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 34-36 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 PLEASE DO NOT CITE OR CIRCULATE WITHOUT AUTHOR'S PERMISSION.]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

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services. Corruption can be perpetrated by both bureaucratic agents and politicians. Reductions in political corruption are hypothesized to occur through the 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; De Vries & Solaz 2017; Klašnja, Lupu, & Tucker 2017; Solaz, De Vries, & de Geus 2018). Others indicate that experiments have provided us with evidence that voters strongly 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 suggests that: (1) In aggregate, the effect of providing information about incumbent corruption on incumbent vote share in field experiments is null, and (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

Experimental support that for the hypothesis that providing voters with information about politicians' corrupt acts decreases their re-election rates is ostensibly 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), giving voters fliers revealing corrupt actions of politicians, (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 sometimes 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); Arias, Larreguy, Marshall, and Querubin (2019) 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, consistently showing large negative effects from information treatments on hypothetical vote share. These experiments often manipulate moderating 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; Breitenstein 2019; 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 only the case for their experiment in Brazil, 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 (Arvate & Mittlaender 2017; Azfar & Nelson 2007; 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 (Breitenstein 2019; 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.[2] 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, Strom, and Peters (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.

[2] See De Vries and Solaz (2017) for an excellent overview.

3.3 Partisanship and in-group attachments

Evidence of co-partisanship as a limiting factor to corruption deterrence is mixed. Anduiza et al. (2013) and Breitenstein (2019) both show that co-partisanship decreases the importance of corruption to Spanish voters using survey experiments. 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,[3] 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 field experiment," "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)[4]) to voters, then measured corresponding voting outcomes. This therefore excludes experiments that

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

[4]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).

randomly assign corruption information, but use favorability ratings or other metrics rather than actual vote share as their dependent variable (Green, Zelizer, Kirby, et al. 2018). I include one natural experiment, Ferraz and Finan (2008), as random assignment was conducted by the Brazilian government.[5] 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 no-information control group versus a corruption information treatment group and measure vote choice for a hypothetical candidate. 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 known to be corrupt (Anduiza et al. 2013; Botero et al. 2015; Konstantinidis & Xezonakis 2013; Muñoz, Anduiza, & Gallego 2012; Rundquist et al. 1977). The "survey experiment" in De Figueiredo et al. (2011) is also excluded as it does not use hypothetical candidates, but instead asks voters if they would have changed their actual voting behavior in response to receiving corruption information.[6] 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.[7] In some cases, point estimates, standard errors and/or confidence intervals are not explicitly reported (4 cases), and in these cases standard errors are estimated by digitally measuring coefficient plots.[8]

[5] 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."

[6] This study has a null finding. The overall results are not sensitive to the inclusion of this estimate. See Figure A.2 for meta-analysis including this study.

[7] 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.

[8] 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.

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 (3 total) can also be found in and Table A.1, although these are not included in 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	Null/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
Breitenstein (2019)	Spain	Information	Negative
Boas et al. (2018)	Brazil	Information	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 36 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 one shows a null effect (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 70% 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 0.33, while the overall estimate across studies is -.34, 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.

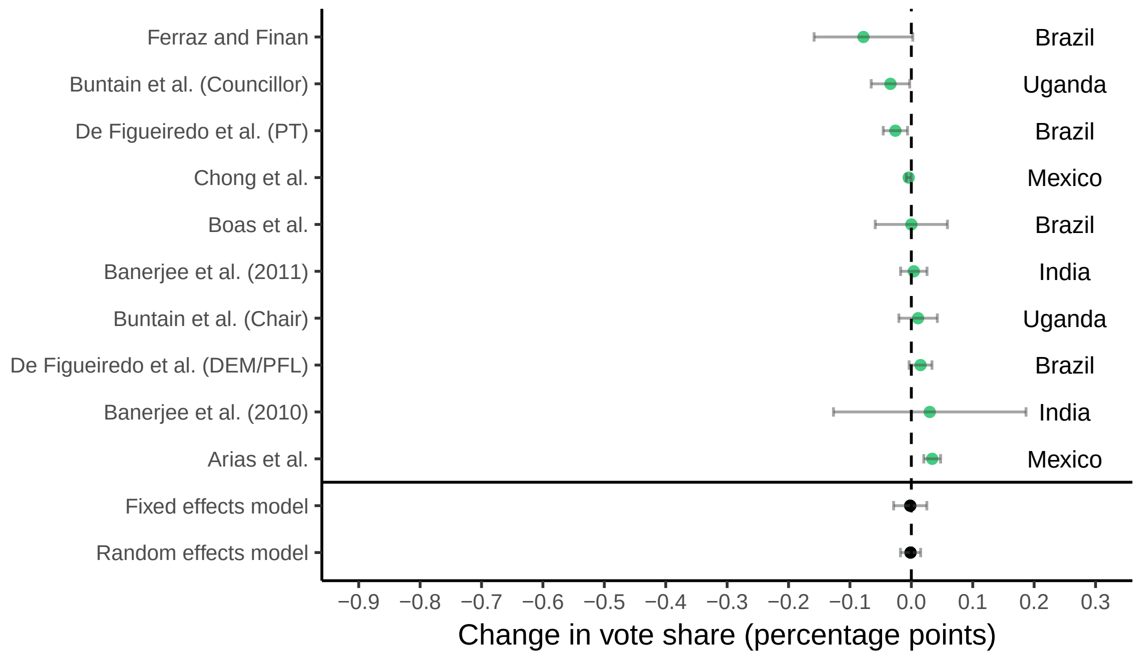

Figure 1: Field experiments: Average treatment effect of corruption information on incumbent vote share

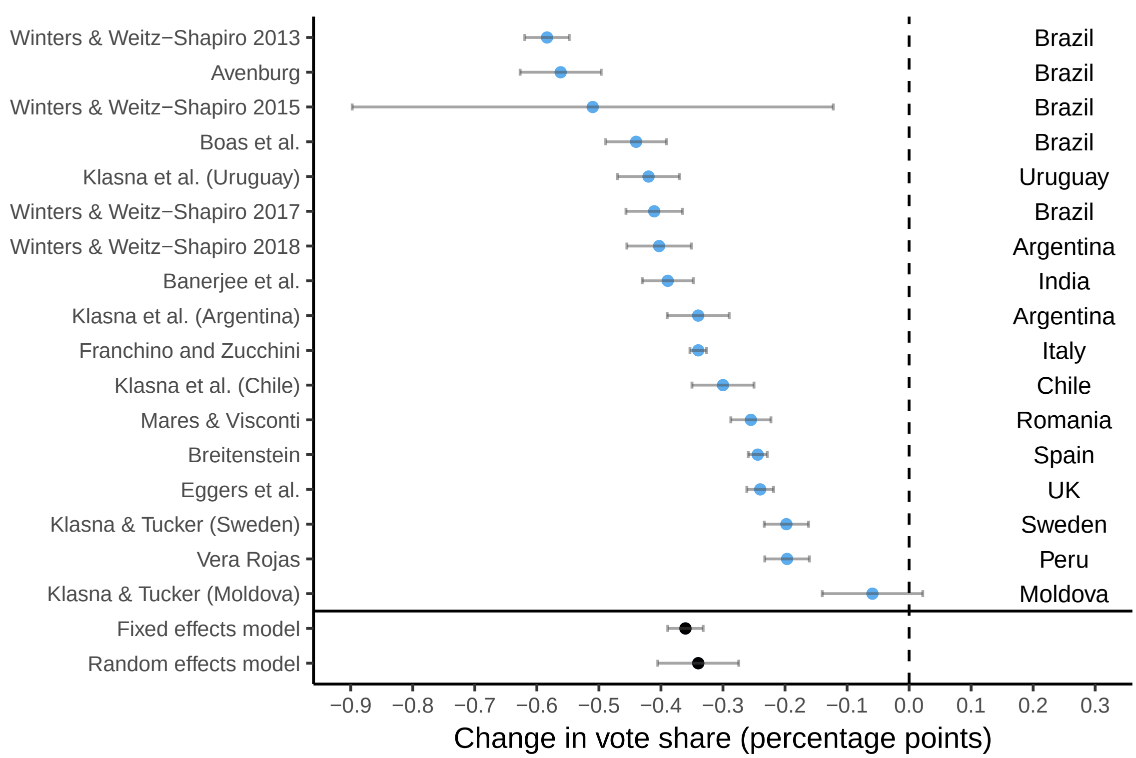

Figure 2: Survey experiments: Average treatment effect of corruption information on incumbent vote share

5 Discussion

What accounts for the large difference in treatment effects between field and survey experiments? Three potential possibilities are publication bias, social desirability bias, and the nature of the survey designs. Null results may be less likely to be published than significant results, particularly in a survey setting. Respondents in survey experiments may also behave in a normatively desirable manner according to the perceived norms of society and/or the researcher. It is also possible that more complex factorial designs - such as conjoint experiments - may more successfully approximate the results obtained in the field.

5.1 *Publication bias and p-hacking*

A quick look at the papers included in the meta-analysis shows that of the ten field experiments found, only six are published. By contrast, only two of the 17 survey experiment papers remain unpublished. This may reflect that the null results that arise from field experiments are less likely to be published than their survey counterparts with large and highly significant negative treatment effects.

In order to more formally test for publication bias, I first attempt to employ the p-curve developed in Simonsohn, Nelson, and Simmons (2014a, 2014b) and Simonsohn, Simmons, and Nelson (2015). The p-curve is based on the premise that only "significant" results are typically published, and depicts the distribution of statistically significant p-values for a set of published studies. The shape of the p-curve is indicative of whether or not the results of a set of studies are derived from true effects, or from p-hacking. If effect sizes are clustered around 0.05 (i.e. the p-curve is "left skewed"), this may be evidence of p-hacking, indicating that studies with p-values just below 0.05 are "selectively reported." If the p-curve is "right skewed" and there are more low p-values (0.01), this is evidence of true effects.

All significant survey experimental results included in the meta-analysis are significant at the 1% level (making construction of a "curve" with bins of width 0.01 impossible), implying that publication bias likely does not explain the large negative treatment effects in survey experiments. Instead, it appears that the difference in experimental design itself accounts for the difference in the magnitude of treatment effects in field versus survey experiments. For field experiments, there is not a large enough number of published experiments to make the p-curve viable. Only six studies are published, and of these only four are significant at at least the 5% level.

Next, I test for publication bias by examining funnel plot asymmetry. A funnel plot depicts the outcomes from each study on the x-axis and their corresponding standard errors on the y-axis. The chart is overlaid with a triangular confidence interval region (i.e. the "funnel"), which should contain 95% of the studies if there is no bias or between study heterogeneity. If studies with insignificant or null results remain unpublished or there is a large degree of asymmetry, the funnel plot may be asymmetric. Both visual inspection and regression tests of funnel plot asymmetry reveal an asymmetric funnel plot when survey and field experiments are grouped together (see Figure A.3 and Table A.2). However, this asymmetry disappears when accounting for heterogeneity by type of experiment, either with the inclusion of a field experiment moderator (dummy) variable or by analyzing field and survey experiments separately (see Figure A.4, Figure A.5, Figure A.6, and Table A.2). Once again, this implies that differences in the experimental design likely account for the difference in the magnitude of treatment effects in field versus survey experiments, not publication bias.

5.2 Social desirability bias

A second possible explanation is social desirability bias, in which survey respondents under-report behavior that they believe to be social undesirable. The respondent may perceive a particular response to be normatively desirable by society as whole, by the researcher(s) conducting the experiment, or both. In the case of corruption, respondents are likely to perceive corruption as both normatively "wrong," as well as harmful to society, the economy, and their own personal well-being.[9] In a hypothetical vignette, they may therefore choose

[9]Non-experimental surveys indicate the respondents in highly corrupt countries tend to view corruption as a serious problem that often tops their list of political considerations.

the socially desirable option (no corruption), particularly when the respondent is aware that he or she is being observed by a researcher.

A related explanation may be the selection of the socially desirable option when there are little downsides to doing so. A hypothetical vignette has virtually no costs to selecting the socially desirable option, even when moderating variables are included. In a field experiment, however, the cost of changing one's vote may be higher. Voters may have pre-existing opinions of real candidates that make them discount corruption information, or may have strong material and/or ideological incentives to stick with their candidate.

How might we overcome social desirability bias in survey experiments? One option is to eschew hypothetical candidates in favor of real candidates during the timing of actual elections. Of course, for ethical reasons this likely limits researchers to having actual information regarding the corrupt actions of candidates. A second option is the use of list experiments or experiments which ask about the expected behavior of other individuals in response to new information. List experiments are surprisingly uncommon in corruption experiments (none of the survey experiments included here use this method), but a vote buying[10] experiment in Nicaragua estimated that only 2% of respondents admitted directly to being offered compensation in exchange for their vote, but 24% of respondents admitted to the practice in a list experiment (Gonzalez-Ocantos, De Jonge, Meléndez, Osorio, & Nickerson 2012). A third option, which I turn to next, is the use of more complex factorial designs such as conjoint experiments.

5.3 Survey complexity and conjoint experiments

As noted in Section 3 above, the fact that moderating variables may dampen the salience of corruption to voters has not been lost on previous researchers, who have attempted to capture these factors via the inclusion of multiple treatment arms that vary policy stances, quality of information, economic benefit, and partisanship. However, the meta-analysis shown above indicates that even the inclusion of these moderators does not move point estimates close

[10] Typically considered a form of corruption.

to the (null) field setting, in which all of these moderating factors may be salient to the voter. Conjoint experiments allow researchers to randomize a much larger host of candidate characteristics and may help illuminate the mechanisms that lead to these null results. This may also minimize social desirability bias, as it reduces the probability that the respondent is aware of the researcher's primary experimental manipulation of interest (e.g. corruption).

Researchers have thus far tended to present the results of conjoint experiments as individual average marginal component effects (AMCEs), then compare the magnitude of these effect sizes. A more appropriate method may be to calculate the average marginal effects of a vector of moderating variables. To illustrate this point, I calculate average marginal effects as a function of two policy positions - tax policy and same sex marriage - and corruption for conservative and liberal respondents using the conjoint experiment conducted in Italy by Franchino and Zucchini (2015)[11]. The results are presented in Figure 3 and Figure 4, and show that even for corrupt candidates in the conjoint, the right policy platform can garner over 50% of the predicted hypothetical vote.

Policy profiles that result in over 50% of voters selecting a "corrupt" candidate may not be outliers in real-world scenarios. Unlike in conjoint experiments, real-world candidates' policy profiles are not selected randomly, but rather represent choices designed to appeal to voters. It may therefore be preferable to analyze conjoint experiments as above, comparing outlier characteristics (e.g. corruption) to realistic candidate profiles rather than fully randomized candidate profiles. For example, in the US context, perhaps the relevant metric of interest would be to look at the impact of corruption on vote choice for a Democratic respondent examining a Democratic candidate who espouses their preferred policy positions and attributes, rather than looking at the magnitude of the corruption AMCE versus each individual policy AMCE.

[11] To my knowledge, this remains the only published conjoint experiment with a corruption treatment and publicly available replication data.

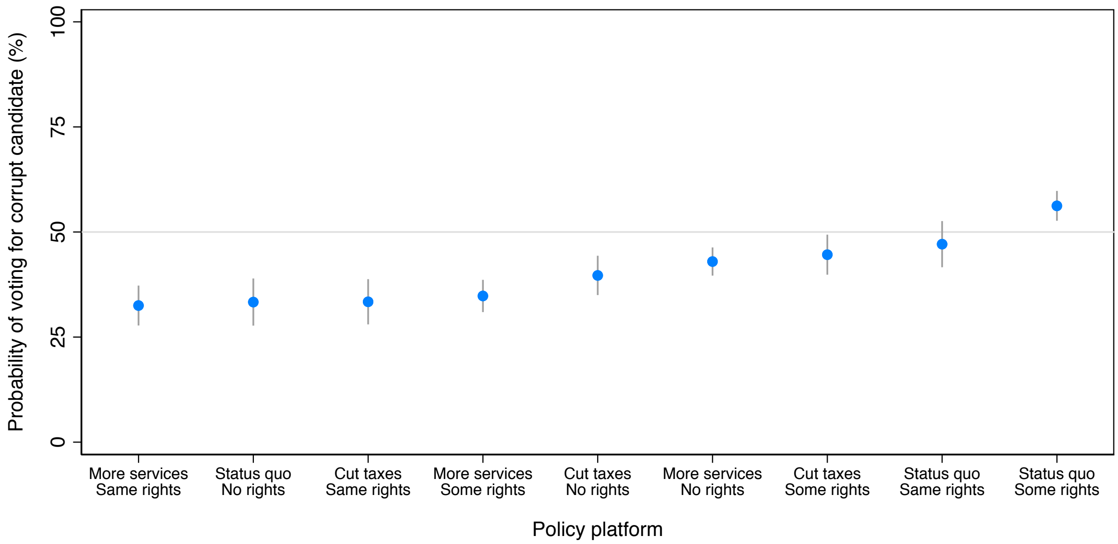

Figure 3: Franchino and Zucchini (2015) conjoint: can policy positions overcome corruption (conservative respondents)?

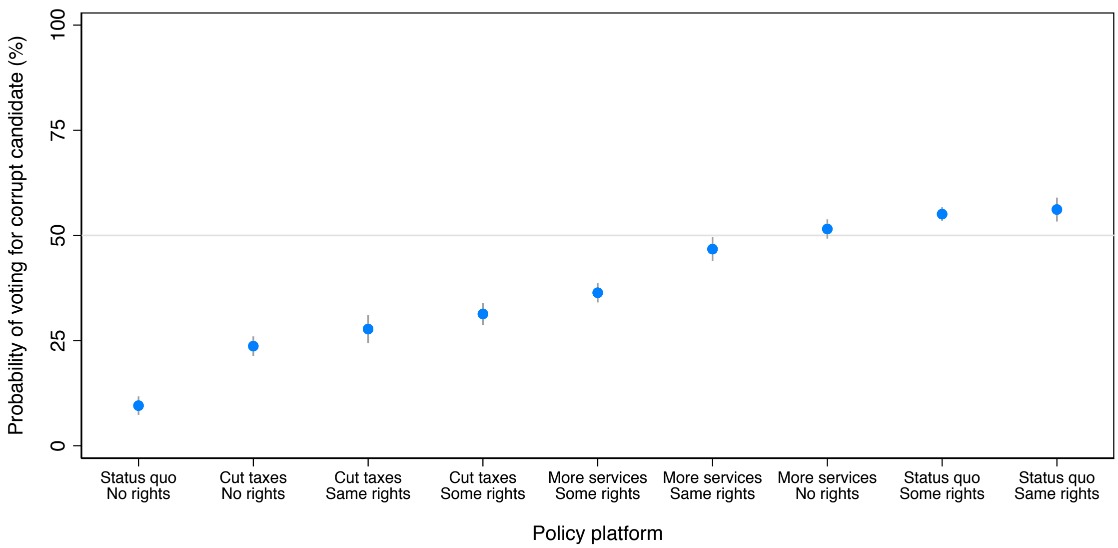

Figure 4: Franchino and Zucchini (2015) conjoint: can policy positions overcome corruption (liberal respondents)?

6 Conclusion

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A Appendix

A.1 Lab experiments

Table A.1: Lab experiments

Study	Country	Treatment	ATE
Arvate and Mittlaender (2017)	Brazil	Information	Negative
Azfar and Nelson (2007)	USA	Information	Negative
Solaz et al. (2018)	UK	Information	Negative

A.2 Robustness checks

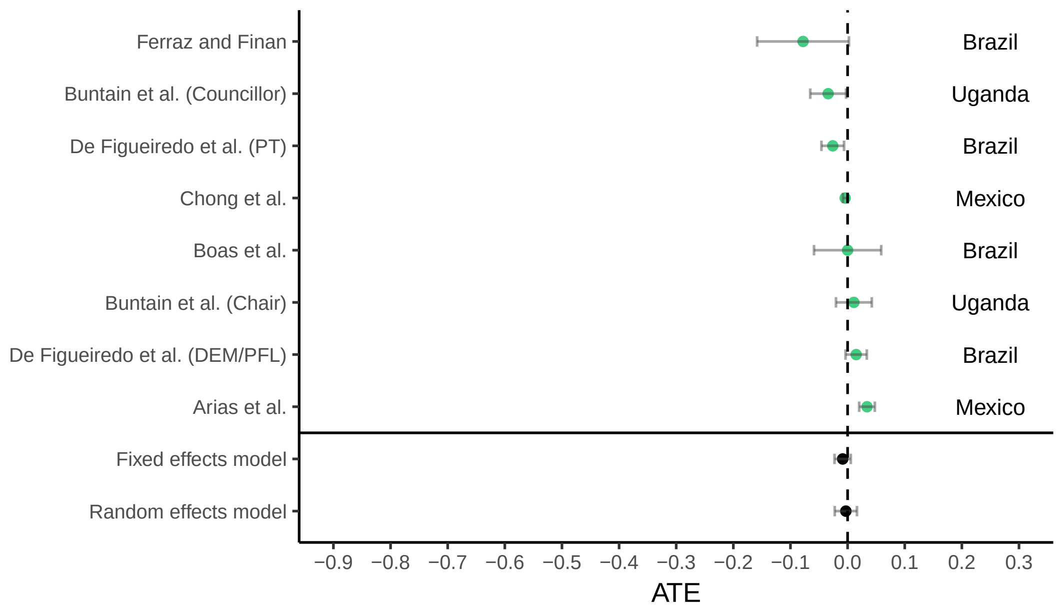

Figure A.1: Field experiments: Average treatment effect of corruption information on incumbent vote share (excluding Banerjee et al. (2010) and Banerjee et al. (2011))

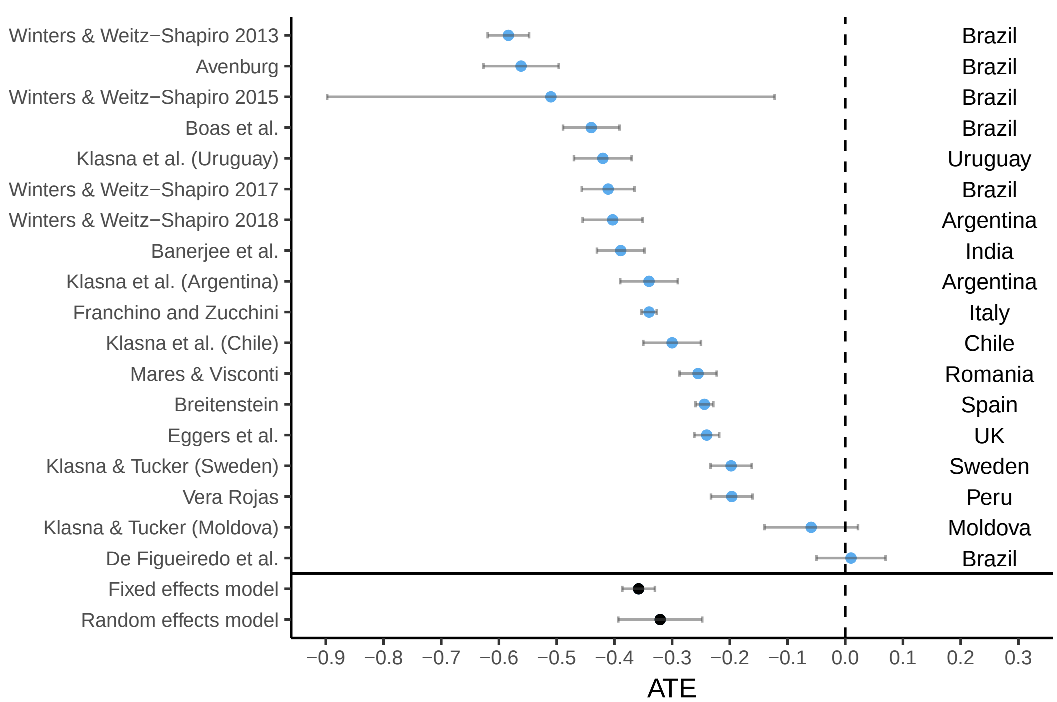

Figure A.2: Survey experiments: Average treatment effect of corruption information on incumbent vote share (including De Figueiredo et al. (2011))

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A.3 Publication bias

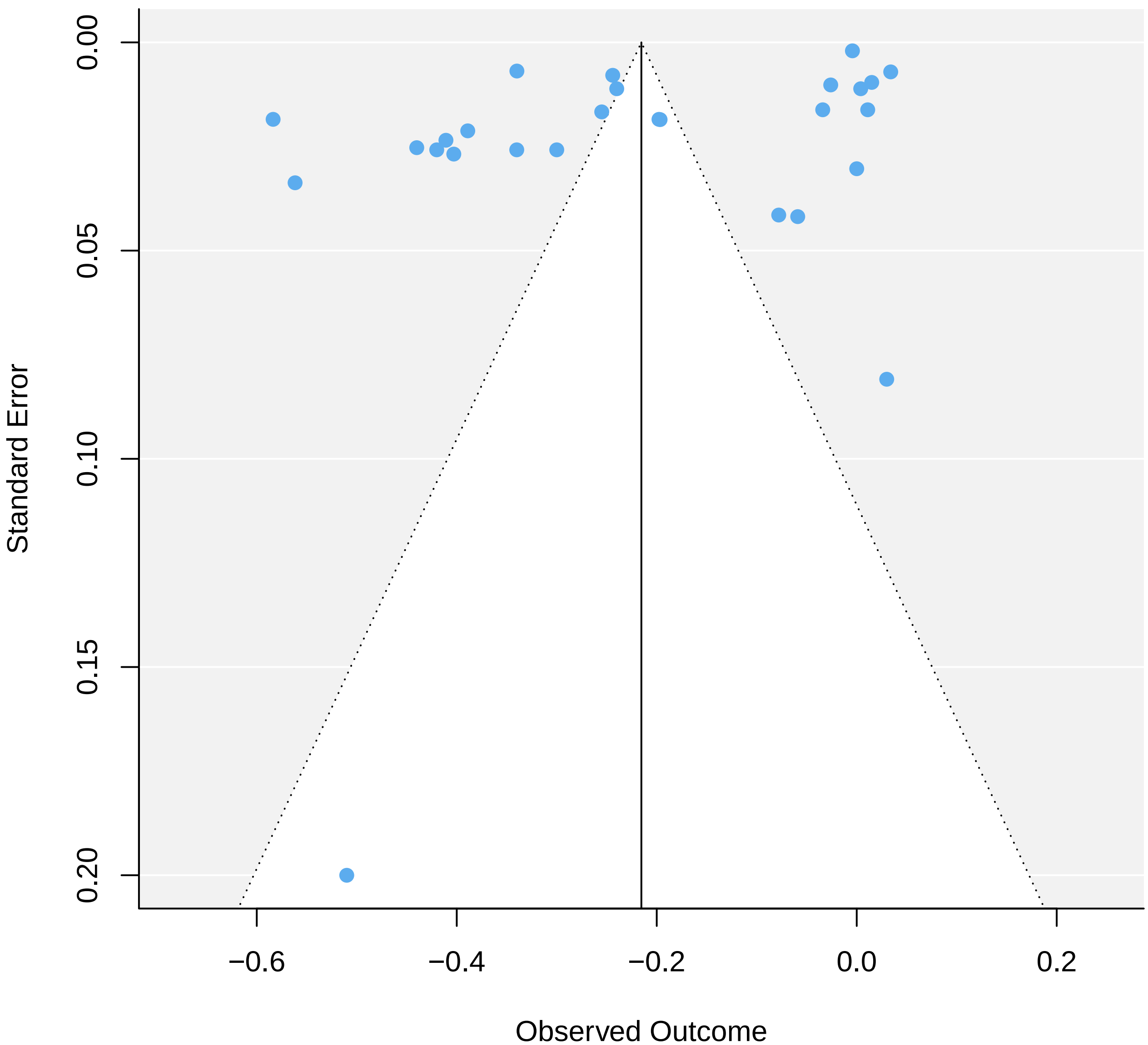

Figure A.3: Funnel plot: all experiments

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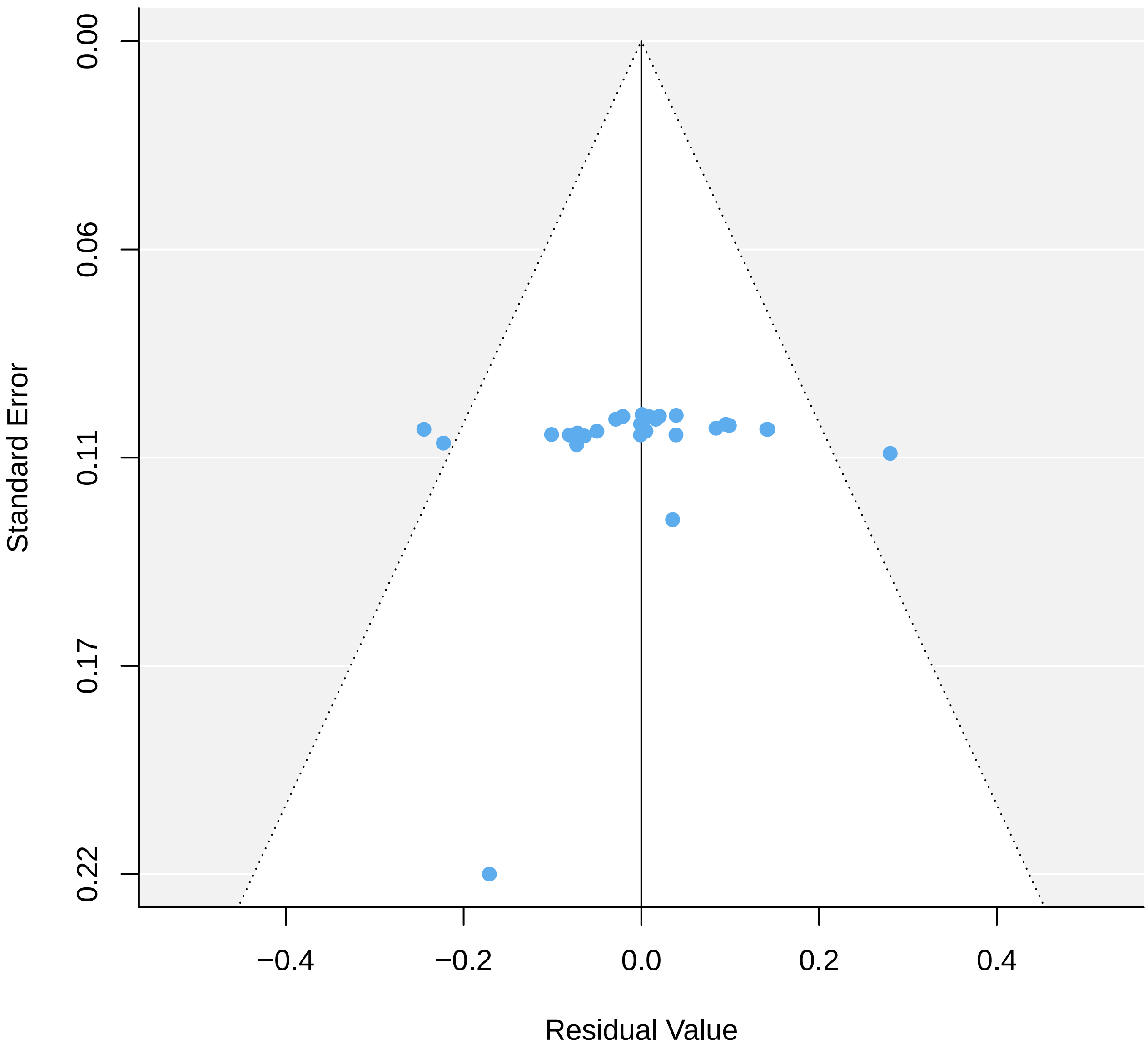

Figure A.4: Funnel plot: all experiments with field experiment moderator

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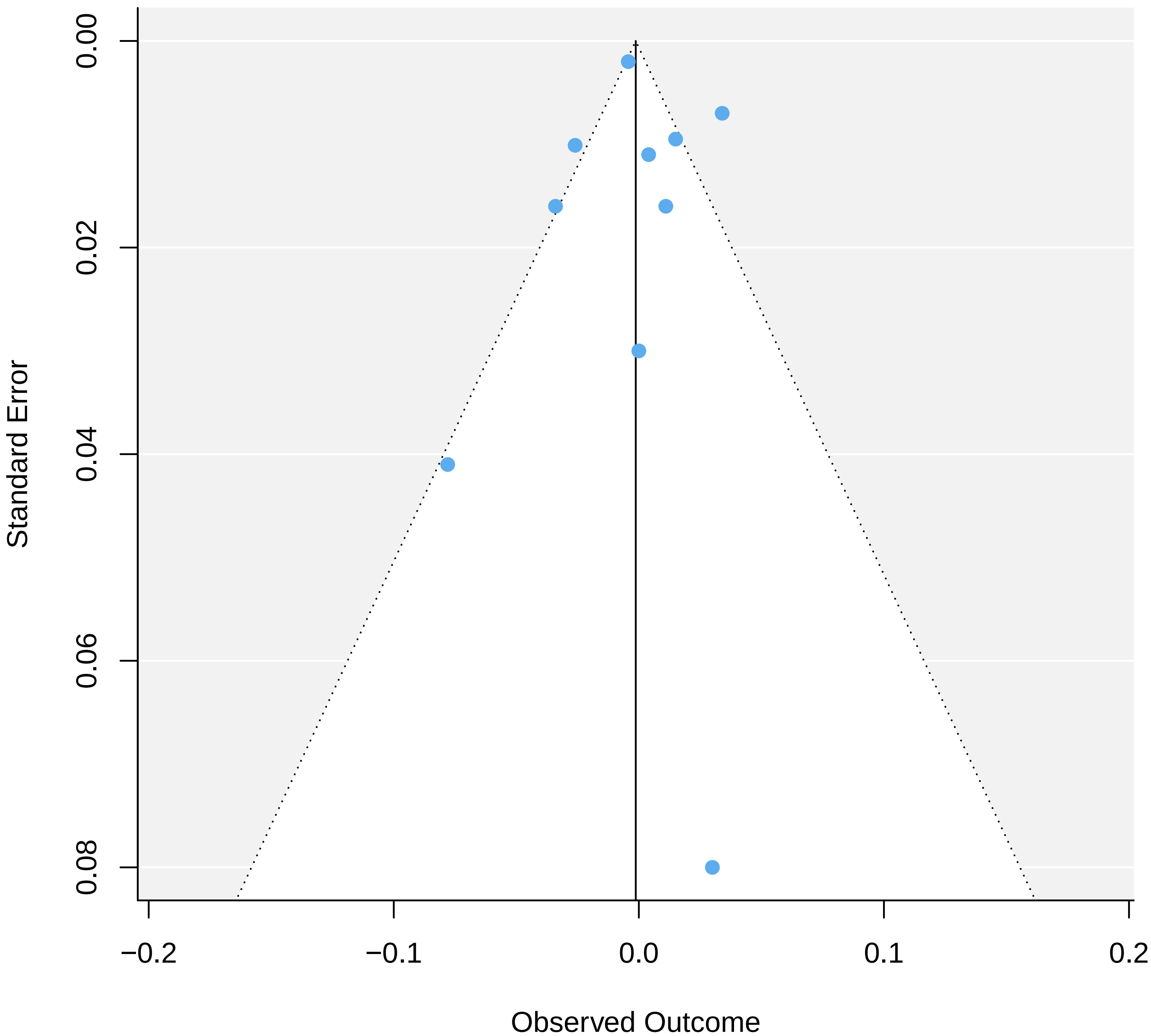

Figure A.5: Funnel plot: field experiments

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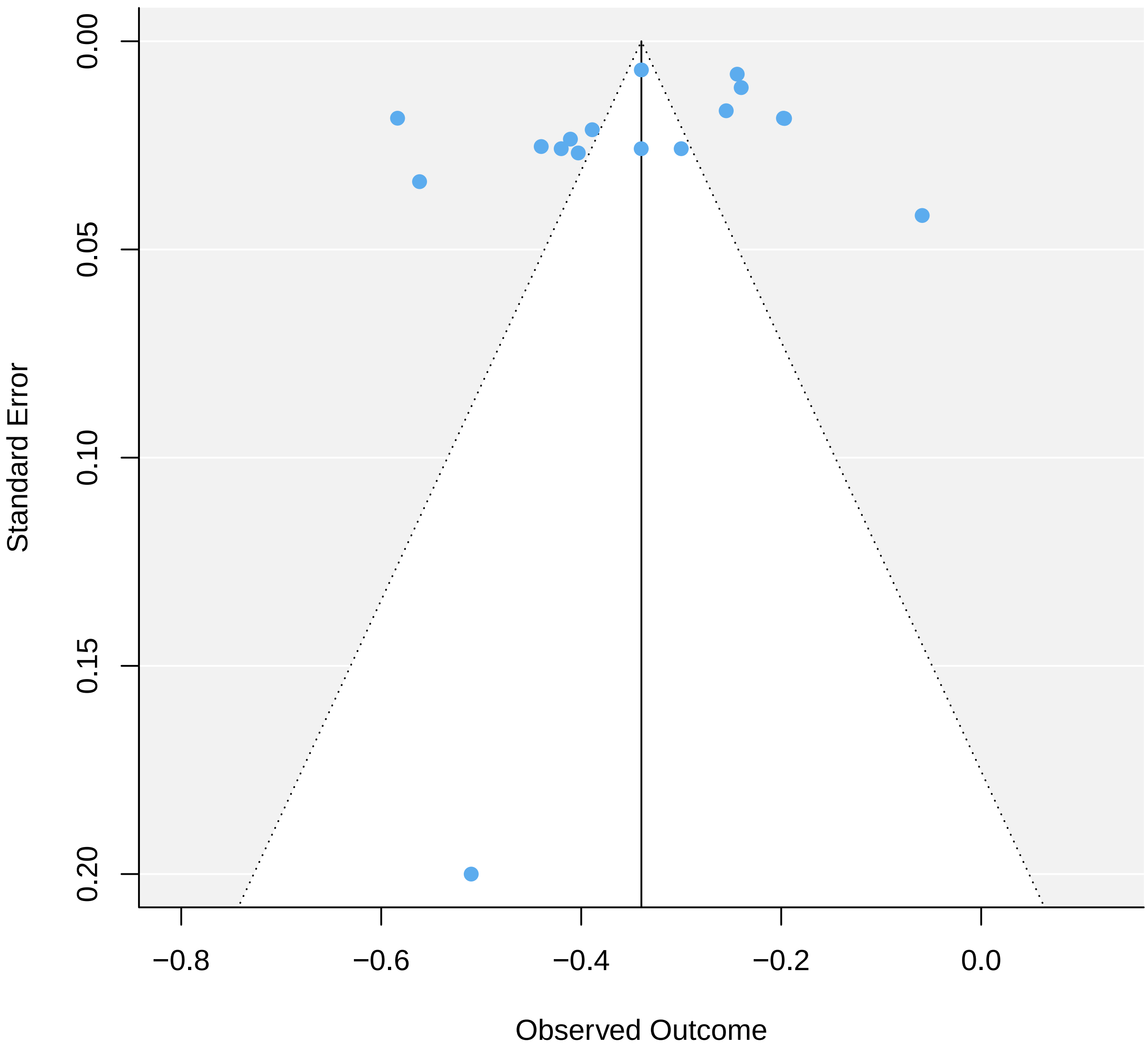

Figure A.6: Funnel plot: survey experiments

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Table A.2: Regression tests for funnel plot asymmetry

Studies included	p-value
All	0.0016
All with moderator	0.4512
Field	0.8403
Survey	0.3159