No face time for Covid: Testing communication strategies to reduce face touching frequency

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ABSTRACT

As societies are learning to live with the pandemic, citizens have gradually re-entered public spaces and many have begun commuting to work or school again.Yet the challenge of fostering behaviour change among citizens, which is at the same time effective and legitimate, remains as pressing as ever. Without the urgency of the lockdown, how can governments communicate messages without encountering resistance or apathy? Governments still need to issue public guidelines, such as maintaining social distance, avoiding touching surfaces that many people touch, and avoiding touching one's face. However, these guidelines are hard to follow as they require a change in behaviours that are habitual and take place unconsciously. This paper aims to assess the effects of public awareness campaigns aimed at helping people to reduce the rate at which they touch their face. Using a large survey experiment of UK residents, we investigate the effectiveness of different messages and test hypotheses related to how different messengers and whether a message becomes politicised affect observance with the advice. We consider the effect on attitudinal outcomes and a novel behavioural outcome where we asked our respondents to record a video response to an unrelated question and count the frequency with which they touch their face.

As societies are learning to live with the pandemic, citizens have re-entered public spaces and many have begun commuting to work or school again. Governments continue to issue public guidelines, such as maintaining social distance, avoiding touching surfaces that many people touch, and avoiding touching one's face. However, these guidelines are hard to follow as they require a change in behaviours that are habitual and take place unconsciously. Citizens are sometimes confused about which activities are desirable or compulsory, where some follow guidelines and others do not (e.g. mask wearing on public transport). Research from the behavioral sciences has some general recommendations to offer policy-makers (e.g. BIT blog, 2020)[1] about how to influence citizen behaviour, but how to implement them is not straightforward. Most likely, public awareness campaigns will remain the primary avenue for promoting behaviour change. Yet we need ways to ensure citizens take notice and act accordingly.

Government-sponsored promotions of health messages are however not without problems. There is a large body of literature which suggests that messages can backfire and can have differing effects on groups, depending on how they view the government and other people supporting the message. In the context of the Covid-19 global pandemic, the danger of messages misfiring has been highlighted. For instance, former U.S. president Donald Trump was recorded publicly ridiculing scientific advice to avoid face-touching.[2]

In this paper, we aim to assess the effects of public awareness campaigns aimed at helping people to reduce the rate at which they touch their face. It investigates the effectiveness of these messages and tests hypotheses related to how different messengers and whether a message becomes politicised affect observance with the advice.

1 Theory

Unconscious behaviours

Many guidelines are hard to follow because they involve regulating largely unconscious behaviours. Despite a general willingness to comply with official guidelines, it is hard for people to do so. Take the recommendation to avoid touching one’s face: The behaviour can transmit virus from one’s hands to one’s eyes, nose, or mouth, which in turn can cause self-infection (Kwok et al. 2015). Unfortunately, face touching is habitual, and evidence shows that when

[1] https://www.bi.team/blogs/how-to-stop-touching-our-faces-in-the-wake-of-the-coronavirus/
[2] https://www.politico.com/news/2020/03/04/trump-jokes-touching-face-coronavirus-121135

people are made aware of touching their face, they do it more (Kwok et al. 2015; Lipinski & Nelson 1974).

In a recent review of more than 100 papers, Lunn et al. (2020) fail to identify any studies that consider strategies to mitigate face touching. To date there also do not exist any reviews on how to mitigate touching surfaces or keeping distance in public space. While some literature on interventions targeted at increasing safe and hygienic practices in the workplace exist (Sámano-Ríos et al. 2019; Rodrigues et al. 2020), no interventions specifically targeted at these behaviours have been tested in the context of a public health crisis or in public space. Since these interventions mostly consist of face-to-face conversations and individual training courses, it is questionable how much they can scale up.

A more appropriate approach could be derived from the currently recommended best-practice approaches to treat harmful tics such as repetitive hair pulling or skin picking (Habit Reversal Training). The training is typically administered individually, but online and remote approaches have been piloted successfully (Martino & Hedderly 2019). However, most robust evidence relates to programmes aimed at children (Edwards & Specht 2016). Albeit seeing parallels with problems addressed by existing interventions, it remains unclear which interventions are most effective in the context of a public health crisis.

Politicisation and messenger effects

Politicisation as defined for this study "means to turn something – an issue, an institution, a policy – that previously was not a subject to political action into something that now is subject to political action" (Palonen et al., 2019). Existing literature on politicisation is mainly situated in international relations (e.g. Hegemann 2018, Franchino and Mariotto 2020), European Union studies (e.g. Turnbull-Dugarte 2020, Bresanelli et al. 2020), and international development (e.g. Ferguson 1990, Lange 2008) whereby for the latter discussions more typically surround the depoliticisation of policy.

Recent research on partisan divides on the response to Covid-19, mainly originating from the U.S., suggest that political affiliation can be a strong predictor to what extent people comply with public health messages. Barrios and Hochberg (2020) look at how political affiliation proxied by county vote share and risk perceptions proxied by Google searches affect social distancing measured as visits to non-essential businesses. Kerr et al. (2020) find that liberals in the U.S. are more trusting in experts and more critical of the then Republican federal government response to the Covid-19 pandemic. Liberals engage in a greater number of health protective behaviours compared to conservatives. Such patterns appear to extend to

groups of people and even organisations: Benton, Cobb and Werner (2020) find that firms that are linked to politicians or political organisations differ in their disclosure of Covid-19-related risks. Those associated with the Democrats disclosed significantly more information than others.

In situations such as the pandemic where it is difficult to deny that a problem exists, mechanisms underlying partisan responses are most likely linked to differences in how people evaluate the credibility and fitness of government responses to the crisis. For instance, studying the global financial crisis Bisgaard (2015) finds when crises are severe enough that everyone agrees that there is a crisis, partisan patterns arise mainly in the form of blame attribution and evaluations of competence. Risk perceptions matter too. It is likely that when people perceive risks to be high that the effect of political affiliation is dampened (Malhotra and Popp, 2010).

Partisan identities

Most research on partisan identities has been conducted on the Democrat-Republican divide in the United States. It is questionable whether partisan identities in other contexts are strong enough for divergent responses to policy to emerge. Schonfeld and Winter-Levy (2019) show that voters in the UK differ significantly on views on redistribution depending on their stance on EU membership. Brexit-identities might be similarly polarising as in the United States. Hobolt et al. (2020) find that identities related to the EU referendum are distinct from party identities yet create as strong emotional animosities as traditional divides between supporters of the Labour and Conservative party. These identities are associated with motivated reasoning, whereby voters interpret evidence in a manner to fit with their ideological stances (Sorace and Hobolt 2020).

We hypothesise that partisan identities will only matter when messages are sufficiently politicised. A sufficiently politicised message moves from the realm of facts discussed as part of current debate and political debate to one where certain interpretations of the evidence are clearly associated with one side of the political spectrum. In the context of a health crisis that has reached all corners of the world, we expect that a scientifically-founded claim about how to stay safe will lose credibility when it is politicised. These effects are expected to be particularly deleterious when they are politicised in a partisan manner as the part of the electorate opposing the government (about 40% of the British electorate) are less likely to believe the message and are less likely to act upon it.

1.1 Hypotheses

We pre-registered the following relationships, which we tested in an experiment (OSF, 10.17605/OSF.IO/BC7MP, June 14, 2021.)

> H1: Any “Substitution message” will reduce the number of face touching compared to a Covid alert (placebo).
>
> H2: A scientist-endorsed message will lead to greater observance than the placebo message.

When a message becomes politicised, we expect that some respondents will perceive it to be less credible. Thus, we hypothesise that

> H3a: Any politicised message (partisan and bipartisan) will lead to different observance rates than the placebo messages. [observance (T6, T7, T2, T3) != observance (T1, T5)].
>
> H3b: A partisan message will be moderated by affiliation with the Conservative party (the party of the partisan treatment) such that observance is lowest among people with an opposing affiliation compared to the placebo message.

When we refer to “observance”, we refer to both the behavioural and the planned behavioural outcomes. Below, we define observance as a concept which captures to what extent a respondent follows the recommendations provided by the experimental messages and in the case of treatment posters, in response to the posters. We hypothesise that the pooled effect of receiving any communication targeted at reducing face touching will be efficient.

While scientists or experts are generally seen as messengers with high levels of credibility, we believe that expert scepticism and the fact that the role of the expert has become politicised as part of the global debate on Covid-19 could lead some respondents to comply less. On the other hand, the importance of science in the global pandemic has arguably bolstered the perceived importance of experts and scientists compared to a pre-crisis climate that might have reacted more negatively to experts (compared to expert scepticism cited as part of the EU referendum debate). We therefore believe that in this context, the message endorsed by scientists will lead to increased observance rates.

1.2 Exploratory hypotheses

We designed the experiment to be powered adequately to test the hypotheses listed above. Since our study is one of the few to investigate partisan messaging effects on the population of England, and potentially the first to investigate this in the context of Covid-19, we also explore relationships:

> H_exp_1: Any politicised message (partisan and bipartisan) will lead to lower observance rates than the scientific message.

Within the category of politicised messages, we expect a hierarchy. One-sided partisan messengers are expected to perform worse to those insinuating cross-party consensus.

> H_exp_2: A bipartisan message will lead to higher observance compared to the partisan message.

> H_exp_3: A bipartisan message will lead to different rates of observance than non-politicised messages.

We also predict that individuals' risk perception of Covid-19 will moderate their response to any treatment.

> H_exp_4: Initial perceptions of risk of catching Covid will moderate observance in response to treatments. Higher risk perceptions will be associated with a higher rate of observance across treatment arms.

Many countries have seen opposition and protests against different measures such as mask wearing, limitations on gatherings, and lockdowns. These protests have been associated with the authoritarian right (Dyer 2020; Kowalewski 2020). We therefore explore if:

> H_exp_5: Respondents who score higher on the right-left scale will have weaker responses to treatments.[3]

It is possible that those on one side of the cultural war will not respond to the scientific message resulting from suspicion of experts. But studies of anti-politics (Stoker and Hay, 2016) suggest

[3] In our pre-analysis plan, we wrote 'left-right authoritarianism'. However, the measure that made it into our survey was the conventional left-right scale.

that suspicion of politicians turns into more trust for experts shielded from partisan politics. It is politicians who are the target of distrust. It is possible that Brexit supporters might react positively either to the partisan and scientific treatments, but it is hard to predict the direction of that responsiveness.

> H_exp_6: Brexit supporters are expected to react similarly to Conservative voters to the partisan treatment.

2 Interventions and design

To test our hypotheses, we fielded a survey experiment with both a behavioural outcome and self-reported outcomes.[4] Based on a pilot study and power calculations, we recruited a sample of 4,000 adult subjects who were currently resident in England. For recruitment, we used the online platform Prolific and survey participation was incentivized. Subjects were pre-screened for their willingness to record video responses as part of the survey in order to maintain full transparency about our methods and reduce attrition at the outcome collection stage. We collected our data between May 3 and June 22, 2021.

Prior to fielding the survey, we piloted the treatment posters. A power analysis based on treatment estimates from these pilots showed that we had sufficient power to detect treatment effects for our first hypothesis of a similar size to those we saw in the pilot. For our other three hypotheses, including our interaction hypothesis, the experiment was powered at 80 per cent for effect sizes around 0.13 to 0.16 standard deviations, under the assumptions of the power calculations. We did not conduct power analyses on our exploratory hypotheses. For the full details on the power calculations, we refer to our pre-analysis plan.

In the survey, participants first saw a consent form explaining (i) the wider purpose of the survey experiment study and (ii) the participants' right to withdraw from the study and how to do so. A second page asked for consent to video record them as part of their study and explained how the video material would be handled in order to safekeep the anonymity of participants in the further processing of data. A third page asked them to consent to short term storage of their IP address for the purpose of linking survey responses to video responses.

After answering some questions on demographics, partisan affiliation, and risk perceptions concerning Covid-19, participants were randomly presented with one of eight treatments. The

[4] The survey experiment was pre-registered on the Open Science Framework (https://osf.io/fgupq). In the paper, we label when we deviated from the preanalysis plan.

treatments used a combination of text and images. The main treatments used posters that were designed with a public awareness campaign in mind. They both prompted respondents to occupy their hands in order not to touch their face. As a placebo control for the behavioral cue, we used a yellow background with the word “Covid-19” in black letters written on it. We used this picture as a placebo treatment as, equivalently to the other treatments, it primed awareness of the Covid-19 pandemic, but without providing any behavioural cues. A professional graphics designer created the treatment posters while we created the placebo poster ourselves. Subjects were assigned to one of the posters with equal probability.[5] Based on a pilot, we did not have strong prior expectations that one of the treatment posters would be more effective, and therefore we pooled them as one treatment for the main analyses.

Each picture was accompanied by a message on Covid-19, which constituted the second dimension. The informational framings consisted of:

(1) a placebo information treatment: a text and poster unrelated to Covid-19 and health messages.

(2) an expert information treatment: a text advising best practices related to Covid-19 as recommended by health experts.

(3) a partisan treatment: a text showing that the party in government supported the message that was being advertised.

(4) a bipartisan treatment: a text showing that the party in government and the opposition supported the message that was being advertised.

Subjects were randomly assigned to one of these four messages, each with a probability of 25%. Assignment to treatment along the picture and message dimension was orthogonal. As the treatments were orthogonal and we implemented a full factorial design, respondents were assigned to one of the treatment conditions with the probabilities that were the product of their marginal probabilities. Respondents were blocked on gender and age group (<35 and >=35). Within each block we used simple random assignment. In Table 1, we provide an overview of the potential treatment conditions.

[5] In the pre-analysis plan, we planned to assign subjects to the placebo poster with 50% probability and to each of the two treatment posters with 25% probability. However, due to an implementation error, they were assigned with equal probability instead.

Table 1: Treatment conditions

Treatment arm	Poster	Message	P(Assignment)
1		The Covid-19 pandemic hit England in Spring 2020. Since then it has had a severe impact on people's lives. (Placebo)	1/12
2		Boris Johnson and the Tories agree that people should avoid touching their eyes, nose and mouth to help stem the spread of Covid-19. (Partisan)	1/12
3		Opposition parties and the Tories agree that people should avoid touching their eyes, nose and mouth to help stem the spread of Covid-19. (Bipartisan)	1/12
4		Scientists agree that people should avoid touching their eyes, nose and mouth to help stem the spread of Covid-19. (Scientist-endorsed)	1/12
5a		The Covid-19 pandemic hit England in Spring 2020. Since then it has has had a severe impact on people's lives. (Placebo)	1/12
5b			1/12
6a		Boris Johnson and the Tories agree that people should avoid touching their eyes, nose and mouth to help stem the spread of Covid-19. (Partisan)	1/12
6b			1/12
7a		Opposition parties and the Tories agree that people should avoid touching their eyes, nose and mouth to help stem the spread of Covid-19. (Bipartisan)	1/12
7b			1/12
8a		Scientists agree that people should avoid touching their eyes, nose and mouth to help stem the spread of Covid-19. (Scientist-endorsed)	1/12
8b			1/12

After the treatment, we measure three types of outcomes (i) behavioural, (ii) behavioural intentions, and (iii) perception of the message. Subjects were first prompted to look at the message and picture. Participants were then asked to take two minutes to talk about their experiences during the Covid-19 pandemic and how they felt about re-entering busy public spaces while the message was still on screen. The behavioural outcome captured how frequently respondents touched their face for the duration of a video response or the first 30

seconds of the video. Behavioural intentions recorded to what extent respondents reported willingness to comply with the message. Attitudes pertained to how credible people found the message presented to them. After the video message, respondents answered some additional questions on *(i)* their trust in the shown message, *(ii)* their intention to follow suit on recommended actions, *(iii)* their attitudes towards the UK government's response to Covid-19. We will use the behavioural outcome, respondents' trust in the message, and their intention to follow suit as outcome variables.

2.1 Processing and anonymisation of video material

We asked participants to talk for two minutes. Participants could not progress to the next page until they had spent 30 seconds on the screen where the video was recorded. We enforced this time constraint to record enough video material to reliably assess the number of face touches and the effects of the treatments on it. We counted submissions as valid if participants did not talk for the entirety of the two minutes. If participants' internet connection reset and they had to re-record a video, we merged the multiple video submissions and counted them as one submission.

We had three research assistants manually code video responses. They were all trained to apply a coding scheme where each uninterrupted sequence of face touching was counted as one incident. An uninterrupted sequence would e.g. be to rest one's head in one's hand or to touch one's cheek before moving the hand directly to one's eye. An interrupted sequence would e.g. be if one first touched one's cheek, moved one's hand away from one's facial region, and then moved it back to touch one's eye. The coders were blind to treatment conditions.

In the appendix, we show correlations between coders for a subset of videos coded by more than one coder and for one coder who coded a subset of the videos twice. We show a total of eight correlations, which are all between 0.74 and 0.91. We also show the means for face touching frequency in the first 30 seconds and in the full video across coders. One coder consistently counts more face touches than the two other coders.

3 Analytical strategy

To evaluate our hypotheses we follow the estimation strategy of our pre-analysis plan.

3.1 Outcomes

We use three different outcomes: *(i)* behavioural observance, *(ii)* planned observance and *(iii)* message credibility. For our scale of observance, we combine these three measures into a scale using confirmatory Principal Component Analysis (PCA) on only those observations with non-missing values on all three measures. For observations with missing values on one out of the three measures, we then impute mean values from other observations and estimate their value on the principal component. Behavioural observance is defined as the rate of face touches per 60 seconds observed by the coders in the video. As we were concerned that video duration might be affected by the treatment, we also pre-registered that we would, as a robustness check, estimate the effect of face touching frequency in the first 30 seconds of the video, the count of face touches, and the video duration.[6]

3.2 Estimation

To estimate the treatment effects, we rely on linear regression with heteroskedasticity robust standard errors (HC2) as linear regression provides unbiased treatment estimates (Gerber & Green 2012). Furthermore, for the average treatment effects, we also find randomization inference based p-values, because some of our outcomes have skewed distributions. To increase precision of our treatment estimates, we control for age category, gender, educational category, an indicator for conservative identification[7], an indicator for having had Covid or being convinced that one has had Covid, an indicator for having had at least one vaccine shot, and one's individual perceived risk of getting Covid.[8]

4 Results

Before turning our attention to the main results, we present the characteristics of our sample. Overall, we have a fairly young sample (53 per cent 35 or younger across treatment conditions) of mostly women (64 per cent). Forty-one percent have a two-year degree or more, but 63 per cent report an income between £20,000 and £29,999, which is the median category in our income distribution. Thirty-three percent have feelings on the middle of the scale or warmer

[6] Less than one percent of the videos were shorter than 30 seconds. These videos are not included in the analysis for frequency in the first 30 seconds.
[7] We asked respondents how strongly they identified with the Conservative party on a scale from 0 to 100 and coded everyone scoring the party at 50 or above as 'Conservative Identity'.
[8] In the pre-analysis plan, we also wrote that we would control for occupation. However, a measure of occupation was left out of the final survey.

towards the Conservative party and 25 percent are pro-Brexit. On a left-right scale where 0 is most left-wing and 1 is most right-wing, the participants have an average score of 0.37.[9] Twenty-four percent have tested positive for Covid-19 or are convinced that they had Covid-19 at some point and 53 per cent have had at least one dose of any Covid-19 vaccine at the time they took the survey. We also asked respondents about their perceived risk of being exposed to Covid-19, contracting Covid-19, and infecting others. Each of these outcomes were measured between '0 Extremely unlikely' and '100 Extremely likely', and we added the measures in a joint index, which we rescaled to a 0-1 scale. Our respondents scored an average of 0.34 on this scale.[10] In Figure 1, we show mean and confidence intervals for background covariates by each condition of our two treatment factors.

In the survey, we asked people to provide a video recorded answer of minutes duration and required them to stay on the video recording page for at least 30 seconds. Less than 0.2 percent recorded a video of less than 30 seconds and average video length was 108 second with a median of 119 seconds. Thirty-one percent recorded a video of exactly 120 seconds, which was the modal response length, followed by 27 percent who recorded 119 seconds. In Figure 2, we show the distributions of response duration over the treatments in the two treatment factors.[11]

4.1 Effect of any poster

We are now ready to turn to our main results. Our first hypothesis was that seeing any poster would reduce face touching frequency. In Figure 3, we show the result of seeing any treatment poster compared to seeing the control poster. All posters were accompanied by a treatment message and we implemented a full factorial design. In Figure 3, we have averaged over the treatment messages and show only the marginal effect of receiving a poster.
length was unaffected by treatment assignment, we report the alternative measures in the appendix and we find no effect on either of them.

[9] In the survey, the left-right scale ranged from 0 to 10, but we rescaled it to be on a comparable scale.
[10] FILL IN DETAILS
[11] The x-axis is truncated below 30 and above 150 seconds, which corresponds to the 1.5 percent of the videos. Anova-tests for the duration by treatment message and duration by treatment poster finds no statistically significant variations between treatments.

Figure 1: Means and confidence intervals for background covariates over treatment message (first panel) and treatment poster (second panel).

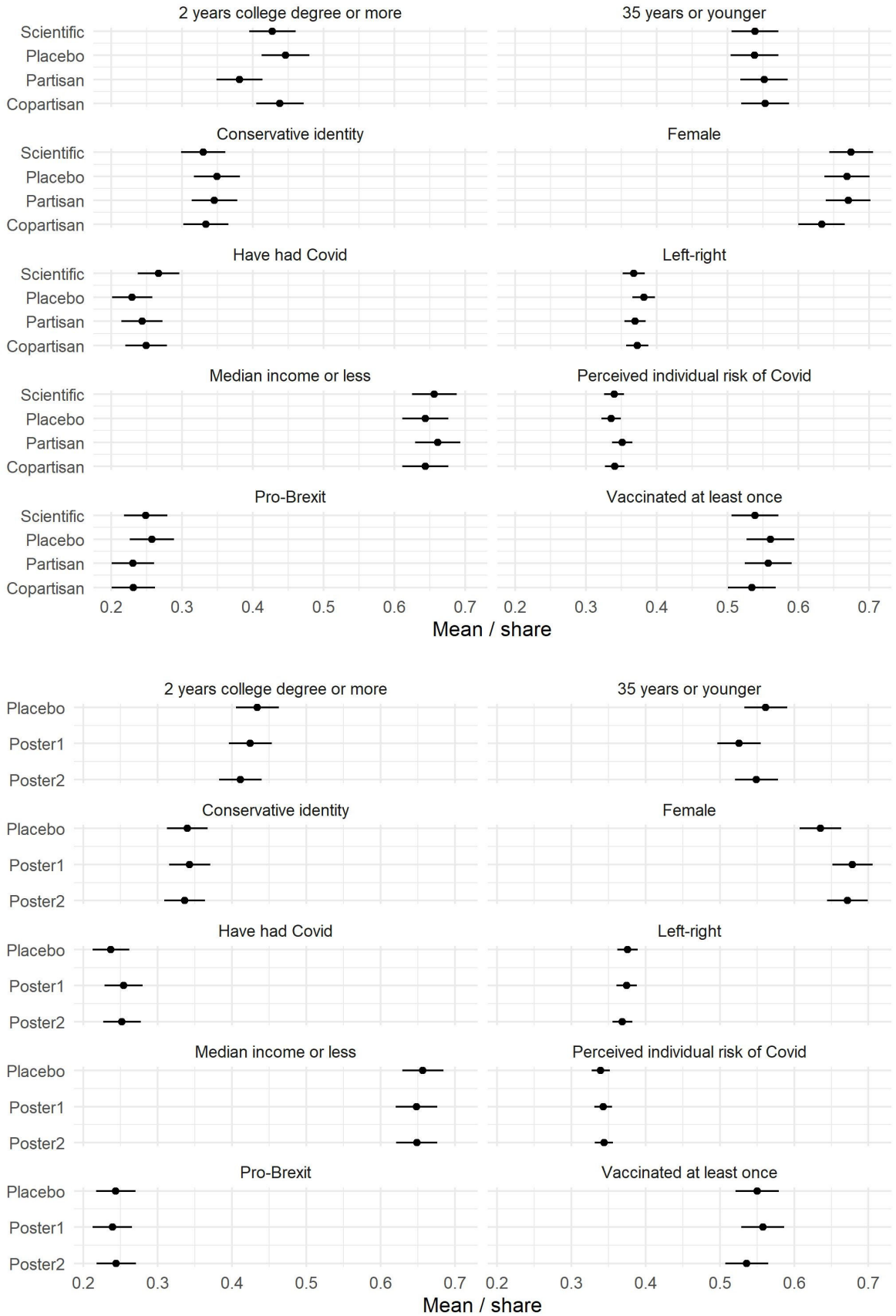
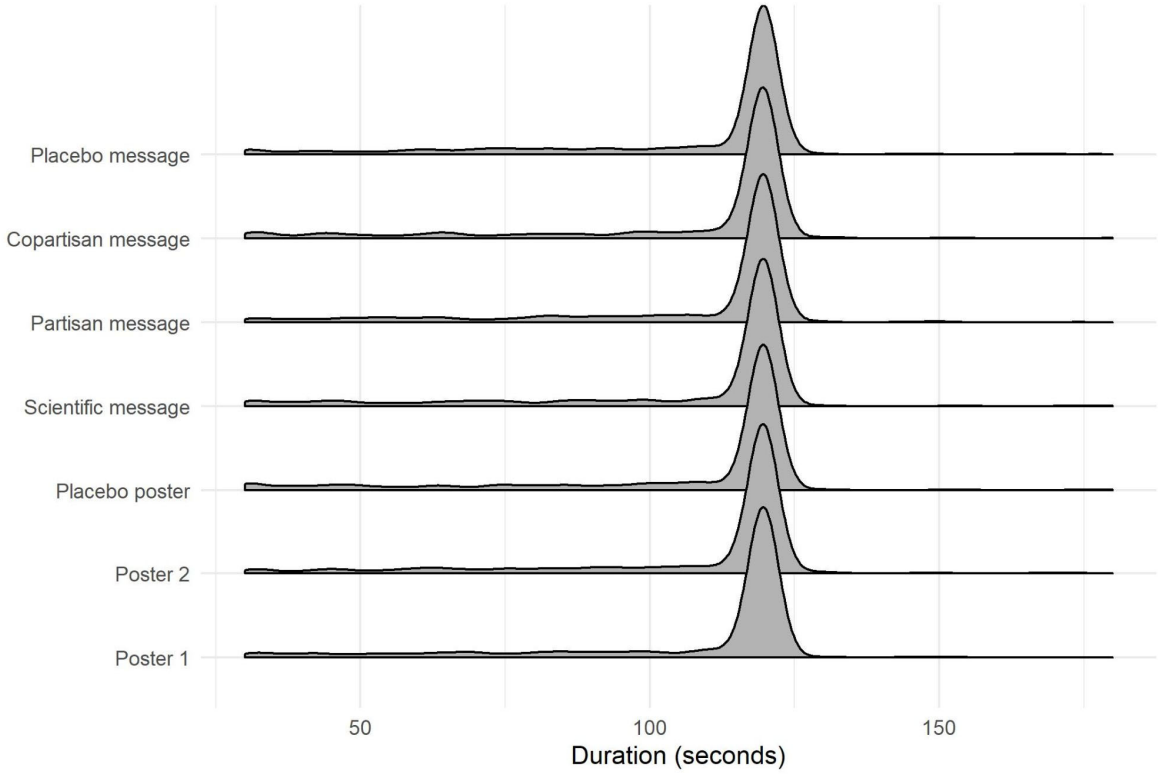


Figure 2: Video duration by treatment condition



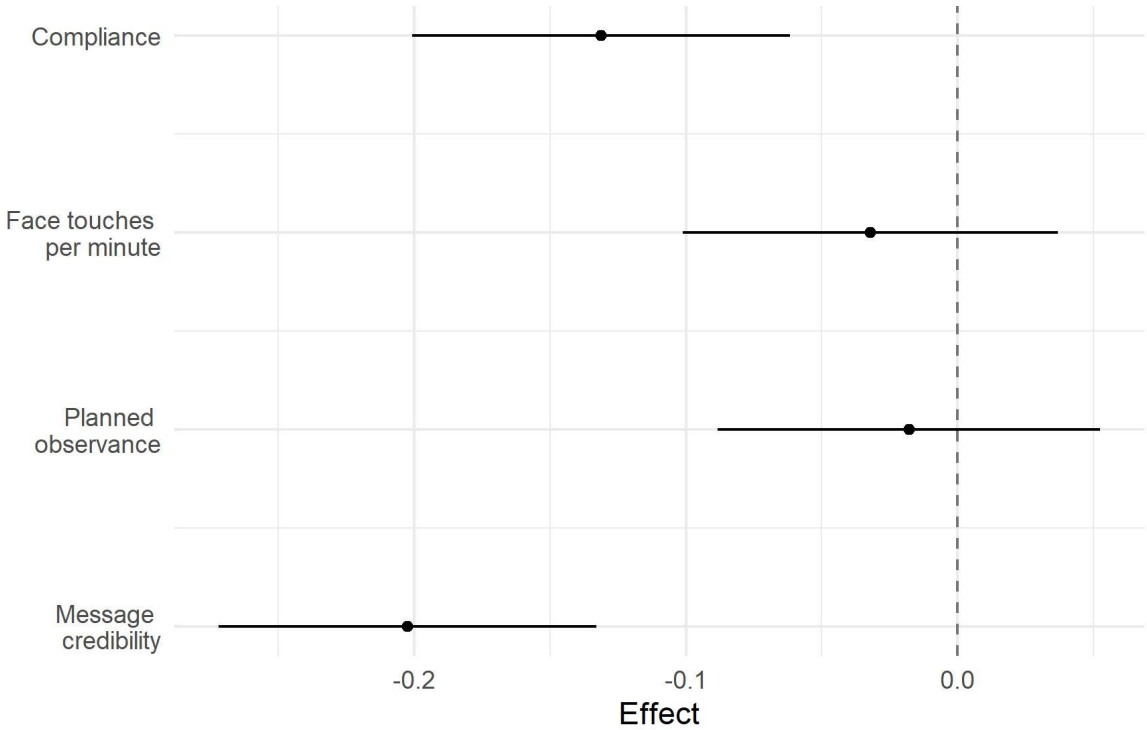
Our pre-registered hypothesis is that the treatment posters would reduce the face touching frequency (measured by face touches per minute). The point estimate is in the right direction and corresponds to 0.03 fewer touches per minute with a 95% confidence interval of [-.10; 0.04] from a baseline of 0.69. As we were concerned that the treatments could also affect video duration, we pre-registered two robustness measures: the number of face touches in *1)* the first 30 seconds and *2)* the full video. As our results in Figure 2 indicate that the video

In Figure 3, we also show the effect on planned observance, message credibility, and our compliance measure, which combines face touching frequency, planned observance, and message credibility. All of these measures are scaled to have a mean of zero and a standard deviation of one. We did not pre-register any effects of the treatment poster on any of these outcomes, but we see that compliance is -0.13 standard deviations lower in the treatment group whereas planned observance is unaffected. Since face-touching frequency correlates negatively with compliance, it means that the negative relationship is driven by negative effects on message credibility. This is confirmed by the results, as message credibility is 0.2 standard deviations lower among those who were exposed to a poster.

In Figure 3, we only test the effect of any treatment poster compared to the control poster. In the appendix, we compare the two treatment posters to each other. This comparison reveals an interesting pattern. While there are no differences between the two posters in their effect

on face touching or planned observance, the credibility of poster 2 is rated much lower than the credibility of poster 1. In fact the confidence interval on the estimate on both message credibility and compliance on poster 1 includes zero, while it does not on poster 2. The difference in treatment effect between the posters on these two outcomes is also statistically significant. We did not pre-register any difference between the two posters, but we can conclude that the respondents in our data rated the second poster less credible, but they did not differ on observed or planned behavior.

Figure 3: Any treatment poster compared to the control poster

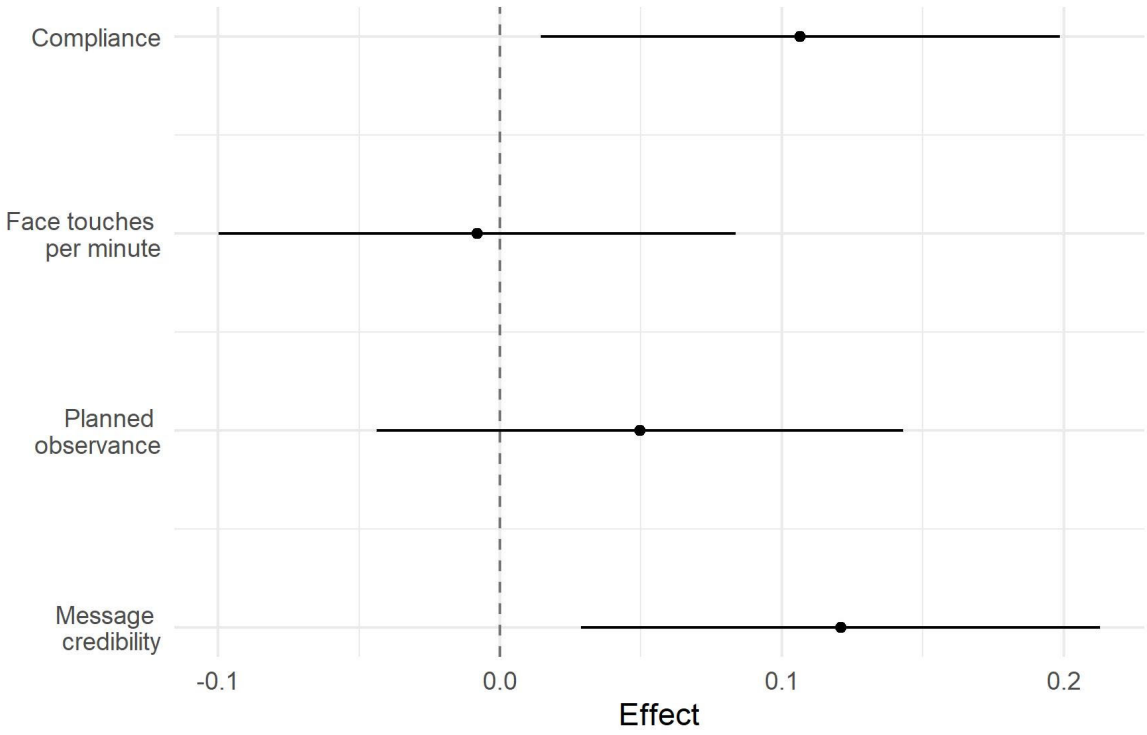


4.2 Messenger effects

For our second hypothesis, we expected that the planned compliance would be higher among those exposed to a message endorsed by a scientist than among those exposed to a placebo message. In Figure 4, we present the effect of the scientific message compared to the control message. We average over the treatment posters for both conditions. Our results show that compliance was indeed 0.11 standard deviations higher among those assigned to the

treatment with a 95% confidence interval of [0.01; 0.20].[12] We did not have any expectation that the scientifically endorsed message would affect face touching frequency and the results in Figure 1 do not point in this direction either. When looking at the other measures that constitute our compliance measure, we can see that the point estimates are positive for both of them, but only the confidence interval for message credibility excludes zero.

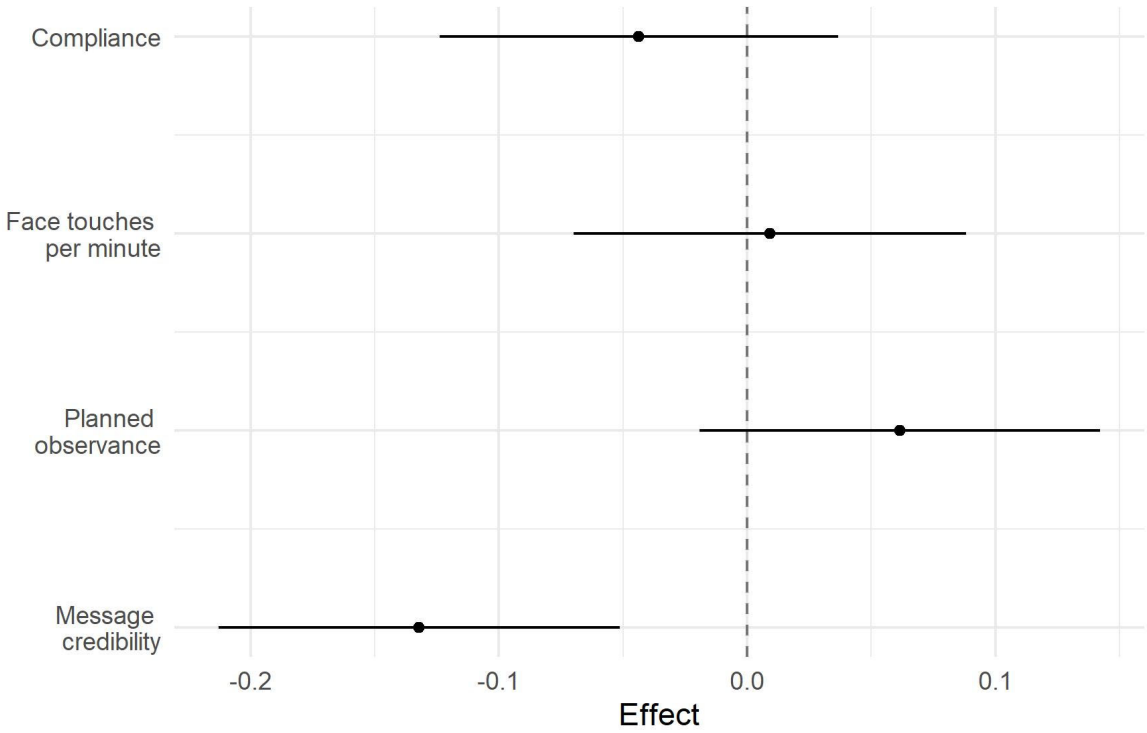
Figure 4: Scientifically endorsed message compared to placebo message



Next, we move to present our results for the third, pre-registered hypothesis where we expected compliance to be negatively affected by assignment to one of politicised messages compared to the placebo messages. In Figure 5, we plot the difference between those assigned to one of the politicised messages and those assigned to the placebo message. We average over the different posters. As is evident from the figure, there is no indication that the politicised message worked any differently than the placebo message for compliance. The point estimate is -0.04 with a 95% confidence interval of [-0.12; 0.04]. We did not pre-register effects on face-touching frequency, planned observance, or message credibility, but we can see that message credibility in our data is rated lower among those assigned to the politicised message compared to the control.

[12] We adjust the the p-values for our four main hypotheses using the Benjamini-Hochberg procedure (Benjamini & Hochberg 1995). The adjusted two-sided p-value for the effect of the scientific treatment on compliance is 0.088.

Figure 5: Politicised message compared to placebo message



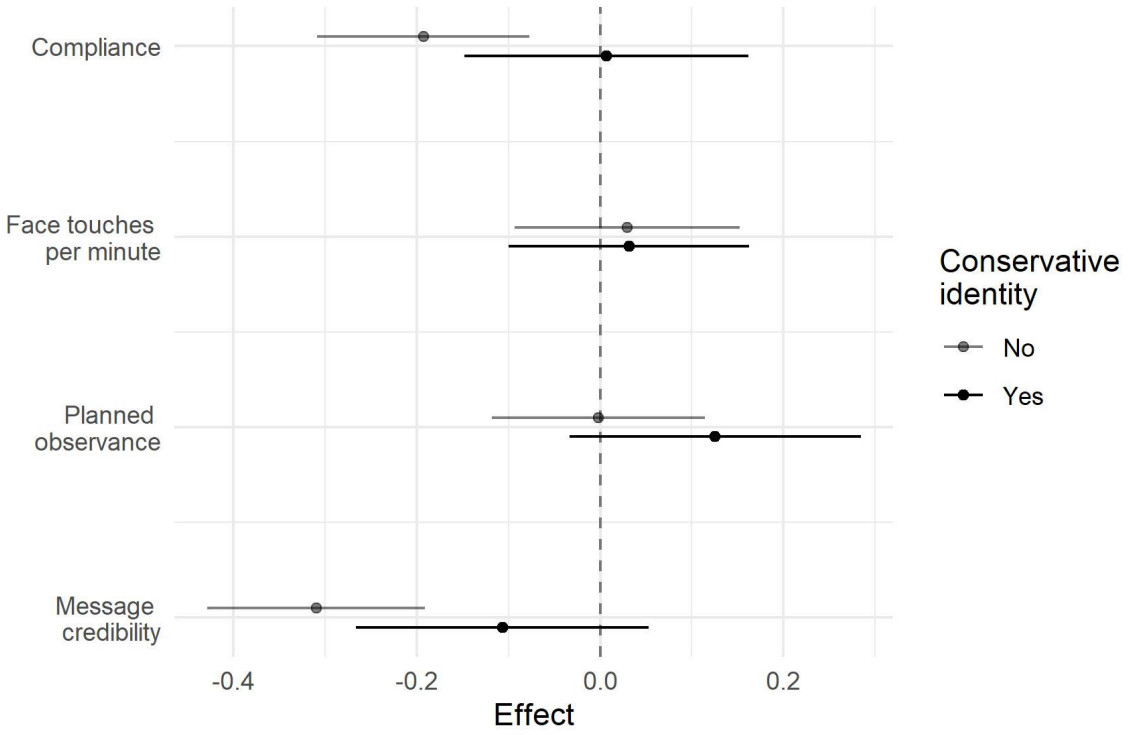
Our final pre-registered, main hypothesis was that a partisan message had a different effect among those who identify with the Conservative party. In Figure 6, we compare the effect of receiving the one-sided partisan message, where Prime Minister Boris Johnson endorsed the message, to the placebo message. We interact assignment to treatment by identifying with the Conservative party. Following our pre-analysis plan, we code as identifiers anyone who scores 50 or above on a thermometer on how strongly someone identifies with the Conservative Party. The scale ranges from '0 = Not at all' to '100 = A lot'. In Figure 6, we average the effect of the message over the posters, and we present marginal effects of the treatment by groups.[13]

The results in Figure 6 are in the expected direction as compliance is virtually unaffected among those who identify with the Conservative, while there is a considerable, negative effect among those who do not identify with the Conservative party. The effect for Conservative identifiers is 0.01 standard deviations with a 95% confidence interval of [-0.18;0.16] and for those who do not identify with the Conservatives it is -0.19 with a 95% confidence interval of [-0.31; -0.08]. The p-value on the interaction term is 0.088 after correcting for multiple hypotheses using the Benjamini-Hochberg correction, so the interaction is not statistically

[13]We use the margins-package for R (Leeper et al. 2017).

significant. Although we did not pre-register any hypotheses regarding what sub-measures were affected, we can see that our results are primarily driven by a differential effect on message credibility and a small difference in planned observance, whereas the rate of face touches per minute is the same in the two groups.

Figure 6: Partisan message compared to placebo message by identification with the Conservative Party



4.3 Robustness checks and alternative specifications

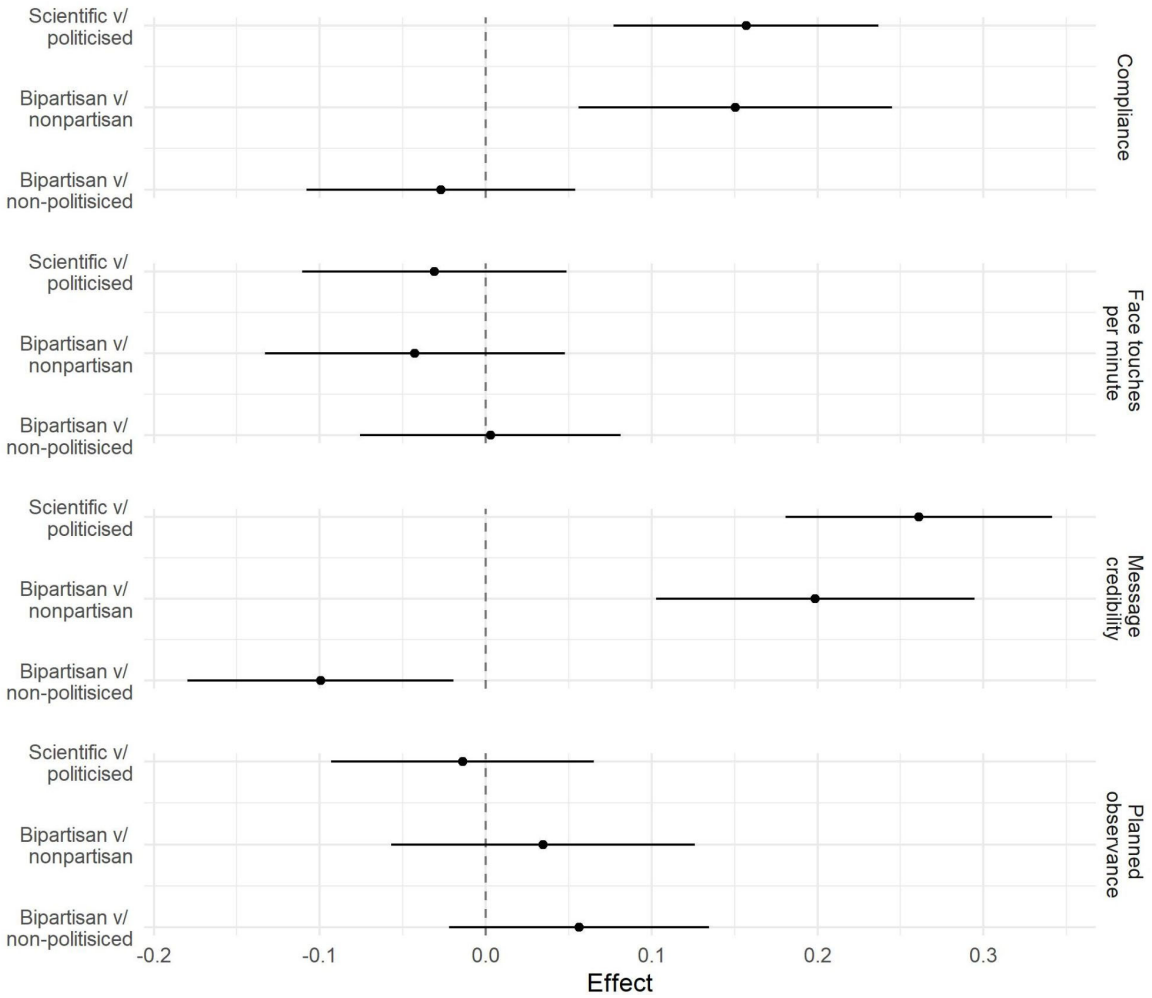
In the appendix, we use randomization inference as an alternative way to find the p-values for our main results. We arrive at similar p-values when we do so. We also present the interaction from Figure 6 in two alternative ways; one where we split the conservative identity scale by its median, and one where we interact treatment with the full scale. The results of these interactions are substantially equivalent to those we present in Figure 6.

4.4 Additional results

In our pre-analysis plan, we also listed a number of exploratory hypotheses, to which we now turn. First, we will explore if any politicised message leads to lower compliance than the scientifically endorsed message; if a bipartisan endorsed message leads to higher compliance than a one-sided partisan message; and if a bipartisan message leads to a different result than a non-politicised message.

In Figure 7, we explore these hypotheses simultaneously. We compare the scientific message to any politicised message, the bipartisan message to the one-sided partisan message, and the bi-partisan message to any non-politicised message, i.e. both the placebo and the scientific message. All messenger effects are averaged over the accompanying posters.

Figure 7: Additional messenger effects



In the first panel, we see that compliance is higher for the scientific compared to the politicised message (the point estimate is 0.16 standard deviations with a 95% CI of [0.08; 0.24] and for the bipartisan message compared to the one-sided bipartisan message (0.15 standard deviations, 95% CI [0.06;0.25], while the effect is insignificant for the bipartisan message compared to the non-politicised message. From panel 3, we see that differences in message credibility are driving the overall results.

4.5 Effect heterogeneity for posters

Next, we consider how the treatment posters interacted with one's left-right position, one's risk perception of Covid, and one's Brexit attitude. In Figure 8, we present marginal effects from the poster over Covid risk (panel 1) and left-right position (panel 2).[14]

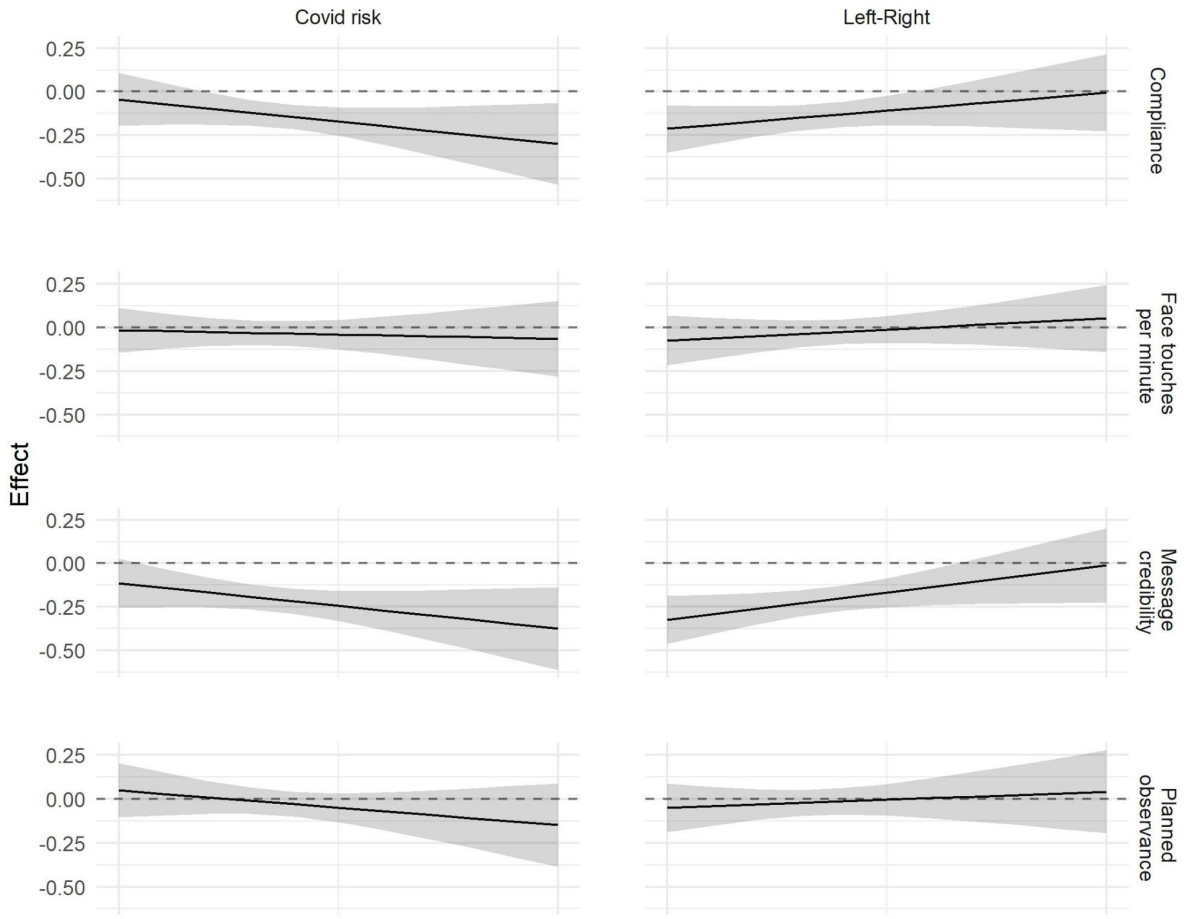
When considering panel 1, it is somewhat surprising to see that the poster had the strongest negative effect on compliance among those who had the highest risk perception of the poster (95% CI on the interaction [-0.61; 0.10]). We had expected the effect to be strongest in the group who perceived Covid to be the highest risk. However, this result is also driven by an effect on message credibility, where the message is rated most negatively among those who feel most at risk (95% CI on the interaction [-0.61; 0.09]). Furthermore, it is important to point out that neither the interaction effect for compliance nor the interaction effect for message credibility is statistically significant.

In the second panel, we see a positive interaction where the effect on compliance is negative among those who are most left wing whereas it is insignificant for the most rightwing (95% CI on the interaction [-0.12; 0.53]). This interaction is in the direction that we had expected, too. Looking at the measure that constitutes compliance, it is again an interaction on message credibility that drives the results. According to the interaction model, the treatment posters are rated negative for those who are most left oriented whereas it is rated equally credible to the placebo poster among the most right oriented (95% CI on the interaction [-0.01; 0.63]).

Overall, there is suggestive evidence that compliance interacts with both covid risk perception and left-right orientation, but that this is almost exclusively driven by an interaction effect on message credibility. Neither our behavioural measure of actual face touching frequency nor planned observance seems to be more or less affected in those who deem the message more or less credible.

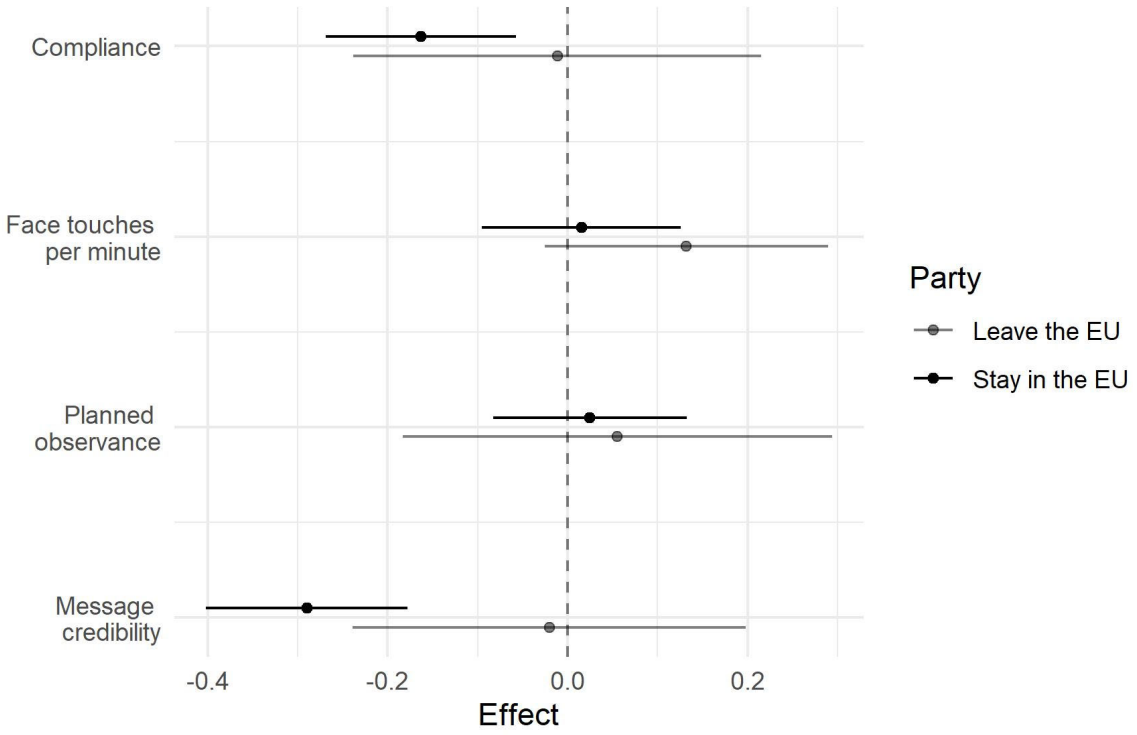
[14] In the pre-analysis plan we wrote that we would interact with right-wing authoritarianism, but this measure was dropped from our final survey.

Figure 8: Marginal effects of the treatment poster over risk perception of Covid and left-right orientation.



As a last result, we wanted to explore if Brexit attitudes shaped the response to the partisan treatment in a fashion similar to the interaction with those who identify with the Conservative party. In Figure 9, we compare the effect of exposure to the partisan treatment with an indicator for whether respondents would vote to leave or stay in the EU in a new referendum. As the figure shows, this does not meaningfully structure how our respondents react. Compared to the interaction for the partisan message with Conservative identification, which tended to be positive in Figure 6, the partisan treatment does not seem to interact differently with Brexit support in any meaningful way.

Figure 9: The effect of any poster by Brexit attitude



5 Discussion and Conclusion

Previous research shows that face touching happens frequently and unconsciously and while it is recognized to be a vector for contagious diseases, little is known on how to reduce such behaviour (Sámano-Ríos et al. 2019; Rodrigues et al. 2020; Lunn et al. 2020). In this paper, we have presented the results from an online experiment with both a behavioural and survey based outcomes to look at the effects of two posters and three messages compared to control conditions. We explored the effect of these communications along three dimensions: behavioural observance, planned observance, and message credibility.

Because face touching is unconscious behaviour, telling people to avoid touching their face might backfire. Therefore, we developed two treatment posters that tried to prime substitution behaviours to keep hands occupied. Our first main hypothesis was that such treatments would reduce the rate of face touching. However, we found no effect. Those exposed to one of the treatment posters did not have a substantially lower rate of touching their face during a video response that they recorded in response to their general experience of the lock down, their feelings towards society reopening, and the prospect of returning to public spaces with other people. When we explore the effect among them on the other outcomes, we do find that they

rated the message less credible when they saw a treatment poster, but that it did not affect their planned observance to general guidelines.

Our remaining hypotheses regarded the message and messengers that accompanied the posters. First, we compared the effect of a message that people should avoid touching their eyes, nose, and mouth, which respondents were informed that scientists agreed on to a placebo message where respondents were just told that the pandemic had had a significant impact on society without any message sponsorship. This message had an effect on a joint measure of compliance for our three sub-measures of 0.11 standard deviations with a 95% confidence interval of [0.01; 0.20]. However, the effect on the compliance measure seems entirely driven by the message being perceived as more credible with no meaningful difference in behavioural or planned observance among those who saw the placebo message and the message endorsed by scientists.

We also compared the placebo message to a politicised message where respondents were either told that the treatment message had support from the Prime Minister, Boris Johnson, and his party or from both the Opposition and the Government. A message politicised in such ways had no effect on the overall level of compliance, but respondents did rate it lower in credibility than the neutral message with no endorsement. For our last main hypothesis, we compared the response to the purely partisan treatment to the placebo treatment for those who identified with the government and those who did not. Here we found virtually no effect on any outcome among those who identified with the Conservatives and a negative effect on compliance among those who did not. This negative effect was driven by the message being perceived as less credible.

Overall, we found no effects on our behavioural outcome, but some effects on our joint measure of compliance; effects mostly driven by effects on the message credibility. One interesting result in this regard is that there is no clear trade-off between message credibility and a behavioural response. Treatments with lower credibility do not cause worse behavioural responses. Nor do treatments with higher probability cause stronger behavioural responses.

In the supporting information, we break down the effects by each of the two treatment posters and find indications that one poster might work better than the other. Here, it is the poster, which is arguably more clear in its instructions, that has a larger negative point estimate for its effect. Interestingly, the message of the poster with the numerically larger point estimates is rated way less credible. This relationship comports with the general direction finding among the main hypotheses that there is no backlash of unpopular treatments on behavioural

outcomes. In fact, the more unpopular treatments are just as effective or more effective in reducing target behaviours. However, we explicitly did not pre-register any effect of one poster being more effective than the other, and the difference between the posters are not statistically significant, so we take these results as merely promising for further inquiries.

Why might messages with lower credibility be as ineffective if not more effective than messages with higher credibility? One suggestion is that the messages with lower credibility attract more attention from the respondents. Respondents might not like what they see, but they might still internalize it to a larger extent. That is at least in the short run. During the pandemic, public authorities have struggled with how to best communicate with the public and one key lesson seems to be that communication should be trust based (Petersen 2021). It is important for authorities to trust the public and to have continuous trust in their communication from the public. Accordingly, if messages with lower credibility are implemented on a large scale, the long term effects might be that they backfire.

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Appendix for

"No face time for Covid: Testing communication strategies to reduce face touching frequency"

A1 Randomization inference based p-values

In the pre-analysis plan, we specified that we would use randomization inference to find p-values for some of our main outcomes, because some of our outcomes potentially would have very skewed distributions. In Table A1, we present p-values from randomization inference for each of our main hypotheses with the same outcomes as we primarily present in the paper. For each hypothesis and outcome, the entries are two-sided p-values from 10,000 simulations. We use the ri2-package to test the first three hypotheses (Coppock 2020). We present p-values without adjusting for multiple comparisons and compare them to their counterparts in the paper, since our purpose is to investigate if the estimation method matters.

Table A1: P-values of main hypotheses from randomization inference

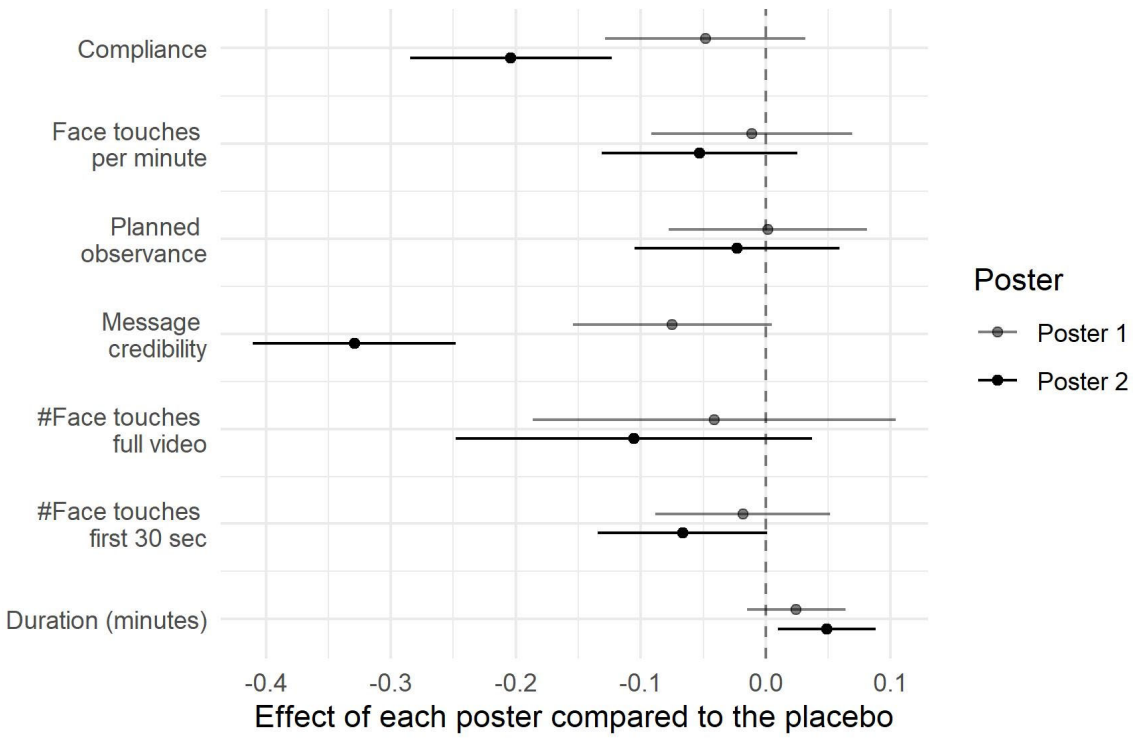
Outcome	Treatment poster vs control (H1)	Scientific message vs placebo (H2)	Politicised messages vs placebo (H3a)	Partisan treatment vs placebo interacted with conservative identifier (H3b)
Compliance	<0.001	0.023	0.277	0.046
Face touches per minute	0.412	0.864	0.774	0.776
Planned observance	0.611	0.306	0.136	0.202
Message credibility	<0.001	0.009	0.001	0.051

For the first hypothesis, we preregistered an effect on face touches per minute. For the result presented in the paper, the p-value was 0.361 without correcting for multiple comparisons, while it is 0.412 when we use randomization inference. For the other three hypotheses, we preregistered an effect on compliance. The p-values from the results reported in the paper are, without adjusting for multiple comparisons, 0.023, 0.286, and 0.044, respectively. When we use randomization inference, we get p-values of 0.023, 0.277, and 0.046, respectively. For all of our pre-registered hypotheses, we arrive at the same conclusion, when we use randomization inference.

A2 Comparing the effect of the individual treatment posters

In Figure A1 we compare each treatment poster to the control poster. As we can see from the figure, the effects of poster 1 on any outcome were statistically insignificant. The effect of poster 2 on the other hand is significant on several outcomes. Those who see this poster have lower overall compliance, which is driven by them rating the message less credible, while their planned observance is unaffected. When we use face touches per minute as our behavioral outcome, the point estimate is negative, which means it is in the desired behavioral direction, but the confidence interval includes zero. Those exposed to the second poster do provide slightly longer video responses, which could affect the face-touching frequency. If we instead rely on the number of face touches in the first 30 seconds, we see a negative point estimate of 0.066 from a baseline of 0.53 in the control group, but the confidence interval of [-0.134; 0.001] still marginally includes zero. Still, it is interesting that the poster, which gave the strongest negative response to credibility, also has the largest behavioral point estimates in the desired direction.

Figure A1: Each treatment poster compared to the control poster



A3 Alternative specifications of the interaction between the partisan message and conservative identity

In Figure A2 and A3, we plot alternative specifications of the interaction that we present in Figure 6. The results are substantially the same regardless of specification.

Figure A2: Partisan message compared to placebo message by identification with the Conservative Party (scale split at the median).

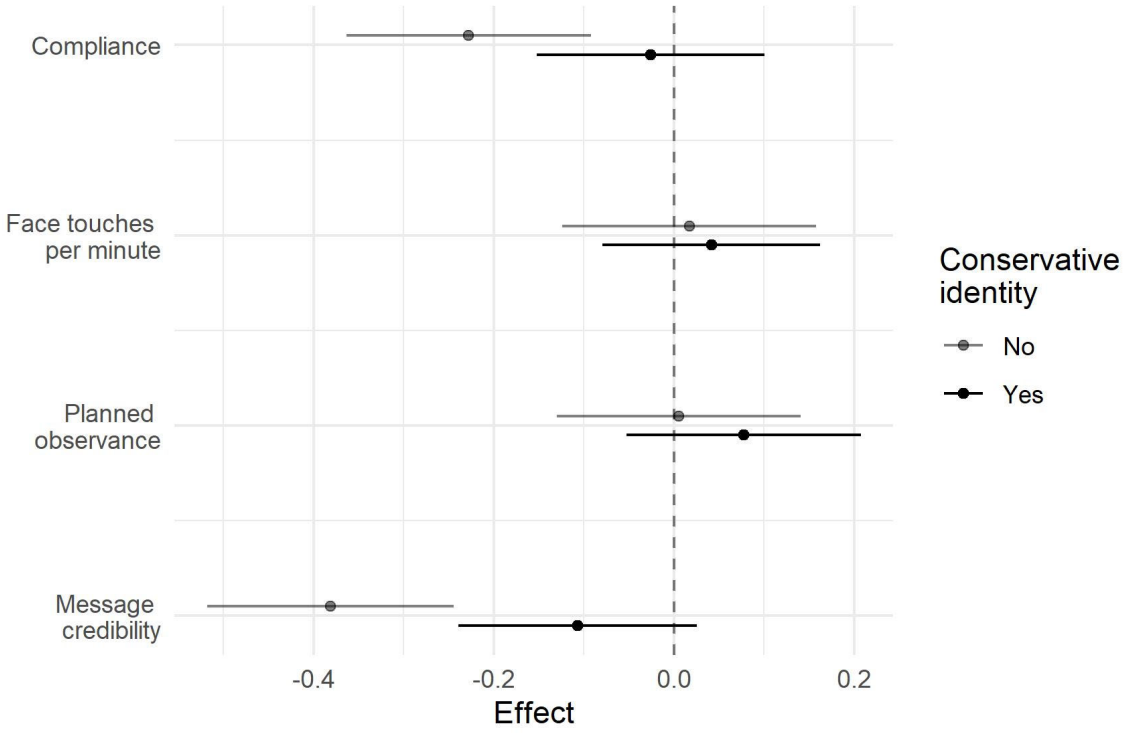
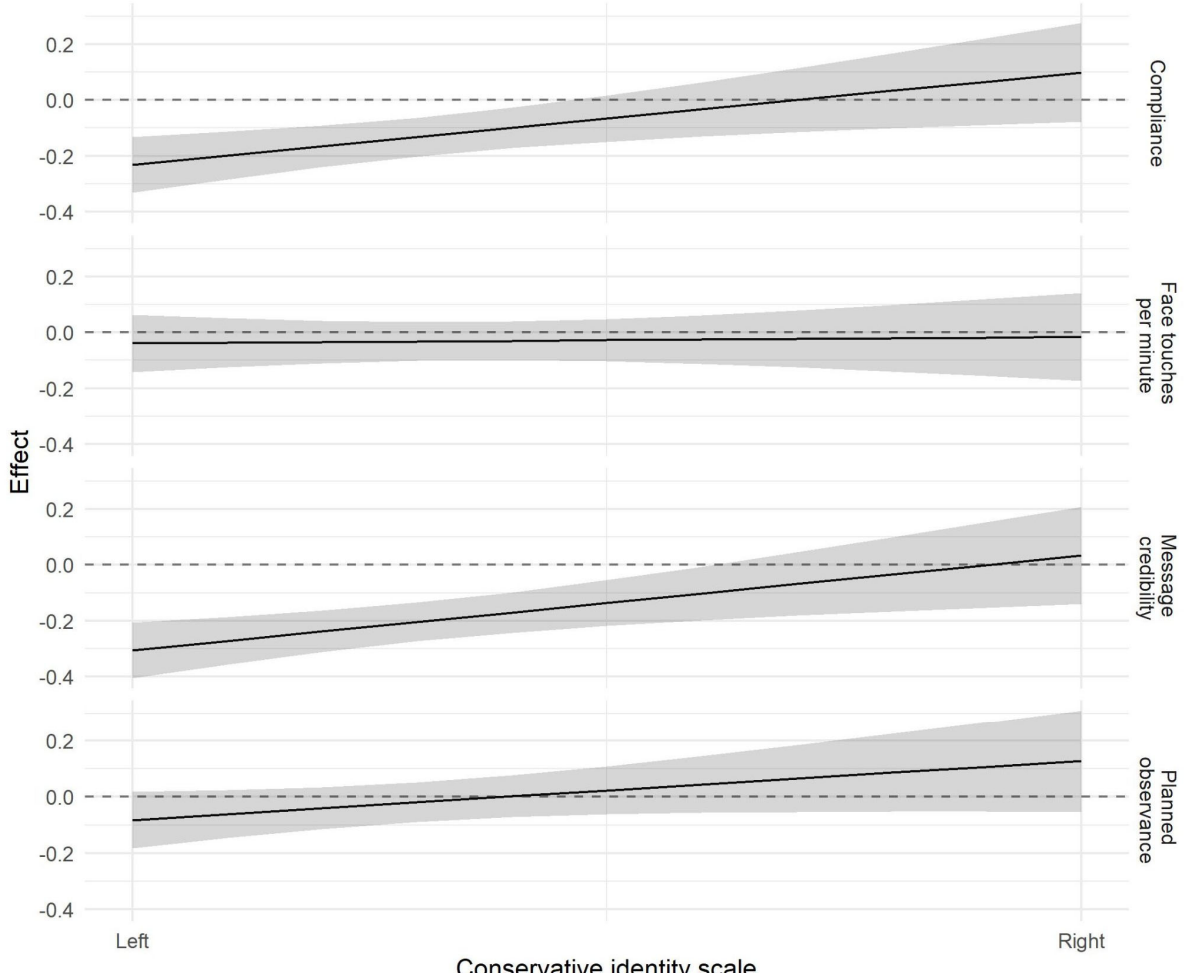


Figure A3: Partisan message compared to placebo message by identification with the Conservative Party (full scale).



A4 Inter-coder reliability

In Table A2, we show means and standard deviations for the first 30 seconds of the video and the full video for each of our coders. We also show the share of videos with no recorded face touches in the first 30 seconds or the full video. We can see that coder 2 and 3 code at similar rates, while coder 1 has higher counts in both the first 30 seconds and the full video. The share of videos with no registered face touches is similar in the full videos but varies somewhat in the first 30 seconds.

Table A2: Mean, standard deviation, and no touches by coders for first 30 seconds and full video

RA	Coder 1	Coder 3	Coder 2
Mean touches first 30	0.71	0.50	0.44
Mean touches full video	1.66	1.21	1.12
Sd touches first 30	1.10	0.76	0.71
Sd touches full video	2.33	1.65	1.52
No registered touches first 30	0.60	0.63	0.67
No registered touches full video	0.45	0.46	0.47
N	2,381	1,013	2,109

A subset of the videos were coded twice by the same coder, and between all coders there was also an overlap of coded videos. In Table A3, we show correlations within the coder who coded some videos twice and between the coders, for the videos that they coded more than once. We do so for both the first 30 seconds and the full video. Overall, we see high correlations.

Table A3: Correlation within one coder and between coders in recorded counts in the first 30 seconds and in the full video.

Coder pair	correlation first 30	N first 30	Correlation full video	N full video
Coder1-Coder1	0.80	275	0.87	269
Coder1-Coder2	0.74	502	0.82	494
Coder2-Coder3	0.79	141	0.91	137
Coder1-Coder3	0.82	574	0.88	565

A5 Placebo and Treatment Posters

Placebo poster

Treatment poster 1

Treatment poster 2