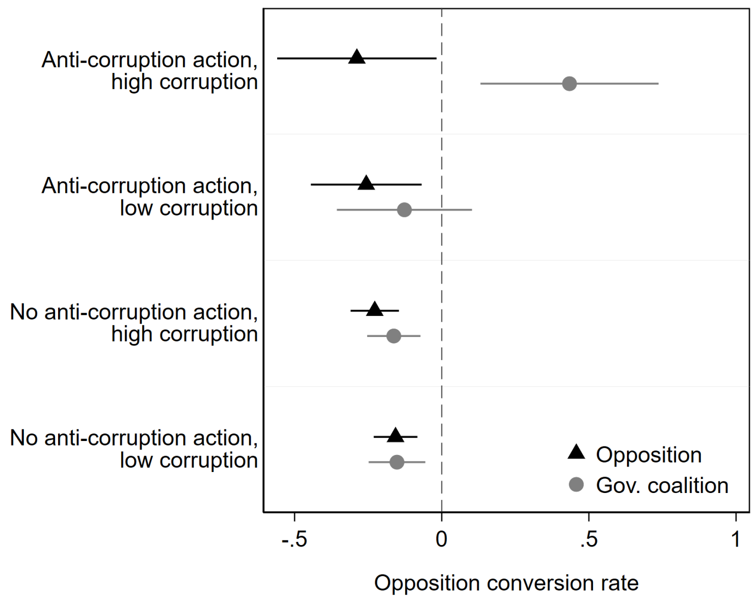
A6 Additional Results

A6.1 Referendum Outcomes in Opposition Localities

In the paper, the analyses focus on localities under the control of the governing coalition. We have argued that local officials from these parties had the clearest incentives and opportunities for electoral manipulation in the 2012 referendum. Here, we demonstrate that this focus is empirically warranted, as we see no evidence of turnout manipulation in localities under the control of the opposition parties.

Figure A9 shows the opposition conversion rates (the correlation between polling-station referendum turnout and the vote share for the main opposition party in the local election seven weeks prior) in localities under the opposition's control. To mirror the main analysis, we once again break estimates down across two dimensions: local corruption, and prior anti-corruption actions. The estimates are shown in black triangles (with the associated confidence intervals). For reference, the main opposition conversion rates in governing coalition-run localities are shown in gray circles (the same ones shown in Figure 2 in the paper).

Figure A9: Opposition conversion rates in governing coalition and opposition localities



Note: Figure shows the estimated opposition conversion rate, for the opposition-run localities (black triangles) and the governing coalition-run localities (gray circles). The conversion rate measure is described in the article. Lines are the 95% confidence intervals, based on standard errors clustered by locality. Estimates are derived from an OLS regression model that includes locality controls described in the article, as well as county fixed effects.

Figure A9 shows that the conversion rates in the opposition-run localities do not vary much by local corruption and prior anti-corruption action. Importantly, if there was manipulation by the opposition, and if it followed the same logic as for the governing coalition politicians, we would expect to see greater referendum turnout *suppression* in the opposition-run localities with higher corruption and prior anti-corruption cases.[20] However, no such evidence is present, as the conversion rate in these localities is not significantly different from the conversion rates in other locality types.

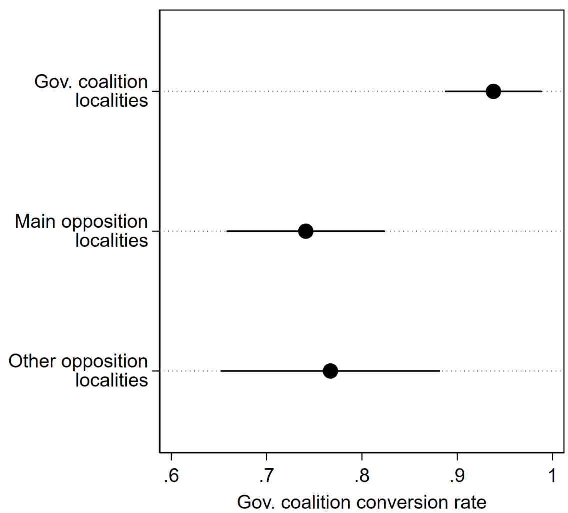
While implausible, our results could still be interpreted mainly as evidence of referendum turnout suppression by the *main* opposition party rather than turnout inflation by the governing coalition (between them, the mayors from the governing coalition and the main opposition parties won 89% of localities in the 2012 local election). One way to evaluate this is to compare the conversion rates in localities run by the main opposition party to those run by the other opposition parties, whose positions on the referendum were more ambiguous.[21] If manipulation is largely

[20]Alternatively, even the opposition politicians in localities with higher corruption and/or prior anti-corruption charges may have had an incentive to support impeachment if it meant weakening the anti-corruption reforms. In that case, we would see greater suppression in localities with lower corruption and/or no prior anti-corruption actions. We do not see evidence of such a pattern in Figure A9 either.

[21]The Hungarian minority party (UDMR), which had previously been part of the governing coalition with the main opposition party (in power prior to 2011) had initially opposed the legislative proposal for impeachment, but then switched sides and voted in its favor. The new populist People's Party (PP-DD), which had come in third place in the June 2012 local elections, did not take a clear position on the referendum, as its leader, TV-host Dan Diaconescu, condemned both the governing coalition and the main opposition party. The remaining localities mostly had mayors

driven by turnout suppression by the main opposition party, conversion rates in localities run by the other opposition parties should be systematically different. Figure A10 shows no such evidence. The figure compares the *governing coalition* conversion rate across three types of localities: those run by the governing coalition, main opposition, and other opposition. The fact that conversion rates were very similar in localities with mayors from the main opposition party (which backed the President) and localities with mayors from other opposition parties (which had neutral positions on the referendum) suggests that there is no evidence for turnout suppression by pro-Băsescu local politicians.

Figure A10: Governing coalition conversion rates in main and other opposition localities



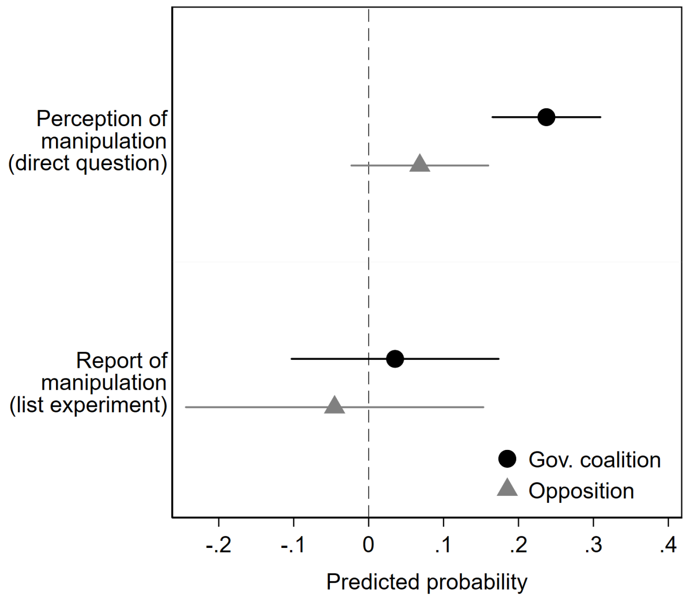
Note: Figure shows the estimated governing coalition conversion rate, for the localities run by the governing coalition (top estimate), main opposition (middle), and other opposition parties (bottom). The conversion rate measure is described in the article. Lines are the 95% confidence intervals, based on standard errors clustered by locality. Estimates are derived from an OLS regression model that includes locality controls described in the article, as well as county fixed effects.

Finally, we can use our survey data to compare respondents' perceptions of and experiences with referendum manipulation in localities run by the governing coalition and by the opposition. Our interpretations would be undermined if the patterns of perceptions and experiences with manipulation in opposition localities equalled or outstripped those in localities run by the governing coalition.[22] Figure A11, however, shows that the perceptions of referendum manipulation are more pronounced in the governing coalition-run localities than in the opposition-run localities (upper panel). The patterns for experiences with manipulation (based on the list experiment) are noisy but point in the right direction.

unaffiliated with a political party (i.e. independents).

[22]Because of sample size constraints, we cannot perform analyses in subsamples defined by anti-corruption actions and local corruption in opposition-run localities (as we do for the governing coalition localities in Figure 3 in the paper), as there are considerably fewer opposition-run localities (only 36%).

Figure A11: Perceptions of and experiences with manipulation in governing coalition and opposition localities



Note: Figure shows the predicted probabilities of perceiving "organized" referendum manipulation (top) and the reported experience with manipulation (bottom), for localities run by the governing coalition (black circles) and opposition (gray triangles). Dependent variables are based on the perception and list experiment survey questions reproduced in Section A3. Lines are the 95% confidence intervals based on standard errors clustered by locality. Estimates for perceptions are derived from an OLS regression model that includes individual socio-demographic and locality controls described in the article, as well as region fixed effects.

On balance, the various results in this section suggest little evidence that the (main) opposition engaged in systematic manipulation of referendum turnout.

A6.2 Results from a Difference-in-Difference Design

Our results show that electoral manipulation is concentrated among corrupt politicians, but only in localities with prior DNA actions. Our favored interpretation is in line with hypothesis H2: electoral manipulation is a reaction of corrupt elites to anti-corruption actions. However, another interpretation is that localities with both DNA investigations and above-median corruption are simply more corrupt than other localities (or otherwise different for unobserved reasons), and that it is for this reason (rather than DNA actions) that electoral manipulation is higher. It is difficult to rule out this alternative interpretation with a cross-sectional design we used in the main text.

We therefore present here the results from a difference-in-difference design that examines the *within-locality* changes in potential electoral manipulation, as a function of the onset of DNA charges. In particular, we focus on the change in another potential proxy for electoral manipulation from the 2008 to the 2012 parliamentary election—the share of null votes (in total votes cast). Null votes are ballots rejected by the local electoral commissions, usually because they are incorrectly marked. However, there have been frequent allegations of null votes being used to fraudulently manipulate vote shares.[23,24]

We compare the within-locality change in the share of null votes from the 2008 to the 2012 election in localities that experience anti-corruption actions between 2008 and 2012 and in those localities that do not.[25] In particular, we run the following model:

$$\text{Null votes}_{i,t} = \beta_0 + \alpha_i + \lambda_t + \beta_1 \text{DNA}_{i,t} + \sum_k \theta_k \mathbf{Controls}_{i,t} + \epsilon_{i,t},$$

in locality i and election $t \in \{2008, 2012\}$; α_i are the locality fixed effects, λ_t are the election fixed effects; $\text{DNA}_{i,t}$ is an indicator for whether a locality had anti-corruption actions between 2008 and 2012, and $\mathbf{Controls}_{i,t}$ are the time-varying locality-level controls. Standard errors are clustered by locality. If there is electoral manipulation in response to DNA actions, we should see a larger increase (or smaller decline) in null votes in localities with DNA actions (i.e. β_1 should be positive and statistically significant).

Results in column 1 of Table A3 are consistent with that expectation: the share of null votes in the 2012 parliamentary election (relative to the share in the 2008 election in the same locality) is higher in localities that experienced anti-corruption actions than in localities that did not (by about .4 percentage points, significant at $p < .01$). This basic difference-in-difference approach helps us eliminate any unobserved time-invariant features of localities. The second column shows that the results continue to hold even if we control for several time-variant characteristics: population, education, share of welfare participants, and minorities (Hungarians and Roma). Column 3 shows that our results hold even when we treat the local corruption binary indicator used in the main analysis as another time-varying 'treatment' (i.e. in the same way as the DNA anti-corruption actions).[26]

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[23]For example, after the 2009 presidential election, the Romanian Constitutional Court ordered a recount of null votes following the accusations that they were fraudulently used. See: `https://lege5.ro/Gratuit/gi3dsnrygazq/iregularitati-si-deficiente-in-procesul-electoral1-raport?dp=gi2teobwhayteni`.

[24]We do not use the null votes as a proxy for manipulation in the referendum because their share is very low—the median share across polling stations is less than half a percent. This is hardly a surprise: with little uncertainty about the share of votes supporting impeachment (given Băsescu's low approval and incentives for the opposition to abstain), there was no incentive for the governing coalition to misuse the null votes.

[25]As discussed in the article, local actors—mayors and local council members—are instrumental in conducting any electoral manipulation, which justifies our focus on locality-level returns for national-level contests, such as the 2008 and 2012 parliamentary elections.

[26]In these analyses, we use all the localities, rather than focusing just on localities under the control of the 2012 governing coalition (the USL coalition). This is because the parties comprising that coalition ran as separate parties in 2008, competing against each other. The results are similar if we focus only on localities locally held in 2008 by the largest USL coalition member—the PSD.

Table A3: Difference-in-difference results

	Basic DiD	+ time-varying controls	+ local corruption	2000-2004 placebo test
Anti-corruption action	0.40***	0.25**	0.23**	-0.24
	(0.09)	(0.11)	(0.12)	(0.20)
Population (log)		-0.01	-0.04	
		(0.81)	(0.80)	
Hungarian (%)		-0.34	-0.37	
		(2.13)	(2.14)	
Roma (%)		0.62	0.63	
		(1.09)	(1.09)	
Education (secondary or higher, %)		2.01***	2.03***	
		(0.62)	(0.62)	
PSD council seat share (%)		0.48***	0.47***	
		(0.15)	(0.15)	
PDL council seat share (%)		0.08	0.08	
		(0.23)	(0.23)	
On welfare (%)		-3.81**	-3.74**	
		(1.55)	(1.55)	
Local corruption			0.07	
			(0.07)	
Constant	3.61***	3.33	3.61	5.15***
	(0.02)	(6.59)	(6.57)	(0.03)
Observations	6364	5079	5079	5659

Estimates are the coefficients from a difference-in-difference model shown in the text. All columns include locality and election fixed effects. Standard errors are clustered by locality. Variables are described in the text. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, two-tailed.

The key identifying assumption is that of parallel trends, that in the absence of DNA actions, the null vote shares between 2012 and 2008 would have changes at a similar rate in both the treated and the control localities. To evaluate the plausibility of this assumption, column 4 compares the trends in these two sets of localities between the parliamentary elections in 2000 and 2004.[27] This is akin to a placebo test: the changes in null vote shares in elections before the DNA actions should be similar. This is indeed what we find.

A6.3 Manipulation and Anti-Corruption Signal Strength

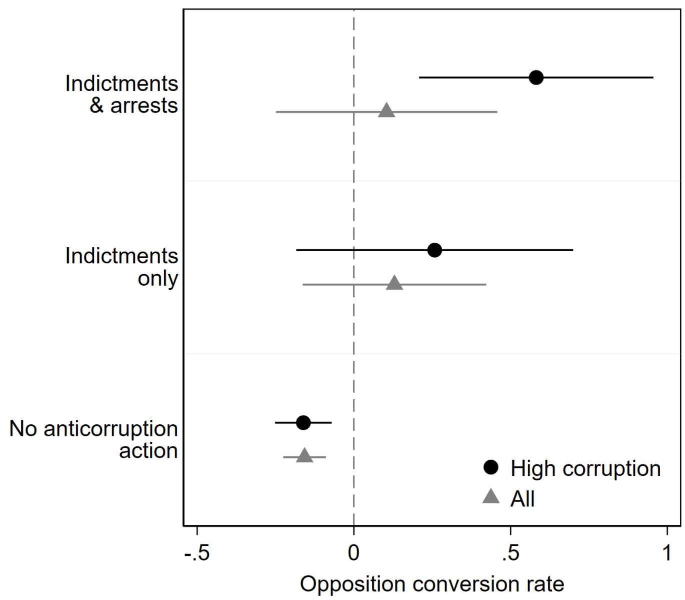
As discussed in the paper, if our tests indeed serve as evidence that corrupt elites from the governing coalition resorted to referendum manipulation as backlash against the anti-corruption agency's campaign, we may expect that clearer threats would spur greater elite reaction. Our dataset on anti-corruption actions contains such information. We coded the strength of the local anti-corruption

[27]We focus on the 2000-2004 period rather than the 2004-2008 period because the DNA was already quite active during the latter period.

'signal' by distinguishing cases that involved at least one arrest in addition to indictments; 61 of the 152 localities (1.91% of all localities) fit this criterion. The logic here is straightforward: arrests in one's locality are likely to be a more forceful and immediate demonstration of threat to the status quo than indictments alone.

Figure A12 examines whether more intense anti-corruption actions trigger a stronger backlash.[28] We focus on the opposition conversion rate diagnostic, because it provides the largest sample size. We compare the conversion rates across localities experiencing signals of different strength. The top estimates show the results for the strongest signal; the middle estimates for the weaker signal, and the bottom estimates for localities not facing any anti-corruption actions. The estimates in gray (triangles) are for all localities, irrespective of the local corruption level; the black (circle) estimates are specifically for higher-corruption localities.[29]

Figure A12: Anti-corruption signal strength and electoral manipulation



Note: Figure shows the estimated opposition conversion rate, described in Section in the paper. Lines are the 95% confidence intervals based on standard errors clustered by locality. Estimates are derived from an OLS regression model that includes locality controls described in the article, as well as county fixed effects.

The patterns are largely consistent with the notion that stronger signals induce greater backlash. As in the main analysis, the opposition conversion rates are negative in localities with no prior DNA actions, with or without higher corruption (around $-.14$, with the standard error of .04). They

[28] It may also be the case that the relationship between signal strength and elite response is non-monotonic. For example, a moderate signal could trigger backlash as corrupt politicians try to defend a still-dominant status quo, while a strong signal could result in deterrence, as the fear of being punished by the new rule-of-law regime intensifies. Conversely, moderate anti-corruption measures could trigger deterrence, for example, because they are seen as appropriate punishments for egregious corruption, while harsh anti-corruption measures could trigger backlash, if enough politicians perceive the new penalties as excessive/unfair. In sum, non-monotonic effects would suggest switching between deterrence and backlash as the signal intensifies. We do not find evidence for such non-monotonicities.

[29] There are few localities with DNA arrests and lower corruption, making it difficult to estimate results separately for the lower-corruption localities.

are less negative in localities with prior DNA actions (.13 in all localities and .25 in the higher-corruption localities), but primarily in localities experiencing at least one arrest—and only in the higher-corruption localities (.58, sig. at $p < .01$).[30] In other words, the backlash effect is most evident where the local corrupt elites likely felt the most threatened.

A6.4 Referendum Turnout Patterns from a 2012 Local Election RDD

We have argued in the paper that in the context of the referendum the deterrent or backlash incentives should mainly apply to the (corrupt) elites from the governing coalition. We therefore assume that local partisan control matters. Such partisan control effects may be correlated with other observable and unobservable locality-level characteristics, which in turn may be correlated with referendum turnout. To minimize any potential confounding—and further check the validity of our assumption to focus mainly on governing coalition-run localities—we utilize here another regression discontinuity design, which compares referendum turnout in localities where the governing coalition narrowly won a mayoral election in 2012 (seven weeks before the referendum) to turnout in localities where it narrowly lost. While this RDD captures the effect of the local governing coalition control on referendum outcomes, we once again examine the variation in the RDD effect in relation to prior anti-corruption efforts and local corruption. Because we are interested in the heterogeneity in the RD effect, we apply the same approach as with the opposition checks RDD discussed in the paper and in Section A5 above. That is, we match on the same covariates across the subsamples defined by anti-corruption actions and local corruption, and run the RD separately in the two subsamples.[31]

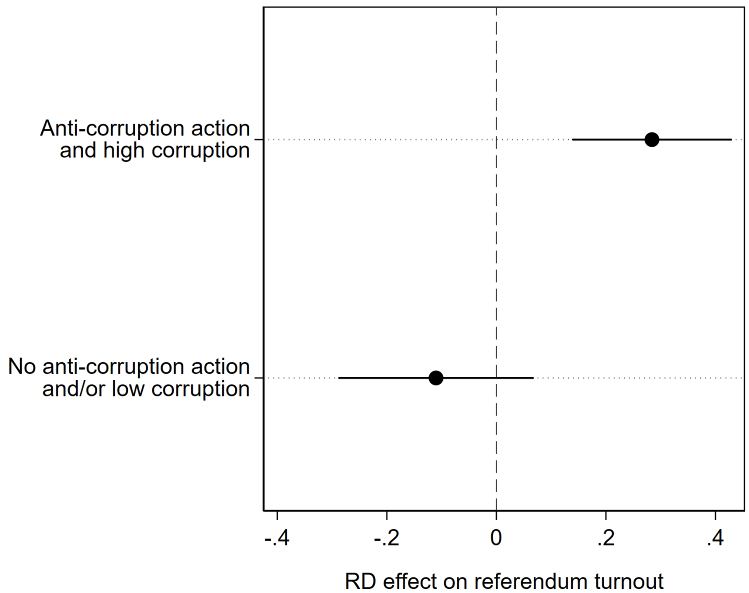
Figure A13 shows the results.[32] Referendum turnout is considerably higher (.26 percentage points, significant at $p < .01$) in localities with both anti-corruption actions and high corruption that were narrowly won by a governing coalition mayor than a non-USL mayor. In localities without either an anti-corruption action or with low corruption, we do not observe a similar turnout boost. These patterns are consistent with the findings based on the other fraud diagnostics in the paper.

[30] The difference between the "indictments only" conversion rate and the rate in localities without prior DNA actions in the higher-corruption localities is significant at $p < .07$. The difference between the localities with some arrests and those with indictments only is significant at $p = .26$.

[31] The specification is: $\text{Turnout}_{i,j} = \beta_0 + \beta_1 \text{Win}_j + \beta_2 \text{Margin}_j + \beta 3 \text{Win}_j \times \text{Margin}_j + \epsilon_{i,j}$, for polling station i in locality j; Margin_j is the governing coalition's vote margin, $\text{Win}_j = \mathbb{1}(\text{Margin}_j \geq x_0)$, with x_0 being the winning cutoff. The model is run separately for sub-samples defined by anti-corruption actions and local corruption. It is estimated with a non-parametric local-linear estimator using a triangular kernel and within the optimal bandwidth (estimated at 11.4 percentage points) around the winning cutoff based on the procedure outlined in Calonico et al. (2017). Standard errors are clustered by locality.

[32] We find mixed evidence for the validity of the no-sorting assumption for this RDD, in contrast to the opposition checks RDD discussed above. While we find no placebo RD effects on relevant predetermined variables, there is some evidence of strategic sorting of governing coalition mayors. However, we think this evidence of sorting makes it *more* difficult to use the RDD approach to uncover referendum fraud, because mayors who narrowly won—but should have lost—would presumably find it harder to commit election fraud than if they had legitimately won the local election.

Figure A13: RD effects of governing coalition local control on turnout, prior anti-corruption actions, and local corruption



Note: Figure shows the regression discontinuity effect of the governing coalition mayors' local control on referendum turnout. The running variable is the vote margin of the governing coalition in the 2012 mayoral elections. Lines are the 95% confidence intervals based on standard errors clustered by locality. The RD models are based on approach to estimating heterogeneous RD effects outlined in Carril et al. (2018), applied in the same way as in Section A5 above.

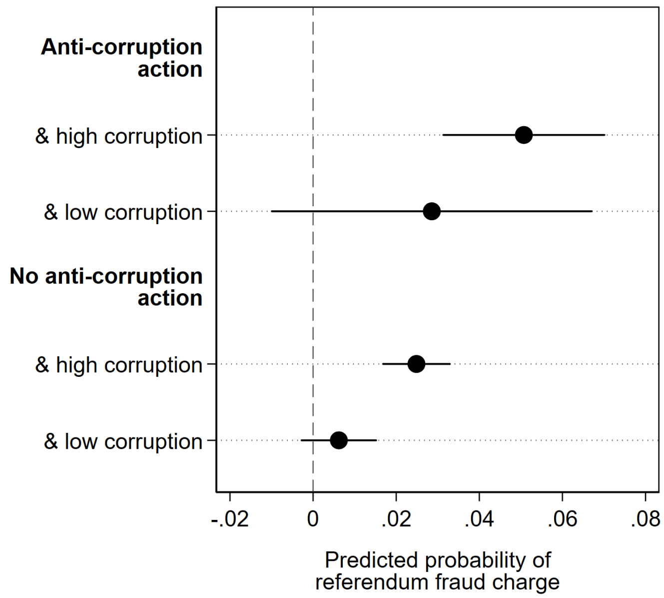
A6.5 Patterns in DNA's Post-Referendum Fraud Cases

Throughout the analysis, our focus was on the indirect diagnostic measures of electoral manipulation, because of the unavailability of direct evidence of fraud. Following the referendum, however, the DNA was given a mandate to pursue election fraud cases related to the referendum. These cases provide an opportunity to examine the validity of our measures and the logic of our argument.

That said, the DNA caseload pertaining to referendum fraud is limited: it brought charges against officials operating in 128 polling stations, belonging to 29 distinct localities. This limited scope constrains our ability to conduct full-scale analyses, and we only examine the basic correlations (without controlling for other covariates used in other analyses). Figure A14 examines the distribution of fraud cases in localities run by the governing coalition, across our two relevant dimensions: DNA's pre-referendum actions and local corruption. Despite the obvious noise due to limited sample size, the patterns of DNA fraud cases generally mirror the patterns we find in the main analysis with indirect measures of fraud.[33]

[33]The analysis is at the locality level. Observations are weighted by the number of polling stations charged with fraud.

Figure A14: Post-referendum fraud charges by the DNA, pre-referendum anti-corruption cases, and local corruption



Note: Figure shows the predicted probability of a charge by the anti-corruption agency (DNA) for referendum fraud in localities run by the governing coalition, across two relevant dimensions: DNA's pre-referendum actions and local corruption. Estimates are based on an OLS regression model, with observations weighted by the number of polling stations charged with fraud.

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