SHORT ARTICLE

Reducing Bias in Citizens' Perception of Crime Rates: Evidence from a Field Experiment on Burglary Prevalence

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Citizens are, on average, too pessimistic when assessing the trajectory of current crime trends. In this study, we examine whether we can correct this perceptual bias with respect to burglaries. Using a field experiment coupled with a large panel survey ($n = 4,895$), we explore whether a public information campaign can reduce misperceptions about the prevalence of burglaries. Embedding the correct information about burglary rates in a direct mail campaign, we find that it is possible to substantially reduce citizens' misperceptions. Importantly, the effects are not short-lived: they are detectable several weeks after the mailer was sent, but they are temporary and eventually the perceptual bias reemerges. Our results suggest that if citizens were continually supplied with correct information about crime rates they would be less pessimistic. Reducing bias in citizens' perception of crime rates might therefore be a matter of adjusting the supply of (dis)information about crime.

In recent decades, crime has fallen markedly in most Western countries (Pinker 2012). Despite this, most citizens think that crime is on the rise. Across the past 30 years, numerous surveys have documented that a large majority of Americans think that crime is increasing when it is, in fact, decreasing (Gallup 2017; Gramlich 2016). This is not a uniquely American phenomenon, as we see similar perceptual biases in, for instance, Italy (Mastrorocco et al. 2016) and Denmark (Fuglsang 2017). This tendency to overestimate crime rates can potentially lead to adverse societal outcomes. Studies have shown that perceptions of crime are related to social trust (Gainey, Alper, and Chappell 2011) and economic outcomes (Buonanno, Montolio, and Raya-Vílchez 2013). In politics, this bias makes it difficult for citizens to hold politicians accountable for their ability to provide public safety. If citizens do not recognize that crime rates are decreasing, politicians have no incentive to focus on crime rates, and politicians who are effective at reducing crime will be reelected at the same rate as politicians who are not (Mansbridge 2009).

This article explores whether there is a role for public information campaigns in reducing misperceptions about crime. We believe this might be the case because previous literature suggests that the supply of information about crime is insufficient and biased. The media tends to cover crime episodically and not thematically (Iyengar 1994, chap. 4), which means that citizens are exposed to specific cases of crime and not the broader context (e.g., information about the prevalence of crime). The news media also has a well-documented negativity bias (Soroka 2006), so they will typically not cover reductions in the crime rate but rather vivid instances of rare crimes (Soroka and McAdams 2015). Finally, the media's focus on current events naturally reduces coverage of long-term trends.

Even so, it is not obvious that citizens will let go of their biased perceptions if a public information campaign presents them with accurate information about crime rates. Motivated reasoning suggests that citizens might resist correct information about crime rates if their misperceptions were borne out of strong affective ties to a political party (e.g., the leader of

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Data and supporting materials necessary to reproduce the numerical results in the article are available in the *JOP* Dataverse (https://dataverse.harvard.edu/dataverse/jop). An online appendix with supplementary material is available at https://doi.org/10.1086/706595.

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this party might insist that crime is not decreasing) or stereotypical beliefs about out-groups (e.g., that a wave of immigration is driving up crime rates; Esberg and Mummolo 2018; Lodge and Taber 2013). Beyond this, people might have good reason to resist information if their everyday experiences contradict it (Hjorth 2017), and, just like the media, individuals are myopic and tend to (over)emphasize negative information (Healy and Lenz 2014).

To study the potential of public information campaigns in reducing misperceptions, we conduct a field experiment coupled with a two-wave panel survey. Specifically, we embed information on burglary rates in a leaflet about how to avoid burglaries and mail the leaflet to the panelists between the two survey waves. In order to explore the temporal dynamics of the leaflets' effect, we randomly assign participants to alternative timings of reinterviews. We find that it is possible to substantially reduce citizens' misperception of crime rates. The effect is not short lived—it is detectable several weeks after the mailer was sent—but it is temporary, and eventually the perceptual bias reemerges.

Apart from giving us an insight into whether public information campaigns can be used to reduce citizens' misperceptions about crime, our study provides important context for existing studies, which have found that it is typically easy to correct citizens' misperceptions about a wide range of issues in a survey experimental setting (Guess and Coppock 2016; Nyhan and Reifler 2010; Nyhan et al. 2017; Wood and Porter 2019). Our findings suggest that it is also possible to correct beliefs outside of a serene survey setting using a scalable intervention, but the effects of the corrections are temporary. As such, permanently correcting citizens' misperceptions about crime, and other issues, might not be a matter of simply supplying them with correct information at one point in time but rather of continually supplying such information.

EXPERIMENTAL DESIGN

To explore the effect of public information campaigns about crime on citizens' misperceptions, we designed a field experiment (see fig. 1) with survey outcomes (Broockman, Kalla, and Sekhon 2017), recruiting 6,481 participants from the survey company Epinion's Danish web panel. The participants had to be over 30 years of age and had to live in a single family home, so that it made sense for them to receive a leaflet about how to avoid burglaries. In addition to this, the participants had to agree to give their address and to be contacted for a follow-up study. As participants were being recruited, they were given a short survey about their attitudes toward various social issues, including specific questions about their perception of crime rates.

Two weeks after the final participant had been recruited, all participants were mailed a leaflet. The treatment group (43% of the sample) received a four-page leaflet about how to avoid burglaries that included statistical information about burglary rates. We embedded the statistical information in a leaflet with other information about burglaries, in order to add to the realism of the treatment and to see whether participants would notice the statistical information in the presence of other information. The remaining participants received either a leaflet about how to avoid burglaries with no information on burglary rates (43%) or a placebo-leaflet on an unrelated topic (14%). We implemented these two different control conditions in order to identify any independent learning effect of receiving a leaflet about burglaries (as opposed to a leaflet on another topic). However, as can be seen in appendix F (apps. A–G are available online), no such effect materialized, and therefore we collapse the two control conditions in the analysis. We used complete random assignment to assign leaflets to participants. All leaflets were sent out by the foundation TrygFonden (that aims to make Denmark safer; see https://www.trygfonden.dk/english/). To avoid experimenter demand effects, participants were not told, and the leaflets gave no indication, that there was any relation between the survey and the leaflets. See appendix A for details about the leaflets.

One week after we sent out the leaflets, participants were invited to a second survey. A random sample of 350 participants were invited each day for 18 days, and on the nine-

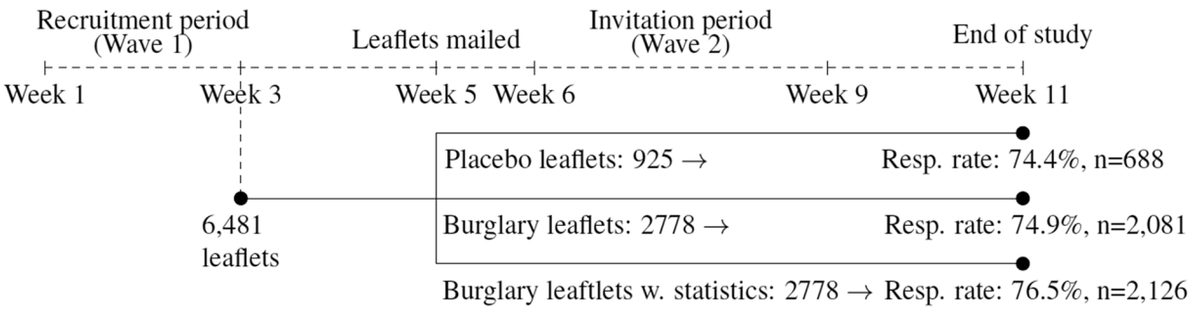

Figure 1. Overview of the experimental design

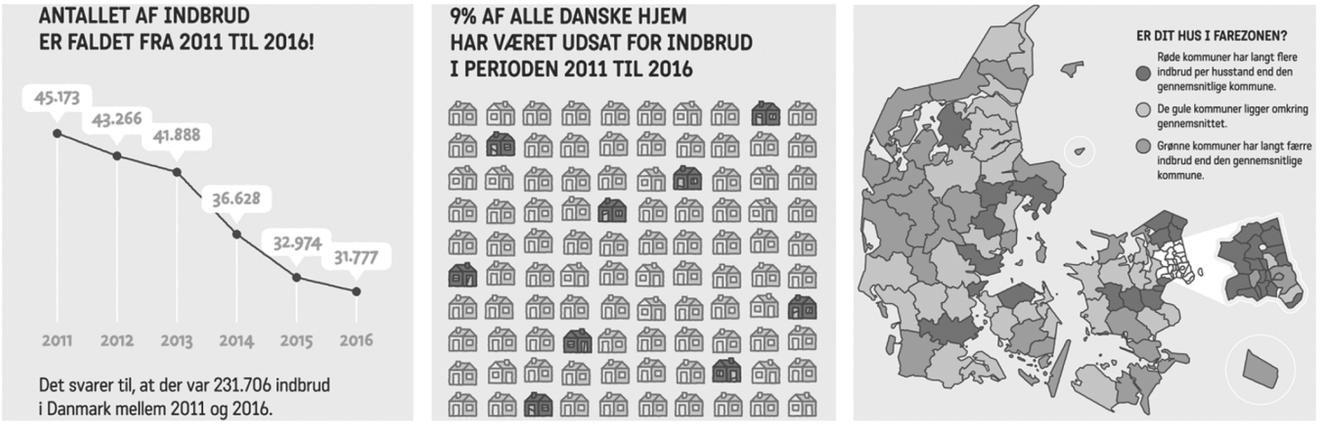

Figure 2. Three data visualizations (translated). *Left*, "The number of burglaries has decreased from 2011 to 2016! In total, there were 231,706 burglaries in Denmark between 2011 and 2016." *Middle*, "9% of all Danish homes have been burglarized in the period between 2011 and 2016." *Right*, "Is your house in danger? Red [dark gray] municipalities have way more burglaries per household than the average municipality. The yellow [light gray] municipalities are close to the average. Green [medium gray] municipalities have way fewer burglaries than the average municipality." Color version available as an online enhancement.

teenth day the remaining 181 were invited. Seventy-six percent of the recruited participants took part in the posttreatment survey ($n = 4,895$). Invitation and participation were closely aligned: among those who participated 74% completed the survey within one day of the invitation, and 92% within five days. To ensure that all treatment conditions were evenly distributed across the timing of the invitation to the posttreatment survey, we block-randomized by which leaflet the participant received, randomly assigning participants to invitation dates within each block. In appendix B, we show that a number of pretreatment participant characteristics are balanced across experimental conditions. We also examine unbalanced attrition, identifying no imbalance across the experimental conditions and only a slight increase in attrition across assignment to reinvitation.

The statistical leaflets consisted of the three data visualizations presented in figure 2. They were all displayed on the same page of the leaflet. They were (1) a downward trending curve diagram of the number of burglaries in Denmark from 2011 to 2016, (2) a "risk characterization theater" (Strauss 2008) illustrating the proportion of households that were burglarized in the last 5 years (9%), and (3) a color-coded map of Danish municipalities indicating whether each of them was in the bottom, middle, or top tercile of burglaries per household. We included different types of information, so that we would be able to gauge the robustness of any potential effects. To maximize the effectiveness of the treatments, they were designed by an advertising bureau that specializes in data visualizations.

We measure participants' perception of crime rates using the following three questions: (1) "Have there been fewer or more burglaries in 2016 compared to 2011?" (2) "Think of the continuous period from 2011 to 2016 as a whole. What percentage of Danish homes were burglarized in this period?" And (3) "Please compare your own municipality to the rest of Denmark. In your municipality, have there been fewer or more burglaries per household in 2016?" Answers were given in percentages for question 2 (participants could write down any integer between 0 and 100). For questions 1 and 3 participants could report "fewer," "about the same," or "more." The three questions match the three different data visualizations presented in the leaflets. The questions were asked in both survey waves, and they were the only ones in the two surveys that asked participants about the prevalence of burglaries. Appendix B presents descriptive statistics. In our analysis, we recode all the dependent variables so that they indicate whether participants answered correctly or not. For question 2, we will also look at what happens if one accepts all responses within 2 percentage points (pp) as correct. For question 3, we split our sample depending on whether the burglary rate in the participant's municipality is in the bottom, middle, or top tercile, as the correct response is contingent on this.

RESULTS

In figure 3, we observe the percentage of correct responses for the three different questions about burglary prevalence among participants who received a leaflet with statistical information and for participants who received a different leaflet.[1] To study the development over time, we group posttreatment responses based on when participants were randomly invited to take

1. In app. E, we redo these analyses using logit models. In app. D, we plot the average treatment effects. In app. C, we reproduce fig. 3 using knowledge about unemployment as a placebo outcome.

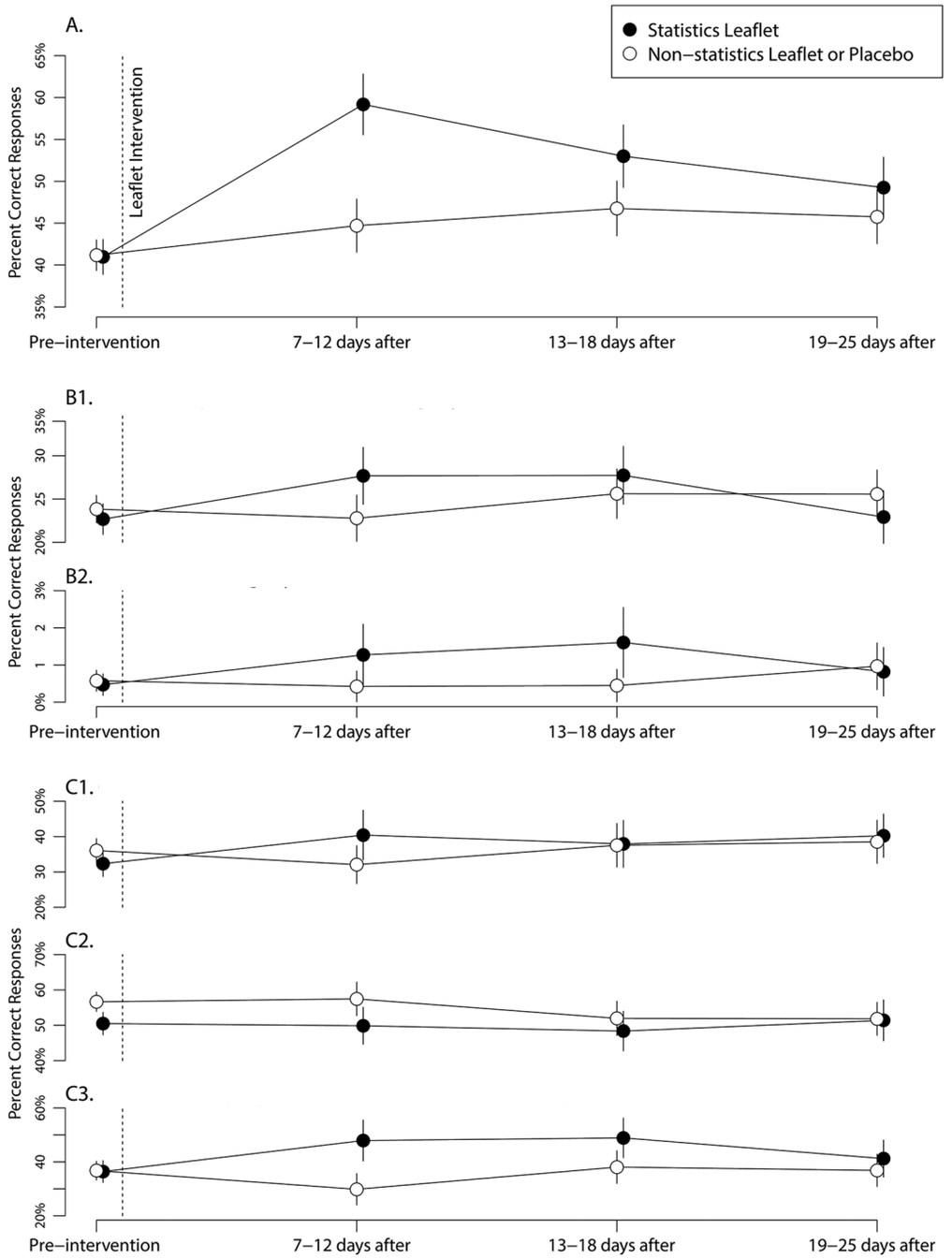

Figure 3. Dots represent the percentage of correct responses with 95% confidence intervals for treatment and control groups across time for each of the three outcomes. *A*, Correct response: declining trend in burglaries; *B*1, correct response: 9% burglary rate (±2 percentage points); *B*2, 9% burglary rate (exact); *C*1, correct response: municipal burglary rate is above the national average; *C*2, municipal burglary rate is at the national average; *C*3, municipal burglary rate is below the national average. *A*, *B*1, and *B*2 each rely on the full sample ($n = 4,895$). *C*1–*C*3 results are divided on the basis of whether participants live in a municipality with an above-average ($n = 1,408$), average ($n = 2,211$), or below-average ($n = 1,276$) burglary rate.

part in the second survey, constructing three groups of equal size: 7–12 days ($n = 1,652$), 13–18 days ($n = 1,579$), and 19–25 days ($n = 1,664$) after the leaflet was sent out. Across all three questions, we find that the percentage of correct responses increases in response to receiving a leaflet with statistical information but also that the effect wanes over time.

In figure 3*A*, we observe the effect of the treatment on participants' ability to correctly state that the burglary rate is lower in 2016 than it was in 2011. Before the treatment, only 41% (confidence interval [CI] = 39.7–42.5) are able to respond correctly, with no significant pretreatment difference ($p = .89$). At 7–12 days after the treatment, we observe a

sizable effect of around 15 pp ($p < .001$), with a clear majority among those assigned to the statistics leaflet correctly reporting the burglary trend. After 13–18 days, the treatment effect narrows to about 6 pp ($p < .05$), and, after 19–25 days, the difference is no longer statistically or substantively significant at 3 pp ($p = .16$). If we compare the difference in treatment effects, then the initial effect is significantly larger than in the second ($p < .05$) and third period ($p < .01$), while the second and third period are not different from each other ($p = .44$). Comparisons across time are complicated by the fact that attrition was slightly larger for those invited later; however, we believe the identified decrease in effect size is credible. A key reason for this is that the drop in effect size is considerably larger than what can be explained by the slight increase in attrition identified in appendix B. Moreover, if attrition was driving the trend in effects, we should see larger effects among those invited later, as marginal participants—who are less engaged and therefore less likely to be affected by the treatment—drop out of the study.

In figure 3*B*1 and *B*2, we observe the effect of the treatment on participants' ability to correctly state that the national burglary rate was 9%. In figure 3*B*1, we extend the range of correct responses to be within ± 2 pp of the true value. In the first 7–12 days after the treatment, the treatment group is about 5 pp more likely to provide a correct response ($p < .05$), but the effect cannot be detected after 13–18 days ($p = .34$) or 19–25 days ($p = .21$). In figure 3*B*2, we look at only exactly correct responses that only .5% (CI = .3–.7) were able to provide. At 7–12 days after the treatment, there is some indication of an improvement in the treatment group by about .8 pp ($p < .1$) and, after 13–18 days, by 1.2 pp ($p < .05$). However, as for the trend results, the effect can no longer be identified after 19–25 days ($p = .75$).

In figure 3*C*1–*C*3, we observe the effect of the treatment on participants' ability to correctly state the relative burglary rate at the municipal level. Since the correct response depends on where participants live, we divide our results by whether the participant's home municipality has a burglary rate below ($n = 1,276$), around ($n = 2,211$), or above average ($n = 1,408$). For those residing in municipalities with a burglary rate below average, we can identify sizable treatment effects. After 7–12 days, those in the treatment group are 18 pp better at correctly identifying the relative burglary rate of their municipality ($p < .001$). After 13–18 days, the difference is 11 pp ($p < .05$) and finally ends up at 4 pp ($p = .35$). A somewhat similar pattern, although with smaller effects, can be identified for participants who live in municipalities with an above-average burglary rate: after 7–12 days it is 8 pp ($p < .1$), after 13–18 days it is .4 pp ($p = .94$), and after 19–25 days it is 2 pp ($p = .70$). For those residing in a municipality with an average burglary rate, we do not find any consistent effects. In fact, for this limited subset, there seems to be an imbalance between the treatment and control before treatment (6 pp, $p < .01$).

DISCUSSION

We can substantially reduce citizens' perceptual biases when it comes to assessing crime rates using a simple, scalable intervention: a leaflet with correct information presented as a set of high-quality data visualizations. Using a field experiment, we showed that misperceptions were reduced, temporarily, among at least 15% of those who received this information. It is important to note that this is an intent-to-treat effect. It is the effect of being mailed the leaflet, not the effect of reading it. We asked participants near the end of the posttreatment survey whether they had received a leaflet from the Tryg-Fonden, with 46% stating that they had. This might seem high; however, it is relatively rare to receive mailers of this type in Denmark. If only half the participants actually read the leaflet, then the effect of receiving the correct information among those who read the leaflet is above 30 pp (see app. D).

Our study also points to some limitations in our ability to correct misinformation in the mass public. First, while the intervention reduced misperceptions of relative and absolute levels of crime, it was considerably more effective with respect to the trend. Since the information on the trend in burglaries was not displayed more prominently than the rest of the statistical information (see app. A), this is surprising. One explanation might be that comparative assessments tend to carry more psychological weight (Olsen 2017). Irrespective of the explanation, this seems to suggest that some misperceptions are more amenable to correction. Second, while the effects we identify were not short lived, they were temporary—lasting a couple of weeks and declining in this period. This suggests that as other considerations, such as news stories or firsthand observations, become top of mind, the effect of the correction wanes. Given what we know about opinion formation, this makes sense (Zaller 1992). If one wants to permanently correct citizens' misperception of crime rates (or other phenomena), our study suggests that the broader information environment, including the media, will need to continually provide correct information. This notion is supported by additional analyses presented in appendix G, which reveal that among participants who are very interested in local affairs, and might therefore be more likely to consume local news, the effect of the leaflet decays rapidly, whereas the effect is more lasting among those who are not interested in local affairs. This might reflect that those who are more politically aware are more likely to resist new information (Lodge and Taber 2013, 131; Zaller 1992). However, it could also suggest

that permanently reducing bias in citizens' perception of crime rates is primarily a matter of adjusting the supply of (dis)information about crime in the news.

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