ORIGINAL ARTICLE

Estimating the causal effects of cognitive effort and policy information on party cue influence

Ben M. Tappin | Ryan T. McKay

Department of Psychology, Royal Holloway, University of London, Egham, UK

Correspondence
Ben M. Tappin, Department of Psychological and Behavioural Science, London School of Eeconomics and Political Science, 65 Aldwych, London WC2B 4DS, UK.
Email: b.tappin@lse.ac.uk

Present address
Ben M. Tappin, Department of Psychological and Behavioural Science, London School of Economics and Political Science, London, UK

Funding information
NOMIS Stiftung; Leverhulme Trust, Grant/Award Number: LIP-2022-001

Abstract
Party cues can influence public opinion, but the extent to which they do so varies dramatically from context to context. Why? The long-standing theory that party cues function as "heuristics" provides an answer, predicting that variation in exposure to policy information, a propensity for effortful thinking, or both causally affects the influence of party cues. However, this prediction has escaped decisive empirical testing to date, leaving in its wake a string of mixed results. Here we characterize the challenges that limit previous tests, and report on two large-scale experiments designed to overcome them. We find that exposure to policy information causally attenuates the influence of party cues, but engagement in effortful thinking per se does not. Our results advance understanding of the "when" and "why" of party cue influence; clarify a number of previously ambiguous findings; and have broad theoretical, methodological, and normative implications for understanding the influence of party cues.

INTRODUCTION

Understanding the causes of public policy opinions is a long-standing goal of political psychology. Among the most well-established causes are cues from political parties: people tend to support policies favored by their preferred party and to oppose those backed by the opposing party. However, the magnitude of party cue influence varies dramatically across studies, sometimes appearing negligible and other times exerting a dominant effect (Bullock, 2011, 2020; Tappin, 2023). This variability challenges the generalizability of findings and demands explanation (Findley et al., 2021; Yarkoni, 2020).

The heuristic theory of party cue influence provides a framework for understanding this variation. It posits that party cues serve as cognitive shortcuts, helping individuals navigate political decision-making when they lack either the ability or the motivation to

process policy information systematically. At the individual level, *both* ability *and* motivation are necessary for systematic processing; neither is sufficient on its own. At the aggregate level, however, a treatment that increases either motivation or ability-relevant conditions can reduce average cue reliance if many respondents already satisfy the other condition. Our study addresses both pathways: we manipulate motivation directly (via a cognitive effort treatment) and alter a necessary precondition for exercising ability—the *availability* of policy-relevant information—rather than cognitive capacity or prior knowledge per se. One cannot process information that one does not have access to. While our ability-relevant manipulation is limited to information availability (and does not speak to other conceptualizations such as cognitive sophistication), our design allows us to assess how cue influence responds to conditions that enable more systematic reasoning in politically relevant contexts.

In this article, we characterize the challenges to both causal inference and empirical generalization that limit previous tests of the heuristic theory of party cue influence. We then report the results of two large-scale survey experiments designed to overcome these challenges. In our experiments, we randomly and independently assigned both exposure to policy information and engagement in effortful thinking, as well as party cues, and measured American adults' opinions regarding 20 contemporary U.S. policy issues. We observed high levels of compliance with both the policy information and cognitive effort treatments.

In the remainder of the introduction, we (1) outline the heuristic theory of party cue influence, (2) describe why it provides a plausible explanation of variation in party cue effects across contexts, but (3) argue that the theory has been inadequately tested to date. We then proceed to test the theory in our experiments.

THE HEURISTIC THEORY OF PARTY CUE INFLUENCE

A large body of evidence indicates that the average American lacks substantial political knowledge (Delli Carpini & Keeter, 1996); holds unstable and ideologically incoherent policy opinions (Kinder & Kalmoe, 2017); and has little interest in thinking about politics and current affairs (Prior, 2018). While none of this evidence is particularly surprising—in the words of Downs (1957), "[political ignorance] is a highly rational response to the facts of political life in a large democracy" (p. 147)—it runs counter to the democratic ideal of an informed electorate, and raises concerns about how citizens can meaningfully participate in the democratic process if they possess little knowledge of government and the policy issues of the day.

The heuristic theory arose in response to this "democratic dilemma," contending that citizens rely on various environmental cues—such as party positions (Leeper & Slothuus, 2014)—as cognitive shortcuts to compensate for their lack of information and effortful reasoning (Downs, 1957; Kam, 2005; Lau & Redlawsk, 2001; Lupia & McCubbins, 1998; Mondak, 1993; Sniderman et al., 1993). By using such cues, citizens can form policy opinions and make political decisions that approximate those they would have done were they better informed or more motivated (Hobolt, 2006; Lupia, 1994; Lupia & McCubbins, 1998; Mondak, 1993).

A central implication of this theory is that cue reliance should diminish when individuals have both sufficient ability and motivation to process policy information systematically. In our tests, we vary information availability (a precondition for the exercise of ability) and motivation to examine how cue influence changes when these conditions are made more favorable to systematic processing.

VARIATION ACROSS PARTY CUE CONTEXTS

Despite the heuristic theory's intuitive appeal, the magnitude of party cue influence varies dramatically across studies. Bullock (2011) estimates that across existing studies, the influence of party cues on people's opinions ranges from 3% to 43% of the scale used to measure opinions. Such large variation undermines attempts to generalize findings and raises questions about the factors that determine cue strength (Findley et al., 2021; Yarkoni, 2020). The heuristic theory of party cue influence offers a plausible explanation of this variation because existing studies differ a great deal along the dimensions the theory identifies as important for determining the magnitude of party cue influence:

1. **Availability of Policy Information:** Some studies provide no information, or only minimal information, about policy positions (Barber & Pope, 2019; Druckman, 2001; Nicholson, 2012; Tappin, 2023; Tappin & Hewitt, 2021), while others include detailed arguments for and against a policy, from a paragraph or two (Cohen, 2003; Nicholson, 2011; Toff & Suhay, 2019; Van Boven et al., 2018) to full newspaper-length articles of policy information (Bakker & Lelkes, 2018; Bullock, 2011; Kam, 2005; Slothuus & de Vreese, 2010). The degree of policy information available to respondents may moderate the influence of party cues.
2. **Motivation to Think About Policy:** Different policy issues elicit varying levels of interest and engagement from the public. More salient, personally relevant, or straightforward policies may foster more political engagement, potentially reducing reliance on cues (Carmines & Stimson, 1980; Ciuk & Yost, 2016). It is plausible that people are more motivated to think about their opinion on such policy issues.

Unfortunately, it is difficult to determine whether variation along these dimensions *causes* differences in party cue influence across existing studies simply by examining the designs and results of those studies. The reason being that the studies simultaneously differ along many other dimensions—many of which may also shape the influence of party cues. This makes causal inference very difficult. Thus, we must look to more controlled empirical tests of the heuristic theory.

INADEQUATE TESTS OF THE HEURISTIC THEORY

The heuristic theory predicts a causal interaction between exposure to the party cue on the one hand, and knowledge of policy information and engagement in cognitive effort on the other. The results of numerous studies offer tests of this interaction. However, as we argue below, these tests are beset by challenges to causal inference and empirical generalizability that severely constrain their ability to provide evidence for or against the heuristic theory.

Tests using covariate designs

Many studies operationalize policy knowledge and cognitive effort using observed covariates, but this approach has significant limitations. For example, in an influential study on the policy issue of food irradiation technology, Kam (2005) found that party cue influence was weakest among individuals with higher political awareness—measured by factual political knowledge—consistent with heuristic theory predictions. However, she did not observe a similar relationship with self-reported need for cognition (Cacioppo & Petty, 1982); somewhat inconsistent with the heuristic theory.

Across many other studies using covariate measures, findings are mixed. Some studies find that higher values of the covariates are associated with weaker party cue influence, consistent with the theory (Anduiza et al., 2013; Barber & Pope, 2019; Boudreau & MacKenzie, 2014; Kam, 2005; Mondak, 1993). Others report no or limited evidence of such an association (Bullock, 2011; Ehret et al., 2018; Mérola & Hitt, 2016; Petersen et al., 2013; Slothuus, 2010). Some studies even find the opposite relationship (Bakker et al., 2020; Bakker & Lelkes, 2018; Slothuus & de Vreese, 2010).

Mixed findings aside, a more fundamental issue in interpreting these studies is confounding bias. For example, political interest likely correlates with both policy knowledge and motivation to think deeply about issues. Those more engaged in politics may already know party positions, meaning their opinions are pretreated, reducing the observed effect of party cues. Similarly, partisan motivation (Petersen et al., 2013) may amplify cue influence and co-vary with political awareness, obscuring the causal role of policy knowledge or cognitive effort. The extent of this confounding varies across policy issues, making generalization difficult (Guisinger & Saunders, 2017; Tappin, 2023).

Given the identification challenges faced by the covariate design, studies that randomize exposure to policy information or engagement in cognitive effort offer more informative tests of the causal interactions predicted by the heuristic theory.

Tests using experimental designs

We are unaware of any studies that randomize engagement in cognitive effort alongside exposure to party cues (though for a somewhat related design, see Groenendyk, 2013), but a number of studies do randomize policy information alongside exposure to party cues and measure policy opinions as an outcome (Agadjanian, 2020; Boudreau & MacKenzie, 2014; Bullock, 2011; Ciuk & Yost, 2016; Cohen, 2003; Nicholson, 2011; Peterson, 2019; Tappin et al., 2023).[1] However, the majority of these studies do not randomize the *extent* of exposure to policy information—for example, by including a no-information control group—but rather only the *type* of information that is available; whether the policy is characterized as consistent with liberal or conservative values, for instance. This design prevents testing the prediction of the heuristic theory that the influence of party cues will be diminished when substantive policy information is available (vs. not), or when the extent of relevant information is increased.

Of the remaining studies that do randomize the extent of policy information alongside party cues, the results are mixed regarding the prediction of the heuristic theory. The estimates of Boudreau and MacKenzie (2014) offer qualitative evidence that the influence of party cues is diminished when policy information is available—consistent with the prediction. However, they do not report a quantitative test of the causal interaction.[2] Agadjanian (2020), on the other hand, does report a quantitative test. He finds that exposure to approx. 120 words describing the negative implications of either an infrastructure or trade policy bill does not reliably diminish the influence of the party cue among Republicans or Democrats, respectively. This is inconsistent with the prediction of the heuristic theory.

Compounding (or perhaps explaining) these mixed findings, the aforementioned studies examined different policy issues, and the magnitude—or indeed existence—of the causal

---

[1]A handful of studies randomize information alongside party cues but the measured outcome variable is candidate support, not policy opinions.

[2]See Figure 1 in their paper. As far as we could tell, they only report quantitative tests of differences between conditions; not quantitative tests of the difference-in-differences between conditions, which are necessary to test the causal interaction.

interactions predicted by the heuristic theory may vary considerably across policy issues. For example, recent research shows there is substantial variation across policy issues in the effects of party cues (Clifford et al., 2023; Tappin, 2023). Thus, a more generalizable test of the theory's predictions demands a larger sample of policy issues than is common in prior work.

METHODS

Experiment design

We conducted two large-scale survey experiments in which we randomly and independently assigned both exposure to policy information and engagement in effortful thinking, as well as party cues, and measured American adults' opinions regarding 20 contemporary U.S. policy issues. The experiments covered a diversity of policy issues, corresponding to the broad policy areas of immigration, education, healthcare, foreign policy, taxation, the economy, and democratic governance, among others. The policy issues are reported in Table 1.

The basic design of the two experiments was the same. Respondents entering the survey answered a brief demographic questionnaire and were then asked for their opinion regarding five policy issues. Respondents gave their opinion on each issue in a sequential order (i.e., answering one issue before moving on to the subsequent survey page for the next issue), and the order of presentation was randomized. The five policy issues seen by each respondent were drawn randomly from a larger set of 10 issues per experiment. On each issue, the respondent was randomly assigned to receive a party cue (vs. no cue); information about the policy (vs. no information); and an inducement to effortful thinking about the policy (vs. no inducement). In other words, we implemented a factorial design with four factors, randomizing the party cue, policy information, cognitive effort, and policy issue—with the first three factors being randomized at the issue level. Policy opinions were always given on a seven-point Likert scale.

The set of 10 policy issues in Experiment 1 was drawn from the battery of policy questions used in phase one of the "Nationscape" surveys,[3] large-scale public opinion surveys of Americans' political attitudes that began in 2019. The set of 10 issues in Experiment 2 was drawn from the website www.isidewith.com, an online encyclopedia that provides Americans with information about the policy positions of dozens of U.S. political candidates and leaders (the encyclopedia professes to be nonpartisan and unaffiliated with any candidate, party, corporate sponsors, investors, or interest groups). The two sets of policy issues were selected on the basis of pilot studies and previous research (Tappin, 2023) to balance three criteria: a detectable influence of the party cue, the contemporary relevance of the policy issue, and to maximize the diversity of policy issues included. The selection strategy is reported in detail in Appendix 1.

Treatments

The party cue treatments consisted in respondents being informed of the positions of other party voters (Experiment 1, hereafter the party-voter cue), or Barack Obama and Donald Trump (Experiment 2, the party-elite cue) on the policy issue in question. To the extent that different

---

[3]https://www.voterstudygroup.org/data/nationscape.

TABLE 1 The policy issues used in Experiments 1 and 2.

Item label	Item text	Experiment
Abortion insurance	Allow employers to decline coverage of abortions in insurance plans	1
Abortion waiting	Require a waiting period and ultrasound before an abortion can be obtained	1
China tariffs	Impose trade tariffs on Chinese goods	1
Estate tax	Eliminate the estate tax	1
Health subsidies	Subsidize health insurance for lower income people not receiving Medicare or Medicaid	1
Immigration system	Shift from a more family-based to a more merit-based immigration system	1
Immigration wire	Require proof of citizenship or legal residence to wire money to another country from the USA	1
Public option	Provide the option to purchase government-run insurance to all Americans	1
Trans military	Allow transgender people to serve in the military	1
Vouchers	Provide tax-funded vouchers to be used for private or religious schools	1
Bank audit	Should the Federal Reserve Bank be audited by Congress?	2
Donations	Should corporations, unions, and nonprofit organizations be allowed to donate to political parties?	2
Foreign aid	Should the U.S. increase or decrease foreign aid spending?	2
Lobbyist ban	Should there be a 5-year ban on White House and Congressional officials from becoming lobbyists after they leave the government?	2
NATO	Should the U.S. remain in NATO?	2
Pensions	Should pension payments be increased for retired government workers?	2
Single payer	Do you support a single-payer healthcare system?	2
Surveillance	Should local police increase surveillance and patrol of Muslim neighborhoods?	2
Tax breaks	Should the President offer tax breaks to individual companies to keep jobs in the U.S.?	2
Work visas	Should the U.S. increase or decrease the amount of temporary work visas given to high-skilled immigrant workers?	2

styles of party cue have different effects on public opinion (Barber & Pope, 2019; Nicholson, 2012), using more than one style of cue increases the generalizability of our results.

The party-voter cue treatment consisted in respondents learning the distribution of policy support among a subset of Republican and Democratic Party voters who answered the Nationscape surveys in 2019. In particular, respondents learned the policy position adopted by the majority of the most politically interested, knowledgeable, and partisan voters from each party. For the 10 issues with party-voter cues, the majority positions of these Republican and Democratic Party voters were always in opposition. That is, if most Republican voters supported the policy, then most Democrat voters opposed it, and vice versa. The party-elite cue treatment consisted in respondents learning the policy positions of Barack Obama and Donald Trump, according to their public statements and voting record, documented by www.isidewith.com. For the 10 issues with party-elite cues, the elites'

positions were always in opposition. The delivery of the party cue treatments is illustrated in Appendix 2.1.

The policy information treatment consisted in respondents receiving a paragraph of information relevant to the policy issue, containing descriptive information as well as arguments for and against implementing the policy. The treatments were typically 120–140 words in length and were written by us using media and other sources for the purposes of the experiment (see Appendix 2.2 for the treatment text). This treatment structure is broadly similar to previous relevant work (Agadjanian, 2020; Boudreau & MacKenzie, 2014) and is reasonably reflective of the type of policy information that people might encounter in the "real" world—such as when reading a news article or talking with friends.

The cognitive effort treatment consisted in respondents being asked to explain in their own words their reasons for supporting or opposing the policy. They were given an open-ended response box in which to write their explanation, which was always presented before they recorded their policy opinion on the Likert scale. Thus, respondents were prompted to think about their position on the policy issue *before* their outcome variable was measured. To increase the strength of the treatment, respondents were also told that their explanations were not anonymous, but might be read by another person to be recruited in a later survey, and that this person would be asked to evaluate the quality of the respondent's reasoning. The theory underlying this addition to the treatment comes from a large body of research on the effects of accountability on human reasoning. In a comprehensive review of this research, Lerner and Tetlock (1999) conclude that unbiased, effortful thinking tends to increase when people are accountable to an audience whose views are unknown, but who are perceived to be interested in evaluating the quality of the reasoning or decision process, rather than its outcome (pp. 259–263).

Unlike the party cue and policy information treatments, the target of randomization in the cognitive effort treatment is an unobserved psychological variable, and can therefore only be encouraged rather than directly manipulated. Thus, it is particularly important to observe compliance with this treatment to ensure a diagnostic test of the causal interaction predicted by the heuristic theory. As we report in the results section below, reassuringly there were very high levels of compliance with the cognitive effort treatment in our experiments.

Sample

Respondents were U.S. adults recruited via the survey provider Prolific (https://www.prolific.co/). Samples recruited using this provider were not at the time representative of the general U.S. population, for example, they tended to be more Democrat. Sample characteristics are reported in Appendix 5. However, this does not significantly affect the ability of our design to test the causal interactions predicted by the heuristic theory. First, the estimates relevant for testing the interactions are all causal quantities obtained via randomization in our design. Recent work indicates that causal estimates obtained in convenience samples of U.S. adults track well with those obtained in national samples of U.S. adults (Coppock, 2019; Mullinix et al., 2015). Second, there are no clear scope conditions in the heuristic theory that indicate only a national (vs. convenience) sample of U.S. adults would conform to the predictions of the theory. Rather, the predictions are expected to apply broadly to those who take surveys and to those who do not. In other words, samples of respondents recruited via Prolific are fit-for-purpose with respect to the aims of our research design (Coppock & McClellan, 2019).

We aimed to recruit 3000 respondents in each experiment, equating to 15,000 respondent-issue observations in each experiment, and therefore, an expected total of 6000 respondents

and 30,000 observations across the two experiments. To mitigate any risk of differential attrition by treatment biasing our estimates, we implemented a planned double-sampling strategy, re-contacting all respondents who dropped out of the experiments during the policy rating segment (i.e., post-treatment) to obtain their missing outcomes (Coppock et al., 2017). Dropout was low in absolute terms: across the two experiments, a total of 6288 respondents started the survey, and 148 (approx. 2%) of these dropped out during the policy rating segment. Furthermore, double-sampling was highly effective: within 1 week of the initial survey being fielded, we recovered the missing outcomes of 145 (approx. 98%) of those who dropped out. Thus, there is negligible risk of bias in our estimates caused by differential attrition. Experiment 1 was fielded July 16, 2020, and Experiment 2 was fielded November 12, 2020.

ANALYSIS AND RESULTS

Treatment compliance

We begin by assessing compliance with the cognitive effort and policy information treatments. This is important for determining the diagnosticity of our tests of the causal interactions predicted by the heuristic theory. High compliance equals more diagnostic tests; low compliance equals fewer diagnostic tests.

We examine two variables to assess treatment compliance. The first variable is the time taken in seconds for the respondent to "submit" their answer to the policy question shown on the survey page. If respondents are reading the policy information and engaging in effortful thinking about the policy issue, the time-to-submit should be greater for policy questions assigned to the information and effort treatments, respectively. The second variable is the number of words written in the open-ended response box provided in the cognitive effort treatment. If respondents are complying with the encouragement to explain the basis of their policy opinion, the average number of words written should be considerably greater than zero.

Figure 1A shows the average time-to-submit in each of our eight conditions, and Figure 1B shows the average number of words written in the conditions that included the cognitive effort treatment. The data are very long-tailed—thus, we show both the mean (with 95% CI) and median of the data in each condition.

There were high levels of compliance with both the policy information and cognitive effort treatments. Regarding the former, as per Figure 1A, the median time-to-submit is approximately three times larger in the condition where (only) policy information was provided (32 s), compared with the control condition (10 s). Coupled with a substantive main effect of policy information on opinions (see below), these markedly longer page times indicate that respondents were able to use the information presented, consistent with baseline ability being sufficient in our sample.

Similarly, the median time-to-submit is dramatically larger in conditions that were assigned the cognitive effort treatment. This is compelling evidence that on average, respondents were complying with the encouragement to justify their policy opinion and engaging in cognitive effort while doing so. This conclusion is corroborated by the values in Figure 1B, which show that respondents wrote a median of between 36 and 42 words when prompted to justify their policy opinion. To illustrate, consider this randomly sampled response to the cognitive effort treatment within that range:

> I support a single-payer healthcare system because healthcare is a human right that everyone in the country should have equal access to. Private health insurance creates income inequalities, giving people with more money better access to healthcare.
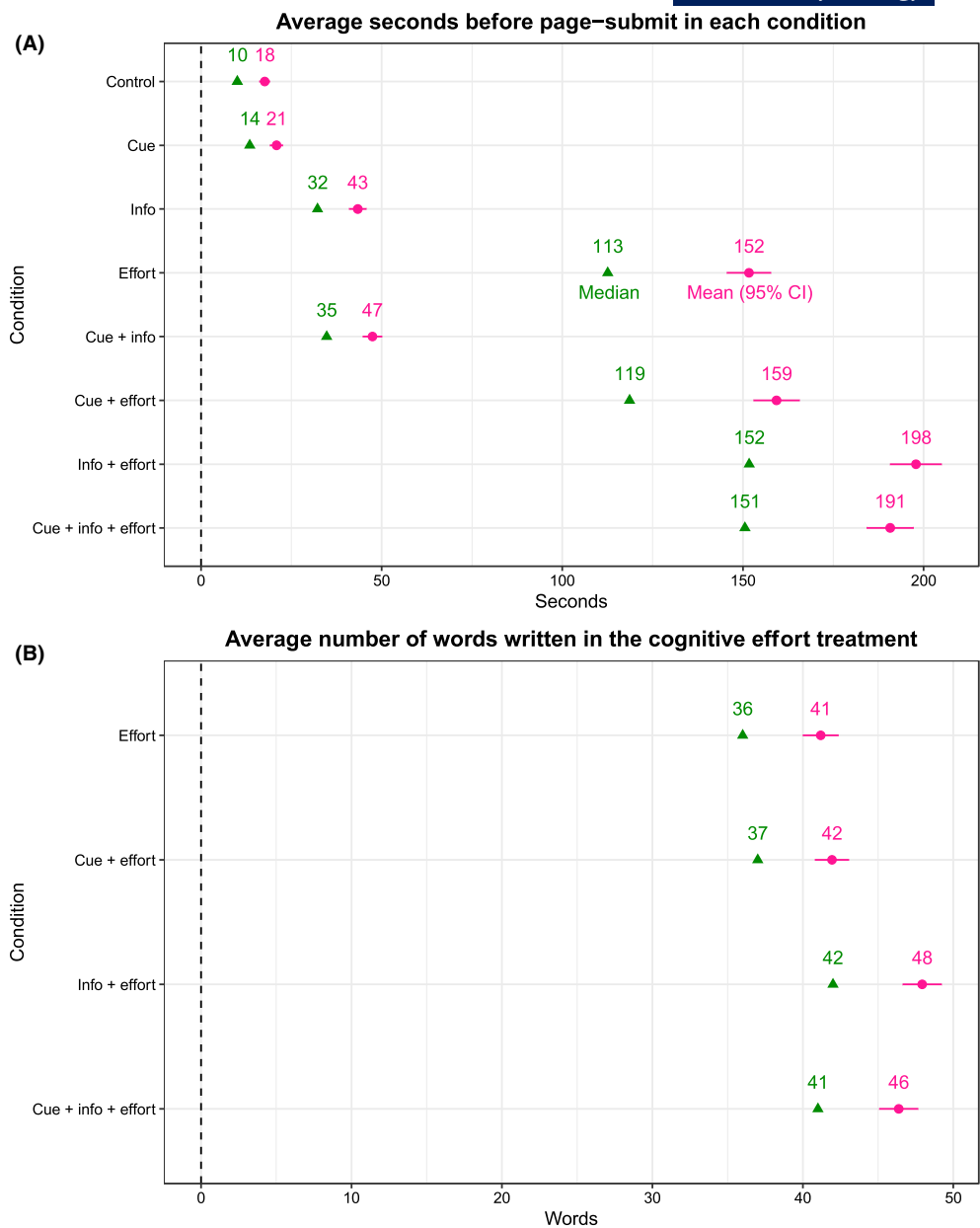
FIGURE 1 Analysis of treatment compliance. (A) Time spent on survey page. (B) Number of words written in open-text box.

We report additional examples in Appendix 6.1. In summary, the results of the treatment compliance analysis indicate that the policy information and cognitive effort treatments had the theoretically intended impact.

Main analysis and results

In this section, we test heuristic theory-consistent predictions by relaxing informational constraints (making relevant policy information available) and by increasing participants'

motivation to engage in effortful thinking, and then examining whether party cue influence diminishes under these conditions that together make systematic processing more likely. The outcome variable is opinion toward the policy in question, given on a seven-point Likert scale. To allow us to meaningfully aggregate across policy issues and respondents in our analysis, we re-code the outcome variable so that higher scores indicate agreement with the in-party cue. For example, a Democrat respondent who strongly supported the *Tax Breaks* policy (see Table 1)—originally a score of 7 on the scale—would receive a score of 1, since the in-party cue (Obama's position) is to oppose this policy. In contrast, a score of 7 from a Republican respondent would retain its original coding, since the in-party cue (Trump's position) is to support the policy. Thus, following this re-coding scheme, the expected sign of the party cue treatment effect is positive for all partisan respondents and policy issues. We include party "leaners" in the analysis but exclude Independents.

To test the predictions, we fit a multilevel linear regression model to the data. This approach is preferable to regular OLS given the clustered structure of our data (Gelman & Hill, 2006; McElreath, 2020). In addition, the multilevel model offers several advantages over OLS with clustered standard errors. For example, as well as estimating average effects aggregated across all 20 policy issues, we also wish to estimate the effects at the level of individual policy issues—to examine heterogeneity. The multilevel model allows us to do so while avoiding overfitting the data. The intuition here is simple: because our design contains many different policy issues and conditions, even though we have a large sample size overall there is still a relatively small number of observations with which to estimate effects at the level of each individual policy issue. Thus, estimating these effects using just the raw data for each policy issue may produce some estimates that are large (or small) simply due to sampling variability. Such estimates would not generalize well to a new data set; they are overfit. The multilevel model addresses this problem by optimally "shrinking" the individual estimates toward the mean estimate, thereby reducing overfitting and improving the out-of-sample accuracy of the individual estimates on average (see Chapter 13, McElreath, 2020).

Our model specification includes a parameter on a dummy variable for each of our treatments—party cue, policy information, and cognitive effort—as well as parameters for all two- and three-way interaction terms. The model specification allows all eight of the resulting parameters (including the intercept) to vary across policy issues as well as across respondents. We fit the model in a Bayesian framework and specify vague, weakly informative priors on all model parameters, allowing the data to "speak for itself."[4] The formal model specification and diagnostics are reported in Appendix 6.2. The model is fitted on a total of 29,099 observations from 5905 respondents who identified with either the Democratic or Republican Party. For each of the parameters estimated by the model, we report the median of the posterior distribution and the 95% highest posterior density interval (HPDI). The HPDI is the narrowest region that covers the value of the parameter with 95% probability, given the data and model.

Figure 2A shows the distribution of policy opinions in each condition, with the raw mean and model-estimated mean overlaid. The uncertainty intervals on the latter means are wider because the multilevel model properly accounts for the clustered structure of the data (Yarkoni, 2020).

The top row of Figure 2B shows the average treatment effect (ATE) of the party cue in the absence of the policy information and cognitive effort treatments—which we refer to as the baseline ATE. Figure 2B also shows the party cue ATE under the various combinations of information and effort treatments (rows 2 through 4). The baseline ATE is about half a Likert scale point and is bounded precisely and clearly to the right of zero; as expected, party cues reliably influenced people's policy opinions on average (Bullock, 2020). Furthermore,

---

[4]The model is fit using the BRMS (Bürkner, 2017) package in R (R Core Team, 2018). Data and plots are wrangled and generated using the tidyverse suite of R packages (Wickham, 2017).
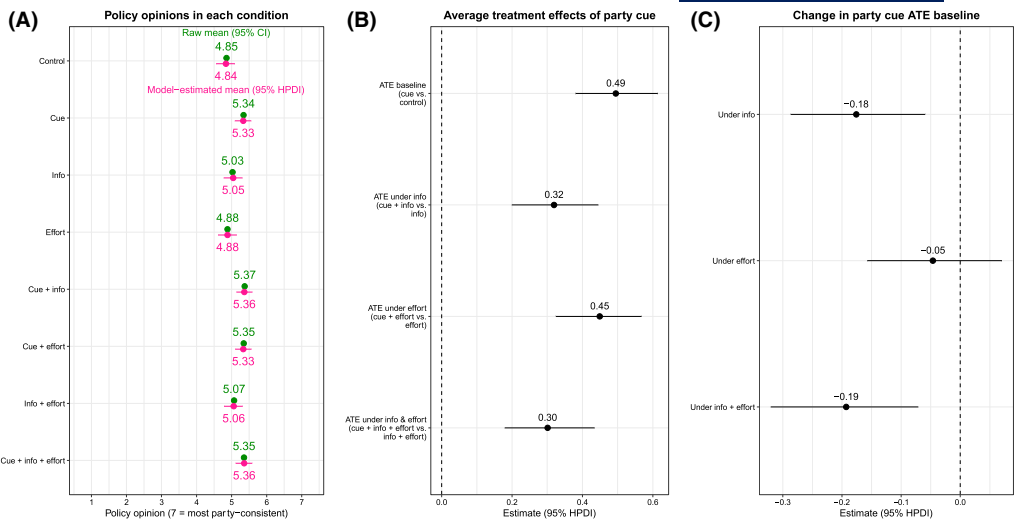
(A) Policy opinions in each condition

Raw mean (95% CI)
Model−estimated mean (95% HPDI)

Control: 4.85 / 4.84
Cue: 5.34 / 5.33
Info: 5.03 / 5.05
Effort: 4.88 / 4.88
Cue + info: 5.37 / 5.36
Cue + effort: 5.35 / 5.33
Info + effort: 5.07 / 5.06
Cue + info + effort: 5.35 / 5.36

Policy opinion (7 = most party−consistent)

(B) Average treatment effects of party cue

ATE baseline (cue vs. control): 0.49
ATE under info (cue + info vs. info): 0.32
ATE under effort (cue + effort vs. effort): 0.45
ATE under info & effort (cue + info + effort vs. info + effort): 0.30

Estimate (95% HPDI)

(C) Change in party cue ATE baseline

Under info: −0.18
Under effort: −0.05
Under info + effort: −0.19

Estimate (95% HPDI)

FIGURE 2 Main results.

qualitatively consistent with the predictions of the heuristic theory, the party cue ATE is smaller under every combination of policy information and cognitive effort treatment (rows 2 through 4) compared with baseline (top row). In other words, the influence of the party cue was attenuated when people were exposed to substantive information about the policy, induced to engage in effortful thinking about the policy, or both. In terms of magnitude, when substantive policy information was available, the influence of the party cue attenuated by approximately 35% (1−.32/.49) on average; when cognitive effort was induced, it attenuated by 8% (1−.45/.49); and under both policy information and cognitive effort, it attenuated by 39% (1−.30/.49).

Figure 2C shows the key quantitative tests of the article: whether these magnitudes of attenuation are significantly larger than zero. As indicated by the estimate in the top row, we can conclude with >95% probability that the attenuation of party cue influence is larger than zero when substantive policy information was available—consistent with the prediction of the heuristic theory. We can draw a similar conclusion when substantive policy information was available *and* effortful thinking about the policy was induced (third row). In contrast, however, there is little evidence that cognitive effort by itself attenuated the influence of the party cue: the data and model are compatible with the true decrease in party cue influence under cognitive effort being zero (middle row). The results also show that inducing cognitive effort in the presence of policy information did almost nothing to attenuate the influence of the party cue beyond exposure to the policy information by itself; the point-estimates in the first and third rows are almost identical.

These results are robust to alternative model specifications, such as ordered-logistic and right-censored versions of the model described above (see Appendix 6.2.3). These models alleviate concerns about spurious effects due to (i) the assumption that the outcome variable is metric (when it is in fact ordinal), and (ii) the raw data being right-skewed (Figure 2A)—potentially inducing ceiling effects.

Heterogeneity across policy issues

To what extent do the foregoing results vary across policy issues? The answer has important implications for the generalizability of the results for any individual policy issue; specifically,
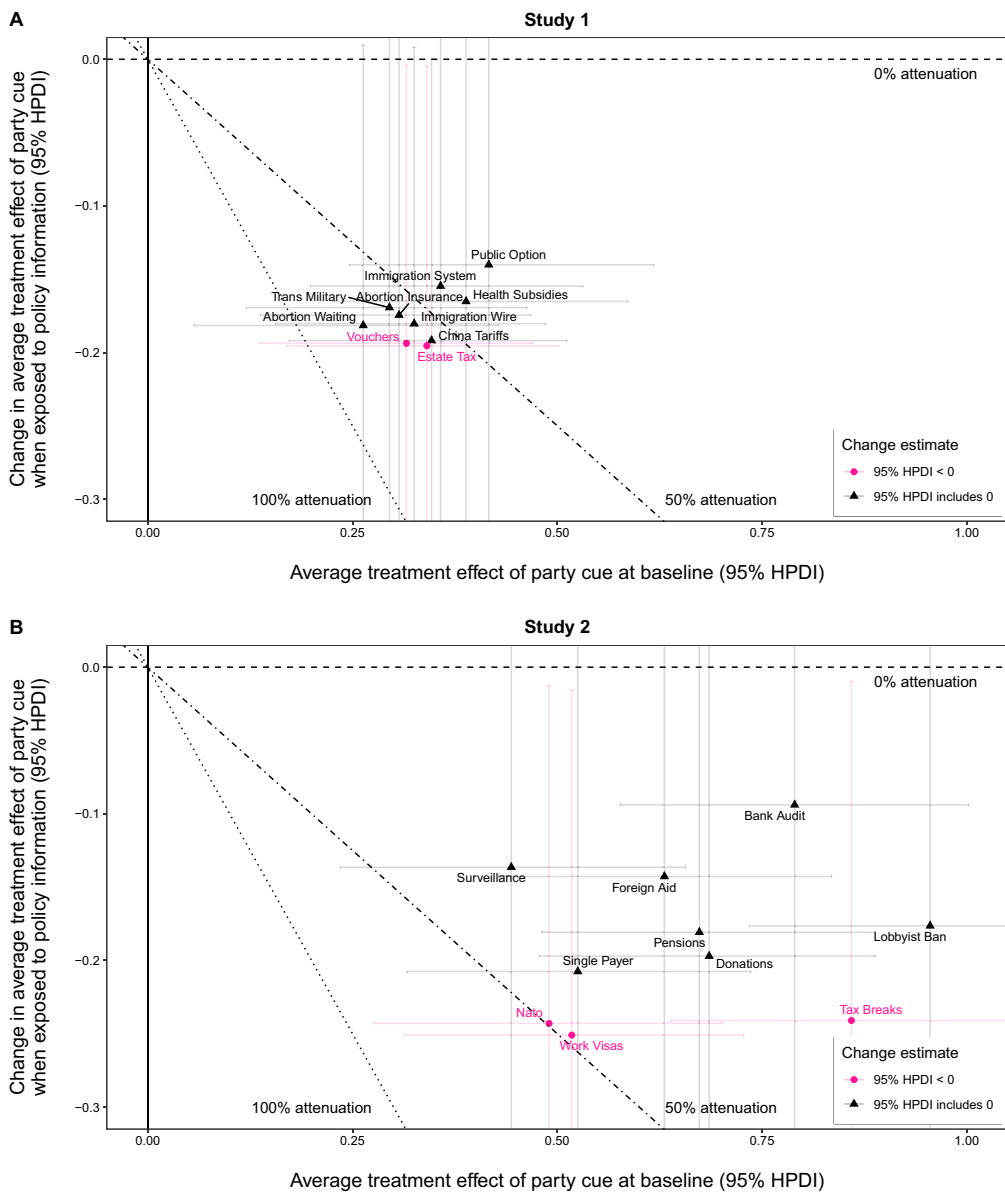
FIGURE 3 Heterogeneity across policy issues, disaggregated by study.

high variation implies low generalizability across policy issues (Findley et al., 2021; Linden & Hönekopp, 2021; Yarkoni, 2020). To answer this question, we compute the same estimates as those in Figure 2B,C, but this time we do so for each individual policy issue in our set of 20. To isolate the variation that is due to policy issues alone, as opposed to different types of party cues, we conduct this analysis separately for each study. This produces a large number of estimates. Thus, to ease interpretation of the pattern across policy issues, we plot a subset of the estimates here, shown in Figure 3 (see Appendix 6.3 for the plots with the additional estimates). As mentioned previously, we compute these estimates directly from the multilevel model, not by fitting separate models for every policy issue. Thus, the estimates are adaptively

"shrunk" toward the mean estimate which minimizes the risk of overfitting (see Chapter 13, McElreath, 2020).

The *x*-axis of Figure 3A (Study 1) and 3B (Study 2) shows the ATE of the party cue at baseline; that is, in the absence of the policy information and cognitive effort treatments. The policy issue with the largest baseline ATE is *Lobbyist Ban* (Study 2)—with an ATE of a little under one Likert scale point—and the smallest is *Abortion Waiting* (Study 1). There is a clear pattern on average such that the party cue effects in Study 2 (panel B) were larger and more variable—suggestive of the idea that party *leader* cues can be more powerful than party-*voter* cues (though caution is warranted because of the different issues used in each study). The *y*-axis shows the *change* in the baseline party cue ATE when respondents were exposed to the policy information treatment; negative values therefore indicate an attenuation of the influence of the party cue under policy information (the value is negative for all policy issues). The policy issues denoted by pink circles indicate that the 95% uncertainty intervals on the estimated change do not overlap with zero; whereas the black triangles indicate that the interval does overlap with zero. However, the pattern of estimates across issues is of greater interest than the statistical "significance" of any individual estimate.

Specifically, because the change in the party cue ATE (*y*-axis) is plotted as a function of the baseline party cue ATE (*x*-axis), the position of the estimates in the plot communicates the percentage by which the party cue ATE attenuated under policy information exposure. For example, consider the estimate for the policy issue *Estate Tax* (Figure 3A). The baseline ATE (*x*) is approximately .35, and the change in the baseline ATE (*y*) is −.20. Thus, when respondents were exposed to policy information, the influence of the party cue on this issue attenuated by more than 50%. We can see this from the plot because the estimate for *Estate Tax* falls just below the diagonal dot-dashed line. The estimates of several other policy issues also fall close to this line, such as *Immigration Wire* (Figure 3A) and *NATO* (3B).

However, other estimates do not. At one end of the extreme is *Abortion Waiting* (left-most estimate in Figure 3A), a policy on which the party cue ATE attenuated by approximately 75% under policy information exposure. At the other end of the extreme are the issues of *Lobbyist Ban*, *Bank Audit*, and *Tax Breaks* (right-most estimates in Figure 3B), where the party cue ATE attenuated by approximately 20%–30%. Thus, the estimated heterogeneity across policy issues is substantial: in one (policy issue) context, the large majority of party cue influence is attenuated by policy information; while, in other (policy issue) contexts, most cue influence remains. Of course, in absolute terms, the heterogeneity is smaller: the estimated change in the baseline ATE is between −.10 and −.25 Likert scale points for all policy issues. However, the interpretation of attenuation on the relative scale is most appropriate here, because the hypothesis in question is that party cue influence attenuates under policy information—irrespective of the size of that influence and the particular scale on which it is measured. For example, observed in isolation, these different relative magnitudes of attenuation paint a very different picture of support for the hypothesis.

This heterogeneity has several implications. For example, it illustrates the importance of studying more than a handful of policy issues when testing general theories of party cue influence (Clifford et al., 2023; Tappin, 2023). The number of issues typically examined is small—between one and four. With the potential for large variation across issues, one can easily imagine different studies arriving at different conclusions regarding the magnitude by which exposure to policy information attenuates the influence of party cues. More generally, studying a small and idiosyncratic set of policy issues can thus foment inconsistencies in the published literature and stall theoretical progress (Findley et al., 2021; Linden & Hönekopp, 2021; Yarkoni, 2020). We return to the discussion of generalizability in our conclusion.

What is the mechanism?

Thus far we have found that on average, the influence of party cues is causally attenuated by exposure to policy information. This is consistent with the assumption of the heuristic theory that people use party cues as informational "shortcuts." Therefore, when people have policy information, they rely less on party cues.

This tells us *when* party cues will exert weaker (vs. stronger) influence, but not *why*. The heuristic theory offers an explanation here, too, because it assumes that cue-following allows people to form policy opinions that approximate those they would have formed if more knowledgeable (Hobolt, 2006; Lupia, 1994; Lupia & McCubbins, 1998; Mondak, 1993). In other words, the theory assumes that party cues are *valid* information shortcuts. This assumption implies that exposure to party cues and to policy information should (separately) move people's opinions in the same direction on average. If this assumption holds, it provides an explanation for why policy information attenuates the influence of party cues: because it already sorts people's opinions along party lines. Previous influential studies testing the valid-shortcut assumption have drawn upon observational data only and focused on a small sample of policy issues. Threats to causal inference and generalizability thus loom large. In contrast, our design is fully experimental and incorporates a large sample of distinct policy issues—mitigating these threats.

Our results to this point do not shed light on the valid-shortcut assumption, because policy information exposure could attenuate cue influence without moving opinions in the same direction; for example, if the mechanism is purely attention-based (e.g., policy information simply draws attention away from the party cue), rather than based on opinion-sorting. Thus, to test the valid-shortcut assumption, we seek to compare the effect of the party cue (vs. control) with the effect of the policy information (vs. control). The assumption implies that these two effects will be similar in direction and magnitude.

The relevant estimates are shown in Figure 4. These estimates are from a joint model analyzing studies 1 and 2 together (to maximize precision and given that heterogeneity across policy issues is not our main focus with this analysis). To facilitate comparison of the party cue and policy information effects, we plot their joint distribution. We do this for both (i) the estimates *averaged across* policy issues (labeled "aggregate" in the figure), as well as (ii) for each individual policy issue. The *x*-axis shows the ATE of the party cue at baseline (i.e., vs. control), and the *y*-axis shows the ATE of policy information at baseline. Thus, estimates falling along the diagonal dotted line show that the party cue and policy information effects are equivalent in direction and magnitude. Recall that the outcome variable is coded such that positive values indicate opinions are more party-consistent.

It is clear from Figure 4 that nearly all estimates fall *below* the diagonal line: the effect of the party cue was stronger than that of the policy information. This is perhaps unsurprising given the party cue treatment has a clear directional element, whereas the policy information treatments were balanced in content. Nevertheless, policy information exposure did cause people to adopt more party-consistent positions overall; the aggregate ATE across policy issues was approximately .21, and the uncertainty intervals bound it precisely above zero. Notably, this is about 40% of the size of the party cue ATE (.49)—which is comparable to the *attenuation* in cue influence when people were exposed to policy information (i.e., 35%, see Figure 2B). This is consistent with the idea that policy information exposure attenuated cue influence because it already (partially) sorted people's opinions along party lines.

Therefore, these results are broadly consistent with the valid-shortcut assumption of the heuristic theory and offer evidence of a potential mechanism by which policy information exposure attenuates cue influence. To quantify the extent of consistency with the valid-shortcut assumption, we categorize the estimates in Figure 4 into three groups. Estimates in circles are strongly consistent with the assumption: both party cue and policy information
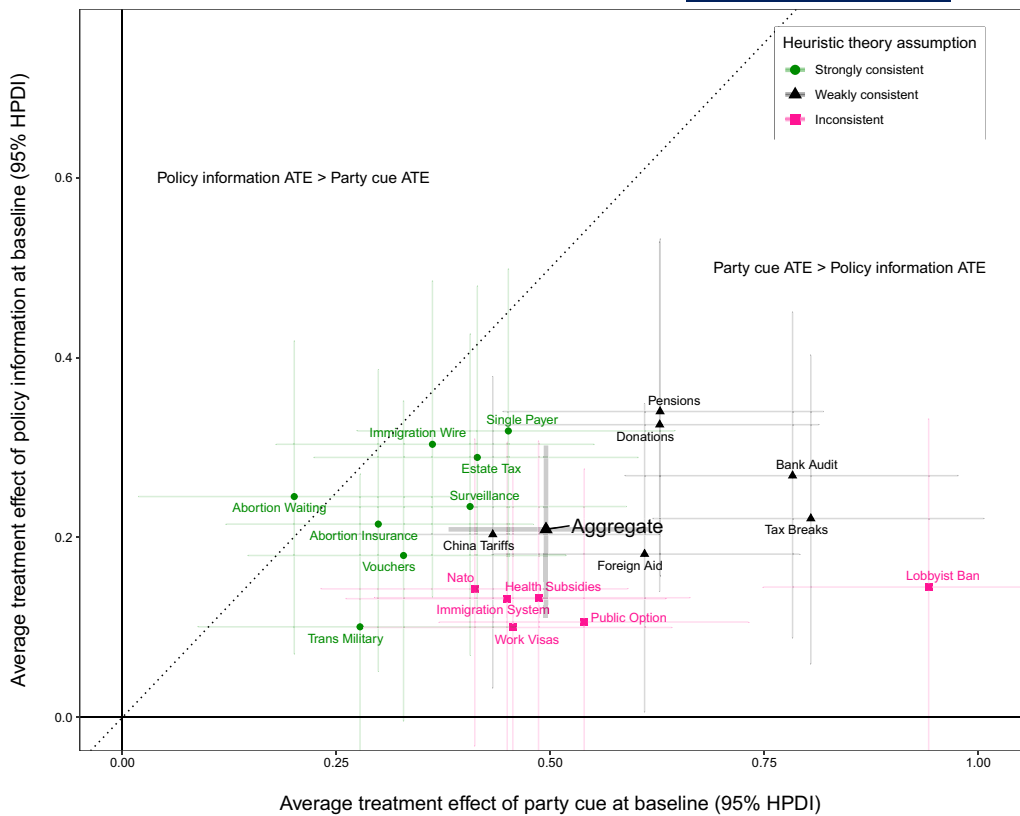
FIGURE 4 Separate party cue and policy information effects at baseline.

ATEs have the same sign and their magnitudes are similar (not distinguishable from each other with 95% probability). Estimates in triangles are weakly consistent: the ATEs have the same sign and are both distinguishable from zero with 95% probability, but the effect of the party cue is larger (with 95% probability). Finally, estimates in squares are inconsistent: the policy information ATE is not distinguishable from zero (with 95% probability) *and* the party cue ATE is larger. On aggregate, the evidence is weakly consistent with the valid-shortcut assumption. (See Appendix 6.4 for the table of the values used to categorize the estimates.)

DISCUSSION

Party cues influence people's policy opinions. Yet, there is enormous variation in the magnitude of their influence across contexts (Bullock, 2011, 2020), undermining the generalizability of estimates of party cue influence and undermining our ability to explain and predict the influence of political parties on public opinion in any given context (Findley et al., 2021; Linden & Hönekopp, 2021; Yarkoni, 2020). While the long-standing theory of party cues as "heuristics" offers a remedy for this situation, previous work has not yielded a clear empirical test of the theory's key predictions. In this article, we sought to address prior limitations in study design, thus providing the most comprehensive test of the heuristic theory to date.

Our key findings are threefold. First, we found that exposure to substantive policy information causally attenuated the influence of party cues. We interpret this as evidence that, in our

setting, relaxing informational constraints enabled many respondents—who had sufficient baseline ability and motivation—to use policy content, thereby reducing the marginal value of the cue. In contrast, an inducement to think effortfully about policy did little to attenuate party cue influence. Together, these results suggest that informational scarcity, rather than motivation, was the primary binding constraint in our experiments.

Second, we found large heterogeneity across policy issues in the extent to which policy information attenuated the influence of party cues. Third, we found evidence of a potential mechanism by which policy information may attenuate party cue influence: it already partially sorts people's opinions along party lines. This evidence is consistent with the assumption that party cues are *valid* shortcuts for policy information.

Here, we reflect upon the implications of these findings.

Our first result shows that variation in policy information exposure explains much of the variation in party cue effects across contexts. This improves our ability to predict when cues will exert a stronger vs. weaker influence on public opinion. For example, several recent studies found no evidence that party cues influenced public opinion regarding COVID-19 (Gadarian et al., 2021). Our findings validate the authors' post hoc speculation that a saturated media environment attenuated cue influence. Specifically, when policy-relevant information is prevalent, party cues tend to be less influential. While this may seem intuitive, it was not a given, as another plausible explanation was that prior exposure to party cues reduced their impact (Slothuus, 2016).

Our findings also offer a new perspective on political sophistication and cue influence (Bullock, 2020; Kam, 2005). Sophistication is often measured through factual political knowledge under the assumption that more informed individuals rely less on cues. However, political sophistication may co-vary with prior exposure to party cues, complicating interpretation of the simple finding that political sophistication correlates with diminished reliance on party cues (Anduiza et al., 2013; Barber & Pope, 2019; Kam, 2005; Mondak, 1993). By experimentally controlling policy information, we clarify that the aforementioned political sophistication results may indeed be driven by awareness of policy information—not simply by prior exposure to the party cue.

The null effect of cognitive effort coheres with studies suggesting that self-reported "need for cognition" does not moderate cue influence (Bullock, 2020; Kam, 2005). Our study improves confidence in this conclusion by avoiding confounding bias and ensuring strong treatment compliance (Figure 1). This further challenges the notion that party cues are heuristics for avoiding effortful thinking (Bullock, 2020; Petersen et al., 2013). However, our results do not rule out alternative explanations, such as the possibility that different types of cognitive effort induction might yield different results or that party cues save people the effort of independent information search.

There are two implications of our finding that party cues and policy information both influenced opinions in the same direction on average.

First, policy information exposure may attenuate the influence of party cues by partially sorting opinions along party lines. This mechanism explains why some studies did not find attenuation—because their policy information treatments failed to sort people's opinions along party lines. For example, Agadjanian (2020) and Tappin et al. (2023) provided policy information that countered party cues, meaning it did not reinforce partisan alignment. Future work should more systematically test this mechanism.

Second, this finding coheres with the claim that party cues allow voters to form policy opinions similar to those they would form were they more knowledgeable. This supports the argument that party cues serve as useful heuristics in democratic decision-making (Downs, 1957; Lupia & McCubbins, 1998; Mondak, 1993; Sniderman et al., 1993).

Caution is warranted, however, before taking our findings as unbridled support for this normative interpretation. The assumption that party cues help voters make informed choices depends on voters' motivations. If voters prioritize policies that benefit them—such as access to

abortion—then our findings cohere with the normative interpretation (Achen & Bartels, 2017). However, if voters primarily seek social conformity within their political communities, cue-following may be more about group identity than informed decision-making (Kahan, 2016; Williams, 2020). In this case, both party cues and policy information may shift opinions simply by signaling socially acceptable positions. Indeed, voters could simply have been looking for other types of group cues in the substantive information—trying to determine which "side" they should take instead of weighing the arguments on their merits (Dias & Lelkes, 2022; Tappin et al., 2023). The extent to which our findings support an interpretation of party-cues-as-heuristics that is normative for democracy depends on the assumptions one makes regarding voters' unobserved motivations.

Finally, though we found that policy information attenuated party cue influence by 35%–39% on average, attenuation varied notably across issues. This illustrates the importance of studying multiple policy issues when testing theories of party cue influence—and of political communication broadly. Scholars often emphasize generalizing from study participants to broader populations, but generalizing across policy issues is also critical (Findley et al., 2021; Linden & Hönekopp, 2021; Yarkoni, 2020). Without broader sampling of policy issues, research may produce inconsistent findings due to issue-specific effects (Clifford et al., 2023; Tappin, 2023).

Concluding on the topic of generalizability, we note that our study used one "type" of policy information treatment. Future work should examine whether different formats or levels of comprehensiveness similarly attenuate cue influence on public opinion.

ACKNOWLEDGMENTS
None.

DATA AVAILABILITY STATEMENT
Data and analysis scripts are available on the OSF (https://osf.io/374sf/?view_only=6f33d b518a5a45d7925a47e17b141b89). Study materials are available in the Appendix. The studies described in this paper were not preregistered.

REFERENCES
Achen, C. H., & Bartels, L. M. (2017). *Democracy for realists: Why elections do not produce responsive government*. Princeton University Press.
Agadjanian, A. (2020). When do partisans stop following the leader? *Political Communication, 38,* 351–369. https://doi.org/10.1080/10584609.2020.1772418
Anduiza, E., Gallego, A., & Muñoz, J. (2013). Turning a blind eye: Experimental evidence of partisan bias in attitudes toward corruption. *Comparative Political Studies, 46*(12), 1664–1692. https://doi.org/10.1177/0010414013489081
Bakker, B. N., & Lelkes, Y. (2018). Selling ourselves short? How abbreviated measures of personality change the way we think about personality and politics. *Journal of Politics, 80*(4), 1311–1325. https://doi.org/10.1086/698928
Bakker, B. N., Lelkes, Y., & Malka, A. (2020). Understanding partisan cue receptivity: Tests of predictions from the bounded rationality and expressive utility perspectives. *Journal of Politics, 82*(3), 1061–1077. https://doi.org/10.1086/707616
Barber, M., & Pope, J. C. (2019). Does party Trump ideology? Disentangling party and ideology in America. *American Political Science Review, 113*(1), 38–54. https://doi.org/10.1017/S0003055418000795
Boudreau, C., & MacKenzie, S. A. (2014). Informing the electorate? How party cues and policy information affect public opinion about initiatives. *American Journal of Political Science, 58*(1), 48–62. https://doi.org/10.1111/ajps.12054
Bullock, J. G. (2011). Elite influence on public opinion in an informed electorate. *American Political Science Review, 105*(3), 496–515. https://doi.org/10.1017/S0003055411000165

Bullock, J. G. (2020). Party cues. In *The Oxford handbook of electoral persuasion*. Oxford University Press. https://www.oxfordhandbooks.com/view/10.1093/oxfordhb/9780190860806.001.0001/oxfordhb-9780190860806-e-2

Bürkner, P.-C. (2017). brms: An R package for Bayesian multilevel models using Stan. *Journal of Statistical Software, 80*(1), 1. https://doi.org/10.18637/jss.v080.i01

Cacioppo, J. T., & Petty, R. E. (1982). The need for cognition. *Journal of Personality and Social Psychology, 42*(1), 116–131. https://doi.org/10.1037/0022-3514.42.1.116

Carmines, E. G., & Stimson, J. A. (1980). The two faces of issue voting. *American Political Science Review, 74*(1), 78–91. https://doi.org/10.2307/1955648

Ciuk, D. J., & Yost, B. A. (2016). The effects of issue salience, elite influence, and policy content on public opinion. *Political Communication, 33*(2), 328–345. https://doi.org/10.1080/10584609.2015.1017629

Clifford, S., Leeper, T. J., & Rainey, C. (2023). Generalizing survey experiments using topic sampling: An application to party cues. *Political Behavior, 46*, 1233–1256. https://doi.org/10.1007/s11109-023-09870-1

Cohen, G. L. (2003). Party over policy: The dominating impact of group influence on political beliefs. *Journal of Personality and Social Psychology, 85*(5), 808–822. https://doi.org/10.1037/0022-3514.85.5.808

Coppock, A. (2019). Generalizing from survey experiments conducted on mechanical Turk: A replication approach. *Political Science Research and Methods, 7*(3), 613–628. https://doi.org/10.1017/psrm.2018.10

Coppock, A., Gerber, A. S., Green, D. P., & Kern, H. L. (2017). Combining double sampling and bounds to address nonignorable missing outcomes in randomized experiments. *Political Analysis, 25*(2), 188–206. https://doi.org/10.1017/pan.2016.6

Coppock, A., & McClellan, O. (2019). Validating the demographic, political, psychological, and experimental results obtained from a new source of online survey respondents. *Research & Politics, 6*(1), 2053168018822174. https://doi.org/10.1177/2053168018822174

Delli Carpini, M. X., & Keeter, S. (1996). *What Americans know about politics and why it matters*. Yale University Press.

Dias, N., & Lelkes, Y. (2022). The nature of affective polarization: Disentangling policy disagreement from partisan identity. *American Journal of Political Science, 66*(3), 775–790. https://doi.org/10.1111/ajps.12628

Downs, A. (1957). An economic theory of political action in a democracy. *Journal of Political Economy, 65*(2), 135–150. https://doi.org/10.1086/257897

Druckman, J. N. (2001). Using credible advice to overcome framing effects. *Journal of Law, Economics, and Organization, 17*(1), 62–82. https://doi.org/10.1093/jleo/17.1.62

Ehret, P. J., Van Boven, L., & Sherman, D. K. (2018). Partisan barriers to bipartisanship: Understanding climate policy polarization. *Social Psychological and Personality Science, 9*(3), 308–318. https://doi.org/10.1177/1948550618758709

Findley, M. G., Kikuta, K., & Denly, M. (2021). External validity. *Annual Review of Political Science, 24*(1), 365–393. https://doi.org/10.1146/annurev-polisci-041719-102556

Gadarian, S. K., Goodman, S. W., & Pepinsky, T. (2021). Partisan endorsement experiments do not affect mass opinion on COVID-19. *Journal of Elections, Public Opinion and Parties, 31*(sup1), 122–131. https://doi.org/10.1080/17457289.2021.1924727

Gelman, A., & Hill, J. (2006). *Data analysis using regression and multilevel/hierarchical models*. Cambridge University Press.

Groenendyk, E. (2013). *Competing motives in the partisan mind: How loyalty and responsiveness shape party identification and democracy*. OUP USA.

Guisinger, A., & Saunders, E. N. (2017). Mapping the boundaries of elite cues: How elites shape mass opinion across international issues. *International Studies Quarterly, 61*(2), 425–441. https://doi.org/10.1093/isq/sqx022

Hobolt, S. B. (2006). How parties affect vote choice in European integration referendums. *Party Politics, 12*(5), 623–647. https://doi.org/10.1177/1354068806066791

Kahan, D. M. (2016). The politically motivated reasoning paradigm, part 1: What politically motivated reasoning is and how to measure it. In *Emerging trends in the social and behavioral sciences*. John Wiley & Sons, Inc. (pp. 1–16). https://doi.org/10.1002/9781118900772.etrds0417

Kam, C. D. (2005). Who toes the party line? Cues, values, and individual differences. *Political Behavior, 27*(2), 163–182. https://doi.org/10.1007/s11109-005-1764-y

Kinder, D. R., & Kalmoe, N. P. (2017). *Neither liberal nor conservative: Ideological innocence in the American public*. University of Chicago Press.

Lau, R. R., & Redlawsk, D. P. (2001). Advantages and disadvantages of cognitive heuristics in political decision making. *American Journal of Political Science, 45*(4), 951–971. https://doi.org/10.2307/2669334

Leeper, T. J., & Slothuus, R. (2014). Political parties, motivated reasoning, and public opinion formation. *Political Psychology, 35*(S1), 129–156. https://doi.org/10.1111/pops.12164

Lerner, J. S., & Tetlock, P. E. (1999). Accounting for the effects of accountability. *Psychological Bulletin, 125*(2), 255–275. https://doi.org/10.1037/0033-2909.125.2.255

Linden, A. H., & Hönekopp, J. (2021). Heterogeneity of research results: A new perspective from which to assess and promote Progress in psychological science. *Perspectives on Psychological Science, 16*(2), 358–376. https://doi.org/10.1177/1745691620964193

Lupia, A. (1994). Shortcuts versus encyclopedias: Information and voting behavior in California insurance reform elections. *American Political Science Review, 88*(1), 63–76. https://doi.org/10.2307/2944882

Lupia, A., & McCubbins, M. D. (1998). *The democratic dilemma: Can citizens learn what they need to know?* Cambridge University Press.

McElreath, R. (2020). *Statistical rethinking: A Bayesian course with examples in R and STAN*. CRC Press.

Mérola, V., & Hitt, M. P. (2016). Numeracy and the persuasive effect of policy information and party cues. *Public Opinion Quarterly, 80*(2), 554–562. https://doi.org/10.1093/poq/nfv051

Mondak, J. J. (1993). Public opinion and heuristic processing of source cues. *Political Behavior, 15*(2), 167–192.

Mullinix, K. J., Leeper, T. J., Druckman, J. N., & Freese, J. (2015). The generalizability of survey experiments. *Journal of Experimental Political Science, 2*(2), 109–138. https://doi.org/10.1017/XPS.2015.19

Nicholson, S. P. (2011). Dominating cues and the limits of elite influence. *Journal of Politics, 73*(4), 1165–1177. https://doi.org/10.1017/S002238161100082X

Nicholson, S. P. (2012). Polarizing Cues. *American Journal of Political Science, 56*(1), 52–66. https://doi.org/10.1111/j.1540-5907.2011.00541.x

Petersen, M. B., Skov, M., Serritzlew, S., & Ramsøy, T. (2013). Motivated reasoning and political parties: Evidence for increased processing in the face of party cues. *Political Behavior, 35*(4), 831–854. https://doi.org/10.1007/s11109-012-9213-1

Peterson, E. (2019). The scope of partisan influence on policy opinion. *Political Psychology, 40*(2), 335–353. https://doi.org/10.1111/pops.12495

Prior, M. (2018). *Hooked: How politics captures people's interest*. Cambridge University Press.

R Core Team. (2018). *R: A language and environment for statistical computing.* R Foundation for Statistical Computing. https://www.R-project.org/

Slothuus, R. (2010). When can political parties Lead public opinion? Evidence from a natural experiment. *Political Communication, 27*(2), 158–177. https://doi.org/10.1080/10584601003709381

Slothuus, R. (2016). Assessing the influence of political parties on public opinion: The challenge from pretreatment effects. *Political Communication, 33*(2), 302–327. https://doi.org/10.1080/10584609.2015.1052892

Slothuus, R., & de Vreese, C. H. (2010). Political parties, motivated reasoning, and issue framing effects. *Journal of Politics, 72*(3), 630–645. https://doi.org/10.1017/S002238161000006X

Sniderman, P. M., Brody, R. A., & Tetlock, P. E. (1993). *Reasoning and choice: Explorations in political psychology.* Cambridge University Press.

Tappin, B. M. (2023). Estimating the between-issue variation in party elite cue effects. *Public Opinion Quarterly. 86*(4), 862–885. https://doi.org/10.1093/poq/nfac052

Tappin, B. M., Berinsky, A. J., & Rand, D. G. (2023). Partisans' receptivity to persuasive messaging is undiminished by countervailing party leader cues. *Nature Human Behaviour, 7,* 568–582. https://doi.org/10.1038/s41562-023-01551-7

Tappin, B. M., & Hewitt, L. B. (2021). Estimating the persistence of party cue influence in a panel survey experiment. *Journal of Experimental Political Science, 10*(1), 50–61. https://doi.org/10.1017/XPS.2021.22

Toff, B., & Suhay, E. (2019). Partisan conformity, social identity, and the formation of policy preferences. *International Journal of Public Opinion Research, 31*(2), 349–367. https://doi.org/10.1093/ijpor/edy014

Van Boven, L., Ehret, P. J., & Sherman, D. K. (2018). Psychological barriers to bipartisan public support for climate policy. *Perspectives on Psychological Science, 13*(4), 492–507. https://doi.org/10.1177/1745691617748966

Wickham, H. (2017). *tidyverse: Easily Install and Load the 'Tidyverse'.* https://CRAN.R-project.org/package=tidyverse

Williams, D. (2020). Socially adaptive belief. *Mind & Language, 36,* 333–354. https://doi.org/10.1111/mila.12294

Yarkoni, T. (2020). The generalizability crisis. *Behavioral and Brain Sciences, 45,* e1. https://doi.org/10.31234/osf.io/jqw35

SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

How to cite this article: Tappin, B. M., & McKay, R. T. (2026). Estimating the causal effects of cognitive effort and policy information on party cue influence. *Political Psychology*, *47*, e70116. https://doi.org/10.1111/pops.70116