

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 mechanisms: *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 voter places on each mechanism depends on the level of certainty about each source of corruption. Since certainty about bribe victimization is inherently high and overall levels of bribe-taking in society are slow-changing, pocketbook corruption voting is expected to be stable. Certainty about societal corruption is inherently low, but it may be increased in the aggregate electorate by the actions of elites, such as corruption scandals, campaigns, or entry of a new anti-corruption party. Using repeated surveys from Slovakia and a novel survey experiment run in Bulgaria, we find support for our theoretical arguments. In the absence of events that raise certainty about sociotropic corruption, we only find pocketbook voting. Sociotropic voting, however, is not activated by scandals alone, but it is by the entry of a credible anti-corruption party. We conclude that previous studies may have underestimated the effect of corruption on voting by missing pocketbook effects, but may potentially have been overstating the importance of corruption scandals.

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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 (Anderson & Tverdova 2003, Della Porta 2000, Lavalley, Razafindrakoto & Roubaud 2008, Seligson 2002). It also typically depresses electoral turnout (Chong et al. 2011, Davis, Camp & Coleman 2004, McCann & Dominguez 1998)¹ and reduces electoral support for the incumbent, although findings in developing countries are more consistent and stronger (Ferraz & Finan 2008, Humphreys & Weinstein 2007, Krause & Mendez 2009, Banerjee et al. 2009) than those in mature democracies (Chang, Golden & Hill 2010, Peters & Welch 1980, Reed 1999, Welch & Hibbing 1997).²

However, little is known about the channels through which corruption may affect citizens' political behavior. Previous findings have been guided more by data availability than by a theoretically posited mechanism. 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 corruption as a perceived societal problem. 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 conditions that affect the weight individuals ought to put on their pocketbook and sociotropic concerns. We offer an informational rationale. While both factors may be at work simultaneously, we argue that voters will often be less certain about the prevalence of corruption in society than their own personal exposure to corruption. We thus expect that when voters lack information about societal levels of corruption and bribe victimization is considerable, they will be more likely to exhibit pocketbook as opposed to sociotropic corruption voting. However, when salience of corruption as a societal problem 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

¹However, it may increase the likelihood of participation in protest (Gingerich 2009).

²From the perspective of democratic accountability, the lack of electoral cost of corruption in mature democracies is puzzling. Explanations range from rational trade-offs by voters between particularized benefits and corruption (Rundquist, Strom & Peters 1977), limitations imposed by electoral process and rules (Chang 2005, Chang & Golden 2006, Persson, Tabellini & Trebbi 2003), cognitive dissonance (Dimock & Jacobson 1995, McCann 2007), and voter ignorance (Klašnja 2011).

increase in prominence. Since bribe extortion is presumably slow-changing, pocketbook voting – if present – should exhibit more stability than sociotropic corruption voting, which we expect to wax and wane with perceived societal salience of corruption.

It is quite 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; and 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 virtually all available cross-national data; moreover, polling in most countries is done relatively rarely. However, we were able to collect satisfactory data from Slovakia. We utilize a number of individual-level surveys and a large volume of exit polls conducted within one election cycle between 2006 and 2010 to test our arguments. We also complement these sources with original data from a survey experiment in Bulgaria. 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. In line with the established wisdom, 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 exhibits stability throughout our period of study. For sociotropic corruption voting, we exploit the presence of several scandals and the entrance of a new party with a strong anti-corruption platform to test our predictions concerning salience. The presence of scandals alone does not seem to induce the effect of corruption perception on intended vote, suggesting that the emphasis by observers and the media on corruption scandals may have been exaggerated. However, in the wake of a new party entrance and its concurrent anti-corruption campaign, sociotropic corruption voting is activated. Finally, the beneficiaries of vote switching differ somewhat: large established opposition parties seem to benefit most from pocketbook corruption voting, whereas the new entrant benefits most from sociotropic corruption voting. This evidence highlights the importance of distinguishing between the two mechanisms. Also, as both types of voting may operate simultaneously, previous studies focused only on sociotropic or pocketbook corruption voting may have underestimated the overall effect of corruption on voting behavior by ignoring the other component.

The paper proceeds as follows. In Section 2, we elaborate on our theoretical argument. Section 3 describes the merits of focusing on Eastern Europe when studying corruption and voting. Section 4 outlines our estimation strategy and data, while Section 5 discusses our main findings, their robustness to most serious concerns, and the potential mechanisms that explain our results. In Section 6, we further disaggregate the effects of corruption on party choice. Section 7 further addresses

the robustness of our findings, including results from a novel survey experiment in conducted in Bulgaria. In the final section of the paper, we suggest a variety of ways forward for testing additional observable implications of our theoretical arguments; a supplemental web appendix provides additional substantive and statistical details, including a variety of additional robustness tests of the results.

2 Theory of Corruption Voting

2.1 Pocketbook Corruption vs. Sociotropic Corruption

While most studies of the political consequences of corruption highlight its negative effect on trust, electoral participation, or incumbent support, these claims often seem to be based more on data availability than on a theoretical account of possible channel(s) of influence. Some studies suggest that corruption matters for political preferences by way of corruption perception, whether directly as an attitude (Anderson & Tverdova 2003, Davis, Camp & Coleman 2004, Della Porta 2000, Krause & Mendez 2009, McCann & Dominguez 1998), or when it is reinforced by revelation of hard information (Chong et al. 2011, Ferraz & Finan 2008, Humphreys & Weinstein 2007, Banerjee et al. 2009). In addition, many studies posit an effect through perception which is implied rather than directly measured (Alford et al. 1994, Chang, Golden & Hill 2010, Dimock & Jacobson 1995, Peters & Welch 1980, Welch & Hibbing 1997). Other studies – though fewer in number – claim that corruption matters through personal exposure (“victimization”) (Gingerich 2009, Lavallee, Razafindrakoto & Roubaud 2008, Seligson 2002).

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 types of corruption - such as soliciting 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 explicitly theorizing about the potential underpinning channels of influence of corruption on vote choice. We focus on this particular relationship between both because it is politically consequential and because it has been understudied compared to other forms of performance voting, particularly economic voting. This lacunae is especially evident in the case of developing democracies, where the prominence of 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 suffered from omitted variable bias and therefore may have miss-estimated the influence of corruption on voting behavior.

To distinguish between the two channels, for convenience we adopt the nomenclature commonly used in the economic voting literature. A wealth of aggregate- and individual-level evidence links economic performance and election outcomes (classic studies and comprehensive reviews include, for example, Kramer 1971, Fiorina 1981, Hibbs, Rivers & Vasilatos 1982, Powell & Whitten 1993, Lewis-Beck & Paldam 2000).³ 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 (e.g. Kinder & Kiewiet 1981). 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 same 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 primarily motivated by the findings from studies on the methodology of corruption measurement. Studies in this literature have repeatedly shown that the relationship between personal experience with corruption and corruption perception is quite tenuous. Personal experience predicts perception of administrative corruption and perception of “grand” corruption equally well (or rather, badly), even though survey respondents are much more likely to have experience with public administration than with high-level politicians (Abramo 2007, Donchev & Ujhelyi 2009). The direction of change in perception does not relate systematically to the direction of change in reported exposure (Krastev & Ganey 2004). Respondents’ attitude towards bribery does not relate to perceptions of corruption, even though it relates to exposure to corruption (Rose & Mishler 2007, Mocan 2004, Morris 2008). Corruption perception is more strongly correlated with other general perceptions and *government* evaluations, such as those of government’s record on human rights, prices and jobs (Abramo 2007), and fairness of public services (Rose & Mishler 2007), as well as with certain country characteristics, such as per capita GDP (Donchev & Ujhelyi 2009).⁴ This is perhaps not so surprising: exposure typically relates to petty corruption, while perceptions of corruption tap into (many forms of) political corruption (McCann & Redlawsk 2006). Moreover,

³For evidence in the post-communist context, see for example Pacek (1994), Fidrmuc (2000), Duch (2001), and Tucker (2006).

⁴For evidence from laboratory experiments to the contrary – that corruption perception may influence bribe-giving and bribe-taking – see Cameron et al. (2009) and Drugov, Hamman & Serra (2011). However, as it is difficult to establish the external validity of these types of lab experiments, these results are not easy to compare to the cited literature.

administrative and political corruption have been shown to often have distinct determinants and different dynamics (Anderson & Gray 2006)

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.⁵ 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.⁶ 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.⁷ Conceptualizing the two forms of corruption voting as separate therefore appears to be meaningful. Below, we find further support for this assumption by observing little change in the estimates of the pocketbook mechanism after omitting the measure of the sociotropic mechanism, and vice versa.⁸

[Table 1 about here.]

2.2 Information, Salience and Certainty

Assuming that pocketbook and sociotropic mechanisms are largely distinct phenomena, we are further interested in the conditions under which we may observe the presence or absence of each form of corruption voting.⁹ We propose an informational argument which is motivated by two sets

⁵As 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.

⁶For Western Europe, all correlations are even lower, and typically not different from zero. Results are available upon request.

⁷We could attempt to estimate the magnitude of priming if we had variation in the order in which the 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). We plan on estimating priming effects by randomizing the question ordering in future survey experiments.

⁸In 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. Ansolabehere, Meredith & Snowberg 2011, Killian, Schoen & Dusso 2008, Mutz & Mondak 1997). While more evidence is needed for corruption voting, it appears that a similar critique is less warranted.

⁹Another logical question is that of the effects of heterogeneity at the individual level. Tverdova (2011) shows that perceptions of corruption vary with factors such as political allegiances, personal economic conditions and education.

of assumptions. At the *individual* level, we assume the following:

1. Certainty about one's own exposure to corruption (or that of one's immediate surrounding) is inherently *high* (e.g., did I, or my family, friends, etc. have to pay a bribe?).
2. Certainty about the level of societal corruption is inherently *low*, since it is fundamentally unobservable (e.g., are government officials bought off in return for state contracts?).

At the *population* level, we rely on two assumptions:

1. Average certainty in the population about personal exposure is *stable*, since bribe extortion is slow-changing.
2. Certainty about societal levels of corruption *varies*, and is a function of the degree to which elite actors (e.g., government officials, party leaders, media) call attention to the prevalence of corruption in society.

Taken together, these assumptions suggest the following. First, in the absence of political events that raise the salience of the society-wide prevalence of corruption, and to the extent that regularized contacts with the bureaucracy or public services are characterized by a sufficiently high likelihood of bribe victimization, we should expect corruption-sensitive citizens to rely on their personal experiences with corruption but not on (highly uncertain) guesses about sociotropic levels of corruption. We also expect pocketbook corruption voting to be stable in the short run, since bribe-extortion is decentralized and difficult to monitor and change in the short term (Anderson & Gray 2006). In line with the established wisdom, we expect that increased personal exposure to corruption will on average drive voters away from the incumbent.

On the other hand, we expect that several developments might increase the salience of societal corruption and, therefore, voter certainty about how prevalent corruption is at a more general level. 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, certainty in sociotropic evaluations 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" (Kumlin & Esaiasson 2012). Similarly, raising corruption in an election campaign may be a less effective tactic when the opposition parties have previously been in office themselves and accused of corruption, a far too often occurrence in

These factors may also cause variation in the weight placed on corruption perception in the voting calculus. On the other hand, the risk of bribe victimization varies with factors such as income (Hunt 2007), which may influence the likelihood with which individuals exhibit pocketbook corruption voting. We do not focus on these types of interactions here.

post-communist Eastern Europe (Krastev & Ganev 2004). 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 may have a particularly strong effect on raising the salience of corruption as a political issue.

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 Context

It is surprisingly 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.¹⁰ Fortunately, we found an exception to this rule across a number of surveys that took place in Slovakia. Slovakia also is an ideal 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 (Karklins 2005, Krastev 2004, Pop-Eleches 2010, Roberts 2008, Tavits 2007).¹¹ Over the course of the transition from communism, corruption has emerged as one of the most pressing developmental and political issues. For example, as noted by the World Bank:

[W]hile corruption was barely mentioned at the start of the 1990s, by the end of the decade it had come to be recognized as a central challenge to progress in many countries in the region. [...] Corruption has been an important issue in the discussions surrounding EU enlargement, has figured prominently in political campaigns, and has been a key concern of citizens, businesses, and international organizations alike. (Anderson & Gray 2006, p. xi)

¹⁰Consider 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. 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. 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.

¹¹In one of the few studies of the relationship between corruption and voting in a post-communist country, Slomczynski & Shabad (2011) show that perceiving a party to be corrupt made voters in Poland less likely to vote for that party.

A 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.¹² Part of this “apparent epidemic” can be explained by the low level of economic development and limited experience with democracy (Treisman 2003). 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 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 to report having spent 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 (except for economic growth rate). 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. Like many of the accession countries, it has a significant ethnic minority population with a neighboring kin state, but as with the other accession countries, ethnic tensions have not risen to the level of public violence. Most importantly, Slovakia is also relatively typical in terms of its corruption experience and perception. For example, Slovakia’s recent “Gorilla” scandal highlights the significance of corruption questions in Slovakia’s politics.¹³ More systematically, we show in the Web Appendix that Slovakia is slightly above the median for the region with respect to citizens’ bribe victimization, and at the median in terms of citizens’ perception of corruption.

4 Data and Estimation

Our estimation strategy combines several types of data. Our main source of data are the five cross-sectional surveys from Slovakia in the period between May 2008 and November 2009.¹⁴ The two main data sources we rely on are the Slovak portion of the International Social Survey Programme

¹²For the whole period since the collapse of communism, corruption features prominently in 38 out of 66 elections (58 percent) covered in the journal. Early elections, however, were dominated by other issues, most notably economic decline, reforms, and nationhood.

¹³As an aside, the international prominence of this scandal may have more to do with the piquancy of its name (allegedly derived from the codeword for the related police investigation) and the proximity of its revelation to Slovakia’s March 2012 elections than to its unusual character (the events involved in the scandal occurred half-a-decade ago and involve rather mundane, if not insignificant, funding of party accounts by economic interests.)

¹⁴Panel data are unfortunately simply not existent.

(ISSP) Role of Government IV module, conducted in October 2008, and the Transparency International Slovakia’s survey conducted in November 2009. Both surveys contain all of our crucial variables of interest – items probing corruption perception and experience, as well as vote choice – and a satisfactory set of other covariates typically found in voting models. In addition, we use two surveys conducted by the Slovak polling firm Focus in May and November 2008, and the Slovak portion of the ISSP Social Inequality module IV from October 2009. These surveys contain questions probing corruption perception and vote choice, but not corruption exposure. We use these three surveys as auxiliary data to further test our expectations about sociotropic corruption voting, while adjusting the estimates for the omission of a measure of corruption exposure. Furthermore, we use 116 public opinion polls conducted by Slovak polling firms Focus, Polis, Median, and UVVM in the period between July 2006 and July 2010. These polls, conducted almost every month by each firm, contain only aggregate estimates of indented vote choice, but are nonetheless useful for further examining the mechanism behind the sociotropic voting we observe. We further rely on original data collected through a novel survey experiment conducted in August 2011 in Bulgaria. While our experiment was conducted in a different country at a different point in time, we argue that it approximates the conditions in Slovakia for testing our expectations and believe that the results are broadly comparable. Our survey experiment serves both as a robustness check of our results from observational data, and a preliminary check of external validity of our findings from Slovakia. Finally, we 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.¹⁵

Aside from data availability, Slovakia provides fertile ground for testing our hypotheses for four other reasons. First, it is marked by several relatively high-profile corruption scandals in the governing coalition. The Minister of Defense (February 2008), two Ministers of Agriculture (November 2007 and August 2008), the Minister of Construction and Regional Development (April 2009), and two Ministers of Environment (August 2008 and August 2009) were recalled or resigned due to various allegations of improper financial conduct.¹⁶ All of the scandals were of 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 soon thereafter launched a campaign for a referendum with a pronounced anti-corruption agenda. The anti-corruption campaign partly coincided with the election for the European Parliament conducted in June 2009.¹⁷ The scandals and the emergence of a new anti-corruption party provide

¹⁵Detailed overview of the variables contained in each data source can be found in the Web Appendix.

¹⁶Only 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.

¹⁷The initiative was called “Referendum 2009.” It asked six questions: to abolish the television licence; to limit parliamentary immunity; to lower the number of MPs from 150 to 100 by 2014; to set a maximum price for limousines used by the government at 40,000 euros; to introduce electronic voting via the internet; and to change the press code by removing politicians’ automatic right of reply to any article published about them. SaS sought to portrait questions

good conditions to test our predictions about sociotropic corruption voting. Third, all of the surveys were conducted during the same election cycle, allowing us to control for unobservable factors related to a change in the composition of the government.¹⁸ Finally, the data comes from a period outside a national election campaign. Even though we argue that the campaign itself may raise salience of corruption, a major campaign may also complicate estimation. National election campaigns may for example include attempts by the incumbent to rein in bribe elicitation by the bureaucracy and public servants. The risk of such contamination in our data is lower, as our observations are drawn mid-cycle (and include only a second-order election). Figure 1 shows a timeline of relevant events and survey dates in Slovakia.

[Figure 1 about here.]

In our main data, 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”). In the main analysis in the next section, we focus on whether the respondent i intends to vote for *any* incumbent party ($vote_i = 1$) or not ($vote_i = 0$). We further disaggregate the intended vote choice results by party in Section 6 below. In the main analysis, the binary vote is modeled with a standard logit model:

$$Pr(vote_i = 1) = \frac{1}{1 + e^{-\mathbf{X}_i\beta}}.$$

Key covariates in \mathbf{X}_i are measures of corruption exposure and corruption perception. Corruption exposure is elicited by asking respondents whether they or someone from their immediate family were asked for a bribe in some previous period of time. For perception, respondents were asked how widespread they thought corruption was among national politicians and possibly other political actors (regional and local politicians) and public sectors (police, education, customs, etc.). The precise wording and subject of corruption probing items differ between surveys, but we strive to provide results that are as comparable across surveys as possible.

Where available, \mathbf{X}_i further includes the report of the vote choice in the last actual election in 2006. Previous vote choice, also coded as a binary 0/1 variable, is a powerful covariate, as it should essentially contain 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.¹⁹ It should also include the effect of previous *levels* of corruption

2-4 and 6 as parts of an anti-corruption initiative. More details about the contents and the outcome of this initiative are given in the Web Appendix.

¹⁸By focusing on one country, we are also able to avoid having to deal with unobservable factors at the country level.

¹⁹Including 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 & 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 & Tucker 1998), and we use them throughout.

exposure, perception, and other changing attitudinal characteristics that determine vote choice. Our preferred specification therefore contains relatively few covariates. However, not all surveys contain previous vote choice, in which case we further saturate the model with (weakly) exogenous variables: demographic, socio-economic and geographical covariates, as well as voter’s positions on various policy issues. In Section 7, we show that the results are insensitive to adding other potentially relevant controls.²⁰

Like most public opinion surveys, our data contain missing values due to item non-response. While missingness in any one variable is relatively minor, listwise deletion, which is typically used in statistical analysis, entails a considerable loss of sample size. It is well-known that this engenders inefficiency at best and bias at worst (Rubin 1987). If missingness is partly a function of a particular survey design or timing, the problem of bias may be even more acute when attempting – as we do – to compare results across different surveys. To guard against such concerns, we multiply impute our data. Multiple imputation eliminates potential bias if the missingness is ignorable (Schafer 1997). The key assumption is that the data are missing at random (MAR) conditional on all the variables included in the imputation stage.²¹ Validity of MAR is not empirically verifiable, but in practice, including as many relevant predictors as possible tends to make the MAR assumption more plausible (Gelman & Hill 2007). Therefore, in addition to all the variables included in the analysis stage, we include a number of other attitudinal, demographic, socio-economic, and geographic covariates in the imputation stage. All scalar quantities and parameter estimates reported below and in the Web Appendix are combined across the five imputed datasets using Rubin’s rules (Rubin 1987).

5 Binary Vote Choice Results

To reiterate, we are examining the extent to which bribe victimization (pocketbook) and/or perceptions of corruption (sociotropic) affect the likelihood of reporting an intention to vote for parties in the incumbent coalition. Because of space constraints, we present our results graphically rather than in tables. Also, graphical presentation of marginal effects is more informative than parameter estimates and hypothesis test statistics in non-linear models such as logit (Greene 2010). Parameter estimates in table format for each survey are presented in the Web Appendix. We present each channel in turn, holding the other channel constant, but we stress that all our models include both channels in the same specification.

²⁰Unfortunately, our data do not contain items probing economic and other performance evaluations and we are unable to include measures thereof, but in Section 7 we provide evidence that our results are insensitive to their omission. Alternatively, we could include objective measures of economic performance such as economic growth, unemployment or inflation rate. These measures are also less subject to endogeneity. Since we have single cross-sections in one point in time, our measures would have to be at the sub-national level (Tucker 2006). The existence of only eight regions in Slovakia, however, gives relatively little variation.

²¹For the definition of the types of missingness mechanisms, see Rubin (1987).

5.1 Pocketbook Results

We first focus on our estimates of pocketbook corruption voting, while keeping the effect of perceptions, and the remaining covariates, fixed. We believe that corruption exposure can affect vote choice in the presence of considerable bribe extortion, which we have shown above to exist in Slovakia. To the extent that exposure to corruption affects vote choice, we expect the evidence to be stable. In line with previous research, increased corruption exposure is expected to reduce support for the incumbent.

Figure 2 graphically displays the results from our data. Only two of our surveys contain corruption exposure items. Results from October 2008 are shown in the left panel of the Figure, and represent the change in the predicted probability of intended vote for incumbent parties for a reported level of victimization *relative* to no victimization. *Ceteris paribus*, an increase in self-reported exposure to corruption measured in October 2008 statistically significantly decreases the probability of reporting a vote intention for an incumbent party. For example, a typical respondent victimized “very often” in the past five years is about 20 percent less likely on average to report an intention to vote for the incumbent party than a respondent who has “never” been victimized. All the estimated changes in predicted probability are significant at conventional levels: the 95 percent confidence intervals do not cross zero. The estimated effect in October 2008 is large, for two reasons. First, sixty percent of respondents in the sample in October 2008 report that they intend to vote for the incumbent coalition.²² Thus the estimated maximum effect of bribe victimization represents about one-third of this base probability. Second, the effect of this size comes on top of the demobilizing effect of experience (not shown, available upon request), which already removes a share of voters with high corruption exposure from the electorate.

[Figure 2 about here.]

Results from November 2009 are shown in the right panel of Figure 2. In this survey, we only have a binary corruption experience item (no/yes). The right panel therefore shows the absolute value of the predicted probability of intended vote for the incumbent when a typical respondent reports having not and having been asked for a bribe, respectively, rather than the change relative to the lowest category (as in the left panel). The results are substantively quite similar. A typical respondent who reports having been asked for a bribe in the past three years is approximately 9.7 percent less likely to report an intention to vote for the incumbent party than a respondent who reports not being victimized. This difference is also significantly different from zero at conventional levels. The size of the effect is broadly similar to that in October 2008. The average estimated effect across all categories of victimization in the left panel (relative to the category “never”) is 9.4 percent. Experience with corruption thus seems to have a potent effect on vote choice, and it

²²Incumbent vote intention and previous vote for incumbent are likely subject to over-report. We address this issue in the Web Appendix.

seems to be stable, in line with our expectations. Unfortunately, our claim about stability can at the present time only be substantiated with these two data points.²³

5.2 Sociotropic Results

We now discuss the sociotropic results based on the main data. To reiterate, we expect the effect of corruption perception to vary with salience of societal corruption. Salience may be raised by opposition parties and/or media through campaigning, entry of new anti-corruption parties, or potentially 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.

Figure 3 shows evidence from our main data that contain both pocketbook and sociotropic measures. Again, the left panel shows the estimates from October 2008, and the right panel those from November 2009. Both panels show the change in the predicted probability of intended incumbent vote at the reported level of corruption perception *relative* to the lowest reported perception. The left panel of the figure suggests that the effect of an increase in corruption perception elicited in October 2008 on intended vote for the incumbent is not statistically different from zero at conventional levels. For example, the maximum likelihood estimate of the change in the incumbent vote probability of a typical respondent who perceives “almost all” politicians in October 2008 as corrupt compared to a typical respondent who perceives “almost none” politicians to be corrupt is an increase of about three percent, but the confidence interval includes zero. The right panel of Figure 3, however, shows statistically significant negative sociotropic effects. A typical respondent who reports that corruption among politicians “exists, but is not widespread” is approximately 10 percent less likely to vote for the incumbent than a respondent who is of the perception that corruption “does not exist” among politicians. The effect doubles for a typical respondent who perceives that corruption is “widespread” among politicians. The sociotropic effect therefore seems to have been activated between October 2008 and November 2009. In the next two sections, we present evidence that it was the entrance and the anti-corruption campaign of a new party that seems to be the major factor in the appearance of sociotropic voting.

[Figure 3 about here.]

We presented evidence of separate *ceteris paribus* effects of personal experience with corruption and perception thereof. It is also important to note that we do *not* find any interaction effect between the two channels. An interaction variable between our measures of exposure and perception

²³It is worth noting that other surveys that probe corruption exposure around this time – but not vote choice – give similar shares of respondents in the sample reporting bribe victimization as the surveys we employ. The weighted shares are as follows: Eurobarometer November 2007: 31.6 percent; ISSP October 2008: 24.9 percent (excluding category “seldom”); Eurobarometer September 2009: 23.3 percent; Transparency Slovakia November 2009: 29.3 percent. These shares are within the 90 percent confidence interval of the estimate for Slovakia in the surveys we analyze (see the Web Appendix for details). So while far from a classical robustness test, these additional surveys at the very least lend credence to the measure of the dependent variable in the data we do analyze.

is statistically indistinguishable from zero at conventional levels in both the October 2008 and the November 2009 specifications. Moreover, the inclusion of the interaction eliminates the statistical significance of our main effects, further suggesting that the interaction only adds multicollinearity rather than additional explanatory power. This is in line with the low correlation between the two channels we presented above in Table 1.

5.3 Robustness of Results and Potential Mechanisms

Since we rely mainly on observational data, here we report a number of robustness checks of our results and also probe the potential mechanisms that can explain the findings presented above. Because of space constraints, details are mostly relegated to the Web Appendix.

5.3.1 Pocketbook Effect

Exposure to corruption is not random. The most serious issue 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 for two reasons: the ballot is undeniably secret in Slovakia (OSCE/ODIHR 2010); and clientelism is not prominent (Kitschelt & Kselman 2011). 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 rerun our models with regional and municipal fixed effects, thus looking at the effect of bribe exposure only within these territorial units. Our results are virtually unaffected.²⁴ 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.²⁵ While the coefficient on previous incumbent vote is negative, it is statistically significantly different from zero only at 74 percent. 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 31 percent. As a third robustness check against potential endogeneity, in Section 7 we present a survey experiment in which endogeneity due to partisanship is removed by construction, and show that our pocketbook effect remains.

²⁴Results are shown in the Web Appendix. The model for the 2009 data contains fixed effects in the original specification presented in the previous section.

²⁵We are able to do this only for 2008, where we have information about previous vote choice.

Another important concern stems from the wording of the questions that elicit bribe exposure. The survey item in 2009 asks for experience with corruption in the previous three-year period, thus obviously including the period covered by our 2008 survey. While we are claiming that evidence presented above points to stability in the pocketbook effect across time, due to the overlap in the coverage of the bribe questions in our surveys, it may be that we are only measuring the *same* pocketbook effect coming from a period before (or during) 2008. Moreover, our 2008 item probes bribe experience in the previous five years, thus encompassing the previous election in 2006. Our results may thus be spuriously driven by considerations relevant for the *previous* election, whereas we want to isolate the pocketbook effect in the current cycle only. To rule out these alternative explanations, we perform a question-wording adjustment based on a simple probabilistic model of response to a retrospective bribe victimization question. The model, presented in the Web Appendix, assumes a stable annual victimization rate, and relies on the estimate from Eurobarometer data used in Table 1 above. Our results are substantively unchanged.²⁶

Even if our results are robust to these concerns, the question remains about the mechanism behind our finding. 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 exposure and corruption perception in Section 3, the distinction between a pocketbook and a sociotropic effect would be empirically unidentifiable. This kind of behavior may also perhaps explain why we observe no sociotropic effect in 2008 (although it would not explain the presence of sociotropic effect in 2009). To check for this explanation, we include into our 2008 model two available measures that reflect perceptions, but are also related to corruption experience: trust in civil servants, and a measure of how fairly the respondent perceives to have been treated by public officials. If the effect of exposure runs through exposure-elicited perception, inclusion of these measures should change our estimate of the pocketbook effect presented above. However, we do not find such evidence: our pocketbook effect is unchanged.²⁷ This suggests that our channel runs through personal experience and further supports our claim that perception and experience are weakly related. The precise mechanism, however, cannot be teased out with the data at hand, and remains as an interesting subject for future research.

²⁶The Web Appendix also reports a placebo test, whereby bribe experience reported in 2008 is used to predict vote choice in the 2006 election. The result suggests that respondents are *not* reporting five years worth of experience with corruption, as bribe exposure is *not* a good predictor of previous vote choice.

²⁷We run a number of different specifications: including both measures 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 to run only through exposure-elicited perceptions, we do not find any pocketbook perceptual effects. All results are available upon request.

5.3.2 Sociotropic Effect

It is also always possible – however unlikely – that the difference between the estimated sociotropic effects in October 2008 and November 2009 may be simply a statistical artifact arrived at only by chance. To increase our certainty, we utilize our auxiliary data to check the robustness of our findings in repeated samples. In these data we are forced to omit a measure of corruption exposure because we do not have the appropriate survey items. However, in the Web Appendix, we show that the estimates are almost completely identical when an adjustment is made for this omission using our main results and the estimated partial relationship between corruption perception and corruption experience from our main data, as well as other surveys that contain both corruption measures (but not vote choice).

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 (Bartels 2002, Evans & Andersen 2006, Gerber & Huber 2009). If voters who intend to vote for the incumbent are less likely to report high corruption perception and vice versa (Anderson & Tverdova 2003), the bias would go in the direction of our finding in November 2009. This bias may be exacerbated by the entrance of a new threat to the incumbent paries – an anti-corruption party like SaS. Therefore, in addition to repeated samples, we also utilize additional items 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. These items are combined with our main perceptual variables into composite sociotropic evaluations. The items tap into similar domains, but taken together should be far less susceptible to political projection: the bias is unlikely to go into the same direction for all actors, and is likely lower for perception of corruption among civil servants compared to those of politicians.²⁸

The upper panel of Figure 4 shows the maximum estimated sociotropic effect from each survey using the composite sociotropic measures: the change in the predicted probability of intended vote for the incumbent for the highest composite perception category relative to the lowest one. The results shown in Figure 3 above are clearly upheld: the sociotropic effect is absent in 2008 and present in 2009. In fact, with the composite measure, the estimated effect in November 2009 data is even stronger than that in the right panel of Figure 3.

[Figure 4 about here.]

We next consider what mechanism may be behind this difference in the sociotropic effect in 2008 and 2009. Recall that the lack of the sociotropic effect in October 2008 comes on the heels of 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). Between October 2008 and November 2009, two other corruption scandals took place (April and August 2009). In Figure

²⁸We either take the average of the different evaluations if they are on the same scale, or create factor scores if they are on different scales. An added benefit of a composite measure is the reduction in potential measurement error.

4, these scandals are represented by the vertical grey lines. But in addition, a new anti-corruption party entered the party system (February 2009) and shortly before the second survey intensified a referendum campaign (launched in June 2009) with a strong anti-corruption agenda. These two events are represented by the vertical dashed red lines, respectively. The evidence presented in Figure 3 in Section 5.2 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 – and clean – anti-corruption party was needed to activate the sociotropic effect. Here, and in the next section, we provide several pieces of evidence in support of this claim.

First, we conjecture that if scandals failed to 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 problems of societal corruption. Our October 2008 data contain a measure of self-reported interest in politics, which we include in our models from Figure 3 along with the interaction with corruption perception. The interaction effect is not statistically different from zero: a sociotropic effect is not present among the respondents reporting higher interest in politics.²⁹

Evidence from repeated samples also helps to rule out two other 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 societal corruption. Related, successive scandals may have been endogenous to attempts of the opposition to use subsequent potentially controversial cases to politicize corruption, which may have trivialized scandal material and lessened its impact (Kumlin & Esaiasson 2012). An observable implication in either case would be that the sociotropic effect diminishes over time – it may be present for earlier scandals and absent for later ones. The upper panel of Figure 4 shows that this is clearly not the case.

A second alternative reason for the lack of sociotropic effect following the scandals could be that the half-life of a corruption scandal may be quite short. Repeated studies in 2008 offer some 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 upper panel of Figure 4 again shows that the estimated effects are nonetheless quite similar.

But what of the claim that it is the new anti-corruption party that raised the salience of societal corruption rather than the 2009 scandals? Here, we provide some indirect evidence, and we show more direct evidence in the next section. First, the August 2009 scandal, which is more proximate to our 2009 surveys, involved the same ministry (Environment) and the same party

²⁹Unfortunately, other data do not contain satisfactory measures of attentiveness to politics in order to re-test this claim.

(Slovak National Party, SNS) as one of the scandals in August 2008. If the 2008 scandal failed to engender the sociotropic effect, the second one is unlikely to do so. Also, both scandals in 2009 were quite similar in nature to all but one scandal in 2008: they involved suspicious public procurement contracts. If such scandals in 2008 failed to elicit the sociotropic effect, there is reason to doubt they would in 2009.

Next, we use the wealth of exit polls conducted in Slovakia between the 2006 election and the 2010 election to estimate how the aggregate vote share of the senior incumbent party (Smer) reacts to our critical events.³⁰ We have 116 exit 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.³¹ Instead, we combine these polls using the approach of Jackman (2005), which takes into account the precision, based on each poll’s sample size and estimated vote share, and the differences in survey methodology employed by each firm. The middle panel of Figure 4 shows the estimated Smer vote share and the associated confidence intervals. Two observations are immediate: during the better half of the period – including most corruption scandals – Smer’s vote share is actually *increasing*; however, the reversal in this increasing trend coincides with the entrance of the new anti-corruption party, Freedom and Solidarity (SaS). Following the SaS’s anti-corruption campaign, Smer’s vote share only declined further. The difference in Smer’s vote share between the high of around 46 percent on the eve of SaS’s entry in February 2009 and the 40 percent vote share at the end of 2009 is statistically significant, as point-wise confidence intervals do not overlap. While this evidence is only suggestive, it is consistent with our account.

Moreover, in the lower panel of Figure 4, we show the standardized Google searches for two relevant categories.³² The dashed grey line represents the searches for several terms denoting corruption in the Slovak language (“korupcia,” “afera,” “skandal,” “kauza”). The black full line represents the searches for the name of the anti-corruption campaign launched by SaS in the summer of 2009 (“Referendum 2009”). The figure shows that the searches for corruption terms are relatively stable and insensitive to the occurrence of the scandals. On the other hand, the spike in the searches for Referendum 2009 clearly corresponds to the actual campaign and shows strong growth during the time of our October and November 2009 surveys. Of course, these are just correlations. We next provide direct evidence by disaggregating the pocketbook and sociotropic effect by party.

³⁰We choose the senior incumbent party for two reasons. First, 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. Second, as we show below, the bulk of the sociotropic vote is estimated to switch away from the senior incumbent, and not the junior incumbent parties.

³¹More details are given in the Web Appendix.

³²The standardized Google searches are expressed relative to the total volume of Google searches at the time. For methodology, see <http://support.google.com/insights/bin/answer.py?hl=en-US&answer=87285>.

6 Disaggregating the Corruption Vote

To estimate the pocketbook and sociotropic effects on *party* choice, we fit a nested logit model (Train 2007). We opt for this model because more standard multinomial choice models, such as multinomial logit, rest on a very restrictive assumption.³³ Nested logit models are not identified if all variables denote attributes of individuals only. Using expert scores of parties in Slovakia (Hooghe et al. 2010) and survey responses to policy-relevant questions, we constructed several party-specific distance measures on issues such as 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.³⁴

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

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

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 nest as well as party choice and vary over each party n within a nest N_k ; γ are the associated parameters.³⁵

³³Multinomial logit assumes the Independence of Irrelevant Alternatives (IIA) – that removing any party from the choice set raises the probability of choosing any other party equally. We are concerned that this assumption is be unreasonable in a multi-party parliamentary democracy such as Slovakia. More importantly, it may be particularly restrictive when the choice set is unstable, as is the case in the period we cover. In the nested logit model, IIA no longer holds (in general) across nests, but is assumed to hold within nests. We let our models define the most suitable nests that parties are grouped into. This is done based on the estimates of the dissimilarity parameters for parties in the three nests that we initially group the parties into: Senior Incumbent, Junior Incumbent, and Opposition. These nests are plausible *ex-ante* because they differentiate parties not only by incumbency/opposition status, but also mark an important difference between left incumbents (Smer), nationalist incumbents (SNS and LS-HZDS), and right mainstream opposition (KDH, SDKU-DS, SMK). These three nests seem to fit the data well in 2008, but less so in 2009, when there is a clear split between the two junior incumbents, possibly because of corruption scandals, which may have affected the two parties differently. In 2009, therefore, there are four nests, with junior incumbent parties representing one nest each.

³⁴Party distance measures are created by standardizing appropriate survey responses as well as expert scores and taking an absolute difference. These distances are not perfect for at least three reasons. First, responses that respondents give are almost invariably on a negative-positive scale (“completely agree” to “completely disagree”), whereas issue positions of parties are on a positive scale only, and so standardized scores of respondents and parties are not necessarily comparable. Second, issue positions are not on the same scale for respondents and for parties; typically, respondents answer on a 4 or 5-point scale, while parties are ranked typically on a 0-6 scale. Moreover, respondent scales are integer-valued, while party scales are not. This difference in range will of course not be eliminated through standardization. Finally, standardization is done separately, and so, voters are standardized with respect to other voters, and parties with respect to other parties. Be that as it may, these measures are indispensable for our analysis below, enter in the right sign, and are significantly different from zero as a group. Results are available upon request.

³⁵The 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.

We present findings only for the non-null effects from the main analysis. We are able to use only the October 2008 and October 2009 data, as they contain satisfactory variables to build party-specific measures. Therefore, in Figure 5, we presents the disaggregated pocketbook results only for October 2008, and the disaggregated sociotropic results only for October 2009. Results are summarized in Figure 5. The left (right) panel shows the change in the probability of choosing a party when shifting from reporting the lowest exposure (perception) to reporting the highest exposure (perception).

[Figure 5 about here.]

Two observations can be made. First, in the case of both the pocketbook and the sociotropic mechanism, vote loss to the incumbent government is concentrated on the senior coalition member (Smer) whereas the junior partners (LS-HZDS and SNS) are basically unaffected. This finding may be interpreted in line with the notion of the clarity of responsibility (Powell & Whitten 1993), to the extent that senior members of the governing coalition are perceived to be the most responsible for policy outcomes (or lack thereof).³⁶

Second, the beneficiaries of the two mechanisms are somewhat different. *Ceteris paribus*, pocketbook votes are redistributed primarily – and relatively evenly – among mainstream opposition parties (KDH, and SDKU-SK) rather than niche parties (SF) or populist parties (the communist KSS).³⁷ On the other hand, the majority of the sociotropic votes seem to be redistributed to the new anti-corruption party (SaS). This result provides further evidence in support of our earlier claim that it is not the scandals but the new party that seems to have activated corruption perceptions. It also highlights the importance of distinguishing between the two mechanisms: personal exposure seems to have *different implications* for vote choice than corruption perception. Recent findings in Eastern Europe suggest that new populists and extremist parties benefit from protest vote (Pop-Eleches 2010). Our findings provide some preliminary refinement of that claim in the context of corruption: only the sociotropic vote fits this pattern of protest vote.

7 Bulgaria Survey Experiment

In addition to issues regarding the causal interpretation of our results discussed above, reliance on observational data makes it hard to rule out concerns such as sample selection, measurement

³⁶Determining the actual role of “responsibility perception” is difficult with these surveys alone, however, since the junior coalition partners at the time were also parties who had engaged in particularly high levels of corruption during previous periods in government (Deegan-Krause 2006) and which may have at that time already shed their corruption-sensitive voters. By 2008 the junior coalition partner Slovak National Party (SNS) relied almost exclusively on ethnic appeals – especially a strong form of anti-Hungarianism – while the junior partner Movement for a Democratic Slovakia (HZDS) relied almost exclusively on voters personal loyalty to the party leader, Vladimir Meciar (Haughton, Novotna & Deegan-Krause 2011).

³⁷Another party with a relatively fixed (and therefore less corruption-sensitive) electorate (SMK, or the Party of the Hungarian Coalition) appears also to benefit from its position in the opposition. This is a somewhat surprising finding for which we have no ready explanation.

error and selection on unobservables. While in the Web Appendix we provide details on several ways we attempt to rule out these problems, the gold standard is experimental analysis (Morton & Williams 2010). As an additional robustness check, therefore, we conducted a pilot survey experiment in Bulgaria during the summer of 2011. Obviously, an experiment in Slovakia would be a better robustness test for results from survey data in Slovakia than an experiment in Bulgaria. That said, given that our goal here is to characterize corruption voting in young democracies more generally, an experiment in Bulgaria is also a convenient preliminary test of the external validity of our theoretical arguments.

The basic scenario concerned being asked whether or not the respondent thought a Bulgarian of the same gender as the respondent would vote for an incumbent mayor of a medium-sized city in a forthcoming election. Two pieces of information about the mayor's current term in office were provided to the respondent: one regarding the state of the economy and one regarding corruption. Respondents were randomly assigned to one of four treatments in a 2X2 experimental design related to this information: half were told the economy had improved and half were told the economy had gotten worse; in addition half were told that the individual in the scenario had been forced to pay a bribe him or herself, while the other half were told that the respondent had heard about allegations of corruption in the city.³⁸ Here is the actual text of the experiment:

[MALE/FEMALE NAME = MATCHING RESPONDENT GENDER] lives in a medium-sized city in Bulgaria. Last month, [NAME] [INSERT1/INSERT2]. The mayor of that city is running for reelection, and in the time since he was originally elected economic conditions in the city have [CONDITIONS1/CONDITIONS2].

INSERT1 = had to spend half of his monthly salary to speed up the approval of permits for his business³⁹

INSERT2 = heard that several city officials have taken bribes in exchange for government contracts

CONDITIONS1 = improved

CONDITIONS2 = worsened

Note that since the experimental vignette does not refer to the respondent personally, we avoid any problems of misreporting of corruption experience that may be present in our observational data. Moreover, since there is no information on the political affiliation of the mayor or political

³⁸As we can not actually assign a subject to be forced to pay a bribe, we instead use this technique whereby we ask the respondent to imagine what another individual would do in this circumstance. The idea is that the "other individual" proxies for the respondent him or herself, which is why we match the gender of the subject in the scenario to the gender of the respondent.

³⁹While this scenario may seem excessive, it is unfortunately quite common in Slovakia (e.g. Anderson 2000), as well as in other Eastern European countries (Anderson & Gray 2006).

history of the town, we alleviate the problem of endogeneity of corruption perception to partisanship.⁴⁰

Using the terminology of this paper, INSTERT1 is intended to capture pocketbook corruption concerns, with INSERT2 picking up sociotropic corruption concerns. Following the question, respondents were asked whether they thought the individual’s vote decision would be “more affected by the changes in the city’s economy or concerns related to corruption.”⁴¹ While our experiment was conducted in a different country, and did not include experimental manipulation of sociotropic corruption salience, we believe that the timing of our experiment approximates reasonably well the conditions in Slovakia in October 2008. Bribe victimization in Bulgaria has been similar to that in Slovakia (see comparison of CEE countries in the Web Appendix). The experiment was conducted outside of the election campaign (the last election was in July 2009), there were no new anti-corruption parties, and no major corruption scandals.⁴²

An additional advantage of our experiment is the inclusion of information on the state of the economy. As mentioned above, in our observational data, we did not have economic evaluations and they are omitted from our specifications. With the experiment, we are able to examine the robustness of our corruption results to this omission. The results are displayed in Figure 6. The panels indicate the shares of respondents answers for each experimental condition. As is evident, regardless of the state of the economy, respondents were approximately ten percent more likely to think that concerns related to corruption would drive the individual’s vote choice following the pocketbook treatment condition compared to the sociotropic treatment condition. These results are statistically significant at a level of $p < .06$. This is in accordance with our Slovakia October 2008 results, although in a setup where we no longer have to worry about endogeneity, misreporting, or selection on unobservables.

[Figure 6 about here.]

Furthermore, we also asked the Bulgarian respondents about their own experiences with bribe victimization and their own perceptions of corruption, with binary (no/yes) responses to both items.⁴³ We then regressed the answer to the experimental question – whether the respondent felt the individual in the scenario would be more influenced by the state of the economy or concerns

⁴⁰In the same survey, a somewhat related survey experiment was ran also involving a hypothetical mayor and a corruption scenario, where respondents were randomly provided with information about the mayor’s party affiliation. A third of the sample received no information on the mayor’s party affiliation, a third was told that the mayor belongs to their favorite party, as elicited previously in the course of the survey, and a third was told that the mayor belonged to their least favorite party. The experiment was designed to measure the effect of party cues on the perception of guilt of the mayor in the face of vague corruption information. We verified that this survey experiment did not contaminate the results presented here, and this was the case. All our results remain unchanged when we control for the scenario the respondent received in this earlier survey experiment.

⁴¹Respondents were also asked whether they thought the individual would vote to re-elect the mayor, and here we were unable to detect a statistically significant difference between the pocketbook and sociotropic corruption cues.

⁴²More details about the context in Bulgaria at the time are given in the Web Appendix.

⁴³These questions were of the same format as those in the Eurobarometer surveys.

related to corruption - on these two variables (personal experience with corruption and perceptions of corruption) along with a rich set of demographic, socio-economic and geographic covariates. Results are presented in Figure 7.⁴⁴ The left panel shows the results for the entire sample, whereas the middle and the right panel show results when we split the sample into those respondents who received the sociotropic and the pocketbook scenario, respectively. All three panels show the change in the predicted probability of choosing corruption as central to the vote when a typical respondent switches from answering “no” to answering “yes” to the exposure or the perception item.

[Figure 7 about here.]

Two conclusions are immediately apparent. First, the left panel shows that personal exposure to corruption makes respondents more likely to think the subject’s vote choice will be motivated by concerns over corruption (by about 10 percent) than a perception that corruption is a problem in Bulgaria, the effect of which is not significantly different from zero. Second, the middle and the right panels show that this effect occurs *only following the bribe victimization prime*. Taken together, both findings are again consistent with our results in Slovakia from October 2008 increasing our confidence of their validity.⁴⁵

8 Way Forward

Our hope is that this paper will serve as an initial step to invigorate what we feel is a potentially rewarding - and substantively important - new subfield of political behavior: 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 extremely 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 mechanisms by 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 their level of certainty. Since certainty about bribe victimization

⁴⁴Results in the table format are presented in the Web Appendix.

⁴⁵One important caveat is in order. A very large proportion of Bulgarians (84 percent) strongly agree (our highest category) that corruption is a problem in Bulgaria. So it is possible that we lack sufficient variation in this variable to pick up an effect that a more highly sensitive variable might.

is inherently high, and exposure in the aggregate is slow-changing, pocketbook corruption voting is expected to be stable. Certainty about societal corruption is inherently low, but it may be increased in the aggregate electorate through 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 seem to find that the myriad scandals present in Slovakia lead to protest vote based on corruption perception. The importance of scandals for electoral outcomes may have been *overstated* in the previous literature.

Finally, *methodologically*, we have introduced an experimental design for testing the relative importance of *pocketbook* and *sociotropic corruption voting*. These experiments can be conducted in additional countries moving forward both to continue to test the external validity of our findings here, as well as to test more comparatively oriented hypotheses.⁴⁶

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. Interesting links could also be developed in this regard with recent work by Egan & Mullin (2011), who find that experiencing different weather patterns affects how people think about global warming.

Methodologically, we know that there will always be concerns with the use of observational data to study voting behavior, and that experiments represent a valuable tool for addressing some of these concerns (e.g., endogeneity, misreporting experience, selection on unobservables) while of

⁴⁶Such comparisons could be cross-national, such as running experiments in countries with different baseline levels of corruption, or over-time in a single country, thus allowing for further tests of our salience-related arguments

course raising other concerns (e.g., external validity). Here, we join a small but growing number of papers that use experiments to supplement results from observation data, hopefully strengthening our confidence in both sets of findings as a result. Nevertheless, the experiment contained here can certainly be improved upon and replicated in additional settings.⁴⁷ In particular, we currently only contrast two “negative” examples of corruption – in essence pitting pocketbook *vs.* sociotropic corruption concerns – but do not compare either against “positive” (or even neutral) reports/experiences regarding 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 and Bulgaria remains an important, if obvious, 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.

⁴⁷See the discussion in this regard in *The Monkey Cage*: <http://themonkeycage.org/blog/2011/08/13/request-for-feedback-on-experiment-corruption-and-voting/>.

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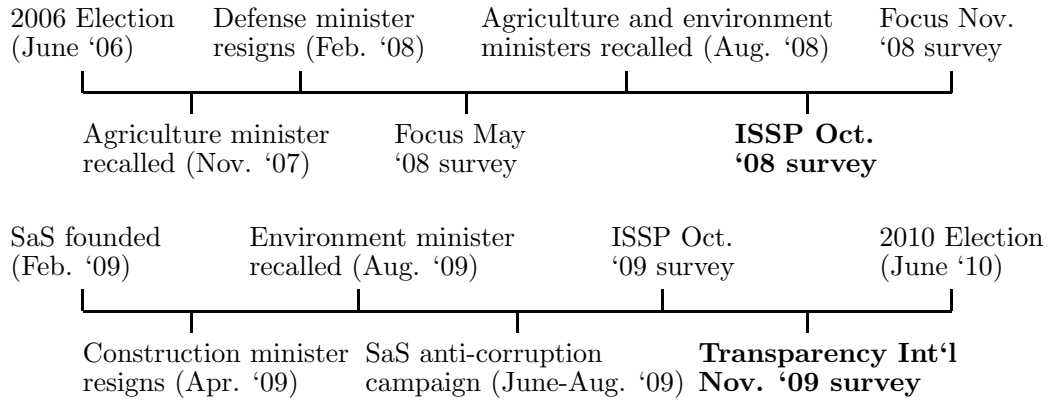
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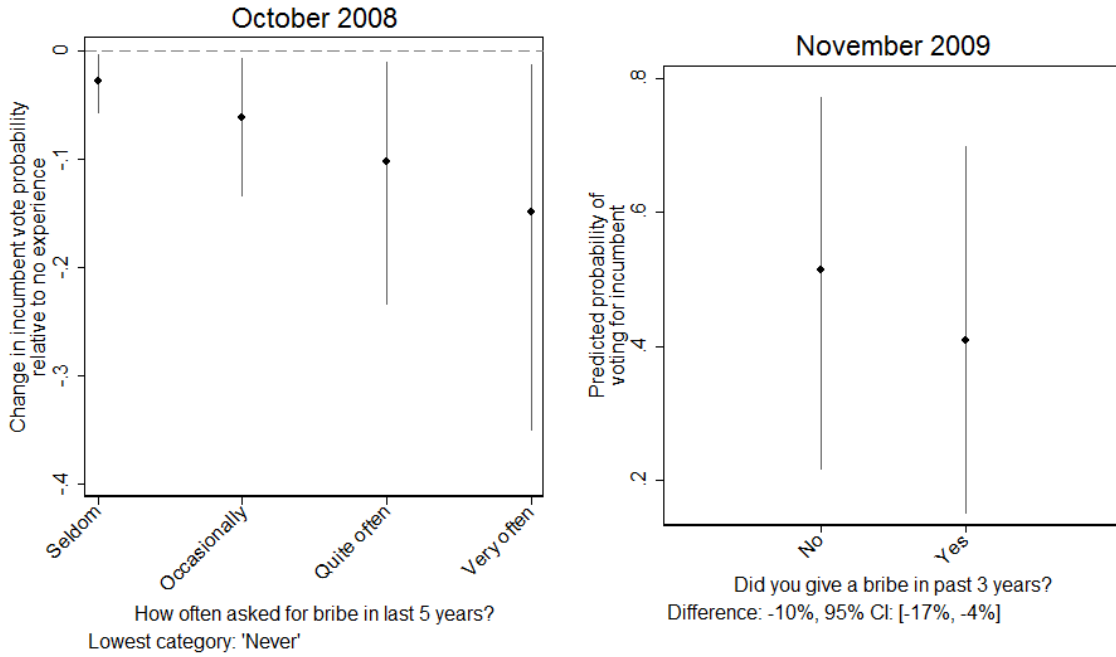
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Figure 1: Data and timeline of events in Slovakia



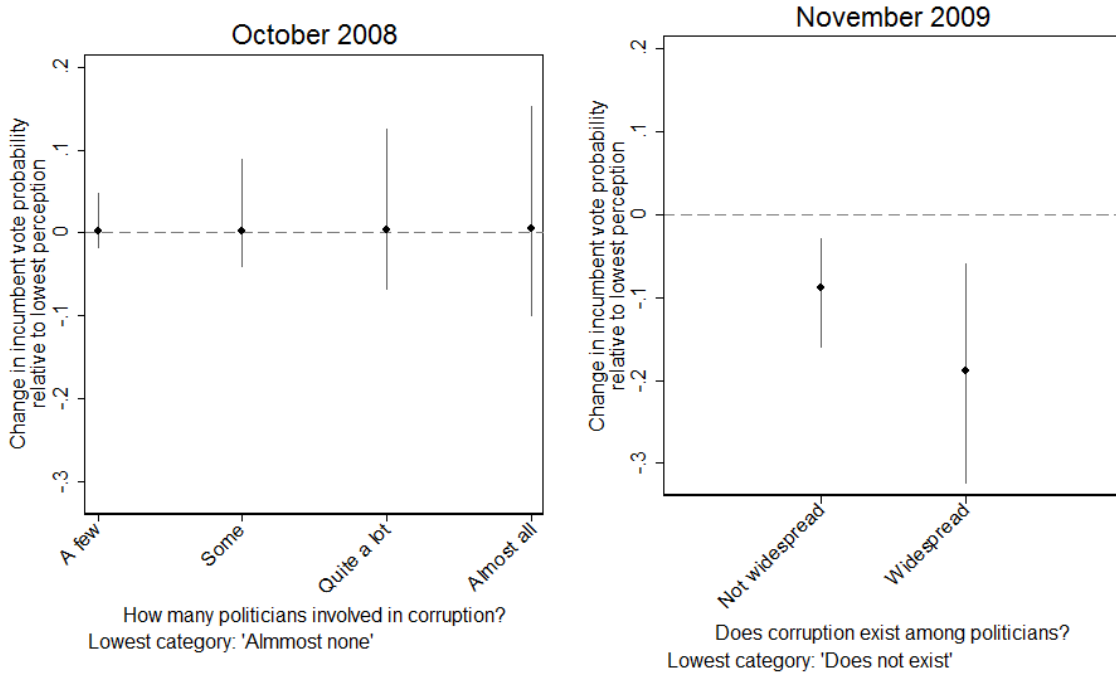
Main data are marked in bold face. Details about the events and the variables available in the data are given in the Web Appendix.

Figure 2: Corruption experience and intended incumbent vote choice



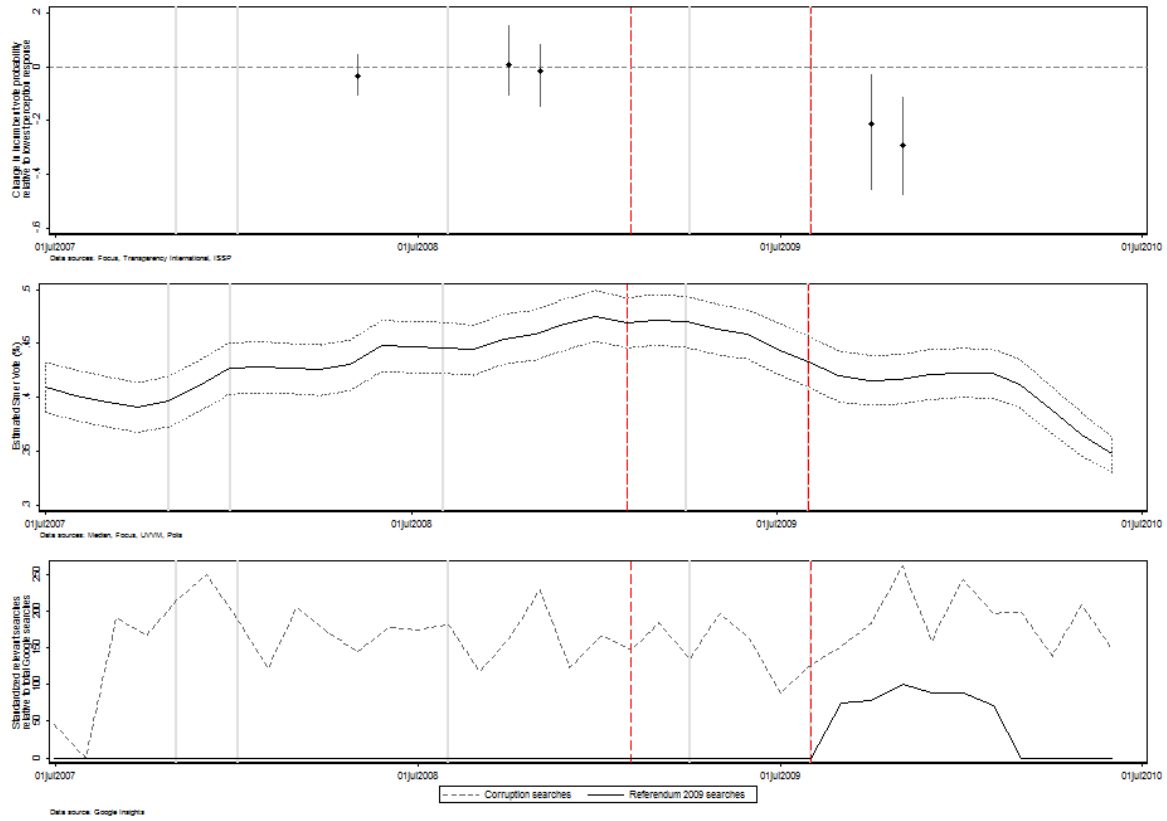
In the left panel, the dots represent the maximum likelihood estimates of the change in the predicted probability of the intended vote for the incumbent relative to the lowest response category. The caps are the 95 percent confidence intervals based on 1,000 simulations. In the right panel, the dots are the maximum likelihood estimates of the predicted probability of the intended vote for the incumbent for a typical respondent who has not and has been asked for a bribe, respectively. In both panels, all other variables in the model are kept at their medians or modes.

Figure 3: Corruption perception and intended incumbent vote choice



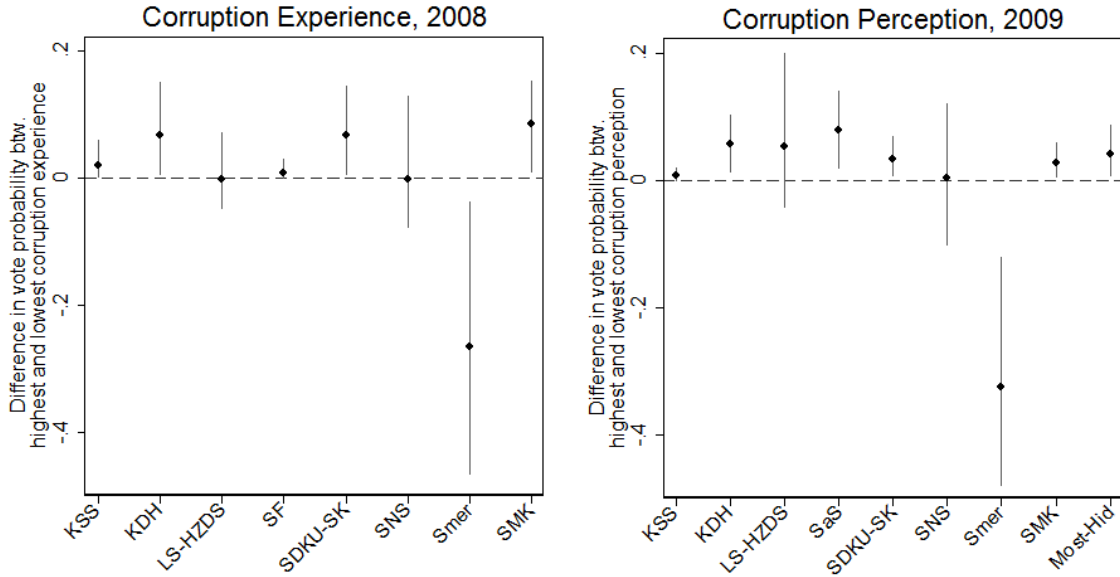
In both panels, the dots represent the maximum likelihood estimates of the change in the predicted probability of the intended vote for the incumbent relative to the lowest response category. The caps are the 95 percent confidence intervals based on 1,000 simulations. All other variables in the model are kept at their medians or modes.

Figure 4: The sociotropic effect over time and the relevant events



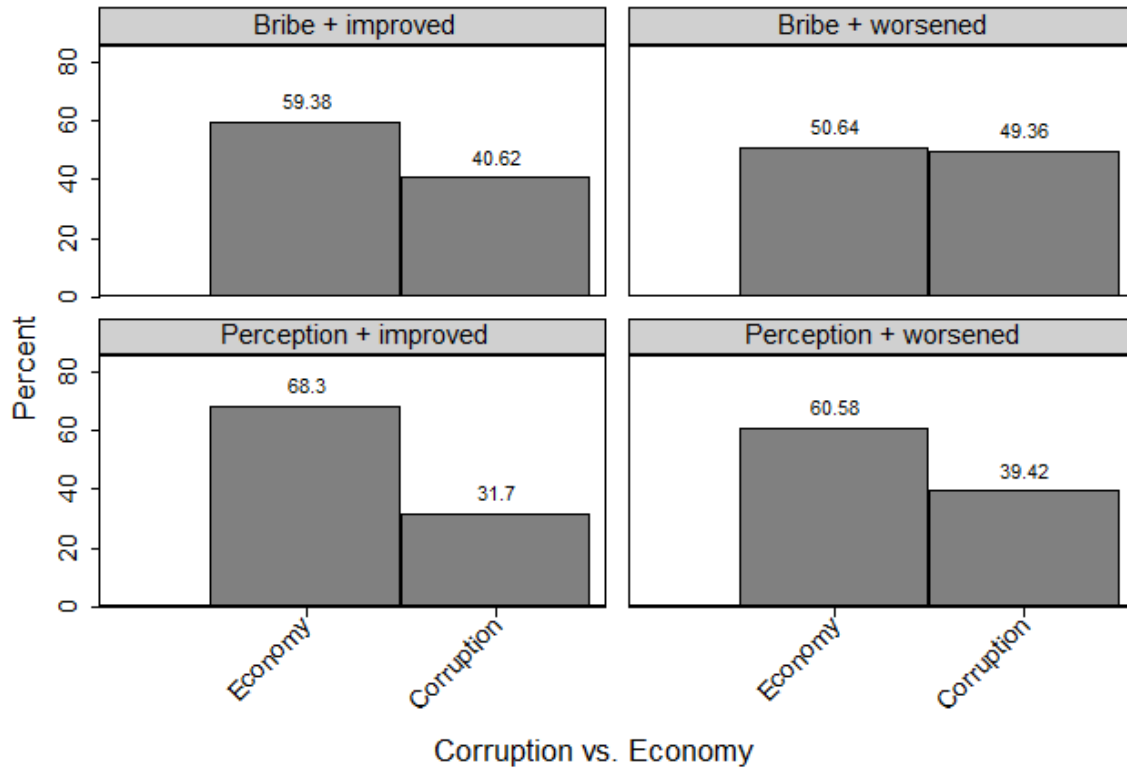
In all three panels, vertical grey lines represent the timing of each corruption scandal; vertical dashed red lines represent the entry of the new party and the approximate timing of its anti-corruption campaign, respectively. In the upper panel, the dots represent the maximum likelihood estimates of the change in the predicted probability of intended vote for the incumbent for the highest composite perception category relative to the lowest one. The caps are the 95 percent confidence intervals based on 1,000 simulations. All other variables in the model are kept at their medians or modes. Composite corruption perception measures are obtained by combining responses to items probing perception of politicians, public officials and civil servants from various government agencies. In the middle panel, the the full black line represents the estimated monthly vote share for the senior incumbent party Smer. The dashed lines are the 90 percent confidence intervals. The estimates are based on the 116 exit polls conducted by the Slovak polling firms Focus, Median, Polis, and UVVM. The estimates are obtained using the approach of Jackman (2005). Finally, in the lower panel, plotted lines represented the standardized Google searches relative to the total volume of Google searches at the time (for methodology, see <http://support.google.com/insights/bin/answer.py?hl=en-US&answer=87285>). The dashed grey line represents the searches for several terms denoting corruption in Slovak language (“korupcia,” “afera,” “skandal,” “kauza”). The full black line represents the searches for the name of the anti-corruption campaign (“Referendum 2009”).

Figure 5: Corruption experience, perception, and intended party vote choice



Dots represent the maximum likelihood estimate of the change in the probability of choosing a party when shifting from reporting the lowest exposure to reporting the highest exposure to corruption in October 2008 (left panel), or from reporting lowest perception to reporting highest perception of corruption in October 2009 (right panel). Caps are 95 percent confidence intervals based on 1,000 simulations. Other variables in the model are kept at their medians or modes.

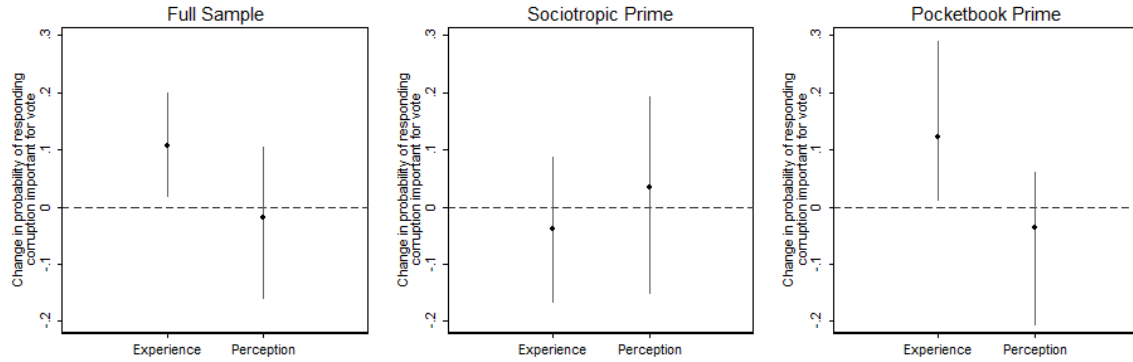
Figure 6: Corruption vs. economy as determinants of vote



Graphs by Exp. condition

The four charts correspond to the four treatment conditions presented above. The upper (lower) panel charts represent the distribution of responses following the pocketbook (sociotropic) prime; left (right) panel charts represent the distribution of responses following the “improved” (“worsened”) economy prime. All shares are weighted by the survey weights.

Figure 7: Experience and Perception as determinants of experimental response to corruption and economy



All three panels show the change in the predicted probability of choosing corruption as being central to the vote when a typical respondent switches from answering “no” to answering “yes” to the experience or the perception survey item. The left panel shows the results for the entire sample, whereas the middle and the right panel show results when we split the sample into those respondents who received the sociotropic and the pocketbook scenario, respectively. Caps are 90 percent confidence intervals based on 1,000 simulations. We use 90 percent confidence intervals because of relatively low power due to small sample size.

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)	.
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)

Notes: 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 with 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 item wording and different country samples. The entries in the parentheses represent the 95 percent confidence intervals calculated by using Fisher's z transform. 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 dependent-variable models are qualitatively similar and are available upon request.