Does providing corruption information reduce vote share? A meta-analysis*

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Abstract

Do voters in democratic countries hold politicians accountable for corruption? Field experiments that provide voters with information about the corrupt acts of politicians, then monitor vote choice have become standard in political science and economics. Similarly, vote choice survey experiments commonly provide respondents with information about the corrupt acts of hypothetical candidates. What have we learned from these experiments? A meta-analysis reveals that the aggregate treatment effect of providing information about corruption on vote share in field experiments is null. Compared to field experiments, survey experiments vastly overestimate the negative effects of corruption information on electoral outcomes. Holding other candidate features fixed by design, corrupt candidates are punished by respondents by approximately 34-37 percentage points across survey experiments, depending on estimation methods. This suggests that while vote-choice survey experiments may provide information on the directionality of informational treatments, the point estimates they provide may not be representative of real-world voting behavior.

[PRELIMINARY DRAFT: ADDITIONAL STUDIES TO BE ADDED AND POINT ESTIMATES REFINED. PLEASE DO NOT CITE OR CIRCULATE WITHOUT AUTHOR'S PERMISSION.]

1 Introduction

Corruption is believed to reduce investment, economic growth, FDI, tax revenues, and regulatory efficacy, encourage over-investment in public infrastructure, and increase costs for basic services. Reductions in political corruption are hypothesized to occur through the

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mechanism of democratic accountability (i.e. voters should vote corrupt politicians out of office) (Rose-Ackerman & Palifka 2016). According to this theory, increases in public information regarding corruption should decrease levels of corruption in government, as voters armed with information should expel corrupt politicians (Gray & Kaufman 1998; Kolstad & Wiig 2009).

Numerous experiments have examined whether providing voters with information about corruption decrease re-election rates of corrupt politicians. Literature reviews preceding these projects often indicate that there is little consensus on how voters respond to information about corrupt politicians (Arias, Larreguy, Marshall, & Querubin 2018; Botero, Cornejo, Gamboa, Pavao, & Nickerson 2015; Buntaine, Jablonski, Nielson, & Pickering 2018; Klašnja, Lupu, & Tucker 2017; Solaz, De Vries, & de Geus 2018). Others indicate that experiments have provided us with evidence that voters punish individual politicians involved in malfeasance (Chong, De La O, Karlan, & Wantchekon 2014; Weitz-Shapiro & Winters 2017; Winters & Weitz-Shapiro 2015, 2016).

By contrast, a meta-analysis provides two primary pieces of information. (1) In aggregate, the effect of providing information about incumbent corruption on incumbent vote share in field experiments is null. (2) Compared to field experiments, survey experiments vastly overestimate the negative effects of corruption information on electoral outcomes. A future draft of this paper explores the mechanisms behind these effects, testing for social desirability bias, publication bias, etc.

2 Corruption information and electoral accountability

A casual examination of experimental support for corruption information provision causing decreases in re-election rates would appear to be mixed. Field experiments have provided causal evidence of negative treatment effects on re-election rates due to providing information through public randomized financial audits (Ferraz & Finan 2008), providing voters with direct information (e.g. fliers) regarding corruption (Chong et al. 2014; De Figueiredo,

Hidalgo, & Kasahara 2011), and even sending SMS messages (Buntaine et al. 2018). However, null findings are also prevalent, and the negative effects reported above often only manifest in particular subgroups. Banerjee, Green, Green, and Pande (2010) primed voters in rural India not to vote for corrupt candidates, and Banerjee, Kumar, Pande, and Su (2011) provided information on politicians' spending discrepancies, with both studies finding null effects on vote share. Boas, Hidalgo, and Melo (2018) similarly find null effects from distributing fliers in Brazil. Finally, Arias et al. (2018) find that providing Mexican voters with information (fliers) about mayoral corruption actually *increased* incumbent party vote share by 3%.

Survey experiments paint a much more optimistic picture, systematically showing large negative effects from information treatments on hypothetical vote share. These experiments have often manipulated factors other than information provision (e.g. quality of information, source of information, whether the candidate is a co-partisan or co-ethnic, whether corruption brings economic benefits, etc.), but even so systematically show negative treatment effects (Anduiza, Gallego, & Muñoz 2013; Avenburg 2016; Boas et al. 2018; De Figueiredo et al. 2011; Eggers, Vivyan, & Wagner 2018; Franchino & Zucchini 2015; Klašnja et al. 2017; Klašnja & Tucker 2013; Vera Rojas 2017; Weitz-Shapiro & Winters 2017; Winters & Weitz-Shapiro 2013, 2015).[1] Boas et al. (2018) note this discrepancy in differential results that they obtain from a field and survey experiment, arguing that this may reflects that norms against malfeasance in Brazil may not translate into action in real life. Meta-analysis confirms that this is not merely the case for Brazil or for their experiment, but extends across a systematic review of all studies conducted to date. Lab experiments that reveal corrupt actions to fellow players appear to have a similar bias to survey experiments, and also tend to show large negative treatment effects (Azfar & Nelson 2007; Rundquist, Strom, & Peters 1977; Solaz et al. 2018).

[1]These experiments have historically taken the form of single treatment arm or multiple arm factorial vignettes, but more recently have tended toward conjoint experiments (Franchino & Zucchini 2015; Klašnja et al. 2017; Mares & Visconti 2019).

3 Interactions with other candidate characteristics

Even if voters generally find corruption distasteful, other positive candidate attributes or policies may outweigh the negative effects of corruption to voters, and thereby mitigate the effects of information provision. These mitigating factors will naturally arise in a field setting, but may only be salient to respondents if specifically manipulated by researchers in a survey setting. A number of survey experiments have therefore added additional factors in addition to corruption as mitigating variables, some of which are described below.

3.1 Policy stances

Response to favorable policy stances has been shown to potentially mitigate the impact of corruption to voters. Rundquist et al. (1977) use a survey experiment to show that a candidate's position on the Vietnam War could significantly increase the likelihood of voting for a "corrupt" candidate in the United States. Franchino and Zucchini (2015) examine corruption in relation to a candidate's education, income, tax policy, and same-sex marriage beliefs in Italy, and show that respondents prefer corrupt but socially and economically progressive candidates to clean but conservative candidates.

3.2 Economic benefit

Economic benefit has been argued to act as a similar mitigating factor. Winters and Weitz-Shapiro (2013) use a survey experiment in Brazil to show that voters punish corrupt politicians at the polls, including those with strong records of past performance as measured by public goods provision. Klašnja et al. (2017) randomize party, economic performance, and whether or not the politician's corrupt act itself brought benefits to their constituents in Argentina, Chile, and Uruguay, finding evidence that voters are more forgiving of corruption when it benefits them personally.

3.3 Partisanship and in-group attachments

Evidence of co-partisanship as a limiting factor to corruption deterrence is mixed. Anduiza et al. (2013) show that co-partisanship decreases the importance of corruption to Spanish voters using a survey experiment. Solaz et al. (2018) induce in-group attachment in a lab-experiment of UK subjects, finding that in-group membership reduces sanction of "corrupt" participants. However, Klašnja et al. (2017) find relatively small effects of co-partisanship compared to corruption allegations (3.5x) in Argentina, Chile, and Uruguay,[2] and Rundquist et al. (1977) find null effects in the US in the 1970s. Konstantinidis and Xezonakis (2013) also find that partisanship does not moderate electoral punishment of corruption in a survey experiment in Greece. This evidence unsurprisingly suggests that strong partisan effects occur where partisan attachments are strongest. Likewise, if co-ethnicity mitigates punishment of corrupt behavior, we are likely to see these effects in highly fractionalized societies.

4 Research Design and Methods

4.1 Search methods and criteria for inclusion

I followed standard practices to locate the experiments included in the meta-analysis. This included following citation chains and conducting internet searches using the terms ("corruption factorial", "corruption candidate choice", "corruption conjoint", "corruption, vote, experiment", "corruption vignette"). I located 10 field experiments from 8 papers, and 17 survey experiments from 14 papers.

Field experiments are included if researchers randomly assigned information regarding incumbent corruption (or possible corruption in the case of Banerjee et al. (2011)[3]) to voters, then measured corresponding voting outcomes. I include one natural experiment, Ferraz and

[2]The authors note that partisan attachments are particularly weak in these three countries

[3]Banerjee et al. (2011) provided information on politicians' spending discrepancies, which may imply corruption but is not as direct as other types of information provision. The overall null results are not sensitive to the inclusion of this estimate (see Figure A.1).

Finan (2008), as random assignment was conducted by the Brazilian government.[4] Effects reported in the meta-analysis come from information treatments on the entire sample of study only, not subgroup or interactive effects that reveal the largest treatment effects.

For survey experiments, studies must test a non-corrupt control group versus a corrupt treatment group. This necessarily excludes studies that compare one type of information provision (e.g. source) to another and the control group is one type of information rather than no information, or where the politician is always corrupt (Anduiza et al. 2013; Botero et al. 2015; Konstantinidis & Xezonakis 2013; Muñoz, Anduiza, & Gallego 2012; Rundquist et al. 1977). In many cases, studies have multiple corruption treatments (e.g. high quality information vs. low quality information, co-partisan vs. opposition party, etc.). In these cases, I replicate the studies and code corruption as a binary treatment (0 = clean, 1 = corrupt) where *all* treatment arms that provide corruption information are combined into a single treatment. Studies that use non-binary vote choices are rescaled into a binary vote choice.[5] In some cases, point estimates, standard errors and/or confidence intervals are not explicitly reported (6 cases), and in these cases standard errors are estimated by digitally measuring coefficient plots.[6]

A full list of all studies - disaggregated by field and survey experiments - that meet the criteria outlined above are provided in Table 1 and Table 2 below. A list of lab experiments (2 total) can also be found in and Table A.1, although these are not included in the present analysis.

[4]Consistent with complete knowledge of the assignment mechanism and randomization, Ferraz and Finan (2008) regress pre-election audit status (i.e. treatment assignment) on electoral vote share to obtain their ATE estimate. The authors note that "because of the randomized auditing, the coefficient [on audit] provides an unbiased estimate of the average effect of the program on the electoral outcome of the incumbent politician."

[5]For example, a 1-4 scale is recoded so that 1 or 2 is equal to no vote, and 3 or 4 is equal to a vote.

[6]I recognize that this introduces non-statistical measurement error into the meta-analysis. However, it is not possible for these errors to be large enough to effect the substantive conclusions of the analysis.

Table 1: Field experiments

Study	Country	Treatment	Vote share
Arias et al. (2018)	Mexico	Fliers	Positive
Banerjee et al. (2010)[1]	India	Newspaper	Null
Banerjee et al. (2011)[2]	India	Canvas/Newspaper	Null
Boas et al. (2018)	Brazil	Fliers	Null
Buntaine et al. (2018)	Ghana	SMS	Negative
Chong et al. (2014)	Mexico	Fliers	Negative
De Figueiredo et al. (2011)	Brazil	Fliers	Null/Negative
Ferraz and Finan (2008)	Brazil	Audits	Negative

[1] Banerjee et al. (2010) treated voters with a campaign not to vote for corrupt candidates, but did not provide voters with information on which candidates were corrupt. The overall null results are not sensitive to the inclusion of this estimate. See Figure A.1.
[2] Banerjee et al. (2011) provided information on politicians' spending discrepancies, which may imply corruption but is not as direct as other types of information provision. The overall null results are not sensitive to the inclusion of this estimate. See Figure A.1.

Table 2: Survey experiments

Study	Country	Treatment	Vote share
Avenburg (2016)	Brazil	Information	Negative
Banerjee, Green, McManus, and Pande (2014)	India	Information	Negative
Boas et al. (2018)	Brazil	Information	Negative
De Figueiredo et al. (2011)	Brazil	Fliers	Negative
Eggers et al. (2018)	UK	Information	Negative
Franchino and Zucchini (2015)	Italy	Information	Negative
Klašnja and Tucker (2013)	Sweden	Information	Negative
Klašnja and Tucker (2013)	Moldova	Information	Null
Klašnja et al. (2017)	Argentina	Information	Negative
Klašnja et al. (2017)	Chile	Information	Negative
Klašnja et al. (2017)	Uruguay	Information	Negative
Mares and Visconti (2019)	Romania	Information	Negative
Vera Rojas (2017)	Peru	Information	Negative
Winters and Weitz-Shapiro (2013)	Brazil	Information	Negative
Winters and Weitz-Shapiro (2015)	Brazil	Information	Negative
Winters and Weitz-Shapiro (2016)[1]	Brazil	Information	Negative
Weitz-Shapiro and Winters (2017)[1]	Brazil	Information	Negative
Winters and Weitz-Shapiro (2018)	Argentina	Information	Negative

[1] Winters and Weitz-Shapiro (2016) and Weitz-Shapiro and Winters (2017) report results from the same survey experiment. The results are therefore only reported once.

4.2 Results

Based on the meta-analyses shown in Figure 1 and Figure 2, survey experiments appear to vastly overestimate the ATE of providing information about corruption to voters relative to field experiments. In fact, based on the results shown in Figure 1, we cannot reject the null hypothesis of no treatment effect in field experiments. Based on a univariate Shapiro-Wilk test of normality, we also cannot reject the null hypothesis that the point estimates are distributed normally around a mean of approximately zero percentage points.

By contrast, holding other candidate features fixed by design, corrupt candidates are punished by respondents by approximately 37 percentage points in survey experiments based on fixed effects meta-analysis and 34 percentage points using random effects meta-analysis. Of the 17 survey experiments, only two show a null effect (De Figueiredo et al. 2011; Klašnja & Tucker 2013), while all others are negative and significantly different from zero at conventional levels. Overall, these studies indicate a large electoral penalty for engaging in corrupt acts when voters are made aware of the malfeasance, but these results are likely not reflective or real-world voter behavior.

Examining all studies together, a test for heterogeneity by type of experiment (field or survey) reveals that up to 60% of the total heterogeneity across studies can be accounted for by including a dummy variable for type of experiment (0 = survey, 1 = field) in the model. This dummy variable has a significant influence on the effectiveness of the information treatment at the 1% level. In fact, the point estimate of this dummy variable is equal to 0.32, while the overall estimate across studies is -.33, implying that the predicted treatment effect across experiments is not significantly different from zero when an indicator for type of experiment is included in the model.

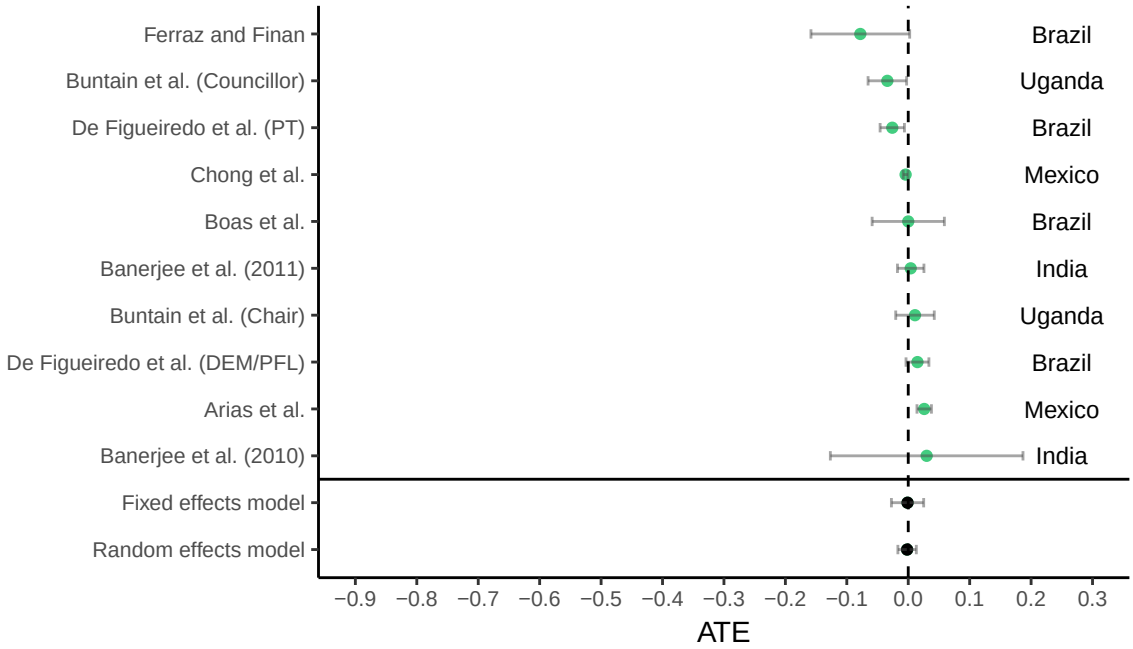

Figure 1: Field experiments: Average treatment effect of corruption information on incumbent vote share

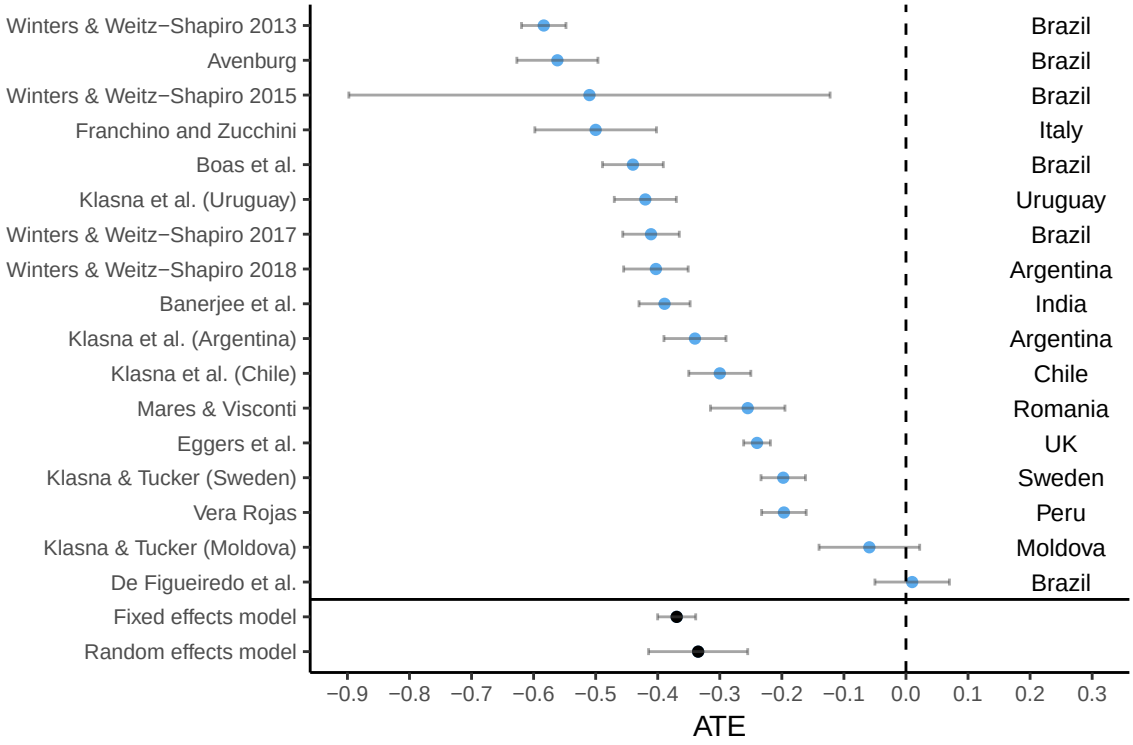

Figure 2: Survey experiments: Average treatment effect of corruption information on incumbent vote share

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A Appendix

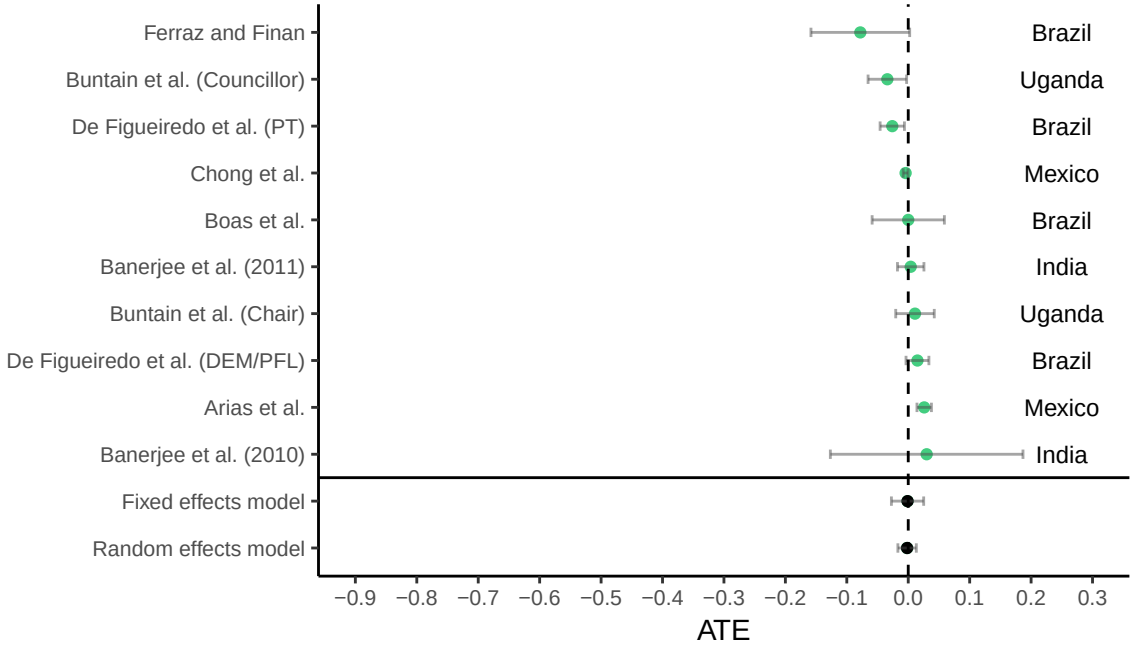

Figure A.1: Field experiments: Average treatment effect of corruption information on incumbent vote share (excluding Banerjee et al. (2010) and Banerjee et al. (2011))

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Table A.1: Lab experiments

Study	Country	Treatment	ATE
Azfar and Nelson (2007)	USA	Information	Negative
Solaz et al. (2018)	UK	Information	Negative