

# Web Appendix for “Pocketbook vs. Sociotropic Corruption Voting”

February 15, 2014

This Web Appendix accompanies the paper.<sup>1</sup> Section 1 compares corruption perception and experience in Eastern and Western Europe, while Section 2 compares Slovakia to other Eastern European countries. Section 3 gives details of the events in Slovakia we refer to in the paper. Sections 4, 5 and 6 provide the sources for the data and variables used in the analysis. Section 7 presents full results of the main models as well as the main robustness checks discussed in the paper. Finally, Section 8 details several other robustness checks briefly mentioned in the paper.

## 1 Corruption in Eastern vs. Western Europe

As mentioned in the paper, based on the Eurobarometer and Transparency International’s Global Corruption Barometer data, we estimate that a typical East European citizen is significantly more likely to perceive their politicians as corrupt and to report having been asked for a bribe than their West European counterpart. These estimates are presented in Table A1.

Table A2 shows that citizens in Eastern Europe report spending considerably more of their income on bribes than citizens of Western Europe.<sup>2</sup>

## 2 Slovakia vs. Eastern Europe

Based on Eurobarometer data, Figure A1 compares Slovakia to other Eastern European countries in terms of reported bribe exposure and corruption perception. Slovakia is fairly typical of the region on both dimensions.

## 3 Description of Context

Our analysis mainly covers the period from 2006 until 2010. Here, we provide a brief description of the political context in Slovakia over this time period.

Slovakia’s parliamentary elections in 2006 ended the nearly eight-year term of pro-market Prime Minister Mikuláš Dzurinda. The primary opponent of the government position – and the

---

<sup>1</sup>Replication archive is available at [https://files.nyu.edu/mk3296/public/files/publications/pocket\\_replication.zip](https://files.nyu.edu/mk3296/public/files/publications/pocket_replication.zip)

<sup>2</sup>Indirect estimates of aggregate costs of administrative corruption in Eastern Europe further suggest that bribing is a considerable phenomenon (Gorodnichenko and Peter, 2007; Gokcekus and Muedin, 2008).

overwhelming victor in the election with 29 percent (11 percentage points more than its nearest competitor, Dzurinda's SDKÚ) – was a party known as Direction (Smer). Smer emerged on Slovakia's political scene in 1999 as a splinter of the former-communist Party of the Democratic Left and in the 2002 elections profiled itself as an anti-corruption party.

After its election victory in 2006, Smer-SD formed a coalition with Vladimír Mečiar's Movement for a Democratic Slovakia (HZDS) and the strongly nationalist Slovak National Party (SNS). The coalition reversed some of the Dzurinda government's reforms, particularly on pension and labor law but also actively pursued adoption of the euro and made only relatively minor changes to the flat tax.

During its tenure, the government faced a series of scandals concerning cabinet ministers, particularly in ministries held by HZDS and SNS. In November 2007, HZDS-nominated Minister of Agriculture Miroslav Jureňa was recalled due to allegations of underpriced transactions to a HZDS-connected firm from the Slovak Land Transfer Fund. Smer-nominated Defence Minister František Kašícký then resigned in February 2009 for overpriced maintenance services contracted in an invalid tender procedure. In August 2008, SNS-nominated Minister of the Environment Jaroslav Izák resigned because of a suspicious sale of Slovakia's carbon emissions quota at a steep discount to an obscure U.S.-based company. In the same month, another HZDS-nominated Minister of Agriculture, Zdenka Kramplová, was recalled because of her role in accepting illicit party contributions.<sup>3</sup> In April 2009, SNS-nominated Minister of Construction and Regional Development Marian Janušek was recalled for two overpriced tenders going to firms suspected of ties with his party. For a similar charge, another SNS-nominated Minister of the Environment, Viliam Turský, was removed after just three months in office in August 2009.<sup>4</sup> For our purposes, it is important to note that all of these scandals were of financial nature; i.e. none were of more ambiguous nature such as sex scandals. Corruption scandals were not restricted to this electoral cycle, however. Revelations about a wider series of related scandals named after an alleged file known as "Gorilla," have been perceived by political observers to have played an important role in the 2012 election.<sup>5</sup>

In February 2009, Richard Sulík, a former economic advisor to Dzurinda's government and one of the architects of Dzurinda's tax legislation, formed a new liberal party, Freedom and Solidarity (SaS). Beginning in late June 2009, SaS began a campaign for a set of six referendums (known as the "Referendum 2009" Initiative) that included questions of taxation (removing mandatory fees for state television), freedom of expression (removing restrictive parts of the 2008 press law), and franchise (internet voting), as well as three questions related directly to accusations of political corruption (removing parliamentary immunity, reducing the number of deputies by a third, and forbidding the purchase of expensive cars for state use). The campaign to gather signatures coincided with the party's campaign for the European Parliament elections of 2009. Although SaS did not cross the five percent threshold in the EP election, it came very close (4.7 percent) and established itself as a potential viable candidate for the country's next parliamentary election which

<sup>3</sup>The party itself recalled a third Minister in September 2009, ostensibly for not providing enough benefit to the party. See: [http://spectator.sme.sk/articles/view/36495/2/agriculture\\_minister\\_becik\\_resigns.html](http://spectator.sme.sk/articles/view/36495/2/agriculture_minister_becik_resigns.html).

<sup>4</sup>For an overview of ministerial changes in Robert Fico's government, see: [http://195.46.72.16/free/jsp/search/view/ViewerPure\\_en.jsp?Document=.%2F.%2FInput\\_text%2Fonline%2F09%2F07%2Ftbbt73b081149.dat.1%40Fondy&QueryText=](http://195.46.72.16/free/jsp/search/view/ViewerPure_en.jsp?Document=.%2F.%2FInput_text%2Fonline%2F09%2F07%2Ftbbt73b081149.dat.1%40Fondy&QueryText=). Just before the 2010 election, another scandal involving Smer broke out, when a tape was released recording a voice similar to Prime Minister Fico's, discussing secret funds raised for the 2002 election campaign. This allegation was quickly shown to be fabricated. See for example [http://spectator.sme.sk/articles/view/39180/2/daily\\_publishes\\_a\\_recording\\_resembling\\_ficos\\_voice.html](http://spectator.sme.sk/articles/view/39180/2/daily_publishes_a_recording_resembling_ficos_voice.html).

<sup>5</sup>See for example: <http://www.tol.org/client/article/23042-slovakia-elections-dzurinda.html>.

could legally take place no later than June of 2010. Bolstered by the EP election results, the party intensified the Referendum 2009 campaign in August 2009. The party's support in the polls rose above the five percent threshold for the first time in December 2009, and it received 12.1 percent of the vote in the June 2010 election. In its campaign the party unambiguously positioned itself as an uncorrupt (and uncorruptable) alternative both to the left- and nationally-oriented parties of the then government and to the parties of the right.<sup>6</sup> SaS received four cabinet posts and became the part of the governing coalition following the election. In September 2010, it organized a formal referendum based on the Referendum 2009 Initiative. All six proposals received support in excess of seventy percent, but the referendum failed due to low turnout (23 percent).

## 4 Individual-level Surveys and Variable Availability

Table A3 lists the surveys used as main and auxiliary data in the paper, and key variable availability for each survey.

## 5 Aggregate Polls

Table A4 provides details on the aggregate opinion polls used in the paper.

## 6 Media Sources

We searched media sources covering Slovak politics on Factiva and Lexis-Nexis in the period July 2006-June 2010. To construct our measure of media coverage of corruption, we divided the monthly count of articles about corruption with the monthly count of articles that contain a reference to the Slovak Government or Prime Minister Fico. To find articles about corruption, we used the following strategy. For individual scandals, in addition to common words denoting corruption, we searched by the name of the minister involved in the scandal, as well as the appropriate key word that most likely identified the scandal. For example, the April 2009 scandal involving the Minister of Construction and Development Marian Janušek was investigated by the Public Procurement Office (Úrad pre verejné obstarávanie) whose acronym (ÚVO) we included in the search. We manually checked the content of a random subset of articles, confirming that we did not get many false positives or false negatives. For articles about corruption unrelated to the scandals, we searched for the mention of one of the standard words denoting corruption, but without the mention of any of the names or institutional acronyms we used in the search for scandal-related coverage.<sup>7</sup> Finally, using filters provided by Factiva and Lexis-Nexis, we confined the articles mentioning corruption to those referencing Slovakia rather than a foreign destination.

The sources used were as follows. From Factiva: SITA Slovenska Tlacova Agentura, Vyber správ zo Slovenska, SME Online, Bleskovky, Pravda, Hospodarske Noviny Online, 24 Hodin, The Slovak

<sup>6</sup>During this same period another new party emerged within Slovakia's Hungarian community: Bridge (Most-Híd). This party also made some efforts to attract new voters and critique the other major Hungarian party as too entrenched, but while such appeals appeared in its campaign they were a relatively small part of the party's overall appeal which focused primarily on the return of a popular former Hungarian party leader, Béla Bugár.

<sup>7</sup>In Slovak, the search terms were: kauz\*, klientiliz\*, korupc\*, netranspar\*, skandal\*, škandal\*, sprenever\*. In English, we used: corrupt\*, scandal\*, allegation\*.

Spectator, Slovo, SME Blog, and HNblogy. From Lexis-Nexis: CTK National News Wire, Esmerk Slovak Republic News, News Bites - Central and Eastern Europe: Slovakia, SeeNews Slovakia, Slovakia This Week (IntelliNews), and Slovakia Today (IntelliNews). Together, they provide a balanced coverage of views in the Slovakian political space.

## 7 Results and Robustness Checks

### 7.1 Individual-Level Results

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 non-negligible 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.<sup>8</sup> 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 and 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. The full list of variables for each survey is available upon request. All scalar quantities and parameter estimates reported in the paper and below are combined across the five imputed datasets using Rubin’s rules (Rubin, 1987).<sup>9</sup>

Table A5 gives the coefficient estimates for the individual-level results discussed in the paper.

### 7.2 Aggregate-Level Results

Results underpinning Table 6 in the paper are given in Table A6. The full specification is as follows:

$$\begin{aligned} \text{Vote share}_t = & \sum_j \alpha_j \text{Vote share}_{t-j} + \sum_{p \times k_p} \beta_{p \times k_p} \text{Scandal}_{p,t-k_p} + \gamma_l \text{SaS entry}_{t-l} \\ & + \delta_m \text{SaS campaign}_{t-m} + \sum_{q \times n_q} \psi_{q \times n_q} \mathbf{X}_{q,t-n_q} + \epsilon_t. \end{aligned} \quad (1)$$

<sup>8</sup>For the definition of the types of missingness mechanisms, see Rubin (1987).

<sup>9</sup>In the Transparency International surveys, one of the response categories to the perceptual questions is “Corruption exists, but I can’t choose the level of spread.” The reader may wonder how we treated this category with respect to imputation. We dealt with it in the following way. In the imputation stage, we created a dummy which equals one if the respondent chose this category, to differentiate this response from the other missing category (“NA”). We then recoded the original “Can’t choose” responses to missing. We subsequently imputed these responses along with other missing data using the “can’t choose” dummy as one of the predictors. Also, we constrained the imputed values to only one of two possible categories: “Corruption exists, but is not widespread,” or “Corruption exists and it is widespread.” The third available category is “Does not exist,” but this category would be inconsistent with the original response. In the analysis stage, we then used two approaches. The results for sociotropic corruption voting shown in the paper were obtained using the imputed data. However, to cross-validate our results, we also reran the analysis (a) with the “can’t choose” dummy included on the right-hand side, and (b) with the original unimputed data. The results are substantively unchanged.

That is, we estimate an auto-distributed lag model on monthly data, with  $p = 5$  scandal dummies<sup>10</sup> and  $q = 7$  control variables in  $\mathbf{X}$  comprising of monthly inflation, unemployment rate, and industrial production, dummies for the European Parliament election (June 2006), local election (December 2006), presidential election (March 2010) and the beginning of the general election campaign (May 2010). We start from a general model with a flexible lag structure that allows each variable to have its own relevant number of lags (De Boef and Keele, 2008). After trial and error, the most appropriate model has  $j \in (1, 2)$ ,  $k_p = 0 \forall p$ ,  $l = 0$ ,  $m \in (0, 1)$ ,  $n = 0$  for the election dummies and unemployment,  $n \in (0, 1)$  for the other two monthly economic series, and  $n \in (0, 2)$  for the general election campaign dummy. The unit-root tests show that the monthly vote share is not stationary, whereas the first-differenced vote share is. We ran all models on the first-differenced vote share and its lag, but the results are nearly identical to the specification with the level of the monthly vote share and its two lags. We opt for the latter because of ease of interpretation.

The specification used for results presented in Table 7 and Figure 2 in the paper is as follows:

$$\begin{aligned} \text{Media coverage}_t = & \beta_0 + \beta_1 \text{Scandals} + \beta_2 \text{Elections} + \beta_3 \text{SaS entry} + \\ & + \beta_4 \text{Scandals} \times \text{SaS entry} + \beta_5 \text{Elections} \times \text{SaS entry} + \sum_j \gamma_j t^j + \epsilon_t. \end{aligned} \quad (2)$$

Unlike in the model in equation 1, where *SaS entry* is a dummy variable, in equation 2 it takes on a value of zero before February 2009, and one thereafter. This is a structural break model, and the hypothesis test on  $\beta_3$  represents the standard Chow test of whether *SaS entry* represents a structural break in the media coverage of corruption (Chow, 1960). The variable *Scandals* takes the value of one whenever there was a scandal, and zero otherwise; ditto for *Elections*.<sup>11</sup> Coefficients  $\beta_1$  and  $\beta_2$  thus give the effect of scandals and election campaigns (local, presidential, and the election for the European Parliament), whereas coefficients  $\beta_4$  and  $\beta_5$  indicate whether any structural break caused by *SaS entry* also changes the effect of scandals and elections. The model also includes a (third-degree) polynomial in time, in order to flexibly control for any time trend in the media coverage of corruption. The results are unchanged when we include lags of media coverage, or use a higher-order polynomial in time. The media coverage series is stationary under the standard as well as structural-break consistent unit-root tests.

## 7.3 Robustness Checks

### 7.3.1 Selection on Unobservables

Bribe victimization and perceptions are distributed in the population in ways that are correlated with personal characteristics that themselves determine vote choice. To the extent that some of these characteristics are not included in the model, parameter estimates will suffer from the classic problem of omitted variable bias. To reduce the risk of omitted variable bias, we saturate our baseline – and parsimonious – October 2008 specification with a rich set of controls. Coefficients for corruption experience and corruption perception, shown in Table A7, are quite similar and substantively unaffected by the inclusion of a range of additional demographic, socio-economic,

<sup>10</sup>We include five rather than six scandals because the effect of the scandal in August 2009 cannot be identified separately from the *SaS* referendum campaign taking place at the same time.

<sup>11</sup>This too is different from the specification in equation 1, where each scandal and election had a separate dummy variable. The difference is driven by the type of test performed.

and geographic controls, measures of political interest, efficacy, and attitudes towards a wide range of policy issues: taxation, regulation of business, redistribution, security, social liberalism, and the role of religion in politics. Moreover, we demonstrate that the previous vote is a powerful control. Adding other controls adds very little explanatory power. In fact, the last two rows of the table show that the added variable as a group are not jointly significantly different from zero when the previous vote is included. On the other hand, our key results do not change much when we keep the added variables but remove the previous vote variable. This suggests that in the surveys where we do not have the previous vote, we are able to proxy for it well with demographic, socio-economic and geographic controls. We get similar results, available upon request, for May and November 2008, and June 2009. The specifications for May 2004, March 2008, November 2009 and December 2011 already exhaust all available non-corruption variables from the source data.

### 7.3.2 Partisan Bias in Bribe Victimization

Another important concern is the potential endogeneity of bribe victimization to partisan affiliation: our main pocketbook results are consistent with an alternative argument that individuals who are less likely to vote for the incumbent parties are more likely to be victimized *because of their partisan affiliation*. We believe that this is unlikely because the ballot is undeniably secret in Slovakia (OSCE/ODIHR, 2010) and clientelism is not prominent (Kitschelt and Kselman, 2011). Nevertheless, we provide three additional 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. However, as shown in Table A7 above, results are unchanged when we include in our specifications regional dummies, thus looking at the effect of bribe exposure only within these territorial units. 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.<sup>12</sup> While the coefficient on previous incumbent vote is negative, it is statistically significantly different from zero only at  $p < 0.26$ . Part of this correlation, however, derives from a strong relationship between previous and intended vote choice, which in turn – as shown above – is strongly related to bribe victimization. When we control for voting intention, significance for previous incumbent vote is found only at a level of  $p < 0.70$ . Third, in our other work in other countries (Klašnja and Tucker, 2013), where we used survey experiments approximating corruption experiences studied here but where endogeneity was removed by construction, we still get statistically significant pocketbook corruption effects.<sup>13</sup>

### 7.3.3 Partisan Bias in Corruption Perceptions

As in the case of bribe exposure, reliance on observational data makes it difficult to rule out the possibility of endogeneity of perceptions due to partisanship. If voters who intend to vote for the incumbent are less likely to report high corruption perception and vice versa (Anderson and

<sup>12</sup>We are able to do this only for 2008, where we have information about previous vote choice.

<sup>13</sup>A related endogeneity concern is that individuals who are less likely to vote for the incumbent parties may be more likely to report having been asked for a bribe even when they had not. The survey experiments in our experimental work remove this concern as well and give strong evidence of pocketbook effects.



Tverdova, 2003), the bias would go in the direction of our findings in June, October, and November 2009. We offer three observations we believe alleviate such concerns. First, the presence of a stable bias is inconsistent with our main results. The bias from partisan perception must have changed between 2008 and 2009. This is not impossible, as the bias may have been exacerbated by the entrance of a new threat to the incumbent parties, such as an anti-corruption party like SaS. However, our composite measure of corruption perception contains questions that probe corruption perception of actors other than national politicians, such as regional and local politicians, and civil servants in health, judiciary, education, police, etc. While all the questions tap the perception of societal corruption, they cover distinct domains which taken together should be less susceptible to political projection: the bias is unlikely to be in the same direction for all actors, and is likely lower for perception of corruption among civil servants compared to those of politicians. Finally, as with corruption exposure, we have found strong evidence of sociotropic corruption voting in our other work using survey experiments in Sweden and Moldova where the possibility of partisan perception was eliminated by construction (Klašnja and Tucker, 2013).

### 7.3.4 Sensitivity of Sociotropic Effect to Omission of Corruption Experience

Next we consider the potential magnitude of any bias in our estimates of the effect of corruption *perception* on voting behavior using datasets without a measures of *exposure* to corruption. To reiterate, in the paper we are interested in the following relationship:

$$Vote_i = F\{\beta_0 + Exposure_i\beta_1 + Perception_i\beta_2 + \mathbf{Z}_i\beta_3 + \epsilon\}, \quad (3)$$

where  $F$  is the “link function,” and  $\mathbf{Z}_i$  is a vector of potential confounders included in the model. In our estimation,  $F$  is the logit function, given that  $Vote_i$  is a binary 0/1 variable. Suppose instead that  $F = 1$ , i.e. that we are estimating a linear probability model.<sup>14</sup>

In some of our survey data we do not have items probing corruption exposure, and so we are forced to omit  $Exposure_i$  from the model in equation (3). Therefore, we are estimating:

$$Vote_i = F\{\alpha_0 + Perception_i\alpha_1 + \mathbf{Z}_i\alpha_3 + \epsilon\}. \quad (4)$$

Compared to the estimator in (3), parameter estimates in (4) are potentially biased because of the omission of the measure of exposure. It is straightforward to characterize this bias. Let  $\mathbf{W}$  be a matrix containing  $Perception_i$  and  $\mathbf{Z}_i$ . Then, the parameter estimates for model (4) are:  $\hat{\alpha} = (\mathbf{W}'\mathbf{W})^{-1}\mathbf{W}'Vote_i$ . Substituting the model in (3) for  $Vote_i$  and taking expectations, we get:  $\alpha = \beta + (\mathbf{W}'\mathbf{W})^{-1}\mathbf{W}'Exposure_i\beta_1$ , where  $\alpha$  and  $\beta$  are the values of the population parameters in models (3) and (4), respectively. Notice that the expression  $(\mathbf{W}'\mathbf{W})^{-1}\mathbf{W}'Exposure_i$  gives the parameter estimates of the following model:

$$Exposure_i = F\{\gamma_0 + Perception_i\gamma_1 + \mathbf{Z}_i\gamma_3 + \xi\}. \quad (5)$$

Therefore, in expectation, the adjustment for the bias from the omission of the measure of exposure can be characterized as follows:

$$\delta = \alpha - \gamma\beta_1,$$

---

<sup>14</sup>Conditional on the model being correctly specified, this change in the functional form should not impact the unbiasedness of the parameter estimates, even though it creates classical problems of non-normal errors, unbounded predicted values and conditional heteroskedasticity. In the exposition that follows, we assume that the model is correctly specified and proceed with an OLS model.

where  $\delta$  denotes our parameters of interest: the adjusted parameters  $\alpha$ , thus approximating  $\beta$  for elements of  $\mathbf{W}$ ;  $\gamma$  are the values of the population parameters from equation (5).

Our data allow us to estimate all the necessary parameters in order to get  $\hat{\delta}$ . We use the surveys for which we are able to run the full model (3) to estimate  $\beta_1$  and  $\gamma$ , whereas estimates of  $\alpha$  are obtained from the surveys containing only the corruption perception variables. In order to make the parameters comparable, we standardize the perception and bribe variables across all the datasets, and use the same set of covariates  $\mathbf{Z}$ .<sup>15</sup> We perform the adjustment only within the same year: we use only October 2008 data for adjustments in our estimation on Focus May and November 2008 data; similarly, we use only November 2009 data to adjust our estimates on ISSP October 2009 data. As a cross-check, however, we also use Eurobarometer surveys (see table A3 above) as supplemental data to our main surveys for the estimates of  $\gamma$ .

$\hat{\beta}_1$ , or the effect of corruption exposure on incumbent vote, is negative. Recall from Section 2.1 in the paper (see Table 1) that  $\hat{\gamma}_1$ , or the partial relationship between corruption exposure and corruption perception, is positive. From the expression for bias adjustment, it can then be readily observed that  $\hat{\delta}_1$ , or the adjusted estimate of the effect of corruption perception, will be *larger* in absolute terms than the unadjusted estimate. In other words, our unadjusted estimate is an *overestimate* of the effect of corruption perception on incumbent vote. Note, however, that the *size* of the bias in the partial effect of corruption perception depends on the product between  $\hat{\gamma}_1$  and  $\hat{\beta}_1$ . We have shown in Table 1 in the paper that the partial correlation between exposure and perception is quite close to zero. Since  $\hat{\gamma}_1$  depends on  $Cov(Exposure, Perception|\mathbf{Z})$ , we expect the bias from omission of corruption exposure to be relatively small.

This is indeed what we observe in Table A8. Since our estimate of interest is the (properly adjusted) effect of corruption perception on vote choice, we compare only the unadjusted and adjusted marginal effects of corruption perception for each survey, rather than the whole set of unadjusted and adjusted estimates. Like in the figures in the text, we show the *ceteris paribus* change in the predicted probability of incumbent vote for a typical respondent from a one-standard deviation change in perception.

Table A8 shows that the adjusted results are not very different from the unadjusted ones. Crucially, substantive interpretation of the effects does not change.<sup>16</sup>

### 7.3.5 Further Evidence of Lack of Scandal Effects

In the paper, we use aggregate monthly data to claim that sociotropic corruption voting was brought about by the entrance of a new party rather than corruption scandals. Here, we look at individual data to provide further adjudication between the effects of scandals and the entry of SaS.

First, if scandals failed to sufficiently raise the salience of corruption, we should find no statistically significant variation of the sociotropic effect across different levels of respondents' attentiveness to politics. If even the more attentive respondents do not engage in sociotropic corruption voting in the face of multiple scandals (controlling for other factors), scandals were unlikely to have been sufficiently salient signals of the importance of societal corruption. The October 2008

<sup>15</sup>In 2008,  $\mathbf{Z}$  consists of a variable measuring respondents' previous vote choice. In 2009,  $\mathbf{Z}$  consists of variables describing respondents' gender, age, nationality, income, occupation, education, size of municipality and administrative region of residence. All covariates in  $\mathbf{Z}$  across appropriate data are identically coded.

<sup>16</sup>The difference between the size of the marginal effects reported in Table A8 and those shown in Table 4 in the paper is due to the fact that in the paper we use composite corruption perception measures, whereas here we use only the perceptions of national politicians; moreover, there, we use a logit model, here, we use a linear-probability model.



survey contains a question on the self-reported interest in politics, which we add to the specification in equation 1 in the paper along with the interaction with corruption perception. The results are unchanged: the marginal effect of corruption perception is not higher among the respondents reporting higher interest in politics.<sup>17</sup>

Moreover, we estimate the effect of corruption perception on *party* choice. All but one scandal (in February 2008) involved ministers from the two junior coalition members, the Slovak National Party (SNS) and the Movement for a Democratic Slovakia (HZDS). Yet, we have seen above that most of the punishment for corruption seems to have been directed at the senior coalition party, Smer. If parties which are considerably more engaged in scandals are not being punished, it is reasonable to conclude that scandals had at best a minor effect. We fit a nested logit model to estimate the individual party choice, because more standard multinomial choice models, such as multinomial logit, rest on a very restrictive assumption.<sup>18</sup>

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

$$P_{nk} = \frac{e^{\mathbf{X}_i\gamma + \lambda_k I_{nk}}}{\sum_{k \in K} e^{\mathbf{X}_i\gamma + \lambda_l I_{nl}}} \frac{e^{\frac{\mathbf{Z}_{ni}\gamma}{\lambda_k}}}{\sum_{m \in N_k} e^{\frac{\mathbf{Z}_{mi}\gamma}{\lambda_l}}}, \quad (6)$$

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  $\beta_k$  are the associated parameters which vary over nests;  $\mathbf{Z}_{ni}$  are party-level covariates – party distance measures and previous vote choice – which are assumed to affect the nest as well as the party choice and vary over each party  $n$  within the nest  $N_k$ ;  $\gamma$  are the associated parameters.<sup>19</sup>

The results are shown in Table A9.<sup>20</sup> The evidence is clear: the increase in the prominence of the sociotropic vote is entirely concentrated on the senior incumbent party (column 1). The two junior incumbent partners suffer no punishment despite being directly involved in all but one corruption scandal (columns 2-3).<sup>21</sup> This finding is again consistent with the results above that the increase in the prominence of sociotropic voting has been driven largely by the new anti-corruption party rather than by the scandals. This result is also consistent with our pocketbook findings, which indicate that most of the effect is borne by the senior incumbent. The remaining columns show that once the sociotropic vote is activated, it is redistributed relatively equally among the mainstream opposition parties (columns 4-5). Moreover, an increasing share of the corruption-sensitive voters seems to turn to the new entrant SaS (column 6).

<sup>17</sup>Unfortunately, other data do not contain satisfactory measures of attentiveness to politics in order to re-test this claim.

<sup>18</sup>See Train (2007). 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 party-specific distance measures on taxes, deregulation, redistribution, security, social liberalism, immigration, attitudes towards the EU, and the role of religion in politics. These measures are added as covariates to our baseline specification that includes previous vote choice.

<sup>19</sup>The 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.

<sup>20</sup>We are unable to run the analysis on November 2009 data because of the lack of party-level attributes.

<sup>21</sup>Both junior members are also parties with strong nationalist platforms, and it may be that this dimension partially overrides the importance of corruption among the supporters of these parties.

## 8 Additional Robustness Checks

Here, we report some additional robustness checks of the findings reported in the main text. All results are available upon request.

### 8.1 Corruption Exposure Question Wording

As mentioned in the paper the survey question probing corruption experience in the October 2008 data asks whether the respondent had been asked to give a bribe in the previous *five years*, thus including a period in the previous election cycle. Similarly, the question in the May 2004 data ask about corruption experience in the previous three years, partially covering the term of the pre-2002 government. What we want to measure is the effect on intended vote in the current election cycle only. Moreover, the questions probing corruption exposure in the November 2009 data and the March 2006 data cover the previous *three years*, thus encompassing partly a period covered in the 2008 and 2004 survey, respectively. As a consequence, we may conflate the claim of stability of the pocketbook effect with an estimate of the *same effect* prior to and during 2008 or 2004.

To assess these concerns, we conduct two tests. First, on 2008 data, we run a placebo model where we regress vote in 2006 (i.e. self-reported vote in the previous election) on exposure, perception, intended vote choice and a set of covariates. If the five-year question about experience in 2008 really is capturing a five-year period, then we would expect the exposure variable to be strongly correlated, *ceteris paribus*, with vote in the previous election, much like it is correlated with intended vote in our results in the paper. On the other hand, if the five-year question is really capturing a shorter period of time, than reported experience should *not* be statistically strongly correlated with vote choice in 2006. This is indeed what we find, which suggests that respondents do not report their experience from the whole five-year period.

Second, we can try to adjust our data on responses to bribe exposure items to the desired length of the coverage period. In 2008, 2006 and 2004, we would ideally want a bribe exposure question that covers only the previous two years of experience. In 2009, we would like a question that covers only the previous year's worth of exposure. In the absence of any auxiliary data that can help us, we develop a simple model of the probability of response to a bribe exposure question covering a certain period of time. For this, we assume that the annual bribe victimization rate in the population is constant. Given the stability of our estimates of corruption experience from three different Eurobarometer surveys (2005, 2007, and 2009) in Table A1, we may be justified in making this assumption. The estimate of this annual average bribe victimization rate for Slovakia is 26.5 percent. We then model the probability of a positive response to an  $n$ -year yes/no bribe exposure question as a simple draw from a binomial distribution:  $P_n = \binom{n}{1} P_1 (1 - P_1)^{n-1}$ , given the annual bribe victimization rate  $P_1$ . This is of course a massive simplification, but a very tractable model to get the difference in the share of positive responses for the 5-year vs. 2-year question for 2008, the 3-year vs. 2-year response for 2006 and 2004, and the 3-year vs. 1-year question in 2009.<sup>22</sup>

Using the annual victimization rate of 26.5, we get that the difference between the 5-year question and a 2-year question is basically zero. We therefore perform no adjustment to the 2008 responses. Together with the placebo test, we are more confident that question wording does not induce a spurious result in 2008. The difference in the share of positive responses between the

---

<sup>22</sup>We thank Patrick Egan for suggesting this idea.

3-year and a 1-year question at an annual average rate of 26.5 is around 17 percent. To perform an adjustment in question wording, we randomly reassign 17 percent of respondents who answered “yes” to the bribe experience question in 2009 to a “no” answer. This is again a simplification, and to provide more credible results, we perform this random reassignment and rerun our model 1,000 times. Our results are substantively unchanged compared to those presented in Table A5: the pocketbook effect remains significant, and the coefficient is of a very similar size. Finally, the difference in the share of positive responses between the 2-year and a 1-year question is around 14 percent. We perform the same adjustment in 2006 and 2004, and again get substantively similar results. In fact, results in 2004 are somewhat stronger as the coefficient is of greater magnitude.

## 8.2 Incumbent Vote Over-Reporting

A common issue in survey research is the tendency of respondents to over-report vote for the incumbent (e.g. Wright, 1993). Since the October 2008 survey contains self-reported vote choice in the 2006 election (the November 2009 survey does not), we can cross-check the reported shares of party vote in our data with the actual vote shares from the 2006 election. The comparison confirms that over-reporting is present: sample-weighted vote for the senior incumbent party is higher by approximately ten percent.<sup>23</sup> This over-reporting is an apparent measurement error problem. If the over-reports are randomly distributed among the respondents, then our estimates suffer from classic attenuation bias – we would be estimating the lower bound of the pocketbook and the sociotropic effect.<sup>24</sup> However, if measurement error is systematically related to either reported corruption exposure or reported perception, or both, our estimates may be systematically biased in either direction.

We perform two sensitivity tests of our October 2008 results to guard against such potential bias. First, we rerun our main model with binary vote choice on the subset of the sample which reported not voting for the incumbent in 2006. Since we are dealing with the over-report, the expectation is that there are fewer misreports in this subsample, as those who admit to not having voted for the incumbent in the past election have demonstrated themselves relatively immune from the response bias that would encourage an insincere report of voting intention. The non-null pocketbook result and the null sociotropic result are substantively unchanged.

Second, the most unfavorable scenario for our significant pocketbook 2008 estimate is to assume that *only* respondents who reported *low or no* bribe victimization misreported their 2006 vote choice, while *all* respondents who reported high bribe victimization reported their previous vote choice truthfully. While unlikely, this scenario would *ceteris paribus* exaggerate the negative association between corruption exposure and incumbent vote intention. We can thus simulate the effect of report-correction by changing the reported vote intention from one to zero (from incumbent to opposition) by the share of over-reports compared to the actual 2006 vote shares for respondents with low experience or perception, while keeping the reported intended vote choice the same for everyone else. The pocketbook effect is again substantively unchanged.<sup>25</sup>

<sup>23</sup>Comparisons of intended vote choice with the actual 2010 vote choice is impossible because the party choice set changed compared to 2008.

<sup>24</sup>Moreover, if reports of actual behavior are correlated with reported intended behavior, our estimator is also inefficient, since we have measurement error on the left-hand side (intended vote) as well as on the right-hand side (reported previous vote).

<sup>25</sup>This is a rather crude sensitivity check, but we do not have any auxiliary information, such as validated vote, used by some other report-correction approaches (Jackman, 1999; Katz and Katz, 2010). Further assumptions needed are that

### 8.3 Sample Selection Bias

Our vote models in the paper are estimated only on the sample of respondents who intend to turn out. This may induce bias from sample selection, to the extent that respondents predicted not to vote still end up doing so for reasons not captured by our voting model (Heckman, 1979). Namely, we find that individuals with high corruption perception as well as high bribe exposure are less likely to turn out. If these individuals nevertheless turn out, vote based on factors other than those we include in our voting, *and* have a lower probability of voting for incumbent parties, our main estimates will be biased upwards. In other words, we will have attributed too much weight to the factors included in our voting model – including corruption perception and bribe exposure. On the other hand, if these reluctant voters are more likely to vote for the incumbent parties than the individuals who intend to turn and are in our sample, our estimated will be underestimated.

One way to measure the potential impact of bias from sample selection is to find additional information on reluctant voters. An additional survey from Slovakia, Focus April 2006 (see Table A3), allows us to get a sense of the political leanings of reluctant voters, since all respondents are asked for party preference, not only those who intend to vote.<sup>26</sup> Breakdowns suggest that reluctant voters are between fifteen and thirty percent more likely to vote *for* the main incumbent party (Smer) than likely voters or likely non-voters, respectively.<sup>27</sup> This suggests that our parameter estimates in the voting model for experience, and possibly perception, may be *attenuated*.

Another way to assess the potential bias is to model the selection into the sample; that is, to model the choice to turn out. Our May, October, and November 2008 data allow us to run a bivariate probit model with sample selection (the so-called Heckman probit model), because we have data on both intended turnout and the turnout decision in the previous election in 2006. In the selection equation, we model the intended turnout decision as a function of our measure(s) of corruption as well as the self-reported turnout in the 2006 election. Our vote choice equation is equivalent to our main voting model. Formally, the model is as follows:

$$\begin{aligned} Pr(Turnout_i = 0) &= \Phi(-\mathbf{X}_i^T \beta^T) \\ Pr(Vote_i = 0 | Turnout_i = 1) &= \Phi(\mathbf{X}_i^T \beta^T) - \Phi_2(\mathbf{X}_i^T \beta^T, \mathbf{X}_i^V \beta^V, \rho) \\ Pr(Vote_i = 1 | Turnout_i = 1) &= \Phi_2(\mathbf{X}_i^T \beta^T, \mathbf{X}_i^V \beta^V, \rho), \end{aligned}$$

where  $T$  denotes turnout,  $V$  denotes vote, the errors for turnout and vote decisions are assumed to be correlated with coefficient  $\rho$ , and  $\Phi_2$  is a bivariate joint function. If the errors of the two equations are correlated (i.e. if  $\rho \neq 0$ , then some individuals who decide to vote do so for unobservable factors not controlled for in the voting model. In other words, there is indication of selection bias. Identification of the parameters in the model comes from the exclusion of previous turnout from the voting equation and previous vote from the turnout equation. Our results suggest that bias from sample selection is not a concern, as the hypothesis test indicated that  $\rho$  is indistinguishable

---

there is no under-reporting, (i.e. only over-reporting is occurring) that misreporting is random among the subsample of those with low experience or perception, and that rates of misreport observed in 2006 carry over to misreports of turnout intention in 2008. Also, since previous vote is also subject to the same over-report bias, we exclude it from the model, and use the most saturated specification instead.

<sup>26</sup>Unfortunately, this survey does not contain corruption items, and so we could not use it in the primary analysis in this paper.

<sup>27</sup>This difference may be even larger, depending on the assumptions about the distribution of responses among the remaining missing values. We consider reluctant voters to be those who answer “probably not” or “maybe” to the turnout intention item.

from zero in each survey. Moreover, the results from the voting equation are substantively very similar to the results from a regular probit model.

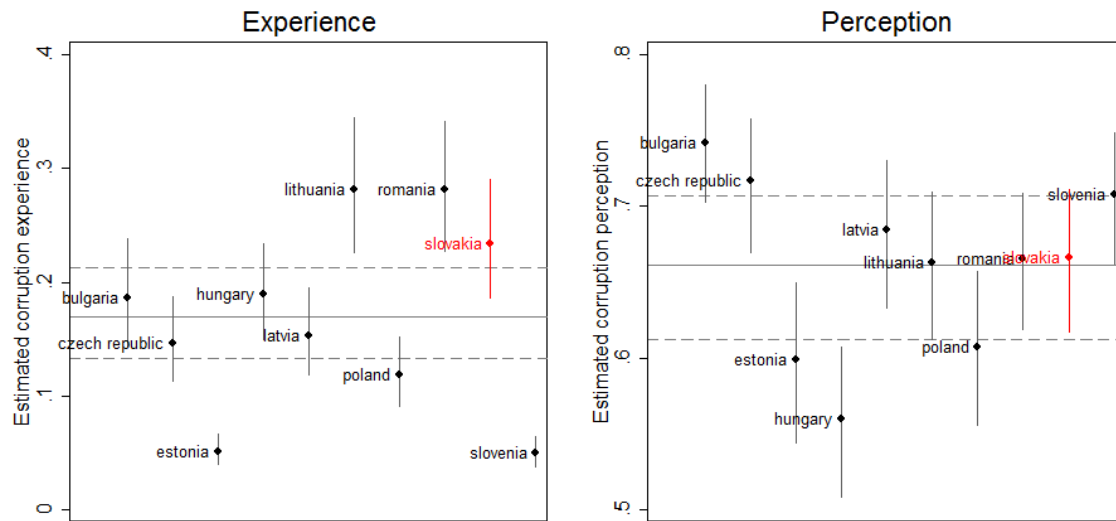
## References

- Anderson, Christopher J. and Yuliya V. Tverdova. 2003. "Corruption, Political Allegiances, and Attitudes toward Government in Contemporary Democracies." *American Journal of Political Science* 47(1):91–109.
- Chow, Gregory C. 1960. "Tests of Equality Between Sets of Coefficients in Two Linear Regressions." *Econometrica* 28(3):591–605.
- De Boef, Suzanna and Luke Keele. 2008. "Taking Time Seriously." *American Journal of Political Science* 52(1):184–200.
- Gelman, Andrew and Jennifer Hill. 2007. *Data Analysis Using Regression and Multilevel/Hierarchical Models*. New York, NY: Cambridge University Press.
- Gokcekus, Omer and Amy E. Muedin. 2008. "Quantifying Corruption by a Human Capital Earnings Equation." *International Review of Economics* 55:243–252.
- Gorodnichenko, Yuriy and Klara Sabirianova Peter. 2007. "Public Sector Pay and Corruption: Measuring Bribery from Micro Data." *Journal of Public Economics* 91:963–991.
- Heckman, James J. 1979. "Sample Selection Bias as a Specification Error." *Econometrica* 47.
- Hooghe, Liesbet, Ryan Bakker, Anna Brigevid, Catherine de Vries, Erica Edwards, Gary Marks, Jan Rovny and Marco Steenbergen. 2010. "Reliability and Validity of Measuring Party Positions: The Chapel Hill Expert Surveys of 2002 and 2006." *European Journal of Political Research* 4:684–703.
- Jackman, Simon. 1999. "Correcting Surveys for Non-response and Measurement Error Using Auxiliary Information." *Electoral Studies* 18:7–27.
- Jackman, Simon. 2005. "Pooling the Polls Over an Election Campaign." *Australian Journal of Political Science* 40:499–517.
- Katz, Jonathan N. and Gabriel Katz. 2010. "Correcting for Survey Misreports Using Auxiliary Information with an Application to Estimating Turnout." *American Journal of Political Science* 54:815–835.
- Kitschelt, Herbert Paul and Daniel M. Kselman. 2011. Paper presented at APSA 2011 Annual Meeting.
- Klašnja, Marko and Joshua A. Tucker. 2013. "The Economy, Corruption, and the Vote: Evidence from Experiments in Sweden and Moldova." *Electoral Studies* 32:536–543.
- OSCE/ODIHR. 2010. Slovak Republic Parliamentary Elections: OSCE/ODIHR Election Assessment Mission Report. Technical report OSCE.

- Rubin, Donald B. 1987. *Multiple Imputation for Non-Response in Surveys*. New York, NY: John Wiley and Sons.
- Schafer, J.L. 1997. *Analysis of Incomplete Multivariate Data*. London: Chapman and Hall/CRC.
- Train, Kenneth E. 2007. *Discrete Choice Models with Simulation*. New York, NY: Cambridge University Press.
- Wright, Gerald C. 1993. "Measuring Vote Choice in the National Election Studies, 1952-88." *American Journal of Political Science* 37(1):291–316.



Figure A1: Corruption experience and perception in Slovakia and other post-communist Eastern-European countries



*Note:* All estimates are averages based on Eurobarometer data for 2005, 2007, and 2009. Dots represent median predicted probabilities of a positive response to a question about self-reported bribe experience or perception of corruption. The model includes the same set of demographic, socio-economic, attitudinal and geographic covariates and country fixed effects as the model in Table A1. Caps are 95 percent confidence intervals. Dashed line is the median estimate for the sample of post-communism countries presented in each graph.

Table A1: Corruption experience and perception in post-communist and other EU member-states

	Post-communist EU members	Other EU members
Eurobarometer Data		
Bribe Experience (in previous year)	.165 (.130, .207)	.045 (.035, .058)
Corruption perception (among national politicians)	.658 (.609, .704)	.584 (.537, .630)
Transparency International Data		
Bribe experience (in previous three years)	.244 (.217, .274)	.081 (.071, .092)
Corruption perception (among political parties)	.853 (.827, .873)	.744 (.700, .779)

*Note:* All entries are predicted probabilities of a positive response to a question about corruption experience or corruption perception among politicians on a set of demographic, socio-economic, and geographic covariates, and country fixed effects. Models are run separately for bribe experience and perception. In all models, the dependent variables are binary. Eurobarometer data include surveys 64.3, 68.2, and 72.2. Transparency International (TI) data include Global Corruption Barometer (GCB) surveys in 2003-2007, 2009, and 2010. Estimates from different data differ partly because of different question wording and different country samples. To calculate predicted probabilities, all covariates except the fixed effects are kept at the *full-sample* medians, simulating a typical individual in all but country-level unobservables. Predicted probabilities are averaged by country, and then further by the groupings showed in the column header. Entries in parentheses are 95 percent confidence intervals. All differences between the two columns are statistically significant on a two-tailed difference-in-means test at  $p < .001$ .

Table A2: Cost of bribes in Eastern and Western Europe

	Western Europe countries	Post-communist Eastern Europe
More than 1 percent of annual family income	.018 (.014, .023)	.073 (.061, .086)

*Note:* All entries are predicted probabilities of a positive response to a question about self-reported cost of bribe extortion on a set of demographic, socio-economic, and geographic covariates, and country fixed effects. Estimates of the cost of bribery are based on question 4B of the 2009 Global Corruption Barometer. To calculate predicted probabilities, all covariates except the fixed effects are kept at the *full-sample* medians, simulating a typical individual in all but country-level unobservables. Predicted probabilities are averaged by country, and then further by the groupings showed in the column header. Entries in parentheses are 95 percent confidence intervals. The difference between the two columns is statistically significant on a two-tailed difference-in-means test at  $p < .001$ .

Table A3: Datasets and coverage of key variables

	Vote	Previous Vote	Bribe Exposure	Perception (Politicians)	Perception (Other Actors) <sup>1</sup>	Demographics & Socio-Economic	Policy Positions <sup>2</sup>
Main Data							
Focus May 2008	x	x		x	x	x	x
ISSP October 2008	x	x	x	x	x	x	x
Focus Nov. 2008	x	x		x	x	x	x
Focus June 2009	x	x		x		x	
ISSP October 2009	x			x		x	x
Transparency Nov. 2009	x		x	x	x	x	
Additional Data							
Transparency May 2004	x		x	x	x	x	
Transparency March 2006	x		x	x	x	x	
Transparency Dec. 2011	x		x	x	x	x	
Focus April 2006	x	x				x	x
Euro. Values Study May 2008	x					x	x
TNS July 2010	x	x				x	x
Eurobarometer Dec. 2005			x	x	x	x	
Eurobarometer Nov. 2007			x	x	x	x	
Eurobarometer Sep. 2009			x	x	x	x	

Note:

1. Other actors include regional and local politicians, and civil servants in sectors such as education, courts, police, etc.

2. Policy positions include items probing respondents' views on taxation and redistribution, regulation of business, law and order, role of religion in politics, social issues such as gay rights and abortion, environment, membership in the EU, and left-right self-placement.

Table A4: Aggregate vote intention polls used in the paper and estimates of firm effects

Polling firm	Polling Method	Number of Surveys	Period of Coverage	Average Sample Size	Firm Effect
Focus	Telephone, closed-ended	43	July 2006–June 2010	1,206.6	0.010 [−0.012, 0.026]
Median	Face-to-face, open-ended	31	August 2006–May 2010	1,036.4	−0.045 [−0.066, −0.029]
Polis	Face-to-face, closed-ended	9	June 2009–July 2010	1,139.4	−0.025 [−0.047, −0.009]
UVVM	Face-to-face, open-ended	33	July 2006–February 2009	1,164.1	0.011 [−0.011, 0.028]
Total		116		1,126.2	

*Note:* Firm effects in the last column denote the estimated bias in the Smer vote share due to each firm's polling technology. Firm effects are quite similar for the estimates of the coalition vote share. The estimates of the vote share and firm effects are based on the approach of [Jackman \(2005\)](#). A 1,000,000 iteration burn-in was used followed by a 25,000,000 iteration run. All the computations were done in WinBUGS 1.43. The estimates accord with our knowledge of the firms' methodologies and their survey results.

Table A5: Main results

	TI 5/2004	TI 3/2006	Focus 5/2008	ISSP 10/2008	Focus 11/2008	Focus 06/2009	ISSP 10/2009	TI 11/2009	TI 12/2011
Corruption experience	-0.394 (0.279)	-0.502 (0.229)		-0.551 (0.281)				-0.354 (0.192)	-0.161 (0.200)
Corruption perception	-0.062 (0.129)	-0.111 (0.095)	-0.088 (0.104)	-0.003 (0.113)	-0.036 (0.108)	-0.332 (0.127)	-0.256 (0.111)	-0.257 (0.086)	-0.166 (0.088)
Previous vote			3.399 (0.259)	2.911 (0.317)	3.386 (0.238)	3.115 (0.264)			
Demographics	Yes	Yes	No	No	No	No	Yes	Yes	Yes
Income	Yes	Yes	No	No	No	No	Yes	Yes	Yes
Occupation	Yes	Yes	No	No	No	No	Yes	Yes	Yes
Education	Yes	Yes	No	No	No	No	Yes	Yes	Yes
Union	No	No	No	No	No	No	Yes	No	No
Religion	No	No	No	No	No	No	Yes	No	No
Village size	Yes	Yes	No	No	No	No	Yes	Yes	Yes
Region	Yes	Yes	No	No	No	No	Yes	Yes	Yes
N	878	833	801	796	899	694	763	822	879

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

*Note:* The dependent variable is the intended vote for any party in the incumbent coalition. Demographics consist of variables for gender, age, size of household and a set of ethnicity dummies; Income denotes dummies for different household income levels; Occupation denotes dummies for occupation status (full-time job, part-time job, self-employed, student, retired, housekeeper); Education denotes dummies for highest completed education (primary, secondary, university); Union denotes a dummy for union membership; Religion denotes dummies for religious denomination; Village size and Region denote the corresponding dummies for the size of the village and the region of residence. All estimates are calculated five imputed datasets using Rubin's rules (Rubin, 1987).



Table A6: Effects of scandals and new party entry from aggregate monthly data

	Coalition	Sr. Incumbent (Smer)
L.Dep. var.	0.780 (0.192)	1.208 (0.175)
L2.Dep. var.	-0.323 (0.156)	-0.417 (0.209)
Nov. 2007 scandal	-0.012 (0.004)	-0.001 (0.005)
Jan. 2008 scandal	0.006 (0.004)	0.009 (0.006)
Aug. 2008 scandals	0.002 (0.004)	-0.006 (0.004)
Apr. 2009 scandals	0.004 (0.002)	0.001 (0.003)
Jan. 2010 scandal	-0.004 (0.003)	0.005 (0.004)
SaS entry (Feb. 2009)	-0.009 (0.003)	-0.012 (0.004)
SaS referendum (July-Aug. 2009)	-0.009 (0.003)	-0.009 (0.004)
L.SaS referendum (July-Aug. 2009)	-0.014 (0.003)	-0.011 (0.003)
Local elections (Dec. 2006)	0.011 (0.003)	0.005 (0.004)
Euro. Parl. elect. (June 2008)	0.002 (0.002)	0.000 (0.003)
Industrial production	0.045 (0.018)	0.013 (0.023)
L.Industrial production	-0.030 (0.020)	-0.032 (0.023)
Inflation rate	-0.009 (0.003)	-0.007 (0.003)
L.Inflation rate	0.011 (0.002)	0.008 (0.003)
Unemployment rate	-0.007 (0.002)	-0.001 (0.002)
2010 election campaign (Apr. 2010)	-0.024 (0.006)	-0.011 (0.005)
L.2010 election campaign (Apr. 2010)	-0.048 (0.008)	-0.014 (0.005)
L2.2010 election campaign (Apr. 2010)	-0.049 (0.011)	-0.012 (0.006)
Presidential election (Mar. 2010)	-0.012 (0.005)	0.001 (0.005)
Constant	0.220 (0.133)	0.037 (0.305)
N	46	46

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

*Note:* The dependent variable is the monthly vote share for the incumbent government (the first column) or the senior coalition party (the second column) estimated from polls conducted by several polling firms. See Section 5 above for more details.

Table A7: Sensitivity to omitted variable bias: saturating the October 2008 model

	(1)	(2)	(3)	(4)
Corruption exposure	-0.551	(0.281)	-0.551	(0.324)
Corruption perception	-0.003	(0.113)	0.014	(0.138)
Previous vote	2.911	(0.317)	2.908	(0.424)
Female				
Age		-0.038	-0.009	(0.264)
Education (years)		-0.012	-0.014	(0.013)
Hungarian		-0.056	-0.076	(0.069)
Household size		-2.087	-2.101	(0.587)
Union member		0.021	0.019	(0.100)
Interest in politics		0.572	0.538	(0.664)
Political efficacy			0.362	(0.216)
Trust in civil servants			-0.214	(0.330)
Treated fairly by civil servants			-0.139	(0.133)
Taxes vs. spending			0.061	(0.154)
Deregulation				
Redistribution				
Security				
Social liberalism				
Religion in politics				
Occupation	No	Yes	Yes	Yes
Village size	No	Yes	Yes	Yes
Region	No	Yes	Yes	Yes
N	796	796	796	796
F-statistic		1.331	1.207	1.265
p-value		0.369	0.444	0.417

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

*Note:* The dependent variable is the intended vote for any party in the incumbent coalition. Occupation denotes dummies for occupation status (full-time job, part-time job, self-employed, student, retired, housekeeper); Village size and Region denote the corresponding dummies for the size of the village and the region of residence. All estimates are calculated from five imputed datasets using Rubin's rules (Rubin, 1987). The  $F$ -statistic is from the  $F$ -test of the joint significance of the variables added to the baseline specification, with the associated  $p$ -value.

Table A8: Sensitivity of sociotropic effect to exclusion of corruption experience

	Unadjusted Estimates	Adjusted Estimates (w/ same-year data)	Adjusted Estimates (w/ additional data)
Focus May 2008	−0.010 [−0.028,0.010]	−0.001 [−0.020,0.021]	−0.008 [−0.027,0.011]
Focus Nov. 2008	−0.016 [−0.041,0.009]	−0.006 [−0.033,0.021]	−0.015 [−0.041,0.010]
Focus June 2009	−0.046 [−0.074,−0.014]	−0.044 [−0.073,−0.014]	−0.043 [−0.072,−0.012]
ISSP Oct. 2009	−0.048 [−0.090,−0.006]	−0.047 [−0.089,−0.006]	−0.046 [−0.087,−0.005]

*Note:* All entries represent the *ceteris paribus* change in the probability of voting for incumbent for a typical respondent from a one-standard deviation change in perception. All other covariates in the model are kept at their medians or modes. Entries in brackets represent the 95 percent confidence intervals. Adjusted estimates are obtained as explained in the text. In the second column, adjustment is based on the estimate of elements of  $\gamma$  from the survey containing both corruption variables in the same year. In the third column, we also use the three Eurobarometer surveys indicated in Table A3.

Table A9: Sociotropic effect on party choice

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

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

Note: The dependent variable is the individual-party choice. We fit a nested logit model and calculate the probability of choosing a party based on equation 6. More details about the choice of nested logit are given in Section 7.2 of the Web Appendix. The main entries in the table represent the marginal effect of a one-standard deviation increase in corruption perception on a standardized scale while keeping all other variables in the model at their mean. Robust standard errors are shown in the parentheses.