



# The economy, corruption, and the vote: Evidence from experiments in Sweden and Moldova

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## ABSTRACT

How does corruption affect voting behavior when economic conditions are poor? Using a novel experimental design and two original survey experiments, we offer four important conclusions. First, in a low corruption country (Sweden), voters react negatively to corruption regardless of the state of the economy. Second, in a high corruption country (Moldova), voters react negatively to corruption *only when* the state of the economy is also poor; when economic conditions are good, corruption is less important. Third, respondents in Sweden react more strongly to corruption stimuli than respondents in Moldova. Finally, in the low corruption country, *sociotropic* corruption voting (or voting based on corruption among political leaders) is relatively more important, whereas in our high corruption country, *pocketbook* corruption voting (or voting based on one's own personal experience with corruption, i.e., being asked to pay bribes) is equally prevalent. Our findings are consistent with multiple stable corruption equilibria, as well as with a world where voters are more responsive to corruption signals more common in their environment.

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## 1. Introduction

One way to conceive of the study of economic voting is as a study of accountability: if incumbent politicians do a poor job managing the economy, will voters punish them at the ballot box? However, the economy is not the only area over in which accountability is enforceable. One other particularly important area – especially in new democracies – is the extent to which political leaders combat, tolerate, or partake in corruption. While there is a voluminous literature on economic voting and a much smaller – although growing – literature on the effect of corruption on voting, we know almost nothing about the interaction between the two, and thus little about how corruption might effect voting in times of economic crisis.

Thus the goal of this manuscript is to explore whether “corruption voting” varies in response to the state of the

economy. To answer this question, though, we need to address a number of other questions as well. First, what are the mechanisms through which corruption affects voting behavior? Specifically, is corruption voting driven by perceptions of corruption among politicians (which we label “sociotropic corruption voting”), personal experience with corruption (“pocketbook corruption voting”), or both? Second, is corruption voting a function of the overall level of corruption prevalent in one's society? And finally, how do these various factors (good vs. bad economy; high vs. low levels of corruption; pocketbook vs. sociotropic corruption voting) interact with one another?

Attempting to answer these questions raises an important methodological challenge. We demonstrate below in Section 3 that one's reaction to corruption can be influenced by partisan cues. Thus in order to conduct our analysis free of such biases, we rely on an original experimental vignette we designed to compare the relative (and interactive) impact of information about the economy and corruption on individual voting behavior. In order to capture the distinction between high corruption and low corruption societies, we embedded

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the experiment in surveys in Sweden (low corruption) and Moldova (high corruption).<sup>1</sup>

We find that there is indeed an interactive effect between economic conditions and corruption in our low corruption country (Moldova): when economic conditions are poor, incumbents are punished for corrupt behavior. However, when economic conditions are better, the effect of corruption is significantly diminished. In Sweden, on the other hand, corruption is always punished by voters regardless of the state of the economy. Moreover, Swedish voters react more strongly to prompts regarding corruption than do Moldovan voters. We believe these findings are consistent with a state of the world where there are essentially two stable equilibria regarding corruption voting: one in which voters are likely to react harshly to allegations of corruption, thus discouraging corrupt politicians from entering politics and constraining politicians that do enter the political arena from engaging in corrupt activities; and one in which corruption is widespread, voters are less attentive to it, and willing to overlook corrupt behavior when other goals (e.g., improved economic conditions) are met, and ultimately allowing corrupt politicians to succeed in politics. Finally, we find that voters in Sweden respond more strongly to corruption at the societal level than to personal victimization, whereas Moldovans are somewhat more responsive to experience with corruption. This result suggests that voters are more responsive to corruption signals more common in their environment.

## 2. Theoretical arguments

How might corruption affect voting behavior? Some studies suggest that corruption matters for political preferences by way of corruption perception, whether directly as an attitude (Anderson and Tverdova, 2003; Davis et al., 2004; Della Porta, 2000; McCann and Dominguez, 1998), or when it is reinforced by revelation of hard information (Banerjee et al., 2012; Chong et al., 2011; Ferraz and Finan, 2008). Other studies – though fewer in number – claim that corruption matters through personal exposure (“victimization”) (Gingerich, 2009; Lavalley et al., 2008; Seligson, 2002).

To distinguish between these two channels, we adopt the nomenclature of the economic voting literature: we term vote choice influenced by personal exposure to

corruption *pocketbook corruption voting*, and vote choice influenced by perceptions of corruption as *sociotropic corruption voting*. For the empirical analysis in this paper, we operationalize personal exposure to corruption via being asked to provide a bribe, and sociotropic corruption via hearing that a leading political official took bribes in return for providing government contracts.<sup>2</sup>

Our conceptualization of pocketbook and sociotropic corruption voting as separate processes is motivated by findings from studies on the methodology of corruption measurement. Scholars have repeatedly shown – along numerous dimensions – that the relationship between personal experience with corruption and corruption perception is quite tenuous (Abramo, 2007; Donchev and Ujhelyi, 2009; Krastev and Ganey, 2004; Mocan, 2004; Morris, 2008; Rose and Mishler, 2007). Our own analysis of Eurobarometer and Transparency International survey data reveals similar findings: personal exposure to bribes is almost entirely uncorrelated with one’s beliefs regarding corruption among national politicians (see [Web Appendix Section 1](#) for details). This is perhaps not too surprising: exposure typically relates to petty corruption, while perceptions are mostly about “grand” corruption among high-level bureaucrats and politicians (McCann and Redlawsk, 2006). Moreover, corruption perception seems similar to other perceptual evaluations that are weakly rooted in experience, such as the government’s record on human rights (Abramo, 2007).

With this framework in mind, we assume that for voters more corruption is worse than less corruption.<sup>3</sup> Paralleling the study of the economic vote, then, we expect that either believing corruption is a problem at the societal level (sociotropic) or personal exposure to corruption (pocketbook) could lead voters to turn against the incumbent candidate or party. However, in line with the theme of this special issue, we are also interested in the extent to which corruption voting varies with the state of the economy. One of us has elsewhere (Tucker, 2006) introduced the idea of “conditional economic voting” hypotheses, or hypotheses concerning when economic voting is more or less likely to be present. “Supply side” conditional economic voting hypotheses suggest that economic voting will be present unless something more important crowds out economic concerns (e.g., a war or secession issues). Applying a similar theoretical logic to corruption voting, we can test a “conditional corruption voting” hypothesis that corruption voting is a second order concern behind the state of the economy. As long as the economy performs well, corruption is tolerated (i.e., no corruption voting); but once the economy is performing poorly, corruption is no longer tolerated (i.e., we observe corruption voting).<sup>4</sup>

<sup>1</sup> Both experiments took place at roughly the same time: January 9, 2012 in Sweden, and February 4–March 29, 2012 in Moldova. We thus hold the state of the global economic crisis relatively constant. The experiment in Sweden was conducted as part of the University of Gothenburg’s Laboratory of Opinion Research (LORE)’s Citizen Panel Study. The Moldovan experiment was conducted as part of our own random probability-based survey and implemented in the field by IMAS-Inc.

<sup>2</sup> These are of course not the only forms of corruption that we could have featured in our experiments, but they are both relatively common, especially in post-communist countries. It is worth noting that “corruption as vote-buying” might result in a different dynamic, as in that case individual citizens would receive material benefits (as opposed to costs) from their interaction with corruption; vote-buying, however, is not particularly common in Europe, where our study is located. We will explore the effects of different types of corruption in future research, but the topic is beyond the scope of the current article.

<sup>3</sup> It is of course possible in some circumstances that citizens might reward corruption (Barbera et al., 2012). While we do not address this hypothesis here, our experiments are suitable for testing for such a reaction and readers can consider this to be an alternative null hypothesis.

<sup>4</sup> Our research design also allows us to observe if support for this hypothesis varies across a high corruption and low corruption country.

**Table 1**  
Partisan bias in perceptions of guilt and fair punishment of corruption.

	Moldova		Bulgaria	
	Mayor guilty?	Fair punishment?	Mayor guilty?	Fair punishment?
No party cue	5.632*** (0.212)	3.569*** (0.128)	8.725*** (0.161)	5.057*** (0.152)
Preferred party cue (change from No cue)	−0.566* (0.319)	−0.166 (0.183)	−2.026*** (0.280)	−0.490** (0.224)
Disliked party cue (change from No cue)	0.381 (0.327)	0.341* (0.191)	0.218 (0.282)	0.122 (0.223)
N	458	481	646	644
Diff. preferred/disliked	−0.947***	−0.507***	−2.244***	−0.612***

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust standard errors in parentheses. *Mayor guilty?* asks how likely the respondent thought the mayor was guilty of corruption, from 0 to 100 percent (measured from 1 to 11). *Fair punishment?* asks what punishment the respondent thought was appropriate if the mayor were found guilty (measured from 1 to 7 in increasing harshness). Exact question wording is given in the [Web Appendix](#). Party cues were randomly assigned.

### 3. Research design

In a study of the relationship between vote choice and perceived or experienced corruption, relying solely on observational data is potentially problematic primarily because of the classic risk of endogeneity: vote choice may influence one's reported corruption perception or experience, rather than the other way around. Previous studies have suggested that this possibility is likely (Anderson and Tverdova, 2003; Anduiza et al., 2013). Two experiments we conducted in Moldova and Bulgaria give added reason for pause. In both countries, we presented respondents with a scenario in which a hypothetical mayor was rumored to have taken bribes in order to dispense government contracts to particular bidders, a common form of corruption in this region of the world. After being told that the mayor denied these allegations, respondents were then asked whether or not they thought the mayor was guilty, and what they felt should be an appropriate punishment. In the control group, the partisan affiliation of the mayor was left unstated. In one treatment group, the mayor was identified as a member of the respondent's preferred party (which was obtained from an algorithm based on answers to earlier questions in the survey); in a second treatment group, the mayor was identified as a member of the respondent's least preferred party.<sup>5</sup>

Table 1 shows that when respondents are told that the mayor is from their preferred party, they are less likely to believe that the mayor is indeed guilty. They are also less punitively inclined than when a respondent is not given any information on the mayor's party affiliation, and in particular, when a respondent is told that the mayor is from their most disliked party.

Similar results on partisanship bias have been found in the economic voting literature.<sup>6</sup> In a study interested in the

interaction between economic and corruption voting, endogeneity concerns are likely only more pronounced, and the use of observational data thus potentially riskier. Experimental randomization should help effectively avoid the endogeneity issues, as well as any concerns related to omitted variables and measurement error, which are also likely to plague a study relying on observational data (Morton and Williams, 2010).<sup>7</sup>

As we are also interested in exploring the impact of corruption on voting in both high and low corruption countries, we conducted our experiments twice: once in Sweden, and once in Moldova. In Figure 1 of the [Web Appendix](#) we provide evidence from Transparency International data substantiating this claim. Here is the actual text of the experimental vignette from Moldova.<sup>8</sup>

[MALE/FEMALE NAME = MATCHING RESPONDENT GENDER] lives in a medium-sized city in Moldova. Last month, [NAME] [INSERT1a/INSERT2a/INSERT1b/INSERT2b]. 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].

INSERT1a = had to spend half of his/her monthly salary to speed up the approval of permits for his/her business.

INSERT1b = promptly received permits for his/her business without having to pay any bribes.

INSERT2a = heard that several city officials have taken bribes in exchange for government contracts.

INSERT2b = heard that several city officials were fired by the Mayor for taking bribes in exchange for government contracts.

CONDITIONS1 = improved

CONDITIONS2 = worsened

Respondents were then asked what they thought was the likelihood that NAME would vote for the mayor in this hypothetical election, with five possible responses ranging

<sup>5</sup> We provide the full text of the vignette and the follow-up questions in the [Web Appendix](#). The Bulgaria experiment was embedded in a survey funded by the same sources identified earlier as funding the Moldova experiment, was also under the direction of Tucker and Ted Brader, and was carried out by Vitosha Research between July 20th and July 28th, 2011. The survey was conducted over the internet using an opt-in sample and had an overall response rate of 17% (i.e., 17% of the people who clicked on the survey link completed the survey).

<sup>6</sup> See for example Anderson et al. (2004), Evans and Andersen (2006), Evans and Pickup (2010), Ladner and Wlezien (2007), Lewis-Beck (2006), Lewis-Beck et al. (2008), and Wlezien and Twiggs (1997).

<sup>7</sup> Endogeneity may cause non-random measurement error in observational data if out-partisans systematically over-report exposure to bribes. Non-random measurement error may thus induce further estimation bias. Our experimental design avoids misreporting entirely by focusing on hypothetical scenarios.

<sup>8</sup> The text of the experiment we ran in Sweden is very similar, with a few conditions slightly modified in consultation with the Swedish survey team, so as to make the vignettes more suitable to the Swedish context.

from “definitely will not” to “definitely will.”<sup>9</sup> As an experiment with a  $4 \times 2$  design, we have 8 conditions that were each randomly assigned to approximately 12.5 percent of the sample.<sup>10</sup>

Before presenting our estimation strategy and results in detail, it is important to discuss the external validity of our experiment. Even though Sweden is a low corruption country, political corruption is not entirely absent from the public debate and can be consequential for electoral outcomes (Svaleryd and Vlachos, 2009).<sup>11</sup> Therefore, our experiment addresses an issue that is important even in a low-corruption setting. Moreover, our paper contributes to the growing evidence from related survey and field experiments on the effects of corruption on voter behavior conducted in other countries, such as India (Banerjee et al., 2011, 2012), Brazil (Weitz-Shapiro & Winters 2013), Mexico (Chong et al. 2011), Uganda (Humphreys and Weinstein, 2012), and Spain (Anduiza et al., 2013). As the knowledge from these studies accumulates, we will be better able to draw more fine-tuned comparative conclusions. Finally, we acknowledge that the use of vignettes may provide an upper bound on the impact of corruption on vote intentions, as the information about corruption is presented in a distilled form, without the interference of other messages, counter-allegations, or other environmental factors present in real-world election campaigns. However, information provided in our vignettes is relatively limited, which is similar to many real-world instances. Also, if we are uncovering upper-bound effects, our null finding in Moldova for respondents who received the positive economy prompt should be particularly robust.<sup>12</sup>

In Section 4.1, we begin by pooling together our different corruption vignettes so that the experiment collapses into a  $2 \times 2$  design, thus allowing us to examine a simple *corruption effect*, which we define as the difference in the vote decisions following a negative corruption vignette (INSERT1a or INSERT2a) and a positive corruption vignette (INSERT1b or INSERT2b). We can then compare

the corruption effect when economic conditions in the city have worsened (CONDITION 1) and when economic conditions have improved (CONDITION 2). Since we are also interested in comparing effects across our two survey experiments, we estimate the following model:

$$Vote_{ij} = J \times \{ \beta_{0j} + \beta_{1j}Corruption_{ij} + \beta_{2j}Economy_{ij} + \beta_{3j}Corruption_{ij} \times Economy_{ij} \} + \epsilon_i, \quad (1)$$

for respondent  $i$  in country  $j$ ;  $J \in \{\text{Sweden, Moldova}\}$ ;  $Vote$  is constructed from the vote question, ranging from 1 (“definitely not”) to 5 (“definitely will vote”);  $Corruption$  equals one if a respondent received a negative corruption vignette, and zero otherwise;  $Economy$  equals one if a respondent received CONDITION2, and zero otherwise. This specification gives coefficient estimates equivalent to those obtained from two separate single-country specifications, but also allows us to test hypotheses across countries.<sup>13</sup> In Section 4.2, we utilize the full experimental design, by examining both the within-country and the cross-country differences in the *composition* of the corruption vote, as well as how the two different types of corruption voting interact with the state of the economy.

When comparing results from the two countries, we need to take into account the differences in the sampling design and survey mode. The Swedish survey was conducted on an online opt-in sample, whereas the Moldovan survey was conducted face to face on a population probability sample. Unlike the Moldovan sample, the online opt-in sample is not representative of the Swedish population, but predictably over-samples younger, male, educated, politically interested, liberal, and richer respondents. Non-probability online surveys may give biased results compared to more traditional representative surveys (Malhotra and Krosnick, 2007; Yeager et al., 2011). As a first remedy, we reweight the Swedish data using post-stratification demographic weights throughout the analysis. Reweighting is not a universally effective solution (e.g. Chang and Krosnick, 2009; Loosveldt and Sonck, 2008). However, for our particular survey, Dahlberg et al., (2012) show that weights substantially reduce the gap in representativeness relative to a nationally representative postal survey of identical content conducted at the same time. Moreover, they find that reweighting the opt-in sample makes estimates from common models of political behavior very similar to those from the nationally representative postal survey. They also find only weak mode effects, as the probability-based postal sample gives similar marginal distributions in key variables as the probability-based online sample.<sup>14</sup> Since reweighting is an imperfect solution, we provide additional evidence of the robustness of our results in the Web Appendix. First, we show that our results are insensitive to several different types of weights. Second, we reweight our Moldova sample to make it similarly skewed as the unweighted Swedish sample, and show that results do

<sup>9</sup> The use of the word “bribes” in the positive statements (INSERT1b and INSERT2b) could have negative effect on some respondents. While we would need to run another experiment to answer this question, any negative effects would likely dampen the difference between the positive corruption statements and the negative corruption statements (INSERT1a and INSERT2a), thus causing us to *underestimate* any corruption effects. As shown below, even these potentially conservative estimates are large.

<sup>10</sup> In the Web Appendix, we demonstrate that the pre-treatment variables are balanced across experimental conditions. In Moldova, another corruption experiment was present on the same survey and preceded our main experiment (results discussed above). We verified that this experiment did not contaminate our results. Further details of this analysis are available upon request.

<sup>11</sup> For example, partly state-owned Swedish telecommunications company TeliaSonera is being investigated for corruption in connection with the expansion of a mobile phone network in Uzbekistan through an offshore firm owned by the daughter of the Uzbek president. See <http://www.thelocal.se/45014/20121212/>.

<sup>12</sup> More generally, these types of survey experiments permit us to more closely analyze hypothesized causal relations in a field of study where actually manipulating the independent variable (i.e., forcing an individual to pay a bribe or creating a corruption scandal) is beyond the bounds of acceptable research practices. This is not to say we do not have much to gain from analyzing observational data, which is also part of our research agenda but outside the scope of this particular paper (Klačnja et al., 2013).

<sup>13</sup> Standard errors are slightly larger in the combined model than in separate country models, due to lower degrees of freedom. None of the inferences are affected, however.

<sup>14</sup> Only minor differences are also reported in other contexts by Sanders et al., (2007) and Hill et al., (N.D.).



**Table 2**

Main results from Sweden and Moldova – estimates.

		Sweden	Moldova	Cross-country difference
Corruption effect   economy = better	$\beta_1$	-0.768 (0.090)***	0.031 (0.165) [0.077]	-0.798 (0.188)*** [0.119]***
Corruption effect   economy = worse	$\beta_1 + \beta_3$	-0.814 (0.097)***	-0.503 (0.166)*** [0.078]***	-0.310 (0.192) [0.124]***
Economy effect   corruption = positive	$\beta_2$	-0.564 (0.090)***	-0.137 (0.172) [0.080]*	-0.427 (0.194)** [0.120]***
Economy effect   corruption = negative	$\beta_2 + \beta_3$	-0.610 (0.098)***	-0.671 (0.159)*** [0.074]***	0.060 (0.187) [0.122]
Interaction: corruption = negative, economy = worse	$\beta_3$	-0.046 (0.133)	-0.534 (0.234)** [0.109]***	0.488 (0.269)* [0.171]***
N		1852	459	

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Robust standard errors in parentheses. The dependent variable is an answer about the hypothetical person's vote for mayor in our vignettes, ranging from 1 ("definitely not") to 5 ("definitely will vote"). Robust standard errors are in parentheses. In brackets, robust standard errors are adjusted for the smaller size of the Moldova sample, as described in the [Web Appendix](#). The corruption "effect" denotes the difference in the vote after receiving a positive corruption vignette and a negative corruption vignette. The economy effect is defined analogously for the economy vignettes.

not change substantively. Finally, we show that respondents in another representative survey in Sweden similar to those who self-selected in our sample have largely indistinguishable corruption attitudes from respondents who were not covered by our survey. If anything, the selection bias attenuates the importance attached to corruption in Sweden, thus possibly *decreasing* the differences between Sweden and Moldova.<sup>15</sup>

## 4. Results

### 4.1. Main results

The results from a  $2 \times 2$  setup (i.e., when we consider a single *corruption effect* without accounting for whether it is a pocketbook or sociotropic cue) are shown in [Table 2](#).<sup>16</sup> First, we observe that Swedish respondents punish corruption irrespective of the state of the economy. Likewise, Swedes punish incumbents for worsening economic conditions irrespective of the information about corruption. In other words, there is no interaction effect: accountability is present in all states of the world. Moldovan respondents, however, punish corruption *only when the economy is bad*, but not when the economy is good, i.e. there is an interaction between corruption and a bad economy. On the other hand, Moldovans appear to punish for bad economic conditions in both states of corruption, although the evidence for economic voting under positive corruption vignettes is (relatively) noisy. Moreover, in absolute terms, Swedish respondents are generally more responsive to both the economy and corruption (3 of 4 cross-country differences in coefficients are statistically significant). Finally, the interaction between a bad economy and corruption is decidedly larger in Moldova.

The findings are consistent with Swedes using a kind of grim-trigger strategy: any deviation from optimal

performance is likely to be punished. The fact that we find this behavior in a country where corruption is rare suggests that it can be thought of as reflecting a "good corruption equilibrium." A bad record on corruption or the economy is immediately punished; this in turn creates strong disincentives to engage in corruption and mismanage the economy (performance effect), and likely creates strong disincentives for dishonest or incompetent politicians to run for office (selection effect). Such positive incentive effects in turn should make corruption (and probably bad economic policies) uncommon, and thus – in a positive feedback loop – make voters more likely to react adversely in the limited instances when they do occur.

Moldova, on the other hand, may represent a "bad corruption equilibrium." Bribe victimization is high, and perception of corrupt officials is widespread (see the [Web Appendix](#)). In this environment, a bad record on corruption is not news; it carries little information about the politician beside the fact that on corruption they are "more of the same." The same holds for the effect of a bad economy. However, a bad economy coupled with corruption may reveal additional information about the politician: the politician is failing on two dimensions rather than only one. In such situations, Moldovans seem to expect that the politician should do better on at least one dimension (either character, or competence). This may be thought of as partial accountability. Our results also indicate that in a good economy, a politician can go unpunished if she decides to become corrupt. This evidence is consistent with the conjectures in the existent literature on the trade-off between competence and corruptness ([Rundquist et al., 1977](#); [Weitz-Shapiro and Winters 2013](#)): voters are willing to put up with corruption as long as they receive benefits on another important dimension (in this case the economy).

### 4.2. Pocketbook vs. sociotropic corruption voting

We now disaggregate our main findings in terms of the pocketbook and sociotropic corruption treatments ([Table 3](#)).<sup>17</sup> There are two important similarities with the main results: we only find an interaction effect between corruption (be it sociotropic or pocketbook) and the

<sup>15</sup> Comparisons across the two experiments are also complicated by unequal sample size. The Swedish survey contains 1852 complete cases, while the Moldovan survey contains only 459. The statistical power of our estimator is considerably higher in Sweden than in Moldova. We thus perform a sample-size adjustment to standard errors of all our hypothesis tests involving data from the Moldova survey, and report such adjusted standard errors in addition to the unadjusted ones. The adjustment is implemented by performing the hypothesis tests on a subsample of Swedish data equivalent to the sample size in Moldova, and comparing the standard errors to those obtained on the full Swedish sample. The details are given in the [Web Appendix](#).

<sup>16</sup> Graphical summaries of the results are shown in the [Web Appendix](#).

<sup>17</sup> The estimated equation is:  $\text{Vote}_{ij} = J \times \{K \times (\beta_{0,jk} + \beta_{1,jk} \text{Corruption}_{i,jk} + \beta_{2,jk} \text{Economy}_{i,jk} + \beta_{3,jk} \text{Corruption}_{i,jk} \times \text{Economy}_{i,jk})\} + \varepsilon_i$ , where  $K \in \{\text{pocketbook vignette, sociotropic vignette}\}$ , and the remaining notation is the same as in [Equation \(1\)](#).

**Table 3**  
Corruption voting in Sweden and Moldova.

	Sweden	Moldova	Cross-country diff.
Economy = better			
Pocketbook effect	−0.561***	−0.057	−0.505***
Sociotropic effect	−1.000***	0.124	−1.124***
Within-country diff.	0.438**	−0.180	0.619***
Economy = worse			
Pocketbook effect	−0.520***	−0.601***	0.082
Sociotropic effect	−1.123***	−0.400***	−0.723***
Within-country diff.	0.604***	−0.201	0.805***
Interaction effect			
Pocketbook effect	0.042	−0.544***	0.586**
Sociotropic effect	−0.123	−0.524***	0.400*
Within-country diff.	0.165	−0.021	0.186

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The dependent variable is an answer about the hypothetical person's vote for mayor in our vignettes, ranging from 1 ("definitely not") to 5 ("definitely will vote"). For readability, standard errors are omitted; instead, we show the significance levels. Full results with standard errors are shown in the [Web Appendix](#). Standard errors are adjusted for the smaller size of the Moldova sample, as described in the [Web Appendix](#). The "pocketbook" effect denotes the difference in the vote after receiving a positive bribe vignette and a negative bribe vignette. The "sociotropic" effect is defined analogously for the corruption perception vignettes.

economy in Moldova; and the overall effect of both types of corruption prompts remains statistically significantly stronger in Sweden in three of the four cases (rows 1, 2, and 5 of column 3 labeled "Cross-country difference" in [Table 3](#)).

The second important point is that we find evidence of both sociotropic and pocketbook corruption voting in both countries. When an effect is present, it is present for both types of corruption treatments; when it is not, it is missing for both. The third important observations is that the relative importance of the two types of corruption voting differs across the two countries. In Sweden, sociotropic voting is statistically significantly stronger than pocketbook voting in both states of the economy (rows 3 and 6, labeled "within-country difference" in column 1 of [Table 3](#); positive difference implies that the sociotropic effect is stronger, and vice versa), even though both are present. In Moldova, however, pocketbook voting is of a similar magnitude to sociotropic voting, or, if anything, is slightly stronger. *Cross-country* comparisons of these within-country differences in the *composition* of corruption voting are statistically significant (rows 3 and 6 of column 3 in [Table 3](#)).

We interpret this finding as being driven by the kind of corruption respondents are more likely to encounter. Swedes are considerably more likely to observe a corruption scandal than be asked for a bribe, and so they may respond more strongly to the sociotropic vignette than a pocketbook vignette. Moldovans are much more likely to be asked for a bribe than Swedes, and may be comparatively more responsive to the pocketbook vignette. We can also provide some additional evidence for this claim. In addition to the vote question, in our experiment we asked each respondent after presenting them with the vignette whether they thought corruption or the economy would be more important for the hypothetical voter. In Moldova, we also asked respondents before presenting them with the

**Table 4**  
Experience and perception as determinants of corruption voting in Moldova.

	Full sample	Pocketbook vignettes	Sociotropic vignettes
Corruption experience	0.175*** (0.062)	0.170* (0.096)	0.188 (0.108)
Corruption perception	0.061 (0.100)	0.168 (0.179)	0.330*** (0.135)
N	330	159	168

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The dependent variable is an answer about whether corruption (=1) or economy (=0) is more important for the hypothetical vote for mayor in our vignettes. Corruption experience is a respondent's binary answer about giving a bribe in the previous year. Corruption perception is a binary answer as to whether political corruption is a major problem in Moldova. The coefficients are marginal effects of the change from "no" to "yes" for each variable. Robust standard errors are in parentheses. All models control for respondent's age, gender, income, occupation, region, partisanship and ethnicity.

vignette about their own experiences with bribes and their own perceptions of corruption.<sup>18</sup> We then regressed the answers to the relative importance of corruption vs. the economy question on respondents' corruption experience and perception, along with a rich set of controls. The results are given in [Table 4](#).<sup>19</sup>

The first column shows that, overall, personal experience with corruption is more strongly associated than corruption perception with a respondents' tendency to choose corruption as more important for the vote. Respondents who gave a bribe were on average 17 percent more likely to choose corruption, whereas the effect of perception is indistinguishable from zero. Columns 2 and 3 suggest that, as expected, personal experience is a stronger predictor following a pocketbook vignette, and perception is a stronger predictor following a sociotropic vignette. We obtained similar results from a previous version of our experiment we ran in Bulgaria, another high-corruption country; these results are shown in the [Web Appendix](#).<sup>20</sup>

## 5. Conclusion

Scholars have long known that evaluations of valence issues can affect vote choice, but in recent decades the vast majority of work in this regard has focused on the state of the economy. In this paper, we have considered another potential valence issue – corruption – as well as its interaction with the state of the economy. Using a novel experimental design and two original survey experiments, we offer four important conclusions. First, in our low corruption country (Sweden), voters react negatively to corruption regardless of the state of the economy. Second, in our high corruption country (Moldova), voters react negatively to corruption *only when* the state of the economy is also poor. Third, respondents in Sweden react more strongly to corruption stimuli than respondents in

<sup>18</sup> We were unfortunately not able to replicate those questions in Sweden.

<sup>19</sup> Full results including the control variables are available upon request.

<sup>20</sup> The Bulgarian experiment was a  $2 \times 2$  rather than a  $4 \times 2$  experiment without the positive corruption prompts, which is why we have omitted the presentation of those results from the main text of the paper.

Moldova. Finally, in our low corruption country, *sociotropic* corruption voting is relatively more important, whereas in our high corruption country, *pocketbook* corruption voting is equally prevalent.

These findings are consistent with a world in which countries settle into either “good corruption” or “bad corruption” equilibria. Voters in a low-corruption country react strongly to corruption, thus inducing politicians to behave, which in turn reduces corruption. High corruption may make voters less sensitive to it, thus failing to provide incentives for politicians in a high-corruption country to refrain from engage in it. While previous studies have recognized the possibility of multiple equilibria in the prevalence of corruption (Andvig and Moene, 1990; Lui, 1986; Tirole, 1996), the consequences of multiple equilibria for electoral accountability have to our knowledge not been systematically examined. We believe this represents an interesting area for future research. Moreover, our results on the mechanisms of corruption voting are consistent with the notion of the credibility of corruption signals. Where bribe victimization is rare – as in Sweden – voters react more strongly to sociotropic signals. On the other hand, where both political corruption and petty corruption are frequent – as in Moldova – voters are likely to react – when they do – to both types of stimuli similarly. Finally, we hope this paper will encourage other studies to explore the effect of corruption on voting behavior, as well as its interaction with other valence dimensions of vote choice.

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## Appendix A. Supplementary material

Supplementary data related to this article can be found online at <http://dx.doi.org/10.1016/j.electstud.2013.05.007>.

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