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-

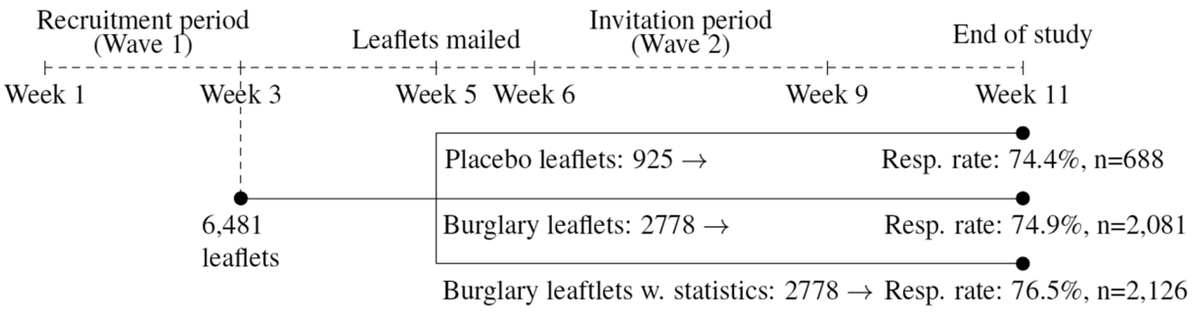

Figure 1. Overview of the experimental design

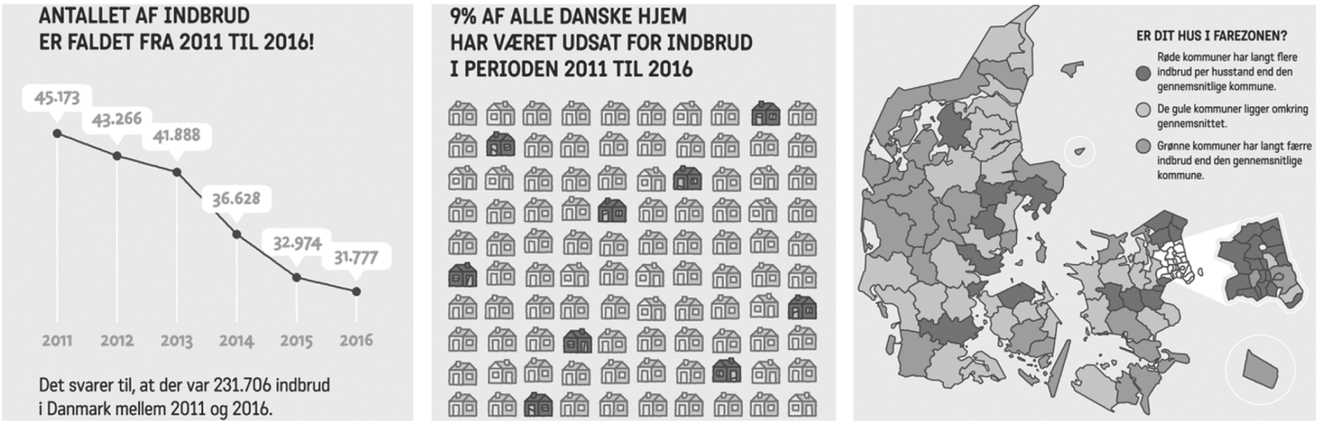

Figure 2. Three data visualizations (translated). *Left*, "The number of burglaries has decreased from 2011 to 2016! In total, there were 231,706 burglaries in Denmark between 2011 and 2016." *Middle*, "9% of all Danish homes have been burglarized in the period between 2011 and 2016." *Right*, "Is your 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.

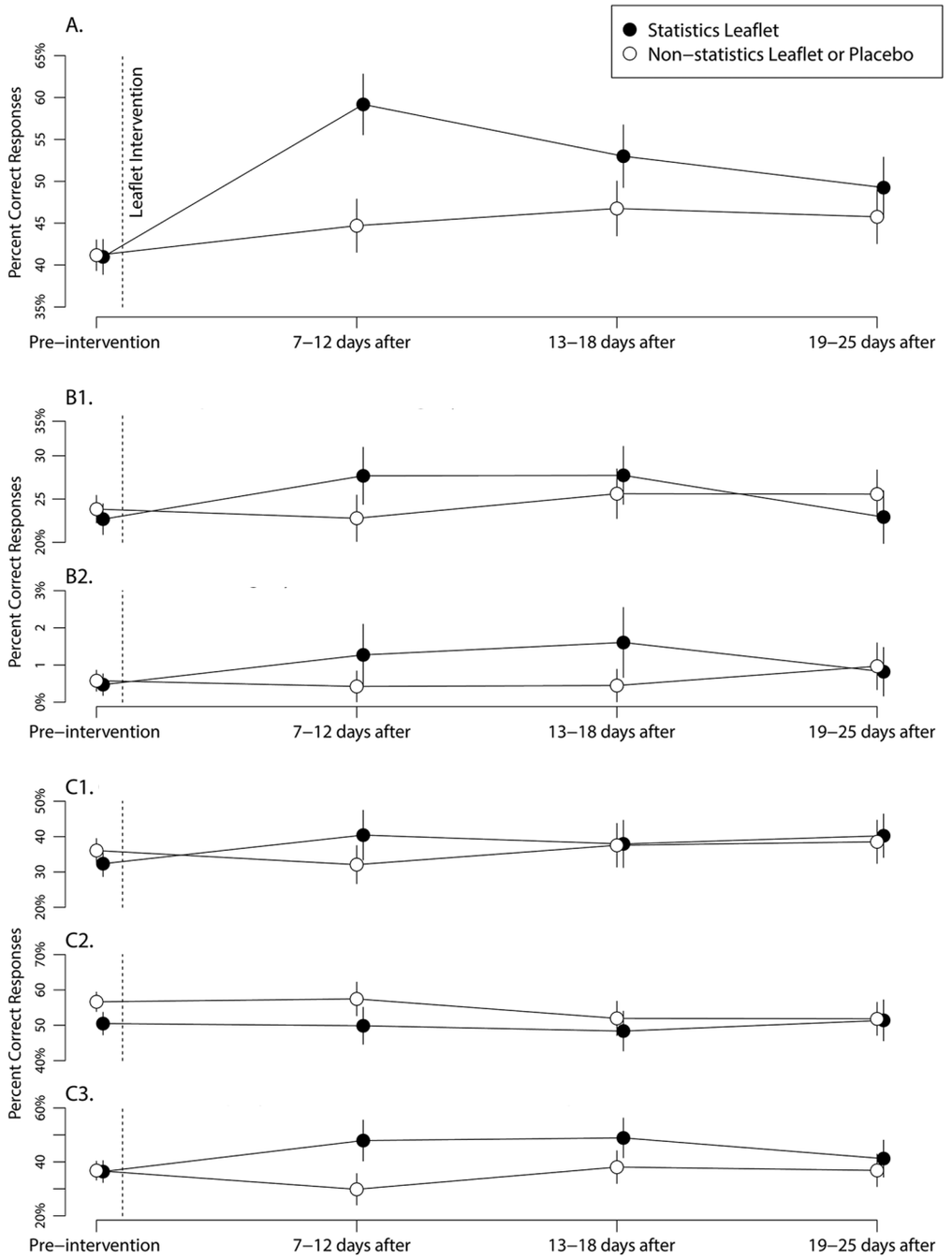

Figure 3. Dots represent the percentage of correct responses with 95% confidence intervals for treatment and control groups across time for each of the three outcomes. *A*, Correct response: declining trend in burglaries; *B*1, correct response: 9% burglary rate (±2 percentage points); *B*2, 9% burglary 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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Online Appendix for "Reducing Bias in Citizens' Perception of Crime Rates: Evidence From a Field Experiment on Burglary Prevalence". The Journal of Politics.

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A Details about the leaflets

We designed seven different leaflets with the help of a professional advertising bureau. All leaflets were four pages long and they all had the same sender: TrygFonden which is a Danish foundation with the stated aim of helping Danes live productive, healthy, and safe lives. There was one placebo leaflet which encouraged families with dogs to visit nursing homes. The remaining six burglary leaflets each contain two of four information packages. The six leaflets included all possible combinations of these packages.

1. Statistical information about the prevalence of burglaries (S; see main text for detailed description). Figure A1 shows how the information was presented in the leaflet (S).

2. Advice about how to avoid burglaries I: Portrays a scene with a family coming home from vacation. They meet their neighbor who tells them that there has been a string of burglaries in another part of town. The neighbor then lists three things that people do in their neighborhood in order to avoid burglaries (P).

3. Advice about how to avoid burglaries II: Shows a family coming home from vacation. They meet their neighbor who tells them that there has been a burglary in their home. The neighbor then lists three things that they could have done in order to avoid being burglarized (the same three things as in the positive narrative) (N).

4. Responsibility assignment for burglaries: A set of scenes with text which are meant to illustrate who is responsible for the prevention of burglaries. A scene with police officers arresting a thief, which informs readers that the police are tasked with solving the crime, and that the police are controlled by the central government. A scene with municipal workers fixing a streetlight, which informs readers that the municipality is responsible for creating safe residential areas, and that the municipality is run by the city council and the mayor. A scene with citizens hanging up a sign for a neighborhood watch group and securing their homes, which informs citizens that they can make a difference when it comes to preventing burglaries (A).

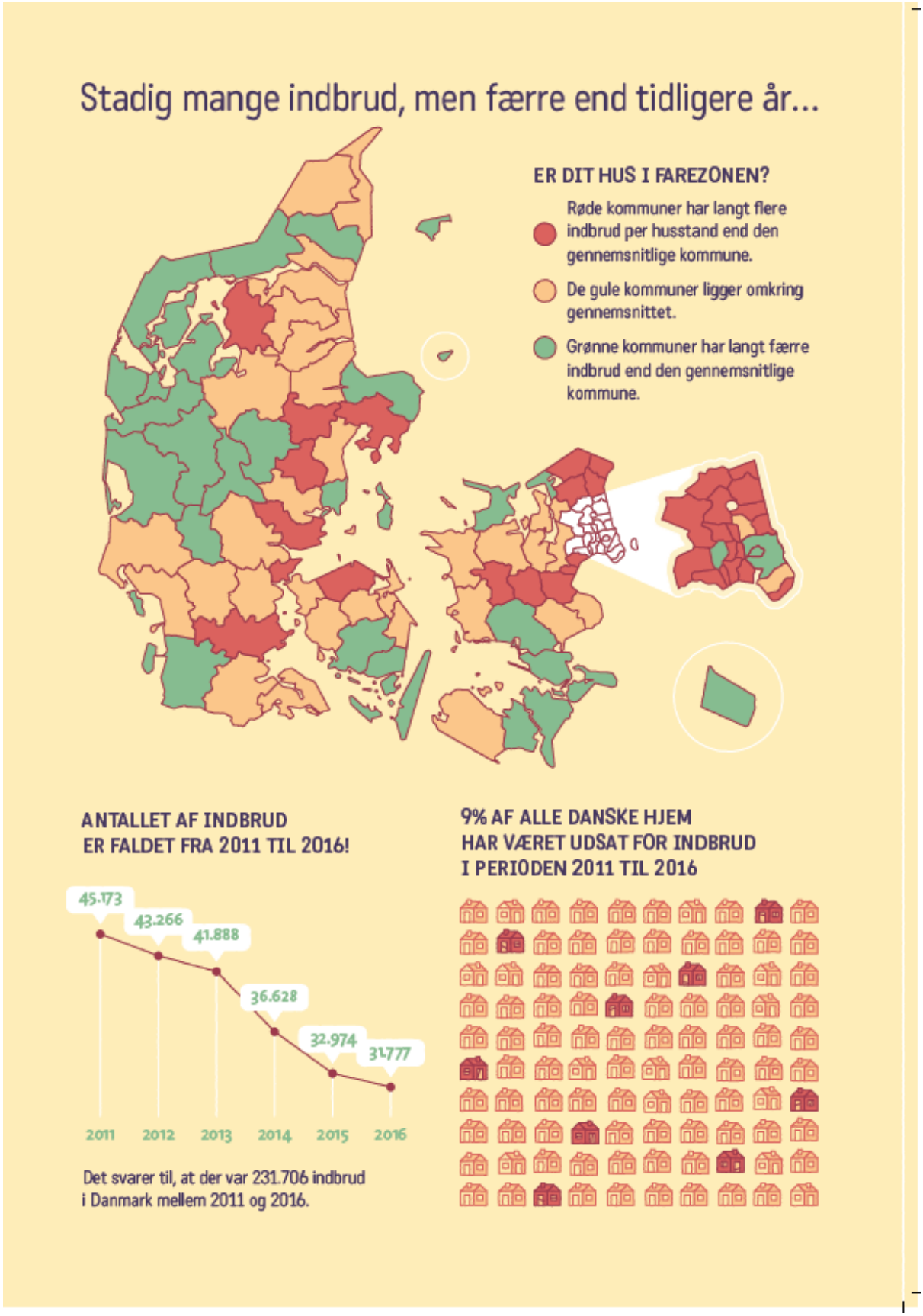

Figure A1: The statistical information as it was displayed in the leaflet.

On the first page of each burglary leaflet is a common headline (Avoid Burglary), the Trygfonden logo, and an excerpt from one of the information packages (the one from page three). The second page includes one of the information packages. The third page includes another of the information packages. The fourth page includes a common headline (Want to know more about how to avoid burglary?), a link to a website where there is more information, the TrygFonden logo, and an excerpt from one of the information packages (the one from page two).

The six burglary leaflets contain the following composition of treatments: S-N, P-S, N-A, A-P, S-A, P-N. The first letter refers to the information package displayed on pages two and four. The second letter refers to the information package displayed on pages one and three.

Since we are only interested in the effect of the statistical information, we collapse participants who received this information package with those who did not. As such, when we look at the effect of receiving statistical information about burglary rates we are comparing those who received information package combinations S-N, P-S and S-A with those who received the information package combinations N-A, A-P, P-N plus those who received the placebo leaflet.

B Descriptive statistics, balance, and attrition

Table B1: Descriptive statistics

Statistic	N	Mean	St. Dev.	Min	Max
DV, wave 1: Trend (pct. correct)	4,895	41.08	49.20	0	100
DV, wave 2: Trend (pct. correct)	4,895	49.21	50.00	0	100
DV, wave 1: Level exact (pct. correct)	4,895	0.53	7.27	0	100
DV, wave 2: Level exact (pct. correct)	4,895	0.88	9.33	0	100
DV, wave 1: Level +/- 2 pp (pct. correct)	4,895	23.33	42.30	0	100
DV, wave 2: Level +/- 2 pp (pct. correct)	4,895	25.25	43.45	0	100
DV, wave 1: Relative (pct. correct)	4,895	43.82	49.62	0	100
DV, wave 2: Relative (pct. correct)	4,895	44.62	49.71	0	100
Females (share)	4,895	0.35	0.48	0	1
Age (years)	4,895	64.90	9.04	31	92
Fear of burglary (1-7)	4,895	2.82	1.74	1	7
Interest in local politics (1-4)	4,895	2.16	0.83	1	5

Table B2: Balance test across treatments

Variable	Statistics leaflet	Non-statistics leaflet	T-test (p-value)
Females (%)	35	35.5	0.7325
Age (years)	64.7	65.1	0.1469
Fear of burglary (1–7)	2.8	2.8	0.5022
Interest in local politics (1–4)	2.2	2.1	0.2736
Attrition (%)	23.4	25.2	0.1032

n=4,895

Table B3: Balance test across time

Variable	7-12 days	13-18 days	19-25 days	F-test (p-value)
Females (%)	35.8	33.8	35.8	0.3778
Age (years)	64.1	65.3	65.3	$p<0.001$
Fear of burglary (1–7)	2.9	2.8	2.8	0.3031
Interest in local politics (1–4)	2.2	2.2	2.1	0.4343
Attrition rate (%)	21.3	24.8	27.1	$p<0.001$
Observations	1,652	1,579	1,664	-

n=4,895

C Placebo outcome: Effect on unemployment

For each outcome variable asking participants about burglary prevalence we included identical questions about unemployment. These items are intended as placebo outcomes because none of the leaflets contained any information on unemployment. We would therefore expect no difference in between leaflets on citizens' knowledge about the trend, level, and relative unemployment rate. As in the case of burglaries, we measure participants' perception of unemployment rates using the following three questions: (A) If you compare year 2011 to year 2016 has there been less or more unemployed people in 2016 compared to 2011? (Less in 2016 compared to 2011, almost the same number in 2011 and 2016, more in 2016 compared to 2011). (B) Think about the continuous period from year 2011 to year 2016 as a whole. What percentage of the Danes were, on average, unemployed in the period? (C) Please compare your own municipality to the rest of Denmark. In your municipality, has there been a lower or higher rate of unemployment in 2016? (lower in my municipality, almost the same as in the rest of the country, higher in my municipality).

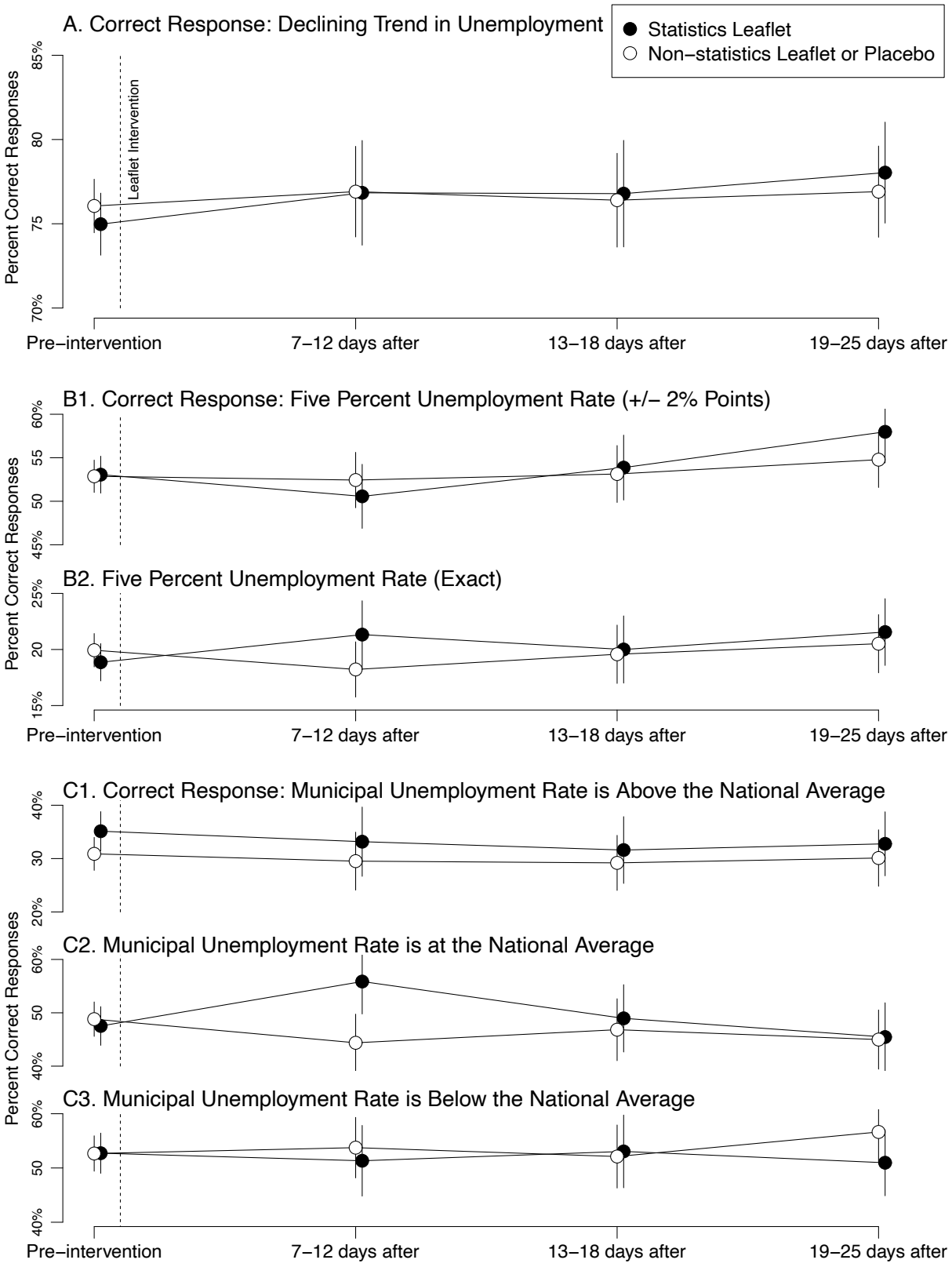

Figure C1: Dots represent the percentage of correct responses with 95% confidence intervals for treatment and control groups across time for each of the three placebo outcomes. Panels A, B1, and B2 each rely on the full sample (n=4,895). In panel C1-C3 results are divided based on whether participants live in a municipality with an above average, around average or below average unemployment rate.

D Average treatment effects and treatment effects on the treated

Figure D1 looks at the difference between the treatment and control group, i.e., the average treatment effect, rather than the levels shown in the main manuscript.

Table D1 also presents the average treatment effects (ATE) as well as their confidence intervals. The ATE is of special interest because it tells us that we can achieve this effect by simply sending a leaflet with correct information to Danish citizens, i.e., it is an intent-to-treat effect. As such, the ATE does not reflect the actual effect of reading the information laid out in the leaflet.

As mentioned in the article, 46 percent of participants said that they had received a leaflet from Trygfonden. If this reflects that 46 percent of participants have read the information laid out in the leaflet, we can tentatively estimate the effect of reading the leaflet among the people who read the leaflet, i.e., the treatment effect on the treated (TOT), by assuming that the ATE is concentrated on the proportion of participants who said they received the leaflet. Following Gerber and Green (2012, Chapter 5) we can calculate this quantity as $TOT = ATE/.46$. We present the result of these calculations in Table D1, so that the readers might get an idea of the sizes of these effects. It is important to note, however, that these TOT estimates could be inflated, because participants might have read the leaflet but simply forgotten that they had done so, when answering the second survey. Potentially, our estimate of the TOT effects could also be too small, if some voters report receiving a leaflet without actually having read it.

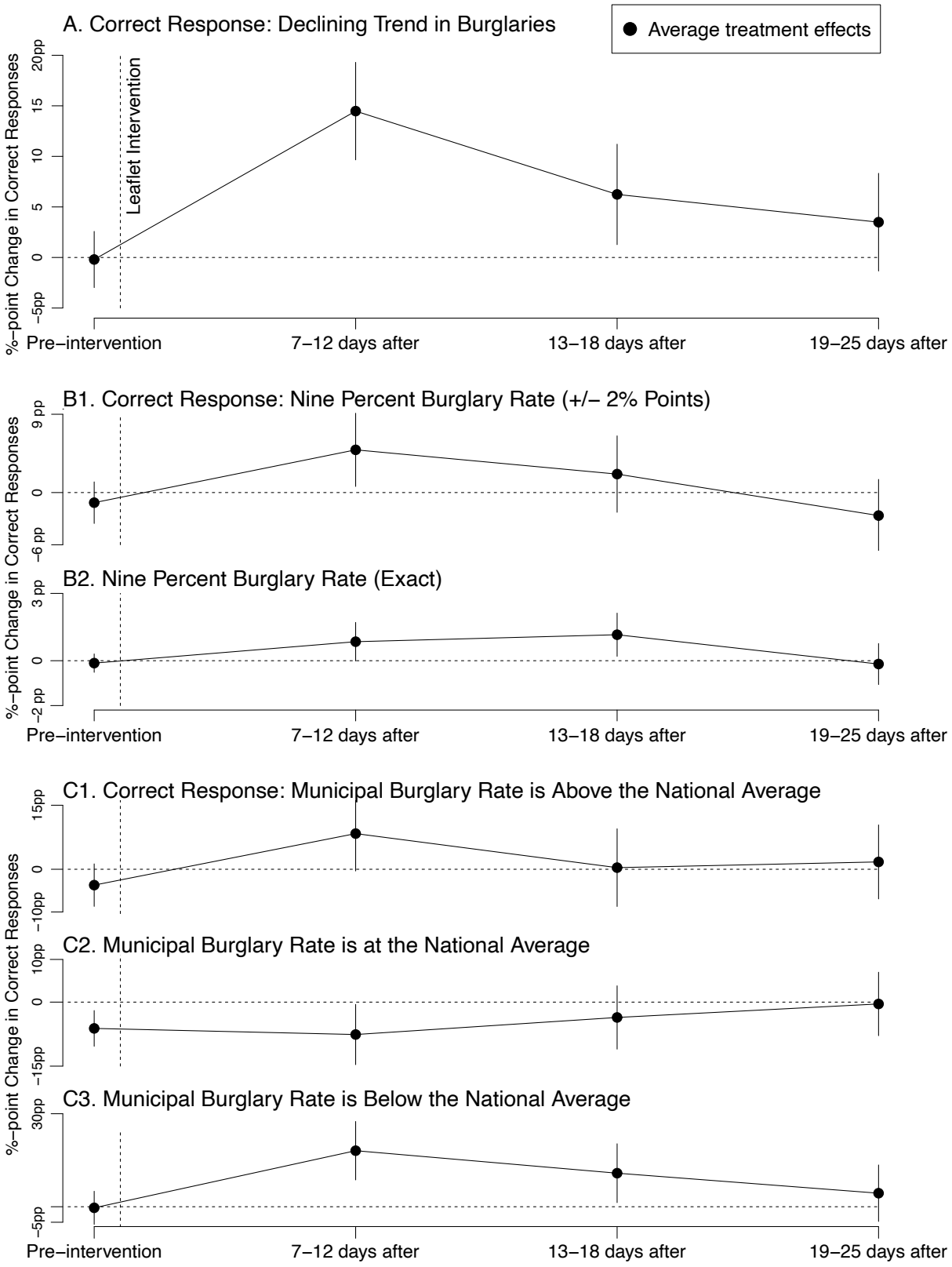

Figure D1: Dots represent the average treatment effect of receiving a leaflet with statistical information on the percentage of correct responses across time for each of the three dependent variables. Panels A, B1, and B2 each rely on the full sample (n=4,895). In panel C1-C3 results are divided based on 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.

Table D1: Average Treatment Effects and Treatment effects on the Treated (TOT)

	Trend		Level (+/-2pp)		Level (exact)		Relative (above)		Relative (average)		Relative (below)	
	ATE	TOT	ATE	TOT	ATE	TOT	ATE	TOT	ATE	TOT	ATE	TOT
Pre	-0.2	-0.4	-1.2	-2.5	-0.1	-0.2	-3.7	-8.1	-6.2	-13.4	-0.4	-0.8
	[-3 ; 2.6]		[-3.6 ; 1.2]		[-0.5 ; 0.3]		[-8.7 ; 1.3]		[-2 ; -10.4]		[-5.7 ; 4.9]	
7-12 days	14.5	31.5	4.9	10.7	0.8	1.8	8.4	18.2	-7.6	-16.5	18.1	39.3
	[9.7 ; 19.3]		[0.7 ; 9.1]		[-0.1 ; 1.7]		[-0.4 ; 17.2]		[-14.7 ; -0.5]		[8.7 ; 27.5]	
13-18 days	6.2	13.6	2.1	4.6	1.2	2.2	0.4	0.8	-3.6	-7.8	10.8	23.5
	[1.2 ; 11.2]		[-2.3 ; 6.5]		[0.2 ; 2.2]		[-8.7 ; 9.5]		[-11 ; 3.8]		[1.3 ; 20.3]	
19-25 days	3.5	7.6	-2.6	-5.7	-0.1	-0.3	1.7	3.7	-0.4	-0.9	4.4	9.5
	[-1.3 ; 8.3]		[-6.8 ; 1.5]		[-1 ; 0.8]		[-7 ; 10.4]		[-7.8 ; 7]		[-4.7 ; 13.5]	

ATE is percentage point difference in correct responses between the treatment and the control group. 95% confidence intervals. TOT effects calculated by dividing the ATE by the overall observed compliance rate (0.46).

E Recreating the results using logistic regression models

Tables F1, F2, F3 and F4 present estimates from a set of logistic regression models models with answering correctly correctly as a function of whether the participants were sent a leaflet with statistical information. Each model includes a number of controls: age, gender, educational attainment, income as well as place of residence (i.e., which region you live in). Each table covers one of the four time periods examined (before the intervention, 7-12 days after, 13–18 days after, 19–25 days after). The results laid out in these tables line up with the results presented in the article. The statistical information makes it more likely that participants give a correct answer, this is the case across dependent variables, and the largest effect is for the trend variable.

Table E1: Pre-intervention: Controlling for pre-treatment variables (Logistic regression)

	Trend	Level: +/-2	Level: Exact	Above avg.	Avg.	Below avg.
	(1)	(2)	(3)	(4)	(5)	(6)
Statistics leaftlet	0.005	−0.06	−0.22	−0.02	−0.25**	−0.17
	(0.06)	(0.07)	(0.41)	(0.12)	(0.09)	(0.12)
Female	−0.38**	−0.01	0.13	−0.25	0.27**	0.05
	(0.07)	(0.08)	(0.45)	(0.13)	(0.10)	(0.13)
Age (years)	0.01**	0.01	0.01	0.01	−0.01	−0.01
	(0.004)	(0.004)	(0.02)	(0.01)	(0.01)	(0.01)
Vocational training (ref: high school)	0.21	0.12	0.75	0.26	−0.03	0.35
	(0.14)	(0.17)	(1.06)	(0.26)	(0.20)	(0.37)
–Short-cycle tertiary	0.30	−0.001	0.78	0.18	−0.18	0.49
	(0.16)	(0.18)	(1.13)	(0.31)	(0.22)	(0.39)
–Medium-cycle tertiary	0.47**	0.20	0.34	0.60*	−0.05	0.69
	(0.14)	(0.16)	(1.08)	(0.26)	(0.20)	(0.36)
–Long-cycle tertiary	0.43**	0.22	0.23	0.46	−0.17	0.93*
	(0.15)	(0.18)	(1.21)	(0.29)	(0.22)	(0.38)
–Other	0.52*	0.14	−15.67	0.55	−0.22	0.64
	(0.21)	(0.24)	(2,194.30)	(0.42)	(0.30)	(0.48)
Income: 150K-249K (ref: <150K)	0.20	0.16	0.33	−0.16	−0.18	−0.15
	(0.14)	(0.17)	(0.81)	(0.27)	(0.20)	(0.32)
–250K-349K	0.16	0.19	0.03	−0.03	−0.04	−0.30
	(0.14)	(0.16)	(0.84)	(0.26)	(0.20)	(0.31)
–350K-499K	0.20	0.25	0.23	0.03	−0.28	−0.38
	(0.14)	(0.16)	(0.83)	(0.27)	(0.20)	(0.31)
–500K-599K	0.39*	0.14	−16.26	0.36	−0.31	−0.24
	(0.16)	(0.19)	(1,437.43)	(0.33)	(0.23)	(0.34)
–600K-699K	0.12	0.09	0.19	−0.02	−0.36	−0.15
	(0.20)	(0.23)	(1.29)	(0.40)	(0.29)	(0.40)
–700K-799K	−0.16	0.04	0.42	0.11	−0.22	0.89*
	(0.22)	(0.25)	(1.29)	(0.44)	(0.31)	(0.43)
–800K-	0.18	0.04	−0.08	−0.03	−0.37	−0.25
	(0.19)	(0.22)	(1.31)	(0.38)	(0.29)	(0.37)
–Do not want to report	−0.09	0.004	−16.34	−0.14	−0.32	−0.27
	(0.15)	(0.17)	(1,095.15)	(0.29)	(0.21)	(0.32)
Region: M. Jutland (ref: N. Jutland)	0.38**	0.13	0.03	0.86**	0.15	−0.80
	(0.12)	(0.13)	(0.60)	(0.24)	(0.15)	(0.49)
–Southern Denmark	0.23*	0.04	−0.68	0.56*	0.20	−0.07
	(0.11)	(0.13)	(0.65)	(0.26)	(0.13)	(0.47)
–Zealand	0.41**	0.08	−1.35	0.01	0.22	−0.81
	(0.12)	(0.14)	(0.87)	(0.27)	(0.15)	(0.48)
–Capital	0.17	0.06	−0.83	0.59*	0.50*	0.59
	(0.12)	(0.13)	(0.72)	(0.26)	(0.21)	(0.45)
Intercept	−1.75**	−1.90**	−5.76**	−1.88**	0.83	−0.73
	(0.31)	(0.36)	(2.14)	(0.63)	(0.44)	(0.76)
N	4,895	4,895	4,895	1,276	2,211	1,408
Log Likelihood	−3,254.63	−2,650.62	−150.49	−812.25	−1,506.27	−834.05
Akaike Inf. Crit.	6,551.27	5,343.24	342.99	1,666.50	3,054.54	1,710.10

Notes: Logit coefficients with standard errors. *p<.05; **p<.01

Table E2: Days 7-12: Controlling for pre-treatment variables (Logistic regression)

	Trend	Level: +/-2	Level: Exact	Above avg.	Avg.	Below avg.
	(1)	(2)	(3)	(4)	(5)	(6)
Statistics leaftlet	0.63**	0.28*	1.26*	0.93**	−0.33*	0.39
	(0.10)	(0.12)	(0.62)	(0.23)	(0.15)	(0.21)
Female	−0.51**	−0.09	0.11	−0.67**	0.20	0.35
	(0.11)	(0.13)	(0.64)	(0.25)	(0.17)	(0.24)
Age (years)	0.02**	0.004	−0.002	0.01	−0.001	−0.001
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)
Vocational training (ref: high school)	0.31	−0.24	−0.69	0.23	0.09	−0.15
	(0.24)	(0.27)	(1.21)	(0.48)	(0.33)	(0.75)
–Short-cycle tertiary	0.46	−0.01	−18.00	0.20	0.06	0.48
	(0.26)	(0.30)	(2,859.85)	(0.55)	(0.36)	(0.79)
–Medium-cycle tertiary	0.67**	0.17	−0.98	1.05*	0.22	0.57
	(0.24)	(0.26)	(1.22)	(0.47)	(0.32)	(0.74)
–Long-cycle tertiary	0.56*	0.32	0.12	0.80	0.28	0.28
	(0.26)	(0.29)	(1.25)	(0.52)	(0.36)	(0.77)
–Other	0.85*	−0.14	0.91	1.45*	0.08	0.51
	(0.35)	(0.41)	(1.53)	(0.74)	(0.48)	(0.89)
Income: 150K-249K (ref: <150K)	0.04	−0.29	−17.77	−0.33	−0.29	−0.58
	(0.24)	(0.27)	(2,732.93)	(0.49)	(0.35)	(0.56)
–250K-349K	−0.01	−0.10	0.02	0.05	−0.16	−0.02
	(0.23)	(0.26)	(1.19)	(0.46)	(0.34)	(0.50)
–350K-499K	0.19	−0.05	0.79	0.52	−0.08	−0.52
	(0.23)	(0.26)	(1.13)	(0.46)	(0.35)	(0.50)
–500K-599K	0.16	−0.52	−18.08	−0.66	−0.60	−0.06
	(0.26)	(0.31)	(3,530.94)	(0.57)	(0.39)	(0.56)
–600K-699K	−0.09	−0.06	−18.01	−1.54	−0.13	−0.29
	(0.33)	(0.36)	(5,568.58)	(0.90)	(0.49)	(0.66)
–700K-799K	0.26	0.15	−18.25	−0.45	−0.22	0.60
	(0.40)	(0.41)	(7,101.14)	(0.77)	(0.59)	(0.78)
–800K-	0.46	−0.37	−18.22	−0.44	0.27	0.21
	(0.34)	(0.38)	(5,381.58)	(0.70)	(0.54)	(0.63)
–Do not want to report	−0.20	−0.44	−18.11	−0.07	−0.31	−0.36
	(0.24)	(0.28)	(2,783.74)	(0.49)	(0.36)	(0.52)
Region: M. Jutland (ref: N. Jutland)	0.37	−0.01	18.37	1.27*	0.28	−2.14**
	(0.20)	(0.22)	(3,290.06)	(0.61)	(0.25)	(0.72)
–Southern Denmark	0.34	−0.19	16.11	0.80	0.49*	−0.83
	(0.19)	(0.21)	(3,290.06)	(0.64)	(0.22)	(0.64)
–Zealand	0.27	−0.18	17.43	0.51	0.41	−1.50*
	(0.21)	(0.23)	(3,290.06)	(0.64)	(0.25)	(0.66)
–Capital	0.01	−0.25	17.74	0.81	0.82*	−0.13
	(0.20)	(0.22)	(3,290.06)	(0.65)	(0.36)	(0.59)
Intercept	−1.88**	−1.17*	−22.15	−3.07*	−0.05	−0.35
	(0.53)	(0.59)	(3,290.06)	(1.26)	(0.75)	(1.21)
N	1,652	1,652	1,652	405	769	478
Log Likelihood	−1,092.52	−912.02	−56.02	−238.05	−519.75	−276.71
Akaike Inf. Crit.	2,227.03	1,866.04	154.05	518.11	1,081.49	595.43

Notes: Logit coefficients with standard errors. *p<.05; **p<.01

Table E3: Days 13-18: Controlling for pre-treatment variables (Logistic regression)

	Trend	Level: +/-2	Level: Exact	Above avg.	Avg.	Below avg.
	(1)	(2)	(3)	(4)	(5)	(6)
Statistics leaftlet	0.25*	0.10	1.21*	0.38	−0.14	0.13
	(0.10)	(0.12)	(0.59)	(0.21)	(0.15)	(0.21)
Female	−0.58**	−0.17	−0.63	−0.65**	0.26	0.17
	(0.12)	(0.13)	(0.69)	(0.24)	(0.17)	(0.24)
Age (years)	0.01*	0.001	0.005	−0.02	0.01	−0.01
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)
Vocational training (ref: high school)	0.16	−0.11	−0.38	0.32	−0.07	1.02
	(0.25)	(0.28)	(1.18)	(0.47)	(0.37)	(0.62)
–Short-cycle tertiary	0.38	−0.09	−0.40	1.08*	0.16	1.19
	(0.28)	(0.31)	(1.46)	(0.55)	(0.42)	(0.66)
–Medium-cycle tertiary	0.59*	−0.02	−0.20	0.43	−0.02	1.16
	(0.25)	(0.27)	(1.14)	(0.47)	(0.37)	(0.61)
–Long-cycle tertiary	0.54*	0.20	1.08	0.17	−0.05	1.44*
	(0.27)	(0.30)	(1.15)	(0.53)	(0.41)	(0.65)
–Other	0.34	−0.34	−16.59	0.25	0.44	−0.48
	(0.39)	(0.46)	(3,908.88)	(0.76)	(0.56)	(1.22)
Income: 150K-249K (ref: <150K)	−0.06	−0.11	−0.57	0.01	0.05	−0.44
	(0.24)	(0.27)	(1.27)	(0.42)	(0.35)	(0.58)
–250K-349K	0.01	−0.12	0.14	0.13	0.12	−0.58
	(0.23)	(0.26)	(1.16)	(0.40)	(0.34)	(0.57)
–350K-499K	0.10	0.14	−0.14	0.22	−0.20	−1.02
	(0.23)	(0.26)	(1.16)	(0.42)	(0.33)	(0.57)
–500K-599K	−0.01	−0.16	−17.39	0.13	−0.29	−1.00
	(0.29)	(0.32)	(2,601.66)	(0.56)	(0.43)	(0.65)
–600K-699K	−0.27	−0.30	−17.65	0.19	−0.25	−0.23
	(0.33)	(0.38)	(3,341.14)	(0.63)	(0.52)	(0.70)
–700K-799K	−0.24	−0.06	−17.76	0.99	−0.11	0.29
	(0.36)	(0.40)	(3,755.75)	(0.75)	(0.51)	(0.79)
–800K-	−0.07	−0.28	−17.82	−0.11	−0.02	−0.65
	(0.32)	(0.36)	(2,968.60)	(0.59)	(0.51)	(0.67)
–Do not want to report	−0.35	−0.24	−0.72	−0.32	0.07	−0.84
	(0.25)	(0.28)	(1.31)	(0.45)	(0.36)	(0.59)
Region: M. Jutland (ref: N. Jutland)	−0.03	0.18	15.49	0.71	−0.11	0.65
	(0.19)	(0.23)	(2,095.53)	(0.37)	(0.25)	(1.16)
–Southern Denmark	0.001	0.35	17.31	0.14	0.40	1.07
	(0.19)	(0.23)	(2,095.53)	(0.40)	(0.23)	(1.15)
–Zealand	0.01	0.59*	16.36	−0.24	0.35	0.62
	(0.20)	(0.23)	(2,095.53)	(0.40)	(0.26)	(1.14)
–Capital	0.10	0.48*	17.06	0.21	1.02*	1.39
	(0.19)	(0.23)	(2,095.53)	(0.40)	(0.44)	(1.11)
Intercept	−1.13*	−1.35*	−21.76	0.08	−0.75	−1.82
	(0.53)	(0.59)	(2,095.53)	(1.04)	(0.78)	(1.57)
N	1,579	1,579	1,579	425	714	440
Log Likelihood	−1,062.51	−902.41	−70.51	−273.77	−483.23	−274.92
Akaike Inf. Crit.	2,167.02	1,846.81	183.02	589.54	1,008.46	591.83

Notes: Logit coefficients with standard errors. *p<.05; **p<.01

Table E4: Days 19-25: Controlling for pre-treatment variables (Logistic regression)

	Trend	Level: +/-2	Level: Exact	Above avg.	Avg.	Below avg.
	(1)	(2)	(3)	(4)	(5)	(6)
Statistics leaftlet	0.17	−0.14	−0.22	0.19	−0.04	0.11
	(0.10)	(0.12)	(0.54)	(0.20)	(0.16)	(0.20)
Female	−0.30**	−0.06	−0.30	−0.23	0.29	0.17
	(0.11)	(0.13)	(0.63)	(0.23)	(0.17)	(0.23)
Age (years)	0.01*	0.01	0.004	0.01	−0.02	−0.002
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)
Vocational training (ref: high school)	0.20	−0.07	17.34	0.02	−0.04	1.09
	(0.24)	(0.28)	(2,848.03)	(0.42)	(0.37)	(0.68)
–Short-cycle tertiary	0.17	0.05	0.23	0.08	0.24	0.81
	(0.26)	(0.30)	(3,495.76)	(0.48)	(0.39)	(0.72)
–Medium-cycle tertiary	0.33	0.01	16.95	0.01	−0.08	1.08
	(0.23)	(0.27)	(2,848.03)	(0.42)	(0.35)	(0.66)
–Long-cycle tertiary	0.43	0.19	17.87	0.14	0.13	1.73*
	(0.25)	(0.29)	(2,848.03)	(0.47)	(0.39)	(0.68)
–Other	−0.14	0.07	0.58	−0.78	−0.17	0.89
	(0.35)	(0.40)	(4,632.43)	(0.77)	(0.55)	(0.81)
Income: 150K-249K (ref: <150K)	0.21	0.07	−1.42	0.02	−0.34	0.11
	(0.25)	(0.30)	(1.03)	(0.60)	(0.35)	(0.60)
–250K-349K	0.42	0.35	−1.57	0.30	0.06	0.16
	(0.25)	(0.30)	(1.04)	(0.61)	(0.34)	(0.58)
–350K-499K	0.24	0.16	−1.06	0.20	−0.07	0.70
	(0.25)	(0.30)	(0.94)	(0.62)	(0.34)	(0.57)
–500K-599K	0.60*	0.58	−0.76	0.27	−0.34	0.71
	(0.29)	(0.34)	(1.11)	(0.71)	(0.41)	(0.63)
–600K-699K	−0.24	0.53	−18.18	0.62	−0.74	0.84
	(0.36)	(0.40)	(3,592.92)	(0.80)	(0.52)	(0.75)
–700K-799K	0.28	0.31	−18.14	−0.23	0.48	1.05
	(0.37)	(0.42)	(3,846.61)	(0.91)	(0.55)	(0.74)
–800K-	0.62	0.45	−18.21	−0.49	−0.74	0.26
	(0.33)	(0.38)	(3,069.87)	(0.81)	(0.50)	(0.67)
–Do not want to report	0.13	0.01	−0.64	0.40	−0.32	0.29
	(0.26)	(0.32)	(0.99)	(0.64)	(0.37)	(0.60)
Region: M. Jutland (ref: N. Jutland)	0.39	−0.01	0.59	0.38	0.37	0.33
	(0.21)	(0.23)	(1.12)	(0.40)	(0.30)	(1.25)
–Southern Denmark	0.34	−0.18	0.40	0.78	0.57*	0.74
	(0.21)	(0.23)	(1.10)	(0.42)	(0.26)	(1.25)
–Zealand	0.33	−0.30	−0.89	0.40	0.88**	1.12
	(0.22)	(0.24)	(1.43)	(0.44)	(0.29)	(1.23)
–Capital	−0.01	−0.30	−0.54	0.56	0.45	1.81
	(0.21)	(0.23)	(1.25)	(0.43)	(0.37)	(1.21)
Intercept	−1.71**	−1.79**	−20.81	−1.62	0.70	−3.40*
	(0.54)	(0.63)	(2,848.03)	(1.11)	(0.81)	(1.68)
N	1,664	1,664	1,664	446	728	490
Log Likelihood	−1,124.58	−913.73	−75.27	−290.75	−489.67	−292.82
Akaike Inf. Crit.	2,291.16	1,869.46	192.53	623.50	1,021.35	627.64

Notes: Logit coefficients with standard errors. *p<.05; **p<.01

F Placebo and individual leaflet effects for trend outcome

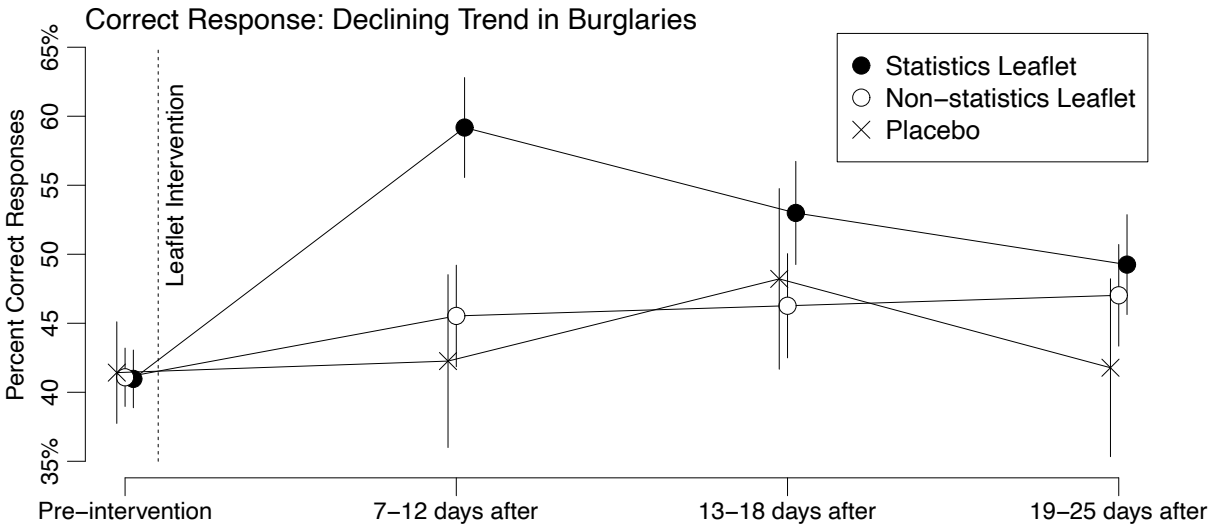

Figure F1: Correct response for the trend question with separate estimates for the placebo group. N=4,895.

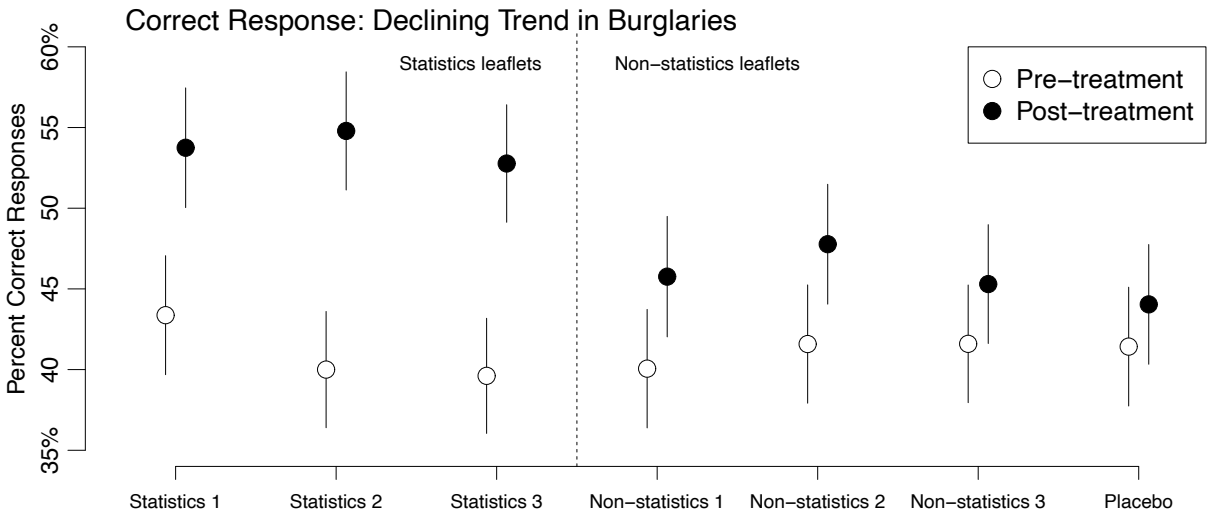

Figure F2: Average correct response for the trend question for each of the seven leaflets described in Appendix A. N=4,895.

G Treatment effects by interest in local affairs

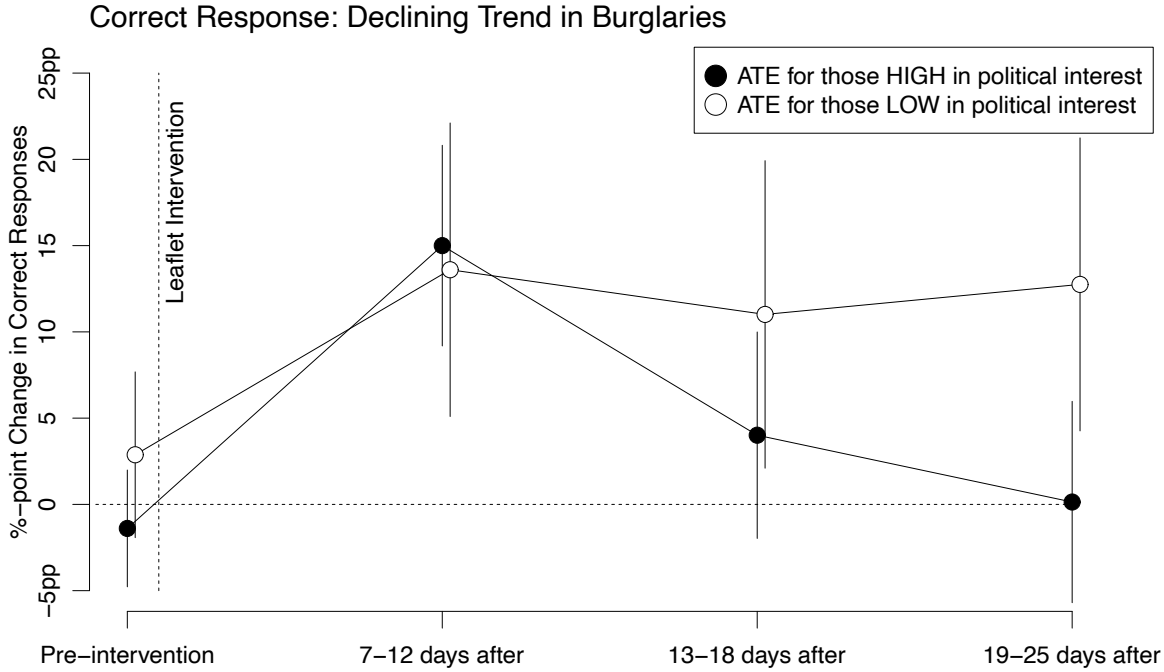

Figure G1: HIGH political interest includes participants indicating that they are "very interested in local politics" (n=1,032) or "quite interested in local politics" (n=2,344). Total n=3,376. LOW political interest includes participants indicating "a little interested in local politics" (n=1,261), "not at all interested in local politics" (n=219), or "don't know" (n=39). Total n=1,519.

Table G1: Declining Trend in Burglaries (Interaction with Level of Political Interest)

	Pre intervention	After 7 to 12 days	After 13 to 18 days	After 19 to 25 days
	(1)	(2)	(3)	(4)
Statistics leaftlet	2.88	13.60**	11.01*	12.75**
	(2.53)	(4.35)	(4.57)	(4.42)
High political interest	11.72**	10.49**	7.11*	16.40**
	(2.02)	(3.46)	(3.58)	(3.62)
Interaction	−4.27	1.40	−7.00	−12.61*
	(3.05)	(5.26)	(5.49)	(5.31)
Intercept	33.02**	37.50**	41.90**	33.97**
	(1.69)	(2.86)	(2.96)	(3.07)
N	4,895	1,652	1,579	1,664
R^2	0.01	0.03	0.01	0.01
Adjusted R^2	0.01	0.03	0.004	0.01
Residual Std. Error	49.00 (df = 4891)	49.26 (df = 1648)	49.90 (df = 1575)	49.64 (df = 1660)
F Statistic	14.73** (df = 3; 4891)	17.72** (df = 3; 1648)	3.33* (df = 3; 1575)	7.85** (df = 3; 1660)

Notes: OLS coefficients with standard errors. *p<.05; **p<.01

References

Gerber, A. S. and Green, D. P. (2012). *Field experiments: Design, analysis, and interpretation*. WW Norton.