Restoring Trust in Public Institutions*

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Trust in public institutions is essential for fostering compliance with behaviors that mitigate societal challenges and reduce negative externalities. However, in the U.S. and other Western nations, such trust has steadily declined, often along ideological lines. While prior research has examined the causes and consequences of this erosion, effective strategies to rebuild institutional trust remain underexplored. This study hypothesizes that diminished trust stems partly from behavioral inattention, where citizens rely on ideological heuristics rather than engaging with institution-sourced information that could inform their decisions. Using two experiments conducted during the 2020 and 2024 U.S. elections, we demonstrate that lowering search costs for valuable institution-sourced information significantly improves factual knowledge, strengthens institutional trust, and promotes compliance with recommended policies and behaviors. Importantly, these effects persist over time and reflect deliberate belief updates, as revealed by meta-cognition measures. Our findings underscore the potential of well-designed educational campaigns to counteract biased perceptions, restore trust, and encourage pro-social behaviors.

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1 Introduction

Trust in public institutions is fundamental to economic stability and social cohesion in liberal democracies[1]. Since the time of Adam Smith, scholars have emphasized that when citizens view institutions as competent and trustworthy, they are more likely to comply with recommended behaviors and believe that others will do the same, creating a virtuous cycle that enhances social welfare (Levi and Stoker, 2000; Tyler, 1990; Levi, 1988; Smith, 1776, 1759)[2]. However, trust in public institutions has been eroding across much of the Western world, with particularly steep declines in the U.S. In 2024, only 37% of Americans reported having trust in public institutions, compared to 70% in 1972. Moreover, this decline has been accompanied by growing polarization, with trust in many institutions increasingly divided along political ideological lines (Gallup, 2024).

Previous studies have shown that institutional trust can quickly erode when citizens encounter information—whether accurate or not—that questions institutional integrity or highlights poor performance. This loss of trust diminishes citizens' willingness to engage in behaviors with significant societal implications, such as adhering to vaccination schedules (Darden and Macis, 2024; Martinez-Bravo and Stegmann, 2022; Alsan and Wanamaker, 2018). However, there is limited understanding of effective strategies to rebuild trust or the mechanisms through which these interventions might operate. This study fills this gap by providing novel causal evidence on the behavioral drivers of institutional trust polarization and evaluating scalable interventions aimed at restoring trust.

We argue that citizens may sometimes lack familiarity with the work of public institutions and how it benefits them. While they could seek information to improve the accuracy of their knowledge and update their trust beliefs, they may be unwilling to incur the search and time costs if they do not perceive an immediate benefit. In the absence of belief-correcting information, citizens thus rely on politically motivated heuristics, leading to biased judgments about institutional trustworthiness. This can create a self-reinforcing cycle in which individuals avoid engaging with valuable institution-provided information — such as updates on public health, environmental risks, or product safety — further entrenching their mistrust. This cycle perpetuates low trust in institutions and reduces their willingness to comply with recommendations or support policies that could improve collective welfare.

We test this hypothesis through two experimental studies grounded in a theoretical model of behavioral inattention, aimed at evaluating scalable interventions to rebuild institu-

[1] In this study, we refer to "public institutions" and "government" as public sector agencies at all levels, from local to state, and federal, that are staffed with public servants designing and implementing public policies and programs. We don't investigate trust in government intended as the branches of the government that are led by publicly elected politicians, such as Congress or the White House.

[2] In "The Wealth of Nations", Adam Smith emphasized the importance of "*confidence in the justice of government*" as fundamental for societal progress, encouraging individuals to "*trust their property to the protection of government [officials]*". In his "Theory of Moral Sentiments", Smith further argued that advanced economies thrive when individuals can pursue their "*interests and inclination*," safeguarded by "*the authority of law*" rather than traditional familial protections.

tional trust. By experimentally varying search costs and the relevance of information, we assess whether these interventions can increase engagement with institutional information, leading to a positive cycle of greater attention, improved factual knowledge, higher trust in institutions, and greater willingness to follow institutional guidance and support their policies. We conduct two experiments with representative samples of Americans from the General Social Survey (GSS) panel, during periods of intense political polarization surrounding the U.S. presidential elections in June 2020 and August 2024. These periods, when criticisms of government agencies were central to political discourse, provide a unique opportunity to evaluate the effectiveness of these interventions in a climate of heightened ideological division and skepticism toward public institutions.

In the first study, we conducted a seven-wave longitudinal experimental survey from April to October 2020 to track Americans' perceptions of the COVID-19 crisis, their personal experiences, and their trust in public institutions. We observed a rapid decline in trust, particularly in institutions managing the pandemic, such as the scientific community and the healthcare sector. This decline was more strongly correlated with misperceptions about the crisis's severity and consumption of partisan media than with personal negative experiences. In the sixth wave, we assessed respondents' factual knowledge by asking them to estimate the number of COVID-19 deaths in their state and nationwide— clear, verifiable metrics critical for preventive health decisions impacting their well-being. To reduce information search costs, we provided half of the respondents with an optional link to the CDC's online tracker, without incentives. This non-mandatory intervention significantly improved all outcomes along the causal chain. Four months after exposure to the link, treated respondents were more likely to display accurate knowledge on an obfuscated follow-up question (56% compared to 52% in the control group), reported higher trust in the CDC (18% versus 12% in the control group), were less likely to view the agency as politically influenced (14% versus 8%), and were more likely to follow CDC-recommended preventive measures, such as getting tested for COVID-19 (36% compared to 28%). Additionally, a novel set of meta-cognitive questions revealed that the long-term improvements in trust reflected a conscious belief shift.

We conducted a second experimental study during the 2024 Presidential election, building on the design and timing of our first study to explore additional mechanisms informed by our theoretical model. This study focused on trust in the Environmental Protection Agency (EPA), which faced heightened scrutiny and politically charged attacks during the election. Using a sample from the General Social Survey (GSS), we replicated key elements of our earlier approach while introducing novel dimensions to test the perceived value of institutional information.

In a three-arm survey experiment, two-thirds of respondents were tasked with a factual knowledge question about current air quality in their zip code, as measured by the EPA—a metric directly relevant to personal health decisions. Half of this group received an optional link to the EPA's online air quality tracker, reducing search costs without forcing reading it nor offering financial or social incentives to access it. A third group received a different factual question about the location of the EPA regional office serving their state, accompanied by a similar optional link to verify their answer. While both groups faced comparable low search costs, the perceived value of the information provided was deliberately varied, allowing us to disentangle whether trust improvements stemmed from simple institutional exposure or from the relevance of the information offered.

In this second experiment, we find that low baseline trust in the EPA was strongly correlated with limited awareness of its work. Further, consistent with the first study, reducing search costs for institution-sourced information significantly improved factual knowledge: 49% of respondents in the low-search-cost groups answered correctly, compared to 25% in the control group. Importantly, the perceived relevance of the information critically influenced trust outcomes. Respondents who engaged with air quality data—valuable for making informed health decisions—were more likely to believe the EPA positively impacted their lives and was free from political influence compared to those who accessed lower-value information, such as office locations. Furthermore, these respondents expressed stronger support for the EPA's role in assessing and maintaining environmental quality in their own communities.

Taken together, these findings suggest that much of the polarization in trust in public institutions may stem from behavioral inattention. Many individuals are unwilling to incur the search costs required to access valuable, institution-sourced information that could help them make informed decisions for their well-being and that of their communities. Instead, they often rely on ideological shortcuts to form judgments about institutional trust (Bénabou and Tirole, 2011). Our study is the first to establish this causal chain and provide experimental evidence on the effectiveness of scalable, low-cost digital interventions for improving institutional trust. These results underscore the importance of government information campaigns that prioritize delivering timely and relevant information over traditional marketing initiatives focused solely on increasing institutional visibility. By equipping citizens with actionable, context-specific information during critical moments, such campaigns can simultaneously enhance trust and encourage compliance with behaviors and policies that promote societal welfare.

Related studies. Our study contributes to several streams of literature. First, we add to the economics literature on trust, which predominantly focuses on interpersonal trust as a driver of economic prosperity (Sapienza et al., 2013; Guiso et al., 2011; Fehr et al., 2002). In contrast, trust in public institutions has received comparatively less attention. Limited applied studies show that institutional trust can deteriorate rapidly in response to information undermining integrity or performance, reducing citizens' willingness to comply with welfare-enhancing behaviors, such as vaccination schedules (Rafkin et al., 2021; Darden and Macis, 2024; Alsan and Wanamaker, 2018). However, there is little evidence on effective strategies to restore or foster institutional trust.

One reason for this gap is the inherent difficulty of influencing trust through exogenous interventions, particularly in real-world settings (Durlauf, 2002). A notable contribution in this area is Acemoglu et al. (2020), who used a lab-in-the-field experiment in Pakistan to examine trust in the judiciary following efficiency reforms. Participants were randomly provided with information highlighting the reforms' positive outcomes, offering valuable insights into the potential of favorable information to build trust. However, the study's external validity may be limited, as individuals in real-world settings typically decide whether to engage with institutional information, unlike the controlled exposure used in the experiment. Additionally, the lack of long-term follow-ups makes it challenging to fully disentangle the effects of the intervention from potential social desirability bias.

Our study advances this literature by demonstrating that low institutional trust often stems from poor understanding of an institution's work. We show that reducing search costs and providing relevant information can significantly restore trust. Importantly, we provide evidence that this effect is sustainable, robust to risks of social desirability bias, and reflects conscious shifts in perception, as measured by novel meta-cognition survey questions.

Second, we contribute to political science research on institutional trust. Foundational theories suggest that trust hinges on perceptions of competence, integrity, and legitimacy (Levi, 1988; Tyler, 1990). While case studies have documented that limited interactions with public services shape trust, empirical evidence on interventions that address biased judgments about institutions is scarce (Grimmelikhuijsen and Meijer, 2014; Van de Walle and Bouckaert, 2003). By experimentally varying access to relevant information, our study provides causal evidence on how institutional communication can enhance trust, particularly when citizens perceive the information as directly beneficial.

Third, we build on research examining government communication campaigns designed to influence pro-social behavior. For instance, Banerjee et al. (2024) demonstrated that SMS messages effectively improved compliance with COVID-19 preventive measures. However, other studies report mixed outcomes, with success often contingent on contextual factors and the perceived relevance of the information (Carson-Chahhoud et al., 2017; Ravallion et al., 2015). Our contribution lies in two areas: (1) introducing a behavioral inattention framework to reconcile mixed findings, and (2) demonstrating that outreach does not require costly platforms. Instead, effective campaigns should not merely increase exposure to institutions but provide citizens with actionable and contextually valuable information.

Finally, we contribute to the literature on behavioral inattention, which explores how limited attention leads individuals to overlook valuable information, resulting in suboptimal decisions (Gabaix, 2019; DellaVigna, 2009). Our findings reveal that politically polarized views on public institutions can be partly attributed to citizens not seeking out and internalizing institution-sourced information, and thus relying on ideological heuristics to form biased perceptions of public institutions.

The remainder of the paper is structured as follows: Section 2 presents the behavioral inattention model. Section 3 details the methodology and findings of our first experiment, while Section 4 presents the second experiment. Section 5 discusses the broader implications of our findings and concludes.

2 Motivating Framework: Behavioral Inattention and Institutional Trust

In this section, we introduce a simple model that informs our experiments' design and interpretation. The model integrates insights from two distinct strands of literature: a qualitative framework from political science and recent developments in structural behavioral economics. Specifically, we build on models of behavioral inattention (Gabaix, 2019) and formalize the interactions between public institutions and citizens as a signaling game.

Institutions. In our proposed framework, institutions are represented as the canonical benevolent social planner, whose primary goal is to maximize citizens' well-being. Achieving this goal depends on citizens' willingness to adhere to institutional advice to (i) adopt recommended behaviors (e.g., preventive health measures) that maximize their utility and (ii) mitigate the negative externalities associated with non-compliance. Trust plays a pivotal role in this dynamic.

Our framework builds on the definitions of "institutional trust" proposed by Braithwaite and Levi (1998) and Tyler (1990). We conceptualize trust as a judgment that may be binary or continuous, reflecting citizens' beliefs about the trustworthiness of institutions. These trust judgments are expected to shape behavior, with higher levels of institutional trust likely increasing individuals' willingness to comply with recommended and cooperative actions. In our context, we posit that public institutions can build trust by signaling their competence (e.g., the utility and complexity of their work) and integrity to citizens. These signals are designed to achieve two key objectives: (i) ensuring compliance with institutional recommendations, and (ii) fostering greater trust among citizens.

Individuals. On the other side of the signaling process are citizens, who act as behaviorally inattentive agents. These individuals have limited attention and incur costs when searching for or processing information. As a result, they tend to focus on a few salient factors rather than considering all available information. Following the model proposed by Gabaix (2014) and summarized in Gabaix (2019)[3], an individual has to determine which action a maximizes their utility, depending on a set of I attributes $x = (x_1^1, x_2^1, ..., x_i^j, ..., x_I^J)$ informed by J institutions. Each institution j can provide one or more attributes, but for simplicity, let's denote each attribute with a unique number i. The agent has an "attention-augmented decision" utility function, with $m = (m_1, m_2, ..., m_N)$ determining the degree of attention paid to each attribute. $m \in [0, 1]$, with $m = 0$ if no attention is paid and the information is entirely discarded and $m = 1$ if the opposite is true.

$$\max_a u(a, x, m) \tag{1}$$

For instance, consider a parent who derives positive utility from playing outdoors

[3]Note that here we are adopting a simplified version of Gabaix's model 2019 to map it onto our research question and experimental design.

with their children. To decide how long to engage in this activity (action a), the parent relies on a set of cues (x): the weather forecast, air quality, the availability of a nearby park, and so on. These cues are provided by various institutions j (e.g., the Environmental Protection Agency, which calculates the Air Quality Index (AQI)).

However, paying attention is costly. This cost rises linearly with the amount of attention m_i devoted to a cue and is further adjusted by a search cost parameter $c_i > 0$, which depends on the specific piece of information being considered. Additionally, the cognitive penalty parameter k, which is determined at the individual level and remains constant across attributes, captures the overall costliness of paying attention. For perfectly rational agents (where attention is costless), $k = 0$, while for others, $k > 0$, reflecting the cognitive burden of attention. Therefore, the cost function of paying attention to a set of attributes x can be expressed as:

$$\mathcal{C}(m) = k \sum_{i \in I} c_i m_i \tag{2}$$

Given that attention is endogenously determined, the agent first has to decide how much attention to allocate to each attribute, solving the following problem:

$$\max_{m} \mathbb{E}\left[u\big(a(x, m), x\big)\right] - \mathcal{C}(m) \tag{3}$$

This equation highlights the trade-off between the benefits of paying attention (higher expected utility from making informed decisions) and the costs of attention (time spent gathering and consuming information and the cost of internalizing it). Hence, agents will only pay attention to attributes that are useful and not too costly to find.

The optimal attention parameter m_i^* is influenced by the information's importance, the cost of paying attention, and the impact of the action of their utility, and it is given by:

$$m_i^* = A\left(\frac{\sigma_i^2 |a_{x_i} u_{aa} a_{x_i}|}{c_i \kappa}\right) \tag{4}$$

where A is the attention function[4]. As in most models of behavioral inattention, we assume that the attention function is both sparse and continuous, capturing the realistic aspect of people ignoring unimportant details while still allowing for smooth adjustments in attention as the relevance of information changes.

[4]In the original model, the function A can be sparse or continuous. In the first case, if a variable is not very important (i.e., has low variance or little impact on the action of the decision-maker), the individual will allocate zero attention to it. In the second case, small changes in the importance of the received information lead to small changes in attention allocation.

The term σ_i^2 captures the variance of the piece of information i, representing its relative importance. In our context, this reflects the degree of variability associated with the information shared by the institution. Continuing the previous example, think of a person who lives in an area where the quality of the air has been consistently good: the variance of this information is low and so citizens may have a low demand for knowing the air quality index in their area regularly, whereas if the air quality score fluctuates wildly, its unpredictability will pose a greater health risk, thus a citizen may want to pay more attention to this information. The opposite may be true as well: citizens may develop a tolerance for bad news if, for instance, the quality of the air where they live is consistently low. The term a_{x_i} indicates the degree to which a change x_i affects the decision of an action. Following our example, the agent's choice to play outdoors with their children might not be affected significantly by the price of electricity (which will be used to wash their clothes afterward), but it might respond to the air quality as if the air is polluted, the agent prefers not to expose their children to the particles. The term u_{aa}, instead, reflects the sensitivity of the agent's utility to the action (i.e., to what extent they will regret taking their children out to play in case it was better not to do so). u_{aa} captures the idea that people are willing to spend cognitive resources (pay attention) to determine the best action only if they care about the outcomes of the action.

In summary, an individual will pay more attention to information attribute x_i if it is more variable (high variance σ_i), if it influences more the choice of action (high a_{x_i}), if an imperfect action leads to significant losses (high u_{aa}), and if the cognitive cost of consuming the information is low (low $c_i\kappa$). Trust can generate a virtuous cycle of information consumption: indeed, if individuals trust an institution, they will (i) face lower costs when consuming the information it produces (lower c_i^j $\forall i$ produced by institution j), and (ii) respond more to the information consumed (higher a_{x_i}). Both aspects will increase the attention m_i^* paid to the attribute x_i, maximizing the efficacy of eventual future outreach campaigns.

After having determined the optimal level of attention m^*, the agent finds the action that maximizes their utility:

$$a^* = \text{argmax}_a u(a, x, m^*) \tag{5}$$

We define institutional trust in terms of the ability an institution has to provide helpful information. An agent will trust an institution j if its work produces attributes $x^j = (x_k^j, ..., x_{I_j}^j)$ that help the agent make better decisions. Hence, trust arises if the individual is better off paying attention to at least one of the attributes produced by the institution. This can be expressed as:

$$\exists i \in (1, ..., I_j) \mid u(a, m, x | m_i^j = 1) > u(a, m, x | m_i^j = 0) \; \forall \; i = (1, ..., I_j) \tag{6}$$

Expanding on the example above, we propose that trust in the EPA increases when parents perceive its information as helpful in deciding whether it is safe to take their children outside. For instance, if the Air Quality Index (AQI) is viewed as valuable, parents are more likely to rely on this resource, fostering greater trust in the EPA.

In the first intervention, we test whether lowering the cost c_i of accessing an attribute x_i produced by an institution j increases the likelihood that some attention will be paid to this cue ($m_i > 0$) and whether this leads respondents to improve their knowledge of the attribute. Secondly, we evaluate whether this increases (i) their trust in the institution j providing the information and (ii) their likelihood to comply with the related recommendations from the institution. In the second experiment, we test whether the quality of the attribute provided by institutions matters. We lower the search costs c_z and c_k of accessing two attributes: a high-value one, x_z, and a low-value one, x_k, produced by the same institution l. While we expect respondents to be more likely to access both, given that the search costs are lower, we hypothesize that their effects on trust will be significantly different: the high-value information should increase trust, while the second one should not, given that the expected utility derived from consuming the low-value information is not larger than one obtained not consuming the information (i.e., with $m_k = 0$):

$$u(a, m, x|m_k^*) = u(a, m, x|m_k = 0) \tag{7}$$

In conclusion, following our model, in order to improve citizens' trust, institutions should provide information that is (i) useful and (ii) easy to access and understand to reduce cognitive costs[5]. This should increase the probability that individuals consider it in their subsequent decision-making processes, with positive effects on institutional trust, which could, in turn, increase the likelihood that individuals will consume information produced by the institution also in the future, generating a virtuous cycle.

3 Experiment I: Reducing the search costs of institution-sourced information

3.1 Data

In our first study, we focus on Americans' trust in the U.S. Centers for Disease Control and Prevention (CDC). During the COVID-19 pandemic, the CDC became a central public institution, guiding the national response with health recommendations and policies. However, its role was frequently debated, with conflicting opinions along political lines regarding its guidance on lockdowns, mask mandates, and vaccines (see selected media extracts in figure OA1 in the Online Appendix).

[5]Indeed, most 'nudge' interventions can be considered light-touch communication interventions that don't restrict nor alter the choice set available to individuals (Thaler and Sunstein, 2021).

We partnered with NORC, the organization that administers the General Social Survey (GSS), using their AmeriSpeak Panel[6]. We believe that integrating our research with the GSS enhances the rigor and credibility of our findings for several reasons. First, it enables direct comparisons with past studies that have relied on this well-established dataset, offering a deeper historical context for understanding changes in public trust. Second, the GSS imposes strict protocols for data collection and outcome traceability, ensuring a higher level of transparency and accountability[7].

We recruited 1,440 U.S. citizens from the GSS AmeriSpeak panel to participate in a seven-wave longitudinal survey (see Table OA1 in the Online Appendix for a summary of demographic and socioeconomic characteristics)[8]. The survey was conducted between April and October 2020. This longitudinal approach allowed us to track shifts in beliefs over time, assess changes in the absence of an intervention (for the control group), and evaluate the long-term effects of our experimental intervention, reducing the risk of social desirability bias. In the specific context of COVID-19, this multi-wave design also enabled us to observe how perceptions evolved in response to key events. This was particularly relevant for the CDC, considering how public trust fluctuated as the agency's recommendations and political discourse around the pandemic unfolded and as citizens' experiences with the pandemic might have shaped their beliefs independently of our intervention.

The first wave of the survey, conducted in early April 2020 as COVID-19 deaths surged across the U.S.[9], gathered baseline data on trust in institutions, media consumption, and beliefs. We also collected information on respondents' personal experiences with the crisis, allowing us to control for the possibility that their beliefs and preferences had already been influenced by unfolding events. The following three weekly waves tracked respondents' lived experiences during the critical first month of the pandemic. The subsequent two waves, conducted during the weeks starting May 18 and June 22, 2020, respectively, measured respondents' perceptions of the severity of the crisis. The sixth wave, administered on June 22, 2020, included the randomized information provision intervention. The final wave was

[6]Funded and operated by NORC at the University of Chicago, AmeriSpeak® is a probability-based multi-client household panel sample designed to be representative of the US household population. Randomly selected US households are sampled using area probability and address-based sampling, with a known, non-zero probability of selection from the NORC National Sample Frame. These selected households are then contacted by US mail, telephone, and field interviewers (face-to-face). The panel provides sample coverage of approximately 97% of the U.S. household population. Those excluded from the sample include people with P.O. Box only addresses, some addresses not listed in the USPS Delivery Sequence File, and some addresses in newly constructed dwellings. While most AmeriSpeak households participate in surveys by web, non-internet households can participate in AmeriSpeak surveys by telephone. Households without conventional internet access but with web access via smartphones are allowed to participate in AmeriSpeak surveys by web.

[7]This differs from platforms like Prolific or Lucid, where repeated iterations of experiments are more common. The GSS's structured framework demands greater rigor and reliability in our results and doesn't allow for multiple pilots or versions of an experiment, eliminating any risk of p-hacking or results-fishing

[8]This was part of a larger study on COVID-19, which also collected data on respondents' mental health and well-being, topics unrelated to this study, and were intentionally gathered in a way that did not interfere with our experimental design (Bertrand et al., 2020)

[9]See Figure OA2 in the Online Appendix to compare the Covid-19 death rate evolution with our survey waves

conducted during the week starting October 19, 2020, specifically timed to capture any changes in beliefs or preferences four months after the intervention and just before the presidential election.

We chose to conduct the main experimental intervention during the sixth wave for several reasons. First, we aimed to reduce the influence of surrounding events, opting for a period when COVID-19 cases and deaths were declining. We also spaced this wave further from the previous ones to minimize the likelihood of recall bias or preferences for consistency on key variables such as crisis-related experiences. This period coincided with the temporary lifting of some pandemic restrictions, further reducing the risk that views about the work done by the CDC were influenced by a recently introduced restriction. Additionally, by this time, the two major presidential nominees were essentially confirmed, and the election campaign was gaining momentum. By this point, most Americans were familiar with the CDC, had likely formed opinions on its role in managing the pandemic and its recommendations, had direct or indirect experiences with the COVID-19 virus that would have shaped their beliefs, and most of them were certain on their vote at the upcoming elections.

3.2 Pre-experimental findings

We begin by reporting the descriptive results from the first five waves of our longitudinal survey, which served as the motivation for our experiment in the sixth wave. The first notable finding is the rapid erosion of trust in public health institutions—specifically hospitals, healthcare professionals, and the scientific community. A similar trend was observed for health insurance companies. This decline in trust occurred over just four weeks, which our repeated survey waves allowed us to track closely. We see that this erosion persisted, and trust didn't return to pre-pandemic levels by the time we completed our last survey wave (see Figure OA3 in the Online Appendix).

The second key result, presented in Table OA2 in the Online Appendix, is that there is a strong correlation between trust in healthcare institutions and the adoption of preventive behaviors, both in levels and in trends. Respondents who reported high levels of trust in health institutions were more likely to follow their guidelines, such as handwashing, respecting physical distancing, and wearing masks. Further, those who increased their confidence between the first and the last week of April were also more likely to increase the number of preventive behaviors adopted over a similar time span. We do not observe this correlation with trust in the government (the White House and the Federal Government). These patterns align with previous quasi-experimental studies showing how declining trust in institutions can lead to suboptimal health behaviors and outcomes (Martinez-Bravo and Stegmann, 2019).

Third, we explored the most important correlates of trust erosion and found that media consumption and personal experiences predict variations in trust. Exposure to slanted news increased polarization along party lines, while negative personal shocks — such as losing a significant portion of income or knowing someone hospitalized with COVID-19 — caused people to lose trust in the government, though not in public health institutions (see Table OA3 in the Appendix). In line with the literature (Bolsen and Druckman, 2018; McLaughlin et al., 2021; Van Scoy et al., 2021; Clark et al., 2023), we also detect a strong correlation between perceived politicization and lack of trust: respondents who believed that an institution was affected by politics displayed lower confidence in the institution compared to the first survey wave. They were also more likely to report having decreased their trust in that institution, as reported in Table OA4 in the Online Appendix.

Additionally, respondents who consumed media with opposing political biases displayed stark differences in their interpretation of the ongoing events. For instance, when asked how many additional COVID-19 deaths they expected by the end of the year, those consuming left-leaning news projected a figure twice as high as those consuming right-leaning news (see Figure OA4 and Table OA5 in the Online Appendix).

Overall, our findings highlight a rapid decline in trust, particularly along political lines, which was further exacerbated by the consumption of slanted media that distorted perceptions of the crisis's severity. This erosion of trust was linked to non-compliance with recommended behaviors that carried significant negative externalities. These insights motivated our research question: Can scalable interventions effectively improve individuals' understanding of the crisis's gravity, rebuild trust in institutions, and subsequently increase their willingness to follow public health recommendations?

3.3 Methodology: a randomized intervention

The core hypothesis of this study is that mistrust in public institutions may stem from a lack of knowledge or misunderstanding — and underappreciation — of the work they do. Drawing on political science frameworks of institutional trust, which suggest that institutional trust requires clear signals of trustworthiness, such as procedural transparency (see Braithwaite and Levi (1998)), we posit that in the absence of such signals, individuals may form trust beliefs based on ideological cues (in the second experimental study we provide further evidence of this mechanism). Since acquiring signals of trustworthiness can be costly, our experiment aimed to reduce these search costs and allow individuals to self-select to consume institution-sourced information. In everyday situations, people may avoid seeking information due to time or effort constraints rather than outright mistrust. By lowering these barriers, our survey simulates real-world scenarios where credible information is more accessible, as in public information campaigns or during crises when timely updates are crucial.

To test this, our experiment asked respondents a verifiable question about the COVID-19 death rate — a quantifiable measure of the crisis that might interest them. Half of the respondents were randomly given a hyperlink to a CDC webpage with a regularly updated tracker for this metric. This design allowed participants to voluntarily engage with the institution, giving them a reason to do so — namely, verifying their knowledge — without coercion[10]. Our design aligns with the conceptual framework that trust beliefs are formed

[10]Some studies advocate for incentive-compatible mechanisms to elicit accurate knowledge, often offering

through inferences about an institution's trustworthiness based on its actions and mission (Braithwaite and Levi, 1998). By reducing the cost of accessing accurate information, we replicated a more natural decision-making process, mirroring real-world behaviors outside the survey context. Additionally, visiting an institution's website represents a scalable and replicable intervention[11][12].

Respondents were asked: *"How many people have died in your state because of coronavirus from the first death until today?"*. Response options were: *"Less than 500; Between 500 and 1,000; Between 1,000 and 3,000; Between 3,000 and 5,000; Between 5,000 and 10,000; Between 10,000 and 15,000; Between 15,000 and 30,000; More than 30,000."*. We chose to elicit responses by means of a categorical answer rather than allowing respondents to input a continuous number to make sure they were not simply copy-pasting the value they saw on the CDC webpage, requiring more of their attention.

All respondents were then asked *"How many people have died in the U.S. because of coronavirus from the first death until today?"*. This time, they were prompted to use a slider ranging from 0 to 200,000, with intervals of 20,000 (e.g., 20,000, 40,000, 60,000, etc.). Like the previous question, this forced respondents to choose from predetermined categories rather than inputting a continuous number. Using a slider allowed us to present multiple options more clearly on the screen and helped respondents visualize the full range of possibilities. Additionally, we designed this question to differ from the earlier state-level death estimate, minimizing the risk of respondents anchoring their answers based on previous inputs. Both questions were placed within the first block of ten questions of that survey wave to minimize survey fatigue.

Our randomized intervention consisted of offering half of the respondents — the treatment group — a prompt encouraging them to look up the answer prior to answering: *"If you wish to do so, you can look up the answer on the official CDC website at the following link*: https://www.cdc.gov/coronavirus/2019-ncov/cases-updates/cases-in-us.html".[13]. The

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financial rewards for correct answers. However, we opted against such incentives to maintain the voluntary nature of engagement with the CDC and to avoid ethical concerns, particularly in a sensitive context like the COVID-19 pandemic. Offering monetary rewards could also have contaminated our treatment effects on trust beliefs outcomes by encouraging individuals to visit the site for financial gain rather than to acquire useful information and signals of trustworthiness, further compromising our ability to observe natural trust-building behaviors. See Belot and Briscese (2023) on the importance of examining non-incentivized self-selection into contact.

[11]Although we do not delve into the specific motivations behind why respondents chose to seek out information, which could include curiosity or a desire to verify existing beliefs, robustness checks in the Online Appendix show no significant correlation between the time spent answering the knowledge accuracy question and personal experiences with the crisis or media consumption patterns. This suggests that neither personal fear nor media-induced avoidance were driving forces behind information-seeking behavior. It is also possible that the treatment was most effective among more skeptical respondents (Norris, 2022)

[12]We did not pre-register the analysis plan for this initial experiment due to an oversight in the days leading up to the trial launch. However, for the second study, we pre-registered a detailed analysis plan, and we applied the same procedures, decision rules, and analytical methods to both studies. This alignment strengthens the reliability of our findings from the initial experiment by ensuring they were analyzed in accordance with a pre-specified plan.

[13]See table OA6 in the Online Appendix for the balance tables

link was spelled out to make it clear that the source was the CDC, so respondents who had a distaste for the institution could choose not to click it. Those who did were redirected, through a new browser window, to the CDC page with up-to-date statistics on COVID-19 deaths at the national level, and a large map of the country was shown so they could hover their mouse on their state of residence to see how many people had died in their state. Respondents who clicked the link could then return to the survey browser window to answer the question. We then matched each respondent's answers to the official CDC statistics from the day they completed the survey to assess accuracy. These questions were the only times across the seven survey waves that respondents were asked about the current COVID-19 death rate and the only time treated respondents were exposed to information sourced from the CDC.

Obfuscated follow up. We implemented a seventh and final survey wave four months after the experiment, during the week commencing October 19, 2020. The four-month gap, combined with the proximity to the Presidential elections, created an optimal setting to minimize social desirability bias. The time delay provided sufficient distance from the initial intervention, reducing the likelihood that respondents would consciously align their answers with perceived expectations from researchers. To further mitigate bias, we employed an 'obfuscated follow-up' approach (Haaland et al., 2023), where respondents were asked a different question for which they would not find an answer on the CDC website. Specifically, we tested their knowledge about the international impact of the COVID-19 pandemic by asking: *"Do you believe the current COVID-19 death rate per capita in the U.S. is"*: response options were: *"(1) The highest in the world; (2) Higher than most countries in the world; (3) Lower than most countries in the world; (4) The lowest in the world"*. This design not only minimized priming effects but also introduced stricter conditions for detecting treatment effects, as it required respondents to be sufficiently familiar with the pandemic's impact to make accurate international comparisons[14].

Outcomes. We track the following outcomes: (i) engagement with the treatment; (ii) response accuracy; (iii) trust in public institutions, and (iv) behavioral change. In the survey wave where our experiment was implemented, we only tracked the first two outcomes, and we deliberately avoided asking any questions about trust in public institutions to minimize social desirability bias. Instead, respondents were presented with a set of questions related to the ongoing George Floyd Black Lives Matter movement protests, ensuring that they concluded the survey by focusing on another politically polarizing issue. This design further reduced the likelihood that they would remember the COVID-19 death rate question or the treatment in the follow-up survey wave.

We measure trust in institutions in two ways: with a direct ask and a meta-cognition that prompts respondents to reflect on how their trust beliefs have changed. While other

[14]We can't exclude that treated respondents learned how to search for reliable COVID-19 information online from alternative sources, and answered our question after looking up the answer. Since we didn't track time spent on this question, we can't rule out this mechanism. However, whether the long-term treatment effect is driven by greater critical thinking or a higher ability to seek reliable information is of secondary importance as it nevertheless demonstrated that the treatment had a sustainable effect.

experimental survey studies measure priors and posteriors (before and after treatment), we don't measure the initial levels of trust to avoid anchoring respondents who may have a tendency for consistency. Further, eliciting trust priors may affect the probability of compliance with treatment due to a political identity salience effect, as respondents might choose to discard information coming from a source that they just declared not to trust in a previous question, affecting the treatment uptake[15].

In the last survey wave, four months after exposure to the treatment, we elicited respondents' trust in the CDC. Since we wanted to obtain a clear and identifiable effect on this institution alone, we opted for an alternative method of obfuscated follow-up by eliciting respondents' trust in a set of public institutions that were shown in random order. The question asked: *"Currently, how independent versus politicized do you think the following institutions are"*, with response options being: "(1) Not influenced by politics at all; (2) Not too influenced by politics; (3) Somewhat influenced by politics; (4) Very influenced by politics". The list of institutions, which was shown in random order, included: (1) the scientific community, (2) banks and financial institutions, (3) the private sector, (4) hospitals and healthcare professionals, (5) health insurance companies, (6) the police force, and (7) the Centers for Disease Control and Prevention (CDC). This design and the list of organizations, with the exception of the introduction of the CDC, mimicked the previous GSS surveys and masked the institution of interest among (randomly shown) others to reduce social desirability bias.

We chose to measure institutional trustworthiness through the lens of perceived politicization because we believe that it is a primary determinant of trust in public institutions. Following our framework of institutional trust belief formation, one of the key factors driving trust is the perception that an institution operates with integrity, benevolence, and competence, which require minimal or no political influence, especially in regard to the sharing of important accurate information such as the COVID-19 death rate as in our experiment. When individuals perceive an institution as highly politicized, they may question its objectivity, transparency, and ability to serve the public interest without bias. This can erode trust, as trustworthiness signals — such as procedural transparency and impartiality — become compromised (Braithwaite and Levi, 1998). In the context of our study, the politicization of institutions like the CDC during the COVID-19 pandemic was a particularly salient issue, given that the information they were releasing could have helped citizens make better-informed decisions but could have also been interpreted as an indicator of the Presidents' team performance in managing the crisis. By asking respondents to rate the level of political influence on a variety of institutions, we could capture a key dimension of their trust judgments. We posited that if respondents viewed the CDC as being highly politicized, they would be less likely to trust its recommendations and more likely to question its motives.

Respondents were then asked a set of unrelated questions to minimize preferences for consistency and then saw our second trust beliefs outcome question. This question prompted respondents to reflect on their changes in trust beliefs and asked: *"Thinking about*

[15]This also improves our external validity since citizens choose to consume information coming from a public institution without being reminded of their ideology just before doing so.

your confidence in the institutions before the COVID-19 crisis started and today, to what extent would you say your views have changed", with response options being: "(1) I have a worse opinion than before, (2) my opinion didn't change, (3) I have a better opinion than before". The list of institutions, also shown in random order, was similar but slightly different than the previous question to minimize preferences for consistency again: *(1) the scientific community, (2) banks and financial institutions, (3) the private sector, (4) hospitals and healthcare professionals, (5) health insurance companies, (6) the police force, (7) the Centers for Disease Control and Prevention (CDC), (8) the President of the United States, and (9) the U.S. Senate and Congress.*

This follow-up question is designed to assess respondents' meta-cognition — that is, their awareness of changes in their trust beliefs over time. We consider this an important metric because it signals a more active process of belief updating and suggests a more sustainable treatment effect. Psychological research has shown that resistance to recognizing and revising incorrect beliefs contributes to polarization and radicalization (Rollwage et al., 2018). Conversely, greater meta-cognitive processing — meaning an individual's ability to recognize that their beliefs are evolving — has been linked to a higher likelihood of mitigating selective information processing (Rollwage and Fleming, 2021). Therefore, by tracking respondents' awareness of their belief changes, we can better understand the sustainable effect of trust-building interventions to reduce ideological entrenchment.

Given the national coverage of our sample, we lacked access to independently verifiable administrative data to track respondents' behavioral changes. Thus, we relied on self-reported behavior through direct questions in our survey. In an effort to reduce social desirability bias, rather than measuring forward-looking behavioral intentions, we asked respondents to reflect on past behaviors. In the final wave, we elicited self-reported compliance with CDC-recommended behaviors. The first question asked: *"Did you ever get a swab or antibody test for COVID-19?"* with a simple yes or no response. If respondents answered yes, they were further asked: *"Have you ever tested positive in a COVID-19 test?"*.

Mediators and other relevant covariates. Our key mediator of interest is the media diet, which, we argue, shapes trust in institutions by reinforcing the ideological cues that behaviorally inattentive individuals rely on when assessing institutions they may not be too familiar with. Individuals may receive signals from a public institution that contradict their pre-existing beliefs (also shaped by their media diet), but they may not update their beliefs. In a highly polarized information environment, biased media can severely undermine efforts to rebuild trust in public institutions by filtering out or distorting credible information, limiting individuals' willingness or ability to reassess their trust in these organizations. In section A1.1.2 of the Appendix, we explain how we built a media slant index based on previous similar studies. We find evidence that respondents consuming slanted media have different perceptions and different levels of knowledge of the pandemic, and their institutional trust evolved differently throughout the crisis, underlining the relevance of this aspect (see section 3.2 above).

A second relevant aspect to consider when analyzing trust dynamics is people's experience with the pandemic. Throughout the survey waves, we collected this information and complemented it with administrative data from multiple sources. In particular, we focus on four measures of shock that could act as mediators of our treatment effects - that is, variables that would explain why individuals may choose to consume and internalize the institution-sourced information. These covariates include: (i) direct economic impact, which we constructed as whether respondents have lost more than 20% of their income (combining both incomes from work and other sources) between any two months over the period of February (the pre-shock baseline level) to October 2020, which we asked respondents to report in the last wave of the survey; (ii) indirect economic impact, as measured by the weekly percentage variations in consumer expenditures in their county of residence; (iii) direct health impact - that is, whether respondents had a family member, a friend, an acquaintance, or themselves, who was hospitalized with COVID-19; and (iv) indirect health impact, which is the COVID-19 cases in the respondents' county of residence. In Appendix A1.1.2, we provide a detailed explanation of how we construct these measures.

3.4 Results

3.4.1 Knowledge accuracy

The first insight we observe is that engagement with the knowledge accuracy question was significantly higher in the treatment group compared to the control group, as tracked by a hidden embedded timer[16]. As shown in Table 1, treated respondents spent an additional 14 seconds on average to answer the question (i.e., a 20% increase from a baseline of just under one minute), which may indicate the additional time some respondents spent clicking on the CDC link. Referring back to equation 4, we see that reducing search costs k by providing a link to the CDC website increased respondents' attention m by getting them to spend more time consuming a critical piece of information x with high variability σ^2 - i.e., the COVID-19 death rate, which was arguably the most salient indicator of the gravity of the pandemic - that could inform their action a.

To check for response correctness, we use a stringent outcome measure and flag as correct only responses where both the national and state death rates matched those provided by the CDC. We see that the treatment significantly increased the probability that respondents answered the question correctly by an extra 36% (an extra 12 p.p from a baseline of 33%).

Using this rigorous measure of long-term effects, we find suggestive evidence the treatment had a long-lasting effect in increasing knowledge accuracy. Treated respondents were 10% more likely than the control group (5.3 p.p. higher from a baseline of 54.5%). We then instrument answering correctly in the experiment wave with the treatment status and see that having answered correctly in June increases the likelihood of answering correctly in

[16]At the time of implementation, NORC didn't have the capabilities to track click-through rates, which we instead do in our second experiment.

October by almost 50 percentage points.

Table 1: Treatment effects on knowledge in the short- and long-run.

	(1) Tot time questions	(2) Tot time questions	(3) Correct death estim. Jun	(4) Correct death estim. Jun	(5) ITT Correct US ranking Oct	(6) ITT Correct US ranking Oct	(7) ATT (2 Stage) Correct US ranking Oct
Treated	13.97**	14.57**	0.101***	0.125***	0.0406	0.0539*	
	(5.375)	(5.766)	(0.0324)	(0.0286)	(0.0336)	(0.0305)	
Correct deaths							0.464*
							(0.276)
Constant	54.05***	90.61***	0.290***	0.334**	0.528***	1.039**	0.571
	(4.185)	(33.20)	(0.0217)	(0.143)	(0.0294)	(0.443)	(0.544)
Observations	1,167	1,167	1,147	1,147	977	977	953
R-squared	0.017	0.095	0.022	0.161	0.002	0.082	
Demographics	No	Yes	No	Yes	No	Yes	Yes
Covid shock	No	Yes	No	Yes	No	Yes	Yes
News	No	Yes	No	Yes	No	Yes	Yes
Mean dep. var.	58.95	58.95	0.330	0.330	0.545	0.545	0.545

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In col (1-2), the outcome time in seconds spent on the information accuracy question; in cols (3-4), the outcome is whether respondents correctly estimated the number of Covid-19-related deaths in the U.S. and in their State, in June; in cols (5-6), whether respondents correctly placed the U.S. among the countries with the highest number of Covid-19 deaths, in October. Col(7) Reports the results of an instrumental variable regression in which correctly reporting the U.S. and State deaths in June is instrumented with the treatment assignment. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

3.4.2 Trust in public institutions

The regression results in cols. 4-6 of Table 2 show that our light-touch self-selected treatment implemented four months earlier increased the perceptions that the CDC was independent (not politicized) by 50%. Using an instrumental variable approach, we see that the effect is driven by higher knowledge accuracy - that is, those who ‘complied’ with the treatment and reported a COVID-19 death estimate in their state and in the country that was as reported by the CDC were much more likely to report a higher trust in the institution’s independence from political interference four months later.

We see a similar effect in terms of statistical significance and magnitude also on the reflective question on self-reported changes in trust in the institution: treated respondents were 40% more likely to state that their confidence in the CDC had increased over time, with the magnitude of the coefficient sharply increasing when considering the instrumental variable approach.

Table 2: Long-term results on trust, perceived politicization, and preventive behaviors.

	(1) ITT Incr. trust in CDC	(2) ITT Incr. trust in CDC	(3) ATT (2 stage) Incr. trust in CDC	(4) ITT CDC not infl. by politics	(5) ITT CDC not infl. by politics	(6) ATT (2 stage) CDC not infl. by politics	(7) ITT Tested for Covid19	(8) ITT Tested for Covid19	(9) ATT (2 stage) Tested for Covid19
Treated	0.0648**	0.0618**		0.0560*	0.0599**		0.0842**	0.0667**	
	(0.0324)	(0.0292)		(0.0289)	(0.0269)		(0.0364)	(0.0308)	
Correct deaths			0.510*			0.551*			0.458*
			(0.295)			(0.280)			(0.272)
Constant	0.143***	0.391	-0.115	0.0908***	-0.0399	-0.657*	0.305***	-0.0498	-0.404
	(0.0282)	(0.281)	(0.432)	(0.0246)	(0.188)	(0.367)	(0.0311)	(0.274)	(0.412)
Observations	978	978	949	990	990	960	987	987	958
R-squared	0.027	0.088		0.036	0.136		0.013	0.099	
Demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Covid shock	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
News	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Mean dep. var.	0.156	0.156	0.156	0.113	0.113	0.113	0.320	0.320	0.320

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In col (1-3), the outcome is whether respondents (in October 2020) report having increased their confidence in the CDC since the beginning of the pandemic; in cols (4-6), the outcome is whether respondents (in October 2020) report that the CDC is not influenced by politics; in cols (7-9), whether respondents report having been tested for Covid-19. Cols (1-2,4-5,7-8) columns report the ITT, while cols (3, 6, 9) report the second stage results instrumenting knowledge accuracy with the treatment assignment. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

3.4.3 Behaviors

In line with our theory of change, we hypothesized that rebuilding trust in institutions would lead to greater compliance with public health guidelines. The literature on government regulation supports this expectation, showing that citizens who perceive the government as more trustworthy are more likely to comply with and consent to its demands and regulations (Tyler, 1990; Braithwaite and Levi, 1998). Conversely, the negative consequences of trust erosion are well-documented, particularly in studies examining vaccine hesitancy and public health compliance (Martinez-Bravo and Stegmann, 2019).

We find that treated respondents were 20% more likely to have taken a COVID-19 nasal swab test (about 6.7 percentage points higher from a baseline of 32%) compared to the control group (See Table 2). This effect was consistent in our instrumental variable regression, supporting our hypothesis that exposure to information sourced from public institutions positively influenced long-term behavioral outcomes in line with the institution's recommendations. Notably, this effect was not driven by a higher likelihood of testing positive for COVID-19 (see Table OA7, in the Online Appendix), indicating that the increase in testing was likely due to greater compliance with public health recommendations rather than illness alone.

3.4.4 Ruling out alternative mechanisms and robustness checks

In this subsection, we summarize the analysis we ran to support our results, which we report in the Online Appendix.

Placebo Outcomes. As mentioned in the previous section, in addition to tracking trust in the CDC, we also measured trust beliefs for a range of other public institutions, including the scientific community, financial institutions, and healthcare providers. Our analysis revealed no significant effects of the treatment on trust in these other institutions, confirming that the increase in trust was specific to the CDC rather than a broad or random shift in trust across all institutions, with the exception of the scientific community, for which there are small positive spillovers. This finding (reported in Tables OA8 and OA9, in the Online Appendix) strengthens the conclusion that our intervention targeted trust in the CDC specifically, with some spillovers onto the other institution that most closely resembled the CDC.

Inter-personal trust. We also show that our results are not driven by general trust. Controlling for the level of interpersonal trust that our respondents reported in the first survey wave, the treatment effect does not change (see Table OA10 in the Online Appendix).

Alternative specifications. We replicate our main specifications in several ways. First, we consider a probit model, rather than a simple OLS, to take into account the binary nature of our outcomes. The coefficients of interest, which are reported in Table OA11, do not vary in magnitude nor in significance. Second, we look at the opposite direction of our main outcomes, that is, we study whether the treatment reduces the likelihood of respondents having decreased their confidence in the CDC and perceiving the institution as independent. The coefficients confirm our main results (see columns (1)-(4) of Table OA12), proving that our effects are not driven by assumptions in the data-generating process or outliers. Lastly, we consider the perception of whether the CDC is affected by politics as a four-point scale outcome, instead of binary, from "heavily influenced by politics" to "not influenced at all", as it is presented in the survey. Results (reported in columns (5) and (6) of Table OA12 in the Online Appendix) are consistent with our main specification.

Validity of meta-cognition. We validate our outcome measure of meta-cognition — respondents' self-awareness of changes in trust beliefs — by observing a correlation between respondents' reported awareness of increased trust in a set of institutions and their actual measured increase in trust between April and October based on our survey data. Respondents' self-assessments of trust changes consistently predict the measured shifts in trust, as shown in Tables OA13 and OA14, in the Online Appendix. This consistency reinforces both the reliability of respondents' awareness of their trust belief changes and the validity of meta-cognition as an effective method for tracking belief updates, supporting the validity of the self-reported change in confidence in the CDC.

Other behavioral outcomes. We did not observe a treatment effect on other behavioral outcome measures, which included staying home unless strictly necessary, sneezing or coughing into a tissue or elbow, using pick-up or delivery options more frequently, maintaining safe distances, avoiding touching objects outside, washing hands more thoroughly, and covering one's mouth in public with a mask or scarf. However, by the time of the final survey wave in October 2020, many COVID-19 restrictions had been lifted, masks were no longer mandatory, and physical distancing measures were not being enforced, largely due to the progress of the vaccination campaign. This shifting context likely diminished the relevance and urgency of these behaviors, explaining the lack of detectable treatment effects. Nevertheless, treated respondents were significantly more likely to adopt at least one of the preventive behaviors listed above, suggesting that the treatment may have had an impact on following at least some of the recommended behaviors, although this effect appears to be driven by a small number of individuals and should be interpreted with caution (see Table OA15, in the Online Appendix). Several months after the pandemic began, the primary measures that remained in place were nasal swab testing for COVID-19 and the recommendation to maintain physical distance after a positive result. If the treatment influenced any behavior, we argue that testing was the most critical one to shift, given its lasting role in controlling the spread of the virus.

Heterogeneity. We do not observe heterogeneous effects according to the media diet or education. The treatment reduces the difference in both knowledge and trust outcomes existing between people consuming Republican-leaning news and those consuming Democratic-leaning news so that treated individuals display comparable outcomes, while the untreated differ significantly. However, while the treatment effect for individuals consuming Republican-leaning news (treated individuals in this group are 36% more likely to correctly estimate the number of casualties, 67% more likely to have increased their confidence in the scientific community, and 35% more likely to have getting tested compared to the control group) is more considerable than the one reported by people consuming Democratic-leaning news (treated individuals in this group are 18% more likely to correctly estimate the number of casualties, 0,04% more likely to have increased their confidence in the scientific community, and 35% more likely to have getting tested compared to the control group), the difference is not statistically significant. This might indicate that respondents consuming Republican-leaning news were less likely to know or trust the CDC before the treatment and had, in general, a worse knowledge accuracy of casualties. Hence, the treatment affects this group more.

Considering the marginal effects of the treatment by education level, we find that the treatment leads to a larger knowledge improvement and a higher likelihood of testing for Covid-19 for people with lower levels of education (less than a Bachelor's degree), suggesting that these respondents were less informed. On the other side, treated respondents with higher levels of education are more likely to increase their level of trust in the CDC, but the differences are not statistically significant.

4 Experiment II: The importance of the value of the information

In our first study, we demonstrated that reducing the search costs for public institution-sourced information could encourage citizens to consume such information, update their factual knowledge, and increase trust in the institution, with potential behavioral improvements. However, several questions remained unanswered: Would this light-touch, self-administered information intervention work equally well for other institutions? Would the intervention be effective in periods outside of a global pandemic, when demand for institution-sourced information might be less urgent and personal experiences less influential? In our experiment, we used the COVID-19 death rate, a simple and salient piece of information that may be unique to the particular context of the pandemic; would eliciting knowledge on another less salient fact be equally effective? What mechanisms explain our results — does increased trust stem from mere exposure to institutional communication material (e.g. its website), or is it driven by the type of trustworthiness signal? Additionally, what other institutional trust beliefs, beyond perceptions of politicization, might shift due to increased information consumption?

To address these questions, we partnered with NORC again to implement a new survey experiment on the GSS AmeriSpeak panel in August 2024, four years after our original study. We intentionally timed this experiment to coincide with a period of high political polarization, ahead of the Presidential elections, mirroring the political context of our first experiment in June 2020. By choosing this comparable time frame, we aim to examine whether the same intervention can have similar effects outside the context of a public health crisis and across different institutions, but still at a time where views about the societal value of public institutions were again influenced by polarizing political debates that may have reinforced ideological heuristics.

4.1 Data

We partnered with NORC to administer another survey experiment to the GSS AmeriSpeak panel leveraging the Time-sharing Experiments for the Social Sciences (TESS) program, which allowed us to field our experiment at no cost, although TESS limits researchers to six items (questions or stimuli) per study[17]. We recruited 1,630 respondents and implemented a three-arm survey experiment designed to replicate the main features of our first study while also teasing out the mechanisms by which the type of trustworthiness signal may matter.

For this study, we chose to test the efficacy of our intervention in strengthening trust in the U.S. Environmental Protection Agency (EPA). This choice was motivated by three key reasons. First, EPA policies and programs directly affect Americans' daily lives, and public trust in this institution - and support for its policies. This contrasts with institutions where the impact on life outcomes may be less immediately salient, such as NASA or the National Oceanic and Atmospheric Administration. Second, greater compliance with EPA-recommended behaviors (e.g., reducing one's carbon footprint) can have positive welfare implications by reducing negative externalities, similar to following CDC-recommended behaviors during the pandemic. Third, like the CDC during the pandemic, the EPA has faced significant politically polarizing attacks, particularly from the Republican party leadership, which has called for the resignation of the EPA's secretary following new environmental protection rules and sought a Supreme

[17]Time-sharing Experiments for the Social Sciences (TESS) is an NSF-funded initiative. Investigators propose survey experiments to be fielded using a nationally representative Internet platform via NORC's AmeriSpeak Panel (see tessexperiments.org for more information).

Court injunction to block their enforcement[18]. According to an August 2024 Pew Research study tracking Americans' trust in government agencies, the partisan divide in favorability was particularly deep for two main agencies: CDC (78% favorable among Democrats vs. 33% among Republicans) and the EPA (73% vs. 32%). These factors make the EPA a suitable candidate for evaluating whether our intervention can help bridge the political gap in institutional trust.

In the first survey question[19], we gauged respondents' baseline knowledge of the EPA to assess whether a lack of knowledge correlates with lower trust in the agency. This will help us validate our behavioral inattention model. The second question introduced our randomized intervention, designed to test respondents' accuracy of knowledge about an EPA-related issue, much like the approach in our CDC study (this is detailed in the next section). For questions three and four, we measured two critical dimensions of trust beliefs: respondents' perceptions of the EPA's benevolence and politicization. The exact wording was, in order, *"Do you think the people running the following institutions recommend policies and regulations that improve or worsen your life and the lives of your loved ones?"* and *"How politicized versus independent do you think the following institutions are?"*. The answers were, following the recommendations of Stantcheva (2023), 7-point Likert scale from 1, "Always worsen" and "Completely politicized", to 7, "Always improve" and "Completely independent", with 4 being neutral. These questions allow us to track another dimension of trust beliefs - agency's expertise and capabilities - besides respondents' trust in its integrity and impartiality. As a placebo, we asked the same questions about the IRS to ensure respondents paid attention to their responses. In question five, we elicited respondents' demand for the EPA's intervention in a hypothetical scenario, designed to reflect a real-world situation where the agency may be expected to intervene.

In our first study, we elicited respondents' willingness to comply with CDC-recommended behaviors, which were relevant and widely applicable during the pandemic. However, applying a similar approach to the EPA presents limitations, as most EPA laws and policies are enforced by government agencies rather than being self-regulated. During the pandemic, the enforcement of CDC guidelines relied heavily on personal willingness to comply, with enforcement often delegated to business owners and employers. In contrast, EPA regulations are typically executed and monitored by government agencies, making personal compliance less directly observable and more challenging to measure in surveys.

To validate survey outcomes, some experimental studies use donations as a proxy for respondents' genuine preferences through monetary allocation, but this method does

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[18]These attacks follow organizational changes initiated under the Trump administration, which included radical budget cuts and a reduction in EPA personnel. The Trump 2024 Presidential campaign has also proposed further cuts and policy reversals if reelected

[19] *"Look at the list of government agencies below and select the ones that you are familiar with — that is, the ones you know something about their mission, role, and responsibilities."* Respondents could select from a randomized list that included: Environmental Protection Agency (EPA), Centers for Disease Control and Prevention (CDC), National Oceanic and Atmospheric Administration (NOAA), National Park Service (NPS), Food and Drug Administration (FDA), Internal Revenue Service (IRS), Federal Laboratory Consortium for Technology Transfer (FLCT), and Open World Leadership Center (OWLC).

not apply here, as government agencies cannot receive donations or gifts. Therefore, we adopted an alternative approach by gauging respondents' support for the EPA's involvement in research activities, a practical indicator of trust and support for institutional intervention.

The question asked: *"Imagine your state is planning a study to measure air pollution levels in our community. They are asking residents like you which organization they trust most to lead this important project. What is the percent chance on a scale from 0% (absolutely no chance) to 100% (absolute certain) to you would recommend the Environmental Protection Agency (EPA)?"*. To make sure respondents understood how to report the percentages and to make sure they were more comparable across respondents, we used Wiswall and Zafar (2015)'s approach. [20].

Since NORC does not systematically collect data on respondents' media consumption habits, we used the sixth and final question to ask respondents about their preferred sources of information. This allows us to construct an index of media slant, replicating the methodology from our prior experiment, which we use as a moderator in our analysis as we did in the first study.

4.2 Methodology: a randomized intervention

Regardless of whether the respondents flagged to be familiar with the EPA, all participants were then shown a brief one-sentence description of the EPA: *"The U.S. Environmental Protection Agency (EPA) is the federal government agency responsible for implementing policies that protect human health and the environment, such as reducing emissions"*. This provided a consistent baseline understanding of the EPA's mission and responsibilities for all respondents. This framing, copied from the EPA's mission statement, was also chosen to clarify to all respondents that this is the government agency responsible for setting the emission targets that have been the subject of political debates they may have heard of from peers or news outlets.

We then tested respondents' factual knowledge in a manner similar to our CDC study, but this time, we implemented a three-arm randomization. In two randomized groups, respondents were asked the following question: *"EPA also manages the NowCast Air Quality Index (AQI), which tracks levels of air pollution in real-time across the country. The higher the NowCast AQI score, the worse the air quality and the greater the health concern. To the best of your knowledge, what is the current level of pollution in your zipcode?"* Responses were collected using a slider ranging from 0 to 500, which covered the range of possible AQI values. The slider was labeled on the survey screen as *"NowCast AQI score in your zipcode"* to clarify the specific metric of interest. One of these two groups was provided with an additional line: *"If you wish to do so, you can look up the answer on the EPA's official*

[20]Below the question we told respondents: *For example, numbers like: 2 and 5 percent may indicate 'almost no chance', 18 percent or so may mean 'not much chance', 47 or 52 percent chance may be a 'pretty even chance', 83 percent or so may mean a 'very good chance', 95 or 98 percent chance may be 'almost certain'*

website at the following link: https://www.epa.gov/aboutepa/regional-and-geographic-offices (Note: if you click on this link, a new browser window will open. You will not be exited from the survey)". This mirrored the approach we used in our previous study by reducing the search costs of obtaining accurate information from the EPA's website and spelling out the URL so respondents could see where they were being redirected to.

In the third randomized group, respondents were asked a different factual question: *"EPA has multiple regional offices to address health and environmental concerns in specific areas of the country, including where you live. To the best of your knowledge, in which city is the EPA regional office that is responsible for your state of residence? If you wish to do so, you can look up the answer on the EPA's official website at the following link: https://www.epa.gov/aboutepa/regional-and-geographic-offices (Note: if you click on this link, a new browser window will open. You will not be exited from the survey)"*. This group was also provided with the option to look up the answer on the EPA website, using identical wording as the previous group, so that we could create a comparable setting where the search costs were identical across two of the three randomized groups.

This additional arm allowed us to experimentally vary the other parameter of the model from equation 4 by reducing the individual's perceived value of the information being consumed - that is, attribute x_i. We chose this setting because, while some citizens may appreciate knowing that a government agency has a physical presence in their region as a signal of greater understanding of their needs, this information is arguably less informative for their actions a_{x_i}. Nevertheless, it is plausible that this subset of respondents would still report a higher level of trust in the EPA due to a mere exposure effect or experimenter demand effects, suggesting that changes in trust beliefs wouldn't follow our theoretical predictions. The direction of this effect is unclear a priori and worth investigating as a way to provide additional causal evidence of our hypotheses[21].

Even though the treatment status was randomly assigned, there were some small significant differences concerning some demographics across treatment groups[22] (see Table OA17 in the Online Appendix).

We verified the accuracy of responses by merging respondents' zip codes and the time of survey completion with publicly available data from the EPA's AQI scores. This enabled us to assess the air quality in the respondents' areas at the time they answered our question. We applied a similar approach for participants in the third randomized group, using their zip codes to determine their state of residence and then cross-referencing this information with the EPA office responsible for that state.

[21]Respondents in this randomized group could still have a distribution of their prior beliefs σ_i about the usefulness of this information that is comparable to that of the other group with equally low information search costs). None of the respondents were incentivized to spend additional time on the survey, and they could freely explore the EPA's website if they chose to click the link, just like in our first study.

[22]Respondents in the first groups (low search costs and high-value information) are less likely to consume Republican-leaning news and are more likely to have internet access at home and to live in a city compared to the third group (low search costs and low-value information).

4.3 Results

4.3.1 Familiarity with the institution

The first notable insight is that 30% of the respondents did not select the EPA as one of the public institutions they were familiar with. In Table OA18 in the Online Appendix, we show that knowing the EPA is significantly correlated with higher trust in the institution, measured both in terms of trustworthiness (cols. 1-2) and competence (cols. 5-6). This descriptive finding provides empirical support for our hypothesis that lack of familiarity and exposure to the work of a public institution may lead to lower, unfounded trust. We do find, however, that knowing the EPA is correlated with a slightly higher perception of politicization. We also see that respondents who consumed Republican-slanted media were significantly less likely to report knowing the EPA, further supporting findings from our first study that citizens may use ideological and media-driven cues as mental shortcuts to infer their trust beliefs on an institution in the absence of any knowledge about it (Table OA19 in the Online Appendix).

4.3.2 Knowledge accuracy

We see that the two groups of respondents who faced lower search costs - that is, respondents who were asked to report the AQI score or the EPA office responsible for their region in their area and were provided with a link to check the answer - were similarly likely to provide a correct response and were both significantly more likely to answer correctly compared to the group that faced higher search costs - that is, respondents who were asked to report the AQI score in their area but were not provided with a link to check the answer (see cols. 1-2 of Table 3). Respondents who faced higher search costs were almost half (44% or about 18 percentage points) as likely to report an accurate response.

4.3.3 Trust in public institutions

In this study, we explore two core aspects of trust in public institutions: perceived benevolent impact and perceived integrity. By distinguishing these dimensions, we can examine whether the mechanisms we study—namely, search costs and information attributes—differently influence trust beliefs.

We find that participants who faced lower search costs and received more relevant information (specifically, those given a link to view air quality data in their area) had a slightly higher belief (3% more than the average) that the EPA's work positively impacts people's lives compared to participants with similar search costs who received less relevant information about the EPA's role (i.e., those given a link to locate EPA offices). However, after adjusting for covariates, this treatment effect diminishes in size and becomes statistically insignificant.

The second trust dimension we examine is perceived integrity. Similar to the first study, we assess respondents' perceptions of the institution's political independence as an essential dimension of integrity and accountability. Among participants who faced low search costs (i.e., those provided with a link to verify their answers), those who were given access to the air quality information were 7% more likely to view the EPA as not affected by politics than those who were given access to the EPA's office location link. This finding supports our hypothesis that, when controlling for search costs (k), the perceived value of institution-provided information (x_i) shapes trust. Respondents who accessed real-time air quality data may have recognized the technical rigor and accountability of the EPA's work, strengthening their confidence in the agency. This effect remains robust, with the treatment coefficient largely unaffected after including a comprehensive set of controls.

Table 3 reveals some differences in trust in the EPA between respondents in the two low search cost trial arms. Specifically, those in the high-value information treatment group, who were given a link to check the AQI score in their zip code, reported are 5% more likely to support for EPA's intervention compared to those in the low-value information group, who were only asked to locate the nearest EPA regional office. However, when the control variables are included, the effect is no longer significant.

Table 3: Intention to Treat results on trust outcomes.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Knowledge accuracy	Knowledge accuracy	ITT EPA improves lives	ITT EPA improves lives	ITT EPA indepen-dence scale	ITT EPA indepen-dence scale	ITT Support for EPA's involvement	ITT Support for EPA's involvement
T1: Low search costs	0.0273	0.0189	0.139*	0.0554	0.241***	0.199**	3.190*	1.640
& high value info	(0.0329)	(0.0329)	(0.0832)	(0.0786)	(0.0864)	(0.0844)	(1.717)	(1.628)
T2: High search costs	-0.185***	-0.178***	0.0327	0.00607	0.162*	0.164**	1.163	0.796
& high value info	(0.0309)	(0.0309)	(0.0818)	(0.0772)	(0.0851)	(0.0829)	(1.696)	(1.605)
Constant	0.489***	0.395***	4.764***	4.386***	3.243***	3.847***	59.41***	59.72***
	(0.0228)	(0.0987)	(0.0582)	(0.240)	(0.0605)	(0.258)	(1.201)	(5.003)
Observations	1,399	1,399	1,626	1,626	1,613	1,613	1,580	1,580
R-squared	0.049	0.082	0.018	0.154	0.007	0.087	0.003	0.136
Demographics	No	Yes	No	Yes	No	Yes	No	Yes
News	No	Yes	No	Yes	No	Yes	No	Yes
Mean dep. var.	0.416	0.416	4.788	4.788	3.360	3.360	60.61	60.61

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. The two treatment groups in the table are: the high-search cost and high-value information group and the low-search cost and high-value information group. The excluded group is the low-search cost and low-information group. In cols (1-2) the outcome is whether respondents correctly answered the information question about the AQI score or the location of the closest EPA office (according to the treatment group); in cols (3-4), the outcome reports whether the EPA recommends policies and regulations that improve or worsen the life of the respondents and their loved ones on a 7-point likert scale from "Always worsen" to "Always improve"; in cols (5-6), the outcome reports whether the EPA is politicized versus independent on a 7-point likert scale from "Completely politicized" to "Completely independent"; in cols (7-8), the outcome is percent likelihood that the respondents would recommend the EPA to conduct a study to measure air pollution levels in their community. The control variables are reported in the Appendix.

It is worth noting that the key distinction between our high and low search cost conditions in the high-value information groups was simply the inclusion of a direct hyperlink. Unlike the CDC study, where respondents could have searched for COVID-19 mortality rates and encountered varying figures from different sources, respondents in the high search cost, high-value information group in Table 3 were asked specifically to locate the AQI score reported by the EPA. Searching online would likely have led them directly to the same

EPA webpage as the low search cost group, minimizing the actual search cost difference to a few extra seconds. Despite this minimal difference in the search effort, the extra step substantially affected respondents' likelihood of reporting the correct AQI score. This further underscores the significant impact that small changes in search costs can have among behaviorally inattentive citizens. The finding highlights how even minor reductions in effort required to access high-value information can effectively improve trust and potentially support governmental intervention.

We also perform an instrumental variable analysis, instrumenting correctly responding to the knowledge accuracy question with the treatment status. Results are reported in Table OA20, in the Appendix. Respondents who accessed the high-value information (the air quality index) are generally more likely to have improved their trust in the EPA. We see that the treatment significantly increased perceptions of EPA's political independence and support for involvement in environmental quality assessments in their communities, albeit the latter loses significance when including a rich battery of controls (e.g., demographics and news consumption). Respondents who answered the air quality question correctly reported a stronger belief that EPA was politically independent (about 20% higher from a baseline of 3.3 out of 7 scale), and were more likely to support its involvement in an assessment study (30% more likely from a baseline of 60 out of 100 scale).

4.3.4 Additional results and robustness checks

Placebo outcomes. We repeat our main analyses considering a placebo institution, the Internal Revenue Service (IRS), in order to establish whether our results are due to the information treatment and not some other spurious correlation. The treatment coefficient is not significant across all specifications (see Table OA21 in the Online Appendix).

Alternative specifications. We adopt a probit model to estimate the first stage and an ordered probit model to take into account the nature of the main dependent variables (7-point likert scales), and results do not vary meaningfully (see OA22 in the Online Appendix).

Previous familiarity with the public institution. In line with our theory, respondents who are familiar with the EPA's mission and scope are more likely to trust it and support its work. In Table OA18 in the Online Appendix, we focus on respondents who are not offered the link to the EPA website (those in the group exposed to the high-cost and high-value information treatment) and show that even after controlling for information accuracy, knowing the EPA is a strong predictor of trust.

EPA's office location. The negative results from the low-information and low-search cost treatment, in which we show respondents where is the closest EPA office, cannot be explained by geographic distance. Indeed, one concern could be that respondents, realizing that the EPA has no offices in their State, might reduce their confidence in the EPA. However, there is no evidence of this phenomenon in the data (see Table OA23, in the Online Appendix).

Heterogeneity We analyze whether the treatments have differential effects according to the respondents' characteristics. In particular, we evaluate whether individuals respond differently to low- and high-value information according to their news consumption (Republican versus Democratic news), their education (less than a Bachelor's degree versus a Bachelor's degree or above), or their age, but find no significant differences. Thus, we conclude that individuals respond more to useful information, regardless of their personal characteristics.

5 Conclusions

Public institutions are essential for providing evidence-based information that enables citizens to make decisions that benefit both themselves and their communities, such as adopting preventive health measures or engaging in environmentally sustainable behaviors. However, trust in these institutions has been declining, particularly along ideological lines. This study provides novel evidence on how behavioral inattention contributes to biased judgments about public institutions and demonstrates that light-touch, scalable digital interventions can effectively restore trust and promote welfare-enhancing behaviors.

Through two theory-driven survey experiments conducted on representative samples of Americans during periods of heightened political polarization, we offer causal evidence that reducing search costs for institution-sourced information significantly improves trust in public institutions and increases compliance with recommended behaviors and support for policy interventions. Importantly, these changes are both sustained and conscious, indicating potential for long-term impact.

Our experimental interventions may have captured attention by leveraging curiosity, raising new questions for future research. For instance, could other mechanisms beyond curiosity, such as personalization achieve similar outcomes? Field experiments conducted in partnership with government agencies could test these interventions at scale and examine their generalizability across diverse settings. Expanding this framework to international contexts could also uncover how cultural, educational, and cognitive differences influence the effectiveness of such interventions.

While our study focused on relatively straightforward information (e.g., COVID-19 deaths, AQI scores), future research could explore responses to more complex, institution-sourced information requiring greater cognitive effort. Existing challenges in public comprehension of intricate government policies are well-documented (Nunnari et al., 2024; Dal Bó et al., 2018). Further exploration is needed to disentangle the roles of cognitive skills and behavioral inattention in shaping trust and engagement with such information.

In addition to these contributions, our study highlights the limitations of traditional survey measures that track generalized trust in public institutions. Aggregate metrics often obscure crucial differences between institutions, making it difficult to diagnose why trust erodes more rapidly in some cases or to design targeted interventions. While these metrics are useful for identifying broad trends, they are less effective for causal evaluations that aim to uncover the root causes of trust erosion and assess corrective interventions. We recommend that survey firms and scholars reconsider the reliance on broad trust indices and adopt data collection practices that capture nuanced variations in trust across specific institutions. This approach would provide more actionable insights for policymakers and researchers seeking to bolster institutional trust.

Finally, an important avenue for future work lies in exploring the relationship between institutional trust and interpersonal trust. Understanding whether rebuilding trust in public institutions can also foster interpersonal trust and mitigate partisan polarization could have profound implications for societal cohesion and democratic stability.

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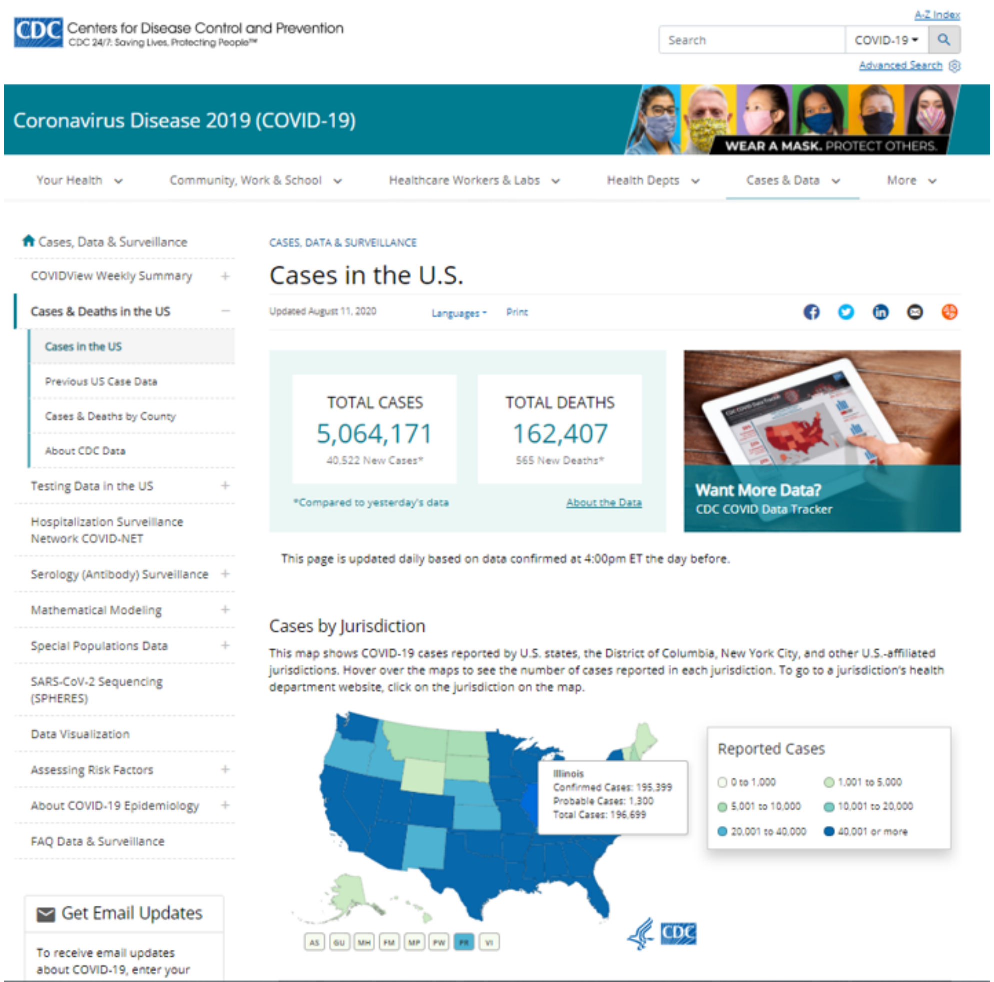
A Appendix

A1.1 Experiment I

A1.1.1 Design

Figure A1: Randomized treatment



Notes: The figure shows the webpage treated respondents would land on if they clicked on the link.

A1.1.2 Controls

Economic shocks. To measure direct economic shocks, we asked all respondents in the last wave of the survey to report their (and their spouse, if present) monthly gross income between

February and October[23]. Further, we also asked respondents' (and their spouses, if present) monthly additional sources of income, the monthly number of hours worked, and whether they received any financial support from the government or non-government organizations at any time during the crisis. This data allows us to estimate both the timing and the magnitude of the economic shocks incurred by respondents' households between waves. We measure direct income shocks in two different ways, and we show that they provide comparable results. In our main specification, we consider whether respondents have lost more than 20% of their income (combining both incomes from work and other sources) between any two months over the period of February (the pre-shock baseline level) to October 2020 to capture the effects of a sudden large drop in income.

$$shock_1 = \begin{cases} 1, & \text{if } \frac{income_t - income_{t-1}}{income_{t-1}} \leq -0.20 \\ 0, & \text{otherwise} \end{cases}$$

. About 38% of respondents lost at least 20% of their household income, between any two months, between February and October 2020[24].

In addition to measures of personal economic shocks, we also control for indirect economic shocks since it is possible that many Americans changed their preferences just by mere exposure to the crisis, such as by knowing someone who got affected economically by the crisis or living in an area that suffered a relatively higher economic distress compared to others (Dyer, 2020; Wright et al., 2020). Measuring economic variations between two months of the same year, however, is a challenge. Many macroeconomic indicators, such as unemployment rate or business closures, are rarely available at the county level, and often they are only released at an aggregate level or on a frequency that is less regular than the timing of our survey waves, making any meaningful comparison difficult. Therefore, we use data collected and updated in real-time by Harvard's Opportunity Insights team on the weekly percentage variations in consumer expenditures with respect to the first week of January 2020 (Chetty et al., 2020). This variable is seasonally adjusted and is available at the county level, which we match with the respondents' residential information[25].

Health shocks. Our main measure of direct health shock is whether respondents had a family member, a friend, or an acquaintance who was hospitalized with COVID-19[26].

[23]In addition to asking in most waves whether respondents incurred any economic or health shock, in the last wave, we asked them to report the exact amount of household income for every month as well as if they knew anyone hospitalized each month. This allows us to have a more granular and quantifiable measure of economic shock beyond the timing of our survey waves

[24]Respondents who lost at least 20% of their household income between any two months from March to October 2020 are more likely to be young, with a low baseline income and to belong to a racial minority group. Furthermore, women, Democrats, and those who live in a metropolitan area have incurred such a negative income shock with a marginal significantly higher probability, while co-habitation (or marriage) seems to smooth the financial impact of the pandemic. We control for all these characteristics in our analysis and show how using different specifications does not change our main results

[25]The Opportunity Insights team uses anonymized data from several private companies to construct public indices of consumer spending, employment, and other outcomes. See Chetty et al. (2020) for further information on series construction.

[26]We consider this combined measure, as 2.4% of the respondents has a family member who has been

About 30% of our respondents knew someone (among family, friends, or acquaintances) who was hospitalized with COVID-19, while 69% knew someone who tested positive. About 33% tested positive for COVID-19 themselves.

We complement these survey-based measures with data on COVID-19 cases in the respondents' county of residence. While this measure might be subject to different data collection and testing regimes across States, these figures were likely to be the same ones reported by the local media. To improve accuracy, we consider COVID-19 cases[27] at the county level reported by the middle of each week. We then consider the population size at the county level in 2019 and construct the following measure of increase in cases between week t and $t\text{-}1$ in county c: $\frac{cases_{ct}-cases_{ct-1}}{population_c}$[28]. When, instead, we consider an outcome that is not in change, we focus on the logarithm of the cumulative number of cases weighted by the county population: $ln\left(\frac{cases_{ct}}{population_c} * 100,000\right)$.

Media consumption A number of paper estimates the impact of media consumption on people's preferences and beliefs (for instance, Martin and Yurukoglu (2017); DellaVigna and Kaplan (2007); Bursztyn et al. (2020)), mostly focusing on exposure to a specific channel (Fox News). In this paper, instead, we consider all the news sources consumed by respondents and assess whether they are overall slanted in a direction or not. Indeed, we aim at understanding how exposure to information affects respondents, hence we treat differently those who have been exposed only to source slanted in one direction from those that have consumed news slanted in different ones, as these latter might have a more heterogeneous perspective on facts. We are also interested in capturing as many media sources as possible (i.e., TV, social media, international news, etc.) as opposed to one news source only, as in Martin and Yurukoglu (2017). Therefore, we collected information on respondents' preferred news sources (including international news and social media) and the number of hours of news they consumed[29]. Based on the news sources they indicated, we constructed a "slant score" using the "*AllSides.com*" platform, one of the most commonly used sources of partisan media slant analysis[30]. The website assigns a score from 1 (Extremely Left) to 5 (Extremely Right) to major sources of news by analyzing their written content

hospitalized, 9.8% has a relative, 14.1% a friend and 14.9% an acquaintance. To control for additional direct health shocks, we also asked respondents their type of health insurance (e.g., public or private), whether they have caring responsibilities towards an elderly or someone with disabilities, which are at greater risk of complications from contracting the virus, and if they knew a healthcare professional who had been working closely with COVID-19 patients

[27]We exploited the data collected by the New York Times from state and local governments and health departments, and available here `https://github.com/nytimes/covid-19-data`.

[28]We multiply this measure by 100 to ease the interpretation of the coefficients in our regressions

[29]The question asked: "Do you get your news from any of these sources (either on television or on the internet)?", and the multiple option answers were: "ABC, CBS, NBC, CNN, Fox News, MSNBC, and 'other, please specify"' (e.g., some respondents added The NY Times, The Washington Post, BBC, NPR, and PBS). While there is no exact methodology to measure the partisan slant of news sources (Budak et al., 2016; Groseclose and Milyo, 2005), and since within each source different programs might cover the same news in different tones (Bursztyn et al., 2020), we measured whether respondents were exposed to different points of view during the crisis.

[30]`https://www.allsides.com/unbiased-balanced-news`

on a regular basis. Matching the scores by Allsides[31] to the respondents' choices, we create an index summing the scores of each source consulted by an individual and divided by the maximum number of possible points.

$$\textit{Media slant index, for an individual consuming N sources of news} = \frac{\sum_{n=1}^{N} score_n}{N}$$

This variable measures how politically homogeneous the news sources that respondents consumed are, by taking any value between 1 and 5: the closer a respondent is to 1, the more they consume homogeneous (i.e., less politically diversified) left-leaning media, while the closer they are to 5 the more homogeneous and right-leaning is their media consumption. A score towards the middle indicates either that respondents consume unbiased news, or that they consume news that is slanted in both directions and so that they are exposed to both partisan narratives. Based on this specification, we see that 51% of the Republicans consume Republican-leaning news, and 46% of the Democrats consume Democratic-leaning news. Among independents and non-voters, around 25% (24%) consume Republican-leaning news (Democratic-leaning news).

Other controls. The baseline controls for the regressions are: Gender, Age (in four generations: Gen Z, Gen X, Millennials, Baby Boomers, and above), Education (in four groups: Less than high school, High school or equivalent, Some college, Bachelor or more), Income (in quintiles), Race (White, African Americans, Hispanics, and other), whether respondents are cohabitating with someone else, whether they have children under the age of 18, whether they were in financial difficulties already prior the pandemic, Whether they are in the labor force, whether they were unemployed in February (before the pandemic), the U.S. region (South, North-East, Mid-west, West), whether they live in a metropolitan area, whether they have health insurance and whether it's public or private, the population density of their zip code of residence, whether they are Catholic, or Protestant. We also control for whether respondents completed the survey in a shorter time than the 99^{th} percentile.

A1.2 Experiment II

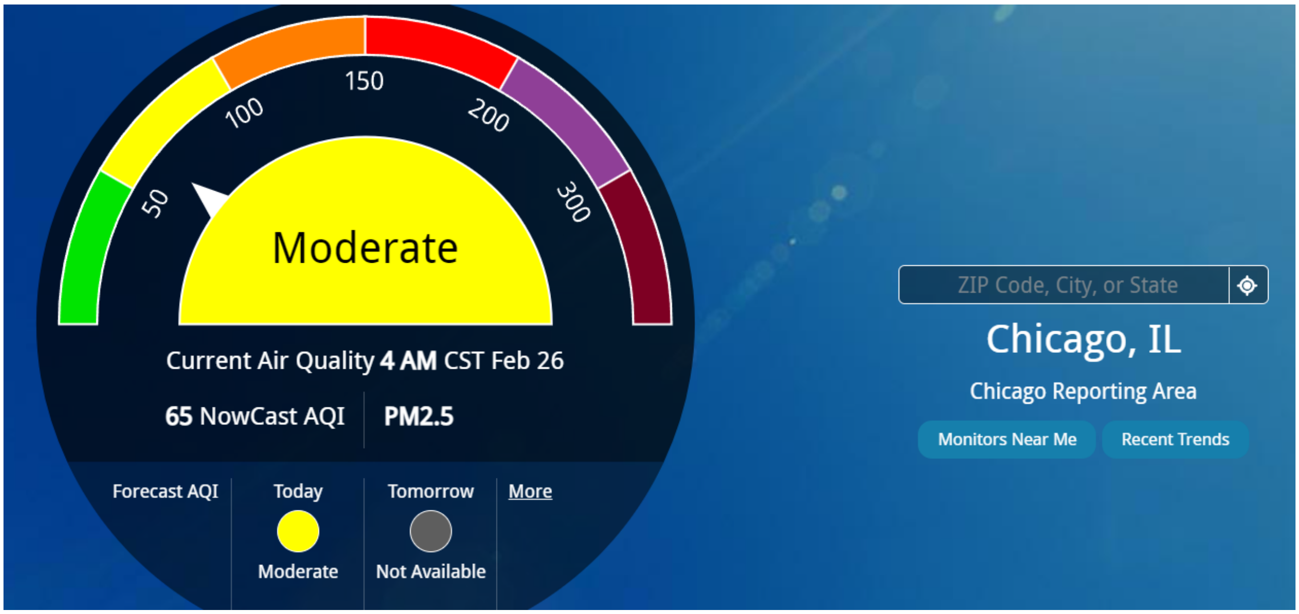
A1.2.1 Controls

The controls considered closely align with the ones adopted in the previous experiment. The news index is built exactly as above, and the other controls are the same, with the exclusion of the information con concerning Covid-19, which is not applicable in this case, and the information on health insurance, which was not available for these respondents. Therefore, the final list of controls is: Gender, Age (in four generations: Gen Z, Gen X, Millennials, Baby Boomers, and above), Education (in four groups: Less than high school, High school or equivalent, Some college, Bachelor or more), Income (in quintiles), Race (White, African

[31]We use the scores of the first week of April 2020, our baseline wave.

Americans, Hispanics, and other), whether respondents are cohabitating with someone else, whether they have children under the age of 18, whether they were unemployed the U.S. region (South, North-East, Mid-west, West), whether they live in a metropolitan area, whether they are Catholic, or Protestant. We also control for whether respondents completed the survey in a shorter or longer time than the 99^{th} percentile.

Figure A2: Randomized treatment A (low search costs, high value)

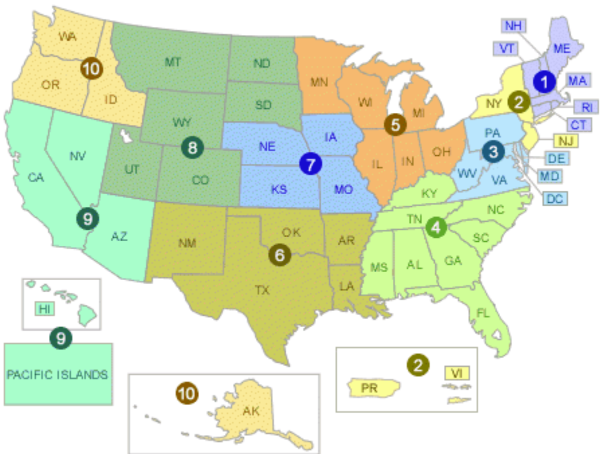


Notes: The figure shows the landing webpage treated respondents would see if they clicked on the link.

Figure A3: Randomized treatment B (low search costs, low value)

Regional Offices

We have ten Regional offices, each of which is responsible for the execution of our programs within several states and territories. To get information about activities in your Regional office, select the appropriate link.



- Region 1 – Boston (serving CT, ME, MA, NH, RI, and VT)
- Region 2 – New York City (serving NJ, NY, Puerto Rico, the U.S. Virgin Islands and 8 federally recognized Indian Nations)
- Region 3 – Philadelphia (serving DE, DC, MD, PA, VA, WV and 7 federally recognized tribes)
- Region 4 – Atlanta (serving AL, FL, GA, KY, MS, NC, SC, and TN)
- Region 5 – Chicago (serving IL, IN, MI, MN, OH, and WI)
- Region 6 – Dallas (serving AR, LA, NM, OK, and TX)
- Region 7 - Kansas City (serving IA, KS, MO, and NE)
- Region 8 – Denver (serving CO, MT, ND, SD, UT, and WY)
- Region 9 - San Francisco (serving AZ, CA, HI, NV, American Samoa, Commonwealth of the Northern Mariana Islands, Federated States of Micronesia, Guam, Marshall Islands, and Republic of Palau)
- Region 10 – Seattle (serving AK, ID, OR, WA and 271 native tribes)

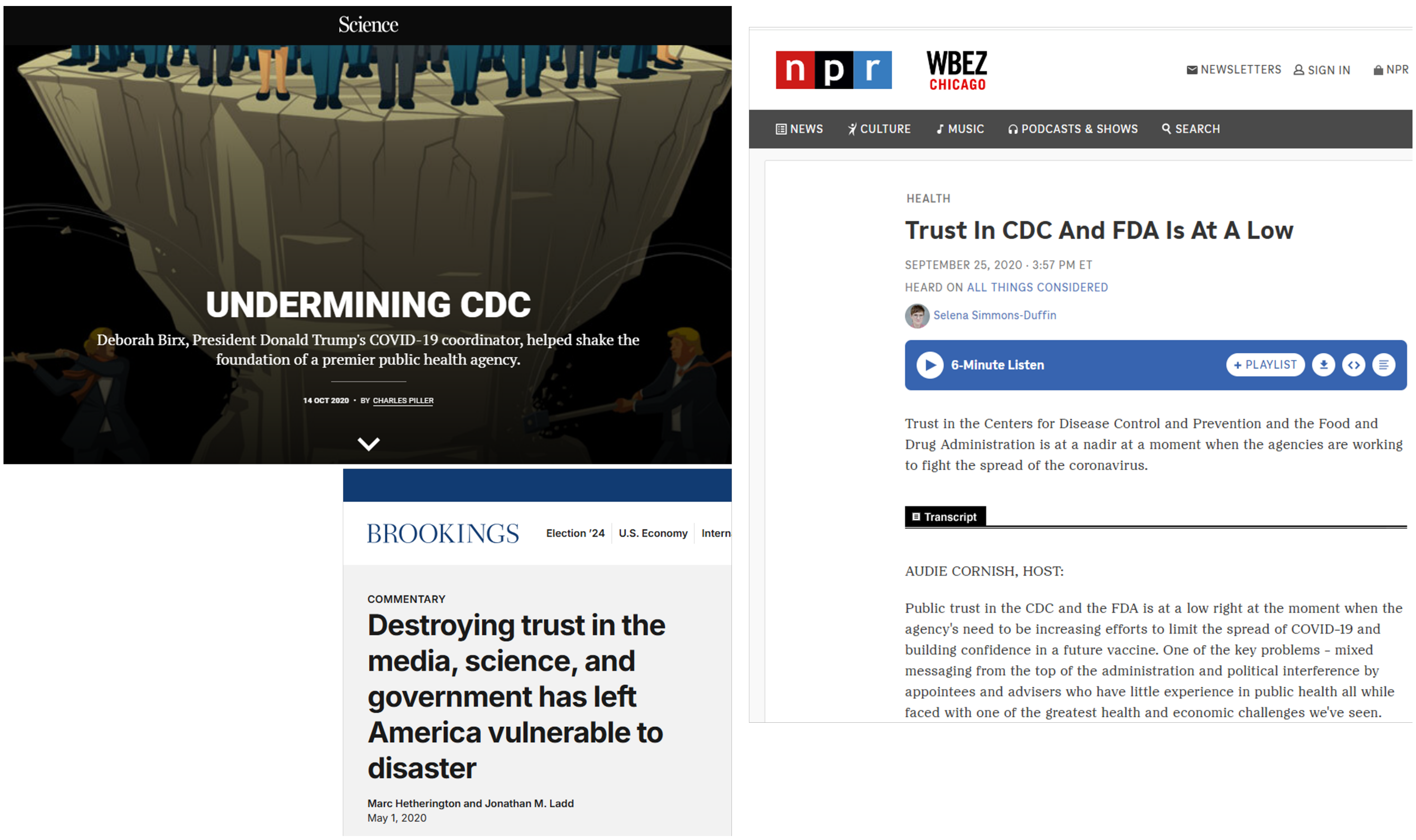
Notes: The figure shows the landing webpage treated respondents would see if they clicked on the link.

A Online Appendix

OA1.1 Experiment I

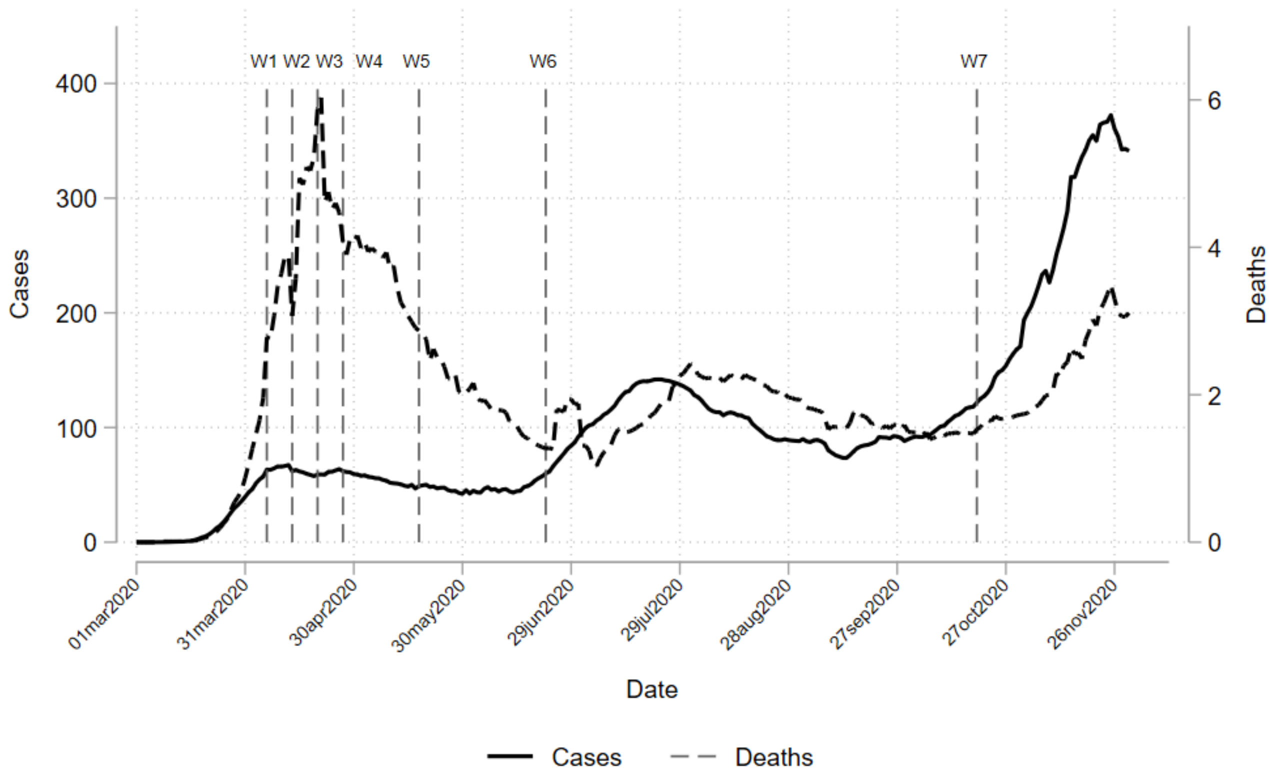
OA1.1.1 Public institution media coverage

Figure OA1: Media coverage about the CDC



OA1.1.2 Survey timing

Figure OA2: Timing of the longitudinal survey waves against health indicators of the COVID-19 pandemic



Notes: The figure shows the timing of the seven survey waves implemented between early April and the end of October 2020 against the curves of the 7-day rate of COVID-19 cases and deaths per 100,000 inhabitants, allowing for comparisons between areas with different population sizes in the United States. The figure is based on the U.S. Centers for Disease Control and Prevention publicly available data.

OA1.1.3 Sample characteristics

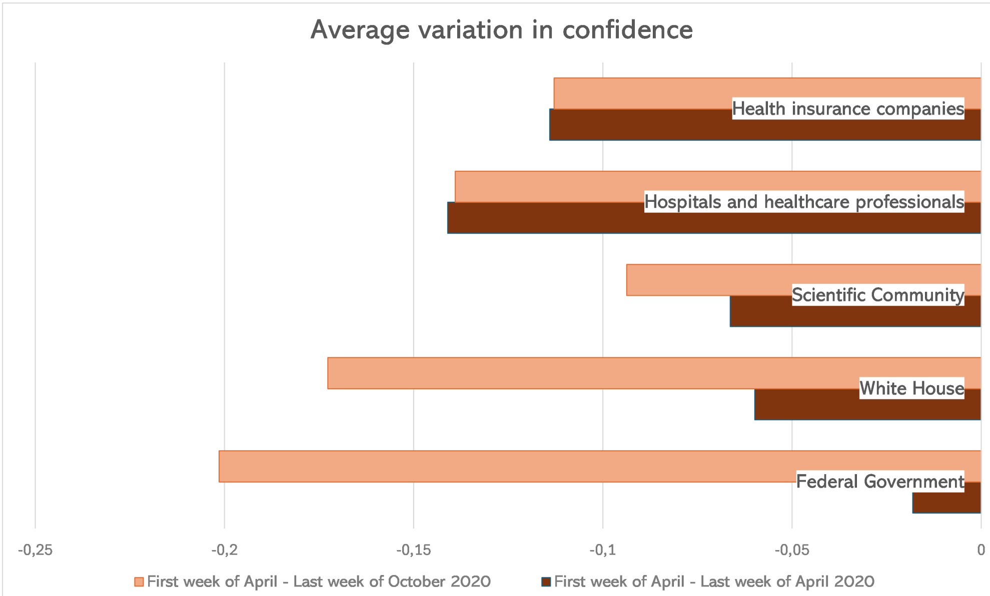
Table OA1: Summary statistics of the key demographics in each survey wave, applying survey weights.

	Wave1	Wave2	Wave3	Wave4	Wave5	Wave6	Wave7
Republican	27.37	25.33	26.24	25.94	25.55	24.53	26.47
Democrat	37.93	37.19	35.87	37.41	36.53	36.2	35.94
Independent & other	34.71	37.48	37.88	36.64	37.92	39.27	37.59
Woman	51.69	51.68	51.68	51.69	51.68	51.68	51.7
Age: 18-29	20.51	20.5	20.5	20.5	20.5	20.5	20.53
Age: 30-44	24.71	24.83	25.52	25.97	25.36	25.45	25.5
Age: 45-59	25.02	24.9	24.21	23.76	24.38	24.29	24.22
Age: 60+	29.76	29.77	29.77	29.77	29.77	29.77	29.75
Less than HS	9.77	9.77	9.77	9.77	9.77	9.77	9.77
High school	28.26	28.26	28.26	28.26	28.26	28.26	28.25
Some college	27.71	27.7	27.7	27.7	27.69	27.7	27.73
Bachelor +	34.27	34.27	34.27	34.27	34.28	34.27	34.26
I income q	22.63	22.89	23.89	24.37	23.5	24.41	26.35
II income q	21.35	21.15	20.47	20.4	21.48	19.99	17.86
III income q	17.99	17.28	17.23	17.65	16.39	17.64	17.9
IV income q	18.98	19.37	19.6	18.66	19.7	19.29	18.69
V income q	19.05	19.3	18.81	18.93	18.93	18.66	19.21
Financial hardship pre-COVID-19	31.52	31.04	31.89	31.33	31.14	31.75	31.38
African American	11.93	11.93	11.93	11.93	11.93	11.93	11.93
Hispanic	16.67	16.67	16.67	16.67	16.67	16.67	16.66
Other Race	8.59	8.58	8.58	8.58	8.58	8.58	8.62
White	62.81	62.82	62.82	62.82	62.82	62.82	62.79
Cohabitating	54.58	57.9	57.36	57.98	58.55	62.43	62.68
Parent of minor	27.87	26.64	27.57	27.13	26.68	26.61	27.07
Caring responsibilities	16.48	15.95	16.65	16.21	15.87	16.42	16.37
Not in the labor force	27.68	27.14	28.18	27.75	26.91	27.24	26.99
Unemployed in Feb	9.81	8.96	10.8	9.73	9.06	8.83	9.56
North-East	17.45	17.45	17.45	17.45	17.45	17.45	17.44
Midwest	20.74	20.74	20.74	20.74	20.74	20.74	20.73
South	37.98	37.97	37.97	37.97	37.97	37.97	38
West	23.84	23.84	23.84	23.84	23.84	23.84	23.83
Metropolitan area	85.49	85.1	86.07	84.78	85.29	86.07	85.49
No health insurance	6.67	6.39	6.54	5.89	6.6	6.29	6.24
Population density (ZCTA)	371350.9	368034.6	379836.2	380862.6	380720.3	373379.7	383178.7
N	1441	1228	1177	1219	1199	1192	1076

OA1.1.4 Motivating evidence

Fact I: Institutional trust has declined and did not recover

Figure OA3: Variation in self-reported institutional trust (on a 5-point likert scale, with 1 being the lowest and 5 the highest score) across waves.



Fact II: Trust in public health institutions predicts the adoption of preventive behaviors both in levels and trends, while trust in the government does not.

List of preventive behaviors:

- I don't leave home unless strictly needed
- I sneeze or cough into a tissue or the inside of my elbow
- I use pick-up or delivery options more frequently
- I keep a safe distance from others
- I try not to touch objects when I am outside
- I wash my hands more thoroughly and more frequently
- I cover my mouth in public (with a mask, a scarf, . . .)

Table OA2: Adoption of preventive behaviors and institutional trust

	(1) % prev. behav. adopted	(2) % prev. behav. adopted	(3) % prev. behav. adopted	(4) Δ % prev. behav. adopted w1 -w3	(5) Δ % prev. behav. adopted w1-w3	(6) Δ % prev. behav. adopted w1-w3	(7) Use mask w7	(8) Use mask w7
Trust index PIs w1	0.198***		0.173**					
	(0.0728)		(0.0756)					
Trust index gov w1		0.114**	0.0638					
		(0.0532)	(0.0531)					
Δ trust index PIs w1-w4				0.144***		0.117**		
				(0.0500)		(0.0582)		
Δ trust index gov. w1-w4					0.0661	0.0288		
					(0.0790)	(0.0878)		
Trust index PIs w7							0.266**	0.344**
							(0.129)	(0.134)
Trust index gov w7								-0.264***
								(0.0909)
Constant	0.508***	0.570***	0.491***	0.129*	0.150**	0.150**	0.549***	0.626***
	(0.0599)	(0.0530)	(0.0632)	(0.0679)	(0.0657)	(0.0680)	(0.147)	(0.142)
Observations	1,421	1,433	1,417	1,082	1,088	1,073	1,063	1,059
R-squared	0.175	0.168	0.176	0.112	0.104	0.108	0.216	0.230
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covid shock	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
News	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Average dep. var.	0.709	0.709	0.709	0.0344	0.0344	0.0344	0.872	0.872

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In col (1)-(3) the outcome is the fraction of the preventive behaviors adopted from the list above; in cols (4-6), the outcomes is the variation in this fraction between the first and the third week of April; in cols (7) and (8), we flag whether respondents declare to wear a mask in May. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

Fact III.a: Variation in institutional trust is partly explained by negative experiences with the Covid-19 pandemic, but the strongest and most significant predictor is consuming slanted news.

Table OA3: Variation in trust, personal experiences, and media consumption

	(1) Δ index health w4-w1	(2) Δ index health w4-w1	(3) Δ index gov w4-w1	(4) Δ index gov w4-w1	(5) Δ index health w7-w1	(6) Δ index health w7-w1	(7) Δ index gov w7-w1	(8) Δ index gov w7-w1
Any direct shock Apr	-0.00905	-0.0117	-0.00181	-0.00683				
	(0.0117)	(0.0175)	(0.0159)	(0.0213)				
Any shock Apr*Dem News		0.00824		0.00303				
		(0.0314)		(0.0297)				
Any shock Apr*Rep News		0.00409		0.0187				
		(0.0211)		(0.0207)				
Any direct shock Apr-Oct					-0.00818	-0.0115	-0.0320***	-0.0445***
					(0.00822)	(0.0121)	(0.0106)	(0.0147)
Any shock Apr-Oct*Dem News						0.0136		0.0254
						(0.0190)		(0.0245)
Any shock Apr-Oct*Rep News						-0.000732		0.0280
						(0.0205)		(0.0255)
Republican leaning news	-0.0396***	-0.0402***	-0.0148	-0.0172*	-0.0418***	-0.0412**	0.0396***	0.0244
	(0.0114)	(0.0128)	(0.00975)	(0.0102)	(0.0140)	(0.0191)	(0.0111)	(0.0169)
Democratic leaning news	-0.0121	-0.0131	-0.0136	-0.0142	-0.0197	-0.0266	0.00429	-0.00865
	(0.0108)	(0.0130)	(0.0116)	(0.0132)	(0.0133)	(0.0174)	(0.0114)	(0.0162)
Constant	0.0332	0.0337	0.0644	0.0632	0.0186	0.0206	-0.116**	-0.109**
	(0.0441)	(0.0440)	(0.0603)	(0.0606)	(0.0410)	(0.0406)	(0.0547)	(0.0539)
Observations	1,150	1,150	1,157	1,157	997	997	1,005	1,005
R-squared	0.113	0.114	0.079	0.079	0.101	0.102	0.111	0.114
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Average dep. var.	-0.0275	-0.0275	-0.00835	-0.00835	-0.0337	-0.0337	-0.0431	-0.0431

Standard errors in parentheses,*** p¡0.01, ** p¡0.05, * p¡0.1

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

OLS regression coefficients. In cols. (1) and (2) the outcome is the variation in the trust index in public health institutions between the first and the last week of April; cols (2) and (3) report the variation in the trust index of government-related institutions in the same time frame; in cols (4)-(8), we report the same outcomes but consider the time span between the first week of April and the third week of October. All regressions take into account population survey weights and the sampling procedure. The control variables are specified in the Appendix.

Fact III.b: Perceived politicization predicts drops in trust, both self-reported and factual

Table OA4: Perceived polarization and trust

	(1) Decrease trust sci. comm. (w7-w1)	(2) Self-reported decrease trust sci. comm.	(3) Decrease trust hospitals (w1-w7)	(4) Self-reported decrease trust hospitals	(5) Decrease trust health insur. (w7-w1)	(6) Self-reported decrease trust health insur. (w7-w1)	(7) Self-reported decr. trust CDC
Sci. comm. politicized	0.122**	0.372***					
	(0.0503)	(0.0370)					
Hospitals politicized			0.160***	0.264***			
			(0.0400)	(0.0448)			
Health insur. politicized					0.0866**	0.180***	
					(0.0397)	(0.0398)	
CDC politicized							0.399***
							(0.0338)
Rep. news	0.0857	0.132***	0.129***	0.0583**	0.0625	-0.0287	0.104***
	(0.0540)	(0.0305)	(0.0473)	(0.0295)	(0.0601)	(0.0363)	(0.0388)
Dem. news	-0.0275	-0.0185	0.0168	-0.0239	0.0527	-0.0117	0.0849**
	(0.0490)	(0.0281)	(0.0477)	(0.0234)	(0.0434)	(0.0409)	(0.0351)
Constant	0.258	0.113	0.337**	0.133	0.0745	-0.0203	0.0853
	(0.172)	(0.103)	(0.162)	(0.115)	(0.159)	(0.138)	(0.120)
Observations	1,004	1,012	1,009	1,014	1,008	1,009	1,005
R-squared	0.091	0.243	0.117	0.166	0.132	0.114	0.249
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covid shocks	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Average decrease	0.286	0.148	0.306	0.0827	0.284	0.187	0.287

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In col. (1) the outcome is a dummy = 1 if the respondent decreased their confidence in the scientific community between wave 7 and wave 1; in col. (2) the outcome is a dummy flagging the self-reported variation in confidence in the scientific community: the dummy =1 if the respondent reports having decreased their confidence in the scientific community from the beginning of the pandemic. Cols (3)-(6) repeat the same exercise but focus on hospitals and healthcare professionals (cols. 3-4) and health insurance companies (cols. 5-6). The outcome of col. (7) is a dummy = 1 if the respondent declared having decreased their confidence in the CDC. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

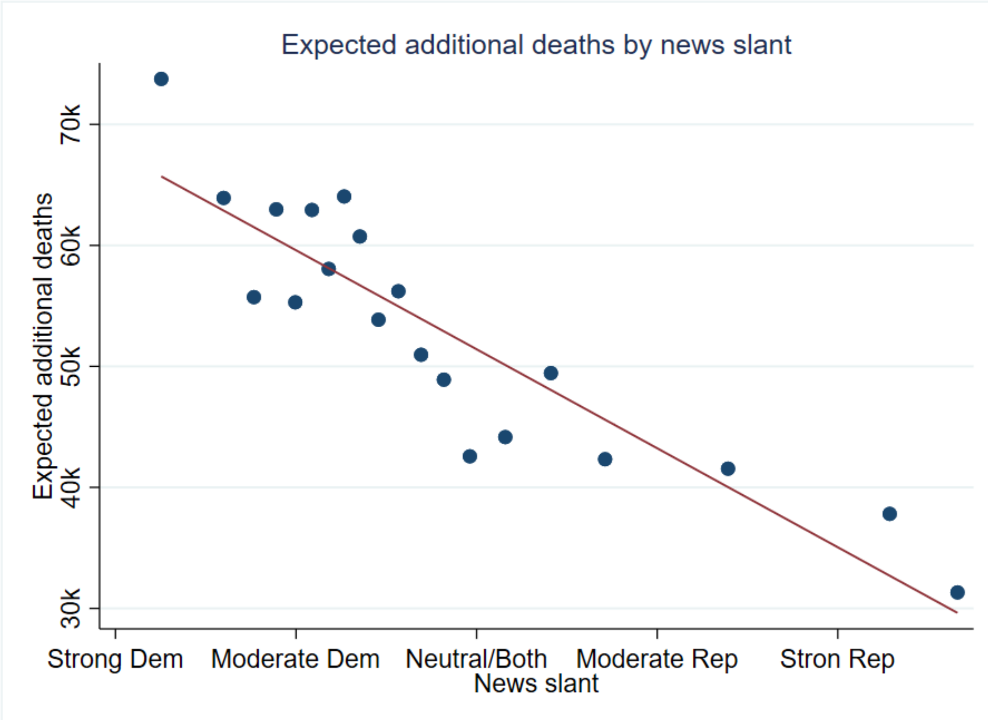
Fact IV: Media consumption predicts the perception of the Covid-19 crisis

In the fifth wave of the survey (week of May 18th), we asked respondents to forecast the COVID-19 death rate in the U.S. by the end of the year, after presenting them with the latest official death rate from the CDC and asked for their judgment on how the government handled the crisis[32]. Our goal was to understand the impact of partisanship on perceptions, partially controlling for a possible information gap by providing the latest official statistics[33].

[32]The questions asked: *By May 17, the U.S. Centers for Disease Control and Prevention (CDC) stated that about 90,000 Americans have so far died from COVID-19 (coronavirus). In addition to this, how many more Americans do you think will die by the end of this year due to coronavirus?*

[33]Gaines et al. (2007) studies a similar setting showing results of a survey where Americans were asked to state the need and support for the Iraqi war in 2003: while the majority of all respondents thought it was unlikely that the U.S. would ever find weapons of mass destruction, Democrats were more likely to conclude that they simply did not exist while Republicans were more likely to state that they believed the Iraqi government moved or destroyed the weapons.

Figure OA4: Expected additional death by news slant: Binscatter, controlling for a set of controls.



Notes: The figure shows a binned scatterplot in which the x-axis variable (news slant) is grouped into equal-sized bins, and the means of the x- and y-axis within each bin are computed. The plot controls for the set of variables described in the notes of table OA5.

Table OA5: News consumption predicts different perception of the pandemic

	(1) Expected additional deaths	(2) Expected additional deaths	(3) Expected additional deaths
Index news slant	-51,825***		
	(5,037)		
Democratic leaning news		14,624***	14,393***
		(2,790)	(2,799)
Republican leaning news		-15,305***	-14,930***
		(2,763)	(2,771)
Any direct shock Apr-May	4,772*	5,727**	5,168*
	(2,860)	(2,825)	(2,782)
Few COVID-19 news			-7,030***
			(2,056)
Constant	54,109***	30,236***	36,097***
	(9,912)	(10,110)	(10,502)
Observations	1,184	1,184	1,184
R-squared	0.195	0.210	0.218
Controls	Yes	Yes	Yes
Mean dep. var.	49827	49827	49827

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In col (1)-(3) the outcome is the expected number of additional deaths from COVID-19 that the respondents were expecting would occurr in the U.S., between May and the end of the year. All regressions take into account population survey weights and the sampling procedure. The control variables are specified in the Appendix.

OA1.1.5 Robustness checks

Balance table

Table OA6: Balance table.

Variable	(1) Control group	(2) Treatment group	(3) Difference
Woman	0.483	0.479	-0.004
	(0.500)	(0.500)	(0.029)
Age: 18-29	0.207	0.216	0.008
	(0.406)	(0.412)	(0.024)
Age: 30-44	0.282	0.286	0.004
	(0.450)	(0.452)	(0.026)
Age: 45-59	0.219	0.224	0.006
	(0.414)	(0.417)	(0.024)
High school	0.150	0.159	0.009
	(0.357)	(0.366)	(0.021)
Some college	0.392	0.400	0.008
	(0.488)	(0.490)	(0.028)
Bachelor +	0.424	0.414	-0.010
	(0.495)	(0.493)	(0.029)
II income q	0.188	0.190	0.002
	(0.391)	(0.392)	(0.023)
III income q	0.189	0.209	0.019
	(0.392)	(0.407)	(0.023)
IV income q	0.194	0.219	0.025
	(0.396)	(0.414)	(0.023)
V income q	0.215	0.191	-0.024
	(0.411)	(0.394)	(0.023)
Financial hardship	0.287	0.255	-0.032
	(0.453)	(0.436)	(0.026)
African American	0.100	0.095	-0.005
	(0.300)	(0.293)	(0.017)
Hispanic	0.150	0.152	0.002
	(0.357)	(0.359)	(0.021)
Other Race	0.116	0.128	0.012
	(0.320)	(0.334)	(0.019)
Cohabitating	0.626	0.614	-0.013
	(0.484)	(0.487)	(0.028)
Parent of minor	0.263	0.286	0.024
	(0.440)	(0.452)	(0.026)
Caring responsibilities	0.152	0.169	0.017
	(0.359)	(0.375)	(0.021)
Not in the labor force	0.256	0.248	-0.008
	(0.437)	(0.432)	(0.025)
Unemployed in Feb	0.059	0.053	-0.005
	(0.235)	(0.225)	(0.013)
Midwest	0.268	0.257	-0.011
	(0.443)	(0.437)	(0.026)
South	0.357	0.352	-0.006
	(0.480)	(0.478)	(0.028)
West	0.235	0.234	-0.000
	(0.424)	(0.424)	(0.025)
Metropolitan area	0.856	0.876	0.019
	(0.351)	(0.330)	(0.020)
Government provided health	0.359	0.338	-0.021
	(0.480)	(0.473)	(0.028)
No health insurance	0.073	0.078	0.004
	(0.261)	(0.268)	(0.015)
Population density in ZCTA	3,895.589	3,765.419	-130.169
	(9,688.460)	(8,374.398)	(525.591)
Catholic	0.171	0.191	0.020
	(0.377)	(0.394)	(0.022)
Protestant	0.276	0.278	0.002
	(0.447)	(0.448)	(0.026)
Any direct shock Apr-Jun	0.276	0.295	0.019
	(0.447)	(0.456)	(0.026)
log county cases/100,000	6.117	6.129	0.012
	(0.954)	(0.957)	(0.055)
Var consumer spending by	-0.097	-0.106	-0.009
	(0.104)	(0.120)	(0.007)
Observations	613	580	1,442

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. OLS regression coefficients of the balance table for the survey experiment.

Testing for Covid-19 Vs contracting Covid-19

Table OA7: Treated respondents are more likely to test for Covid-19, but not contract the virus.

	(1)	(2)	(3)	(4)
	ITT	ITT	ITT	ATT (2 stage)
	Respondent tested for COVID	Respondent had COVID	Respondent had COVID	Respondent had COVID
Treated	0.0667**	0.0132	0.0101	
	(0.0308)	(0.0115)	(0.00950)	
Correct deaths				0.0815
				(0.0865)
Constant	-0.0498	0.0169**	-0.195***	-0.263*
	(0.274)	(0.00705)	(0.0723)	(0.144)
Observations	987	990	990	960
R-squared	0.099	0.003	0.080	0.011
Demographics	Yes	No	Yes	Yes
Covid shock	Yes	No	Yes	Yes
News	Yes	No	Yes	Yes
Mean dep. var.	0.320	0.0223	0.0223	0.0223

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In col (1) the outcome is whether respondents declared to have been tested for Covid-19 between June and October; in cols (2-4), the outcomes is whether respondents declared to have contracted Covid-19 between June and October. Cols (1-3) report the ITT, while col (4) reports the second stage results instrumenting knowledge accuracy with the treatment assignment. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

Placebo institutions

Table OA8: The treatment has no effect on trust in placebo institutions.

	(1)	(2)	(3)	(4)	(5)	(6)
	ITT Incr. trust in scientific comm.	ATT (2 stage) Incr. trust in scientific comm.	ITT Incr. trust in healthcare inst. (index)	ATT (2 stage) Incr. trust in healthcare inst. (index)	ITT Incr. trust in financial inst. (index)	ATT (2 stage) Incr. trust in financial inst. (index)
Treated	0.0374		0.0307		-0.00108	
	(0.0352)		(0.0216)		(0.0117)	
Correct deaths		0.320		0.220		-0.0264
		(0.299)		(0.188)		(0.0994)
Constant	0.346	0.0103	0.257	0.0791	0.147*	0.196
	(0.266)	(0.373)	(0.192)	(0.259)	(0.0887)	(0.131)
Observations	986	957	988	959	988	959
R-squared	0.173	0.064	0.148		0.127	0.117
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Covid shock	Yes	Yes	Yes	Yes	Yes	Yes
News	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.205	0.205	0.168	0.168	0.0430	0.0430

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In col (1-2), the outcome is whether respondents have increased trust in the scientific community between April and October; in cols (3-4), whether respondents have increased trust in the healthcare institutions (hospitals and medical staff, and health insurance companies) measured with an index between April and October; in cols (5-6), whether they have increased trust in the economic institutions (banks and financial institutions and the private sector) measured with an index between April and October. Odd columns report the ITT, while even columns report the second stage results instrumenting knowledge accuracy with the treatment assignment. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

Table OA9: The treatment has no effect on the perception of politicization of placebo institutions.

	(1) ITT Scientific comm. not infl. by politics	(2) ATT (2 stage) Scientific comm. not infl. by politics	(3) ITT Healthcare inst. not infl. by politics (index)	(4) ATT (2 stage) Healthcare inst. not infl. by politics (index)	(5) ITT Financial inst. not infl. by politics (index)	(6) ATT (2 stage) Financial inst. not infl. by politics (index)
Treated	0.0429		0.0160		0.00658	
	(0.0268)		(0.0152)		(0.00790)	
Correct deaths		0.375		0.156		0.0480
		(0.246)		(0.117)		(0.0663)
Constant	-0.0144	-0.364	0.251**	0.0268	0.0716	0.0374
	(0.196)	(0.353)	(0.123)	(0.153)	(0.0597)	(0.0687)
Observations	990	960	990	960	990	960
R-squared	0.105		0.101		0.142	0.100
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Covid shock	Yes	Yes	Yes	Yes	Yes	Yes
News	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.137	0.137	0.0708	0.0708	0.0184	0.0184

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In col (1-2), the outcome is whether respondents believe that the scientific community is not influenced by politics; in cols (3-4), whether healthcare institutions (hospitals and medical staff, and health insurance companies) measured with an index are not influenced by politics; in cols (5-6), whether economic institutions (banks and financial institutions and the private sector) measured with an index are not influenced by politics. Odd columns report the ITT, while even columns report the second stage results instrumenting knowledge accuracy with the treatment assignment. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

Controlling for interpersonal trust

Table OA10: Controlling for interpersonal trust does not change the results.

	(1) ITT Incr. trust in CDC	(2) ATT (2 stage) Incr. trust in CDC	(3) ITT CDC not infl. by politics	(4) ATT (2 stage) CDC not infl. by politics	(5) ITT Tested for Covid19	(6) ATT (2 stage) Tested for Covid19
Treated	0.0596**		0.0596**		0.0650**	
	(0.0291)		(0.0269)		(0.0302)	
Correct deaths		0.495*		0.547*		0.449*
		(0.296)		(0.280)		(0.269)
Most people	-0.0239	-0.0551	-0.0343	-0.0565	0.0699	0.0593
can be trusted	(0.0388)	(0.0485)	(0.0350)	(0.0447)	(0.0476)	(0.0458)
Can't be too	-0.0781**	-0.0902**	-0.0238	-0.0386	-0.0187	-0.0177
careful with people	(0.0380)	(0.0409)	(0.0305)	(0.0324)	(0.0445)	(0.0420)
Constant	0.423	-0.0589	-0.0237	-0.625*	-0.0583	-0.404
	(0.283)	(0.418)	(0.187)	(0.358)	(0.272)	(0.407)
Observations	978	949	989	960	986	958
R-squared	0.095		0.137		0.104	
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Covid shock	Yes	Yes	Yes	Yes	Yes	Yes
News	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.162	0.162	0.113	0.113	0.331	0.331

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In col (1-2), the outcome is whether respondents (in October 2020) report having increased their confidence in the CDC since the beginning of the pandemic; in cols (3-4), the outcome is whether respondents (in October 2020) report that the CDC is not influenced by politics; in cols (5-6), whether respondents report having been tested for Covid-19. Odd columns report the ITT, while even columns report the second stage results instrumenting knowledge accuracy with the treatment assignment. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

Alternative specifications

Table OA11: Repeating the analyses using a probit model does not affect results.

VARIABLES	(1) ITT Correct U.S. rank Oct	(2) ATT (2 stage) Correct U.S. rank Oct	(6) ITT Incr. trust in CDC	(7) ATT (2 stage) Incr. trust in CDC	(11) ITT CDC not infl. by politics	(12) ATT (2 stage) CDC not infl. by politics	(16) ITT Tested for Covid-19	(17) ATT (2 stage) Tested for Covid-19
Treated	0.144*		0.272**		0.394**		0.199**	
	(0.0812)		(0.121)		(0.168)		(0.0901)	
Correct deaths		1.096**		1.567***		1.923***		1.222**
		(0.517)		(0.414)		(0.314)		(0.490)
Constant	1.481	0.235	0.102	-1.504	-2.999**	-3.838***	-1.591*	-2.241***
	(1.190)	(1.325)	(1.125)	(0.922)	(1.165)	(0.825)	(0.835)	(0.819)
Observations	977	953	964	938	984	955	984	951
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covid shock	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
News	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.545	0.545	0.156	0.156	0.113	0.113	0.320	0.320

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
probit regression coefficients. In col (1-2), the outcome is = 1 if the respondent correctly ranked the U.S. in terms of deaths in the world; in cols(3-4), the outcome is whether respondents (in October 2020) report having increased their confidence in the CDC since the beginning of the pandemic; in cols (5-6), the outcome is whether respondents (in October 2020) report that the CDC is not influenced by politics; in cols (7-8), whether respondents report having been tested for Covid-19. Odd columns report the ITT, while even columns report the second stage results instrumenting knowledge accuracy with the treatment assignment. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

Table OA12: Repeating the analyses considering alternative measures of the main outcomes confirms the results.

	(1) ITT Decr. trust in CDC	(2) ATT (2 stage) Decr. trust in CDC	(3) ITT CDC very infl. by politics	(4) ATT (2 stage) CDC very infl. by politics	(5) ITT CDC indep scale	(6) ATT (2 stage) CDC indep scale
Treated	-0.0209		-0.0658**		0.199***	
	(0.0363)		(0.0298)		(0.0735)	
Correct deaths		-0.0751		-0.449		1.644**
		(0.280)		(0.293)		(0.791)
Constant	-0.416	-0.322	0.0238	0.509	1.797***	-0.0216
	(0.270)	(0.415)	(0.265)	(0.386)	(0.593)	(1.083)
Observations	978	949	983	954	983	954
R-squared	0.110	0.110	0.106		0.137	
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Covid shock	Yes	Yes	Yes	Yes	Yes	Yes
News	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.280	0.280	0.240	0.240	2.239	2.239

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In col (1-2), the outcome is whether respondents (in October 2020) report having decreased their confidence in the CDC since the beginning of the pandemic; in cols (3-4), the outcome is whether respondents (in October 2020) report that the CDC is very influenced by politics; in cols (5-6), the perception of whether the CDC is influenced by politics measured with a 4-point likert scale (from "very influenced", to "not influenced at all"). Odd columns report the ITT, while even columns report the second stage results instrumenting knowledge accuracy with the treatment assignment. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

Validity of meta-cognition

Table OA13: Self-reported variation in confidence in an institution predicts the variation in trust recorded in previous surveys in time.

	(1) Variation in confidence in the scientific community	(2) Variation in confidence in the scientific community	(3) Variation in confidence in hospitals and medical staff	(4) Variation in confidence in hospitals and medical staff	(5) Variation in confidence in health insurance companies	(6) Variation in confidence in health insurance companies
Self-reported var. in trust in sci. comm.	0.198***	0.153***				
	(0.0412)	(0.0394)				
Self-reported var. in trust in hospitals			0.155***	0.150***		
			(0.0438)	(0.0437)		
Self-reported var. in trust in health insur.					0.0863	0.0935*
					(0.0554)	(0.0540)
Constant	-0.505***	0.0460	-0.478***	-0.348	-0.279***	-0.0468
	(0.0871)	(0.476)	(0.0967)	(0.404)	(0.107)	(0.513)
Observations	1,064	1,064	1,066	1,066	1,060	1,060
R-squared	0.042	0.093	0.017	0.072	0.006	0.095
Demographics	No	Yes	No	Yes	No	Yes
Covid shock	No	Yes	No	Yes	No	Yes
News	No	Yes	No	Yes	No	Yes
Mean dep. var.	-0.0873	-0.0873	-0.139	-0.139	-0.113	-0.113

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In col (1-2), the outcome is the variation in confidence in the scientific community between the surveys run in April and October; in cols (3-4), the outcome is the variation in confidence in hospitals and medical staff between the surveys run in April and October; in cols (5-6),the outcome is the variation in confidence in health insurance companies between the surveys run in April and October. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

Table OA14: Self-reported variation in confidence in an institution predicts the variation in trust recorded in previous surveys in time.

	(1) Variation in confidence in banks and financial inst.	(2) Variation in confidence in banks and financial inst.	(3) Variation in confidence in the private sector	(4) Variation in confidence in the private sector
Self-reported var. in trust in fin. inst.	0.202***	0.191***		
	(0.0632)	(0.0577)		
Self-reported var. in trust in priv. sect.			0.221***	0.184***
			(0.0541)	(0.0528)
Constant	-0.555***	-1.139***	-0.480***	0.209
	(0.125)	(0.407)	(0.107)	(0.393)
Observations	1,060	1,060	1,061	1,061
R-squared	0.020	0.104	0.032	0.106
Demographics	No	Yes	No	Yes
Covid shock	No	Yes	No	Yes
News	No	Yes	No	Yes
Mean dep. var.	-0.163	-0.163	-0.0719	-0.0719

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In col (1-2), the outcome is the variation in confidence in banks and financial institutions between the surveys run in April and October; in cols (3-4), the outcome is the variation in confidence in the private sector between the surveys run in April and October. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

OA1.1.6 Additional results

Table OA15: Treated respondents are more likely to have adopted at least a preventive behavior, but no significant effects on mask-wearing.

	(1) ITT Adopts any preventive behav.	(2) ITT Adopts any preventive behav.	(3) ATT (2 stage) Adopts any preventive behav.	(4) ITT Covers mouth in public	(5) ITT Covers mouth in public	(6) ATT (2 stage) Covers mouth in public
Treated	0.0241**	0.0261***		0.0215	0.0262	
	(0.0108)	(0.00979)		(0.0289)	(0.0214)	
Correct deaths			0.152*			0.183
			(0.0845)			(0.181)
Constant	0.976***	1.056***	0.916***	0.882***	0.794***	0.503*
	(0.00886)	(0.0926)	(0.139)	(0.0228)	(0.163)	(0.278)
Observations	986	986	958	986	986	958
R-squared	0.045	0.122		0.053	0.217	0.207
Demographics	No	Yes	Yes	No	Yes	Yes
Covid shock	No	Yes	Yes	No	Yes	Yes
News	No	Yes	Yes	No	Yes	Yes
Mean dep. var.	0.984	0.984	0.984	0.881	0.881	0.881

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In cols (1-3) the outcome is whether respondents declared to have adopted any preventive behavior in June-October; in cols (4-6), the outcomes is whether respondents declared to have covered their mouth when in public in June-October. Cols (1-2, 4-5) report the ITT, while cols (3,6) report the second stage results instrumenting knowledge accuracy with the treatment assignment. All regressions take into account population survey weights and the sampling procedure. The control variables are reported in the Appendix.

OA1.1.7 Heterogeneous effects by media consumption

Table OA16: There are no heterogeneous effects by media consumption

	(1) Correctly estimated deaths	(2) ITT Incr. trust in CDC	(3) ATT (2 stage) Incr. trust in CDC	(4) ITT CDC not infl. by politics	(5) ATT (2 stage) CDC not infl. by politics	(6) ITT Respondent tested COVID	(7) ATT (2 stage) Respondent tested COVID
Treated	0.150***	0.0722*		0.0866**		0.0252	
	(0.0332)	(0.0376)		(0.0375)		(0.0349)	
Rep news	-0.0228	-0.115***	-0.111	-0.00325	0.252	-0.0699	-0.291
	(0.0569)	(0.0375)	(0.279)	(0.0386)	(0.219)	(0.0610)	(0.332)
Dem news	0.0718	0.0250	0.326	0.0358	-0.227	-0.0852*	-0.550
	(0.0469)	(0.0495)	(0.465)	(0.0451)	(0.488)	(0.0477)	(0.574)
Treated*Dem news	-0.0796	-0.0593		-0.0218		0.116	
	(0.0608)	(0.0737)		(0.0626)		(0.0714)	
Treated*Rep news	-0.0226	0.0211		-0.0999*		0.0644	
	(0.0659)	(0.0625)		(0.0596)		(0.0581)	
Correct deaths			0.742		0.520		0.1000
			(0.458)		(0.367)		(0.417)
Correct deaths*Rep news			0.0979		-0.864		0.843
			(0.904)		(0.631)		(1.012)
Correct deaths*Dem news			-0.903		0.560		1.242
			(1.141)		(1.183)		(1.426)
Constant	0.330**	0.112	0.0635	0.245*	0.276	0.384**	0.683**
	(0.144)	(0.155)	(0.225)	(0.139)	(0.264)	(0.166)	(0.322)
Observations	1,147	978	820	990	828	987	826
R-squared	0.162	0.092		0.137		0.104	
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covid shock	Yes	Yes	Yes	Yes	Yes	Yes	Yes
News	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	0.330	0.156	0.156	0.113	0.113	0.320	0.320

OA2 Experiment II

Figure OA5: Media coverage about the EPA

'It would be devastating': inside Trump's plan to destroy the EPA

Trump has made campaign promises to toss crucial environmental regulations - including dismantling the federal body with the most power to tackle the climate crisis

POLITICO

How Trump could reshape EPA on 'Day 1'

By FRANCISCO "A.J." CAMACHO | 07/18/2024 06:16 PM EDT

CNN Climate Solutions Weather

(CNN) — Environmental Protection Agency administrator Michael Regan is bullish on his agency's prospects against the harshest headwinds in its history, as Republicans, anti-climate industry and an ultra-conservative Supreme Court take aim at its core mission.

"I feel extremely confident in the product we put out," Regan told CNN in an interview at a Climate Action Campaign event on Tuesday, just hours after Republican attorneys general and industry groups asked the Supreme Court to temporarily block the EPA from enforcing new rules that aim to curb planet-warming emissions from power plants.

The EPA's new rules will compel some coal and natural gas power plants to either cut or capture 90% of their climate pollution by 2032. The rules are expected to reduce the carbon dioxide emissions from the sector by 75% compared to a peak in 2005, and they are a huge part of President Joe Biden's strategy to quickly cut US planet-warming pollution.

Balance table

Table OA17: Balance table.

Variable	(1) Mean T1	(2) Mean T2	(3) Mean T3	(4) T1 vs T2	(5) T1 vs T3	(6) T2 vs T3
Knows EPA	0.731	0.679	0.714	0.052*	0.017	0.035
	(0.444)	(0.467)	(0.452)	(0.028)	(0.027)	(0.028)
Rep news	0.144	0.183	0.193	-0.039*	-0.049**	0.010
	(0.352)	(0.387)	(0.395)	(0.023)	(0.023)	(0.023)
Dem news	0.190	0.185	0.174	0.005	0.017	-0.012
	(0.393)	(0.389)	(0.379)	(0.024)	(0.024)	(0.023)
Woman	0.521	0.555	0.533	-0.034	-0.012	-0.022
	(0.500)	(0.497)	(0.499)	(0.030)	(0.030)	(0.030)
African	0.125	0.118	0.116	0.007	0.009	-0.002
American	(0.331)	(0.323)	(0.321)	(0.020)	(0.020)	(0.019)
Hispanic	0.152	0.178	0.161	-0.026	-0.009	-0.017
	(0.359)	(0.383)	(0.368)	(0.023)	(0.022)	(0.023)
Not white,	0.085	0.100	0.072	-0.015	0.013	-0.028*
black or hisp	(0.279)	(0.300)	(0.258)	(0.018)	(0.016)	(0.017)
Age: 18-29	0.142	0.147	0.163	-0.005	-0.020	0.016
	(0.350)	(0.354)	(0.370)	(0.022)	(0.022)	(0.022)
Age: 30-44	0.269	0.312	0.297	-0.043	-0.028	-0.015
	(0.444)	(0.464)	(0.457)	(0.028)	(0.027)	(0.028)
Age: 45-59	0.246	0.229	0.242	0.017	0.005	0.013
	(0.431)	(0.420)	(0.428)	(0.026)	(0.026)	(0.025)
HS	0.196	0.171	0.195	0.026	0.001	0.024
	(0.397)	(0.376)	(0.397)	(0.024)	(0.024)	(0.023)
College	0.381	0.388	0.399	-0.008	-0.018	0.011
	(0.486)	(0.488)	(0.490)	(0.030)	(0.030)	(0.029)
BA or above	0.371	0.385	0.343	-0.014	0.028	-0.041
	(0.484)	(0.487)	(0.475)	(0.030)	(0.029)	(0.029)
$30k-$60k	0.252	0.232	0.233	0.020	0.019	0.000
	(0.435)	(0.423)	(0.423)	(0.026)	(0.026)	(0.025)
$60k-$100k	0.213	0.269	0.265	-0.055**	-0.051**	-0.004
	(0.410)	(0.444)	(0.442)	(0.026)	(0.026)	(0.027)
¿$100k	0.313	0.296	0.284	0.018	0.029	-0.011
	(0.464)	(0.457)	(0.452)	(0.028)	(0.028)	(0.027)
Midwest	0.244	0.269	0.274	-0.024	-0.029	0.005
	(0.430)	(0.444)	(0.446)	(0.027)	(0.027)	(0.027)
South	0.348	0.301	0.347	0.047	0.001	0.046
	(0.477)	(0.459)	(0.476)	(0.029)	(0.029)	(0.028)
West	0.248	0.305	0.252	-0.057**	-0.004	-0.053*
	(0.432)	(0.461)	(0.435)	(0.027)	(0.026)	(0.027)
Employed	0.612	0.633	0.628	-0.022	-0.016	-0.005
	(0.488)	(0.482)	(0.484)	(0.030)	(0.030)	(0.029)
Unemployed	0.056	0.071	0.068	-0.015	-0.012	-0.003
	(0.230)	(0.257)	(0.252)	(0.015)	(0.015)	(0.015)
Widowed,	0.535	0.501	0.503	0.034	0.032	0.002
divorced,	(0.499)	(0.500)	(0.500)	(0.031)	(0.030)	(0.030)
Parent of minor	0.321	0.376	0.358	-0.055*	-0.037	-0.018
	(0.467)	(0.485)	(0.480)	(0.029)	(0.029)	(0.029)
HH internet	0.937	0.936	0.905	0.000	0.031*	-0.031*
access via	(0.244)	(0.244)	(0.293)	(0.015)	(0.016)	(0.016)
State has Rep.	0.387	0.383	0.417	0.004	-0.030	0.034
gov.	(0.487)	(0.487)	(0.493)	(0.030)	(0.030)	(0.029)
State has Rep.	0.387	0.383	0.417	0.004	-0.030	0.034
gov.	(0.487)	(0.487)	(0.493)	(0.030)	(0.030)	(0.029)
Metropolitan	0.885	0.838	0.846	0.046**	0.038*	0.008
area	(0.320)	(0.368)	(0.361)	(0.021)	(0.021)	(0.022)
catholic	0.196	0.162	0.191	0.035	0.005	0.030
	(0.397)	(0.368)	(0.394)	(0.023)	(0.024)	(0.023)
protestant	0.231	0.243	0.270	-0.012	-0.039	0.027
	(0.422)	(0.429)	(0.444)	(0.026)	(0.026)	(0.026)
Observations	520	551	559	1,071	1,079	1,110

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. OLS regression coefficients of the balance table for the survey experiment.

Table OA18: Knowing the EPA is correlated with higher levels of trust

	(1) EPA improves lives	(2) EPA improves lives	(3) EPA independence scale	(4) EPA independence scale	(5) Support for EPA's involvement	(6) Support for EPA's involvement
Knows EPA	0.420***	0.285***	-0.147**	-0.176**	10.47***	7.634***
	(0.0684)	(0.0734)	(0.0732)	(0.0784)	(1.467)	(1.579)
Constant	4.516***	4.216***	3.481***	4.069***	53.36***	55.20***
	(0.0550)	(0.228)	(0.0593)	(0.252)	(1.226)	(4.685)
Observations	1,626	1,626	1,613	1,613	1,580	1,580
R-squared	0.035	0.160	0.004	0.085	0.033	0.148
Demographics	No	Yes	No	Yes	No	Yes
News	No	Yes	No	Yes	No	Yes
Mean dep. var.	4.788	4.788	3.360	3.360	60.61	60.61

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In cols (1-2), the outcome reports whether the EPA recommends policies and regulations that improve or worsen the life of the respondents and their loved ones on a 7-point likert scale from "Always worsen" to "Always improve"; in cols (3-4), the outcome reports whether the EPA is politicized versus independent on a 7-point likert scale from "Completely politicized" to "Completely independent"; in cols (5-6), the outcome is percent likelihood that the respondents would recommend the EPA to conduct a study to measure air pollution levels in their community. The control variables are reported in the Appendix.

OA2.1 Knowing the PI

Table OA19: Predictors of knowing the PI

	(1) Knows EPA
Rep news	-0.0576*
	(0.0306)
Dem news	0.00276
	(0.0266)
Woman	-0.139***
	(0.0211)
African American	-0.143***
	(0.0374)
Hispanic	-0.118***
	(0.0330)
Not white, black or hisp	-0.109***
	(0.0418)
Age: 18-29	-0.207***
	(0.0426)
Age: 30-44	-0.172***
	(0.0319)
Age: 45-59	-0.0990***
	(0.0302)
HS	0.156***
	(0.0590)
College	0.259***
	(0.0567)
BA or above	0.347***
	(0.0577)
\$30k-\$60k	-0.0121
	(0.0343)
\$60k-\$100k	0.0183
	(0.0351)
>\$100k	0.0479
	(0.0367)
Midwest	-0.0522
	(0.0349)
South	-0.0460
	(0.0364)
West	-0.101***
	(0.0340)
Employed	0.0115
	(0.0266)
Unemployed	-0.0356
	(0.0503)
Widowed, divorced,	0.0745***
separated, never married	(0.0244)
Parent of minor	-0.00250
	(0.0265)
HH internet access	0.0591
	(0.0455)
State has Rep. gov.	-0.00998
	(0.0251)
Metropolitan area	-0.0284
	(0.0308)
Catholic	-0.0241
	(0.0295)
Protestant	0.0511**
	(0.0255)
Constant	0.661***
	(0.0828)

Instrumental Variable regressions

Table OA20: Instrumental Variable regressions instrumenting information accuracy with the treatment status.

	(1)	(2)	(3)	(4)	(5)	(6)
	ATT (2 stage) EPA improves lives	ATT (2 stage) EPA improves lives	ATT (2 stage) EPA independence scale	ATT (2 stage) EPA independence scale	ATT (2 stage) Support for EPA's involvement	ATT (2 stage) Support for EPA's involvement
T1: Low search costs	0.907	0.474	0.668	0.302	18.26	8.417
& high value info	(0.723)	(0.767)	(0.744)	(0.815)	(15.79)	(16.84)
T3: Low search costs	0.588	0.341	0.0740	-0.199	10.57	4.308
& low value info	(0.669)	(0.700)	(0.687)	(0.741)	(14.44)	(15.26)
Constant	4.530***	4.267***	3.211***	3.924***	55.16***	58.09***
	(0.259)	(0.338)	(0.266)	(0.358)	(5.636)	(7.560)
Observations	1,626	1,626	1,613	1,613	1,580	1,580
R-squared		0.154		0.079		0.136
Demographics	No	Yes	No	Yes	No	Yes
News	No	Yes	No	Yes	No	Yes
Mean dep. var.	4.788	4.788	3.360	3.360	60.61	60.61
p-val correct T1 = correct T3	0.117	0.492	0.00517***	0.0169**	0.0712*	0.314

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS IV regression coefficients reporting the second stage results. Correctly answering the information question about the AQI score or the location of the closest EPA office (according to the treatment group) is instrumented with the treatment group. The two treatment groups in the table are: the low-search cost and high-value information group and the low-search cost but low-value information group. The excluded group is the high-search cost and high-information group. In cols (1-2), the outcome reports whether the EPA recommends policies and regulations that improve or worsen the life of the respondents and their loved ones on a 7-point likert scale from "Always worsen" to "Always improve"; in cols (3-4), the outcome reports whether the EPA is politicized versus independent on a 7-point likert scale from "Completely politicized" to "Completely independent"; in cols (5-6), the outcome is percent likelihood that the respondents would recommend the EPA to conduct a study to measure air pollution levels in their community. A t-test comparing the coefficient associated with the two treatment groups allows for comparing the effects of being exposed to high-value vs. low-value information on trust. The control variables are reported in the Appendix.

OA2.1.1 Robustness checks

Placebo outcomes

Table OA21: Repeating the analyses with a placebo institution, the IRS.

	(1)	(2)	(3)	(4)
	ITT IRS improves lives	ITT IRS improves lives	ITT IRS independence scale	ITT IRS independence scale
T1: Low search costs	0.120	0.0710	0.183*	0.113
& high value info	(0.0808)	(0.0789)	(0.0946)	(0.0923)
T2: High search costs	0.0269	0.0201	0.101	0.0998
& high value info	(0.0802)	(0.0781)	(0.0934)	(0.0917)
Constant	3.515***	3.835***	3.117***	3.941***
	(0.0574)	(0.235)	(0.0667)	(0.263)
Observations	1,628	1,628	1,606	1,606
R-squared	0.002	0.083	0.004	0.082
Demographics	No	Yes	No	Yes
News	No	Yes	No	Yes
Mean dep. var.	3.560	3.560	3.197	3.197

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In cols (1-2), the outcome reports whether the IRS recommends policies and regulations that improve or worsen the life of the respondents and their loved ones on a 7-point likert scale from “Always worsen” to “Always improve”; in cols (3-4), the outcome reports whether the IRS is politicized versus independent on a 7-point likert scale from “Completely politicized” to “Completely independent”. The control variables are reported in the Appendix.

Alternative specification

Table OA22: Repeating the analyses with alternative specifications (probit and ordered probit) does not affect results.

	(1) Knowledge accuracy	(2) Knowledge accuracy	(3) ITT EPA improves lives	(4) ITT EPA improves lives	(5) ITT EPA idenpendence scale	(6) ITT EPA idenpendence scale
T1: Low search costs	0.0700	0.0462	0.107*	0.0448	0.171***	0.150**
& high value info	(0.0858)	(0.0874)	(0.0628)	(0.0640)	(0.0634)	(0.0647)
T2: High search costs	-0.490***	-0.486***	0.0279	0.00432	0.118*	0.126**
& high value info	(0.0829)	(0.0842)	(0.0622)	(0.0625)	(0.0613)	(0.0617)
Constant cut1			-2.010***	-1.909***	-1.228***	-1.731***
			(0.0767)	(0.187)	(0.0544)	(0.198)
Constant cut2			-1.529***	-1.392***	-0.495***	-0.959***
			(0.0600)	(0.185)	(0.0479)	(0.194)
Constant cut3			-0.962***	-0.790***	0.242***	-0.188
			(0.0507)	(0.182)	(0.0472)	(0.193)
Constant cut4			-0.259***	-0.0322	0.903***	0.500***
			(0.0471)	(0.181)	(0.0501)	(0.192)
Constant cut5			0.388***	0.678***	1.503***	1.127***
			(0.0476)	(0.182)	(0.0579)	(0.196)
Constant cut6			1.502***	1.888***	2.267***	1.921***
			(0.0596)	(0.187)	(0.0877)	(0.199)
Constant	-0.0260	-0.284				
	(0.0597)	(0.264)				
Observations	1,399	1,399	1,626	1,626	1,613	1,613
Demographics	No	Yes	No	Yes	No	Yes
News	No	Yes	No	Yes	No	Yes
Mean dep. var.	0.416	0.416	4.788	4.788	3.360	3.360

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
OLS regression coefficients. In cols (1-2), the outcome is whether the respondent correctly answered a question asking about the AQI score in their zipcode or the location of the closest EPA office (according to the treatment status); in cols (3-4), the outcome reports whether the EPA recommends policies and regulations that improve or worsen the life of the respondents and their loved ones on a 7-point likert scale from “Always worsen” to “Always improve”; in cols (5-6), the outcome reports whether the EPA is politicized versus independent on a 7-point likert scale from “Completely politicized” to “Completely independent”. The control variables are reported in the Appendix.

Heterogeneous effects by EPA’s office location

Table OA23: Residing in States that do not host an EPA office does not affect consumers

	(1) Knowledge accuracy	(2) Knowledge accuracy	(3) ITT EPA improves lives	(4) ITT EPA improves lives	(5) ITT EPA idenpen-dence scale	(6) ITT EPA idenpen-dence scale	ITT Support for EPA's involvement	ITT Support for EPA's involvement
T3 (Low info)	0.186***	0.174***						
	(0.0403)	(0.0409)						
T3*State has EPA office	0.0143	0.0291	-0.0301	0.0601	0.00933	0.0529	1.184	3.081
	(0.0616)	(0.0621)	(0.165)	(0.156)	(0.169)	(0.165)	(3.439)	(3.295)
State has EPA office	0.0983**	0.0780*	0.178	0.0251	0.0795	-0.00967	1.919	-0.743
	(0.0395)	(0.0438)	(0.118)	(0.123)	(0.121)	(0.122)	(2.405)	(2.479)
Constant	0.255***	0.227**	4.712***	4.583***	3.368***	4.184***	59.59***	64.05***
	(0.0264)	(0.112)	(0.0834)	(0.275)	(0.0891)	(0.294)	(1.725)	(5.744)
Observations	985	985	1,109	1,109	1,099	1,099	1,075	1,075
R-squared	0.056	0.092	0.023	0.170	0.006	0.092	0.003	0.133
Demographics	No	No	No	Yes	No	Yes	No	Yes
News	No	No	No	Yes	No	Yes	No	Yes
Mean dep. var.	0.379	0.379	4.748	4.748	3.309	3.309	59.77	59.77