

Pocketbook vs. Sociotropic Corruption Voting*

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Abstract

We theorize and examine the channels by which corruption may affect voting behavior. First, motivated by low empirical correlation between exposure to corruption and perceptions thereof, we postulate two distinct channels: *pocketbook corruption voting*, defined as the effect of personal experiences with corruption on voting behavior; and *sociotropic corruption voting*, defined as the effect of perceptions of corruption in one's society on voting behavior. Second, we argue that the weight the voters place on each channel depends on the salience of each source of corruption. Since importance of bribe victimization to those victimized is inherently high and overall levels of bribe-taking in society are slow-changing, pocketbook corruption voting is expected to be persistent. Conversely, salience of societal corruption depends on the actions of elites, such as corruption scandals, campaigns, or entry of a new anti-corruption party, and so sociotropic voting is expected to be variable. Using data from Slovakia, we find support for our theoretical arguments. In the absence of events that raise salience of sociotropic corruption, we only find pocketbook voting. Sociotropic voting is not activated by scandals alone but it is by the entry of a credible anti-corruption party, which brings about a shift in the media coverage of corruption. Our results suggest that previous studies may have underestimated the effect of corruption on voting by missing pocketbook effects.

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1 Introduction

The political consequences of corruption have been well-documented. Corruption undermines political trust and legitimacy in a variety of institutional settings.¹ It also typically depresses electoral turnout² and reduces electoral support for the incumbent.³ However, little is known about the channels through which corruption may affect citizens' political behavior. In this paper, we seek to further the literature by explicitly positing and investigating potential channels of influence. We focus on the relationship between corruption and vote choice, and introduce a framework for analyzing *corruption voting*. Our argument is two-part. First, we argue that corruption-sensitive voters may potentially respond to the direct impact of corruption in their lives – e.g., by being asked to pay a bribe – or, alternatively, to the perception of corrupt politicians. While we are not the first to investigate either channel, we are the first to our knowledge to posit them explicitly and jointly. Drawing on previous studies on corruption measurement and our own analysis, we hypothesize that these two domains are largely separate – and different – and can plausibly serve as distinct platforms of influence of corruption on voting behavior. For simplicity, we adopt the nomenclature commonly used in the economic voting literature to distinguish between *pocketbook corruption voting* and *sociotropic corruption voting*, respectively.

The second component of our argument relates to the relative prevalence of pocketbook and sociotropic corruption voting. While both channels may be at work simultaneously, we argue that the prevalence of the two forms of corruption voting depends on the importance to the voters of the two aspects of corruption. Salience of corruption experience is likely high for those asked to give a bribe. To the extent that bribe victimization in the society is considerable, we should expect to observe pocketbook corruption voting. Salience of corruption perception at the societal level likely varies, depending on the actions of elites. When it is raised – be it through a political scandal, a national election campaign, the emergence of a new anti-corruption political party, or some other factor – we expect sociotropic corruption voting to increase in prominence. Since bribe extortion is presumably slow-changing, pocketbook corruption voting should exhibit more stability than sociotropic corruption voting, which we expect to wax and wane with the perceived salience of corrupt behavior by politicians.

¹Anderson and Tverdova (2003); Della Porta (2000); Lavalée, Razafindrakoto and Roubaud (2008); Seligson (2002).

²Chong et al. (2011); Davis, Camp and Coleman (2004); McCann and Dominguez (1998).

³For example, Ferraz and Finan (2008); Peters and Welch (1980); Welch and Hibbing (1997), although evidence has been mixed (e.g. Barbera, Fernandez-Vazquez and Rivero, 2013; Golden, 2006).

It is difficult to find reliable data to test our hypotheses. Most studies that ask about both exposure to corruption and perceptions of corruption do not include vote preference questions; most election studies do not ask sufficiently detailed questions about corruption. Also, the second part of our argument has a dynamic component. Thus, even if appropriate survey questions are available in a common source, we require data of relatively high frequency. The two criteria combined exclude almost all available cross-national data; moreover, polling in most countries is done relatively rarely. However, we were able to collect satisfactory data from Slovakia between 2004 and 2011. We utilize a number of individual-level surveys and a large volume of aggregate polls spanning three electoral cycles to test our arguments. As we document below and in the Web Appendix, post-communist Eastern Europe – of which Slovakia is quite representative – offers rich grounds for testing our theory. On the one hand, corruption has had a very prominent role in daily as well as political life in these countries. On the other hand, the region has fully democratic elections, thus allowing for a meaningful study of voting behavior.

We find evidence of both pocketbook and sociotropic corruption voting. Increased personal exposure to corruption and increased perception of the prevalence of corruption in politics drive voters away from the incumbent. Pocketbook voting is present and stable throughout our period of study. We show that this effect of experience is not simply mediated by perception, nor is there an interaction effect between experience and perception. These results further reinforce the finding that corruption experience and perception are largely unrelated. Moreover, we find that the effect of local exposure on the support for the national-level incumbent runs partially through the co-partisanship of the local and central governments: the effect of bribe victimization is stronger in cities whose mayors are from the same coalition as the incumbent government than in cities governed by opposition parties.

For sociotropic corruption voting, we exploit the breakout of several corruption scandals and the entrance of a new party with a strong anti-corruption platform during the 2006-2010 electoral cycle to test our predictions concerning salience. We find that repeated scandals do not by themselves induce the effect of corruption perception on intended vote. However, we show that the entrance of a new party and its subsequent campaign with a pronounced anti-corruption component activate sociotropic corruption voting. We find evidence that this change is related to salience: entrance of a new party is associated with a notable shift in the intensity of the coverage of corruption in the media.

Our results highlight the importance of distinguishing between the two channels. Since the two types of voting may operate separately and simultaneously, previous studies focusing

only on sociotropic or pocketbook corruption voting may have underestimated the overall effect of corruption on voting behavior by ignoring the other component. This is particularly likely in developing countries, where bribe victimization is considerably higher than in wealthy democracies.

2 Theory of Corruption Voting

2.1 Pocketbook Corruption vs. Sociotropic Corruption

For the purpose of this paper, we adopt a conventional definition of corruption: the misuse of public office for private gain. Corruption that directly impacts the lives of individual citizens, therefore, could include being asked to pay a bribe by a police inspector to avoid a ticket for a fictional offense or by a doctor to be seen at what is supposed to be a free public medical clinic. Corruption among politicians could include politicians taking bribes to award public contracts or simply pilfering money from the state directly.⁴

How might the presence of corruption in a society affect voting behavior? Some studies suggest that corruption matters for political preferences by way of corruption perception, whether directly as an attitude,⁵ or when it is reinforced by revelation of hard information.⁶ In addition, many studies posit an effect through perception which is implied rather than directly measured.⁷ Other studies – though fewer in number – claim that corruption matters through personal exposure (“victimization”).⁸

However, little is known about the relationship between these different channels. The effect of corruption on political behavior may run concurrently through perception and exposure. High-level political corruption, which is typically assumed to drive voter perceptions and is more frequently the subject of information campaigns, differs in many ways from the

⁴It is of course possible that different forms of corruption could trigger different reactions at the ballot box. This is an excellent subject for future research, but other than the general distinction between pocketbook and sociotropic corruption that we focus on here explicitly, it is beyond the scope of this paper. Readers in particular should note that we are consciously not addressing “corruption as vote-buying,” which might result in a different dynamic, as in that case individual citizens would receive material benefits (as opposed to costs) from their interaction with corruption. Vote-buying, however, is not particularly common in post-communist countries, and is certainly not the dominant form of corruption that citizens in these countries would either encounter or of which they would be aware.

⁵Anderson and Tverdova (2003); Davis, Camp and Coleman (2004); Della Porta (2000); Krause and Mendez (2009); McCann and Dominguez (1998).

⁶Chong et al. (2011); Ferraz and Finan (2008); Humphreys and Weinstein (2007); Banerjee et al. (2009).

⁷Alford et al. (1994); Chang, Golden and Hill (2010); Dimock and Jacobson (1995); Peters and Welch (1980); Welch and Hibbing (1997).

⁸Gingerich (2009); Lavalée, Razafindrakoto and Roubaud (2008); Seligson (2002).

types of corruption – such as the solicitation of bribes – that voters may be exposed to in their daily lives. Voters therefore may be reacting differently – and separately – to these two aspects of corruption. Moreover, we do not know the relative prevalence and importance of each channel, what conditions may favor one channel over another, and whether they have different implications for electoral outcomes.

In this paper, we aim to further the literature on the consequences of corruption by theorizing about the underpinnings of these two potential channels of influence for corruption on vote choice. We focus on the relationship between both channels and the vote because corruption is politically consequential and because it has been understudied compared to other forms of performance voting, particularly economic voting. This is particularly important in developing democracies, where corruption is especially high. Our argument is two-fold. First, we posit that voters may potentially respond to the direct impact of corruption in their lives (or those in the proximate environment, such as family) or, alternatively, to corruption as a perceived problem in society at large. Studies before us have examined the effects of each form of influence, but only separately, and without explicitly positing the nature of the influence. To the extent that the two mechanisms may be substitutes, previous studies may potentially have misattributed the effect of one mechanism to the other. To the extent that the two mechanisms are complements, previous studies may have underestimated the influence of corruption on voting behavior.

To distinguish between the two channels, for convenience we adopt the nomenclature used in the economic voting literature. A wealth of evidence links economic performance and election outcomes.⁹ One facet of this literature centers on whether voters are more likely to respond to their own personal (“pocketbook”) economic circumstances, or are guided by perceptions of the state of the wider (“sociotropic”) economic context.¹⁰ Drawing the parallel with the economic vote, we term the vote choice influenced by personal exposure to corruption *pocketbook corruption voting*, and vote choice influenced by perception of corruption in the society as *sociotropic corruption voting*.

While we adopt the nomenclature, we want to be clear that we are not adopting identical concepts. Instead, our conceptualization of pocketbook and sociotropic corruption voting as separate processes is motivated by the findings from studies on the methodology of corruption measurement. Studies have repeatedly shown that the relationship between personal

⁹Classic studies and comprehensive reviews include, for example, [Kramer \(1971\)](#); [Fiorina \(1981\)](#); [Hibbs, Rivers and Vasilatos \(1982\)](#); [Powell and Whitten \(1993\)](#); [Lewis-Beck and Paldam \(2000\)](#). For evidence in the post-communist context, see for example [Tucker \(2006\)](#).

¹⁰For example, [Kinder and Kiewiet \(1981\)](#).

experience with corruption and corruption perception is quite tenuous. Personal experience predicts perception of administrative corruption and “grand” corruption equally well – or rather, badly – even though survey respondents are much more likely to have experience with the former than the latter.¹¹ The direction of change in perception does not relate systematically to the direction of change in reported exposure.¹² Respondents’ attitudes towards bribery do not relate to perceptions of corruption, even though they are related to exposure to corruption.¹³ Corruption perception is more strongly correlated with other general perceptions and government evaluations, such as those of government’s record on human rights or fairness of public services, as well as with certain country characteristics, such as per capita GDP.¹⁴ This is perhaps not so surprising: exposure typically relates to petty bureaucratic corruption, while perceptions are more likely to be directed towards corrupt politicians.

Our estimates for the context we study in this manuscript – post-communist Eastern European countries – conform with these findings. We estimate partial correlation coefficients between reported perceptions and experiences for a number of public domains (politicians at different levels of government, judiciary, police, health and education), taking into account demographic, socio-economic and geographic characteristics of respondents and country fixed effects.¹⁵ The estimated correlations are rather low (see Table 1). While the sign of the coefficients accords with our priors of positive reinforcement – that increased exposure is associated with increased perception – none of the coefficients exceeds 0.2. In particular, the correlation between overall exposure to bribes and corruption perception of politicians, which are measures we rely on in the analysis in the following sections, is nearly zero.¹⁶ This is particularly striking considering that there is likely some priming in our data, as survey questions probing experiences and perceptions are invariably clustered together and asked consecutively.¹⁷ Conceptualizing the two forms of corruption voting as separate therefore

¹¹Abramo (2007); Donchev and Ujhelyi (2009).

¹²Krastev and Ganev (2004).

¹³Rose and Mishler (2007); Mocan (2004); Morris (2008).

¹⁴Abramo (2007); Donchev and Ujhelyi (2009); Rose and Mishler (2007).

¹⁵As discussed in more detail below and in the Web Appendix, the correlation we are interested in is the one that is consequential for vote choice. Rather than focusing on raw correlations, we therefore look at the correlation conditional on common demographic, socio-economic and geographic correlates of political behavior. Moreover, because our data represent cross-national samples of individuals, we also condition on country fixed effects for reasons of comparability. Still, raw correlations are also quite low, although typically somewhat higher than partial correlations presented here.

¹⁶For Western Europe, all correlations are even lower, and typically not different from zero. Results are available upon request.

¹⁷We could attempt to estimate the magnitude of priming if we had variation in the order in which the

appears to be meaningful. Below, we find further support for this assumption by observing little change in the estimates of the sociotropic channel after omitting the measure of the pocketbook channel, and by finding virtually no interaction effects between experience and perception.¹⁸

2.2 Salience

Assuming that pocketbook and sociotropic mechanisms are largely distinct phenomena, we are further interested in the conditions that determine the relative weight the voter may place on each form of corruption voting.¹⁹ Our argument is informed by the literature on the role of issue salience in evaluations of politicians' performance and policy output. Researchers have shown that greater exposure to an issue increases its weight when evaluating politicians.²⁰ For example, the increase in the salience of European integration in Britain in the 1990s considerably strengthened its importance for vote choice among British voters between the 1992 and 1997 elections.²¹

Being asked to pay a bribe imposes a direct cost on the citizen, and provides a highly precise signal about this aspect of performance of the political system. Personal experience with corruption may therefore increase the importance of corruption in voting behavior. To the extent that regularized contacts with public officials are characterized by a sufficiently high likelihood of bribe victimization, we may expect to see pocketbook corruption voting.²² If pocketbook corruption voting is prominent, we may also expect it to be stable in the short run, since bribe-extortion is decentralized and difficult to monitor and change in the short

key questions eliciting reports of exposure and perceptions were asked. Unfortunately, in all our data the questions were asked in the same order: perception item(s) followed by experience item(s).

¹⁸In this respect, the nomenclature of pocketbook and sociotropic concerns in the voting calculus is perhaps more suitable for the context of corruption voting than that of economic voting. Several studies have questioned the utility of pitting one mechanism against the other in the economic voting context, arguing that differences in personal economic experiences and personal environment affect how voters perceive the general state of the economy, and how and what information they use to form political attitudes (e.g. Killian, Schoen and Dusso, 2008; Mutz and Mondak, 1997). While more evidence is needed for corruption voting, it appears that a similar critique is less warranted.

¹⁹Another logical question is that of the effects of heterogeneities at the individual level. We do not focus on these types of interactions here and leave them for future research.

²⁰For example, Iyengar and Kinder (1987); Iyengar (1990); Krosnick (1988); Krosnick and Brannon (1993).

²¹For example, Evans (1999).

²²Elsewhere, we demonstrate that in Sweden, a low corruption country, pocketbook corruption voting is indeed lower than sociotropic corruption voting, thus adding additional justification for conditioning our prediction on being in a country with a sufficiently high likelihood of bribe victimization (self-citation omitted).

Table 1: Partial correlations between corruption experience and perception in Eastern Europe

	Eurobarometer	Transparency International
Politicians and any bribe experience	0.02 (0.00, 0.04)	0.06 (0.04, 0.08)
National politicians	-0.01 (-0.03, 0.01)	.
Regional politicians	0.02 (-0.00, 0.04)	.
Local politicians	0.04 (0.02, 0.06)	.
Police	0.08 (0.06, 0.10)	0.09 (0.07, 0.11)
Judiciary	0.03 (0.01, 0.05)	0.06 (0.04, 0.08)
Health	0.16 (0.14, 0.18)	0.18 (0.16, 0.20)
Education	0.10 (0.08, 0.12)	0.10 (0.08, 0.12)

Note: The main entries represent the Pearson correlation coefficients between the residuals of the linear regression model of the reported bribe experience and perception on a set of demographic, socio-economic, and geographic covariates, and country fixed effects. All regressions are weighted by respondent-level weights, recalculated to the restricted sample of post-communist Eastern European countries. The Eurobarometer data consist of the surveys 64.3, 68.2, and 72.2. The Transparency International (TI) data consist of the Global Corruption Barometer (GCB) surveys in 2003-2007, 2009, and 2010. The estimates from different data differ partly because of the different question wording and different country samples. The entries in the parentheses represent the 95 percent confidence intervals. The first row presents the partial correlation between bribe experience with any of the sectors examined in the Eurobarometer and Transparency International data and the perception of corruption among national-level politicians and political parties, respectively. The remaining rows give the estimates of the partial correlation between bribe experience with and perception of corruption in the specified sector. All corruption variables in the Eurobarometer data are binary. Bribe variables in the GCB data are also binary, and perception variables are ordered-categorical. Results obtained using limited a logit model are qualitatively similar and are available upon request.

term.²³ In line with the established wisdom, we expect that increased personal exposure to corruption will on average drive voters away from the incumbent.

We acknowledge that although this logic of the salience of personal experience for political preferences can potentially be extended to many other personal experiences, the existing

²³Anderson and Gray (2006).

literature offers mixed results. Evidence on pocketbook economic voting is considerably weaker than evidence on sociotropic economic voting.²⁴ On the other hand, experiences such as being a victim of a crime,²⁵ being eligible for a military draft,²⁶ or living through extreme weather conditions²⁷ have effects on political preferences. We do not seek to resolve the issue of mixed evidence here. Corruption experience has been previously shown to matter for political evaluations (see footnote X above). Perhaps experiences where the individual has little or no control, such as acts of bribe or crime victimization or extreme weather patterns, have a more well-defined effect than experiences where the individual may have more control and thus ascribe personal responsibility, such as changes in income or unemployment spells.

Extending the insights of the priming literature to sociotropic voting is straightforward. In line with the argument that the salience of the issue determines the weight it plays in voting behavior, we expect that the prevalence of sociotropic corruption voting depend on the perceived salience of societal corruption. Several developments likely increase the salience of societal corruption. First, we would expect public corruption scandals to increase the salience of corruption for self-evident reasons. Second, election campaigns may increase the salience of corruption in countries where corruption is a non-trivial issue because opposition parties may have a political incentive to raise the issue of corruption as a means of winning votes away from incumbent parties. Note, however, that since the societal level of corruption is fundamentally unobservable, salience may depend on the strength or credibility of the signal. Corruption revelations vary in seriousness and verifiability. Also, the politicization of corruption may trivialize scandal material, potentially resulting in “scandal fatigue.”²⁸ Similarly, raising corruption in an election campaign may be a less effective tactic when opposition parties have previously been in office themselves and accused of corruption, a far too often occurrence in post-communist Eastern Europe.²⁹ Thus, we also expect that the emergence of new, anti-corruption parties ought to increase the salience of corruption as a political issue. Finally, the last two factors may have an interactive effect: new anti-corruption parties contesting an electoral campaign or emerging in the wake of a scandal may have a particularly strong effect on raising the salience of corruption as a political issue.

²⁴For example, [Kinder and Kiewiet \(1981\)](#); [Lewis-Beck \(1988\)](#); [Colton \(1996\)](#); [Duch \(2001\)](#). However, some studies do find evidence for pocketbook economic voting (e.g. [Nannestad and Paldam, 1997](#); [Palmer and Whitten, 1999](#)).

²⁵[Marschall \(2004\)](#); [Sears et al. \(1980\)](#).

²⁶[Erikson and Stoker \(2011\)](#).

²⁷[Egan and Mullin \(2012\)](#).

²⁸[Kumlin and Esaiasson \(2012\)](#).

²⁹[Krastev and Ganev \(2004\)](#).

Therefore, when the salience of corruption is high (e.g., corruption scandals, during an election campaign where parties are highlighting problems with corruption, following the emergence of anti-corruption political parties), we expect to find evidence of sociotropic corruption voting. However, since salience varies (at least according to the electoral cycle), we expect sociotropic corruption voting to be volatile. Again, our expectation is that increased perception of corruption will on average decrease support for the incumbent.

3 Focus and Data

It is difficult to find reliable data to test our hypotheses. As mentioned above, most available data fail on one of two accounts: they do not contain all essential variables, or are not available in sufficient frequency.³⁰ Fortunately, we found an exception to this rule across a number of surveys that took place in Slovakia. Slovakia is an appealing initial case on which to test our theoretical arguments. Numerous scholars of post-communist politics in Eastern Europe have noted the potential role of political corruption in chronic anti-incumbency bias.³¹ Over the course of the transition from communism, corruption has emerged as one of the most pressing developmental and political issues.

A casual look at the data supports this notion. An examination of the election summaries in *Electoral Studies* reveals that of the forty elections in the region between 2000 and 2010, in 28, or 70 percent, corruption was a major issue, by way of allegations, scandals, and revelations of serious wrongdoings.³² Part of this “apparent epidemic” can be explained by the low level of economic development and limited experience with democracy.³³ However, despite the many developments of the past twenty years – including rapid economic development, the entrenchment of democratic government, and even accession to the European Union for

³⁰Consider a few examples. Transparency International’s Global Corruption Barometer surveys cover many countries almost every year since 2003 and include a rich battery of corruption questions, but not political behavior items. The Comparative Studies of Electoral Systems (CSES) data feature a rich set of political preference items, but contain only one item on corruption that elicits perceptions, but not experience; moreover, modules in any one country are relatively infrequent. The International Social Survey Programme (ISSP) contains satisfactory corruption items in addition to political variables, but only in a single cross-section conducted as part of the 2006 Role of Government IV module.

³¹See for example [Karklins \(2005\)](#); [Krastev \(2004\)](#); [Pop-Eleches \(2010\)](#); [Roberts \(2008\)](#); [Tavits \(2007\)](#). In one of the few direct tests of the relationship between corruption and voting in a post-communist country, ([Slomczynski and Shabad, 2011](#)) show that perceiving a party to be corrupt made voters in Poland less likely to vote for that party.

³²For the whole period since the collapse of communism, corruption features prominently in 38 out of 66 summaries (58 percent) appearing in the journal. Early elections, however, were dominated by other issues, most notably economic decline, reforms, and nationhood.

³³[Treisman \(2003\)](#).

many countries – concern with corruption still seems much more pronounced among East European citizens than those of Western Europe. In the Web Appendix, we show that a typical East European citizen is significantly more likely to perceive their politicians as corrupt, to report having been asked for a bribe, and reports spending significantly more of their income on bribes than a typical citizen in Western Europe.

Post-communist Eastern Europe therefore offers rich grounds for empirical tests of corruption voting. And Slovakia is quite representative of the region. While it is slightly smaller than the average country in the region and somewhat richer, it does not stand at a regional extreme on any major demographic or economic index. Like all of the EU accession countries, it has a functioning democracy with relatively high levels of civil liberties, and like most of them it has a parliamentary form of government using a proportional electoral system with a five percent threshold. Most importantly, Slovakia is also relatively typical in terms of its corruption experience and perception. We show in the Web Appendix that Slovakia is close to the median for the region with respect to citizens’ bribe victimization and citizens’ perception of corruption.

We utilize nine cross-sectional individual-level surveys in the period between May 2004 and December 2011.³⁴ We mainly focus on the 2006-2010 electoral cycle, within which we have six surveys, but we also use the three remaining surveys to cross-validate some of our results.³⁵ Five of the nine surveys contain all crucial variables of interest simultaneously – items probing corruption perception and experience, as well as vote choice and important control variables. The remaining surveys contain questions probing corruption perception, vote choice and controls, but not corruption exposure. We use these surveys to test our expectations about sociotropic corruption voting, while adjusting the estimates for the omission of a measure of corruption exposure.

To supplement individual-level data, we collected 116 aggregate polls of Slovak public opinion polls conducted by four Slovak polling firms in the period between July 2006 and July 2010. These polls, conducted almost every month, contain aggregate estimates of intended vote choice, which we use to construct the monthly vote share of the incumbent coalition and the senior incumbent party. We use these time series to examine the temporal patterns of sociotropic corruption voting and cross-validate results from individual-level surveys.

Finally, we collect original data on the coverage of corruption by the Slovak media to

³⁴Ideally, we would rely on panel data, but they are unfortunately simply not existent.

³⁵We use several other individual-level surveys, which do not contain corruption and vote choice items jointly, for an additional set of robustness checks discussed below and in the Web Appendix. The details about these surveys are given in the Web Appendix.

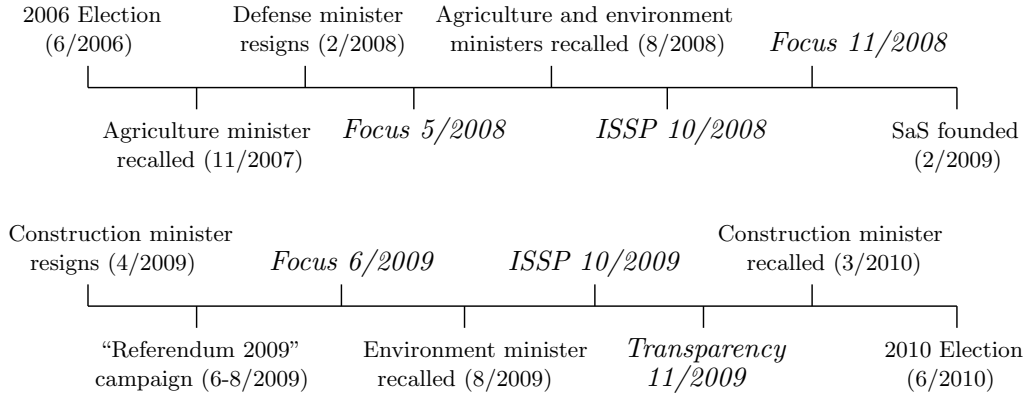
examine the variation in the salience of societal corruption over time. We searched media sources covering Slovak politics on Factiva and Lexis-Nexis between July 2006 and June 2010. To construct the measure of media coverage of corruption, we divided the monthly count of articles about corruption with the monthly count of articles that contain a reference to the Slovak Government or the Prime Minister Robert Fico. To identify articles about corruption scandals, we search by the name of the minister involved in the scandal, as well as the appropriate key word that most likely identifies the scandal. For articles about corruption unrelated to the scandals, we search for the mention of one of the standard words in Slovak language denoting corruption, but without the mention of any of the names or institutional acronyms we used in the search for the scandal-related coverage. More details on the media sources and our search strategy are given in the Web Appendix. The Web Appendix also contains details on the individual and aggregate level surveys.

Aside from data availability and the general prominence of corruption in Eastern Europe, Slovakia provides fertile ground for testing our hypotheses for four other reasons. First, the 2006-2010 electoral cycle was marked by several relatively high-profile corruption scandals within the governing coalition. The minister of defense (February 2008), two ministers of agriculture (November 2007 and August 2008), two ministers of construction and regional development (April 2009 and March 2010), and two ministers of environment (August 2008 and August 2009) were recalled or resigned due to various allegations of improper financial conduct.³⁶ All of the scandals were of a financial nature, and thus directly linked to the issue of corruption. Second, a new party, Freedom and Solidarity (SaS), led by a technocratic leader, entered the political scene in February 2009, and in the summer of the same year launched a campaign for a referendum with a pronounced anti-corruption agenda.³⁷ The scandals and the emergence of a new anti-corruption party provide good conditions to test our predictions about sociotropic corruption voting. Third, focusing largely on one electoral cycle allows us to control for unobservable factors related to changes in the composition of the government. Finally, by focusing on one country, we are also able to avoid having to deal with unobservable factors, such as culture or history, at the country level. Figure 1 shows a timeline of relevant events and individual-level survey dates during the period representing the main focus of our study.

³⁶Only one of the cases involved a minister nominated by the senior coalition party, Smer; other scandals involved ministers nominated by the two junior coalition partners, LS-HZDS and SNS. More details about each case are given in the Web Appendix.

³⁷The anti-corruption campaign partly coincided with the election for the European Parliament conducted in June 2009. The initiative was called “Referendum 2009.” More details are given in the Web Appendix.

Figure 1: Data and timeline of events in Slovakia



Surveys are indicated in italic. Details about the events and the variables available in each survey are given in the Web Appendix.

4 Results

We begin by presenting results based on individual-level survey data. In the surveys, respondents are asked whether they would vote, and if so, for which party, if the election was held in the week following the survey (“the Sunday question”). We first focus on whether the respondent i intends to vote for *any* incumbent party ($\text{Vote}_i = 1$) or not ($\text{Vote}_i = 0$). Further below, we disaggregate the intended vote choice results by party. The binary vote is modeled with a standard logit model:

$$\Pr(\text{Vote}_i = 1) = \text{Logit}^{-1} \left(\beta_1 \text{Exposure}_i + \beta_2 \text{Perception}_i + \sum_k \gamma_k \mathbf{X}_{i,k} + \epsilon_i \right) \quad (1)$$

Key variables are measures of corruption exposure and corruption perception. For exposure, respondents were asked whether they or someone from their immediate family had to give a bribe in some previous period of time.³⁸ In four of the five surveys containing an exposure item, the response was binary, with one denoting that a respondent has given a bribe and zero otherwise. We recode the variable in the remaining survey into a binary response as well.³⁹ For perception, respondents were asked several questions about the im-

³⁸The length of the period ranges between a year and five years. We show below and in the Web Appendix that our results are insensitive to this difference.

³⁹The exposure question in the 2008 ISSP survey contains five response categories, ranging from “never” giving a bribe to “very often” in the previous period. Respondents answering “often” and “very often” are

portance of corruption as a national problem and how widespread they thought corruption was among politicians, and sometimes among public officials (police, education, customs, etc.). The precise wording and subject of corruption-probing questions differ between surveys, particularly for the perceptual questions. To make the results as comparable across the surveys as possible, we build a composite sociotropic evaluation by applying factor analysis on all relevant perceptual questions available in each survey, and then standardizing the scale by its mean and standard deviation.⁴⁰ There are two added benefits from this strategy. First, a composite measure reduces measurement error compared to a single variable.⁴¹ Second, this composite measure likely reduces the concerns about endogeneity of perceptions to partisanship. We discuss this point in more detail further below.

\mathbf{X} is vector of control variables. Where available, we include the report of the vote choice in the last actual election in 2006. Previous vote choice, also coded as a binary variable, is a powerful control, as it should essentially subsume the effect of all time-invariant determinants of vote, such as demographic, geographic, and probably any slow-changing socio-economic characteristics (such as income or social class) of the respondents.⁴² Indeed, in the Web Appendix, we show that saturating the specification containing previous vote choice with these other standard predictors adds very little, if any, explanatory power. Previous vote choice should also include the effect of previous *levels* of corruption exposure, perception, and other changing attitudinal characteristics that determine vote choice. Our preferred specification therefore contains relatively few variables. However, not all surveys contain previous vote choice, in which case we saturate the model with (weakly) exogenous variables: demographic, socio-economic and geographical variables, as well as voter's positions on various policy issues.⁴³

given the value of one, and zero otherwise. The marginal distribution of such a variable is very similar to the distribution of the binary variable in the remaining surveys.

⁴⁰The first factor was strong in each survey, suggesting a common dimension. This is unsurprising, given that the items tap into similar domains of corruption perceptions. The results are available upon request.

⁴¹Like most public opinion surveys, our data contain missing values due to item non-response. While missingness in any one variable is relatively minor, we multiply impute data to avoid efficiency losses and potential bias of listwise deletion. More details are given in the Web Appendix.

⁴²Including previous vote choice essentially amounts to including a lagged dependent variable. This presents a somewhat unusual mix of a binary time-series cross-sectional and ordinary cross-sectional application. Logit is consistent in the case of potential serial correlation (Poirier and Ruud, 1988), which may have been induced if we had a full panel structure. Huber's (1967) robust standard errors are a reasonable correction for the variance-covariance matrix in this case (Beck, Katz and Tucker, 1998), and we use them throughout.

⁴³Unfortunately, our data do not contain items probing economic and other performance evaluations and we are unable to include measures thereof, but we provide evidence with aggregate data, where we are able to control for economic factors, that our results are qualitatively similar to our individual-level results.

Since the model in equation 1 is non-linear, we present marginal effects of a meaningful change in the key variable when holding the other variables at their mean. The full results with coefficient estimates for each survey are given in the Web Appendix. We present the results for pocketbook and sociotropic corruption voting separately, but stress that our specifications typically include both channels – where allowed by the availability of data.

4.1 Pocketbook Results

We first focus on our estimates of pocketbook corruption voting, while controlling for the effect of perceptions, and the remaining covariates. Only two of our surveys during the 2006-2010 electoral cycle, in October 2008 and November 2009, contain corruption exposure items. Results from these two surveys are given in the first two rows of Table 2. The first column shows the marginal effect on incumbent vote probability of the change from reporting not having given a bribe to having given a bribe in the previous period for a typical respondent. The second column gives the robust standard errors, the third column shows the p -value on the one-tailed test of the hypothesis in line with previous research that increased corruption exposure reduces support for the incumbent. The fourth column shows the p -value on a two-tailed test of the hypothesis that the effect of bribe victimization is different from zero in either positive or negative direction.

Table 2: Marginal effect of corruption experience

	Marginal effect	Standard error	One-tailed p -value	Two-tailed p -value
October 2008 ISSP	-0.130	0.066	0.024	0.048
November 2009 TI	-0.087	0.046	0.028	0.056
May 2004 TI	-0.078	0.053	0.072	0.143
March 2006 TI	-0.070	0.029	0.009	0.018
December 2011 TI	-0.031	0.038	0.201	0.416

Note: The dependent variable is the incumbent vote choice. The results are based on the model in equation 1. The entries in the first column represent the marginal effect on incumbent vote probability of the change from reporting not having given a bribe to having given a bribe in the previous period when all other variables in the model are fixed at the mean. The full results are given in the Web Appendix. Robust standard errors are given in the second column.

The results in the first two rows suggest that *ceteris paribus* experience of giving a bribe decreases the probability of voting for the incumbent by around 13 percentage points based

on the October 2008 data and around nine percentage points based on the November 2009 data. Both results are statistically significant at conventional levels. The estimated effects are not of trivial size. Around 58 and 51 percent of respondents report that they intend to vote for the incumbent coalition in October 2008 and November 2009, respectively.⁴⁴ Thus, the estimated effect of bribe victimization represents an approximately 22 and 17 percent decrease in the incumbent vote probability in October 2008 and November 2009, respectively.⁴⁵

Evidence from October 2008 and November 2009 suggests that pocketbook corruption voting exists. However, evidence from only two surveys is insufficient to address our hypothesis about its stability over time. As explained above, because bribe-taking is slow-changing we expect that any evidence of pocketbook corruption voting be persistent. Indeed, reported bribe victimization is stable over time. The five surveys between May 2004 and December 2011, as well as three Eurobarometer surveys that contain an exposure item (but not the vote choice variable),⁴⁶ indicate that between 22 and 30 percent of respondents reported giving bribes. We use the three remaining surveys outside of the 2006-2010 cycle that contain the exposure question to examine the persistence of pocketbook corruption voting.⁴⁷ Rows 3-5 in Table 2 display the results. All three specifications control for corruption perception and other relevant predictors of vote choice (see the Web Appendix for details). The results largely confirm that pocketbook corruption voting is persistent. The marginal effects based on the May 2004 and March 2006 data are of similar size to the estimates from the 2006-2010 electoral cycle: a reduction of approximately 8 percentage points (25 percent) and 7 percentage points (30 percent) in the probability of voting for the incumbent government. The marginal effect in December 2011 is smaller, and imprecisely estimated, but still negative. One potential reason why the pocketbook channel is weaker and less precise in 2004 and 2011 is that the exposure question extends into the previous electoral cycle, necessarily inducing measurement error. The measurement error is larger in 2011 because the party composition of the government changed entirely, whereas in 2004 this was not the case.⁴⁸

⁴⁴Incumbent vote intention and previous vote for incumbent are likely subject to over-report. We show in the Web Appendix that the results are largely insensitive to this concern.

⁴⁵Bribe victimization also reduces the probability of turning out in October 2008 (not shown, available upon request), thus already removing a share of voters with high corruption exposure from the electorate. Had these voters expressed a voting preference, the negative effect on the incumbent vote might have been even higher.

⁴⁶Eurobarometers 64.3 (November 2007), 68.2 (May 2008), and 72.2 (September 2009).

⁴⁷While it would be better if we had more data within the 2006-2010 cycle to avoid any unobservable factors related to changes in the governing coalition, these data do not exist. On the other hand, surveys outside of the 2006-2010 cycle provide an opportunity to cross-validate our results from the first two rows.

⁴⁸Another important concern stems from the fact that the period covered by the questions somewhat

4.1.1 Robustness and Potential Mechanisms

Exposure to corruption is not random. The most serious concern is the potential endogeneity to partisan affiliation: our results presented above are consistent with an alternative argument that individuals who are less likely to vote for the incumbent parties are more likely to be victimized *because of their partisan affiliation*. We believe that this is unlikely because the ballot is undeniably secret in Slovakia⁴⁹ and clientelism is not prominent.⁵⁰ Nevertheless, we provide several robustness checks.

First, although the ballots are secret, it may be that the government would victimize individuals more frequently in areas where it previously received or expects lower support, and less frequently where it is popular. To account for this possibility, we include in our specifications regional and municipal fixed effects, thus looking at the effect of bribe exposure only within these territorial units. Our results are virtually unchanged. Second, we estimate whether incumbent support in the previous election is a strong predictor of reported bribe exposure, since this information (rather than vote intentions) is what is possibly available to public officials when trying to decide whether to request a bribe. We run a model of bribe victimization as a function of previous incumbent vote, and a set of demographic, socio-economic and geographic covariates.⁵¹ While the coefficient on previous incumbent vote is negative, it is statistically significantly different from zero only at $p < 0.26$. Part of this correlation, however, derives from a strong relationship between previous and intended vote choice, which in turn – as shown above – is strongly related to bribe victimization. When we control for voting intention, significance for previous incumbent vote is attained at only $p < 0.70$. Third, in our other work in several other countries, where we used survey experiments approximating the corruption experience studied here but where endogeneity was removed by construction, we still get statistically significant pocketbook corruption effects.⁵²

Even if our results are robust to some important concerns, the question remains about the mechanism behind our finding. Why are voters who are typically asked for a bribe by a

overlaps in all our surveys. We may be estimating the *same* pocketbook effect rather than the stability of separate pocketbook effects over time. In the Web Appendix, we report robustness checks showing that our results are insensitive to this issue.

⁴⁹OSCE/ODIHR (2010).

⁵⁰Kitschelt and Kselman (2011).

⁵¹We are able to do this only for 2008, where we have information about previous vote choice.

⁵²A related endogeneity concern is that individuals who are less likely to vote for the incumbent parties may be more likely to report having been asked for a bribe even when they had not. The survey experiments in our other work remove this concern as well and give strong evidence of pocketbook effects.

local public official inclined to punish a national-level incumbent? It may be that corruption exposure acts as a signal about how corrupt the government is. In that case, despite the low correlations we report between corruption experience and corruption perception in section 2.1, the effect of personal experience would be mediated by perception. This would invalidate our distinction between pocketbook and sociotropic corruption voting as largely separate channels. To check for this possibility, we modify our specifications by adding available measures that reflect perceptions related to corruption experience: perception of prevalence of bribe giving (2004, 2006, 2009 and 2011), or trust in civil servants and a measure of how fairly the respondents believe they were treated by public officials (2008).⁵³ If the effect of exposure runs through exposure-elicited perception, inclusion of these measures should weaken or eliminate the pocketbook effects presented in Table 2. However, we do not find such evidence: the pocketbook effects based on all five surveys are unchanged.⁵⁴ This suggests that our channel runs through personal experience and further supports our claim that perception and experience are weakly related. Another way to check for the correlation between experience and perception is to examine whether there are any interaction effects between the two channels. Intuitively, respondents with higher perception of corruption may be more willing to sanction any corruption experience; and vice versa, respondents with greater exposure to corruption may be more likely to exhibit sociotropic corruption voting. However, we do not find any such effects, as the inclusion of the interaction term does not change the marginal effects of either channel noticeably.⁵⁵ This is again in line with the low correlation between the two channels we presented above in Table 1.

Instead, we examine whether the connection between the local authorities under which a respondent is victimized and the national government affects the strength of the pocketbook channel. Intuitively, we expect that respondents who were victimized in a town run by the government from the same parties as the national government should be more likely to punish the national incumbent than if they had to pay a bribe in a town run by the opposition. We

⁵³When these variables were included in the perception factor scales (2004, 2006, 2009 and 2011), we replace the composite perception measure with the variable measuring corruption perception among national-level politicians to avoid collinearity problems.

⁵⁴We run a number of different specifications: including both the exposure measure and the perceptual measure simultaneously, including one at the time, and excluding the sociotropic measure so as to avoid potential multicollinearity. None of the combinations change our estimate of the pocketbook effect. Moreover, when we exclude the pocketbook measure to allow the effect of experience to run only through exposure-elicited perceptions, we do not find any pocketbook perceptual effects in four out of five cases, and in the fifth case, it is of the opposite sign. All results are available upon request.

⁵⁵Results available upon request. We stress that we evaluate the evidence on the interaction effects not by the significance of the interaction term, but by recalculating the marginal effects for corruption experience and perception upon incorporating the interaction term.

call this tendency co-partisan pocketbook corruption voting. This can be simply because the performance evaluation of the local government translates into the performance evaluation of the national government. In the context of corruption, similar behavior has been shown in Bolivia, where protests against the president were more likely to erupt where bribe victimization took place under perpetrators linked to the ruling government through patronage networks than where such links were not present.⁵⁶ In the context of economic voting, it has recently been argued that voters in the United States incorporate local economic information to form performance evaluations of the president.⁵⁷

Table 3 shows evidence in favor of co-partisan pocketbook voting. We combine the October 2008 survey, where we have information on respondents' place of residence, with the data on the party affiliation of town mayors in order to code towns run by co-partisan and opposition parties. The first two rows of the first column give evidence that the co-partisan pocketbook effect exists. Pocketbook corruption voting is more than twice as strong in towns run by a mayor from any of the parties constituting the national incumbent government (the first row) than in opposition towns (the second row). The results in the second column represent a form of a placebo test. If the mechanism indeed runs through bribe victimization under a co-partisan government, and if corruption perception and experience are indeed largely unrelated, we should *not* see a different pattern in voting based on *societal* perception under co-partisan and opposition governments. This is what we find: the sociotropic effect is similar (and insignificant – more on this below) across the two sets of towns. Moreover, the partial correlation between corruption experience and corruption perception of *national* politicians, calculated in the same way as in Table 1, is very similar – and low – in co-partisan and opposition towns: 0.145 and 0.163, respectively. That once again suggests that corruption experience and perception are largely distinct, as voters with bribe exposure in co-partisan towns do *not* seem to channel their exposure through an updated perception of the national incumbent.

In rows 3-4, we show that much of the co-partisan pocketbook effect is drawn by the senior incumbent party, “Smer.” The evidence is consistent with the hypothesis that a more prominent member of the government will be punished more strongly for bad performance than junior coalition members.⁵⁸ We find more evidence for this hypothesis when we turn to aggregate data and to individual party choice further below.

⁵⁶Gingerich (2009).

⁵⁷Ansolabehere, Meredith and Snowberg (2012).

⁵⁸Tucker (2006).

Table 3: Co-partisanship of local and central government and pocketbook corruption voting

	Pocketbook Effect	Sociotropic Effect
Mayor from incumbent party	-0.221* (0.133)	-0.013 (0.055)
Mayor from an opposition party	-0.083 (0.077)	-0.017 (0.035)
Mayor from senior incumbent party (Smer)	-0.251** (0.126)	-0.002 (0.060)
Mayor from any other party	-0.100* (0.054)	0.017 (0.030)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Note: The dependent variable is the intended vote for the national-level incumbent government (first two rows) or the senior incumbent party Smer in the bottom two rows. The model consists of variables for corruption experience, corruption perception, reported vote choice in the previous election, party of the mayor in the respondent’s municipality, and the interactions between party of the mayor and corruption experience and corruption perception. The main entries in the table are the marginal effects of a change from not giving a bribe to giving a bribe, or a one-standard deviation increase in corruption perception, while holding other covariates fixed at their mean.

4.2 Sociotropic Results

Next we turn to the sociotropic results. To reiterate, we expect the effect of corruption perception to vary depending on the salience of societal corruption. Salience may be raised by electoral campaigns, the emergence of new anti-corruption parties or movements, or by contextual factors such as corruption scandals. When salience is high (low), we expect (not) to find statistical evidence of corruption perception on vote choice.

The first column of Table 4 gives the marginal effects on incumbent vote probability of a one-standard deviation increase in corruption perception on a standardized scale by a typical respondent. The results are from the six surveys during the 2006-2010 electoral cycle. The remaining columns give the same quantities as in Table 2.

The results suggest that sociotropic corruption voting was not present in 2008, as the estimates of the marginal effects based on the May, October and November 2008 surveys are both substantively and statistically very close to zero. In 2009, however, the sociotropic voting seems to be prominent. The marginal effect of a one-standard-deviation increase in the perception of societal corruption is associated with a drop in the probability of incumbent vote of between 6 and 8 percentage points, or between 12 and 14 percent. The results based

Table 4: Marginal effect of corruption perception

	Marginal effect	Standard error	One-tailed p -value	Two-tailed p -value
May 2008 Focus	-0.017	0.021	0.205	0.411
October 2008 ISSP	0.000	0.026	0.500	1.000
November 2008 Focus	-0.008	0.024	0.374	0.747
June 2009 Focus	-0.081	0.030	0.004	0.008
October 2009 ISSP	-0.063	0.028	0.011	0.022
November 2009 TI	-0.061	0.020	0.001	0.002

Note: The dependent variable is the incumbent vote choice. The results are based on the model in equation 1. The entries in the first column represent the marginal effect on incumbent vote probability of a one-standard deviation increase in corruption perception on a standardized scale while all other variables in the model are fixed at the mean. The full results are given in the Web Appendix. Robust standard errors are given in the second column.

on all three surveys are statistically significant at the conventional levels. The sociotropic effect therefore seems to have been activated between November 2008 and June 2009. We turn to examining the robustness of these results and what may have induced them.

4.2.1 Robustness and Potential Mechanisms

For the results based on the May 2008, November 2008, June 2009 and October 2009 surveys, we are forced to omit a measure of corruption exposure from the specification in equation 1 because the surveys do not contain the appropriate survey questions. However, in the Web Appendix, we show that the estimates are almost completely identical when an adjustment is made for this omission using the the results from the October 2008 and November 2009 surveys, as well as other surveys that contain both corruption measures (but not vote choice). The idea is that by using the information on the conditional partial correlation between corruption experience, perception, and vote choice, we can estimate the degree of the omitted variable bias caused by the omission of the measure of corruption exposure. Since we have already shown in several ways that exposure and perception are only tenuously related, it is not surprising that this bias is very close to zero.

Moreover, as in the case of bribe exposure, reliance on observational data makes it difficult to rule out the possibility of endogeneity due to partisanship.⁵⁹ If voters who intend to vote

⁵⁹For example, see [Bartels \(2002\)](#); [Evans and Andersen \(2006\)](#); [Gerber and Huber \(2009\)](#).

for the incumbent are less likely to report high corruption perception and vice versa,⁶⁰ the bias would go in the direction of our findings in June, October, and November 2009. We offer three observations we believe alleviate such concerns. First, the presence of a stable bias is inconsistent with the results we observe in Table 4. The bias from partisan perception must have changed between 2008 and 2009. This is not impossible, as the bias may have been exacerbated by the entrance of a new threat to the incumbent parties – an anti-corruption party like SaS. However, our composite measure of corruption perception contains questions that probe corruption perception of actors other than national politicians, such as regional and local politicians, and civil servants in health, judiciary, education, police, etc. While all the questions tap the perception of societal corruption, they cover distinct domains which taken together should be less susceptible to political projection: the bias is unlikely to be in the same direction for all actors, and is likely lower for perception of corruption among civil servants compared to those of politicians. Finally, as with corruption exposure, we have found strong evidence of sociotropic corruption voting in our other work using survey experiments in other countries where the possibility of partisan perception was eliminated by construction (self-citation omitted).

We turn to examining the mechanism behind the apparent shift in the prominence of the sociotropic channel between 2008 and 2009. The lack of the sociotropic effect in 2008 was amidst no less than four scandals in which government ministers were recalled or resigned because of alleged improprieties (November 2007, February 2008, and two in August 2008; see Figure 1). Between October 2008 and November 2009, two other corruption scandals took place (April and August 2009). But in addition, a new anti-corruption party called Freedom and Solidarity (SaS) entered the party system (February 2009) and led a referendum campaign (launched in June 2009) with a strong anti-corruption agenda. The evidence presented in Table 4 thus suggests that the scandals in and of themselves seem to have been insufficient in raising the salience of societal corruption and that an entrance and a campaign of a new anti-corruption party were needed to activate sociotropic corruption voting. Here, we examine this conjecture more directly.

Evidence from repeated surveys helps to rule out two potential reasons why we may have failed to observe sociotropic voting in the presence of scandals alone. First, it is possible that repeated scandals may have made voters inured to the issue of corrupt politicians. Related, successive scandals may have been endogenous to attempts by the opposition to use subsequent potentially controversial cases to politicize corruption, which may have trivialized

⁶⁰This tendency, for example is found by [Anderson and Tverdova \(2003\)](#).

scandal material and lessened its impact.⁶¹ An observable implication in either case would be that the sociotropic effect would diminish over time, being present for earlier scandals and absent for later ones. Results in Table 4 show that this is clearly not the case. A second reason for the lack of a sociotropic effect following the scandals could be that the half-life of a corruption scandal may be quite short. Repeated studies in 2008 offer variation in how much time elapsed between the survey and the scandal. While the May 2008 survey was conducted approximately three months after the most proximate scandal (that of February 2008), the October 2008 survey was conducted less than two months after a double scandal (in August 2008). The results in the first and second row again show that the estimated effects are nonetheless quite similar.

But what of the claim that it is the new anti-corruption party that induced the sociotropic vote rather than the 2009 scandals? Here, we turn to a wealth of aggregate polls conducted between the 2006 election and the 2010 election to estimate how the aggregate vote share of the governing coalition and the senior incumbent party react to our critical events.⁶² We have 116 polls conducted by four different survey firms almost every month. These polls provide only aggregate vote shares for the major parties, and so we cannot perform the individual-level analysis we did above. Instead, we combine these polls by taking into account the precision, based on each poll’s sample size, and the differences in survey methodology employed by each firm to estimate the monthly vote share time series.⁶³ We then model the monthly vote share in the following way:

$$\begin{aligned} \text{Vote share}_t = & \sum_j \alpha_j \text{Vote share}_{t-j} + \sum_{p \times k_p} \beta_{p \times k_p} \text{Scandal}_{p,t-k_p} + \gamma_l \text{SaS entry}_{t-l} \\ & + \delta_m \text{SaS campaign}_{t-m} + \sum_{q \times n_q} \psi_{q \times n_q} \mathbf{X}_{q,t-n_q} + \epsilon_t \end{aligned} \quad (2)$$

In words, we estimate an auto-distributed lag model on monthly data, with $p = 5$ scandal dummies⁶⁴ and $q = 7$ control variables in \mathbf{X} comprising of monthly inflation, unemployment

⁶¹Kumlin and Esaiasson (2012).

⁶²Looking at the senior incumbent party in addition to the coalition vote makes sense for two reasons. First, we want to compare the harm to the senior party to that sustained through pocketbook voting, which as we saw was high. Second, the new entrant, SaS, portrayed itself as the right-centrist mainstream party, thus being more of a competitor to the left-centrist Smer than to the more nationalist junior incumbents SNS and LS-HZDS.

⁶³We follow the approach to combining aggregate polls developed by Jackman (2005).

⁶⁴We include five rather than six scandals shown in Figure 1 because the effect of the scandal in August 2009 cannot be identified separately from the SaS referendum campaign taking place at the same time.

rate, industrial production, dummies for the European Parliament election (June 2006), local election (December 2006), presidential election (March 2010) and the beginning of the general election campaign (May 2010). We start from a general model with a flexible lag structure that allows each variable to have its own relevant number of lags.⁶⁵ After trial and error, the most appropriate model has $j \in (1, 2)$,⁶⁶ $k_p = 0 \forall p$, $l = 0$, $m \in (0, 1)$, $n = 0$ for the election dummies and unemployment, $n \in (0, 1)$ for the other two monthly economic series, and $n \in (0, 2)$ for the general election campaign dummy.

Based on this model, full results of which are given in the Web Appendix, we calculate the short-term and long-term effects of all scandals combined, and of the new anti-corruption party SaS and its referendum campaign.⁶⁷ The results are shown in Table 5. They give a clear picture: the total effect of the five scandals, whether short-run or long-run, is indistinguishable from zero, whereas the effect of the new anti-corruption party is noticeable. The entry of SaS and its referendum campaign produce a combined contemporaneous (within-month) reduction in the coalition vote share of around 3 percent (second row of the first column), and a longer-term (until the end of the electoral cycle) reduction of around 6 percent (second row of the second column). The difference between the two sets of events (third row) is highly statistically significant. Columns 3-4 show that most of the effect of the new party is born by the senior incumbent party. This is another piece of evidence in support of the clarity of responsibility argument we saw at work for the pocketbook effect.

In section 2.2, we hypothesize that the prevalence of sociotropic corruption voting depends on the salience of societal corruption. The results from Tables 4 and 5 imply that the salience of corruption may have been raised by the entry of the new party rather than, or at least in concert with the corruption scandals, which do not seem to have been sufficient alone. To examine this claim, we collected data on the coverage of corruption by the Slovak print and internet media during the 2006-2010 cycle (for more details, see section 3 and the Web Appendix). Specifically, we examine whether the entry of the new party induces a shift in the media coverage in the following way:

⁶⁵De Boef and Keele (2008).

⁶⁶The unit-root tests show that the monthly vote share is not stationary, while the first-differenced vote share is. We ran all models on the first-differenced vote share and its lag, but the results are nearly identical to the specification with the level of the monthly vote share and its two lags. We opt for the latter because of ease of interpretation.

⁶⁷For example, the combined short-term effect of scandals is the sum of the coefficients of each scandal dummy: $\sum_p \beta_p$. Their long-term effect is equivalent to: $\frac{\text{Short-term effect}}{1 - \sum_j \alpha_j}$. See De Boef and Keele (2008).

Table 5: Short-run and long-run effects of scandals and new party entry

	Coalition		Sr. Incumbent	
	Short Run	Long-Run	Short Run	Long Run
Total effect of scandals	-0.004 (0.009)	-0.008 [0.24]	0.007 (0.012)	0.031 [0.28]
Total effect of SaS	-0.032*** (0.005)	-0.060*** [37.12]	-0.032*** (0.008)	-0.153*** [14.93]
Difference between total effects	-0.028*** (0.013)	-0.052*** [4.06]	-0.039** (0.015)	-0.184** [6.62]

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Note: The dependent variable is the monthly vote share for the incumbent government (the first two columns) or the senior coalition party (columns 3-4) estimated from 116 aggregate polls. The results are based on the model in equation 2. For more details on the estimation procedure and the poll data, see the Web Appendix. The short-run effect of an event represents the coefficient on each variable (when only the level is included) or the sum of the level and the lags. The full results are given in the Web Appendix. The long-run effect equals the short-run effect divided by $(1 - \text{sum of lags of } Y)$ (De Boef and Keele, 2008). Numbers in square brackets represent the F -statistic for the nonlinear null hypothesis test that the long-run effect is equal to zero.

$$\text{Media coverage}_t = \beta_0 + \beta_1 \text{Scandals} + \beta_2 \text{Elections} + \beta_3 \text{SaS entry} + \beta_4 \text{Scandals} \times \text{SaS entry} + \beta_5 \text{Elections} \times \text{SaS entry} + \sum_j \gamma_j t^j + \epsilon_t \quad (3)$$

Unlike in the model in equation 2, where *SaS entry* is a dummy variable, in equation 3 it takes on a value of zero before February 2009, and one thereafter. This is a structural break model, and the hypothesis test on β_3 represents the standard Chow test of whether SaS entry represents a structural break in the media coverage of corruption.⁶⁸ The variable *Scandals* takes the value of one whenever there was a scandal, and zero otherwise; ditto for *Elections*. Coefficients β_1 and β_2 thus give the effect of scandals and election campaigns (local, presidential, and the election for the European Parliament), whereas coefficients β_4 and β_5 indicate whether any structural break caused by SaS entry also changes the effect of scandals and elections. The model also includes a (third-degree) polynomial in time, in order to flexibly control for any time trend in the media coverage of corruption.⁶⁹ The results are

⁶⁸Chow (1960).

⁶⁹The results are unchanged when we include lags of media coverage, or use a higher-order polynomial

given in Table 6.

Table 6: New party entry as a structural break in media coverage of corruption

	Media coverage
Scandals	0.105** (0.050)
Election campaigns	0.080*** (0.022)
SaS entry	0.248*** (0.053)
Scandals*SaS entry	-0.031 (0.081)
Campaigns*SaS entry	-0.029 (0.030)
N	48

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Note: The dependent variable is the share of articles about corruption as a proportion of all articles mentioning the government and Prime Minister Robert Fico appearing in any of the Slovak-language or English-language sources covering Slovakia on Factiva and Lexis-Nexis in the period July 2006-June 2010. Sources are listed in the Web Appendix. “Scandals” is a dummy variable equal to one when a scandal happens. “Election campaigns” is a dummy variable equal to one during the month an election took place (local election, European parliament election, and the presidential election). The model also includes a cubic polynomial in time to control for any time trends in the media coverage of corruption.

Table 6 supports the notion that the entry of SaS generally raises the salience of corruption through an increased coverage of corruption in the media. Entry of the new party increases the average media coverage by as much as 25 percent compared to the period prior to the entry. The scandals and election campaigns also increase the media’s focus on corruption, but the change is considerably smaller than the change induced by the new party. Moreover, we find no evidence that the effect of scandals or an election campaign changes after the entry of SaS, given that β_4 and β_5 are essentially zero. In other words, the entry of the new party increased the average salience of corruption, but did *not* make subsequent scandals or election campaigns more effective at raising salience of corruption in the media.

While the estimates from Table 6 clearly point to a break occurring shortly after the entry of SaS, this may not be the only structural break during the 2006-2010 electoral cycle.

in time. The media coverage series is stationary under the standard as well as structural-break consistent unit-root tests.

While scandals induce a smaller increase in the coverage of corruption than the entry of SaS, they still increase the salience of corruption. However, we provide three additional pieces of evidence that scandals did not seem to have sufficiently increased salience to bring about sociotropic corruption voting.

First, we rerun the structural break model from equation 3, with *SaS entry* replaced with a variable denoting a structural break in *any* month between January 2007 and November 2009.⁷⁰ In other words, we assume that the breaks are ex-ante unknown and let the data tell us where the break is most likely to be (subject to the specification imposed by the model in 3). We therefore conduct a series of Chow tests, which now follow a different distribution, whose critical value is higher than for the standard Chow F -test. The critical value is determined by the Quandt Likelihood Ratio (QLR) statistic. The date at which the value of the Chow F -statistic is at the maximum and is higher than the QLR critical value represents the estimate of the structural break.⁷¹ The results of this test are given in Figure 2. The figure strongly confirms our earlier results – the only structural break in the media coverage of corruption during the 2006-2010 election seems to occur shortly after the entry of SaS.

Second, we return to our individual data to provide further adjudication between the effects of scandals and the entry of SaS. We conjecture that if scandals failed to sufficiently raise the salience of corruption, we should find no statistically significant variation of the sociotropic effect across different levels of respondents' attentiveness to politics. If even the more attentive respondents do not employ sociotropic corruption voting in the face of multiple scandals (controlling for other factors), we may be more confident in claiming that scandals were not sufficiently salient signals of the importance of societal corruption. The October 2008 survey contains a question on the self-reported interest in politics, which we add to the specification in equation 1 along with the interaction with corruption perception. The results are unchanged: the marginal effect of corruption perception is not higher among the respondents reporting higher interest in politics.⁷²

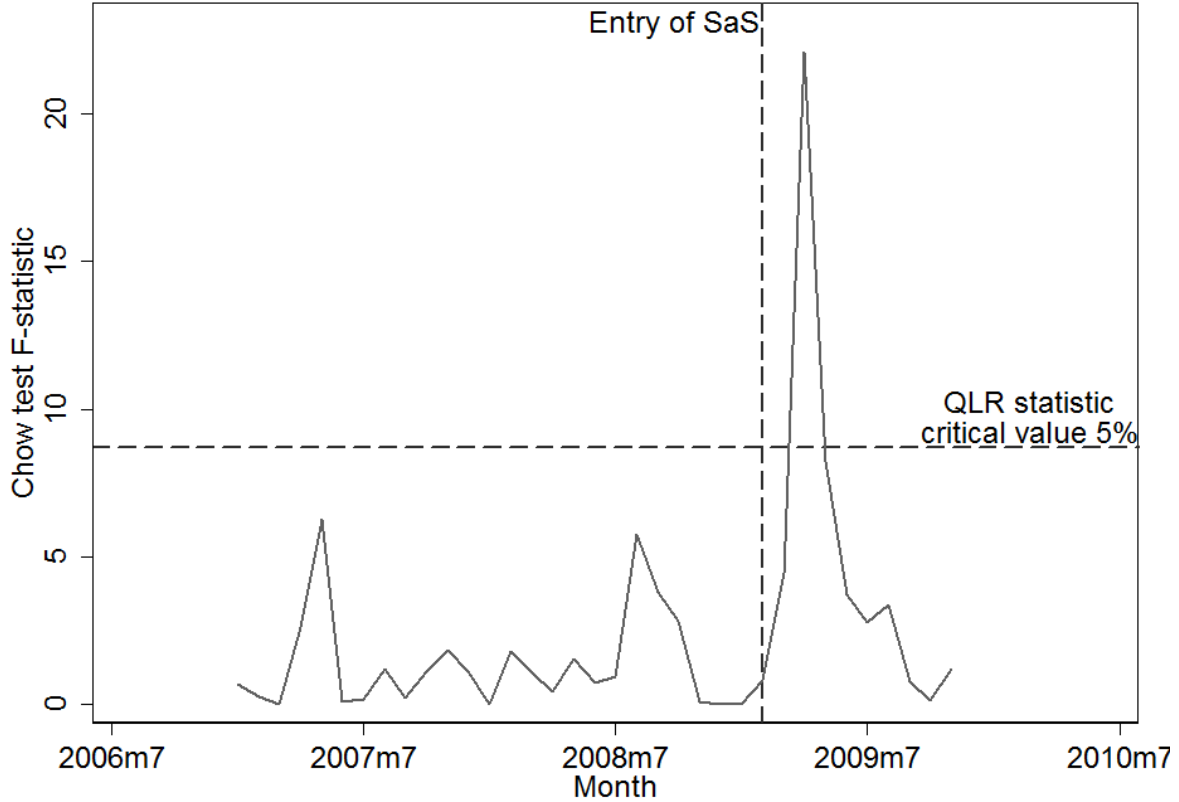
Third, we estimate the effect of corruption perception (and corruption experience) on *party* choice. All but one scandal (in February 2008) involved ministers from the two junior coalition members, the Slovak National Party (SNS) and the Movement for a Democratic

⁷⁰We need to leave some observations on either side of the rolling window to be able to estimate the model.

⁷¹Andrews (1993).

⁷²Unfortunately, other data do not contain satisfactory measures of attentiveness to politics in order to re-test this claim.

Figure 2: Testing for other structural breaks in media coverage of corruption



Note: The y -axis plots the value of the Chow-test F -statistic, which in this case is the square of the t -statistic for the null hypothesis that the coefficient for a structural break in month t plotted on the x -axis is not different from zero. The horizontal dashed line represents the critical value of the Quandt Likelihood Ratio (QLR) statistic at 5 percent (Andrews, 1993). The QLR statistic is a modified Chow test where the structural break is treated as unknown ex ante. Every point on the line thus represents a test for a break in that month. The date at which the Chow-test F -statistic is at its maximum represents an estimate of the structural break. When this maximum is above the QLR critical value, the test rejects the null at $\alpha = 5$ percent. The vertical dashed line represents the month of entry of the new party (SaS).

Slovakia (HZDS). And yet, we have seen above that most of the punishment for corruption seems to have been directed at the senior coalition party, Smer. If parties which are considerably more engaged in scandals are not being punished, it is reasonable to conclude that scandals had at best a minor effect. We examine whether the above patterns of punishing the senior incumbent party are replicated in the individual-level data. We fit a nested logit model because more standard multinomial choice models, such as multinomial logit, rest

on a very restrictive assumption.⁷³ Nested logit models are not identified if all variables denote attributes of individuals only. Using expert scores of parties in Slovakia⁷⁴ and survey responses to policy-relevant questions, we constructed party-specific distance measures on taxes, deregulation, redistribution, security, social liberalism, immigration, attitudes towards the EU, and the role of religion in politics. These measures are added as covariates to our baseline specification that includes previous vote choice.⁷⁵

For our purposes, the probability of interest is that of choosing the party conditional on choosing the nest:

$$P_{nk} = \frac{e^{\mathbf{X}_i\gamma + \lambda_k I_{nk}}}{\sum_{k \in K} e^{\mathbf{X}_i\gamma + \lambda_l I_{nl}}} \frac{e^{\frac{\mathbf{Z}_{ni}\gamma}{\lambda_k}}}{\sum_{m \in N_k} e^{\frac{\mathbf{Z}_{mi}\gamma}{\lambda_l}}}, \quad (4)$$

where $\lambda_k I_{nk}$ represents the expected utility to individual i from each party n in a nest k from the set of nests K ; \mathbf{X}_i contains individual-level covariates – corruption experience and perception which are assumed to affect the choice of the nest – and β_k are the associated parameters which vary over nests; \mathbf{Z}_{ni} are party-level covariates – party distance measures and previous vote choice – which are assumed to affect the nest as well as the party choice and vary over each party n within the nest N_k ; γ are the associated parameters.⁷⁶

The results are shown in Table 7. We are unable to run the analysis on November 2009 data because of the lack of party-level attributes. For the remaining surveys, the evidence is clear: the increase in the prominence of the sociotropic vote is entirely concentrated on the senior incumbent party (column 1). The two junior incumbent partners suffer no punishment despite being directly involved in all but one corruption scandal (columns 2-3).⁷⁷ This finding is again consistent with the results above that the increase in the prominence of sociotropic voting has been driven largely by the new anti-corruption party rather than the scandals. This result is also consistent with our findings from Tables 3 and 5 above which too indicate that most of the effect is bore by the senior incumbent. The remaining columns show that once sociotropic vote is activated, it is redistributed relatively equally among the mainstream opposition parties (columns 4-5). Moreover, an increasing share of the corruption-sensitive voters seems to turn to the new entrant SaS (column 6).

⁷³More details are given in the Web Appendix. See also Train (2007).

⁷⁴See Hooghe et al. (2010).

⁷⁵Details on the construction of these variables are given in the Web Appendix.

⁷⁶The second part of the product is the conditional probability of choosing a party conditional on choosing a nest, but it is uninteresting for us since it does not directly depend on experience and perception.

⁷⁷Both junior members are also parties with strong nationalist platforms, and it may be that this dimension partially overrides the importance of corruption among the supporters of these parties.

Table 7: Sociotropic effect on party choice

	Smer (Sr. Incumbent)	SNS (Jr. Incumbent)	HZDS (Jr. Incumbent)	SDKU (Opposition)	KDH (Opposition)	SaS (Opposition)
May 2008	-0.008 (0.027)	-0.007 (0.010)	-0.005 (0.007)	0.008 (0.008)	0.007 (0.007)	
October 2008	0.006 (0.030)	-0.002 (0.010)	-0.003 (0.011)	-0.000 (0.007)	0.000 (0.001)	
November 2008	-0.026 (0.030)	-0.006 (0.016)	-0.002 (0.005)	0.007 (0.006)	0.009 (0.007)	
June 2009	-0.077** (0.036)	-0.002 (0.012)	-0.001 (0.005)	0.017** (0.008)	0.014** (0.007)	0.010* (0.006)
October 2009	-0.066*** (0.020)	0.001 (0.022)	0.012 (0.012)	0.007* (0.004)	0.012** (0.005)	0.017** (0.007)

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Note: The dependent variable is the individual-party choice. We fit a nested logit model and calculate the probability of choosing a party based on equation 4. More details about the choice of nested logit are given in the Web Appendix. The main entries in the table represent the marginal effect of a one-standard deviation increase in corruption perception on a standardized scale while keeping all other variables in the model at their mean. Robust standard errors are shown in the parentheses.

We acknowledge that beside showing that the sociotropic vote is variable, that it seems to be activated by the entry of a new party rather than corruption scandals, and that the new party increases the salience of corruption in the media, we are unable to show the precise micro-level mechanisms driving sociotropic corruption voting. For this, we would need better individual-level data than are available. It could be that the increase in salience leads to an increase in certainty about the prevalence of corruption in society at large. We find some evidence that the share of “don’t know” answers to perceptual questions decreases as salience of corruption increases, but we are unable to test this claim robustly due to the lack of consistent question wording or panel data. It could be that the increase in salience of corruption makes some voters learn more about other aspects of incumbent performance or policy positions they previously ignored. It could be that bad performance on corruption may weaken partisan identification with the incumbent, or that the new anti-corruption party represents a coordination device among voters with weak party identification. These are all interesting areas for future research.

5 Way Forward

Our hope is that this paper will help invigorate the study of the impact of corruption (and perceptions of corruption) on political behavior. Much like the effort to chart the impact of economic considerations on voting in established democracies, we believe that a thorough understanding of the ways in which corruption affects voting in new democracies (and especially new democracies wrestling with corruption as a major issue, as in many of the post-communist countries) will prove valuable in the long run.

In that vein, our paper offers several important contributions. *Theoretically*, we have introduced a framework for thinking explicitly about the channels through which corruption may affect voting behavior: *pocketbook corruption voting* is defined as the effect of personal experiences with corruption on voting behavior; while *sociotropic corruption voting* is defined as the effect of perceptions about corruption in one’s society on voting behavior. Our taxonomy is motivated by a somewhat surprising yet repeated observation that experiences with and perceptions of corruption are only tenuously correlated. Moreover, we argued that the relative weight individuals put on these two mechanisms depends on the salience of each form of corruption. Since importance of bribe victimization to those victimized is inherently high, and exposure in the aggregate is slow-changing, pocketbook corruption voting is expected to be stable when bribe victimization in the society is considerable. Salience

of societal corruption is variable and depends on the actions of elites, such as corruption scandals, campaigns, or entry of new parties with anti-corruption platforms.

Empirically, to our knowledge we have provided the first evidence that the two mechanisms can co-exist, suggesting that some previous studies may have *underestimated* the effect of corruption on voting. In particular, the effect of personal exposure to corruption has been largely overlooked in the previous literature on voting behavior. Moreover, we confirm that the effect of perception varies. We believe that another contribution lies in the evidence that sociotropic corruption voting may require credible or very strong signals. We do not find that the myriad scandals present in Slovakia lead to protest vote based on corruption perception.

While there are obviously many ways in which we can proceed, we wish to highlight three which we think are particularly pertinent. *Theoretically*, it seems prudent to try to better understand the relationship between corruption experience and corruption perception. While our analysis suggests that bribe victimization may be largely independent of corruption perception, it is certainly possible that experiences with corruption will lead one to view corruption in one's country differently. Similarly, it is interesting to consider whether changes in the governing status of political parties might shape the attitudes of party faithful about corruption and whether those changes could alter the relative weight of experience and perception. It is also possible that bribe victimization is not the only form of corruption experience we should be measuring: perhaps it is possible to somehow experience "grand corruption" as well.

More generally, it will be important to flesh out theoretically the differences between pocketbook economic concerns (my personal financial situation is good or bad) and pocketbook corruption experiences (I was asked to pay bribes or not). It may be that the reason that pocketbook corruption voting sometimes trumps sociotropic corruption voting while pocketbook economic voting does not has to do with the actual psychological experience of being victimized by corruption.

Empirically, the next step forward seems obvious, which is to extend the research we have done in Slovakia into other countries, especially but not limited to post-communist countries. The challenge in doing so, unfortunately – and indeed the reason we focus on the Slovak case in the first place – is that for now we have only been able to find all of the variables we need to carry out all the analyses contained in this paper in Slovakia. Thus our hope is that one consequence of this paper will be to encourage those studying corruption to add questions about political behavior (especially regarding both future and prior vote

choices) to their surveys, and for election studies to include both pocketbook and sociotropic corruption questions. Either way, replicating the findings in this paper outside of Slovakia remains an important future task. Moving beyond the data from a single country will also allow us to further test the “supply-side” part of the equation to see whether the presence of new parties and parties with strong anti-corruption appeals is indeed primarily responsible for the sociotropic effects.

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