

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 three important conclusions based on our empirical analyses. 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. 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 more prevalent.

Key Words: Corruption, Economy, Voting, Political Behavior, Pocketbook, Sociotropic, Moldova, Sweden

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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 which voters might seek to enforce accountability in their political leaders. One other particularly important area - especially in new democracies - is the extent to which political leaders combat, tolerate, or even 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, as per the theme of this special issue.

Thus the goal of this manuscript is to explore whether “corruption voting” varies across good and bad economic conditions. To answer this question, though, we need to address a number of other questions as well. First, what are the micro-level mechanism 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. Recent developments in the economic voting literature have highlighted the fact that one’s partisan predisposition (or even preferred candidate in a single election) can have an effect on one’s economic evaluations; we demonstrate below in Section 3 that one’s reaction to corruption can also be influenced by partisan cues. Thus in order to conduct our analysis free of such partisan biases, we rely on an original experimental vignette we designed to compare

the relative (and interactive) impact of information about the state of the economy and levels of corruption on individual voting behavior. In order to capture the distinction between high corruption and low corruption societies, we embedded the experiment in surveys in Sweden (low corruption) and Moldova (high corruption).¹

Our experiments produce numerous empirical results, but two are particularly germane. First, 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 becomes the norm, 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.

We proceed as follows. In the following section we briefly present a theoretical framework we have developed in more detail elsewhere (self-citation omitted) of the micro-level mechanisms that underlie “corruption voting,” which we refer to as “pocketbook corruption voting” and “sociotropic corruption voting,” and use the framework to develop our experiments. In Section 3, we present new evidence regarding the potential for partisan bias in the evaluation of corrupt behavior – which we suggests gives added credence to our experimental

¹Both experiments took place at roughly the same time, in the spring of 2012, thus holding the state of the global economic crisis relatively constant.

approach in this paper – justify our case selection of Moldova and Sweden, and present our experimental design. In Section 4, we present our findings, and in Section 5 we conclude.

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 & Tverdova 2003, Davis, Camp & Coleman 2004, Della Porta 2000, McCann & Dominguez 1998), or when it is reinforced by revelation of hard information (Banerjee et al. 2009, Chong et al. 2011, Ferraz & Finan 2008, Humphreys & Weinstein 2007). Other studies – though fewer in number – claim that corruption matters through personal exposure (“victimization”) (Gingerich 2009, Lavalley, Razafindrakoto & Roubaud 2008, Seligson 2002).

To distinguish between these two channels, we adopt the nomenclature commonly used in 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.²

Our conceptualization of pocketbook and sociotropic corruption voting as separate processes is justified by findings from studies on the methodology of corruption measurement. Studies in this literature have repeatedly shown - along numerous dimensions - that the relationship between personal experience with corruption and corruption perception is quite

²These 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. Exploring the effects of different types of corruption is an excellent subject for future research, but beyond the scope of the current paper.

tenuous (Abramo 2007, Donchev & Ujhelyi 2009, Krastev & Ganev 2004, Mocan 2004, Morris 2008, Rose & Mishler 2007). Our own analysis of Eurobarometer and Transparency International data reveals similar findings: personal exposure to demands for bribes is almost completely uncorrelated with one's beliefs regarding overall levels of corruption among national politicians (see Web Appendix Section 1 for details).

With this framework in mind, we view corruption as a valence issue: we assume that for voters more is worse and less is better.³ 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, research we conducted in Bulgaria revealed that effects from our pocketbook vignette were stronger among respondents who had personal experience with corruption in their own lives (self-citation omitted). Should this hold in a cross-national context, then we would expect the pocketbook prompt to be correspondingly more important in Moldova – where more people have had personal experiences with corruption - than in Sweden.

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. For example, one might hypothesize that corruption voting is a second order concern behind the state of the economy: as long as the economy performs well, corruption is tolerated; but when the economy is performing poorly, corruption voting kicks in. Alternatively, it may be the case that corruption voting is a luxury only associated with good times: if the economy is performing poorly, then corruption becomes irrelevant; but when the economy is performing well, then voters can have the luxury to care about issues beyond the state of the economy.

³It is of course possible in some circumstances that citizens might reward corruption (Barbera, Fernandez-Vazquez & Rivero 2012); while we do not explicitly address this hypothesis here, our experiments are completely suitable for testing for such a reaction and interested readers can consider this to be an alternative null hypothesis.

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 & Tverdova 2003, Anduiza, Gallego & Munoz Forthcoming).

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 an appropriate punishment should be. 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.⁴

Table 1 shows that when a respondent is told that the mayor is from her preferred party, she is less likely to believe that the hypothetical mayor is indeed guilty than when a respondent is not given any information on the mayor’s party affiliation, and in particular, than when a respondent is told that the mayor is from their most disliked party. Similarly, there is a significant difference in the proposed punishment for the mayor between those who received a preferred party cue and those who received a disliked party cue, with the former being much less punitively inclined than the latter, or those who did not receive any information on the mayor’s partisanship.

⁴We provide the full text of the vignette and the follow-up questions in the Web Appendix.

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)
Observations	458	481	646	644
Diff. preferred/disliked	-0.947	-0.507	-2.244	-0.612
p-value	0.006	0.009	0.000	0.009

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

Note: The text of the experimental vignette is given in the Web Appendix. *Mayor guilty?* is constructed from a question of how likely the respondent thought the mayor was guilty of corruption, from 0 to 100 percent in 10 percent increments. The variable ranges from 1 to 11. *Fair punishment?* is constructed from a question of what punishment the respondent thought was appropriate if the mayor were found guilty. The variable ranges from 1 to 7, in increasing harshness of punishment. Exact question wording is given in the Web Appendix. Party cues were randomly assigned. Preferred and disliked party were determined over the course of the survey from a battery of questions.

Of course, similar results have been found in the economic voting literature.⁵ 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, however, 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 & Williams 2010).⁶

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

⁵See for example Anderson, Mendez & Tverdova (2004), Evans & Andersen (2006), Evans & Pickup (2010), Ladner & Wlezien (2007), Lewis-Beck (2006), Lewis-Beck, Nadeau & Elias (2008), and Wlezien, Franklin & Twiggs (1997).

⁶Endogeneity 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.

in Moldova. Although it is probably not that debatable to call Moldova a high corruption country and Sweden a low corruption country, in Figure 1 of the Web Appendix we provide evidence from Transparency International substantiating this claim. Here is the actual text of the Moldovan experiment.⁷

[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

In your opinion, what is the likelihood that NAME will vote for this mayor in this election?

1. Definitely will not
2. Probably will not
3. Might or might not
4. Probably will
5. Definitely will

As an experiment with a 4X2 design, we have 8 conditions that were each randomly assigned to approximately 12.5 percent of the sample.⁸ In Section 4.1, we begin by pool-

⁷The one 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.

⁸In the Web Appendix, we demonstrate that most pre-treatment variables are balanced across experimental conditions. In Moldova, a different experiment with a hypothetical mayor and corruption allegations, the results of which we presented in Table 1 above, was present on the same survey, and preceded our main

ing together our different corruption vignettes so that the experiment collapses into a 2X2 design, thus allowing us to examine a simple *corruption effect*, which we define as the comparison between respondents’ vote decisions upon receiving any negative corruption vignette (INSERT1a or INSERT2a) compared to receiving a positive corruption vignette (INSERT1b or INSERT2b). We can then compare the corruption effect when the economy has worsened (CONDITION 1) to the corruption effect when the economy has improved (CONDITION 2). Since we are also interested in comparing effects across our two survey experiments, we pool the data together and estimate the following model:

$$Vote_{i,j} = J * \{\beta_{0,j} + \beta_{1,j}Corruption_{i,j} + \beta_{2,j}Economy_{i,j} + \beta_{3,j}Corruption_{i,j} * Economy_{i,j} + \epsilon_{i,j}\}, \quad (1)$$

where i denotes a respondent in country j ; $J \in \{\text{Sweden, Moldova}\}$ denotes the country sample; $Vote$ is constructed from the vote question shown above, and ranges from 1 (“definitely not”) to 5 (“definitely will vote”); $Corruption$ equals one if a respondent received a negative corruption vignette (INSERT1a or INSERT2a), and zero otherwise; $Economy$ equals one if a respondent received CONDITION2, and zero otherwise. This specification gives point estimates and standard errors equivalent to those obtained from two separate single-country specifications, but also allows us to test hypotheses across countries.⁹

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.

We utilize a specification similar to that in equation 1, whereby $Corruption$ is divided into

experiment. We verified that this survey experiment did not contaminate the results presented in what follows, as all our results remain unchanged when we control for the scenario the respondent received in this earlier survey experiment.

⁹That the specification in equation 1 is equivalent to running two separate regressions for each country is seen by simply distributing J through the equation. The key assumption is that the data-generating process is the same in both countries allowing the data to be pooled and stacked.

Pocketbook and *Sociotropic* terms, each equal to one if a respondent received the appropriate negative vignette (INSERT1a and INSERT2a for the pocketbook and the sociotropic term, respectively), and zero otherwise. Consequently, we have two interaction terms per country, as opposed to one in equation 1.

In comparing results from the two countries, we need to take into account differences in the sampling design. The Swedish survey was based on an online opt-in sample, whereas the Moldovan survey was conducted on a population probability sample.¹⁰ Unlike the Moldovan survey, the online opt-in sample is not representative of the Swedish population. To rectify this, we reweight the Swedish data using post-stratification demographic weights throughout the analysis. While this is an imperfect solution, we further show in the Web Appendix that our results are insensitive to several different types of weights.¹¹

4 Results

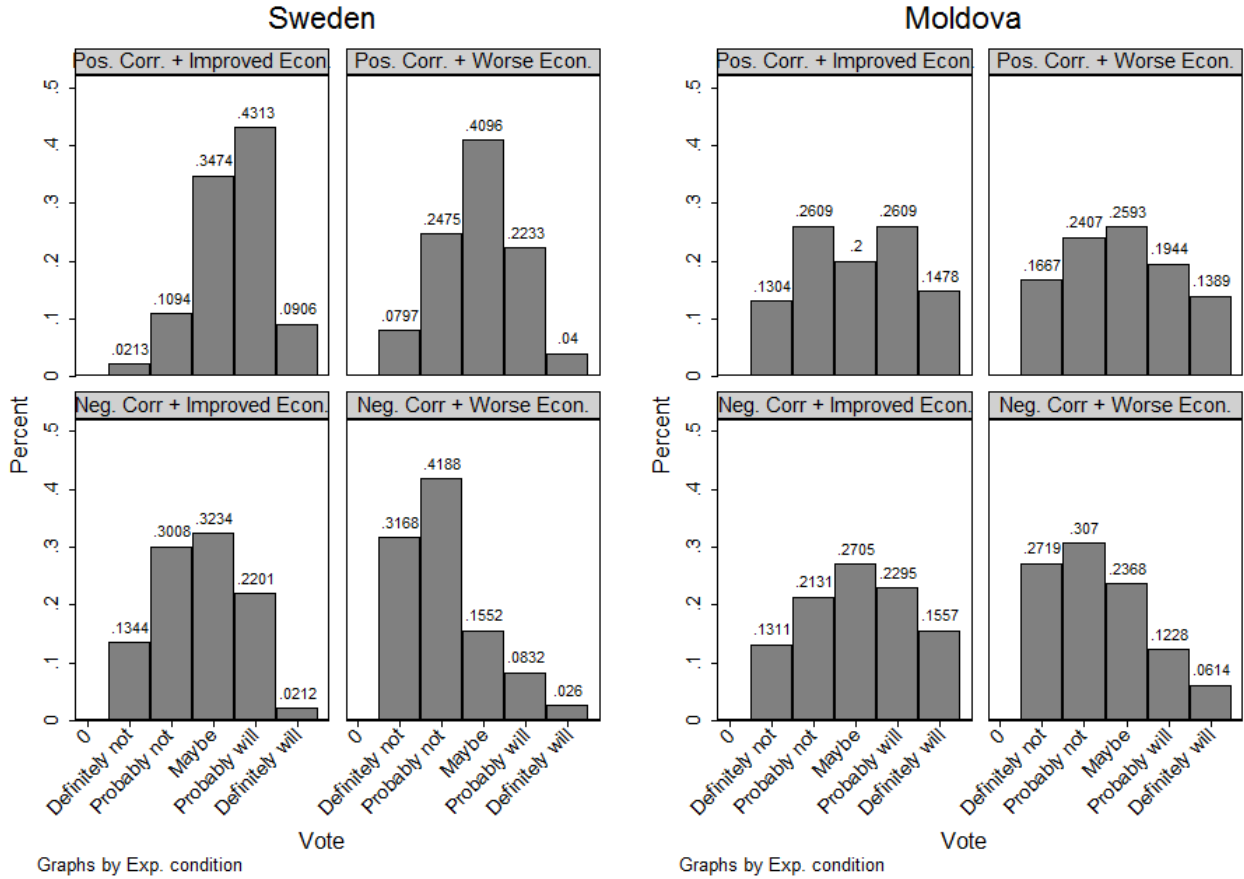
4.1 Main Results

The summary of the results from a 2X2 setup (i.e., when we consider a single *corruption effect* without accounting for whether it is a pocketbook or sociotropic cue) are given in Figure 1, and the inferences are drawn from the estimates given in Table 2. We observe first of all that Swedish respondents punish corruption irrespective of the condition of the economy.

¹⁰The experiment in Sweden was conducted as part of the University of Gothenburg’s Laboratory of Opinion Research (LORe)’s Citizen Panel Study (<http://mod.gu.se/english/LORe/>). The Moldovan experiment was conducted as part of our own random probability-based survey and implemented in the field by IMAS-Inc. (<http://www.imas-inc.md/>).

¹¹Moreover, comparisons across the two experiments are also complicated by the different sample sizes. The Swedish survey contains 1,852 complete cases, while the Moldovan survey contains only 459. The statistical power of our estimator is considerably higher in Sweden than in Moldova. In order to make better inferences when comparing the effects across the two countries, we perform a sample-size adjustment to the 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 performed by estimating the hypothesis tests on a subsample of Swedish data equivalent to the Moldova sample size, and comparing the standard errors to those obtained on the full Swedish sample. Due to space constraints, all the details are given in the Web Appendix.

Figure 1: Main Results from Sweden and Moldova



Note: The shares shown as bars are weighted by probability weights.

Likewise, Swedes punish incumbents for worsening economic conditions irrespective of the corruption level. 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 bad 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

Table 2: Main Results from Sweden and Moldova - Estimates

		(a) Sweden	(b) Moldova
Corruption effect economy = better	(1)	-0.768 (0.090) ^{***}	0.031 (0.165) [0.077]
Corruption effect economy = worse	(2)	-0.814 (0.097) ^{***}	-0.503 (0.166) ^{***} [0.078] ^{***}
Economy effect corruption = positive	(3)	-0.564 (0.090) ^{***}	-0.137 (0.172) [0.080] [*]
Economy effect corruption = negative	(4)	-0.610 (0.098) ^{***}	-0.671 (0.159) ^{***} [0.074] ^{***}
Interaction: corr. = negative, econ. = worse (2)-(1), (4)-(3)	(5)	-0.046 (0.133)	-0.534 (0.234) ^{**} [0.109] ^{***}
N		1852	459
Diff. corruption econ. = better	(1 a)-(1b)	-0.798	(0.188) ^{***} [0.119] ^{***}
Diff. corruption econ. = worse	(2 a)-(2b)	-0.310	(0.192) [0.124] ^{***}
Diff. econ. corruption = positive	(3 a)-(3b)	-0.427	(0.194) ^{**} [0.120] ^{***}
Diff. econ. corruption = negative	(4 a)-(4b)	0.060	(0.187) [0.122]
Diff. interaction effect	(5 a)-(5b)	0.488	(0.269) [*] [0.171] ^{***}

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

Note: The dependent variable is constructed from a question as to whether the respondent thinks the hypothetical person in our vignettes would vote for the mayor. It ranges from 1 (“definitely not”) to 5 (“definitely will vote”). In brackets, standard errors are adjusted for the smaller size of the Moldova sample, to make it comparable to the Sweden sample. The adjustment approach is described in the text and the details are given in the Web Appendix. The corruption “effect” denotes the change in the vote from receiving a positive corruption vignette to receiving a negative corruption vignette. The economy effect is defined analogously for the economy vignettes.

out of 4 cross-country differences in coefficients are statistically significant).¹² Finally, the interaction between the bad economy and bad corruption is decidedly larger in Moldova.

Our interpretation of these findings is that it appears that Swedes use 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.” In such cases, a bad record on corruption and/or the economy is immediately punished; this in turn creates strong disincentives to engage in corruption and/or mismanage the economy (performance effect), and likely creates strong disincentives for bad types (both dishonest and incompetent) to run for political office (selection effect). Such positive incentive effects in turn should make corruption (and probably bad economic policies) less common, and thus – in a positive feedback loop – make voters more likely to react adversely in the limited instances when they do occur. This is exactly what we observe in the Swedish data: despite being a country with low levels of corruption (see Web Appendix), voters react dramatically to the corruption stimuli in our experimental treatments.

Moldova, on the other hand, seems to represent a “bad corruption equilibrium.” Bribe victimization is rampant, and perception of corrupt officials is widespread (see 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.”¹³ The same holds for the effect of the bad economy. However, a bad economy coupled with bad

¹²This may partly be an artifact of the sampling designs. The online opt-in sample in Sweden consists of respondents who are younger, more highly educated, more liberal and politically interested than the average individual from the Swedish population, while the Moldovan sample is representative of the Moldovan population. The fact that we use post-stratification demographic weights (based on gender, age and education) on the Swedish cases should mitigate against this concern. Additionally, in the Web Appendix, we provide some evidence that self-selection is not driving this finding: our results are substantively unchanged when we adjust for the skewness in political interest and left-right self-placement by reweighting the data not just based on demographic characteristics, but also based on these political characteristics.

¹³This is consistent with the moral hazard agency models with only one type of politicians, in which any equilibrium is supported with voters who are indifferent between keeping the incumbent or replacing him with the same type of challenger (Ferejohn 1986, Besley 2006)

corruption may reveal additional information about the politician: the politician is failing on two rather than one dimensions. In such a 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. But, our results also indicate that in a good economy, the politician can go unpunished if he decides to become corrupt. This evidence is consistent with the conjectures in the existent literature on the trade-off among dimensions of type (Rundquist, Strom & Peters 1977, Weitz-Shapiro & Winters Forthcoming): 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). The first point to note is two important similarities with the main results: we only find an interaction effect between corruption (be it sociotropic or pocketbook) and the economy in Moldova, but the overall effect of both types of corruption prompts remains statistically significantly stronger in Sweden in three out of four cases (rows 1, 2, and 4 of column 3 labeled “Cross-country difference” in Table 3).

A second important point is that we find evidence of effects for *both* the sociotropic and pocketbook prompts in both countries. When an effect is present, it is present for both types of corruption treatments; when it is not (i.e., when the economy is performing well in Moldova), it is missing for both. However, the relative importance of the two different 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 for a negative difference), even though both are present. In Moldova, however, pocketbook voting is slightly stronger (although

Table 3: Corruption Voting in Sweden and Moldova

	Sweden	Moldova	Cross-Country Diff.
<i>Economy = better</i>			
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***
<i>Economy = worse</i>			
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***
<i>Interaction effect</i>			
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

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is constructed from a question as to whether the respondent thinks the hypothetical person in our vignettes would vote for the mayor. It ranges from 1 (“definitely not”) to 5 (“definitely will vote”). For purposes of readability, standard errors are omitted; instead, we show the significance levels. The full table including standard errors is shown in the Web Appendix. Standard errors are adjusted for the smaller size of the Moldova sample, to make it comparable to the Sweden sample. The adjustment approach is described in the text and the details are given in the Web Appendix. The “pocketbook” effect denotes the change in the vote from receiving a positive bribe vignette to receiving a negative bribe vignette. The “sociotropic” effect is defined analogously for the corruption perception vignettes.

the differences are not statistically significant; rows 3 and 6 of column 2). *Cross-country* comparison of these within-country differences in the *composition* of corruption voting is statistically significant (rows 3 and 6 of column 3 in Table 3); sociotropic voting dominates pocketbook voting in Sweden (as shown by a positive difference) but the reverse is true in Moldova. This finding is thus consistent with the cross-country hypothesis – suggested by our within country findings from previous research in Bulgaria (self-citation omitted) – that pocketbook corruption prompts ought to have a relatively larger impact on voting in countries where more citizens have personally experienced requests for bribes.

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 focussed 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 based on our empirical analyses. 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; 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 our low corruption country, *sociotropic* corruption voting is relatively more important, whereas in our high corruption country, *pocketbook* corruption voting is more prevalent.

These findings are consistent with a world in which corruption voting is fueled by the level of informativeness of corruption: in Moldova, a scenario about being asked to provide a bribe resonates with citizens; in Sweden, where politicians are much less likely to be assumed to be corrupt, finding out a politician is corrupt may provide more novel information. Moreover, they are also consistent with a state of the world in which countries settle into either “good corruption” or “bad corruption” equilibria. While previous studies have recognized the possibility of multiple equilibria in the prevalence of corruption (Andvig & Moene 1990, Lui 1986, Rasmusen 1996, Tirole 1996), the consequences of multiple equilibria on electoral accountability have to our knowledge not been systematically examined. This is an interested area for future research. Moreover, we hope this paper will encourage other studies that look at corruption as a valence issue, and its interaction with other valence dimensions of vote choice.