

# Facts in Context: Problem Perceptions, Numerical Information, and Policy Attitudes

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## Abstract

How does policy-relevant information change citizens' policy attitudes? Though giving numerical information about social conditions has been found, at times, to change policy attitudes, why it works (or doesn't) is poorly understood. I argue new or corrective information may not translate into policy-attitude change in part because it fails to instill a sense of need for change. *Perceived problem seriousness*, an affect-laden judgment about the acceptability of the status quo, may therefore be an important psychological mechanism through which information changes people's minds. To perceive a problem, conditions must seem worse than they ought to be. Previous research, however, presents numerical information without a point of reference from which citizens can base their judgments. By contextualizing facts with reference points from the past (time) as well as other countries (space), four survey experiments show that numerical information about a range of social problems can change policy attitudes by first changing their perceived seriousness.

## Keywords

policy attitudes, perceived problem seriousness, policy-relevant information, social problems, innumeracy

Policy attitudes are shaped by how citizens apply their values to what they know about real-world conditions and public policy (A. Campbell et al., 1960; Chong & Mullinix, 2019). For this reason, citizens' ignorance of real-world conditions is thought to stymie their support for government intervention (Hochschild, 2001; Thal, 2017). If only people knew, for instance, the poverty rate or the size of the Black population, then their policy attitudes would change in turn. But such efforts have yielded mixed results, and the reasons are unclear (Gilens, 2001, p. 391; Grigorieff et al., 2020; Hopkins et al., 2019; Lawrence & Sides, 2014, p. 6; Kuklinski et al., 2000). The centrality of information in both political science theory and real-world politics, and its inconsistent effects, points to an important gap in our understanding of how information works.

The classic theory of value-based reasoning would attribute these mixed results to failures to “arouse some minimal intensity of feeling” toward the policy goal (A. Campbell et al., 1960, p. 170). But what kind of feeling could explain when and why information changes policy attitudes? I propose and test a psychological mechanism—*perceived problem seriousness*—through which such change occurs. Perceived problem seriousness is an affect-laden judgment of how problematic or unacceptable a set of social conditions is. By integrating the cognition and affective reaction to social conditions, perceived problem seriousness is a potentially powerful motivator of support for government action

(Lodge & Taber, 2013, ch. 1; Weiner, 1980). It thus helps explain why some policy-relevant information persuades citizens to shift their policy attitudes while other information does not. To find evidence of this mechanism, I focus on a common form of policy-relevant information: numerical/statistical information.

Because social problems—the material conditions under which people live and work—are *harms at scale*, the more people harmed, the more serious the problem should be perceived to be. Thus, numerical information about the scale of a problem is one common way political and media elites try to change people's minds about policy. Experimental studies' attempts at doing so, however, are often unsuccessful (Hopkins et al., 2019; Thorson & Abdelaaty, 2022; Jørgensen & Osmundsen, 2022; Kuklinski et al., 2000; Lawrence & Sides, 2014). Why? Kuklinski et al. (2000, p. 806) argue information should explicitly show people their previous beliefs were incorrect, “hitting them between the eyes” with it to get them to update their policy attitudes. The numerous experiments in Hopkins et al. (2019) suggest even that does

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not consistently work. Instead, I argue two conditions must hold. First, the statistic must be clearly related to some harm. The population size of a given minority group, for example, is not clearly causing or describing a specific harm. Second, information changes policy attitudes insofar as it heightens the perceived seriousness of the problem that the policies being considered are meant to address.

What information raises how serious problems seem? Information that suggests conditions are not what they *should be*. In most cases, ordinary citizens are not policy experts (Delli Carpini & Keeter, 1996), so they are unlikely to hold strong expectations about what numerical level for a given problem is acceptable or normative. For example, is a poverty rate of 20% too high? How high is too high? Given this ambiguity, the persuasiveness of numerical information would benefit from being provided with a relevant benchmark to help citizens form such an expectation (Kahneman & Tversky, 1979, p. 277). For poverty, inequality, drug addiction, and other social problems, the past and other places provide benchmarks for making such judgments.

In two survey experiments embedded in a nationally representative survey and two in online convenience samples, I show that, when provided with a reference point, numerical information changes policy attitudes by changing the perceived seriousness of the underlying problem. This finding sheds new light on how policy-relevant information affects policy attitudes, and shows that contextualizing such information can help citizens evaluate social problems and subsequently support policy action. By identifying problem perception as a causal mediator in the formation of policy attitudes, this research helps fill the gap in our understanding of policy-relevant information effects (Gilens, 2001, p. 391).

## Social Problems and Policy-Relevant Information

Since social problems, by definition, affect large numbers of people, numerical information is part and parcel of judging their *scale*, their apparent spread across the polity. The numerical sizes of various policy-relevant groups, whether they are construed as victims (e.g., children in poverty, war casualties) or as threats (e.g., undocumented immigrants), are thus key facts in mass belief systems (Gaines et al., 2007). Though such numerical information about society is abundant, “notorious innumeracy” is pervasive (Sigelman & Niemi, 2001, p. 86). Most notoriously, people overestimate the sizes of minority groups, particularly when they feel threatened by minorities (Nadeau et al., 1993). They also overestimate the proportion of people in poverty and unemployment (Lawrence & Sides, 2014). Because policy attitudes are shaped by policy-relevant knowledge, correcting these misperceptions should have knock-on effects on support for policies directed at helping those minorities (Gilens, 2001; Hochschild, 2001; Nadeau et al., 1993).

Political scientists have used numerical information to correct misperceptions and change policy attitudes numerous times (Gilens, 2001; Hopkins et al., 2019; Hochschild, 2001; Jørgensen & Osmundsen, 2022; Kuklinski et al., 2000; Nyhan & Reifler, 2010; Thorson & Abdelaaty, 2022), but the anticipated knock-on effects have largely been elusive. Using a variety of numerical facts, including the average household income in the US, the percentage of Americans with four-year degrees, the unemployment rate, poverty rate, and the percentages of Americans that are white, Black, and Hispanic, Lawrence and Sides (2014) found no effects on policy attitudes. Kuklinski et al. (2000, p. 802) found no effect on welfare-policy attitudes, despite informing people of many facts, including the percentage of families on welfare, the percentage of welfare mothers who are on welfare for more than eight years, and the percentage of welfare families who are Black. Similarly, learning the number of war deaths in the Iraq War did nothing to change support for the war (Berinsky, 2007). Nor did correcting perceptions of the proportion of Americans who are immigrants change support for more permissive policy, no matter how far off their original misperception (Hopkins et al., 2019).

Two prominent studies, in contrast, find significant effects of numerical information about social conditions on related policy attitudes. Crucially, the information they provided came with a reference point. When Gilens (2001) showed participants that crime had decreased, their support for prison construction decreased. Similarly, Boudreau and MacKenzie (2018) showed Californian participants how income inequality in their state had increased over the prior 30 years, and found increased support for raising income taxes on the rich. These experiments suggest reference points can make a difference in how people interpret and assess statistical information about the status quo. If a mechanism by which information changes attitudes is the increased perceived seriousness of a policy-relevant problem, then we should consider what types of information change perceived problem seriousness. Decontextualized numerical facts often fail to do this for at least two reasons.

First, people do not know what to make of a statistic if it is not clearly related to a harm. Though many Americans may be wrong about what proportion of the population that is Hispanic, Black, or college-educated, for example, correcting these misperceptions does not necessarily evoke a problem demanding urgent action. While it may seem less costly to provide aid to Blacks if their population is smaller than you thought, there is no impending problem suggested by that fact. Second, for a given statistic to seem troubling requires a benchmark against which to compare it. For example, being told that the poverty rate is actually 20% even though you thought it was 15% does not mean that you now think poverty is a serious problem. You may have thought 15% wasn't that high to begin with or that 5 points more isn't much either.

Judging whether these numbers are problematic—and whether social problems are serious—may be helped by

knowing what the poverty rate *ought* to be. A point of reference is needed (Kahneman & Tversky, 1979, p. 277). A *contextualized* numerical fact is one that is given with a point of reference. Though people's ideal poverty rate (or unemployment- or college-dropout rate) may be 0, no countries achieve it, so it isn't a useful benchmark. What's more likely is that people consider how the rate has changed over time (e.g., the crime experiment in Gilens, 2001) or what it is in other countries (Aytaç, 2020). Static statistics may therefore benefit from temporal context (change over time) and spatial context (differences between places).

Depending on the reference point, context can be either *problem-amplifying*, making present conditions appear worse, or *problem-minimizing*, making them seem not so bad. The choice of reference point is inherently arbitrary and contestable. Comparing this year's (hypothetically) higher crime rate to last year's is natural, but it may hide an overall downward trend, so people wishing to portray crime as a serious problem may elide that fact. Choosing which country to compare the US's crime rate to can similarly be done to make US citizens feel superior or feel inferior, depending on the reference point.

Some evidence suggests that problem seriousness is sensitive to contextualized information. Public administration research has found people's performance evaluations of public facilities, like schools, are responsive to what is going on in other district schools (*spatial* context) and how the school's performance has trended over time (*temporal* context) (Charbonneau & Van Ryzin, 2015; Olsen, 2017). Judgments of national economic growth are made relative to that of other similar countries (Aytaç, 2020). Even perceptions of personal financial well-being depend on whether one is looking up toward the rich or down toward the poor (Condon & Wichowsky, 2020).

A growing literature also finds problem perceptions to be amenable to messaging, though these studies have focused on elite cues rather than policy-relevant information (Bisgaard & Slothuus, 2018; Oxley et al., 2014; Moniz, 2022). These articles suggest that when elites say, "such and such is a serious problem," people, especially fellow partisans, tend to adopt that view. None of these studies, however, test the effect of numerical policy-relevant information, but such information, especially when placed into context, should also increase perceived problem seriousness.

### Hypothesis 1:

Contextualized (problem-amplifying) policy-relevant information increases the perceived seriousness of social problems.

While the perceived seriousness of social problems is important in its own right, as it may motivate people to pay more attention to politics, engage with their friends and communities, and thus learn more about the issue, it

takes on new importance because it may motivate people to wish something be done about it. If policy-relevant information works by adding a strong affect-laden consideration, that is, the perception of a serious problem, then policy attitudes should change *because* people perceive the problem to be more serious than they thought it was (see Figure 1).

Different presentations of information could be used to do this; for instance, a story could describe in detail someone who is harmed by a social problem; in media-effects research this would be a case of episodic framing (e.g., Iyengar & Kinder, 1987). Or people could be asked to imagine themselves as the victims of the problem in a perspective-taking exercise (e.g., Condon & Wichowsky, 2020 but see Bor & Simonovits, 2021).

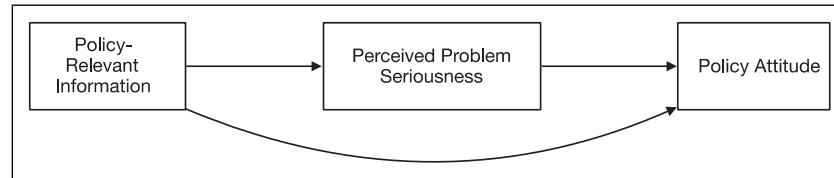
Given the need to understand the elusive effect of numerical information (Gilens, 2001; Kuklinski et al., 2000; Lawrence & Sides, 2014), I employ it here to raise perceived problem seriousness and policy support. To do this, I focus on facts that (1) relate to a harmful social condition and (2) add temporal or spatial context, allowing people to judge the present against the past or against other nations. The addition of temporal context is similar to thematic framing in communication research (Gross, 2008; Iyengar & Kinder, 1987).

Figure 1 depicts the basic structure of the model. Policy-relevant information about social conditions changes how serious the problem seems to be, which then causes a shift in support for (presumably effective and helpful) policy. This indirect effect of policy-relevant information is represented by a pair of arrows: one connecting information to problem seriousness and another from seriousness to policy support. Problem-amplifying information raises the perceived seriousness of the problem, which then increases support for policy intervention, whereas problem-minimizing information would have the opposite effect. Affirmative evidence of this indirect effect would support a problem-based reasoning model and shed light on how information can change policy attitudes.

### Hypothesis 2:

Policy-relevant information increases support for policy indirectly by increasing perceived problem seriousness.

Not all policy-attitude change occurs through perceived problem seriousness, however, as a large literature has shown that elite position-taking (Lenz, 2012), social groups' preferences (Elder & O'Brian, 2022), policy design (Haselswerdt & Bartels, 2015), target population (Jensen & Petersen, 2017; Nelson & Kinder, 1996), and perceived self-interest (Chong et al., 2001) matter as well. The curved arrow traveling from policy-relevant information directly to policy attitudes represent the effects through all these (and other) mechanisms.



**Figure 1.** Mediation model of problem-based reasoning.

## Data and Methods

To test these hypotheses, I conducted three experimental studies in which respondents were provided temporally or spatially contextualized policy-relevant information. The studies employ similar designs but focus on different social problems and rely on different sampling frames. The first study uses a nationally representative sample of Americans and focuses on two economic problems: economic inequality and rent burden (spending 30% or more of income on rent). The second study uses a convenience sample recruited through Amazon’s Mechanical Turk (MTurk) and focuses on the problem of opioid addiction and overdose. The third study uses a nationally representative convenience sample obtained on Lucid Theorem and examines attitudes toward child poverty. Taken together, the three studies cover social problems that vary in their partisan relevance, victim populations, and social or economic nature. Their diversity helps provide a strong initial test of the problem-perception hypotheses laid out above.

### Empirical Strategies

For the average treatment effects predicted in Hypothesis 1, I conduct two-tailed difference-in-means t-tests. I estimate the average causal mediation (indirect) effects of the informational treatments on policy attitudes through perceived problem seriousness using the “mediation” R package (Tingley et al., 2014). The package’s calculation of the indirect effect is equivalent to the product of two coefficients: the effects of the treatment on the mediator and the mediator on the outcome, controlling for covariates (Imai et al., 2011). While the treatment variable is strictly exogenous because it was administered randomly, the mediator is not (Bullock et al., 2010). By incorporating covariates in the model, we can account for possible confounding of the relationship between the mediator and the outcome variable (VanderWeele, 2015), and the package provides sensitivity tests to examine the robustness of the results to an omitted mediator-outcome confounder.

The mediation analyses provide estimates of three effects: the average indirect, direct, and total effects. The average indirect effect is the theoretically relevant quantity here because it represents the effect of policy-relevant information on policy attitudes *through* its effect on the mediator, perceived problem seriousness, controlling for covariates. A significant positive indirect effect in the

opioid experiment, for example, would be evidence that, relative to the control group, the informational treatment raised the perceived seriousness of opioid addiction as a problem in the country and this subsequently raised support for government intervention. The average direct effect represents the effect of the informational treatment through all mediators other than perceived problem seriousness. This is akin to the residual effect of the treatment after partialing out the effect through the measured mediator. The average total effect is the sum of the indirect and direct effects. It is equivalent to the coefficient on the treatment variable in a linear regression.

No respondents were dropped for the t-tests unless they had missing data on the dependent variable. For the mediation analyses, which include pretreatment covariates, respondents with missing data on any variable were dropped. I rely on complete-case analysis because the mediation R package cannot handle missing data. Lastly, no survey weights are used in any of the analyses.

### Study 1: Economic Problems and Temporal Context

The first study focuses on economic inequality and rent burden, chosen for several reasons. First, because they are truly worsening problems, the informational treatments could claim some external validity. Second, because economic inequality does not refer to particular individuals’ experiences whereas rent burden does, they may differentially evoke mental images of suffering people (Aarøe, 2011; Iyengar & Kinder, 1987). A weaker evocation of sympathy may translate into a weaker effect for information on inequality. Third, rent burden and economic inequality differ in their levels of media salience, especially because economic inequality was a focal issue during the 2020 presidential campaign, during which the survey was fielded.

These two experiments were embedded in the 2020 Cooperative Congressional Election Study ( $N = 1,000$ ), a nationally representative sample of Americans surveyed by YouGov between September 29, 2020 and November 2, 2020. First, one-third of respondents were randomly assigned to read temporally contextualized policy-relevant information about economic inequality and then answer some questions related to it. Another third were assigned to a no-information control.<sup>1</sup> Next, one-half of the sample were randomly assigned to an experimental condition about rent burden and

asked questions about it.<sup>2</sup> The crux of the experiment lay in giving people context with which to judge present conditions. The economic inequality experiment gave *temporal context* by saying:

“According to a Census report, from 1979 to 2015, the average income for the top 1% of income-earners has grown 229%, while the growth for the bottom 90% has grown only 46%. Over the same period, the total wealth owned by the top 1% grew from 22% to 37%, meaning that the top 1% richest Americans now own over one-third of all the wealth in the country.”

The rent-burden stimulus did similarly by reading:

“According to a Census report, in 2001, 41% of all renters were rent-burdened—they had to pay at least 30% of their income on rent. By 2017, 47% of all renters had become rent-burdened.”

## Measures

After reading the informational stimulus, respondents’ perceived problem seriousness was measured by asking, “How serious a problem do you think economic inequality/rent burden is in the country today?” Responses were selected from a labeled 5-point scale going from “not at all serious” to “extremely serious.”

Policy attitudes were measured using a single item that asked, “In general, do you support or oppose the U.S. government increasing regulations and spending more, even if it means higher taxes, to reduce the type of activities that cause economic inequality to worsen?” After answering this question, respondents progressed to the rent-burden portion of the survey, which instead asked about the “lack of affordable housing.” All continuous variables except for age were recoded to range from 0 to 1. Larger values of policy-attitude items and perceived problem seriousness correspond to greater support and seriousness, respectively.

The covariates included to control for possible confounding between the mediator and outcome are partisanship, ideological identification, political interest, personal experience with the problem, perceptions of inequality in your area, education, income, home ownership, union membership, race, age, and gender. Personal experience with economic inequality was measured by asking, “How big is the gap between the rich and poor in the area where you live?” They could answer on a 5-point scale from “not big at all” to “extremely big.” A yes-or-no question was asked for personal experience with rentburden: “Do you or anyone you know suffer from being rent-burdened, that is, pay more than 30% of your income on rent?” Exact wording and coding for the remaining covariates for this and the other studies are in the appendix.

## Results

### Perceived Problem Seriousness

Baseline partisan differences in the perceived seriousness of economic inequality were stark: in the control group,

Republicans’ average seriousness was 0.47; it was nearly twice as high among Democrats at 0.87. Independents fell closer to Democrats with a mean of 0.71. Such stark differences between partisans may suggest relatively crystallized opinions on the matter and portend possible ceiling effects of the treatment. It seems, however, there was still some room for perception change, as subjects in the Temporal Context condition ( $M = 0.744$ ) reported 0.048 points higher perceived problem seriousness than those in control ( $M = 0.696$ ,  $t = -1.98$ ,  $N = 660$ ,  $p = 0.048$ , two-tailed).

The results of the rent-burden experiment mirror those of the economic inequality one. Again, in the control group, Democrats ( $M = 0.88$ ) perceived the problem as more seriousness than Republicans did ( $M = 0.56$ ) with Independents near Democrats at 0.78, but the Temporal Context raised perceived seriousness only 0.028 points from 0.738 in the control group to 0.766 in the treatment group ( $t = -1.57$ ,  $N = 1000$ ,  $p = 0.12$ , two-tailed).

### Mediation Analysis

Even though contextual information raised the perceived seriousness of the economic problems, it remains to be seen whether this effect led to an increase in policy support. First, as in prior studies (Hopkins et al., 2019; Lawrence & Sides, 2014), the informational treatments, despite being contextualized, had no statistically significant average *total* effect on support for policy in either experiment. The differences were smaller than 0.02 points and the  $p$ -values greater than 0.25 in two-tailed  $t$ -tests. While the average total effect of the treatments were not significant, this does not mean that all of its constituent *indirect* effects were (Hayes, 2018). It is possible that the total effect is mediated through different causal pathways, some positive and some negative, and therefore cancel out to produce a null total effect.

The results from the mediation analysis, found in Table 1, suggest that this is what may be going on here and in other information-effects studies that find null results. In both experiments, the average causal indirect effect is positive and statistically significant, whereas the direct effect, which is the effect of the treatment through all other mechanisms, is not significant.<sup>3</sup> These results suggest that attitude change did in fact occur among those who perceived greater seriousness of the problem.

The point estimates of the indirect effects hover around 0.02, meaning that the contextual information raised support for policy 2 percentage points on average through the perceived seriousness pathway. Although the effect size is small, it is comparable to average indirect effects in highly cited framing studies, with effect sizes of about 4 percentage points (e.g., Brader et al., 2008; Druckman & Nelson, 2003; Slothuus, 2008).<sup>4</sup>

This effect is also reasonably robust to omitted mediator-outcome confounders. Sensitivity analyses from both

**Table 1.** Perceived problem seriousness mediates effect of numerical information on attitudes.

	Economic Inequality		Rent Burden	
	Estimate	CI	Estimate	CI
Indirect effect	0.019*	(0.003, 0.04)	0.016*	(0.004, 0.03)
Direct effect	-0.019	(-0.06, 0.019)	0.006	(-0.028, 0.04)
Total effect	-0.002	(-0.043, 0.04)	0.02	(-0.012, 0.06)
	542		812	

Note. Estimates come from a causal mediation model with normal-linear mediator and outcome variable models. Confidence intervals are boot-strapped using 1000 samples. \* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$ .

experiments indicate that an omitted variable would need to correlate with the mediator and outcome at 0.4 for estimated average indirect effect to completely diminish to 0 and 0.3 for its confidence bounds to cross 0 (VanderWeele, 2015). An example of a particularly strong correlation in the data is the sample correlation of  $-0.49$  between 7-point party ID and perceived problem seriousness of the lack of affordable housing.

It could be argued that rather than perceived problem seriousness causing support for policy, it is the other way around, and that policy support shapes how serious people admit the problem to be. Indeed there is evidence that people downplay problems because they are averse to the solutions (T. H. Campbell & Kay, 2014). I tested this possibility by modeling problem seriousness as the outcome and policy support as the mediator. In both experiments, the indirect effects were non-significant ( $ps > 0.15$ ). The evidence thus suggests that perceived problem seriousness caused policy attitudes, not the other way around.

## Study 2: Opioid Addiction and Temporal Context

The previous study showed that when information changed policy attitudes, it did so by changing respondents' perceptions of the problem. The experimental treatment in Study 1 relied on the idea that temporally contextualizing information would give people a reference point with which to assess the seriousness of the problem. But, because the control condition provided no information whatsoever, the results may be due to information generally and not contextual information specifically. The null findings in prior studies between control conditions with no information and treatment conditions with decontextualized information make it fairly safe to assume that there is no difference between giving people static statistical facts and no information at all. Still, a stronger test could help assuage this concern. So, in Study 2, contextualized information is pitted against decontextualized information (rather than no information at all).

Study 2 also builds on the previous by addressing a different social problem: opioid addiction. Unlike economic

inequality and rent burden (and many other social problems), which Democrats see as much more serious than Republicans do, almost equal percentages of both parties regard opioid addiction as a very big problem (Pew Research Center, 2018). In our sample, Democrats ( $M = 0.76$ ) and Republicans perceived opioid addiction as an equally serious problem ( $M = 0.75$ ). The relatively bipartisan concern over opioid addiction is likely due to its prevalence in all sorts of communities, both white and non-white and rural and urban (Jalal et al., 2018), and its relatively non-racialized portrayal in the media (Netherland & Hansen, 2017). Since opioid addiction involves distinctly physical harm rather than economic (though its harm is not exclusively physical), this study also tests the transferability of perceived problem seriousness across types of harm.

As mentioned above, Study 2 improves upon Study 1 by adding an additional condition, the No Context condition. It is similar to the treatment stimuli in previous research, as it gives participants a good amount of information about the problem but with no contextualizing information. The Temporal Context stimulus read:

*“Rates of drug overdose deaths have increased in the last twenty years. In 2006, more than 17,500 (or about 6 per 100,000) Americans died as a result of an opioid overdose, including prescription opioids, heroin, and illicitly manufactured fentanyl, a powerful synthetic opioid. By 2017, more than 47,000 (or about 15 per 100,000) Americans died as a result of an opioid overdose, a rate 2.5 times larger than it was in 2006.*

*As rates of overdose have increased, so has the economic cost to society of opioid abuse. The Centers for Disease Control and Prevention estimates that the total ‘economic burden’ of prescription opioid abuse alone in 2006 in the United States was \$53.4 billion a year, including the costs of healthcare, lost productivity, addiction treatment, and criminal justice involvement. In 2017, the cost was estimated to have increased 47% to \$78.5 billion per year.”*

The italicized portions are omitted from the No Context treatment (with other language altered to read smoothly; see the appendix for full treatment wordings.) To buttress the point that decontextualized and contextualized information do not exert the same effect on

policy attitudes through perceived problem seriousness, the experiment also includes a “pure” No Information control condition.

### Sample and Measures

The opioid experiment was embedded in a survey fielded April 27, 2019 on an online convenience sample ( $N = 663$ ) drawn from Amazon’s Mechanical Turk. Like most MTurk samples, it is younger, more liberal, Democratic, and male than national probability samples. Nonetheless, it possesses considerable variation across all variables, and such samples are often suitable for drawing inferences about average treatment effects (Druckman & Kam, 2011).

The main variables, perceived problem seriousness and policy attitudes, were measured with slightly different items on this survey. Seriousness was measured with 2 4-point items (averaged together and recoded to range from 0 to 1) that asked, “How much of a problem do you think opioid abuse is in the country today?” with response options ranging from “Not a problem” to “A very serious problem.” The other item asked, “Would you describe the problem of opioid abuse in this country as a national emergency, a major problem but not an emergency, a minor problem, or not a problem at all?” Policy attitudes were again measured with a general-purpose item which read, “Do you think the federal government is doing the right amount to address opioid misuse in the United States, should it do more, or should it do less?” Respondents who said “do more” were coded 1 and 0 otherwise.

Due to the shorter survey, the models incorporate fewer controls than those in Study 1, but do include partisanship, ideological identification, political knowledge, gender, age, ethnicity, education, and political interest. I model the outcome variable with a linear model so that effect sizes can be compared to those in Study 1. The estimates are statistically and substantively similar when using a probit model or leaving the outcome variable as a 3-point ordinal variable and using ordered probit (available in the appendix).

### Results

**Perceived Problem Seriousness.** The Temporal Context treatment ( $M = 0.80$ ) increased the perceived seriousness of the problem from the No Information control group ( $M = 0.75$ ,  $t = -2.41$ ,  $p < 0.05$ , two-tailed) by 5 percentage points. Importantly, the contextualized information treatment also raised problem perceptions by 4 percentage points from the No Context group ( $M = 0.76$ ,  $t = -2.24$ ,  $p < 0.05$ , two-tailed). Supporting Hypothesis 1, these results are the first evidence showing that the contextualized information—rather than information at all—is doing the

work of raising the apparent seriousness of social problems, which may thereby increase policy support. The difference between the No Information and No Context groups is not significant, showing that without a reference point, numerical information did not alter perceptions of the seriousness of the opioid epidemic.

**Mediation Analysis.** Again as in Study 1, the contextualized-information treatment did not alter policy attitudes in the aggregate; the total effect is not different from 0. Not surprisingly, the decontextualized information had no total effect either. The null hypothesis of no effect could not be rejected, as all t-tests yielded  $p$ -values greater than 0.1 and often greater than 0.2. However, the total effects of the treatments may conceal significant indirect effects through the perceived problem seriousness pathway.

Reported in Table 2 are the results of the main causal mediation analyses pertaining to Hypothesis 2. Support for the government to “do more” is the outcome variable. We see that the contextualized-information treatment about the opioid crisis had a significant positive indirect effect through perceived seriousness in comparison to both the No-Information and decontextualized-information baselines. The effect sizes are very similar, if slightly larger, compared to those in Study 1, giving some reassurance that these are generalizable effects. The results further demonstrate that perceived problem seriousness mediates the effect of policy-relevant information on policy attitudes. They also show that contextualized information helps to bring this effect about whereas static information does not.

Sensitivity analyses yield a similar robustness of these estimates to a violation of the no-confounding assumption: both indirect effect estimates are robust to an omitted confounder with correlation 0.5. These estimates seem especially robust to violations of this assumption because the inter-correlations among the regressors and the outcome are relatively small compared with those in Study 1 (none larger than  $r = 0.24$ ), presumably because concern about opioids is less partisan (see Appendix for correlation matrix). Furthermore, a test of a model placing policy support before perceived problem seriousness also yielded a highly non-significant indirect effect.

### Study 3: Child Poverty and Spatial Context

The previous studies relied on temporal reference points to help people judge the seriousness of present conditions, but *spatial* reference points are also theoretically useful (Charbonneau & Van Ryzin, 2015; Olsen, 2017). Spatial reference points—information about conditions in other places—permit a judgment based on social comparison. Conditions elsewhere may be better, making those at home seem inadequate and the people experiencing them suffering needlessly since, in those other places, they’ve

**Table 2.** Average Treatment Effects of Contextualized Numerical Information on Opioid Policy Attitudes.

	No-Info Baseline		De-Context. Baseline	
	Estimate	CI	Estimate	CI
Indirect effect	0.028**	(0.006, 0.06)	0.052*	(0.01, 0.09)
Direct effect	-0.025	(-0.06, 0.01)	0.067	(-0.15, 0.01)
Total effect	-0.003	(-0.04, 0.05)	-0.02	(-0.11, 0.07)
	431		429	

Note. Estimates come from a causal mediation model with normal-linear mediator and outcome variable models. Confidence intervals are bootstrapped using 1000 samples. \* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$ .

**Table 3.** Average Treatment Effects of Spatially Contextualized Numerical Information on Child Poverty Policy Attitudes.

	No-Info Baseline	
	Estimate	CI
Indirect effect	0.029**	(0.006, 0.06)
Direct effect	0.027	(-0.01, 0.07)
Total effect	0.056*	(-0.01, 0.11)
Observations	288	

Note. Estimates come from a causal mediation model with normal-linear mediator and outcome variable models. Confidence intervals are bootstrapped using 1000 samples. \* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$ .

figured out how to make things better. Such information would constitute an *upward comparison*, and should likely make the observer feel more concern (Condon & Wichowsky, 2020). In Study 3, subjects will be presented with such problem-amplifying spatial context.

Adding to the diversity of social problems studied here, Study 3 examines attitudes toward child poverty in the United States. Perhaps because children are an archetypically vulnerable population (Schneider & Ingram, 1993), problem perception attitudes toward child poverty are not as polarized as those toward economic inequality, though they aren't as bipartisan as those toward opioid addiction. In the sample, perceived seriousness of child poverty is moderately high for both Democrats ( $M = 0.75$ ) and Republicans (0.68) as well as Independents (0.69).

Study 3 continues with the use of static information in the control condition, providing another stronger test of the role of *contextualized* information rather than information generally. The Control condition asks respondents if they've heard of a recent report:

"According to a recent report by the OECD, an international think tank, 21%, or just over 1 in 5, children in the United States were living in poverty. Living in poverty means having a household income less than half the country's average."

### The Spatial Context condition adds

"Most rich countries have much lower rates of child poverty. Among the 36 richest countries, the US is the 5th worst. In Hungary, which has the 9th best rate, the percentage of children in poverty is only 8.4%. Even in a large country like Germany, it is 11.1%, which is about half that of the US."

The stimulus information came from the OECD's website (OECD, 2022), giving it a degree of external validity. The choice of Hungary as a reference point was based on its being a country that most Americans were most likely not familiar with having a lower child poverty rate (as opposed to, say, Finland or Denmark). Germany was chosen because it was the country closest in population size to the US, in case participants thought that the US's large population made it uniquely prone to poverty.

### Sample and Measures

The pre-registered child poverty survey experiment was fielded on a nationally representative non-probability sample ( $N = 314$ ) from Lucid Theorem on April 20, 2022.<sup>5</sup> Respondents were randomized into either the Control or Spatial Context condition and then answered a questionnaire. The scale of perceived problem seriousness comprised 5 labeled 5-point items (one asking "how serious a problem do you think child poverty is in the US?", the other "how important") and an additional item asking, "how widespread do you think child poverty is in the US?" Two others asked about the perceived harm children experience from poverty ( $\alpha = 0.89$ ).

Support for poverty-alleviating policy was measured using 4 items that were averaged into a scale ( $\alpha = 0.85$ ) and recoded from 0 to 1. Using a 7-point scale, respondents were asked how much they supported or opposed "giving a monthly payment to families of \$250-\$300 per child per month," "increasing the federal minimum wage to \$15 an hour," and "providing free pre-Kindergarten to all children. They were also asked about general spending to lower child poverty. Political interest, ideological identification, partisanship, importance of religion, personal experience with poverty, income, race, age, gender, and education variables were also collected. The full question wording and coding are in the [appendix](#).

### Results

**Perceived Problem Seriousness.** Subjects in the Spatial Context condition ( $M = 0.79$ ) reported 5 percentage points higher perceived seriousness for child poverty than those in the Control condition ( $M = 0.74$ ,  $t = -2.33$ ,  $N = 313$ ,  $p < 0.05$ , two-tailed).

**Mediation Analysis.** Unlike in the previous studies, the Spatial Context treatment significantly increased average total support for child poverty-alleviating policy. Support rose 7



percentage points from 0.65 in the Control condition to 0.72 in the Spatial Context condition ( $p < 0.05$ ,  $t = -2.56$ ,  $N = 314$ , two-tailed). Mediation analysis also supports the hypothesis that part of this total effect operated by raising the perceived seriousness of child poverty. Table 3 shows a total effect of 5.5 percentage points ( $p < 0.05$ ) and an indirect effect of 2.9 percentage points ( $p < 0.01$ ), accounting for one-half of the total effect.

Sensitivity analysis reveal an average indirect effect that is robust to an omitted confounder with correlation 0.5, or something on par with the sample correlation of policy support and partisanship ( $r = -0.44$ ).

## Discussion and Conclusion

Though information about social conditions may be new and corrective, using it to change policy attitudes is hard (Berinsky, 2007; Kuklinski et al., 2000; Lawrence & Sides, 2014). This paper has shown that policy-relevant numerical information, when contextualized, can help people make judgments that have consequences for their policy attitudes. Providing context, through either temporal or spatial reference points, is impactful because it helps citizens form expectations for what conditions could and ought to be. It provides a reasonable baseline. The stronger the expectation, the more serious the problem should seem when that expectation is broken. A temporal reference point can suggest that things *were* better and could again be better. A spatial reference point can suggest things *are* better somewhere else and could be so here. By pointing to actual cases of better conditions, these types of comparison imply better conditions are attainable and realistic, and make present conditions seem more unacceptable.

Such comparisons may also simply that policy change could be effective. Learning that things are better elsewhere may make citizens wonder if it's because they are doing something differently. Gun-control advocates, for example, cite the precipitous drop in mass shootings that Australia experienced after a large-scale gun-buyback initiative as an argument for the effectiveness of policy to reduce gun deaths (e.g., Leigh, 2014). Thus, contextualized information may work on policy attitudes not only by affecting perceived problem seriousness but also by highlighting causal connections between conditions and policy. This possibility isn't tested empirically here, but should be in future work.

The large and statistically significant total average effect of spatial, but not temporal, context on policy attitudes suggests spatial context may be the more persuasive of the two. Whether this holds for other policies, problems, and reference points is left open for future research. There are reasons to believe it is plausible. For one, spatial context may be more effective because, as mentioned above, it implies that better conditions are attainable, because they've already attained

them elsewhere. Conditions that held in the past may seem less attainable because they are less vivid than conditions actually occurring now. Spatial context may also yield stronger effects because they evoke a stronger sense of relative deprivation.

More generally, by identifying a mechanism through which policy-relevant information changes policy attitudes, this paper points toward a theory of information effects driven by problem-based reasoning. As a judgment, rather than a personal characteristic like attitude importance (Krosnick, 1988) or a measure of cognitive engagement like issue salience (Miller et al., 2017), problem seriousness is a more dynamic, information-sensitive affect-laden perception, and it connects information about conditions to policy. As such, a problem-based model provides a promising new strategy for changing policy attitudes by changing perceptions of social conditions.

Though this paper shows a consistent indirect effect of contextualized policy-relevant numerical information on policy attitudes using different samples and social problems, it is not without its limitations. First, these experiments all took place in the United States using English-speaking Americans. Though the psychological mechanisms are proposed to apply to people generally, a stronger test would incorporate data from a variety of countries. Second, all the stimuli were designed to *increase* the perceived seriousness of the given problem, but the theory should work in the opposite direction as well, with *problem-minimizing* information reducing support for government intervention. Third, the stimuli here focus on information from non-governmental expert sources, while in the real world, partisan actors are also vying to influence problem perceptions. Future research should explore the role of source cues.

Fourth, these studies do not grapple with the question of, once a problem is perceived as serious, what leads people to support government intervention, as opposed to other forms of intervention, like charity, protest, direct action, or civil society groups. People's beliefs about problems' causes—and their trust in government—most likely play a role in what they prefer be done about it. Fifth, though Study 3 provides novel evidence of Americans' reactions to international comparison, it leaves open numerous questions about the conditions under which it operates. Are international comparisons more impactful for citizens with more national pride? Are problem perceptions moved more by upward comparison to peer countries, like Germany, or to less prominent countries, like Hungary? They are perhaps even more moved by "adversarial" or rival nations, like Russia or China, which may be due to feelings of national pride or perceived threat. Spatial comparisons leave open a great deal of latitude for political entrepreneurs to strategically choose reference points. The same is true of comparisons with the past, when things could have been "great," depending on your perspective.

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## Supplemental Material

Supplemental material for this article is available online.

## Notes

1. The remaining third were given a stimulus meant to combine both temporal and spatial context to see if it would produce an even larger effect. However, due to errors in the stimulus I made, its effect is uninterpretable. It varied multiple factors at once: it omitted mention of wealth inequality and the spatial context minimized perceived problem seriousness rather than amplifying it. Because interpretations of its results are inconclusive, I don't analyze it here. Full treatment wording and results are in the [appendix](#).
2. Note that this means subjects could be in the treatment condition for both experiments, raising the possibility of a spillover effect from one to the next. Regressions testing for the effect of the economic inequality treatment, and its interaction with the rent burden treatment, on the rent burden experiment's dependent variables yielded no significant effects, suggesting that there were no differences between those who saw both treatments and those who saw only one. See the [appendix](#) for regression tables.
3. The total effect is the sum of the indirect and direct effects. Full regression tables of the mediator and outcome models can be found in the [appendix](#).
4. The estimates of these indirect effects come from [Imai and Yamamoto \(2013\)](#).
5. Preregistered analysis plan is at <https://osf.io/z94n3>. Two deviations from the plan were made. First, respondents who failed either one or both attention-check questions were dropped. This adjustment helps improve data quality ([Alvarez et al., 2019](#)) and increases the precision of the estimates. Second, the policy support scale was constructed using 5 items rather than 3. Without them, some of the coefficients fail to reach statistical significance, though the point estimates remain similar. Furthermore, assignment to treatment did not cause differential failure to passing the attention checks. See [appendix](#) for details.

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