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 note, we propose a diagnostic test and recommendations for treatment design that will help researchers evaluate theoretical mechanisms in survey experiments. This placebo test repurposes treatmentrelevant manipulation checks to evaluate responses under control conditions. This approach offers a path toward more robust and more informative survey experiments.

Keywords: information provision, survey experiments, manipulation checks, noncompliance.

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

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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 note, we show how noncompliance arises in survey experiments with information treatments and how this affects the theoretical interpretation of treatment effects. If the theoretical mechanism hypothesized in a study is correct, noncompliance will attenuate effect sizes. 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 interpret the estimated as driven by a set of information, experimental researchers need to make at least two assumptions: (1) relative to a placebo or control group, the information of interest does change respondents' ability to recall information or answer survey questions; (2) outside of the theorized information, there is no channel through which the provision of treatment can affect the outcome of interest.

To assess the plausibility of these assumptions, we offer a diagnostic placebo test that 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 treatment-relevant manipulation checks (MC), which are still uncommon but have been receiving more attention in political science experiments (Kane and Barabas 2019). An MC assesses whether respondents can recall the information intended by the researcher. Our test is inspired by recent advances in placebo tests (Eggers, Tuñón, and Dafoe 2023). It relies on the idea that, if the two assumptions above hold, there should be no difference in outcomes between respondents who are assigned to treatment and placebo if they do not recall the information.

In order for this approach to work, researchers must commit to a particular set of information to be manipulated by the survey experiment. This assumption relates to Dafoe, Zhang, and Caughey's (2018) paper on "information equivalence." Information equivalence is concerned with both the 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." A placebo belief is one that (1) can plausibly be affected by the information provision, (2) can affect the out-

come, and (3) is unrelated to any attribute affected by the treatment of interest. While such a test can be useful in many cases, it requires the researcher to specify the nuisance beliefs ex ante and include measurements of it. This can be challenging as the placebo belief needs to satisfy all three criteria above for the test 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 approach and 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 Mutz and Kim (2017), 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. More specifically, we consider settings where researchers compare some information of interest with some placebo information. 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, noncompliance can arise from failing to read or understand the information or taking up unanticipated information.

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 instance, researchers might replicate real campaign fliers or public messages to help adjudicate the efficacy of various programs. In such studies, the researchers' goal can be to design treatments that closely resemble real-life politics. 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 target is the treatment effect of a particular set of information, realism is secondary to an understanding of whether and how the relevant information changes. 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

In such cases, noncompliance may still pose little issue for 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 (weakly) attenuate the magnitude of the ITT relative to the effect of theoretical interest, such as inattention (Berinsky, Margolis, and Sances 2014; Bansak et al. 2018),² (2) we can statistically reject a null ITT despite noncompliance, and (3) the researcher theorizes only about the direction of the effect.³ 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 effect of the intended information 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) suggest it is 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

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

²This falls under our Condition 1 in Section 3.

³We 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).

remain uncommon.⁴ When they are used, the passing rate is highly variable: 29%–93%. All of these studies still reported statistically significant ITT estimates, 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. The test uses responses to manipulation checks to assesses the extent to which noncompliance poses a threat to the theoretical interpretation of experimental results. The key idea here is that, for an information experiment to be valid, the treatment only affects outcomes through the information the researcher seeks to convey. 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 can lower our confidence in the substantive hypothesis.

Consider a two-arm experiment where each respondent *i* is randomly assigned to treatment $Z_i \in \{0,1\}$: a placebo or baseline information $(Z_i = 0)$ versus the researcher-intended information $(Z_i = 1)$. Let $Y_i(0), Y_i(1)$ be the potential outcomes under placebo and treatment, respectively, and let $Y_i = Y_i(Z_i)$ be the observed outcome. We measure a treatment-relevant factual manipulation check after outcomes. Let $M_i(0), M_i(1) \in \{0, 1\}$ denote whether respondent *i* would correctly recall the information if assigned to the placebo arm $(M_i(0) = 1)$ or if assigned to the treatment arm $(M_i(1) = 1)$, respectively In other words, $M_i(Z_i)$ denotes whether a respondent would be able to recall what arm they were assigned to if they had been assigned to that arm.

Because $M_i(0)$ and $M_i(1)$ can each be 0 or 1, a respondent can belong to one of four "recall types" in their $M_i(0), M_i(1)$:

- $(M_i(0), M_i(1)) = (0, 0)$, Never Recallers: fail to recall in both arms;
- $(M_i(0), M_i(1)) = (1, 1)$, Always Recallers: recall in both arms;
- $(M_i(0), M_i(1)) = (0, 1)$, *Treatment-Only Recallers*: fail in placebo, recall in treatment;
- $(M_i(0), M_i(1)) = (1, 0)$, *Placebo-Only Recallers*: recall in placebo, fail to recall in treatment.

We adopt two standard assumptions for identification of the average intention-totreat effect: random assignment of Z_i and SUTVA.

Assumption 1 (A1) Random Assignment. The treatment assignment Z_i is independent of potential outcomes:

$$\{Y_i(0), Y_i(1), M_i(0), M_i(1)\} \perp Z_i.$$

⁴The Online Appendix details our review procedure. For every article included in our search, we reviewed both the main text and the online appendices. Therefore, it seems this cannot be explained by the lack of available space for researchers to mention manipulation checks.

That is, each unit i has the same probability of receiving the treatment, irrespective of individual characteristics and potential outcomes.

Assumption 2 (A2) SUTVA.

No Interference: For any respondent i and for any two treatment assignment vectors Z = (Z₁, Z₂,..., Z_n) and Z' = (Z'₁, Z'₂,..., Z'_n) that satisfy Z_i = Z'_i, the potential outcome for respondent i depends only on their own treatment assignment:

$$Y_i(\mathbf{Z}) = Y_i(Z_i).$$

• *Consistency:* For any respondent *i*, the potential outcome is unique:

$$Y_i(z) = Y_i(z')$$
 and $M_i(z) = M_i(z')$ if $z = z'$.

For the ITT to be informative about the direction of the treatment effect of the intended information, we need to assume that (1) the effect is not driven by differences in the ability to recall information between the treatment and placebo groups and that (2) the treatment provision does not induce changes in other, untheorized information that can affect the outcome. We state these two conditions formally below.

Condition 1 (C1) No Differential Recall.

$$M_i(0) = M_i(1) \quad \forall i.$$

Each respondent is either *never recall* (π_{00}) or *always recall* (π_{11}). This requires that an individual's ability to recall the information assigned to them to be the same under treatment and placebo. This condition is satisfied when, for example, noncompliance is driven by uniform inattention.

Condition 2 (C2) Exclusion Among Never-Recallers. If $(M_i(0), M_i(1)) = (0, 0)$, then

$$Y_i(1) = Y_i(0).$$

That is, for "never recall" types, the outcome does not depend on which arm they receive.

Assumptions A(1) and conditions C(1)-(2) jointly imply no difference in the expectation of potential outcomes for nonrecallers assigned treatment and control:

$$\mathbb{E}[Y_i(1) \mid Z_i = 1, \, M_i(1) = 0] = \mathbb{E}[Y_i(0) \mid Z_i = 0, \, M_i(0) = 0].$$
(1)

Let

$$\hat{\delta}_{\text{nonrecaller}} = \frac{1}{N_1} \sum_{Z_i=1, M_i=0} Y_i - \frac{1}{N_0} \sum_{Z_i=0, M_i=0} Y_i$$

where N_1 and N_0 are the number of respondents assigned to treatment and placebo, respectively.

With assumption A(2), we have

$$\mathbb{E}[Y_i(1) \mid Z_i = 1, \ M_i(1) = 0] - \mathbb{E}[Y_i(0) \mid Z_i = 0, \ M_i(0) = 0]$$

=\mathbb{E}(\delta_{nonrecaller}) (2)

See the Online Appendix for more details. Thus, we can use $\hat{\delta}_{nonrecaller}$ to test the following null hypothesis:

$$H_0: \mathbb{E}[Y_i(1) \mid Z_i = 1, M_i(1) = 0] - \mathbb{E}[Y_i(0) \mid Z_i = 0, M_i(0) = 0] = 0.$$
(3)

The hypothesis tests whether there is a difference in the expected outcomes between nonrecallers in the treatment and placebo groups. If there is a difference and we accept assumptions A(1)-(2), then one or both of conditions C(1)-(2) must be wrong. This would then pose difficulty for the interpretation of the ITT.

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



Figure 1. Analyses with a factorial coding of treatment assignment of the Ingroup Favoritism study (Brutger et al. 2022). 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.

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.⁵ 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."

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 as the reference level. We calculate robust standard errors.⁶

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 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. Relative gains seem to reduce support.

⁵See Brutger et al. (2022).

⁶In the Online Appendix, we provide more detail on how our analysis differs from Brutger et al.'s.

The result shows that the null hypothesis in (3). Since A(1)-(2) and C(1)-(2) jointly imply the null hypothesis and A(1)-(2) are highly plausible by design, one or both of C(1)-(2) must fail to hold. We believe C(2) is more likely to fail in this case because there is no apparent reason to suspect that the highly similar treatment conditions may cause a discrepancy in the respondents' ability to recall information.⁷ Thus, the 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 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 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.

5 Discussion

Our re-analysis of an ingroup favoritism experiment (Mutz and Kim 2017; Brutger et al. 2022) demonstrates that noncompliant respondents may exhibit treatment effects, calling into question the validity of the theoretical interpretation.

We offer the following recommendations for researchers conducting information experiments:

- (1) Narrow the informational difference between treatment conditions to minimize unintended manipulations.
- (2) Include treatment-relevant checks that directly assess recall of the key information and report these findings.
- (3) Compare treatment effects between respondents who pass and those who fail the manipulation check.

A precise null from our diagnostic does not guarantee that the treatment works solely through the targeted mechanism, but it reduces concerns about alternative channels (Dafoe, Zhang, and Caughey 2018). Incorporating this diagnostic can help link

⁷If C(1) is false, the interpretation of the results can also become tricky. It would mean that the treatment arms cause a difference in the participants' ability to recall what information they received. Any difference we observe or fail to observe in the outcomes between any two treatment arms can thus be due to this discrepancy in information recall between the arms instead of the difference in the underlying theoretical variable.

empirical findings more closely to theoretical claims. In other words, a well-powered null result can help strengthen a skeptical audience's belief that the study's intended theoretical interpretation is valid.

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Data Availability Statement

Replication code and data for this note will be made available.

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Appendix for Noncompliance with Information Treatments

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1 Derivation of Results

For never-recall individuals, we have $Y_i(1) = Y_i(0)$. Therefore,

$$\mathbb{E}[Y_i(1) \mid Z_i = 1, \ M_i(1) = 0] = \mathbb{E}[Y_i(0) \mid Z_i = 1, \ M_i(1) = 0].$$
(1)

By random assignment ($\{Y_i(0), Y_i(1), M_i(0), M_i(1)\} \perp Z_i$) and the fact that nonrecallers under treatment are a subset of the never-recall group, we have

$$\mathbb{E} [Y_i(0) \mid Z_i = 1, \ M_i(1) = 0]$$

= $\mathbb{E} [Y_i(0) \mid M_i(1) = 0]$
= $\mathbb{E} [Y_i(0) \mid M_i(0) = 0]$
= $\mathbb{E} [Y_i(0) \mid Z_i = 0, \ M_i(0) = 0].$

The never-recall group is the same principal stratum, whether or not these individuals were assigned to treatment or placebo; the event $\{Z_i = 1, M_i(1) = 0\}$ identifies the same never-recall type as $\{Z_i = 0, M_i(0) = 0\}$.

From (1) and (2), we obtain

$$\mathbb{E}[Y_i(1) \mid Z_i = 1, M_i(1) = 0]$$

= $\mathbb{E}[Y_i(0) \mid Z_i = 1, M_i(1) = 0]$
= $\mathbb{E}[Y_i(0) \mid Z_i = 0, M_i(0) = 0].$

Thus, under A(1), A(2), (C1) and (C2), the expected outcome among nonrecallers assigned to treatment is the same as the expected outcome among nonrecallers assigned to placebo.

2 More Details on the Illustrative Re-analysis

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

We provide more details on our re-analysis of the experiment in Brutger et al. (2022) and Mutz and Kim (2017). In their replication of Mutz and Kim's study, Brutger et al. coded Condition 3 in Table 1as 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.

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 1) shows, the assignment to treatment has a positive effect even for those who failed to recall the information hypothesized to be theoreti-



Figure 1. 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.

cally 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.

3 Checking the Use of Checks

In this section, we demonstrate the lack of attention to noncompliance and treatmentrelevant manipulation checks. Table 2 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 2 shows the results of this review, and Table 2 shows the full list of papers in our review.

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%.

Paper	MC^1	TRMC ²	SMC^3	Pass ⁴	Journal
Arriola and Grossman (2021)	0	0	0		JoP
Aytaç (2021)	1	0	1		APSR
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) ⁴	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) ⁵	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
Continued on next page					ext page

 Table 4. A Review of Papers That Use Informational Survey Experiments.

⁴The field experiment in this paper uses a manipulation check but the survey experiment does not. ⁵Data on the manipulation checks are not available in the public data set.

Paper	MC	TRMC	SMC	Pass	Journal	
Herzog, Baron, and Gibbons (2022)	0	0	0		JoP	
Hill and Huber (2019)	1	0	0		AJPS	
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) ⁶	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	
	Continued on next page					

Table 4 – continued from previous page

⁶Data on the manipulation checks are not available in the public data set.

Paper	MC	TRMC	SMC	Pass	Journal
Tomz and Weeks (2020b)	1	0	0		APSR
Velez, Porter, and Wood (2023)	0	0	0		JoP
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

Table 4 – continued from previous page

¹ Manipulation checks

² Treatment-relevant manipulation checks

³ Subjective manipulation checks

⁴ For treatment-relevant manipulation checks

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Table 2. The shares of APSR, JoP, and AJPS papers with no manipulation checks (MCs), any MCs, and treatment-relevant MCs.

Paper	MC ¹	TRMC ²	SMC ³	Pass ⁴	Journal
Arriola and Grossman (2021)	0	0	0		JoP
Avtac (2021)	1	0	1		APSR
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 $(2021)^1$	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 $(2021)^2$	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		A.JPS
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	0 0		JoP
Fang and Li (2020)	0	0 0	0 0		JoP
Findor et al. (2023)	0	0	0		APSR
Gaikwad and Nellis (2021)	0	0 0	0 0		AJPS
Gandhi and Ong (2019)	0	0	0		AJPS
Gerber, Patashnik, and Tucker (2022)	0	0 0	0 0		JoP
Gerver, Lown, and Duell (2023)	0	0	0		JoP
Gottlieb (2022)	0	0 0	0 0		AJPS
Herzog Baron, and Gibbons (2022)	0	0 0	0		JoP
Hill and Huber (2019)	1	0 0	0		AJPS
Jones and Brewer (2019)	0	0 0	0		JoP
Kam and Deichert (2020)	0	0 0	0		JoP
Karpowitz et al. (2021)	1	0 0	0		JoP
Kenwick and Maxey (2022)	1	1	0 0	0.54	JoP
Kim et al. (2023)	1	0	0	0.01	AIPS
Klar and McCov (2021)	0	0	0		AJPS
Kobayashi et al. $(n d)$	0	0	0		AIPS
Kruppikov and Levine (2019)	0	0	0		IoP
Larsen (2021)	0	0	0		JoP
Lunu and Wallace $(2010)^3$	1	1	0		AIPS
Madsen et al. (2022)	0	0	0		ADCB
Malhotra Monin and Tomz (2010)	0	0	0		ADCB
Manekin and Mitts (2012)	1	0	1		ADSB
Martin and Raffler (2022)	1	0	0		
Matters and Weeks (2010)	1	1	0	0 41	
1 1 1 1 1 1 1 1 1 1	1	1	0	0.41	A J L J

 Table 3. A Review of Papers That Use Informational Survey Experiments.