Noncompliance with Information Treatments*

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Abstract

Social scientists use survey experiments to study the effect of information on individual attitudes and behaviors. However, such experiments may fail to provide respondents with the information as intended. If the theorized mechanism is correct, noncompliance attenuates results, but noncompliance can also arise if the experiment exposes respondents to unintended information, affecting the substantive interpretation of results. In this letter, we propose a diagnostic test and recommendations for treatment design that will help researchers evaluate theoretical mechanisms in survey experiments. This placebo test repurposes treatment-relevant manipulation checks to evaluate responses under control conditions. This approach offers a path toward more robust and more informative survey experiments.

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1 Introduction

Information plays a central role in connecting material conditions to political attitudes and behavior. Studies of the causal effects of information often use survey experiments that randomly assign respondents to facts embedded in a questionnaire. This information provision is then assumed to expose individuals to informational content, allowing scholars to study how information exposure affects downstream attitudes or behavior. However, these experiments produce credible estimates of the effects of exposure to information only if participants consume the information as the researchers intend; that is, they comply with the experimental protocol.

In this letter, we show that there is reason to believe that (1) noncompliance is common in survey experiments with information treatments and (2) noncompliance can be a problem for theoretical interpretation of treatment effects. If the theoretical mechanism hypothesized in a study is correct, noncompliance will attenuate effects. However, respondents may not only fail to be exposed, they might be affected by unanticipated or unintended aspects of the treatment or control conditions.

To better assess the theoretical relevance of survey experimental results, we offer a diagnostic placebo test. This test relies on the idea that participants who are presented but nonetheless not exposed to the information should not be affected by it (Eggers, Tuñón, and Dafoe 2023). The presence of a treatment effect among noncompliers does not indicate what mechanism is at play, but it casts doubt on the intended theoretical interpretation of the study.

Our proposal builds on a growing literature that examines noncompliance in survey experiments (Berinsky, Margolis, and Sances 2014; Dafoe, Zhang, and Caughey 2018; Kane and Barabas 2019; Harden, Sokhey, and Runge 2019). To implement our placebo test, we follow Harden, Sokhey, and Runge (2019) in repurposing a type of manipulation check that is still uncommon but has been getting more attention in political science experiments (Kane and Barabas 2019). These treatment-relevant placebo checks, and our overall approach, requires committing to a particular set of informa-

tion to be manipulated by the survey experiment and a particular mechanism by which information affects behavior.

This note also relates to Dafoe, Zhang, and Caughey's paper on "information equivalence." Information equivalence is concerned with both the multiplicity of causal mechanisms and downstream consequences of information. When a survey experiment generates simultaneous effects it can produce ambiguity about the particular mechanism at play. Their methodological suggestions work by specifying the particular form of nuisance information provided by an experiment. Among other things, they recommend researchers conduct placebo tests to assess information equivalence using "placebo beliefs." Researchers identify a placebo belief ex ante as a belief that: (1) can plausibly be affected by the information provision, (2) can affect the outcome, and (3) is unrelated to any attribute affected by the treatment of interest. While such a test can be useful in many cases, it has two shortcomings: it requires the researcher to specify the nuisance beliefs ex ante, and this can be challenging as the placebo belief needs to satisfy all three criteria above to be informative. By contrast, our diagnostic assesses the threat to substantive interpretation when researchers cannot fully anticipate what other information may be manipulated.

We illustrate our argument with a re-analysis of Brutger et al.'s (2022) replication of Mutz and Kim (2017). Our diagnostic test indicates the existence of a channel other than the one theorized by the original authors, casting doubt on the substantive interpretation of the original study. We conclude by listing several recommendations for researchers to minimize the risk that noncompliance poses to the theoretical interpretation of their experimental results.

2 Information Exposure in Survey Experiments

We focus on experimental studies of how the *exposure* to some experimentally manipulated information affects political attitudes and behavior. In these studies, researchers

randomly assign participants to be presented different information, often embedded in a survey questionnaire. The researcher then measures the effect of the manipulation on some outcomes of interest, such as attitudes or behavior. When the explanatory variable of theoretical interest is information, non-compliance can arise from failing to read or understand the information or taking up unanticipated information from other aspects of the study.

In the presence of noncompliance, the typical difference-in-means estimator recovers an intent-to-treat effect, or ITT. The ITT is the average effect of the information provision on the outcome, regardless of whether participants comply with the information treatment. This quantity may be of direct interest in a number of contexts across political science and policy analysis that seek an optimal intervention. For example, researchers may use survey experiments to identify the most persuasive, cost-effective, or broadly appealing political message.

In such studies, the researchers' goal can be to design treatments that closely resemble real-life politics. For instance, researchers might replicate real campaign fliers or public messages to help adjudicate the efficacy of various informational programs, at the cost of knowing which aspect of the message was generating an effect. In such cases, researchers may benefit from using an adaptive experimental design that allows them to learn the most effective arm in the space of various information treatments (Offer-Westort, Coppock, and Green 2021).

However, when the goal is the theoretical understanding of the treatment effect of a particular set of information, realism can be counterproductive. A realistic treatment will often vary more than just the information, bundling different facts together and raising the risk of providing unintended information. As Druckman (2022) puts it, "sound treatments do not depend on their mundane realism but rather on whether the relevant independent variable changes" (p. 54).[1]

When the goal is not policy relevance, noncompliance may still pose little issue for

[1]However, as we discuss below, experimentally changing the relevant independent variable is often insufficient for mapping empirical results onto theoretical expectations.

theoretical interpretation. Ignoring noncompliance amounts to using the intent-to-treat effect as a proxy for the treatment effect of interest. This may pose no threat to a study's substantive findings if three conditions hold: (1) the noncompliance is driven only by factors known to attenuate the ITT relative to the effect of theoretical interest, such as inattention (Berinsky, Margolis, and Sances 2014; Bansak et al. 2018), (2) we can statistically reject a null ITT despite noncompliance, and (3) the researcher theorizes only about the direction of the effect.[2]

However, if the problem goes beyond inattention and noncompliers change another aspect of their information environment unknown to the researcher, the ITT may be misleading. In such cases, even a substantively large ITT can be uninformative about the treatment effect of interest because it can be a mixture of the treatment effect of interest and the effect of the unobserved change in other information.

3 A Simple Diagnostic Test

One approach to addressing noncompliance in survey experiments is to report responses to factual manipulation checks (Kane and Barabas 2019; Harden, Sokhey, and Runge 2019). In contrast to subjective manipulation checks, which ask the respondents what they think of the manipulation of interest, or instructional manipulation checks, which evaluate attentiveness more generally, treatment-relevant factual manipulation checks evaluate simple recall about the main elements in the experiment. This requires researchers to be explicit about the intended interpretation of the information treatments.

What distinguishes manipulation checks from other post-treatment outcomes is that the researchers only ask the participants what the text provided to them earlier in the survey said vis-à-vis some aspect of the world, not what the participants themselves know and/or believe about it. Kane and Barabas (2019) find that posing such questions does not affect outcomes, offering a low-cost diagnostic for the study, and suggest it is

[2]We thus argue that noncompliance by itself is not necessarily a problem even in studies where the ITT is not of direct interest. For another perspective, see Harden, Sokhey, and Runge (2019).

possible to use the outcomes of these checks to help interpret experimental findings. However, once one estimates a passing rate, it is not clear how to incorporate the result into the study itself.

In the Online Appendix, we review survey experimental papers published recently in major political science journals. We find that treatment-relevant manipulation checks remain uncommon.[3] When they are used, the passing rate is highly variable: 29%–93%. All of these studies still reported statistically significant ITTs, raising the concern that, in some studies with high noncompliance, the results were driven by changes in some information not known or theorized by the researchers.

We propose a simple diagnostic test on responses to manipulation checks that assesses the extent to which noncompliance poses a threat to the theoretical interpretation of experimental results. The key idea here is that, for an informational experiment to be valid, the treatment only affects outcomes among the information as intended by the researcher. Thus, there should be no effect in the subsample that fails the treatment-relevant manipulation check. If, on the other hand, we do find such an effect, we should lower our confidence in the substantive hypothesis.

This diagnostic test relies on assumptions about treatment relevance. Those who received the information and changed their attitudinal outcomes in response to it will also pass the manipulation check. This is a reasonable assumption so long as the manipulation check is a precise evaluation of exposure. Inattentiveness in some respondents will not threaten this assumption if the inattentiveness persists between the manipulation check and the measurement of the outcome.

Some information experiments use only one treatment arm and do not expose respondents to any "placebo" text, while others expose respondents in different arms to different texts. The diagnostic test can be informative in both situations. In the first, only respondents in the treated group will answer a manipulation check, and

[3]The Online Appendix details our review procedure. For every article included in our search, we reviewed both the main text and the Online Appendix. Since online appendices are usually not subject to page limits, it seems this phenomenon cannot be explained by the lack of available space for the authors to mention manipulation checks.

the diagnostic test will compare those with incorrect recall with the control group. In the second, we recommend that researchers compare respondents with incorrect recall from different treatment arms.

4 An Illustration

For the purpose of exposition, we use Brutger et al.'s (2022) replication of Mutz and Kim (2017), for which the diagnostic test indicates the existence of a channel other than the theoretical mechanism hypothesized by the authors.

Table 1. Treatment arms in the ingroup favoritism experiment

Condition 1	US gains 10 jobs, other country gains 1,000
Condition 2	US gains 10 jobs, other country loses 1,000
Condition 3	US gains 1,000 jobs, other country gains 10

Kim and Mutz's study evaluated the role of ingroup favoritism in American attitudes toward trade agreements. Replicating the original design, Brutger et al. presented respondents with a vignette describing the expected relative gains in jobs for the US versus its partner across three conditions, listed in Table 1. Conditions 2 and 3 communicate relative gains, as in either case, the US gains more than the partner country. In Condition 1, the US loses in both absolute and relative terms. Respondents were then asked how likely they were to support or oppose this trade policy on a four-point scale ranging from strongly support to strongly oppose. The original authors then standardized this outcome variable by dividing by the standard deviation.

The original study also included a manipulation check asking whether the vignette said the US gains more, less, or the same as the other country.[4] According to the theory, a respondent would be exposed if they can recall the direction and extent of the job losses. We code the results of the manipulation check as correct recall if a respondent assigned to Condition 2 chooses "The US gains more."

[4]See Brutger et al. (2022a) and Brutger et al. (2022b).

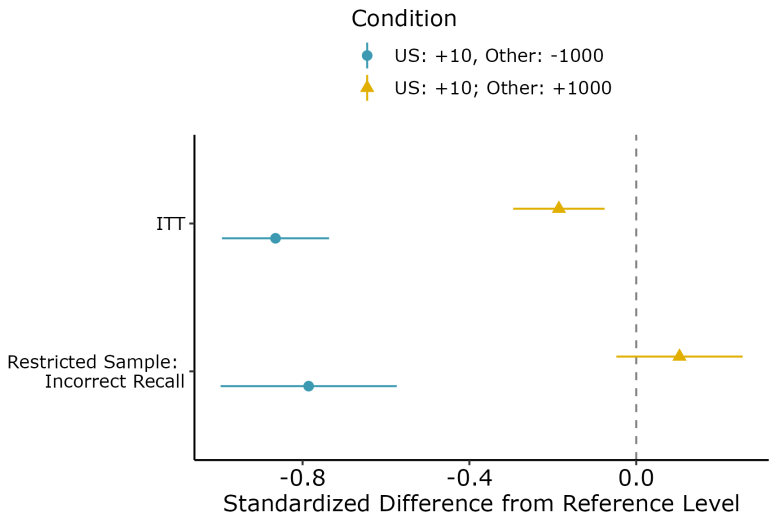

Figure 1. Analyses with a factorial coding of treatment assignment of the Ingroup Favoritism study (Brutger et al. 2022a). The original authors measured the outcome variable using a standardized four-point scale ranging from "strongly oppose" to "strongly support." Baseline: US: +1000; Other: +10. Horizontal bars indicate 95% confidence intervals with HC2 standard errors. N: 1507 for ITT estimation and 668 for the diagnostic test. No control variables are included.

If ingroup favoritism is indeed driving trade attitudes, we should expect the indicator of relative gains—in this case, the provision of the information that the US would benefit more—to have a positive effect on respondents' evaluation of the trade agreement and have no effect on trade attitudes for those who failed the manipulation checks. We create a factorial variable to indicate which condition a respondent is assigned to and perform several analyses with "Condition 3" (US: + 1,000 jobs; Other Country: -1,000) as the reference level. We calculate robust standard errors.[5]

At first glance, the results offer little support for the hypothesized effects of relative job losses and gains in evaluating trade agreements. First, respondents seem to prioritize absolute gains. As Figure 1 shows, relative to the reference condition of both absolute and relative gains for the US, respondents assigned to high relative gains but weak absolute gains ("US: +10; Other: -1,000") are much less likely to favor the trade deal. Respondents assigned to this condition also apparently dislike this trade deal even more than those assigned to a condition with high relative loss for the US ("US: +10;

[5]In the Online Appendix, we provide more detail on how our analysis differs that by Brutger et al. (2022a) Mutz and Kim (2017)

Other: +1,000"). Relative gains seem to reduce support.

However, these results may have arisen from unintended information embedded in the experiment. The result of the diagnostic test shows that, among those who cannot recall which country gains relatively, those assigned to the high relative gains and weak absolute gain condition, ("US: +10; Other: -1,000") still dislike the trade deal compared to those assigned to the reference condition. This suggests the difference in the respondents' attitudes toward the two trade deals is not driven primarily by considerations of the relative gain or loss for the US. While the diagnostic test does not specify the alternative information, in this case the results are consistent with the treatment arms unintentionally varying formation about total welfare improvement for both countries. The unintended information arises from a design that manipulates relative gains, which cannot hold constant both absolute own-country job gains and joint welfare for both countries.

5 Conclusion

We conclude by listing three recommendations for researchers using information survey experiments:

(1) The information that differs between the treatment and control groups should be the narrowest possible that still induces the intended variation in a theoretical construct.

(2) Include treatment-relevant manipulation checks that ask respondents to recall the information of interest; report the results of these checks.[6]

(3) Use the diagnostic we suggest in this note to assess the correlation between the treatment provision and the outcome for respondents who are unexposed.

We caution that, even when the diagnostic fails to reject the null with sufficient power, it does not mean that the treatment thus operates only through the theorized

[6]Doing so is consistent with Dafoe, Zhang, and Caughey's recommendation to measure the causal factor (2018).

channel. It could be that the information provision equally affects both the treatment of interest and another variable while only its effect on the irrelevant variable moves the outcome. When researchers can anticipate such a variable ex ante, we suggest they use the covariate control design recommended by Dafoe, Zhang, and Caughey (2018). Using these combined approaches will help researchers rule out some of the most plausible threats to inference and increase their confidence in the intended substantive interpretation of their results.

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A Appendix for *Information Manipulation in Survey Experiments*

A.1 More Details on the Replication

In their analysis, Brutger et al. coded Condition 3 as the treatment condition and the other two conditions as the control. We disagree with this coding because both Conditions 2 and 3 present a trade deal in which the US gains relative to the country and, if the theoretically relevant feature is the relative gain for the US, it would be more in line with the theory to code both as the treatment condition. However, the results of their manipulation check were coded in line with the theory being tested in that if a respondent assigned to Condition 2 chooses "The US gains more," the recall was coded as correct.

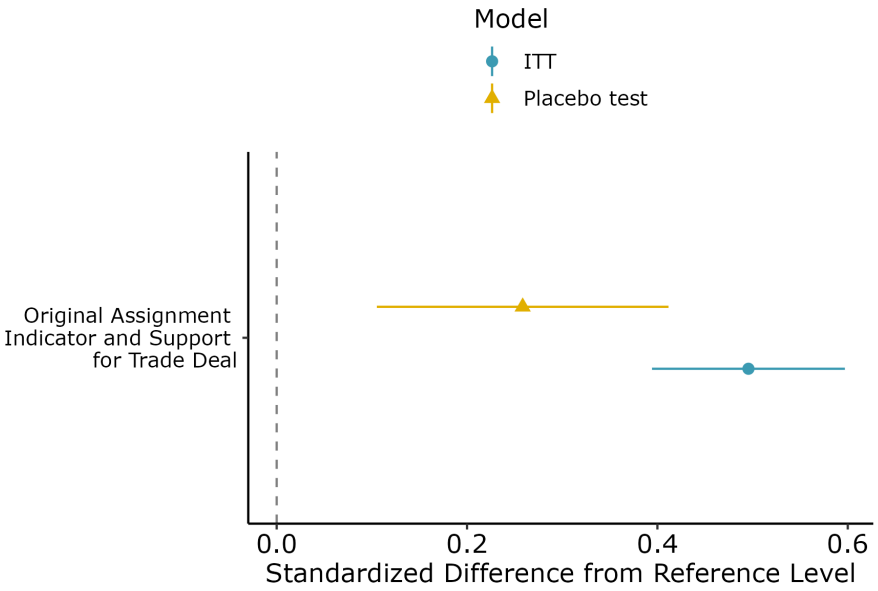

Figure A1. Exact replication of Brutger et al. (2022) and a diagnostic test using their manipulation check. The original authors measured the outcome variable using a standardized four-point scale ranging from "strongly oppose" to "strongly support." Horizontal bars indicate 95% confidence intervals with HC2 standard errors. N: 1507 for ITT estimation and 668 for the diagnostic test. No control variables are included.

We discovered this after our first diagnostic test, in which we restricted our analysis to those with incorrect recall across the three conditions but left the coding of the indicator of treatment assignment unchanged. The estimate thus targets the ITT for those respondents who failed to recall the treatment. As the coefficient from the diagnostic test (in the upper right in Figure A1) shows, the assignment to treatment has a positive effect even for those who failed to recall the information hypothesized to be theoretically relevant. This indicates that something other than relative gain for the US may be driving the respondents' support for a trade policy. In the main text, we argue that each arm by itself may be a bundled treatment. Grouping two bundled treatment arms makes results even harder to interpret. We thus chose to code the treatment indicator as a factorial variable.

A.2 Checking the Use of Checks

Table A1 shows the shares of papers published in the *American Journal of Political Science*, the *American Journal of Political Science*, and the *Journal of Politics* between 2019-2023 that deploy survey experiments with information treatments.

We first ran a search for articles that included the word "experiment" in either the title, abstract, or keyword list. We then excluded articles that used conjoint, discrete choice, or field experiments. We included experiments that manipulated a piece of information between treatment arms to change respondents' beliefs about some aspect of the world, real or hypothetical. We thus excluded articles that used textual variation to arouse different psychological states in the respondents that could not be fully characterized by changes in factual beliefs. We also excluded studies that varied non-textual visual stimuli, such as the skin tone of a hypothetical candidate.

We included the resulting papers as studies that involved survey experiments with information treatments. We then searched in the main articles and the appendices for one of the following word stems: "check," "manipu," and "atten" to examine if the papers mentioned they included manipulation or attention checks in their main studies

(not just in their pilot studies).

We categorized qualified papers into those with manipulation checks and those without. For those with manipulation checks, we further checked whether the manipulation check was "treatment-relevant" (Kane and Barabas 2019) in that it asked about the aspects of the information treatments that were directly relevant to the authors' explanatory variable of interest. If it did, we coded the study as having a treatment-relevant check. Table A1 shows the results of this review, and Table A.2 shows the full list of papers in our review.

Table A1. The shares of APSR, JoP, and AJPS papers with no manipulation checks (MCs), any MCs, and treatment-relevant MCs.

Category	Count
No mention of MC	43
Treatment-relevant (TR) MC	9
Any MC (excl. TRMC)	15
Total	67

Of the 67 papers we reviewed, only 9 have a treatment-relevant manipulation check and 15 have a manipulation check that we classify as not directly related to the treatment of theoretical interest: five are subjective manipulation checks while the rest are often attention checks. The remaining 45 papers do not mention the use of a manipulation check in either the main paper or the appendix. Of the experiments in the five papers that have manipulation checks, the median is 66%. These figures mask wide variation, even within such a small sample: The lowest passing rate is 29% and the highest 93%.

Table A2. A Review of Papers That Use Informational Survey Experiments.

Paper	MC[1]	TRMC[2]	SMC[3]	Pass[4]	Journal
Arriola and Grossman (2021)	0	0	0		JoP
Aytaç (2021)	1	0	1		APSR

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Table A2 – continued from previous page

Paper	MC	TRMC	SMC	Pass	Journal
Bakker, Lelkes, and Malka (2020)	0	0	0		JoP
Bayram and Graham (2022)	0	0	0		JoP
Bisgaard (2019)	0	0	0		AJPS
Boas, Hidalgo, and Toral (2021)[7]	0	0	0		JoP
Boudreau, Elmendorf, and MacKenzie (2019)	0	0	0		AJPS
Bush and Zetterberg (2021)	1	1	0	0.29	AJPS
Bøttkjær and Justesen (2021)	0	0	0		JoP
Campbell et al. (2019)	0	0	0		JoP
Campbell and Spilker (2022)	0	0	0		JoP
Cebul, Dafoe, and Monteiro (2021)[8]	1	1	0		JoP
Chapman and Chaudoin (2020)	0	0	0		JoP
Chow and Han (2023)	1	0	0		JoP
Chu and Recchia (2022)	0	0	0		JoP
Clayton, O'Brien, and Piscopo (2019)	1	1	0	0.93	AJPS
Condon and Wichowsky (2020)	1	0	0		JoP
Croco, Hanmer, and McDonald (2021)	0	0	0		JoP
Culpepper, Jung, and Lee (2023)	1	0	0		AJPS
Dias and Lelkes (2022)	0	0	0		AJPS
Druckman et al. (2022)	1	0	0		JoP
Duell et al. (2023)	0	0	0		JoP
Eck et al. (2021)	1	0	0		JoP
Fang and Li (2020)	0	0	0		JoP
Findor et al. (2023)	0	0	0		APSR
Gaikwad and Nellis (2021)	0	0	0		AJPS
Gandhi and Ong (2019)	0	0	0		AJPS
Gerber, Patashnik, and Tucker (2022)	0	0	0		JoP
Gerver, Lown, and Duell (2023)	0	0	0		JoP
Gottlieb (2022)	0	0	0		AJPS
Herzog, Baron, and Gibbons (2022)	0	0	0		JoP
Hill and Huber (2019)	1	0	0		AJPS

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[7]The field experiment in this paper uses a manipulation check but the survey experiment does not.
[8]Data on the manipulation checks are not available in the public data set.

Table A2 – continued from previous page

Paper	MC	TRMC	SMC	Pass	Journal
Jones and Brewer (2019)	0	0	0		JoP
Kam and Deichert (2020)	0	0	0		JoP
Karpowitz et al. (2021)	1	0	0		JoP
Kenwick and Maxey (2022)	1	1	0	0.54	JoP
Kim et al. (2023)	1	0	0		AJPS
Klar and McCoy (2021)	0	0	0		AJPS
Kobayashi et al. (n.d.)	0	0	0		AJPS
Krupnikov and Levine (2019)	0	0	0		JoP
Larsen (2021)	0	0	0		JoP
Lupu and Wallace (2019)[9]	1	1	0		AJPS
Madsen et al. (2022)	0	0	0		APSR
Malhotra, Monin, and Tomz (2019)	0	0	0		APSR
Manekin and Mitts (2022)	1	0	1		APSR
Martin and Raffler (2021)	0	0	0		AJPS
Mattes and Weeks (2019)	1	1	0	0.41	AJPS
Mullin and Hansen (2022)	0	0	0		AJPS
Mutz and Lee (2020)	1	0	1		APSR
Myrick (2020)	1	1	0	0.47	JoP
Nelson and Gibson (2019)	0	0	0		JoP
Batista Pereira et al. (2022)	0	0	0		JoP
Porter and Wood (2022)	0	0	0		JoP
Powers and Altman (2023)	1	1	1	0.93	AJPS
Powers and Renshon (2021)	1	1	0	0.87	AJPS
Robison (2022)	0	0	0		JoP
Sances (2021)	0	0	0		AJPS
Stephens-Dougan (2023)	0	0	0		APSR
Thachil (2020)	1	0	1		JoP
Todd et al. (2021)	0	0	0		JoP
Tomz and Weeks (2020a)	0	0	0		JoP
Tomz and Weeks (2020b)	1	0	0		APSR
Velez, Porter, and Wood (2023)	0	0	0		JoP

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[9]Data on the manipulation checks are not available in the public data set.

Table A2 – continued from previous page

Paper	MC	TRMC	SMC	Pass	Journal
Westwood, Messing, and Lelkes (2020)	0	0	0		JoP
Xu, Kostka, and Cao (2022)	1	0	0		JoP
Yair, Sulitzeanu-Kenan, and Dotan (2020)	1	0	0		JoP
Zhu and Shi (2019)	0	0	0		JoP

[1] Manipulation checks

[2] Treatment-relevant manipulation checks

[3] Subjective manipulation checks

[4] For treatment-relevant manipulation checks

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