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Exposition to Corruption and Political Participation: Evidence from Italian Municipalities

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**Exposition to Corruption and
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Exposition to Corruption and Political Participation: Evidence from Italian Municipalities

Abstract

The aim of this paper is to study the effect of local corruption on political participation which is mediated by the press. Focusing on Italy, we generate a daily measure of exposition to local corruption screening articles of main Italian press agency. Applying an event-study methodology on local elections, two results emerge. First, corruption exposition reduces citizens participation: voter turnout decreases but characteristics of elected politicians are not affected; second, politicians participation modifies: number of candidates lowers along with proportion of running freshmen. These results suggest that corruption exposition produces resignation rather than retaliation in terms of political participation.

JEL-Codes: D720, D730, H700, K420.

Keywords: corruption, media, turnout, political selection, electoral competition.

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1 Introduction

Corruption is a broad phenomenon with negative and complex effects in economic, social and institutional terms; it has been found to be a real obstacle to economic growth (Mauro 1995) and to generate additional, and non negligible, costs for societies¹. Political Economy as well as Political Science deeply studied this phenomenon and three main categories of results are now established. First, corruption affects voters' confidence and trust in public institutions (Clausen, Kraay and Nyiri 2011), in governments capacity (Caillier 2010) and in political system and politicians (Morris, Klesner 2010); second, malfeasance decreases electoral consensus of incumbents (Chang, Golden and Hill 2011); finally, media coverage of political corruption is marked by consistent media biases (Puglisi, Snyder 2010, Di Tella, Franceschelli 2009) and the press is a vital mediator for its effects (Fiorino, Galli and Petrarca 2012)².

In this contribution, we want to focus on political participation. First, we consider the effect of corruption on participation of citizens: on the one hand, on *whether* people vote, voter turnout. On the other hand, on *how* people vote, selection of elected politicians (characteristics of mayors and councillors) as well as political background of elected mayors. Second, we look at the impact on participation of local politicians (*who* runs), focusing on the endogenous political supply: number of candidates running for mayor, number of presented lists and political background of candidates (whether they belongs to the civic society, not being supported by any party, or not). The focus of the study is local: notably, we take into account political scandals involving politicians at the sub-national level (*i.e* regions, provinces and municipalities) and we study the effect on municipal elections. Moreover, we want to capture the effect of corruption which is mediated by the press, studying the effect of mediatic exposition to scandals. Generally speaking, we can expect two possible outcomes. On the one hand, people can react with *retaliation*: voters may be motivated by the desire to improve political environment and could increase political participation as well as civil engagement; also, from the politicians side, more efforts could be observed to counterbalance negative image associated to politics because of corruption. On the other hand, *resignation* could prevail: voters may be discouraged, lower political participation, and reduce effort exerted in civic and political commitments; further, local political supply could shrink, with static debates and lower competition.

The paper focuses on Italy: this country represents an important outlier in the international panorama in terms of public and political corruption as it is considered one of the most corrupted nation in Europe. On the one hand, there is international evidence confirming this: Transparency International ranks Italy in position 61 out of 168 in 2015 ranking of corruption perception³. On the other hand, there are several domestic pieces of evidence pointing this out; in this regards, Italian accounting court, *Corte dei Conti*, classifies corruption as a widespread phenomenon with deleterious and durable effects on economic growth and, in an estimate of 2012, estimated Italian *cost of corruption* in €60 billion.

To capture exposition to local political corruption we generate an index by looking at newspaper articles of the main Italian press agency, ANSA, involving local politicians

¹World Bank identifies corruption as one of the main obstacle for economic growth and includes its reduction as one of the Sustainable Development Goals. In a recent estimate, World Bank estimated the amount paid in bribes each year around the word in \$1 trillion (<http://www.worldbank.org/en/topic/governance/brief/anti-corruption>).

²Another related result is that exposition to scandals through media influences perception of corruption as shown in Rizzica, Tonello 2015.

³The index is a self-reported measure of perception of public corruption in a certain country (<http://www.transparency.org>).

in corruption cases. We identify an article as dealing with local scandals if a) it talks about corruption (we make use of keywords related to corruption, *e.g.* "bribe", to screen articles in an automatic web search) and b) it mentions the surname of a local politician in charge in the period the article was issued, in the place the article was geo-localized. We obtain a local and high-frequency (daily) measure of corruption where most scandals emerge at intermediate administrative levels (*i.e.* provinces or regions). Moreover, we generate other versions of the measure increasing the *corruption content* requirement and selecting smaller sub-samples of articles characterized by higher frequency of keywords related to corruption, to see whether the effect varies as content becomes more *corruption related*.

Then, in order to capture the causal effect of media exposition to local corruption on political participation we rely on an event-study design: we take advantage of the large geographical and temporal variation of local political scandals to see how the treatment affects participation over time. The key idea of our identification is the comparison of the impact of scandals taking place before elections with those occurring after. We deal with main assumption that timing of scandals is plausibly exogenous respect to election dates in three ways. First, we make use of a national source, ANSA, so that it is unlikely that articles timing is driven by local confounding factors such as local negative sentiment toward politics. Second, the treatment is composed by corruption news pertaining all administrative levels (*i.e.* regions, provinces and municipalities); and this fairly limits the concern of endogeneity of scandals' timing as it is implausible that municipal factors affect the propensity to publish news pertaining higher administrative levels. Finally, we show that there is no discontinuity in scandals density around the cutoff represented by the election day (running a McCrary test): this suggests that articles timing is not manipulated in proximity of electoral dates and further supports the key identifying assumption. Moreover, we account for endogenous distribution of scandals across entities exploring the effect of non-local scandals alone (regional corruption) on local political outcomes.

Three main results emerge from the analysis. First, local corruption exposition significantly reduces voter turnout in municipal elections: one standard deviation increase in the treatment lowers, on average, voter turnout by 0.29% after one year and by 0.41% after two years, while the effect vanishes for older scandals. Further, the effect turns out to be stronger for municipalities that are large, located in the centre-north of Italy, marked by high past turnout and electoral competition and with high levels of newspaper circulation. Finally, as corruption content of articles raises, the effect strengthens. The direction and magnitude of this effect are coherent with related contributions in economic literature. Moreover, since ANSA is not a direct source of information, but works as a sources of news for the press, it is reasonable to think that newspapers devote, on average, more space to corruption stories than ANSA (because of comments, editorials, . . .); and so what we are capturing is the tip of the iceberg, and the real effect is likely to be wider than ours. As a second result we find that there is no evidence of politicians selection: characteristics of elected mayors and councillors do not improve, while there are clues that elected politicians are more likely to belong to the "old political class". Thirdly, in terms of political supply, exposition to corruption narrows political offer of candidates: number of candidates running for mayor decreases, as one standard deviation increase in corruption index lowers number of candidates by 0.05 units (all municipalities) and 0.23 units (large municipalities); the effect is significant only two years after the scandal and this is coherent with the fact that, differently from voter turnout, the decision to run as a candidate is made several months before elections and cannot react to shocks close to the electoral date; moreover, this is associated with a reinforcement of the "old political class" as proportion of old runners and the proba-

bility the incumbent reruns raise. These results suggest that exposition to corruption does not generate positive reactions as few people vote, quality of elected politicians does not raise and political supply shrinks without any renewal of new administrators. Resignation hypothesis seems to be validated by these pieces of evidence.

This contribution is related to several papers in Political Economy: first, it is linked with the literature studying effects of corruption on voter turnout. On the one hand, there is a large set of papers in Political Science on this issue that do not agree on sign of the effect: most of these papers find that corruption negatively affects turnout (Anderson, Tverdova 2003; Kostandinova 2003, Bauhr, Grimes 2013), then there is a small group of papers finding a positive relation (Stockemer, Calca 2013) and, finally, there is another set of studies that do not find any relation (Rothstein, Solevid 2013, Stockemer 2013). These papers consider a very general definition of corruption (from political to public), they mostly focus on cross-country comparison and they generally lack of an identified effect. On the other hand, there is a small group of papers in Economics exploring this relation; Chong et al. 2015 and Costas-Perez 2013 are papers most closely related to ours. The first one focuses on local incumbents in Mexico and finds that random dissemination of information on politicians' malfeasance affects electoral support as well as voter turnout (negatively). While the second paper concentrates on corruption in Spain and relies on a dataset of documented cases of corruption to measure perception of corruption and political participation. Our contribution differentiate from these papers in three aspects: 1) it focuses on a causal effect of local corruption mediated by the press, 2) it identifies short and long run effect of exposition to corruption and 3) it inspects all aspects of political participation, from citizens to politicians participation. Second, the paper is related with the large literature exploring determinants of politicians selection; several factors has been inspected: remuneration/compensation (Gagliarducci, Nannicini 2013), electoral competition (Galasso, Nannicini 2011), the presence of gender quotas (De Paola et al. 2010, Baltrunaite et al. 2014) and the activity of organized crime (Daniele 2015, Daniele, Geys 2015). In these regards, there are no papers, to the best of our knowledge, dealing with the impact of (exposition to) corruption on selection of politicians. Third, this paper deals with a small literature studying entry of politicians according to individual levels of honesty and corruption degree of the society (Bernheim et al. 2014). Finally, our contribution is related with the large Media Economics literature: in particular studies on the effect of media on political participation (Snyder, Stromberg 2010, Drago, Nannicini and Sobbrío 2014) and the ones inspecting media bias regarding news on corruption (Di Tella, Franceschelli 2009, Puglisi, Snyder 2010, Fiorino, Galli, Petrarca 2012).

The paper is organized as follows. Section 2 describes data on corruption and on political participation. Section 3 presents identification strategy and validity check. Section 4 shows main results on political turnout, selection of politicians and political supply. Section 5 discusses several robustness checks and Section 6 concludes.

2 Data

2.1 Political Corruption

2.1.1 *Main index*

The first contribution of this paper is the construction of the measure of exposition to local corruption on Italian media. We rely on main Italian press agency ANSA⁴; this

⁴ANSA, *Agenzia Nazionale Stampa Associata*, is the first press agency in Italy and the fifth in the world, it has been established in 1945 and it has 22 offices in Italy and 81 in other countries

is an important and highly prestigious institution in the Italian press market and it is reasonable to assume that it is not affected by ideological and geographical bias (as it would be probably the case for local/national newspapers). Local corruption in Italy can take place in different levels of local administration: municipalities, provinces and regions⁵. Local administrations are in charge of a large set of services and, especially from last constitutional reform in 2001, they face a certain degree of revenues/expenditures autonomy. We collect scandals for these three administrative levels and we aggregate the variable at the province level for the time span 1997-2014 (1997 is the first year data are available on Factiva). We construct the index following an automatic keyword-based three steps procedure:

- First, we screen first paragraphs of ANSA articles (using the web portal Factiva) in order to identify articles dealing with political corruption; notably, we collect articles based on two criteria: 1) the presence of at least one keyword related to political corruption, *e.g.* *abuso d'ufficio* (malfeasance in office), 2) the presence of the surname of a local politician in charge in the place where the article was geo-localized, in the period when the article was written⁶. This procedure guarantees us that selected articles deal with corruption involving local politicians in charge in place/time the article refers to⁷. Figure 1 shows one instance: the article contains one corruption term, *indagato* (under investigation), as well as the surname of a local politician, *Boni (Davide)*, that was in charge in the place (Milan) the article refers to, in the period (march 2012) when the article was written. This procedure allows to identify and geo-localize 16,478 articles about political corruption.
- We perform a random check to be sure the procedure works correctly and we generate monthly articles counts by province⁸.
- Finally, we generate an index of relative frequency, *i.e.* we express the number of article in ‰ terms with respect to the total number of articles generated by

(<https://it.wikipedia.org/wiki/ANSA>).

⁵Italy is divided into 8092 municipalities, 110 provinces and 20 regions (in 2011).

⁶To be more precise on this first step:

- First, we screen first paragraphs of ANSA articles, making use of keywords related to corruption.
- Second, we geo-localize selected articles based on places mentioned in the article (we exploit the fact that in the standard structure of articles the first word is the name of the place where piece of news comes from); we use the province as unit of our analysis.
- Third, we further screen selected articles identifying names of local politician. In particular, we search within article text for names of local politicians (*i.e.* all politicians in charge in regions, provinces and municipalities between 1997-2014; this information comes from *Anagrafe degli Amministratori Locali e Regionali*-Italian Ministry of interior affairs) and we identify an article whether it mentions one local politician, in charge in the place where the article was geo-localized, in the period when the article was written.

⁷Most of selected articles deal with corruption or malfeasance in office involving regional/province councilors and presidents; sometimes, but less frequently, involved politicians are mayors or city councilors. News about corruption cover all phases of judiciary procedure: from beginning of investigation to the sentence; furthermore, more common misconducts include corruption, bribery, misuse of power, collusive tendering, and infractions in refunds usage.

⁸As a technical remark, we need to add that we imputed corruption score of region capitals, where regional governments take place, to all other provinces within that region. We proceeded like this since regional scandals are recorded in the regional capital while they represent a common treatment for all provinces in the region. This assumption can lead to measurement error due to the fact that we could not disentangle between regional and provincial scandals of the regional capital and while the former are common treatment, the latter only involve regional capitals (nevertheless, we can think of provincial scandals of regional capital as a common treatment in the region as well).

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HD TANGENTI: NOTIZIA DI **BONI INDAGATO** ARRIVA DURANTE CONSIGLIO.

WC 146 words

PD 6 March 2012

ET 12:52

SN [ANSA - Regional Service](#)

SC ANSARE

LA Italian

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LP (ANSA) - MILANO, 6 MAR - La notizia dell'indagine per corruzione a carico del presidente del Consiglio regionale della Lombardia, Davide Boni (Lega), ha raggiunto il Pirellone durante la seduta dell'Aula, poco prima della pausa pranzo. Il Consiglio sta discutendo della nuova legge sull'edilizia, ma ovviamente in pochi minuti le accuse a Boni sono diventate l'argomento di conversazione alla buvette.

Figure 1: Example of Factiva extraction-corruption case in regional council of Lombardy

ANSA in that year:

$$corruption_{ity} = \frac{n_{ity}}{N_y} * 1000$$

with $corruption_{ity}$ corruption index of province i in month t in year y , n_{ity} number of articles about political corruption in i at time ty and N_y total number of articles of ANSA in year y ⁹.

Figure 2 shows political corruption index for 4 large Italian cities with monthly aggregation; several remarks can be done: first, average variation of the index increases after 2003, this can be due to higher attention devoted to political scandals by the press or to better coverage of Factiva; second, there are peaks in correspondence of important corruptive events (*e.g.* we can see a huge spike in January 2004 in Milan, corresponding to the Parmalat crac, the biggest bankruptcy scandal of a private company in Europe; the scandal involved national and local politicians and was massively reported in the press. Milan court was in charge of financial crimes).

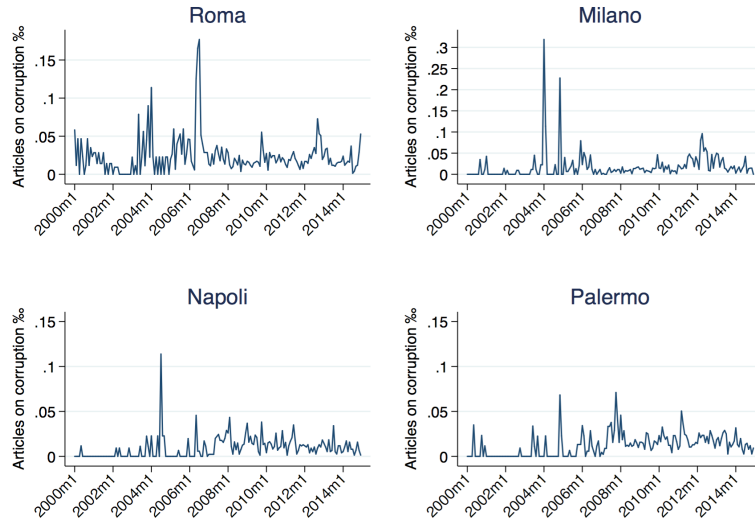


Figure 2: Political corruption exposition in four Italian large cities

In terms of distribution across time, Figure 3 reports the aggregate monthly frequency of the corruption index by year: we can see that there is a first increase in articles frequency from 2003 and a second one from 2007¹⁰. From 2007 on, the index is quite stable and it seems to emerge a weak seasonality. In terms of distribution across

⁹We generate another version of the index where we rescale absolute number of articles with the total number of articles of ANSA in the reference month of the reference year, *i.e.* using N_{my} , with m reference month, as denominator of the index. We include the analysis preformed with this index in the robustness checks section.

¹⁰This weak upward trend in (local) political corruption has been highly discussed in Italian public debate, and some accused constitutional reform of 2001 (*Riforma del Titolo V della Costituzione*) that stated fiscal autonomy (revenues/expenses) of local administration (regions, provinces and municipalities). One advocate of this hypothesis is Raffaele Cantone, president of Italian Anti-Corruption Association (ANAC-*Associazione Nazionale Anti Corruzione*), claiming that the reform "[...] increases units of expenses in a meaningless fashion leading to situations where local administrations, as regions, spend lot of money." (http://www.rainews.it/dl/rainews/articoli/Cantone-Norma-criminogena-la-riforma-del-Titolo-V-Costituzione-91737009-8668-4d93-8139-74b73cc33138.html?refresh_ce).

geography, the region with the highest average is Lazio (where Rome is located, more than four times higher than the national average), followed by Lombardy (region of Milan) and Sicily.

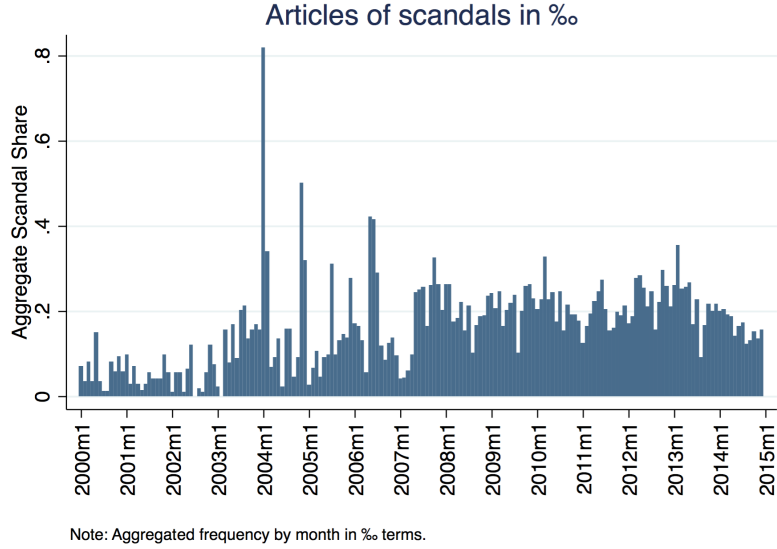


Figure 3: Aggregate corruption exposition

Furthermore, Table 1 shows the descriptive statistics of corruption indices, we focus on three levels of time aggregation month/semester/year: the average mediatic exposition to local political scandals is around 0.007‰ points, with an average variation level of around 0.017‰ points; moreover, as time aggregation raises, mean and standard deviation go up; lastly, Table 1 includes the descriptive statistics of the version of the index with the absolute value of articles, with an average value of 3.3 articles and an average variation around 6.6.

As a second step, we generate another group of indices capturing the exposition of local corruption at the municipality level. On the one hand, these measures let us better characterize the phenomenon of local corruption as they vary at the municipality level; on the other hand, they can be imprecise as newspapers can impute scandals to the city where the court in charge of the case is located; moreover, it is reasonable to think that there is less coverage of small cities scandals and this can select the sample. Table 1 includes the descriptive statistics of these versions of the index; we include three versions: the count at municipality level, the count of scandals of the region capital and an aggregate version with sum of scandals of the municipality, of the province capital and of the region capital. Mean of the municipal count is very low as main variation comes from large cities while this raises if we consider the other two versions.

Finally, to validate our measure we raise the issue whether high levels of corruption exposition are correlated with peaks in attention toward corruption related topics: in other words, whether supply of corruption topics (in the press) leads to demands of corruption issues. We explore this point using Google trend data on keywords related to corruption (*i.e.* *corruzione*-corruption- and *abuso d'ufficio*-malfeasance in office). From this simple analysis it is evident that when articles on corruption come out, there are peaks in attention in these keywords (details on this analysis are contained in the Appendix A1).

Table 1: Descriptive statistics of corruption exposition measures

	Mean	Standard deviation	Minimum	Maximum
Corruption exposition (monthly)	.0073	.0173	0	.3186
Corruption exposition (six-monthly)	.0438	.0694	0	.8844
Corruption exposition (yearly)	.0875	.1238	0	1.183
Corruption exposition 70th (monthly)	.0068	.0167	0	.3186
Corruption exposition 80th (monthly)	.0043	.0117	0	.2503
Corruption exposition 90th (monthly)	.0022	.0069	0	.1820
Corruption exposition Absolute (monthly)	3.3092	6.5895	0	76
Municipality corruption (monthly)	.00002	.0009	0	.3186
Region cap. corruption (monthly)	.0080	.0193	0	.3186
Municipality aggregate corruption (monthly)	.0084	.0195	0	.3186

Corruption exposition Absolute (monthly) is the absolute value version; *Municipality corruption (monthly)* contains municipality-specific scandals; *Region cap. corruption (monthly)* contains scandals of the regional capital; *Municipality aggregate corruption (monthly)* aggregates scandals of municipality, province capital and region capital.

2.1.2 Additional indices: varying corruption content

In order to improve identification of articles about political corruption, we generate an additional set of indices trying to measure the corruption content within articles: in particular, we rely on content analysis techniques to measure how often terms related to corruption are mentioned in the article. First, we compute the *specificity*¹¹ index, using the set of selected keywords on political corruption, for each article in the database; as a second step we generate different variables containing articles with increasing degrees of corruption content (70th percentile, 80th percentile and 90th, representing respectively top 30%, 20% and 10% of the distribution); as corruption contents raises the set of articles shrinks but the "strength" of content increases. Frequency of the index goes down, but all these indices maintain non-negligible levels of variation. We can note this from Table 1 (we only present descriptive statistics for the monthly version); as corruption content raises, variables mean decreases as well as variance.

2.2 Political Participation Measures

In this paper we focus on municipal elections: Italian electoral system for municipalities implies a single ballot for city with less than 15,000 inhabitants and runoff for the others (in the analysis we only focus on first ballot); between 1993 and 2000 legislative term was four years, and after 2000 it has been extended to five years and mayors face a two-term limit. Our sample includes municipal elections for all Italian municipalities in the time span 1997-2014¹².

We want to explore the impact of corruption exposition on all aspects of political participation. First, we study the effect on electoral turnout (ratio between voters and eligible citizens) for municipal polls at the municipal level. Second, we want to shed light on the effects on selection of politicians: background of elected mayors (whether she/he was the incumbent, runs in past elections or belongs to a civic list¹³) and characteristics

¹¹Specificity or keyness is a measure used in content analysis to capture how specific (frequent) is a term in a text relative to the average importance (frequency) of that term in all other texts of the corpus.

¹²There are few missing years for three Italian autonomous regions, in particular Valle d'Aosta (1997), Trentino-Alto Adige (1997-1998) and Sicily (1997-2006).

¹³In Italian local elections a civic list, *lista civica*, is a list of candidates running for mayor or councillor, which is not, officially, affiliated with any national political party. We focus on mayors elected in these lists to account for local power/consensus of national political party. These lists are very important for local politics, they were, for instance, crucial for the early eradication of *Five Star Movements* (born after electoral experiences of lists such as *Amici di Beppe Grillo* and *Liste Civiche a Cinque Stelle* in municipal elections in the early 2000).

Table 2: Descriptive statistics of outcome variables

	Mean	Standard deviation	Minimum	Maximum
Municipal turnout	76.137	9.724	21.568	100
Number of candidates	2.743	1.313	1	19
Number of lists	3.521	3.634	1	46
Civic lists	.712	.356	0	1
Old candidates Last ES	.330	.315	0	1
Old candidates All ES	.402	.332	0	1
Old candidates elected Last ES	.467	.498	0	1
Old candidates elected All ES	.531	.499	0	1
Civic mayor	.679	.466	0	1
Victory margin	.395	.284	0	1
Age candidates (av.)	49.263	7.305	22	85
Gender candidates (female)	.101	.203	0	1
Age mayor	48.957	9.909	19	94
Gender mayor (female)	.102	.302	0	1
Education mayor	4.301	.725	1	6
Age elected (av.)	43.892	4.185	25	77
Gender elected (av. female)	.196	.121	0	1
Education elected (av.)	3.964	.364	2	6
Incumbent reruns Last ES	.492	.499	0	1
Incumbent reruns All ES	.560	.496	0	1
Incumbent elected Last ES	.407	.491	0	1
Incumbent elected All ES	.474	.499	0	1

Municipal turnout is expressed in percentage points; *Civic lists* is measured as a share (over total number of lists); *Old candidates/Old candidates elected* is measured as a share (over total number of candidates) [*Last/All ES* denotes whether they run in last/all past elections]; *Civic mayor* is a dummy denoting if the mayor was supported only by civic lists; *Victory margin* is measured as a share (over the votes of the most voted candidate); *Gender candidates/mayor/elected* denotes proportion of candidates/mayor/elected that are female; *Education mayor/elected* are measured in years (from 1 to 6); *Incumbent reruns/elected* is a dummy denoting if the past incumbent reruns/is re-elected.

of elected politicians (age/gender/education of mayors and councillors). Third, we consider the impact on local political supply, *i.e.* characteristics of the pool of candidates; in particular, we look at number of candidates and their background (*i.e.* whether they run in previous elections, they belongs to civic lists or they include the incumbent) as well as number of presented lists. All these data on municipal election come from the Italian Ministry of Internal Affairs and from the *Anagrafe degli Amministratori Locali e Regionali* (published by the Italian Ministry of Internal Affairs). Table 2 shows descriptive statistic of outcome variables. It is worth mentioning that in following analysis the treatment varies at province level while dependent variables at the municipality level¹⁴.

3 Identification Strategy

3.1 Event Study Analysis

We are interested in the causal effect of corruption exposition on political participation, but this link can be problematic to compute. On the one hand, it could be that places with lower political participation may happen to be those with higher political corruption (and mediatic coverage of corruption); this can be due, for instance, to unobservable variables such as *civic spirit* or *loyalty to institutions*. Moreover, it can be that political participation was already decreasing *before* the scandal takes place. As a result there could be a spurious negative correlation between corruption exposition and political participation.

In order to identify the effect of main explanatory variable on the set of dependent

¹⁴By the way in robustness checks section, we run a specification including the dependent variable aggregated at the province level.

variables, we base our identification strategy on an event-study analysis¹⁵ that takes advantage of the large geographical and temporal variation of political scandals. The key idea of this identification is the comparison of the impact of scandals taking place before elections with those occurring after. The key identifying assumption is that the timing of scandals is plausibly exogenous with respect to elections: *i.e.* it is not correlated with electoral dates. This assumption could be violated if scandals' timing is somehow strategically decided by the local press, for instance by editors and journalists. We may think that negative sentiment toward local politics can affect press behaviour leading local journalists to emphasize political scandals before elections, in order to maximize its impact.

We deal with this empirical challenge in three ways: first, we use ANSA, a national press agency, which can be reasonably considered unbiased in ideological and geographical terms; it is indeed difficult to think that there are local confounding factors affecting report timing of ANSA articles about local corruption; moreover, there are market reasons making convenient to publish a piece of news as soon as possible¹⁶. Second, the treatment is composed by corruption news pertaining all administrative levels (*i.e.* regions, provinces and municipalities), and this fairly limits the concern of endogeneity of scandals' timing as it is implausible that local factors affect the propensity to publish news on regional or provincial scandals (taking place far away from that municipality). We will deal with this issue in the robustness check section where we study the effect of regional scandals alone, excluding regional capitals from the sample¹⁷, showing that the relation is still significant. Thirdly, we formally test whether there is manipulation in the number of scandals released by the press around the cutoff represented by election dates: in particular, we run a McCrary test checking whether the treatment is discontinuous around the threshold (results of this validity check are contained in section 3.2); and we find that density of scandals immediately before the cutoff (election date) is not statistically different from the one immediately after the cutoff.

The event-study gives the possibility to exploit the highly frequency nature of our scandals database and to explore how the treatment affects the set of dependent variables and how the effect evolves over time. We will show two different specifications: a classic event-study analysis (to study the effect in the short-run, *i.e.* in the first two years after the scandal) and a compact version (similar to the one in Bottan, Perez-Truglia 2015) to explore short/long run effects in the same specification.

The first regression specification, the classic event-study one, is as follows:

$$y_{i,t} = \sum_{p=0}^{+P} \alpha_p^{lag} C_{i,(t-p)} + \sum_{p=-1}^{-1} \alpha_p^{lead} C_{i,(t-p)} + X'_{i,t} \beta + \gamma_i + \delta_t + \epsilon_{i,t} \quad (1)$$

With $y_{i,t}$ the outcome variable (*e.g.* voter turnout) in municipality i in month t ; $C_{i,(t-p)}$ is the main explanatory variable and represents corruption exposition measure

¹⁵For introduction on event-study see MacKinlay 1997. Other contributions are for example Jacobson, LaLonde, Sullival 1993, Kline 2011.

¹⁶There is a literature in Communication Science studying how editors/journalists choose news to be published. There are two opposing hypothesis: the one considering editors choosing news based on their professional judgements (biases can arise here), the *trustee model*, and the other considering editors following audience interests, the *market model* (Schudson 2003). There are several papers testing empirically these models and main findings are that audience revealed preferences on stories (generally measured with views to on-line articles) influence journalists decision-making (Anderson 2011, Welbers et al. 2015). So, it seems that market forces shape journalists' decision to publish a piece of news and this considerably limits space for journalist's discretion, weakening the possibility of endogenous timing in corruption news release.

¹⁷We run two different specifications excluding regional capitals municipalities or all municipalities in the province of regional capitals.

in municipality i at time $t - p$ ($C_{i,(t-1)}$ reports index value in month $t - 1$ [lag 1] and $C_{i,(t+1)}$ value at month $t + 1$ [lead 1]). Lagged variables ($C_{i,(t-1)}, \dots, C_{i,(t-P)}$) are explanatory variables of the analysis, while forward variables ($C_{i,(t+P)}, \dots, C_{i,(t+1)}$) are included as a falsification test, to check whether the outcome was already decreasing (or increasing) before the scandal takes place. Moreover, we use different versions of corruption index to capture the impact as covered period of time widens: aggregated by month, by semester and by year. $X_{i,t}$ is the vector of controls, γ_i are municipality fixed effects and δ_t year/month fixed effects.

The second specification is the compact version:

$$y_{i,t} = \sum_{p=1}^{+2} \alpha_p^S C_{i,(t-p)}^{Short} + \alpha^L C_{i,t}^{Long} + \alpha^P C_{i,t}^{Placebo} + X'_{i,t} \beta + \gamma_i + \delta_t + \epsilon_{i,t} \quad (2)$$

With $C_{i,(t-p)}^{Short}$ representing political corruption in the short-run, *i.e.* number of scandals in years $t - 1$ and $t - 2$. $C_{i,t}^{Long}$ represents the corruption in the long-run, *i.e.* aggregate number of scandals before $t - 2$ years. And $C_{i,t}^{Placebo}$ is the placebo test and it is defined as the number of scandals in year $t + 1$. Finally $y_{i,t}$, $X_{i,t}$, γ_i and δ_t are defined as in the classic specification.

For the analysis on voter turnout all regressions contain following list of controls: municipality and year/month fixed effects, province specific time trend, log of population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (yearly at the province level), number of candidates, dummy whether incumbent reruns, share of old candidates, victory margin (political variables refer to past elections) and number of years of *commissariamento* (which takes place in case of municipal government dissolution imposed by the national government, this can be due to several reasons such as mafia). For the analysis on political selection and supply regressions include the controls: municipality and year/macregion fixed effects, province specific time trend, log of population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (yearly at the province level) and number of years of *commissariamento*. Finally, in both analysis robust standard errors are clustered at the province level.

3.2 Validity Check

The main empirical challenge for estimating models (1) and (2) is to show that there are not confounding factors affecting timing of media coverage of scandals. In this section, we want to deal with this issue by studying whether scandals timing is strategically manipulated close to election dates.

In particular, we analyse whether there are systematically more articles about corruption right before elections, compared to following days; in particular, we study whether there is manipulation of the explanatory variable of the analysis, the corruption index, around the threshold represented by the election day. If we found that there are typically more articles about corruption in days before elections this could signal some form of strategic manipulation in articles' timing casting doubts on the presence of potential confounding factors at the local level. To do this, we run a McCrary test (McCrary 2008) on the entire sample of provinces, in a daily analysis where we consider that a province holds municipal elections whether at least one municipality vote in that day; we focus on a time window on 240 days (120 before and 120 after elections)¹⁸.

¹⁸Optimal bandwidths are computed according to McCrary 2008.

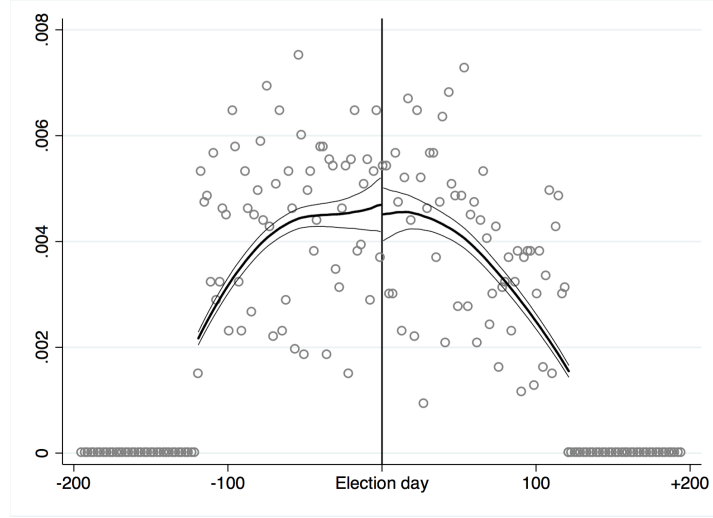


Figure 4: McCrary test

Figure 4 shows the output of McCrary test: it seems that there is not manipulation around election day, this allows us to reject the null hypothesis that there is a discontinuity in the density of scandals around the cutoff. Moreover, we raise the issue whether manipulation could emerge in a sub-sample of provinces where elections are more salient: results are similar if we limit the sample to regional capitals alone or to provinces with many municipalities voting (outputs for these checks are not shown and are available upon request). We can conclude that there is no evidence of manipulation of scandals timing and this validates the key assumption of the event study in analysis.

4 Results

4.1 Political Turnout

4.1.1 *Main Analysis*

In the first part of the paper we use political turnout as the dependent variable; it is a direct measure of political participation and it has been employed as a proxy for civic spirit and pro-social behaviour. The outcome of the classic event-study analysis is the event study-graph represented in Figure 5: where we inspect the effect of corruption exposition on voter turnout in the short run. Coefficients to the right are lags, explanatory variables, while coefficients to the left are leads, placebo variables; graphs include 95% confidence intervals.

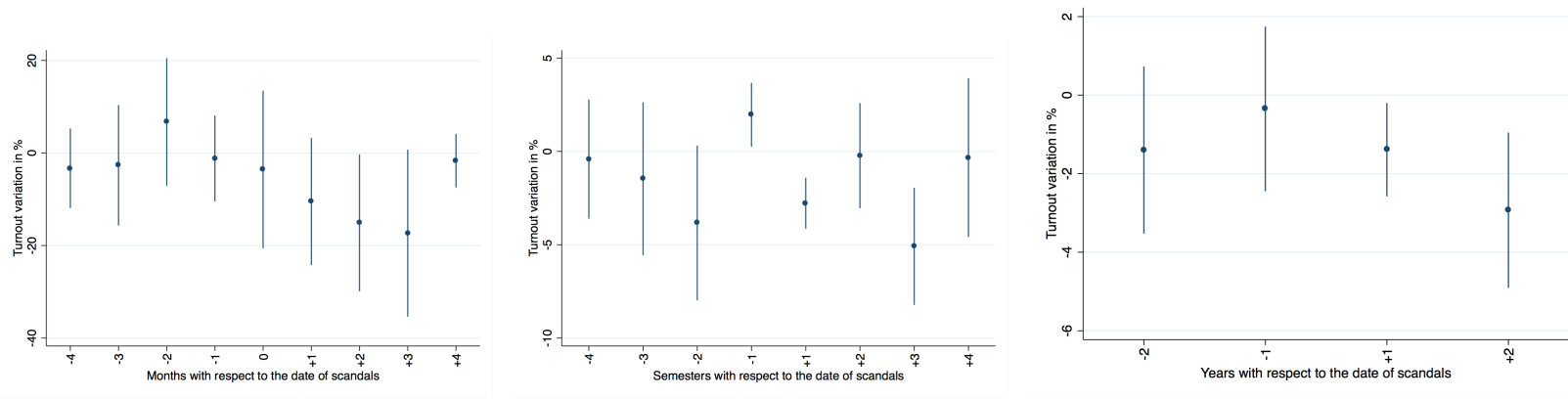
Left sub-figure in Figure 5 represents the analysis done with monthly aggregation, we can see that the impact of scandals the same month of election is zero, but as we move to older lags, coefficients become negative reaching a peak in the second and third lags; then, after the third lag the effect becomes zero again (in terms of magnitude one standard deviation increase in corruption exposition lowers turnout by 0.29% and 0.30% respectively after two and three months). This implies that scandals take time to become effective, and to affect voter turnout, and this is reasonable if we think that after scandal discovery public debates rise, because of the trial or pursuance of investigations, and this makes the event more salient to the audience. Furthermore, we cannot completely record these local public debates because of the source of our data, ANSA, it is so reasonable

to think that what we are capturing is the tip of the iceberg, a smaller subset of articles people are exposed to. On the other hand, the leads are not distinguishable from zero, meaning that voter turnout was not decreasing before scandals (no pre-trends). In central sub-figure of Figure 5, we study the effect with semester aggregation. The same pattern emerges: first, leads are not statistically different from zero, second lags are negative and as scandal becomes older the effect raises (one standard deviation increase in corruption exposition lowers turnout by 0.19% after one semester and 0.35% after three semesters). What is remarkable is that it seems that only first and third lags are significant. This is probably due to the seasonality in the corruption index as well as in election months: first, during summer and Christmas holidays there are systematically fewer articles (due to practical reasons as reduced activity of courts and newspapers), second most of the elections are in Spring; so, for the majority of elections, second and four lags include the summer period and this probably weakens the effect of the treatment: thus, emerging outcome is probably due to the lower number of articles on corruption as well as to a reduced level of attention towards politics and political scandals in that period from the audience. Right sub-figure of Figure 5 shows the effect of corruption exposition for the yearly analysis. We can observe a similar pattern to previous specifications. Leads are not distinguishable from zero while first and second lags are negative and significant: in particular, the effect of political scandals after two years is stronger than after one, this confirms the idea that public debate on a corruption case reinforces the (negative) effect on political participation (if the index raises by one standard deviation, turnout lowers by 0.17% after one year and 0.36% after two years).

For the compact analysis, we present a single specification where we include short and long run coefficients together (as in Bottan, Perez-Truglia 2015). Table 3 shows the results: first, the effect in the short run is captured by variables "Short-run effect (1 year)" and "Short-run effect (2 years)", representing the effect of scandals taking place one and two years before elections; column (1) Panel A contains the main specification for the entire sample and we can see that the effect is negative and significant for both explanatory variables. Moreover, the magnitude of the effect is not negligible, as corruption index raises by one standard deviation, voter turnout decreases by 0.29% after one year and by 0.41% after two years; this means that between 2.8% and 3.9% of average variation is explained by the corruption index. The magnitude of this effect is not trivial if we think that the effect of a newspaper entry increases turnout in Italian municipal elections by 0.45% (Drago et al. 2014)¹⁹. Second, the effect in the long run is captured by the variable "Log-run effect (3+ years)", representing the aggregate number of scandals before two years; we can see that despite being negative, the coefficient is not statistically different from zero. Finally, variable "Pre-scandal effect" represent the placebo test, *i.e.* aggregate number of scandals one year after elections, and it is evident that there are no pre-trends, as coefficient is non significant and almost zero; this means that before a scandal voter turnout between treated and control units evolves similarly²⁰. In Panel B of column (1) there is the main specification for large cities (*i.e.* cities with more than 15,000 inhabitants) and the effect is almost one third larger (negative and significant) than the one in the main specification for first lag, while it is not significant for second lag; nevertheless, this result holds despite evidence that above 15,000 inhabitants threshold turnout is higher than below (Barone, De Blasio 2013).

¹⁹In particular, Drago et al. 2014 focus only on large municipality, with more than 15,000 inhabitants, while our estimates cover the entire sample. As we show in this section, the effect is stronger for this sub-sample of municipalities, despite being less persistent.

²⁰We run the compact analysis with semester aggregation as well and results are similar to the yearly specification: scandals after 1 semester and 3 semesters negatively affect voter turnout while scandal after 2 and 4 semesters are not distinguishable from zero (this is due to the seasonality as already discussed). Moreover the effect in long-run is zero as well as after one year.



Note: All specifications include municipality and year/month fixed effect as well as province specific time trend, log of population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (province level), number of candidates, dummy whether incumbent reruns, share of old candidates, victory margin [political variables refers to past elections] and number of years of *commissariamento*. Each bracket represent a 95% confidence interval, and the centre of the bracket the point estimate. Confidence intervals are constructed with robust standard errors clustered at the province level.

Figure 5: Event study analysis

study this issue as an RDD exploiting the fact that 15,000 inhabitants is the threshold for electoral law to have a runoff and find out that runoff system increases electoral turnout). Furthermore, the result remains robust despite the sample shrinks (less than 10% of the entire sample).

Table 3: Compact analysis on voter turnout

	Municipal turnout	
	Main index (1)	Regional capital scandals (2)
Panel A: whole sample		
Short-run effect (1 year)	-2.310 (0.587)***	-2.235 (0.574)***
Short-run effect (2 years)	-3.327 (1.056)***	-3.244 (1.184)***
Long-run effect (3+ years)	-0.196 (0.277)	-0.188 (0.280)
Pre-scandal effect	-0.0535 (0.742)	-0.172 (0.758)
<i>N</i>	22892	22892
adj. R^2	0.830	0.830
Panel B: large cities (>15,000)		
Short-run effect (1 year)	-3.285 (1.136)***	-3.271 (1.204)***
Short-run effect (2 years)	-0.806 (1.898)	-0.515 (1.989)
Long-run effect (3+ years)	0.316 (0.279)	0.309 (0.286)
Pre-scandal effect	1.293 (1.442)	1.112 (1.506)
<i>N</i>	2078	2078
adj. R^2	0.844	0.844
The specification includes municipalities and year/month fixed effects as well as province specific time trend, log of population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (province level), number of candidates, dummy whether incumbent reruns, share of old candidates, victory margins [political variables refers to past elections] and number of years of <i>commissariamento</i> . Robust standard errors clustered at the province level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$		

Finally, column (2) of Table 3 contains the analysis using the version of the index capturing the scandals of regional capitals alone: same results holds for the entire and large cities panels. Coefficients are very similar, although slightly weaker, than the main specification. This seems to suggest that most effective scandals are the ones taking place in regional capitals compared to province and municipal administrative levels: this can be either due to the fact that regional capitals, where regional government and legislative power take place, host most serious scandals or that these cases have better media coverage compared to those in smaller cities.

From this analysis we can conclude that corruption exposition significantly reduces voter turnout in Italian municipalities; moreover, the effect reinforces with time and, finally, vanishes after two years from the scandal. And it does not seem to be pre-trends in voter turnout prior to scandals occurrence. Nevertheless, the effect is stronger for large cities but it fades sooner and it is mainly driven by scandals taking place in regional capitals.

4.1.2 Heterogeneity analysis

We want now to explore heterogeneous effects of corruption exposition. We conduct four different analysis. In the first inspection, we study whether differences in news media circulation mediates the effect of exposition to scandals: media economics literature underlines that the presence/entry of newspaper raises electoral participation (Gentzkow

et al. 2011) and affects other political aspects such as incumbency advantage (Snyder and Stromberg 2010). Moreover, Drago et al. 2014 show how (local) Italian newspapers play an important role in terms of electoral turnout and efficiency of municipal governments. In our setting, we want to see whether newspaper circulation is able to amplify the effect of scandals, focusing on national newspapers. To do this we collected data on circulation at the province level from *ADS-Accertamenti Diffusione Stampa*, for national newspapers²¹ and we run the main specification for the sub-sample of municipalities below/above median level of newspaper circulation, in the time span 2003-2014²². Columns (2) and (3) of Table 4 show results: municipalities above the median react more to scandals compared to those below where the effect is much weaker. We could conclude that diffusion of newspaper is an important mediator for the effect of political scandals on voter turnout and this can be due to the fact that news circulation makes corruption stories more salient or that it is associated to higher interest in politics.

As a second analysis we study differences in local political background: columns (4) and (5) of Table 4 shows the effect for municipalities below/above median turnout in previous elections; it is evident that cities with higher past turnout react more than the other group. We can link the result on voter turnout with the social capital literature (Putnam 1993) that identifies turnout as a measure of civic spirit; in these regards, places with higher social capital face less vote buying as well as clientelism and political participation is more likely to be motivated by civic engagement; thus, corruption exposition is more effective where citizens vote according to moral concerns rather than to material interests. Second, it seems that the effect becomes stronger as electoral competition raises, *i.e.* as margin of victory decreases (columns 6-8). This result can be explained by the fact that scandals are more salient as political debate becomes more intense and this leads people reacting more to perceived corruption.

Third, we examine differences in cities size and location: in terms of size, we may think that dimension are related with citizens characteristics (*e.g.* income, education, attention to politics) which mediates the effect of the treatment, or that these are the cities (such as province/region capitals) where scandals actually take place. In Columns (9), (10) and (11) of Table 4, we study main specification with three sub-samples, cities with more than 5,000, 10,000 and 15,000 inhabitants: As population raises the effect is stronger. As a second analysis, we study geographical heterogeneity of the effect (columns (12), (13)): we can see that the effect is strong and significant for centre-north and negative but not distinguishable from zero for centre-south²³: this evidence is related with the literature on social capital in Italy (Putnam 1993, Banfield 1958) underlying that northern regions are marked by high levels of civic spirit opposed as southern regions; this result, along with previous evidence on turnout, seems to suggest that civic spirit is an important mediator of the effect of exposition to scandals.

Finally, we raise the issue whether municipality demographic structure affects the way people react to local scandals; in these regards, there is a large literature in Political Science pointing out that young cohorts use to participate less in politics, in terms of voting turnout (Quintelier 2007), partly because of lack of material interests to protect, partly because they are diverted by other forms of participation, such as activism in social media. In this context, young cohorts could react less to scandals because they care less, in the traditional way, about politics, or they could react more because their activism on the web and on social media makes them more exposed to news and debates on political scandals. An additional result (not shown, and available upon request)

²¹We include top 18 Italian national newspapers in 2013, according to Wikipedia (https://it.wikipedia.org/wiki/Quotidiani_in_Italia).

²²We were not able to collect circulation data for the period 1997-2002.

²³We divide Italian territory in two macroareas: centre-north including northern regions as well as Tuscany, Umbria and Marche, and centre-south, including southern regions, islands and Lazio.

Table 4: Heterogeneity analysis

Dep. var.: Municipal turnout	Main specification	National newspaper		Past turnout		Margin of victory			Population			Macro-area	
	Whole sample (1)	Below median (2)	Above median (3)	Below median (4)	Above median (5)	First tercile (6)	Second tercile (7)	Third tercile (8)	>5,000 (9)	>10,000 (10)	>15,000 (11)	Centre-north (12)	Centre-south (13)
Short-run effect (1 year)	-2.310 (0.587)***	-2.518 (1.367)*	-3.387 (1.119)***	-1.943 (1.065)*	-2.869 (0.908)***	-2.217 (1.192)*	-2.361 (1.288)*	-2.602 (1.797)	-2.796 (0.817)***	-3.049 (1.009)***	-3.285 (1.136)***	-2.695 (1.023)**	-1.224 (1.290)
Short-run effect (2 years)	-3.327 (1.056)***	0.388 (2.233)	0.417 (1.799)	-3.145 (1.535)**	-3.304 (1.139)***	-4.163 (1.743)**	-3.372 (1.447)**	-1.017 (2.735)	-1.945 (1.392)	-1.506 (1.624)	-0.806 (1.898)	-0.778 (1.831)	-1.719 (1.426)
Long-run effect (3+ years)	-0.196 (0.277)	-0.187 (0.603)	0.153 (0.469)	-0.213 (0.505)	-0.174 (0.283)	-0.299 (0.479)	0.0483 (0.372)	0.0512 (0.542)	0.350 (0.200)*	0.356 (0.210)*	0.316 (0.279)	0.154 (0.386)	-0.468 (0.426)
Pre-scandal effect	-0.0535 (0.742)	1.271 (1.914)	0.797 (1.531)	-3.128 (1.768)*	2.101 (0.967)**	-0.552 (1.383)	0.207 (1.497)	-0.426 (2.581)	0.905 (0.888)	1.410 (1.328)	1.293 (1.442)	1.034 (1.341)	-0.509 (0.903)
N	22892	7160	8199	10901	11991	7699	7695	7498	6768	3404	2078	14191	8701
adj. R^2	0.830	0.888	0.842	0.798	0.601	0.899	0.890	0.795	0.834	0.819	0.844	0.777	0.868

The specification includes municipalities and year/month fixed effects as well as province specific time trend, log of population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (province level), number of candidates, dummy whether incumbent reruns, share of old candidates, victory margins [political variables refers to past elections] and number of years of *commissariamento* [time span covered for the *National newspaper* columns is 2003-2014]. Robust standard errors clustered at the province level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

shows that proportion of young, adult and old population does not mediate the effect on political participation; demographic structure does not matter and the effect is general across cohorts.

In this section we shed light on factors mediating the effect of being exposed to corruption of local politicians; it turns out that municipalities reacting more are those with higher levels of newspaper circulation, where people participated more in the past, where elections are more competitive and located in the centre-north of the country. It is surprising, though, that demographic structure does not play any role in this process. Nevertheless, we should take these results with caution as it is possible that these mediators are associated with unobservables correlated with voter turnout as well. By the way, we try to account for this issue controlling for a large set of municipality characteristics.

4.1.3 Varying corruption content

As a second step, we want to show how the effect of being exposed to political scandals changes as articles corruption intensity raises. Table 5 shows the compact analysis including main specification as well as three versions of the index with increasing corruption content: column (1) contains main specification, column (2) results for the specification with the first version of the coefficient (70th percentile), column (3) with the second version (80th percentile), column 4 with the third one (90th percentile). We can confirm what emerged in the main specification. Lag 1 and 2 negatively affect the dependent variable, and the effect after two years is stronger than after one; the effect in the long-run is negative but not distinguishable from zero and there is no evidence of pre-trends. Moreover, it is evident that the marginal effect of the treatment is heavier as corruption content raises: considering the effect of scandals after two years, the effect of one standard deviation increase in the index lowers turnout by 0.39% (70th percentile) by 0.40% (80th percentile) and by 0.41% (90th percentile).

Table 5: Compact analysis-varying corruption intensity

	Municipal turnout			
	Main specification (1)	Corruption intens. 70th (2)	Corruption intens. 80th (3)	Corruption intens. 90th (4)
Short-run effect (1 year)	-2.310 (0.587)***	-2.268 (0.608)***	-2.967 (0.722)***	-5.538 (2.325)**
Short-run effect (2 years)	-3.327 (1.056)***	-3.344 (1.065)***	-5.085 (1.452)***	-10.04 (2.843)***
Long-run effect (3+ years)	-0.196 (0.277)	-0.178 (0.290)	-0.332 (0.445)	0.0196 (0.901)
Pre-scandal effect	-0.0535 (0.742)	-0.160 (0.759)	-0.294 (1.026)	-1.905 (1.351)
<i>N</i>	22892	22892	22892	22892
adj. <i>R</i> ²	0.830	0.830	0.830	0.830

The specification includes municipalities and year/month fixed effects as well as province specific time trend, log of population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (province level), number of candidates, dummy whether incumbent reruns, share of old candidates, victory margin [political variables refers to past elections] and number of years of *commissariamento*. Robust standard errors clustered at the province level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

From this analysis we can confirm that as articles contain more corruption keywords, the impact on voter turnout becomes more effective. On the one hand, this can be due to the fact that higher corruption intensity is associated with more serious/large scandals in a certain place leading people to react more; on the other hand, the index becomes more precise, *i.e.* we get rid of false positive articles that can be captured, by mistake,

by the index (at the expenses of discarding true positives because the criterion is now so stringent). To conclude, this subsection shows that people's indignation depends both on *whether* they are exposed to news on corruption and on *what* scandals, in terms of severity and dimension, they find in the newspaper.

4.2 Political Selection

In this section we are interested in studying whether exposition to scandals affects selection of politicians, *i.e.* *how* people vote. We may expect a positive reaction consisting in renovation and improvement of local political class (retaliation) or negative reaction without any, or negative, effects in terms of selection (resignation). For the sake of brevity, in this and in following section, we only present the compact study.

To shed light on this point we evaluate the impact on characteristics of elected mayors and councillors: first, age, gender and education; on the one hand female and young politicians are generally regarded as "underdogs" in Italy (Nannicini, Galasso 2011), on the other hand, education is commonly used as a measure of politicians quality (*e.g.* Merlo et al. 2010, Galasso and Nannicini 2011); columns 1-6 of Table 6 contain results on politicians characteristics for two samples: complete and reduced (only municipalities with more than 15,000 inhabitants); two results emerge: first, there is no effect on characteristics of elected mayors (columns 1-3) as well as councillors (columns 4-6) both in complete and in reduced sample. Second, there is weak evidence that scandals negatively impact on education for councillors elected. These results suggest that exposition to corruption does not impact on the selection of politicians. As a second analysis we focus on the background of elected mayors to see whether chosen politicians belong to the political class or they are freshmen; we concentrate on the probability that the incumbent is re-elected, on the probability that politicians who already run in the past are elected²⁴ and on the civic nature of the mayor, *i.e.* whether elected mayor is not affiliated to any national party (as a measure of direct engagement of citizens as well as demand for change from the electorate). Results are contained in columns 7-11 of Table 6: there is evidence, mostly in the long run, that the probability of the incumbent to be re-elected raises (columns 8 and 9) as well as the probability that an old candidate is elected (columns 10 and 11), and this is stronger for large cities and if we consider all past elections. Finally, the probability that mayor is civic raises (column 7), although the evidence is quite limited. These results suggest that there is no demand for change from the electorate after a scandal, rather old politicians are more represented as mayors and councillors.

From this section it emerges that being exposed to local scandals does not modify elected characteristics, suggesting that the electorate does not react to perceived corruption trying to improve or renew political class²⁵. Moreover, elected mayors belongs more likely to the old political class.

4.3 Political supply

As a second analysis, we consider how local politics endogenously responds to the occurrence of local scandals; similarly with voter turnout, we may think about two possible effects: retaliation, positive reaction of political life, with emergence of new (civic) forces

²⁴For these two variables we focus both on last elections and on all past elections held in a certain municipality.

²⁵The eradication of Italian *Movimento Cinque Stelle* has been due, among other reasons, to the reaction to widespread public corruption, we cannot properly observe the impact of the movement in this analysis as it runs in few municipalities within this time span. First elections where the movement obtains consistent results has been the one of June 2016 (where the party wins in Rome).

Table 6: Compact analysis on Political Selection - characteristics and background

	Mayors' characteristics			Elected characteristics			Mayors' background				
	Age	Female Prop.	Education	Age (Av.)	Female Prop.	Education (Av.)	Civic mayor	Incumbent elected (Last ES)	Incumbent elected (All ES)	Old candidate elected (Last ES)	Old candidate elected (All ES)
Panel A: whole sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Short-run effect (1 year)	0.904 (1.522)	-0.0137 (0.0462)	0.0646 (0.101)	0.754 (0.591)	0.0256 (0.0140)*	-0.00377 (0.0447)	0.281 (0.121)**	0.128 (0.0996)	0.211 (0.102)**	0.172 (0.119)	0.243 (0.124)*
Short-run effect (2 years)	-0.875 (1.743)	0.0160 (0.0351)	0.0506 (0.0902)	0.468 (0.499)	0.00195 (0.0144)	-0.0731 (0.0343)**	0.0842 (0.156)	-0.0949 (0.121)	-0.0881 (0.141)	-0.0846 (0.115)	-0.0794 (0.121)
Long-run effect (3+ years)	0.252 (0.230)	-0.00167 (0.00796)	-0.0199 (0.0170)	0.143 (0.101)	-0.00544 (0.00329)	-0.0125 (0.00824)	0.00961 (0.0351)	0.0112 (0.0119)	0.0206 (0.0124)	0.0183 (0.0134)	0.0219 (0.0129)*
Pre-scandal effect	3.140 (1.748)*	0.00603 (0.0466)	-0.0373 (0.110)	-0.511 (0.767)	-0.0372 (0.0155)**	-0.00250 (0.0453)	-0.140 (0.172)	0.0851 (0.0965)	-0.0341 (0.0977)	0.0203 (0.0959)	-0.0807 (0.0986)
<i>N</i>	24954	25170	24217	25030	25170	25011	25170	25170	25170	22892	22892
adj. R^2	0.259	0.223	0.367	0.495	0.427	0.715	0.644	0.248	0.242	0.264	0.274
Panel B: large cities (>15,000)											
Short-run effect (1 year)	1.677 (4.357)	-0.0484 (0.113)	0.0558 (0.205)	0.744 (0.934)	0.0307 (0.0322)	0.00395 (0.0687)	0.0418 (0.0672)	-0.0682 (0.239)	-0.0276 (0.250)	-0.0370 (0.270)	0.0144 (0.302)
Short-run effect (2 years)	-1.395 (3.550)	0.0271 (0.0980)	-0.418 (0.227)*	-0.785 (1.096)	-0.0395 (0.0226)*	-0.00806 (0.0746)	0.00334 (0.103)	0.203 (0.333)	0.181 (0.363)	0.360 (0.321)	0.264 (0.366)
Long-run effect (3+ years)	1.097 (0.476)**	-0.00542 (0.0234)	-0.0511 (0.0635)	0.148 (0.191)	-0.00888 (0.00531)*	-0.0100 (0.0206)	0.000770 (0.0218)	0.0922 (0.0354)**	0.126 (0.0392)***	0.100 (0.0346)***	0.119 (0.0402)***
Pre-scandal effect	5.421 (4.072)	0.0697 (0.133)	0.356 (0.274)	-1.062 (1.330)	-0.0326 (0.0230)	-0.0239 (0.0694)	-0.141 (0.0986)	0.312 (0.249)	0.290 (0.304)	0.248 (0.291)	0.358 (0.302)
<i>N</i>	2181	2212	2127	2189	2212	2189	2212	2212	2212	2078	2078
adj. R^2	0.182	0.168	0.255	0.539	0.662	0.630	0.646	0.291	0.269	0.282	0.302

The specification includes municipalities and year/macro-region fixed effects as well as province specific time trend, log of population, net number of firms (province level) and number of years of *commissariamento*. Robust standard errors clustered at the province level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

as well as regeneration of local political class, or resignation, with decrease in direct political engagement of citizens and the stasis of local political class and debate. To answer this question we inspect a series of variables. First, we want to explore how candidate pool modifies: number of candidates running in an election for mayor and number of presented lists; second, we want to measure whether political background of candidates changes after a scandal, *i.e.* whether they are supported by the civic society (using as proxy the share of civic lists presented) or they belong to the political class (using as proxy the share of candidates that already run in past elections as well as probability for the incumbent to rerun).

Columns 1-4 of Table 7 contain results on number of candidates/lists for two samples: complete and reduced (only municipalities with more than 15,000 inhabitants). Three results emerge: first, number of candidates significantly decreases (column 1) and the effect is almost five time stronger for the reduced sample; in terms of magnitude one standard deviation increase of the corruption index decreases, on average, number of candidates by 0.05 units (complete sample) and by 0.23 units (reduced sample), explaining respectively 3.8% and 17.5% of variable standard deviation. Second, the negative effect on the number of candidates is stronger once we consider scandals of regional capitals alone (column 2), one standard deviation increase of the corruption index decreases number of candidates by 0.06/0.27 units (respectively in complete/reduced sample); furthermore, the treatment negatively affects number of presented lists (columns 3-4) but the effect is not different from zero for both samples (complete, reduced), for both treatments (main index, regional scandals). One remark of these results is that the effect is significant only after two years from the scandals: this is coherent with the fact that, differently from voter turnout, the decision to run as a candidate is made several months before elections; so, it is unlikely that this variable react to scandals close to electoral date.

On the other hand, columns 5-9 of Table 7 contain results for the analysis on the background of candidates and are obtained using the main index: First, scandal exposition increases the probability that old candidates run (columns 6 and 7), and the result holds whether we focus on old candidates in last elections or in all past polls; moreover, the effect is stronger in the long-run. Second same result applies if we consider the probability that the incumbent runs (columns 8 and 9); finally there is no effect for the proportion of civic lists (column 5). This means that the (relative) importance of the old political class raises and that there is no positive reaction in terms of direct engagement of citizens, since the relevance of civic lists does not vary; this result suggests that in the local political environment there is less space for freshmen and direct citizens participation while old political class does not falls behind, gaining further space.

An important *caveat* is that local politics could be directly affected by corruption, *e.g.* a scandal leading to detention of mayor and several councillors would definitely affect political supply of that municipality; in that case the effect would be a "mechanic" consequence of the scandal, and this can be a concern especially for the variable number of candidates. In order to rule out this alternative explanation, we rely on two pieces of evidence. First, most of recorded scandals mention politicians in charge in higher levels of local administration; in particular, 54% of cases pertain politicians in regional administration and 24% politicians in the provincial administration. Overall, 72% of mentioned scandals pertain different administrative levels from municipalities²⁶: this means that what we are measuring is exposition to local scandals which are not likely to be at the municipality level, so without direct effect on municipal elections. Second, we run in the robustness checks section the main specification using as treatment regional scandals and excluding large cities (region capitals, province capitals, regional capitals

²⁶The overall percentage is not a simple sum of regional and provincial scandal share because there are cases where several politicians are mentioned in the same article, being involved in the same scandal.

Table 7: Compact analysis on Political Supply - number and background

	Number of candidates		Number of lists		Civic list	Old candidate runs (Last ES)	Old candidate runs (All ES)	Incumbent runs (Last ES)	Incumbent runs (All ES)
	Main index (1)	Regional capital (2)	Main index (3)	Regional capital (4)	(5)	(6)	(7)	(8)	(9)
Panel A: whole sample									
Short-run effect (1 year)	-0.0865 (0.188)	-0.0554 (0.195)	0.0142 (0.296)	0.123 (0.303)	0.110 (0.0884)	0.0992 (0.0803)	0.153 (0.0958)	0.102 (0.113)	0.181 (0.130)
Short-run effect (2 years)	-0.375 (0.181)**	-0.480 (0.179)***	-0.277 (0.245)	-0.276 (0.274)	0.203 (0.148)	0.00679 (0.0656)	0.0245 (0.0724)	-0.0456 (0.126)	-0.0511 (0.120)
Long-run effect (3+ years)	-0.0439 (0.0372)	-0.0465 (0.0411)	0.00680 (0.0612)	0.0297 (0.0651)	0.00913 (0.0347)	0.00889 (0.00832)	0.0194 (0.00946)**	0.0225 (0.0158)	0.0401 (0.0156)**
Pre-scandal effect	-0.255 (0.212)	-0.275 (0.218)	-0.141 (0.271)	-0.169 (0.279)	-0.0203 (0.155)	0.0901 (0.0570)	0.0200 (0.0732)	0.140 (0.0908)	0.0477 (0.106)
N	22891	22891	22892	22892	22892	22892	22892	22892	22892
adj. R^2	0.565	0.565	0.894	0.894	0.685	0.334	0.332	0.277	0.289
Panel B: large cities (>15,000)									
Short-run effect (1 year)	1.075 (1.043)	1.497 (1.038)	1.818 (1.865)	2.628 (1.902)	-0.0218 (0.0517)	-0.0238 (0.127)	-0.0746 (0.137)	0.173 (0.319)	0.140 (0.336)
Short-run effect (2 years)	-1.863 (0.713)**	-2.195 (0.747)***	-2.332 (1.403)*	-2.277 (1.401)	0.0812 (0.0734)	0.219 (0.106)**	0.146 (0.133)	0.0324 (0.347)	0.0152 (0.366)
Long-run effect (3+ years)	-0.121 (0.169)	-0.106 (0.170)	0.395 (0.350)	0.511 (0.343)	0.0261 (0.0160)	0.0254 (0.00953)***	0.0228 (0.0136)*	0.102 (0.0342)***	0.0979 (0.0396)**
Pre-scandal effect	-0.762 (0.699)	-1.009 (0.776)	0.489 (1.759)	0.256 (1.894)	0.0153 (0.0650)	0.0442 (0.0991)	0.0228 (0.132)	0.294 (0.234)	0.191 (0.295)
N	2078	2078	2078	2078	2078	2078	2078	2078	2078
adj. R^2	0.451	0.452	0.705	0.705	0.802	0.381	0.377	0.301	0.311

The specification includes municipalities and year/macro-region fixed effects as well as province specific time trend, log of population, net number of firms (province level) and number of years of *commissariamento*. Robust standard errors clustered at the province level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

districts) and we show that the result does not vanish.

This section suggests that exposition to local political scandals decreases number of running candidates and raises the space of old political class, without affecting direct engagement of citizens; this coincides with a decrease in the level of electoral competition. Nevertheless, these results are stronger in large cities (above 15,000 inhabitants) and this is coherent with the result, of previous heterogeneity analysis, that large cities react more in terms of turnout than others.

5 Robustness checks

5.1 Voter turnout

In this section we run several robustness checks relative to the analysis on voter turnout; in particular, we want to show that the result does not depend on how the sample is structured and the index is constructed as well as on the specification we use.

In columns 1-5 of Table 8 we run main specification modifying the index and the specification itself. Column (1) contains main specification; column (2) contains the output for the analysis done with province aggregation; *i.e.* we run main specification at the province level using as dependent variable the average turnout of municipalities in a province in a certain year/month: on the one hand, the sample shrinks and results become much weaker; on the other hand, coefficients have same signs as the main specification (with the exception of the long run effect) and the impact after two years is significant and negative²⁷. Columns (3) and (5) include different versions of the index: first, column (3) shows results for the analysis done with the index rescaled with monthly amount of articles, instead of yearly²⁸; second, column (5) includes the analysis done with the index varying at the municipality level, in particular with the aggregate version where the treatment for municipality x is the amount of scandals in x plus the scandals in province and region capital of x . In both specifications coefficients are slightly weaker but the same pattern emerges: exposition to corruption negatively and significantly affects voter turnout in the short run, and this is true in complete and reduced samples. Finally, column (4) shows the main specification without pre-scandal effect and coefficients are very similar to the ones in the main specification. In general, it seems that the negative effect of the treatment on voter turnout persists as we make use of different versions of the index and different specifications.

In columns 6-10 of Table 8 we study main specification modifying the sample. First, we study the relation excluding large cities from the analysis: as most of the scandals take place in populous cities, we want to check whether these are driving main results. In column 6-8 we restrict the sample excluding respectively region capitals, province capitals and municipalities in the province of region capitals. It is remarkable that in these three specifications main relation persists and magnitudes of coefficients do not modify significantly (unique exception is the positive and significant long run effect in column 8). It seems that the relation holds without large cities in the sample. As a second test, we repeat previous analysis excluding large cities and we restrict the treatment to scandals taking place in large cities themselves: we want to check whether municipalities react to scandals that took place outside. This could dispute the presence of unobservables that drive corruption as well as political participation at the local level as we evaluate the impact of corruption level outside the city on political participation of

²⁷We did not present the analysis with large cities sample as now unit of observation is province.

²⁸To be more precise, in the main index we rescale the absolute count of articles for the total number of articles in the year the news come out, in this version we are scaling the absolute number of articles with the total number of news in the month/year it appears (using as denominator N_{my} instead of N_y).

Table 8: Robustness checks - voter turnout

Municipal Turnout										
	Main specification	Province variation	Index with monthly scale	Without leads	Municipality specific index (ComProvReg scandals)	No regional cap.	No province cap.	No regional cap. district	No regional cap. (Region cap. scandals)	No regional cap. district (Region cap. scandals)
Panel A: whole sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Short-run effect (1 year)	-2.310 (0.587)***	-1.746 (1.271)	-2.166 (0.540)***	-2.340 (0.685)***	-1.566 (0.704)**	-2.280 (0.584)***	-2.268 (0.587)***	-2.200 (0.605)***	-2.203 (0.572)***	-2.150 (0.579)***
Short-run effect (2 years)	-3.327 (1.056)***	-1.988 (1.116)*	-1.888 (0.974)*	-3.323 (1.034)***	-2.540 (1.086)**	-3.356 (1.046)***	-3.388 (1.058)***	-3.326 (1.105)***	-3.276 (1.175)***	-3.273 (1.275)**
Long-run effect (3+ years)	-0.196 (0.277)	0.130 (0.311)	-0.0424 (0.242)	-0.196 (0.277)	-0.161 (0.289)	-0.190 (0.276)	-0.191 (0.280)	-0.0591 (0.312)	-0.182 (0.280)	-0.0600 (0.314)
Pre-scandal effect	-0.0535 (0.742)	2.111 (1.132)*	0.170 (0.601)		-0.0927 (0.739)	-0.0580 (0.742)	-0.0690 (0.747)	0.256 (0.786)	-0.176 (0.758)	0.165 (0.799)
<i>N</i>	22892	1385	22892	22892	22892	22840	22553	17726	22840	17726
adj. <i>R</i> ²	0.830	0.637	0.829	0.830	0.829	0.830	0.830	0.823	0.830	0.823
Panel B: large cities (>15,000)										
Short-run effect (1 year)	-3.285 (1.136)***		-2.211 (0.861)**	-2.678 (1.210)**	-2.406 (0.876)***	-3.175 (1.101)***	-3.324 (1.131)***	-3.339 (1.866)*	-3.146 (1.167)***	-3.531 (2.049)*
Short-run effect (2 years)	-0.806 (1.898)		0.408 (1.706)	-1.018 (1.815)	-0.329 (1.514)	-0.852 (1.813)	-1.044 (2.012)	-0.337 (1.619)	-0.567 (1.908)	0.240 (1.901)
Long-run effect (3+ years)	0.316 (0.279)		0.389 (0.252)	0.298 (0.275)	0.341 (0.258)	0.366 (0.272)	0.377 (0.280)	0.750 (0.285)**	0.358 (0.278)	0.753 (0.283)***
Pre-scandal effect	1.293 (1.442)		0.980 (1.067)		0.312 (1.200)	1.283 (1.470)	1.198 (1.528)	3.893 (1.168)***	1.102 (1.534)	3.885 (1.348)***
<i>N</i>	2078		2078	2078	2078	2026	1756	1168	2026	1168
adj. <i>R</i> ²	0.844		0.843	0.844	0.843	0.841	0.837	0.866	0.841	0.865

The specification includes municipalities and year/month fixed effects as well as province specific time trend, log of population, share of population in cohorts 15-25, 26-65, 66+, net number of firms (province level), number of candidates, dummy whether incumbent reruns, share of old candidates, victory margins [political variables refers to past elections] and number of years of *commissariamento*. For the specification *Province variation* province and year fixed effects are included instead of municipality and year/month fixed effects. Robust standard errors clustered at the province level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

that municipality. In particular, we use as treatment scandals of region capitals and we evaluate the effect on the complete sample excluding regional capitals (column 9) and municipalities in the province of region capitals (column 10). In both cases estimates are very similar to the main specification for complete and reduced sample as the treatment negatively and significantly impacts voter turnout in the short run (unique exception, similarly to column 8, is the positive and significant long run effect in column 10). From these results we can infer two conclusions on the negative effect of corruption exposition: first, it is not driven by large cities into the sample; second, the possible role of time-varying local confounding factors seems negligible as small cities react strongly to scandals taking place outside.

5.2 Number of candidates

In this sections we repeat the set of checks performed in previous section for the analysis on variable number of candidates. In columns 1-5 of Table 9 we run main specification modifying the index and the specification itself. Column (2) shows the outcomes for the analysis done with province aggregation: coefficients signs are similar to main specification although being much weaker, but the impact is significant in the long run; columns (3) and (5) include different versions of the index, respectively monthly version of the index and municipality specific index: estimates are very similar, although slightly weaker, to the main specification as exposition to corruption negatively and significantly affects the number of candidates; and this is true for both samples, complete and reduced. Finally, column (4) includes main specification without pre-scandal effect and coefficients are very similar to the main analysis. In conclusion it seems that the negative relation persists as we use different versions of the index as well as different specifications.

In columns 6-10 of Table 9 we study main specification modifying the sample. In columns (6-8) we restrict the sample excluding respectively region capitals, province capitals and municipalities in the province of region capitals. Three remarks can be done: first, main results hold with a negative and significant effect of the treatment on dependent variable; second, the effect is stronger than the standard one in the specification without province capitals (column 7) and third, in the specification without municipalities in the province of region capitals the effect is significant in the long run and not in the short run. Finally, in columns (9-10) we study the effect of scandals of region capital on the entire sample excluding respectively regional capitals/municipality of province of regional capitals: the negative and significant effect persists for both complete and reduced sample. From these results it seems that large cities do not drive the effect and that time-varying local confounding factors do not play an important role in this relation.

6 Concluding Remarks

In this contribution we explore the effect of corruption exposition through the press on local political participation. We focus on several aspects of political participation: voter turnout, selection of politicians and political supply. Our results suggest that what prevails is a resignation effect, rather a retaliation one: both in terms of citizens and politicians participation.

First, we find that municipalities which are exposed to local political corruption face lower levels of voter turnout and that scandals of the regional capitals are most effective ones. Moreover, we find evidence of heterogeneous effects as the relation is stronger for cities that are large, located in the centre-north, with higher past participation,

Table 9: Robustness checks - number of candidates

	Number of candidates									
	Main specification	Province variation	Index with monthly scale	Without leads	Municipality specific index (ComProvReg scandals)	No regional cap.	No province cap.	No regional cap. district	No regional cap. (Region cap. scandals)	No regional cap. district (Region cap. scandals)
Panel A: whole sample	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Short-run effect (1 year)	-0.0865 (0.188)	5.104 (8.567)	-0.0766 (0.143)	-0.191 (0.157)	0.0132 (0.165)	-0.0858 (0.190)	-0.0531 (0.197)	-0.227 (0.170)	-0.0556 (0.197)	-0.193 (0.181)
Short-run effect (2 years)	-0.375 (0.181)**	-13.21 (12.09)	-0.281 (0.128)**	-0.384 (0.185)**	-0.358 (0.117)***	-0.361 (0.181)**	-0.385 (0.176)**	-0.277 (0.207)	-0.468 (0.179)**	-0.422 (0.201)**
Long-run effect (3+ years)	-0.0439 (0.0372)	-3.000 (1.610)*	-0.0384 (0.0296)	-0.0434 (0.0367)	-0.0361 (0.0375)	-0.0411 (0.0373)	-0.0359 (0.0375)	-0.0704 (0.0397)*	-0.0442 (0.0412)	-0.0759 (0.0452)*
Pre-scandal effect	-0.255 (0.212)	-7.474 (7.613)	-0.173 (0.148)		-0.0874 (0.196)	-0.240 (0.215)	-0.275 (0.202)	-0.184 (0.252)	-0.261 (0.222)	-0.182 (0.265)
<i>N</i>	22891	1527	22891	22891	22891	22839	22552	17725	22839	17725
adj. R^2	0.565	0.884	0.565	0.565	0.565	0.543	0.503	0.541	0.544	0.541
Panel B: large cities (>15,000)										
Short-run effect (1 year)	1.075 (1.043)		0.494 (0.659)	0.793 (0.953)	0.767 (0.691)	1.029 (1.060)	1.524 (1.079)	-0.762 (1.410)	1.446 (1.060)	-0.472 (1.499)
Short-run effect (2 years)	-1.863 (0.713)**		-1.265 (0.545)**	-1.757 (0.697)**	-1.329 (0.587)**	-1.804 (0.741)**	-2.450 (0.838)***	-2.013 (1.091)*	-2.156 (0.773)***	-2.331 (1.194)*
Long-run effect (3+ years)	-0.121 (0.169)		-0.119 (0.128)	-0.104 (0.169)	-0.0817 (0.158)	-0.117 (0.168)	-0.178 (0.173)	-0.323 (0.237)	-0.106 (0.170)	-0.291 (0.250)
Pre-scandal effect	-0.762 (0.699)		-0.411 (0.514)		-0.462 (0.458)	-0.586 (0.717)	-1.061 (0.718)	0.554 (1.118)	-0.840 (0.808)	0.445 (1.267)
<i>N</i>	2078		2078	2078	2078	2026	1756	1168	2026	1168
adj. R^2	0.451		0.451	0.451	0.451	0.389	0.305	0.435	0.390	0.434

*The specification includes municipalities and year/macro-region fixed effects as well as province specific time trend, log of population, net number of firms (province level) and number of years of *commissariamento*. For the specification *Province variation* region fixed effects are included instead of municipality. Robust standard errors clustered at the province level are in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

electoral competition and newspaper circulation levels. This result can be read in light of the model by Persson, Tabellini (2000) where corruption works as a signal for the entire political class leading voters to, negatively, update their beliefs and to reduce their participation in equilibrium. Nevertheless, we find that articles content matters: as corruption content raises, the marginal effect is stronger. This suggests that voters' reaction depends both on *whether* they are exposed to corruption stories and *what* scandals they are exposed to. Second, we focus on selection of politicians, mayors and councillors, and we do not find clues that corruption exposition leads to politicians selection; the unique result of the analysis is that old political class wins more often as incumbents and old candidates are elected more likely. Finally, we study the impact on political supply. On the one hand we find that few candidates run, and the effect is stronger for larger cities. On the other hand, we shed light on the fact that old politicians do not leave the competition, gaining further space, at the expenses of freshmen as there is evidence that the proportion of old candidates and incumbents who run raises. We could read this last result in the light of the model by Caselli, Morelli (2004) as the presence of corruption could modify politician image affecting costs of running for freshmen. This would work as a negative externality raising entry costs for freshmen without modifying incentives for "old politicians" and could generate, in equilibrium, a lower number of non-politicians running for mayor.

In conclusion, general effect of exposition to corruption seems to be that citizens care less about local politics, turnout less, and do not select better politicians; while political class does not renew and leave space to old politicians. These results seem to confirm resignation hypothesis in terms of citizens and politicians participation.

Appendix A1: Corruption exposition index validation

We want to provide evidence that the index we generate really captures people exposition to political corruption; to do this we make use of Google trend, as a tool to explore people attention in a certain moment of time; as already anticipated, we select two keywords related to corruption, *i.e.* *corruzione* (corruption) and *abuso d'ufficio* (malfeasance in office) and we study whether there are peaks in searches of these keywords immediately after an article on local corruption appears in the newspaper. Furthermore, we can exploit the high-frequency nature of our index and we perform a daily analysis, with regional aggregation (the drawback of Google trend data, for Italy, is this very aggregate geographical variation). We run the following specification:

$$K_{r,d} = \sum_{p=0}^{+14} \alpha_p^{lag} C_{r,(d-p)} + \sum_{p=-14}^{-1} \alpha_p^{lead} C_{r,(d-p)} + \gamma_r + \delta_m + \zeta_w + \epsilon_{r,d} \quad (3)$$

With $K_{r,d}$ Google trend index for selected keyword (either *corruzione* or *abuso d'ufficio*) in region r , in day d . $C_{r,(d-p)}$ main explanatory variable, representing the value of the index in region r , in day $d - p$: $C_{r,(d-1)}, \dots, C_{r,(d-14)}$ are lagged variables while $C_{r,(d+14)}, \dots, C_{r,(d+1)}$ are leads, to test whether attention on corruption was already raising before the article comes out. γ_r is region fixed effect, δ_m year/month fixed effect and ζ_w day of the week fixed effect (to account for trends within weeks in attention to corruption - a result is that during the weekend attention to corruption and in general on politics shrinks-). Moreover, robust standard errors are clustered at the region level.

We present the graphical analysis of this regression in Figure 6; we can note three things: first, both keywords, show an attention peak when the article appears in the newspaper lasting from 4 to 2 days, the spike is large but highly temporary. Second, lead 1 is significant (and positive) in both regressions, this can be explained by the fact that while newspaper in day d reports news of day $d - 1$, the web knows information the same day this is released; moreover, there can be anticipation of some news triggering a wave of interest before the story is mentioned in the press. Finally, estimates for the first keyword are more precise than those for the second one and this is due to technical reasons (*i.e.* the first keyword is a Google category, it contains other words representing that concept, while the second it is not). We can conclude saying that the index of corruption exposition seems to be a good proxy to measure the phenomenon and that it is evident that people pay consistent attention to corruption story but that this attention vanishes soon.

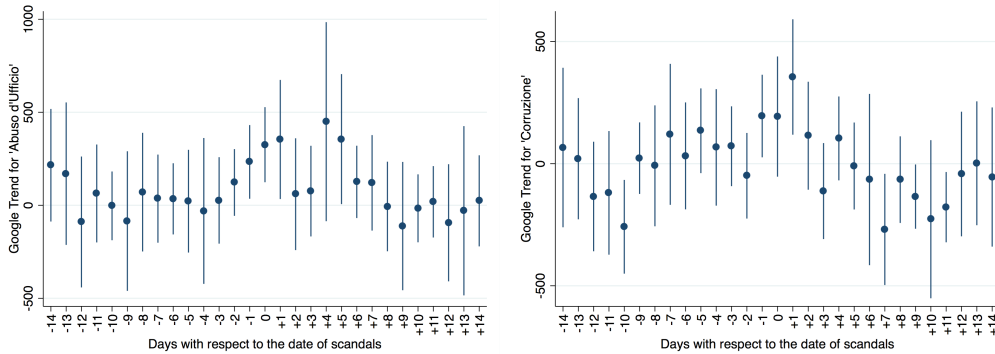


Figure 6: Attention to corruption-Google trend

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