Web Appendix for "The Economy, Corruption, and the Vote: Evidence from Experiments in Sweden and Moldova"

August 18, 2013

This Web Appendix accompanies the paper. In Section 1 we present partial correlations between corruption perception and experience, justifying the decision to treat pocketbook and sociotropic corruption voting not as substitutes but as separate causal mechanisms. Section 2 graphically shows estimates of corruption perception and experience for Sweden, Moldova, and other European countries. In Section 3 we show tests of the validity of our experimental randomization. Section 4 graphically summarizes our main results. Section 5 discusses the procedure behind the sample-size adjusted standard errors we report in the text. Section 6 reports the full results from Table 3 in the paper. Section 7 reports the main results for Sweden using alternative weights. Section 8 reports the results when Moldovan sample is skewed towards the unweighed Swedish sample. In Section 9, we use auxiliary data to compare the corruption attitudes of similar respondents to those in our Swedish sample and respondents not in our Swedish sample. In Section 10, we replicate the text of the partisan bias experimental vignettes we discuss in the second section of the paper. Finally, in Section 11, we show and discuss the results from our earlier 2×2 experiment in Bulgaria.

1 Correlation between Corruption Perception and Experience

Using the Eurobarometer and Transparency International's Global Corruption Barometer surveys, we estimate the partial correlation coefficients between reported perceptions and experiences in all available European countries for a number of public domains (politicians at different levels of government, judiciary, police, health and education), taking into account demographic, socio-economic and geographic characteristics of respondents and country fixed effects.[1] The estimated correlations are rather low (see Table 1). While the sign of the coefficients accords with our priors of positive reinforcement – that increased exposure is associated with increased perception – none of the coefficients exceeds 0.15. In particular, the correlation between overall exposure to bribes and corruption perception of politicians, which are measures we rely on in the analysis in the paper, is nearly zero. This is particularly striking considering that there is likely some priming in our data, as survey questions probing experiences and perceptions are invariably clustered together and asked consecutively. Conceptualizing the two forms of corruption voting as separate therefore appears to be meaningful.

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

2 Corruption in Moldova and Sweden in Comparative Perspective

See Figure 1.

3 Balance Tests

In Table 2, we present results of the balance tests. We regress each reported variable on a set of indicators for each experimental condition (in total eight of them, as shown in the paper), and report the p-value associated with the F-test statistic on the hypothesis that all experimental condition indicators are jointly equal to zero. Where the p-value is greater than 0.05, experimental condition indicators as a group are not jointly statistically significantly different from zero at the conventional level. We interpret this as evidence of a variable being balanced across experimental conditions, suggesting that randomization was successful. Table 2 shows that by and large this is the case in our survey experiments.[2]

4 Graphical Summary of Main Results

The summary of the results from a 2×2 setup (i.e., when we consider a single *corruption effect* without accounting for whether it is a pocketbook or sociotropic vignette) are given in Figure 2.

5 Sample-Size Variance Adjustment

Our two survey experiments differ markedly in the available sample size. The Swedish survey contains 1,852 complete cases, while the Moldovan survey contains only 459. The statistical power of our estimator is thus considerably higher in Sweden than in Moldova. To make the inferences based on the two samples more comparable, we adjust the variances of the estimates based on the Moldova sample in the following fashion.

We begin by assuming that the Swedish data were created by the same data generating process that created the data in Moldova. This should be true by construction, since we ran the same experiment in both places.[3] This assumption allows us to treat the behavior of the variance-covariance matrix using different sample sizes of the data from Sweden as informative about the behavior of the variance-covariance matrix obtained by using only the data from Moldova.

We therefore compare the estimates of the variance-covariance matrix from the specification given in equation 1 in the paper on the full Swedish sample (1,852) to the estimates on progressively smaller samples in increments of approximately 10 percent, until we hit the sample size of our Moldova survey. We do this by taking 1,000 bootstrapped subsamples for each incrementally

[2]Controlling for variables for which p-values in Table 2 are below 0.1 does not affect the results we present in the paper or below.

[3]This is not entirely true, given the differences in the sample design and different rates of missingness. We deal with the sample design below. The rate of missingness is about 6 percent in Sweden and about 27 percent in Moldova. The higher rate of missingness in Moldova is due to the inclusion by the survey administrator of the "Hard to say" category to the responses to our follow up vote question. Because of randomization, missingness is not a particular issue, however. We verified that missingness is uncorrelated with our experimental conditions, and therefore is not likely to induce bias when we use listwise deletion. It does induce inefficiency, but we address this issue shortly.

smaller sample size, and averaging the estimates across these 1,000 subsamples.[4] We then compare the variance-covariance matrix obtained by estimating the model in equation 1 on the full sample, Ω^F, to the variance-covariance matrix based on the appropriate subsample, Ω^S.[5] We define *the sample-size variance-covariance inflation factor* as:

$$f = \frac{1}{k^2} \sum_{1}^{k^2} \frac{\Omega^F_{[j,m]}}{\Omega^S_{[j,m]}},$$

where $k = 4$ is the number of parameters from equation 1 estimated on Swedish data, and $\frac{\Omega^F_{[j,m]}}{\Omega^S_{[j,m]}}$ is the element-wise ratio of the $[j, m]$th element of the two variance-covariance matrices. In words, f is the average of the ratio of each element of the two matrices, and represents a single metric by which we judge how much larger on average the estimate of the variance around a parameter in equation 1 is because of a smaller sample size.[6]

Figure 3 shows f for various subsamples of the Swedish survey. The figure shows that the variance around the estimates obtained on a subsample of the Swedish data of the size of the Moldova sample is on average as much as 80 percent larger than when the model is estimated on the full sample. This is the amount by which we adjust the estimates of the variances around the parameters estimated on the Moldovan data. Part of this adjustment of course also affects the estimates of the uncertainty around the hypothesis tests of the equality of coefficients across the two countries.

A potential drawback to this approach is that we use the data from Sweden to adjust inferences on the parameters obtained on the data from Moldova. If there is something perfectly collinear with the country dummy (such as the sampling design), our adjustment might be inappropriate. An alternative way is to directly perform a sample-size adjustment on the estimates obtained on the Moldova sample. For the standard errors around the the parameters in equation 1 in the paper, this is indeed straightforward. Consider for example $\beta_{1,M}$ from equation 1, where M stands for Moldova. Since *Corruption* and *Economy* are binary variables,

$$\beta_{1,M} = \mathbb{E}\left\{[Vote|Corruption = \text{negative}, Economy = \text{improved}] - \right.$$
$$\left. \mathbb{E}\left[Vote|Corruption = \text{positive}, Economy = \text{improved}\right]|Country = \text{Moldova}\right\} = \bar{x} - \bar{y}$$

In other words, $\beta_{1,M}$ represents a simple two-groups means comparison test. The test-statistic[7] is:

$$t = \frac{\bar{x} - \bar{y}}{\left(\frac{(N_x-1)s_x^2+(N_y-1)s_y^2}{N_x+N_y-2}\right)^{1/2}\left(\frac{1}{N_x}+\frac{1}{N_y}\right)^{1/2}},$$

[4] Since we need observations from each experimental condition to properly estimate the model in equation 1 in the paper, we cluster the bootstrapping procedure by the eight vignettes.

[5] More appropriately, we would compare standardized variances, as point estimates may differ too. In practice, point estimates across the different subsamples are very similar to those from the full sample – as they should be under random subsampling – and so we focus only on the Ω's.

[6] With k = 4, f is an average of 16 elements – ratios of four variances and 12 covariances from each variance-covariance matrix. Of course, in practice, we have fewer unique elements because of the symmetry of the variance-covariance matrix. In fact, because all our independent variables are binary, f is described entirely by the four variances, and in our procedure, we restrict our attention to them.

[7] Assuming equal but unknown variances.

where s_i is the standard deviation in group i. This test statistic can be directly adjusted by replacing N_x and N_y with the values we would have had if we had the sample size in Moldova equivalent to that in Sweden. With this kind of adjustment, we get remarkably similar estimates to those of f (results are available upon request). Since this direct adjustment is more complicated with the tests of the cross-country equality of coefficients, we prefer to use f.

6 Full Corruption Voting Results

In Table 3, we provide the point estimates reported in Table 3 in the paper, as well as all the associated standard errors. We report the sample-size adjusted standard errors using the procedure explained in Section 5 above. Results with unadjusted standard errors are available upon request.

7 Alternative Weights

In the paper, we show the estimates where our Swedish respondents are reweighted using demographic weights based on age, gender and education, so as to make the Sweden sample more comparable to the Moldova sample. In Table 4, we show that the results from the paper are unaffected when we also account for the skewness in political interest and left-right self-placement. If anything the results are slightly stronger with these alternative weights. These results further the concerns that our results are solely driven by differences in sampling.

8 Reweighting the Moldova Sample

Another way to examine the effect of sampling is to compare results in Moldova and Sweden after reweighting the Moldovan sample to make it similarly skewed as our Swedish sample. The opt-in survey in Sweden over-samples younger, male, and educated respondents. Therefore, when re-weighting the Moldovan sample, we need to downweight women, the older, and less educated, and weight up the younger, male and more educated respondents.[8] We perform the reweighting in Moldova based on the deviations in Sweden between the opt-in sample and the *Swedish* population.[9] On the other hand, we leave the Swedish sample unweighted. We then rerun our analysis and present results in Table 5.

While the point estimates change, the interpretation and inference largely remain the same compared to Table 2 in the paper. Moldovan respondents are now somewhat more responsive to corruption in general, but not sufficiently so to change the substantive interpretation of the results presented in the paper.

9 Addressing Selection Bias with Auxiliary Data

Weighting does not necessarily solve the selection problem caused by the opt-in sampling in Sweden, as it can correct only for the misrepresentation of those who took the survey. Namely, the concern

[8] The survey in Sweden also over-represents the politically interested, liberal, and richer respondents. Unfortunately, our Moldovan survey does not contain comparable measures of ideology, political interest or income. We therefore ignore these dimensions.

[9] In other words, we assume that Swedish and Moldovan populations are identical. This is clearly not true, but is unavoidable in this context.

is that those who self-selected into the Swedish sample are more sensitive to corruption than those not covered by our survey, and that this in turn drives the differences we uncover between Sweden and Moldova, rather than any substantive difference between otherwise representative populations. To address this selection issue more directly, we use auxiliary data to compare the attitudes about corruption and experiences with corruption between similar respondents to those in our Swedish sample and respondents not in our Swedish sample. We use the 2007 Global Corruption Barometer (the last such survey to include Sweden). Conveniently, the 2007 GCB is a nationally-representative online survey, allowing us to utilize the representativeness of the 2007 GCB sample while keeping the survey mode constant.[10] We perform exact matching (with replacement) of respondents from our survey and the 2007 GCB survey on four demographic variables available in both datasets: age, gender, education, and income. We then examine the responses of matched and unmatched respondents in the GCB survey to four different questions about corruption: (1) expectation about corruption in the next 3 years, (2) view on governments effectiveness in fighting corruption, (3) perception of corruption among various political and economic sectors, and (4) experience with bribe extortion.

We compare average responses to these four questions between matched and unmatched respondents. The matched respondents approximate those who self-selected into our survey. Analogously, unmatched respondents approximate those who are missing from our survey. Comparing the responses of these two groups should give us an idea in which direction – if any – self-selection into our online sample biases the results we presented in the paper. We further compare weighted and unweighted responses among matched respondents only, to check whether and to what extent weighting self-selected respondents successfully corrects for selection bias we identify when comparing matched and unmatched respondents. The results for these comparisons, for all four corruption variables, are given in Figure 4.

Considering the scale, the differences in Figure 4 are minor. As expected, unmatched respondents are older, less educated, more likely to be female, and with lower incomes than matched respondents. Confidence intervals are widest for the unmatched sample because only one tenth of the respondents from our survey are unmatched with those from the 2007 GCB survey. If anything, Figure 4 shows that unmatched respondents have *more negative* attitudes about corruption. Also, as desired, weighting the matched respondents generally moves the average attitudes in the direction of the attitudes of the unmatched respondents. This suggests that the weighting strategy we used in the paper was quite successful at recovering unbiased quantities of interest. To the extent that more negative attitudes make respondents more likely to respond strongly to our vignettes, the selection bias works *against* us, in that our results on only self-selected respondents should *underestimate* the effects of corruption we want to measure. This implies that the differences we find between Sweden and Moldova are *smaller* than they would have been had we had a more representative sample in Sweden.

10 Partisan Bias Experimental Vignette

We reproduce the text of the partisan bias experiment, the results of which are reported in the paper. We reproduce the version run in Bulgaria; the one run in Moldova is very similar, with country-specific information changed as appropriate.

[10] Transparency International Global Corruption Barometer 2007, Appendix 2, `http://archive.transparency.org/content/download/27256/410704/file/GCB_2007_report_en_02-12-2007.pdf`.

There has been a lot of talk about corruption in Bulgaria. Let's say that you hear that the mayor of a medium-sized city is accused of influencing the awarding of public tenders in his jurisdiction to benefit personal friends or supporters. Then you find out that the mayor [INSERT1/INSERT2/INSERT3] strongly denies these allegations and insists they are lies spread by political opponents.

INSERT1 = a member of [MOST PREFERRED PARTY].
INSERT2 = a member of [MOST DISLIKED PARTY].
INSERT3 = NO PARTY INFORMATION.

1. In your opinion, how likely is it that this mayor is guilty of these charges?

0% (absolutely certain he is NOT guilty)
10%
...
90%
100% (absolutely certain he is guilty)

2. If the mayor were found guilty of these charges, what do you think would be a fair punishment?

1) publicly admonished, but allowed to keep his position as mayor.
2) removed from his position as mayor.
3) removed as mayor and banned from serving in government again.
4) removed as mayor, banned from serving in government again, and fined BGN 50,000.
5) removed as mayor, banned from serving in government again, fined BGN 50,000, and sent to prison for 1 year.
6) removed as mayor, banned from serving in government again, fined BGN 50,000, and sent to prison for 3 years.
7) removed as mayor, banned from serving in government again, fined BGN 50,000, and sent to prison for 5 years.

11 Results from Experiment in Bulgaria

We conducted an earlier version of the corruption experiment in Bulgaria. The main difference from the experiment presented in the paper is that it is a 2×2 rather than a 4×2 experiment, without the positive corruption prompts (INSERT1b and INSERT2b). The Bulgarian experiment is therefore not directly comparable to the results we present in the paper, given that we cannot measure the difference in the vote between a positive and a negative corruption vignette. However, as with the experiment in Moldova, we asked the respondents in Bulgaria after they were shown the vignette whether they thought corruption or economy was more important for the hypothetical voter's decision. We present those results in Figure 5.

Regardless of the state of the economy, respondents were approximately ten percent more likely to choose corruption following the (negative) pocketbook treatment condition compared to the (negative) sociotropic treatment condition. These results are statistically significant at $p < .06$. Like in Moldova, respondents from Bulgaria – also a high-corruption country – seem to be relatively more responsive to the pocketbook prompt.

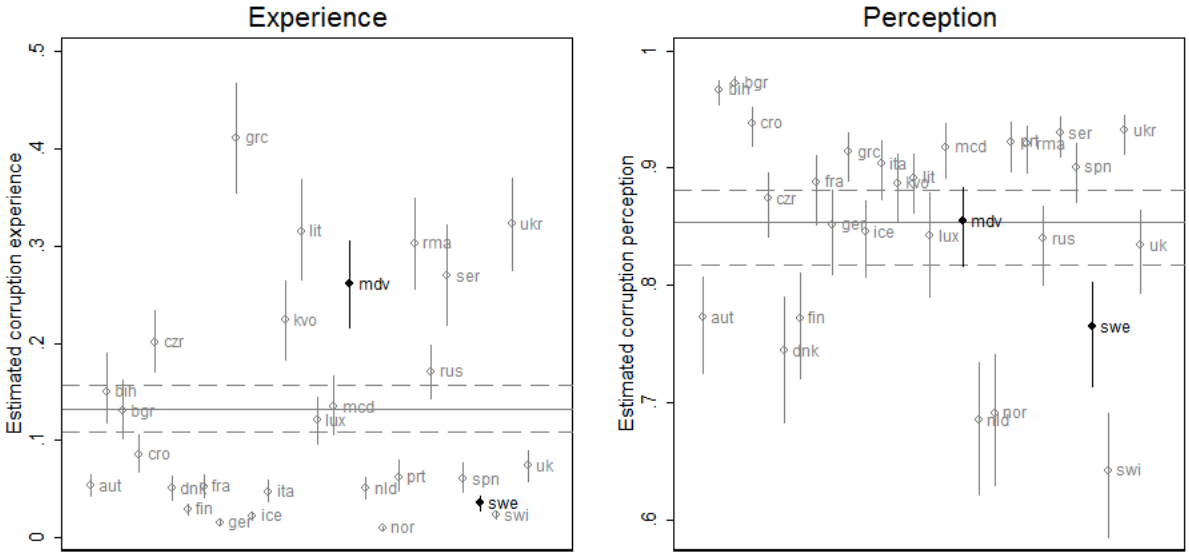
Moreover, as in Moldova, we also asked respondents before presenting them with the vignette about their own experiences with bribes and their own perceptions of corruption. Thus, as in the paper, we regress the answers to the corruption vs. economy question on respondents' corruption experience and perception, along with a rich set of controls.

The results, shown in Table 6, are similar to those from Moldova we presented in Table 4 in the paper (full results are available upon request). The first column shows that personal exposure to corruption makes respondents about 10 percent more likely to choose corruption as more important for the hypothetical vote, whereas the effect of perception is not significantly different from zero. Columns 2 and 3 show that this effect occurs only following the pocketbook vignette.

References

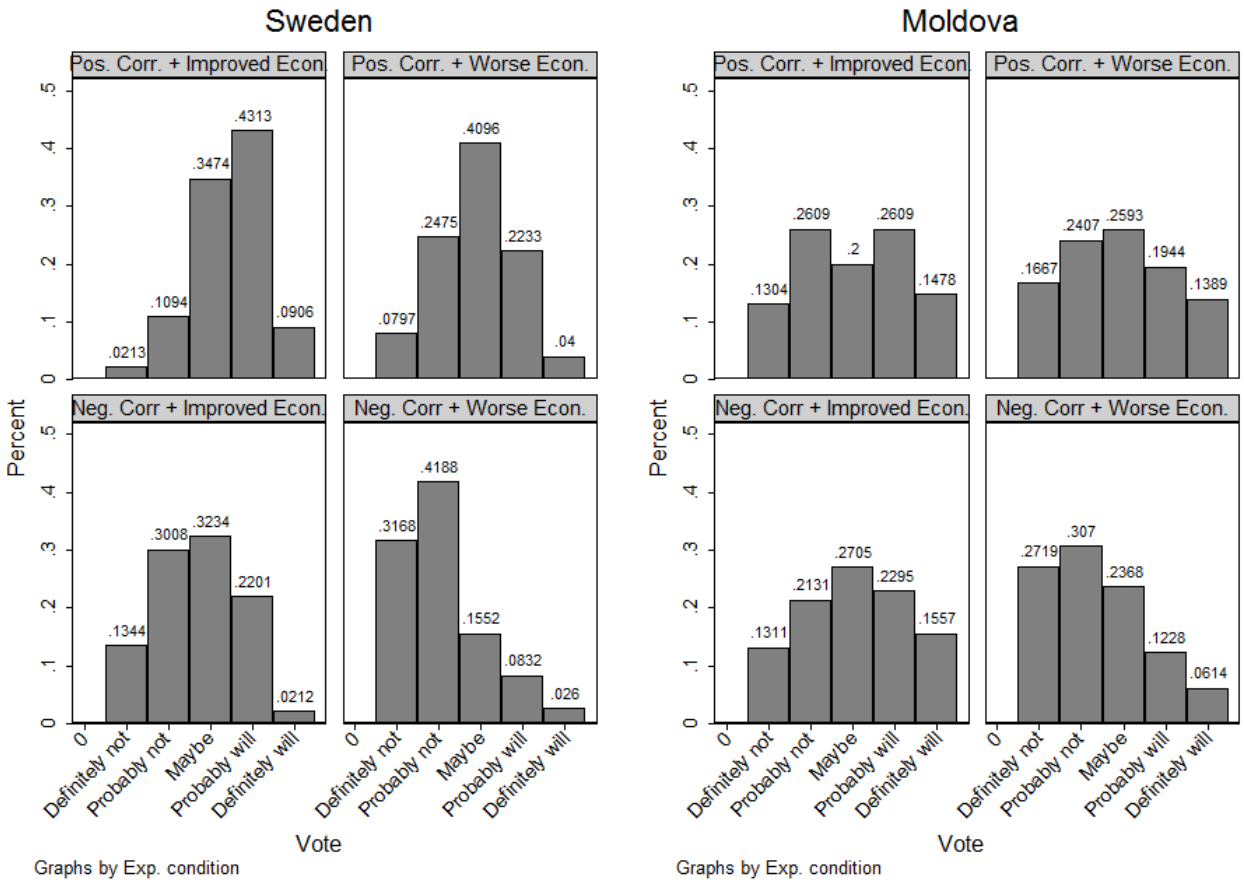
Dahlberg, Stefan, Sebastian Lundmark & Johan Martinsson. 2012. "How Representative is a Self-Selected Web Panel? The Effect on Representation of Different Sampling Procedures and Survey Modes." Working Paper.

Figure 1: Corruption Experience and Perception in Comparative Perspective



Note: All estimates are averages based on the Transparency International's 2007 Global Corruption Barometer data. The dots represent the median predicted probability of a positive response to a question about having been asked for a bribe (left panel), or perceiving politicians as corrupt (right panel), as a function of a set of demographic, socio-economic, attitudinal and geographic covariates and country fixed effects. The estimate for each country is obtained by keeping all the variables except the fixed effects at the medians or means of the *entire* sample. The caps are the 95 percent confidence intervals based on 500 simulations. The horizontal line is the median estimate for the entire sample of European countries presented in each panel and the dashed lines are the associated 95 percent confidence intervals.

Figure 2: Main Results from Sweden and Moldova



Note: "Pos. Corr." means a respondent received the positive corruption vignette, "Neg. Corr." means a respondent received the negative corruption vignette. "Improved Econ." means a respondent received an improved economy vignette, "Worse Econ." means a respondent received a worsened economy vignette. The shares shown as bars are weighted by probability weights.

Figure 3: Sample-Size Variance-Covariance Inflation Factor

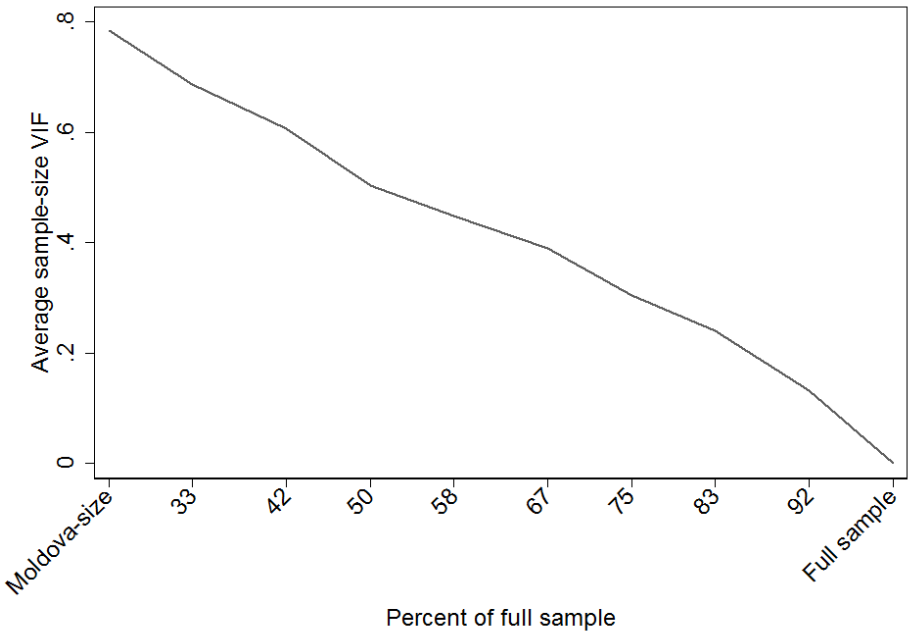
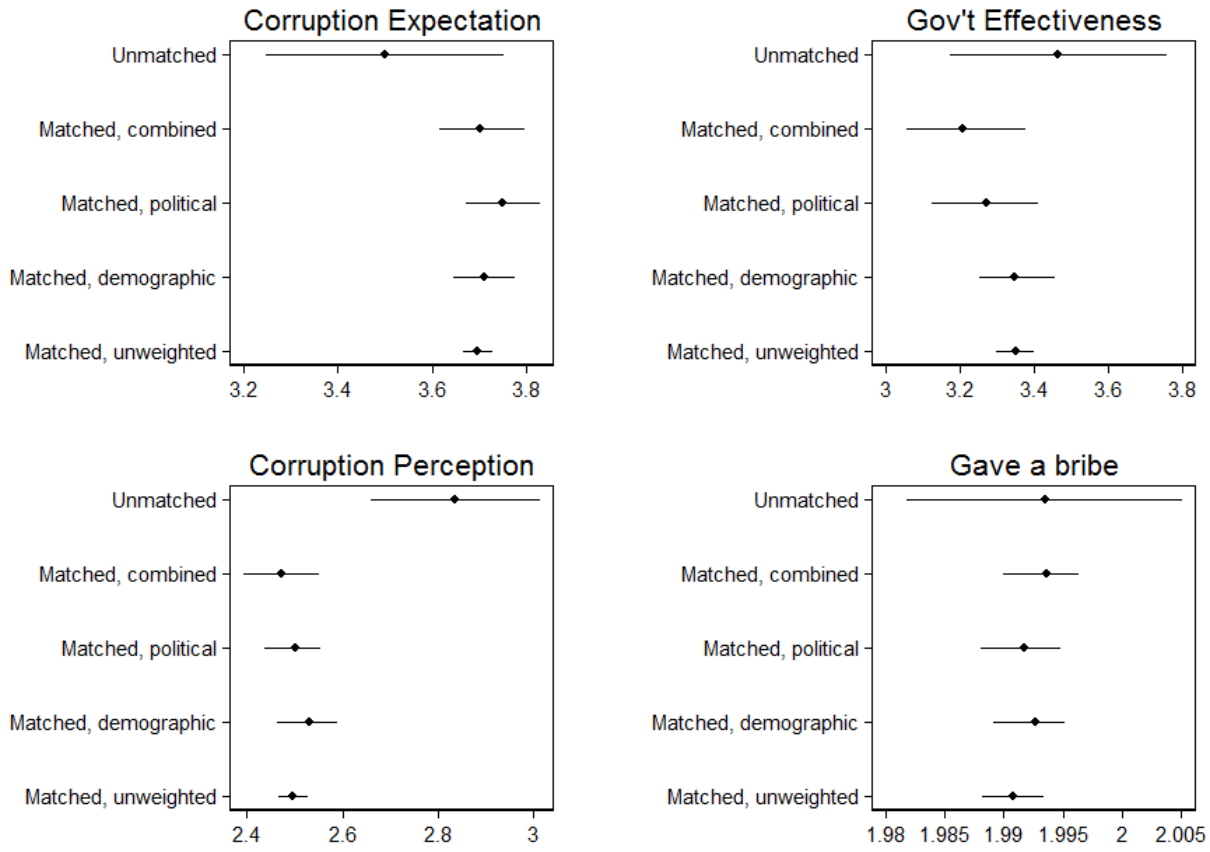
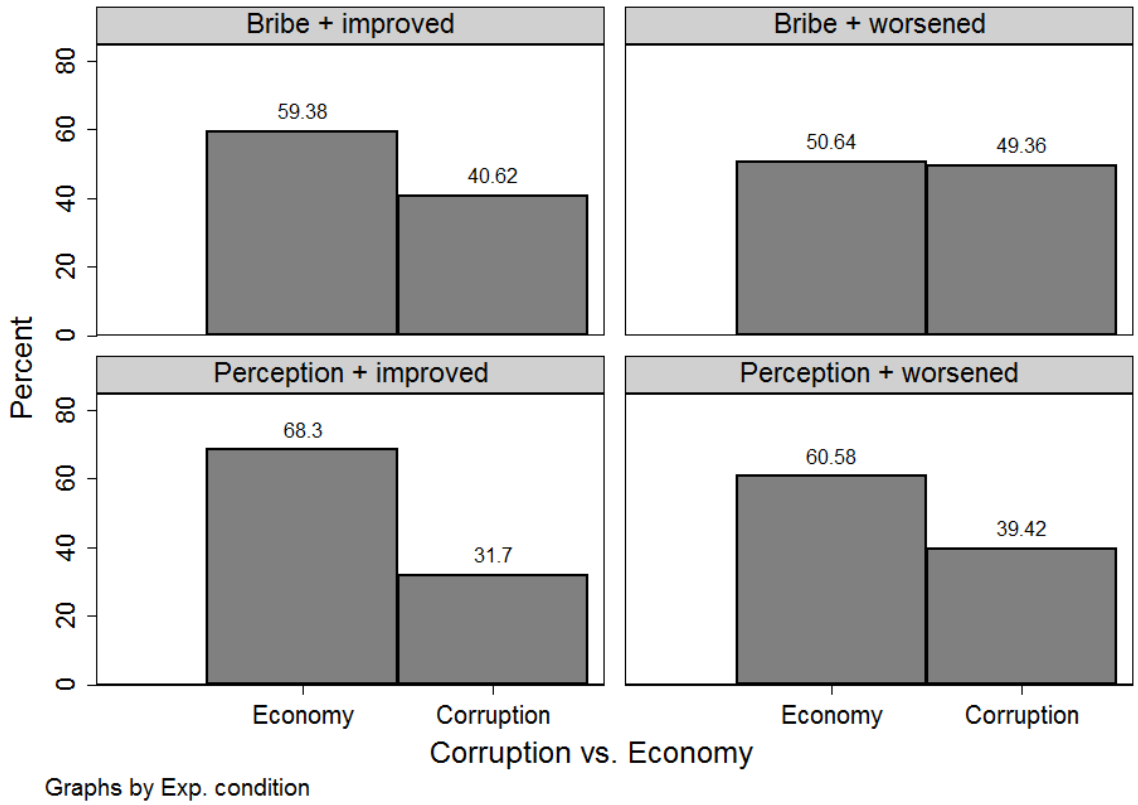


Figure 4: Estimating Selection Bias with Auxiliary Data



Note: Dots represent average responses for each question. Caps are 95% bootstrapped confidence intervals based on 500 estimations. "Expectation about Corruption" is based on the question: "Do you expect the level of corruption in the next 3 years in this country to change?" (1 "Decrease a lot," ..., 5 "Increase a lot"). "Gov't Effectiveness" is based on the question: "How would you assess your current government's actions in the fight against corruption?" (1 "The government is very effective," ..., "The government is very ineffective"). "Corruption perception is an average response to a series of questions of the form: "To what extent do you perceive the following categories in this country to be affected by corruption?" ("Political parties, Parliament/Legislature, Business/ private sector, Media, The military, NGOs, Religious bodies, Education system, Legal system/Judiciary, Medical services, Police, Registry and permit services, Utilities, Tax revenue"; the responses are 1 "Not at all corrupt," ..., 5 "Extremely corrupt"). "Gave a bribe" is based on a series of questions of the form: "In the past 12 months have you or anyone living in your household paid a bribe in any form to each of the following institution/organisation?" (institutions are the same as for corruption perception questions; the responses are 1 "yes" 2 "no").

Figure 5: Importance of Corruption vs. Economy in Experiment Responses in Bulgaria



The four charts correspond to the four treatment conditions. The upper (lower) panel represents the distribution of responses following the pocketbook (sociotropic) vignette; the left (right) panel represents the distribution of responses following the "improved" ("worsened") economy prompt. All shares are weighted by the survey weights.

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

	Eurobarometer	Transparency International
Politicians and any bribe experience	-0.00	0.05
	(-0.02, 0.01)	(0.04, 0.07)
National politicians	-0.02	.
	(-0.03, -0.00)	.
Regional politicians	0.01	.
	(-0.01, 0.02)	.
Local politicians	0.02	.
	(0.01, 0.03)	.
Police	0.05	0.07
	(0.03, 0.06)	(0.06, 0.09)
Judiciary	0.03	0.05
	(0.01, 0.04)	(0.04, 0.06)
Health	0.12	0.14
	(0.10, 0.13)	(0.12, 0.15)
Education	0.07	0.08
	(0.05, 0.08)	(0.07, 0.10)

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

Table 2: Balance Tests

Sweden	
	F-test p-value
Female	0.539
Age	0.215
Pre-university	0.250
University	0.297
PhD	0.778
Income <300	0.051
Income 300-500	0.475
Income 500-700	0.514
Income >700	0.484
High interest	0.731
Medium interest	0.812
Low interest	0.768
Low Trust	0.326
High trust	0.326

Moldova	
	F-test p-value
Female	0.940
Age	0.430
Elementary school	0.353
Secondary School	0.352
University	0.487
High interest	0.763
Medium interest	0.774
Low interest	0.641
Very low trust	0.033
Moderately low trust	0.634
Small distrust	0.072
Neutral trust	0.703
Moderate trust	0.823
Income <1000	0.275
Income 1000-3000	0.361
Income >3000	0.077
Corruption perception	0.379
Bribe experience	0.423

Table 3: Corruption Voting in Sweden and Moldova

	Sweden	Moldova	**Cross-Country Diff.**
Economy = better			
Pocketbook effect	-0.561^{***}	-0.057	-0.505^{***}
	(0.134)	(0.111)	(0.173)
Sociotropic effect	-1.000^{***}	0.124	-1.124^{***}
	(0.117)	(0.115)	(0.164)
Within-Country Diff.	0.438^{**}	-0.180	0.619^{***}
	(0.178)	(0.159)	(0.239)
Economy = worse			
Pocketbook effect	-0.520^{***}	-0.601^{***}	0.082
	(0.144)	(0.112)	(0.183)
Sociotropic effect	-1.123^{***}	-0.400^{***}	-0.723^{***}
	(0.129)	(0.114)	(0.172)
Within-Country Diff.	0.604^{***}	-0.201	0.805^{***}
	(0.194)	(0.159)	(0.251)
Interaction effect			
Pocketbook effect	0.042	-0.544^{***}	0.586^{**}
	(0.197)	(0.157)	(0.252)
Sociotropic effect	-0.123	-0.524^{***}	0.400^{*}
	(0.175)	(0.161)	(0.238)
Within-Country Diff.	0.165	-0.021	0.186
	(0.263)	(0.225)	(0.346)

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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"). Standard errors are adjusted for the smaller size of the Moldova sample. The adjustment approach is described in Section 5. 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.

Table 4: Results in Sweden using Alternative Weights

	Demographic weight	Unweighted	Political weight	Combined weight
Corruption | economy = better	-0.768^{***}	-0.775^{***}	-0.796^{***}	-0.867^{***}
	(0.090)	(0.061)	(0.187)	(0.248)
Economy | corruption = positive	-0.564^{***}	-0.652^{***}	-0.753^{***}	-0.671^{***}
	(0.090)	(0.057)	(0.224)	(0.233)
Interaction: corr. = negative, econ = worse	-0.046	0.032	0.166	0.325
	(0.133)	(0.086)	(0.280)	(0.287)
N	2311	2399	2314	2309

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

Note: Demographic weight is the probability weight for gender, education and age based on the 2011 Swedish census data. Political weight is the probability weight for political interest and subjective left-right placement based on the proportions in the representative sample of approximately 9,000 Swedish citizens carried out as a traditional postal survey by the Swedish SOM Institute. Combined weight combines the demographic and the political weights. More details on the construction of these weights are given in Dahlberg, Lundmark & Martinsson (2012). "Corr." means corruption.

Table 5: Main Results with Unweighted Swedish Sample and Skewed Moldovan Sample

		Sweden	Moldova	Cross-Country Difference
Corruption effect | economy = better	β_1	−0.774	−0.189	−0.586
		(0.061)***	(0.252)	(0.259)**
			[0.126]	[0.140]***
Corruption effect | economy = worse	$\beta_1 + \beta_3$	−0.742	−0.835	0.092
		(0.061)***	(0.263)***	(0.270)
			[0.105]***	[0.145]
Economy effect | corruption = positive	β_2	−0.652	−0.200	−0.452
		(0.057)***	(0.282)	(0.288)
			[0.080]*	[0.152]***
Economy effect | corruption = negative	$\beta_2 + \beta_3$	−0.620	−0.847	0.227
		(0.065)***	(0.231)***	(0.239)
			[0.092]***	[0.132]*
Interaction: corruption = negative, economy = worse	β_3	0.032	−0.636	0.678
		(0.086)	(0.364)*	(0.374)*
			[0.182]***	[0.202]***
N		1852	459	

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

Note: The variables are the same as in Table 2 in the text. Robust standard errors are in parentheses. In brackets, robust standard errors are adjusted for the smaller size of the Moldova sample (see Section 5).

Table 6: Experience and Perception as Determinants of Corruption Voting in Bulgaria

	Full Sample	Pocketbook Vignettes	Sociotropic Vignettes
Corruption Experience	0.109**	0.184**	−0.032
	(0.054)	(0.081)	(0.079)
Corruption Perception	−0.018	−0.066	0.048
	(0.081)	(0.097)	(0.107)
N	581	286	282

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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 in Bulgaria. 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 Bulgaria. 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.