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Larsen, Martin Vinæs; Olsen, Asmus Leth

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

Martin Vinæs Larsen, Aarhus University  
Asmus Leth Olsen,<sup>1</sup> University of Copenhagen

**Abstract:** 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. The effects are not short lived – they are detectable several weeks after the mailer was sent, but they are temporary. Eventually the perceptual bias re-emerges. 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.

<sup>1</sup>Corresponding author, [ajlo@ifs.ku.dk](mailto:ajlo@ifs.ku.dk). The authors would like to thank TrygFonden for funding this research as part of the project "Raising Awareness without Raising Fears" (ID: 119942). The authors would also like to thank Cecilie Weischer for her diligent work as a research assistant on the project. For ideas and suggestions regarding the design of the study the authors would like to thank Martin Bisgaard and Jens Olav Dahlgaard.

In most western societies crime is on the retreat (Pinker, 2012). Yet, most citizens think that crime is on the rise. Across the past 30 years numerous surveys have documented that a large majority Americans think that crime is increasing when it is in fact decreasing (Gramlich, 2016, 2017; Gallup, 2017). This is not a uniquely American phenomenon, as we see similar perceptual biases in, for instance, England (Office for National Statistics, 2017), 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 mental health (Dustmann and Fasani, 2016), social trust (Gainey et al., 2011) and economic outcomes (Buonanno et al., 2013). In politics, this bias makes it difficult for citizens to hold politicians accountable for their ability to provide public safety. A top priority for many voters. 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 the type of information citizens are typically exposed to is about specific cases of crime and not the broader context (i.e., 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 distorts 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 (i.e., the leader of this party might insist that crime is not decreasing) or stereotypical beliefs about outgroups (i.e., that a wave of immigration is driving up crime rates) (Lodge and Taber, 2013). Studies have found that beliefs about crime is strongly linked to citizens' attitudes towards immigrants and

racial minorities (Hurwitz and Peffley, 1997). Beyond this, people might have good reason to resist information if their everyday experience contradicts 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 for public information campaigns, we conduct a field experiment coupled with a two-wave panel survey. Some of the panelists thus receive correct information about the prevalence of burglaries between the two survey waves. The correct information is embedded in a leaflet about how to avoid burglaries, which is mailed directly to the panelists. In order to explore the temporal dynamics of the leaflets' effect, we randomly assign respondents to timing of re-interview. 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 re-emerges.

Besides giving us 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 et al., 2017; Wood and Porter, 2018; Nyhan and Reifler, 2010). Our findings suggest that while it is also possible to correct beliefs outside of a serene survey setting using a scalable intervention, the effect of the corrections are temporary. As such, permanently correcting citizens' misperceptions about crime, and other issues, might not simply be a matter of supplying them with correct information at one point in time, but of supplying such information on a continual basis.

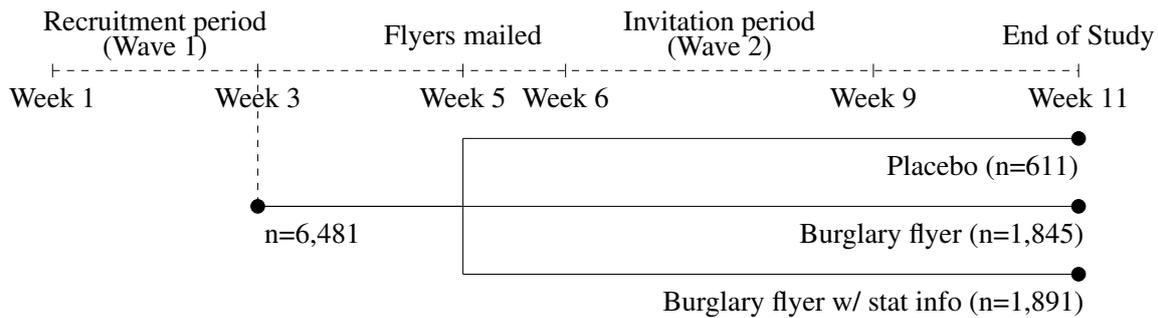
## **EXPERIMENTAL DESIGN**

To explore the effect of public information campaigns about crime on citizens' misperceptions, we designed a field experiment with survey outcomes (Broockman et al., 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 respondents were being recruited, they were given a short survey about their attitudes towards various social

issues, including specific questions about their perception of crime rates.

Two weeks after the final respondent had been recruited, all participants were mailed a leaflet. 43 percent of respondents were in the treatment group. They 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 to add to the realism of the treatment, and to see whether respondents would notice the statistical information in the presence of other information. The remaining respondents received either a leaflet about how to avoid burglaries with no information on burglary rates (43 percent) or a placebo-leaflet on an unrelated topic (14 percent). 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 E, no such effect materialized, and therefore we collapse the two control conditions in the analysis. We used complete random assignment to assign leaflets to respondents. All leaflets were sent out by the organization TrygFonden (a foundation that wants to make Denmark safer; see more at [www.trygfonden.dk/english/](http://www.trygfonden.dk/english/)). To avoid experimenter demand effects, respondents were not told, and the leaflets gave no indication, that there was any relation between the survey and the leaflets. See Appendix A for more details about the content and design of the leaflets.

One week after sending out the leaflets, some participants were invited to a post-treatment survey. A random sample of 350 respondents were invited each day for 18 days and on the 19th day the remaining 181 respondents were invited. Invitation and participation tracked each-other nicely, with 60 percent of participants completing the survey within one day of the invitation and 90 percent within five days. To ensure that all treatment conditions were evenly distributed across timing of invitation to the post-treatment survey, we block randomized by which leaflet the respondent received, randomly assigning respondents to invitation dates within each block. 75 percent of the recruited participants took part in the post-treatment survey (n=4,895). In Appendix B we show that a number of pre-treatment 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 re-invitation.



**Figure 1:** Overview of the Experimental Design

## The statistical information about burglary rates

The statistical information consisted of the three data visualization 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 five years (9 pct.), and (3) a color-coded map of Danish municipalities indicating whether each municipality was in the bottom, middle, or top tercile with respect to burglaries per household. We included different types of information, so that we would be able to gauge the robustness of and variation in any potential effects. To maximize the effectiveness of the treatments, they were designed by an advertising bureau that specializes in data visualizations.

## Dependent variable

We measure participants’ perception of crime rates using the following three questions: (1) “Has there been fewer or more burglaries in 2016 compared to 2011?”; (2) “Think of the continuous period from 2011 to 2016 altogether. What percent of Danish homes were burglarized in this period?”; and (3) “Please compare your own municipality to the rest of Denmark. In your municipality, has there been fewer or more burglaries per household in 2016?”. Answers were given in percent for question 2 (respondents could write down any integer between zero and 100). For questions 1 and 3 respondent’s could report either “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 it was the only question in the two surveys that asked respondents to assess the prevalence of burglaries. Appendix B presents

descriptive statistics. In our analysis, we recode all the dependent variables so that they indicate whether respondents' answered correctly or not. For question 2 we will also look at what happens if one accepts all responses within 2 pp of being correct. For question 3 we will split our analysis depending on the burglary rate in the respondent's municipality (i.e., whether it is in the bottom, middle or top tercile), as the correct response is contingent on this.



**Figure 2:** The three data visualizations. Left (translated): The number of burglaries has decreased from 2011 to 2016! In total there was 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 municipalities have way more burglaries per household than the average municipality. The yellow municipalities are close to the average. Green municipalities have way fewer burglaries than the average municipality.

## RESULTS

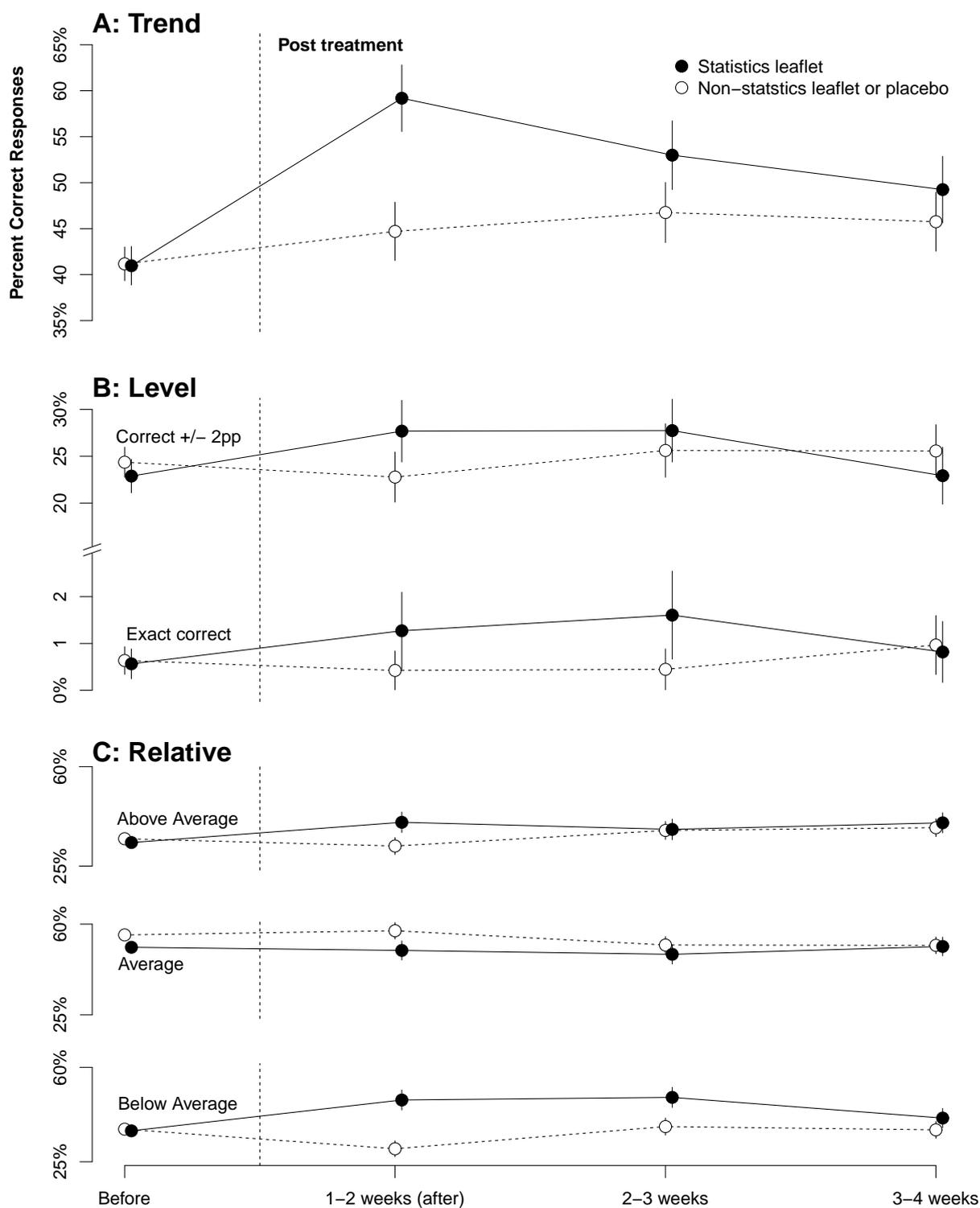
In Figure 3 we observe the proportion of correct responses for the three different questions about burglary prevalence among respondents' who received a leaflet with statistical information and for respondents' who received a different leaflet. To study how these proportions develop over time, we group post-treatment responses based on when respondents were randomly invited to take part in the second survey, constructing three groups of similar size; 1–2 weeks ( $n=1,652$ ), 2–3 weeks ( $n=1,579$ ), and 3–4 weeks ( $n=1,664$ ) after the leaflet was sent out. Across all three questions, we find that the number of correct responses increase in response to receiving a leaflet with statistical information, but also that the effect wanes over time.

In panel A we observe the effect of the treatment on respondents' ability to correctly state that the burglary rate is lower in 2016 than it was in 2011. Prior to the intervention only 41 per-

cent (CI=39.7–42.5) are able to respond correctly with no significant pre-treatment difference ( $p=0.887$ ). 1–2 weeks after the intervention we observe a sizable treatment effect of about 15 percentage points (pp) ( $p<0.001$ ), with a clear majority among those assigned to the statistics leaflet correctly reporting the burglary trend. After 2–3 weeks the treatment effect narrows to about 6 pp ( $p<0.05$ ) and after 3–4 weeks the difference is no longer statistically or substantively significant with a 3 pp difference ( $p=0.157$ ). 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 nonetheless. For one, the drop in effect size is much larger than what can be explained by the slight increase in attrition identified in Appendix B. Further, if attrition was driving the trend in effects, we should see larger effects among those invited later, as marginal respondents – who arguably are less engaged and therefore less likely to be affected by the treatment – drop out of the study.

In panel B we observe the effect of the treatment on respondents' ability to correctly state that the national burglary rate was 9 percent. Prior to the intervention only 0.5 percent were able to provide the exact correct answer (CI=0.35–0.7). 1–2 weeks after the intervention there is some indication of an improvement in the intervention group by about 0.8 pp ( $p<0.1$ ) and after 2–3 weeks by 1.2 pp ( $p<0.05$ ). However, as for the trend results, the effect can no longer be identified after 3–4 weeks ( $p=0.751$ ). If we extend the range of correct responses to be within  $\pm 2$  pp of the true value we find a similar pattern. In the first weeks after the intervention the treatment group is about 5 pp more likely to provide a correct response ( $p<0.05$ ), but the effect cannot be detected after 2–3 weeks ( $p=0.344$ ), or 3–4 weeks ( $p=0.213$ ).

In panel C we observe the effect of the intervention on respondents' ability to correctly state the relative burglary rate at the municipal level. Since the correct response depends on where respondents live, we split our results by whether respondents' 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 1–2 weeks those in the treatment group are 18 pp better at correctly identifying the relative burglary rate of their municipality ( $p<0.001$ ). After 2–3 weeks the difference is 10 pp ( $p<0.05$ ) and finally ends up at 4 pp ( $p=0.348$ ). A somewhat similar pattern, although



**Figure 3:** Dots represent percent of correct responses for treatment and control groups across time for each of the three dependent variables. Lines represent 95 percent confidence intervals. A, correct response to question about trend in burglaries (i.e., lower since 2011). B, correct response to question about level of burglaries (9% rate). C, correct response to question about relative level of burglaries in the respondent's home municipality; results split based on whether respondents live in municipality with an above, below or an average burglary rate. In Appendix C we reproduce this figure using knowledge about unemployment as a placebo test. N=4,895.

with smaller effects, can be identified for respondents who live in municipalities with an above average burglary rate: after 1–2 weeks it is 8 pp ( $p < 0.1$ ), after 2–3 weeks it is 0.4 pp ( $p = 0.935$ ), and after 3–4 weeks it is 2 pp ( $p = 0.698$ ). For those residing in a municipality with an average burglary rate, we do not find any consistent effects. For this limited subset, there seem to be an imbalance between the treatment and the control group prior to the intervention (6 pp,  $p < 0.01$ ).

## 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 attractive data visualizations. Using a field experiment, we showed that misperceptions were reduced, temporarily, among at least 15 percent of those who received this information. It is important to note this is an intent-to-treat effect, in that it is the effect of being mailed the leaflet rather than the effect of reading it. We asked respondents near the end of the post-treatment survey whether they had received a leaflet from the Trygffonden. 54 percent said that they had. If this number reflects that only half the respondents actually read the leaflet, then the effect of receiving the correct information among those who read the leaflet is closer to 30 pp.

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 way 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 Appendix A), this is surprising. One explanation might be that comparative assessments tend to carry more psychological weight (Olsen, 2017). No matter the explanation, this difference seems to suggest that some types of 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 first-hand observations, become top of mind, the effect of the correction wanes. Given what we know about opinion formation, this makes sense (Zaller, 1992). Accordingly, 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. As mentioned in the introduction, this is not what

the media is presently doing. This notion is supported by additional analyses presented in Appendix D, which reveal that among respondents 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 lasting among those who are not interested in local affairs. This seems to suggest that permanently reducing bias in citizens' perception of crime rates is largely a matter of adjusting the supply of (dis)information about crime.

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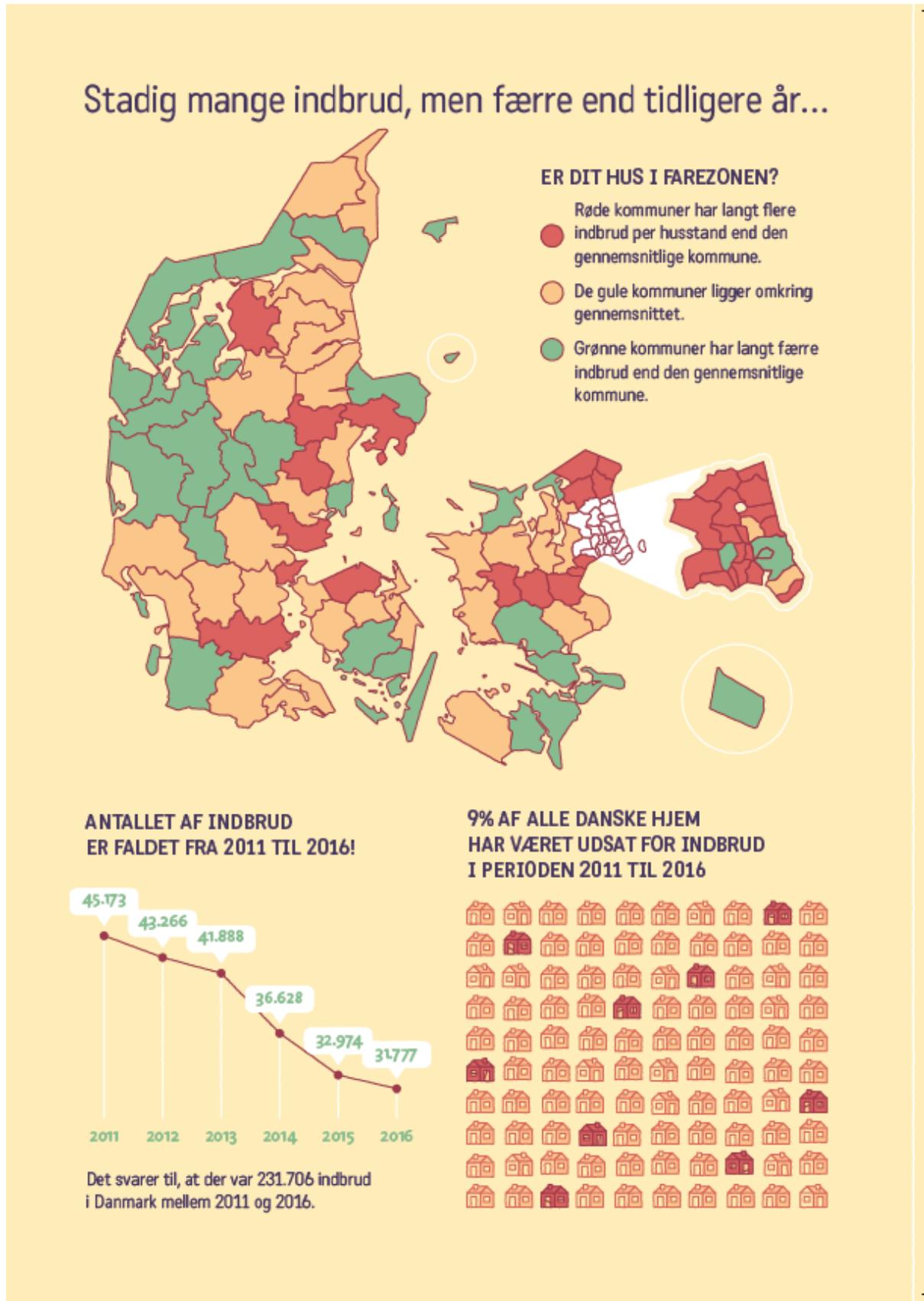
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## **A Details about the leaflets**

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 - a Danish foundation that is focused on helping Danes live productive, healthy, and safe lives. There was one placebo leaflet which was about letting the family dogs 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 policemen arresting a thief, which informs readers that the police are tasked with solving the crime, and that the police is 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 one 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, S-A, N-A, A-P, P-N. The first letter refers to the information package displayed on page two and four. The second letter refers to the information package displayed on page one and three.

Since we are only interested in the effect of the statistical information, we collapse respondents who got 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 got the information package combinations N-A, A-P, P-N plus those who got the placebo leaflet.

## B Descriptive statistics, balance, and attrition

**Table B1:** Descriptive statistics

Variable	Mean	SD
DV: Trend (% correct)	41.1	-
DV: Level	15	13.9
DV: Level exact (% correct)	0.5	-
DV: Level +/- 2 pp (% correct)	23.3	-
DV: Relative - above average (% correct)	34.4	-
DV: Relative - average (% correct)	54	-
DV: Relative - below average (% correct)	36.6	-
Females (%)	35.2	-
Age (years)	64.9	9
Fear of burglary (1–7)	2.8	1.7
Interest in local politics (1–4)	2.2	1.8

n=4,895

**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

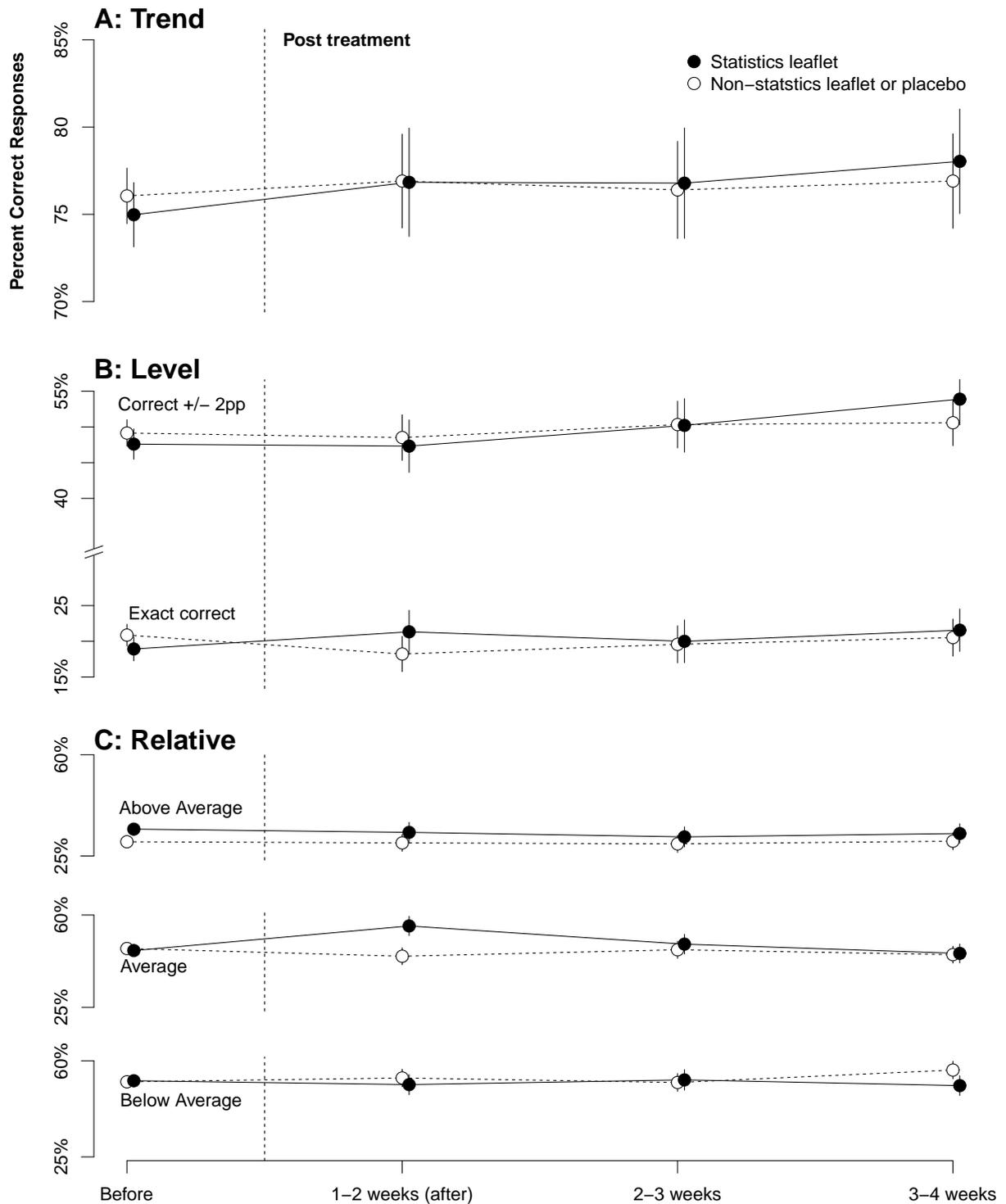
**Table B3:** Balance test across time

Variable	Weeks 1-2	Weeks 2-3	Weeks 3-4	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
Observation	1,652	1,579	1,664	-

n=4,895

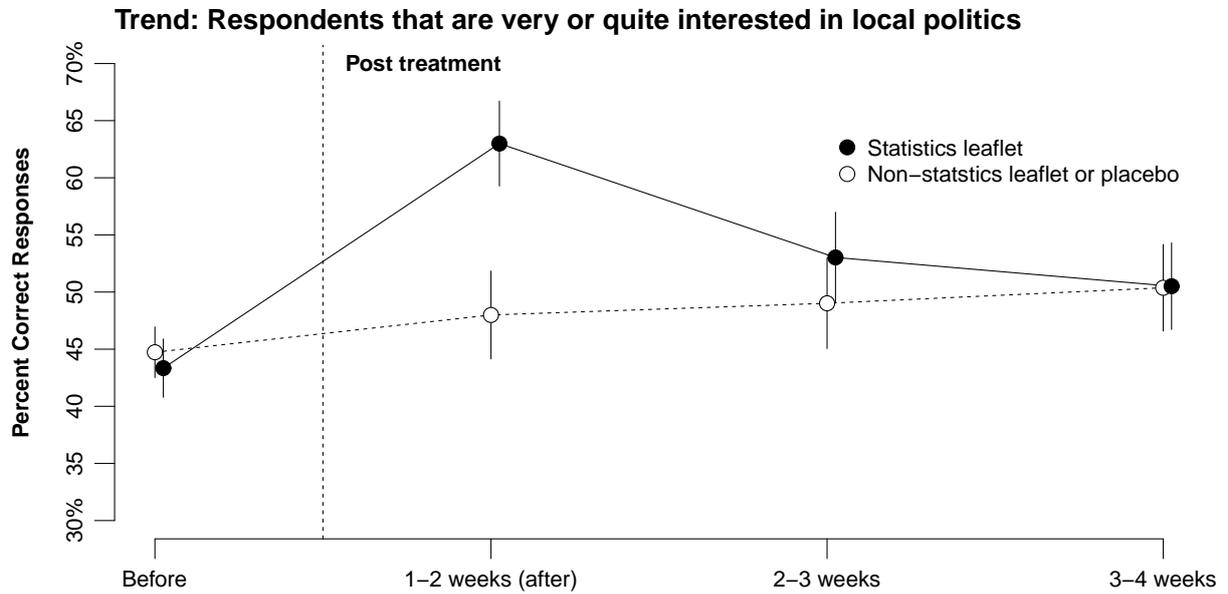
### **C Placebo outcome: Effect on unemployment**

For each outcome variable asking respondents about burglary prevalence we included the exact same 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 for 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 of the continuous period from year 2011 to year 2016 altogether. How many percent of the Danes were on average unemployed in the period altogether? (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).

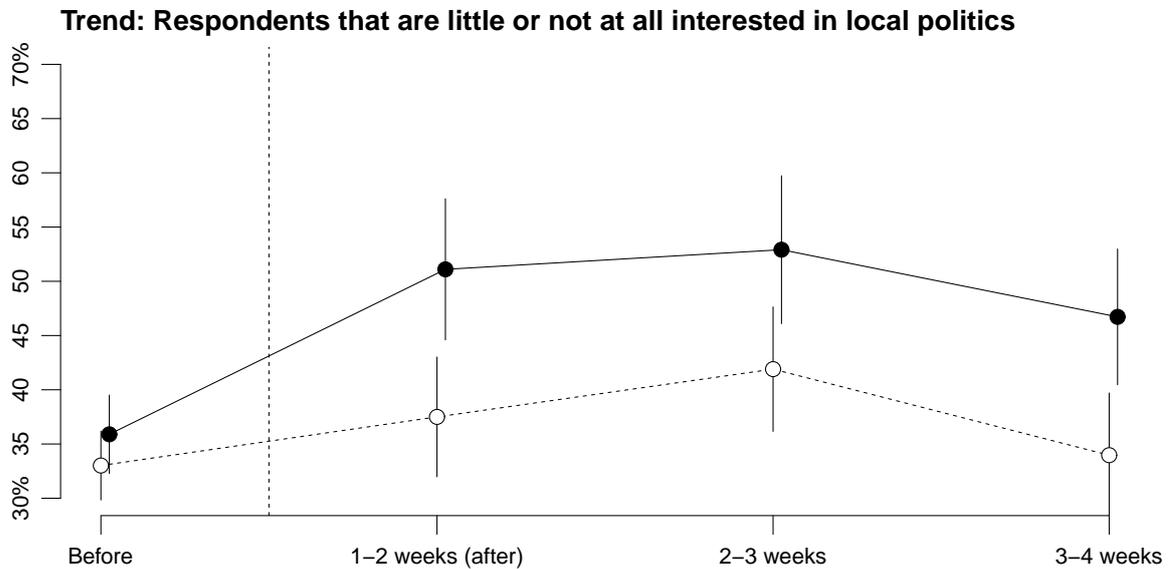


**Figure C1:** Dots represent percent of correct responses for treatment and control groups across time for each of the three outcome variables. Lines represent 95 percent confidence intervals. A, correct response to question about the trend in unemployment (i.e., lower since 2011). B, correct response to question about level of unemployment (5% rate). C, correct response to question about relative level of unemployment in the respondent's home municipality; results split based on whether respondents live in municipality with an above, below or an average burglary rate. N=4,895.

## D Treatment effects by interest in local affairs

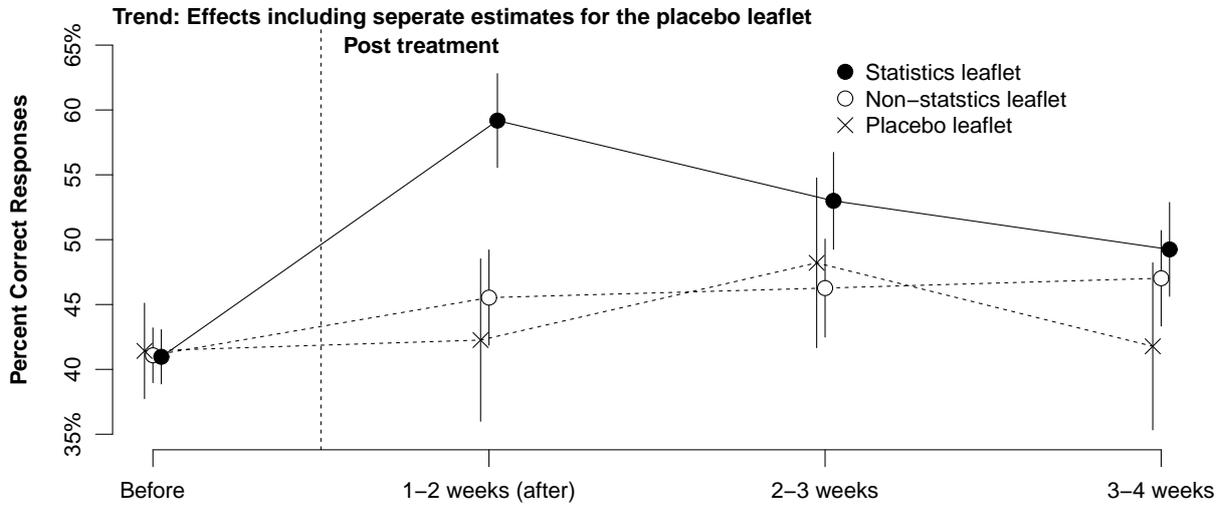


**Figure D1:** The main results for the correct response for the trend question for a subset with high interest in local politics. This includes respondents indicating that they are 'very interested in local politics' (n=1,032) or 'quite interested in local politics' (n=2,344). Total n=3,376.

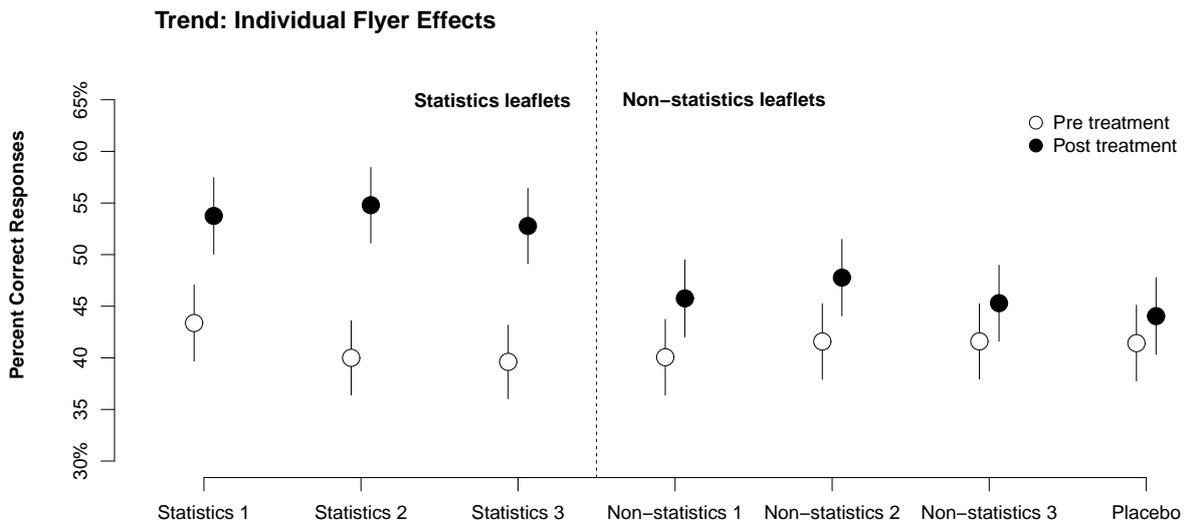


**Figure D2:** The main results for the correct response for the trend question for a subset with low interest in local politics: Respondents indicating 'a little interested in local politics' (n=1,261), 'not at all interested in local politics' (n=219), or 'don't know' (n=39). Total n=1,519.

### E Placebo and individual leaflet effects for trend outcome



**Figure E1:** Correct response for the trend question with separate estimates for the placebo group. N=4,895.



**Figure E2:** Average correct response for the trend question for each of the seven leaflets described in Appendix A. N=4,895.