When Behavioural Science Can Make a Difference in Times of COVID-19

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Abstract

In a large study that involved 2637 participants recruited from a representative UK and US sample, we tested the influence of four behavioural interventions (vs. control) on a range of behaviours important for reducing the spread of COVID-19 a day after the interventions were administered. Even if people largely complied with social distancing measures, our analyses showed that for certain subgroups of the population the interventions made a positive difference. More specifically, for those who started practising social distancing relatively recently (e.g. 14 or fewer days ago), an information-based intervention increased general compliance with social distancing and reduced both the number of times people went out and the number of hours they spent outside. However, for people who started practising social distancing relatively early (e.g. 37 or more days ago), the interventions tended to backfire and, in some cases, reduced compliance with social distancing. Overall, this research has various policy implications and shows that, although behavioural interventions can positively impact compliance with social distancing, their effect may depend on personal circumstances.

Keywords: COVID-19, isolation, distancing, behavioural science.

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Introduction

Starting in the Hubei province of China in December 2019, COVID-19 spread across the globe and had a major impact on the world in the following months. Many countries took unprecedented measures to curtail the pandemic and "flatten the curve". They asked their citizens to wash the hands frequently, keep distance from others when outside, and declared lockdowns, meaning that people had to stay at home unless undertaking essential activities, such as shopping for groceries and medicine. With this sudden need to convince millions of people to behave in ways radically different from their routine, governments started looking at behavioural science to encourage compliance with the newly introduced measures (e.g. Barari et al., 2020; Gumber and Bulsari, 2020; Knight, 2020; Yates, 2020). At the same time, social and behavioural scientists wrote recommendations for policymakers concerning the interventions and scientific principles they should consider using (Brooks et al., 2020; Hahn et al., 2020a; Haushofer and Metcalf, 2020; Johnson et al., 2020; Lunn et al., 2020; Van Bavel et al., 2020).

However, the results of the first controlled experiments deploying such interventions are mixed. Barari et al. (2020) investigated whether messages designed to evoke norms and prompt reflection about the social impact of one's actions would improve attitudes and increase intentions to comply with governmental measures in Italy. They found no effect of the interventions compared to a control group. In another study, Blagov (2020) tested schema-congruence theory—according to which people prefer and are more easily persuaded by messages aligned with their own views of themselves—in the context of COVID-19. Participants rated the appeal of five public health messages directed at people with different personalities. Fewer than half of the hypotheses were supported: although certain personality traits predicted message appeal, these links were largely message non-specific. Moreover, in a wider study testing moral messages, Everett et al. (2020) employed the "messenger effect" as one of the widely used behavioural science intervention techniques (Dolan et al., 2012). They manipulated the messenger as being either a citizen (high school teacher) or a leader (Department of Education Director) but did not find any meaningful effect on behavioural intentions. Finally, Pfattheicher et al. (2020) tested whether showing a short empathy-inducing video combined with information about the use of physical distancing would increase the motivation to adhere to the distancing compared to information alone or control. In contrast to the other studies cited, they did find an impact of their intervention. The empathy and information condition increased respondents' motivation to comply with physical distancing relative to both control and information alone.

Based on these mixed findings, are we to conclude that behavioural science interventions are not what they are cut out to be when push comes to shove, or that they only "sometimes work"? The reality is that there could be many explanations for the mixed results. However, one plausible explanation, as indicated by Barari et al.'s (2020) findings, is that most participants were already complying with the governmental measures and there may have been a limited space for the interventions to make a difference. This echoes Fetzer et al.'s (2020) finding according to which, in a study covering 58 countries and over 100,000 respondents, many people took the pandemic seriously and complied with the governmental measures. Nevertheless, understanding how to influence those individuals who do not comply is crucial given that, even if not the majority, they can have a large impact on the spread of the virus (Liu et al., 2020; Shereen et al., 2020). Therefore, it is important to uncover which behavioural science interventions can further improve compliance with behaviours aimed to reduce the spread of COVID-19, and under which circumstances.

Exploring the Space for Influence

In the present research, we aimed to further examine whether, and under which conditions, behavioural science can make a difference concerning the COVID-19 pandemic by focusing on the "space" that other researchers—to the best of our knowledge—have not yet explored in detail. More specifically, we developed four interventions grounded in psychological and behavioural literature and tested several moderator variables to identify circumstances in which these interventions are most likely to be effective. Moreover, in contrast to the previously conducted studies, our research tested actual self-reported behaviours aimed to reduce the spread of COVID-19 rather than the intentions to undertake these behaviours. This was important considering that research on the intention—behaviour gap indicates that intentions do not always result in behaviour (Webb and Sheeran, 2006).

One of the interventions we designed focused on having participants reflect on an activity they find meaningful and formulating a clear plan (i.e. implementation intentions, Oettingen et al., 2015) on how they would start doing the activity the next day and overcome any potential obstacles in this regard. Given that motivation has been found to have a strong relationship with desired behaviours during COVID-19 (Miller et al., 2020), meaningfulness combined with implementation intentions and mental contrasting may activate motivational states that would encourage people to stay at home and pursue the planned activity instead of going out (Lunn et al., 2020; Oettingen et al., 2015).

Another intervention we developed was a message describing how adhering to strict social distancing is important for saving the economy. There are several reasons why the economic message may have a positive impact on protective behaviours during COVID-19. Given that people who report higher self-interest engage less in social distancing (Oosterhoff and Palmer, 2020), saving the economy, which serves one's self-interest, may appeal to these individuals because of matching their values and thus "paradoxically" prompt them to engage in distancing behaviours (Blagov, 2020; Van Bavel et al., 2020). At the same time, statements revolving around an outcome that is "best for all" (i.e. saving the economy) should encourage feelings of collaboration, which might equally prompt individuals to comply with the desired behaviours (Lunn et al., 2020).

Moreover, an "information intervention" we created leveraged insights from inoculation theory which postulates the possibility of administering a "psychological vaccine" against common misconceptions. Just like one would expose people to a weakened virus in the case of a biological vaccine, individuals are exposed to a weakened version of a misconception that is subsequently refuted with an evidence-based counterargument (Eagly and Chaiken, 1993; McGuire and Papageorgis, 1961; Roozenbeek and Van Der Linden, 2019; Van Der Linden et al., 2017). Because participants are cognitively engaged in the process of assessing the misconceptions, resistance can be conferred against future exposure to similar misconceptions. An additional reason why this intervention may be effective is that it might help mitigate the "optimism bias" that people have been found to carry regarding the risk of contracting COVID-19 themselves and infecting others (Kuper-Smith et al., 2020; Raude et al., 2020).

A fourth intervention had respondents write a letter to a loved one who is vulnerable to COVID-19. Previous studies that tested interventions grounded in empathy and concern toward others produced mixed results. Whereas Everett et al. (2020) and Pfattheicher (2020) showed that these interventions positively impact people's intentions to comply with the desired behaviours, in Barari et al. (2020) asking people to write the name of a loved-one they wanted to protect had no effect. We therefore wanted to explore whether a more "immersive" version of such interventions, where people would not only think about a loved one vulnerable to COVID-19 but also write a letter to that person explaining that they will do everything that is necessary to stop the spread of the virus, would create a behavioural change. Importantly, we also wanted to understand whether the effects of interventions based on empathy and concern for others are restricted to specific subsets of the population, as examined via the moderators we describe next.

A crucial part of our research was to test whether the effects of the four interventions we developed are bounded by several moderator variables. Noting Barari et al.'s (2020) and Fetzer et al.'s (2020) reported "ceiling" effects, we were aware that the behavioural interventions were unlikely to influence people already complying with the governmental policies and guidelines. However, several studies conducted on US and UK participants indicated that not all subsets of the population are complying equally and there is a space for improvement (e.g. Atchison et al., 2020; COVID-19 Psychological Research Consortium, 2020; Oosterhoff and Palmer, 2020; Wise et al., 2020). We therefore decided to conduct our study on a representative sample of US and UK participants to uncover variables that explain when interventions may be effective in influencing behaviours to reduce the spread of COVID-19.

The variables on which we focused were “living situation” (i.e. to what degree people’s living situation allowed them to sufficiently self-distance if necessary) and “economic reasons” (i.e. whether people found it difficult to practise self-distancing for economic reasons). We expected that the interventions we developed would work specifically for people who find it difficult to practise self-distancing for economic reasons or due to their living situation, given that such individuals may be less likely to practise social distancing and could further improve (see Im et al., 2020). Moreover, recognising the reigning debate around “behavioural fatigue” in the media and among scientists themselves (Hahn et al., 2020b; Yates, 2020), we also focused on the length of time people had already spent in quarantine. Arguments around behavioural fatigue would suggest that people become tired after being in quarantine for a long time (Collinson, Khan, and Heffernan, 2015; Yates, 2020). One might therefore predict that those who have been self-isolating for longer may require interventions to continue to comply. However, another possibility is that people who have been self-isolating for longer could possibly have done so because of taking the disease more seriously than others and may thus continue being highly compliant. This assumption is also in line with previous research showing that past behaviour is one of the strongest predictors of future behaviour (Ajzen, 2002). Therefore, it is also possible that our interventions will not be effective for individuals who have already spent a long time in isolation. Instead, in this line of reasoning, interventions should be effective for those who started distancing more recently, given that their behaviour may indicate they are not taking the governmental recommendations seriously and there is further space for improvement.

In short, the present study aimed to investigate how a series of newly designed behavioural interventions impact self-reported protective behaviours against COVID-

19. The moderating effects of participants' living situation, economic reasons, and the length of time they had already spent social distancing were tested.

Method

Behavioural Interventions

Four different behavioural interventions were used in the present research. In the *letter* condition, participants were asked to think about a person vulnerable to COVID-19 they know and who means a lot to them, and to write a letter to that person explaining that they will do everything that is necessary to stop the spread of the virus and ensure this person survives the crisis.

In the *meaningful activity* condition, participants were asked to take a moment to reflect on an activity they find meaningful and they could realistically do under the current circumstances. Then they formulated a clear plan regarding how they would start doing this activity tomorrow: they were instructed to consider the necessary steps to ensure they are ready to start pursuing the activity, to think about any obstacles that could stop them from doing this activity and how to overcome them, etc. (Oettingen et al., 2015).

In the *economy* condition, participants were asked to read a text providing an economic argument regarding why adhering to strict social distancing measures is important and can save the economy in the long run.

Finally, in the *information* condition, participants were presented with six hypothetical scenarios inspired by examples of real-life situations in which people may violate behavioural recommendations aimed at tackling the spread of COVID-19 due to various misconceptions (e.g. socializing with neighbours who live in the same building and have been compliant with staying at home). Then, after reading each scenario, they were asked a question regarding the appropriateness of actions described in the scenario (e.g. whether it would be appropriate to visit the neighbours or invite their kids over during lockdown). Upon answering, participants were immediately provided with feedback clarifying why their answer was correct or correcting their wrong answer with a detailed explanation that debunked the related misconceptions.

In the *control* condition, participants did not receive any experimental manipulation. Exact procedures used for each of the four interventions are available in the Supplementary Materials (pp. 3-14).

Determining Sample Size

Considering that we aimed to obtain a nationally representative sample consisting of UK and US participants stratified according to age, gender, and ethnicity via Prolific.co, we decided to recruit the maximum number of participants this platform allows—3000 (1500 for the US and 1500 for the UK). We estimated that potentially 20% of these participants would not enter the final analyses either because some of them would not complete both parts of the study, or because they would not meet the exclusion criteria (see the section *Exclusion Criteria* below), thus amounting to 2400 participants in total (or 480 per condition). We then conducted sensitivity power analyses using G-Power (Faul et al., 2009) to estimate the smallest effect sizes the study was sufficiently powered to detect, using the power of 99% and significance criterion of 0.00136, which is the significance level that the most significant effect for each set of analyses (e.g. testing main and moderated effects) would need to obtain to pass the false discover rate (FDR) corrections for multiple tests (Benjamini and Hochberg, 1995) that we used (sections *Testing main effects* and

Testing moderated effects below describe across how many significance tests the FDR corrections were applied for each set of analyses). We performed sensitivity power analyses only for continuous dependent variables (see the section *Dependent Variables* below) by relying on multiple linear regression, given that power analyses for categorical dependent variables based on logistic regression required inputting various parameters we could not determine in advance based on previous research and produced highly variable results depending on the values of these parameters.

The sensitivity power analyses showed that the smallest main effect of an intervention (vs. control) condition on a continuous dependent variable the study was sufficiently powered to detect was 0.0127664 (Cohen's f^2), whereas the smallest moderated effect was 0.0127665 (Cohen's f^2). These effects sizes are generally considered small (Cohen, 1988), and the study was therefore sufficiently powered to capture small effects for continuous dependent variables considering that the final number of participants included in statistical analyses exceeded the initial estimate of 2440.

Participants, Design, and Procedure

Out of 3014 participants who were initially recruited for Part 1, 2863 participants (1442 from the UK and 1421 from the US) completed both parts of the study (Males = 1401, Females = 1456, Other = 6, M_{age} = 45.744). They were recruited via Prolific.co using their representative sample (i.e. a sample that reflects the demographics of a country in terms of age, gender, and ethnicity) functionality. We used a between-subjects design consisting of five levels (Condition: Control, Letter, Meaningful Activity, Economy, and Information). The study received ethical approval from the Research Ethics Committee of the university of the first author and was conducted between 8-17 April 2020.

In Part 1 of the study, all participants first read the consent form, and after agreeing to participate received the questions measuring moderators and covariates (see the *Measures* section below). Thereafter, they were randomly allocated to the five study conditions. A stratified randomization procedure was implemented, which means that randomization was performed separately within the UK and US samples to achieve a comparable distribution of participants from these two countries across conditions. After being subjected to different manipulations pertaining to each condition (see the *Behavioural Interventions* section above and the Supplementary Materials, pp. 3-14), all participants completed the items measuring eight mediator variables (see the *Mediators* section below) and were then given the opportunity to write their comments regarding the study, after which Part 1 was finished.

For Part 2, participants were contacted on the second day after finishing Part 1. They again read the consent form and were then given the questions measuring 11 different dependent variables relevant to COVID-19 (see the *Dependent variables* section below). Finally, after being given the opportunity to write their comments regarding the study, they were debriefed and asked whether they agree that their responses are used in our scientific analyses (see the *Exclusion Criteria* section below), which was required for ethical purposes.

Measures

Dependent variables

We tested 9 continuous and 2 categorical (dichotomous) dependent variables that tackled various behaviours aimed at reducing the spread of COVID-19, from social distancing to hygiene. All the behaviours were measured regarding "yesterday" (in relation to Part 2 of the study) rather than regarding a longer period to minimize the potential confounding effects of forgetting on participants' self-reports.

The continuous dependent variables involved *general distancing* (i.e. the extent to which participants practised social distancing); *going out times* (i.e. how many times people left their house to do any activities except for the essential ones, defined as buying food or medication, going to the doctor, or working if considered an "essential worker" according to their country's guidelines); *going out hours* (i.e. for how many hours they left their house to do any activities except for the essential ones); *physical fitness times* (i.e. how many times people left their house to maintain their physical health); *physical fitness hours* (i.e. for how many hours people left their house to maintain their physical health); *keeping distance* (i.e. whether people kept the recommended distance of at least 1.5-2 meters or 5-7 feet between themselves and other people if they left the house); *relative hand washing* (i.e. whether people washed their hands more than they would usually wash them before the COVID-19 crisis); *disinfect* (i.e. whether people were disinfecting any packages or foods they brought into the house); and *hand washing times* (i.e. how many times approximately they washed their hands). *General distancing* was measured on a scale from 1 (Not at all) to 5 (Extremely). *Going out times* and *physical fitness times* were measured on a scale from 0 (Staying at home all the time) to 11 (More than ten times) in increments of 1 time. *Going out hours* and *physical fitness hours* were measured on a scale from 0 (Staying at home all the time) to 11 (More than ten hours) in increments of 1 hour. *Keeping distance, disinfect,* and *relative hand washing* were measured on a scale from 1 (Strongly disagree) to 7 (Strongly agree):

for the first two variables, a response option “Does not apply to me” was also allowed, and participants who selected it were not included in statistical analyses involving these variables.

The categorical dependent variables involved *out family friends* (i.e. whether people left their house to meet their family members or friends), and *social gatherings* (i.e. whether people allowed their family members, friends, or other people who don’t live with them to visit them). Both variables were measured on a dichotomous response scale 0 (No) and 1 (Yes).

The exact questions and response options for all dependent variables are available in the Supplementary Materials (pp. 14-18).

Moderators

Three moderator variables were assessed. *Distancing history* (i.e. how many days ago people first started practising social distancing) was measured on a scale from 0 (I do not practise social distancing) to 100 (More than 100 days ago) in increments of 1 day. *Living situation* (i.e. whether people’s living situation allows them to sufficiently self-distance if necessary) and *economic reasons* (i.e. the extent to which people cannot afford to practise self-distancing for economic reasons) were both assessed on a scale from 1 (Strongly disagree) to 7 (Strongly agree). The exact questions and response options for all moderator variables can be found in the Supplementary Materials (pp. 18-20).

Mediators

To get an insight into the mechanism behind any potential main and moderated effects of our interventions, we measured several different mediators that are conceptually linked to the interventions and/or have been identified as potential drivers of COVID-19 related behaviours in previous research (e.g. Li et al., 2020;

Oosterhoff and Palmer, 2020; Pfattheicher et al., 2020). These mediators are *serious disease* (i.e. to what extent participants think that COVID-19 poses a serious risk for all humans), *health concern* (i.e. to what extent they think that they could be severely affected if they were to catch COVID-19), *concern close others* (i.e. concern for their close ones who are vulnerable and could get COVID-19), *concern vulnerable others* (i.e. concern for anyone who is vulnerable and could get COVID-19), *economic concern* (i.e. being concerned about how COVID-19 may impact the economy), *meaningful time* (i.e. to what extend people feel that the time they will spend at home throughout this period will be meaningful), *knowledge* (i.e. how they would rate their current knowledge regarding COVID-19), and *future intentions* (i.e. to what extent they are intending to undertake behaviours that could reduce the spread of COVID-19 going forward). *Serious disease, health concern, concern close ones, concern vulnerable ones, economic reasons, meaningful time*, and *future intentions* were assessed on a scale from 1 (Strongly disagree) to 7 (Strongly agree), whereas *knowledge* was assessed on a scale from 1 (Not knowledgeable at all) to 5 (Extremely knowledgeable).

Covariates

The following covariates were measured in the present study: *household income* (i.e. participants' feelings about their own household income these days); *education* (i.e. highest education level); *prior home* (i.e. how many days per week participants typically spent at home prior to the COVID-19 crisis); *household* (i.e. how many people, in addition to the participant, currently live in their household), *property* (i.e. the size of property in which they live), *garden* (i.e. whether participants have access to an outdoor space they can use without being in danger of encountering other people), *key worker* (i.e. whether the participant can be considered a key worker as defined by their home country), *gender* (i.e. male vs. female vs. other); *country* (i.e. whether participants were from the UK or US sample); and *on time* (i.e. whether participants completed the survey for Part 2 on time—on the second day after they completed Part 1). The exact questions and response options for the covariates are presented in the Supplementary Materials (pp. 23-26) given the space restriction.

Exclusion criteria

To determine which participants should be excluded from statistical analyses, we used two *instructed-response items* (Kung, Kwok and Brown, 2018; Meade and Craig, 2012; Thomas and Clifford, 2017), one measured in Part 1 and one in Part 2, and two *seriousness checks* (Aust et al., 2013), one per each part. Also, at the end of the study (Part 2) participants were asked regarding the *agreement* for their data to be used in our scientific analyses. Only participants who successfully completed both the instructed response items and seriousness checks and gave consent to use their data were included in the statistical analyses. The exact questions and response options for all exclusion criteria items are available in the Supplementary Materials (pp. 26-28).

Results

Preliminary Analyses

Excluded data

Out of 2863 participants who completed both parts of the study, 2637 were included in statistical analyses (control condition: 550; letter condition: 478;

meaningful activity condition: 525; economy condition: 539; information condition: 545) after the exclusion criteria were applied.

General compliance with behaviours to reduce the spread of COVID-19

To understand general level of compliance with social distancing and other behaviours aimed at reducing the spread of COVID-19, we computed the percentage of participants who selected a particular response option for each of the behavioural dependent variables (Table 1). As can be seen from Table 1, most participants were highly compliant with social distancing. Roughly 76% of them responded with 5 for *General Social Distancing*, which indicates extreme compliance. Moreover, between 62%-71% participants responded with 0 for *Going Out Times*, *Going Out Hours*, *Physical Fitness Times*, and *Physical Fitness Hours*, which means that they generally stayed at home. Also, most participants did not leave their house to meet family members or friends (*Out Family Friends*; 96%) and did not allow others to visit them (*Social Gatherings*; 97%). Finally, responses for *Distancing, Relative Hand Washing, Hand Washing Times*, *and Disinfect* indicate that people largely tried to keep distance of 1.5-2 meters between themselves and others when outside and maintain appropriate hygiene.

Table 1 about here

Main Analyses

We tested both main and moderated effects of the interventions on the dependent variables. Our general analytic approach was to first test all the main effects of the interventions (vs. control) and the moderated effects for each moderator. Then, we applied the false discovery rate (FDR) correction by Benjamini and Hochberg (1995) to probe whether the significant effects we identified remained significant despite multiple comparisons. Finally, for all the effects that remained significant after the FDR correction, we tested whether they would remain significant when controlling for covariates. Therefore, for an effect to be identified as robust, it had to both pass the FDR correction (Benjamini and Hochberg, 1995) and remain significant despite covariates. For all the effects we identified as robust, we then conducted mediation analyses (for main effects) or moderated mediation analyses (for moderated effects) to identify whether the mediators we measured could provide further insights into the mechanism behind the effects. Descriptive statistics and zero-order correlations between all continuous variables tested in the study can be seen in the Supplementary Materials.

Testing main effects

For each of the nine continuous dependent variables, we performed a multiple linear regression analysis, and for each of the two dichotomous dependent variables we performed a logistic regression analysis. In each regression analysis, four dummy variables were included as predictors, one for the letter condition, one for the meaningful activity condition, one for the economy condition, and one for the information condition. Therefore, 44 effects in total were tested across 11 regression analyses (11 analyses x four effects corresponding to each of the four conditions per analysis). Only two effects were significant. More specifically, the meaningful activity condition decreased the number of hours participants spent outside compared to the control condition, b = -0.126, 95% CI [-0.247, -0.005], $t(2632)$ = -2.041, p = .041, whereas the information condition made participants less likely to allow their family members, friends, or other people to visit them compared to the control condition, Odds Ratio = 0.423, 95% CI [0.192, 0.932], Wald = 4.556, $p = .033$. However, after the FDR correction (Benjamini and Hochberg, 1995) that considered all the 44 significance tests concerning the main effects was implemented, no effects remained significant. Therefore, we concluded that no interventions had a robust influence on one or more dependent variables. We thus present all 11 regression analyses that probed the main effects in the Supplementary Materials (pp. 31-36) to meet the space restrictions.

Testing moderated effects

Distancing history. To test the effect of the intervention conditions (vs. control) on the dependent variables as moderated by *distancing history*, we performed 11 multiple regression analyses—nine linear regressions for the continuous dependent variables and 2 logistic regressions for the categorical dependent variables. In each regression analysis, four interaction effects (one between each condition and distancing history) were computed, thus amounting to 44 interaction effects in total across the 11 regression analyses. Seven significant interaction effects were identified (see Supplementary Materials, pp. 36-51). However, after the FDR correction (Benjamini and Hochberg, 1995) that considered all the 44 significance tests concerning the interaction effects was implemented, three of these effects remained significant. These were the interactions between the information condition and distancing history for *general distancing*, *going out times*, and *going out hours* as dependent variables. Linear regression analyses involving these interaction effects are presented in Tables 2, 3, and 4, respectively.

Tables 2, 3, and 4 about here

To further probe the pattern of these three interactions, we used the Johnson-Neyman technique (Bauer and Curran, 2005; Johnson and Fay, 1950) that was implemented via the *interactions* package in R (Long, 2019). Given that this technique involves computing the effect of conditions on a dependent variable for many different levels of the moderator, a false discovery rate correction by Esarey and Sumner (2018) was used to minimize the chance of false positive findings when it comes to interaction patterns. As can be seen from Figure 1A, the information condition (vs. control) positively impacted general distancing for participants who started practising social distancing 14.472 or fewer days ago, whereas it had a negative effect on general distancing for participants who started practising it 31.608 or more days ago. Similarly, the information condition (vs. control) decreased the number of times people went out for those who started practising social distancing 18.016 or fewer days ago, whereas it increased it for people who started practising social distancing 28.785 or more days ago (Figure 1B). Finally, the information condition (vs. control) decreased the number of hours people spent outside for those who started practising social distancing up to 19.415 days ago, whereas it increased it for people who started practising social distancing 37.269 or more days ago (Figure 1C).

Figure 1 about here

To further demonstrate the robustness of the three significant interaction effects, we computed the same analyses testing these effects as described above, but this time with covariates included in the regression models (Supplementary Materials, pp. 82-90).

The interaction between the information (vs. control) condition and distancing history was again significant in influencing general distancing, b = -0.011, 95% CI [-0.018, -0.004], t(2600) = -3.050, p = .002. Further analysing the pattern of the interaction using the Johnson-Neyman technique (Johnson and Fay, 1950) again showed that the information condition (vs. control) positively impacted general distancing for participants who started practising social distancing 14.560 or fewer days ago, whereas it had a negative effect on general distancing for participants who started practising social distancing 31.853 or more days ago (Supplementary Materials, pp. 82-84). Moreover, the interaction between the information (vs. control) condition and distancing history also remained significant in influencing going out times, b = 0.021, 95% CI [0.011, 0.031], t(2600) = 4.164, p = <.001. More specifically, the information condition (vs. control) decreased the number of times people went out for those who started practising social distancing 18.009 or fewer days ago, whereas it increased it for people who started practising social distancing 28.663 or more days ago (Supplementary Materials, pp. 84-86). Finally, the interaction between the information (vs. control) condition and distancing history was again significant in influencing going out hours, b = 0.021, 95% CI [0.009, 0.033], t(2600) = 3.398, p = .001. The information condition (vs. control) decreased the number of hours people spent outside for those who started practising social distancing up to 20.051 days ago, whereas it increased it for people who started practising social distancing 36.644 or more days ago (Supplementary Materials, pp. 87-89). Therefore, the results with and without the covariates included in the regression models were almost identical, thus demonstrating the robustness of the findings.

Considering that all the three interaction effects remained significant despite the FDR corrections (Benjamini and Hochberg, 1995) or covariate testing and were therefore robust, we finally conducted a moderated mediation analysis for each interaction effect. This analysis allows probing whether some of the mediators we measured can explain the influence of the information (vs. control) condition on *general distancing*, *going out times*, or *going out hours* at different levels of the moderator (distancing history) for which these effects were significant. All the moderated mediation analyses were conducted using the Process package (Model 8) by Hayes (2018) implemented in SPSS. Percentile bootstrapping procedure with 10000 resamples was used. All eight mediators were included in a moderated mediation analysis in parallel, which means that the analysis accounted for the correlations between them when computing the mediated effects. The analyses showed that none of the moderated mediation effects were significant at $p < .05$, given that the 95% Confidence Interval of the Index of Moderated Mediation for each mediator contained 0 (Hayes, 2018).

Living situation. To test the effect of the intervention conditions (vs. control) on the dependent variables as moderated by *living situation*, we computed the same analyses as for the previous moderator. More specifically, we conducted 11 multiple regression analyses—nine linear regressions for the continuous dependent variables and 2 logistic regressions for the categorical dependent variables. In each regression analysis, four interaction effects (one between each condition and living situation) were computed, thus resulting in 44 interaction effects in total. Nine significant interaction effects were identified, which generally showed that our interventions tended to improve different behaviours aimed at reducing the spread of COVID-19 only for participants whose living situation did not allow them to sufficiently self-distance from others if necessary. However, after the FDR correction (Benjamini and Hochberg, 1995) that considered all the 44 significance tests concerning the interaction effects was implemented, none of the effects remained significant. We therefore present the analyses for all the interactions between the intervention (vs. control) conditions and living situation in the Supplementary Materials (pp. 51-68) for informative purposes.

Economic reasons. To test the impact of the interactions between the intervention conditions (vs. control) and *economic reasons* on the dependent variables, we again conducted 11 multiple regression analyses—nine linear regressions for the continuous dependent variables and 2 logistic regressions for the categorical dependent variables. In each regression analysis, four interaction effects (one between each condition and economic reasons) were computed, which resulted in 44 interaction effects in total. Five significant interaction effects were identified, which generally indicated that our interventions tended to improve behaviours aimed at reducing the spread of COVID-19 only for participants who could not easily afford to practice self-distancing due to economic reasons. However, after the FDR correction (Benjamini and Hochberg, 1995) that considered all the 44 significance tests concerning the interaction effects was implemented, none of the effects remained significant. We therefore present the analyses for *economic reasons* as a moderator in the Supplementary Materials (pp. 68-82) for informative purposes.

General Discussion

The present research sought to investigate the impact of four behavioural interventions (vs. control) on respondents' self-reported social distancing and protective behaviours against COVID-19 a day after the intervention. We found indications that the meaningful activity condition decreased the number of hours participants spent outside compared to the control condition and that the information condition made participants less likely to allow their family members, friends, or other people to visit them compared to the control condition. However, these effects did not survive corrections for multiple comparisons. Therefore, like other previous research (e.g. Barari et al., 2020; Everett et al., 2020), we had to conclude that behavioural interventions did not have a direct, robust influence on behaviours that could reduce the spread of COVID-19.

Where our research adds new insights, however, is in showing that there is a subgroup of people who do seem to benefit from behavioural interventions during the COVID-19 pandemic. Specifically, for people who started practising social distancing only recently (i.e. 14-19 or fewer days ago), the information (vs. control) condition improved general distancing and made them go outside less, whereas it had an undesirable effect regarding these variables for those who had been practising social distancing for longer (i.e. 29-37 or more days). These effects remained significant even after controlling for a wide range of demographic variables. Considering that the correlations between distancing history and various behaviours aimed at reducing the spread of COVID-19 that we measured generally indicated that people who had been practising social distancing for longer were also more likely to comply with these behaviours (Supplementary Materials, p. 30), our findings suggest that the interventions worked for less disciplined individuals for whom there was a space for further improvement. Although the significant interactions between our interventions and living situation or economic reasons did not withstand corrections for multiple comparisons, the patterns of these interactions did point in the same direction that behavioural interventions work better for people who have more trouble complying with social distancing measures (Supplementary Materials, pp. 51-82).

These results have various policy implications. For one, we have confirmed what other researchers have found as well, that a blanket approach to implementing behaviour interventions in a situation where many people already comply may not be meaningful. However, our findings indicate that certain behavioural interventions may work for subgroups of individuals, specifically for those who started practising social distancing only recently—an insight that may more generally apply to individuals who have trouble complying with social distancing. The reason why this effect occurs remains to be further explored, given that none of the eight mediating variables we tested yielded significant effects. One possible clue might lie in Wise et al.’s (2020) work who showed that the ‘personal risk of infection’ is one of the strongest drivers of social distancing and handwashing. We did not explicitly test for this narrow mediator, but it is plausible that our information condition may have created the behavioural change by influencing people’s perceived personal risk of getting infected with COVID-19. Regardless, our research suggests that targeting people who are late adopters, and potentially those who have trouble self-distancing with information driven behavioural interventions can be fruitful. Practically, this may mean that governments would want to share more information or do a social inoculation game with people they find transgressing, or in areas where social distancing is known to be difficult.

Another interesting finding was that the information intervention could backfire for those who practised social distancing for longer periods (i.e. between 29-37 days or more). This would suggest that the “behavioural fatigue” explanation does not hold, at least not to the extent that people who have been distancing for a long while already need extra encouragement to continue to adhere to the guidelines. What the “backfiring” result might indicate is a psychological reactance effect (Brehm, 1966; Rains, 2013), meaning that participants who already were convinced of the need to distance themselves might have become irritated by being told to comply even more and therefore “lashed out” by complying less. Alternatively, these people may have been the ones who took COVID-19 very seriously from the very start, perhaps even over-catastrophising the situation. It is possible that the information condition corrected their understanding in making them more realistic about the situation in the opposite direction from those who started distancing only recently, thus leading them to see the disease as a bit less of a risk for themselves than they previously thought. More specifically, “Scenario 6” of the information condition where more concrete numbers around the risk of contracting COVID-19 were given could have had this effect.

Finally, to understand the implications of our research, it is also important to understand its limitations. First, we investigated an online sample of participants. Although we tested a representative sample in terms of age, gender, and ethnicity, we cannot exclude the possibility that respondents on Prolific.co are more used to computer work at home or otherwise different from the “average” UK or US citizen. Moreover, we did not have the opportunity to replicate our findings, which would in a normal research scenario be the preferred route of action (although we did conduct a highly powered study with many participants). Finally, given that we tested the intervention effects over a relatively short period of time in order to minimize potential confounding effects of forgetting on behavioural self-reports, we cannot ascertain whether the effects we demonstrated would also occur over a longer period.

Conclusion

Overall, this research found that behavioural science can inform meaningful interventions in times of COVID-19, and that the specific circumstances in which people find themselves matter when it comes to the impact of these interventions. An information based intervention increased compliance with social distancing and reduced the number of times and hours people went outside for non-essential errands, but only for those who started practising distancing relatively late, whereas it backfired for those who had been practising social distancing for a long time. Future research might look to replicate this effect as well as explore further mechanisms and moderator variables that may help inform policymakers concerning whom to target with behavioural interventions to reduce the spread of COVID-19.

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Table 1

The Percentage of Participants Who Selected a Response Option for Each of the 11 Behavioural Dependent Variables

Response Option	A	B	C	D	E	F	G	H	I	J	K
0		70.99	71.44	62.76	62.42	96.40	96.85				
1	0.38	23.28	20.93	31.74	30.91	3.60	3.11	0.38	1.37	0.11	6.33
2	0.83	4.21	5.38	4.10	5.54			0.57	4.25	0.57	12.32
3	2.96	0.72	0.99	0.64	0.72			0.57	3.53	2.05	4.97
4	20.17	0.19	0.38	0.49	0.27			0.91	6.79	4.13	5.23
5	75.65	0.34	0.23	0.15	0.04			3.41	14.33	7.62	8.61
6		0.08	0.08	-	0.08			13.80	32.92	10.54	12.40
7		-	-	0.04	-			40.12	36.82	12.67	17.22
8		0.08	0.23	-	-					6.83	
9		-	0.15	0.04	-					9.94	
10		0.04	-	0.04	0.04					3.00	
11		0.08	0.19	-	-					19.04	
12										0.83	
13										6.03	
14										0.61	
15										1.33	
16										5.95	
17										0.68	
18										0.34	
19										0.53	
20										0.11	
21										2.39	
Total N	2637	2637	2637	2637	2637	2637	2636	1576	2637	2637	1769

Note: **Response Option** refers to a response option that participants could select for a dependent variable (see the section *Dependent Variables* as well as the Supplementary Materials, pp. 14-18, for specifics regarding the response options). **Columns *A-E*** correspond to each of the 11 dependent variables as follows: A – General Distancing; B – Going Out Times; C – Going Out Hours; D – Physical Fitness Times; E – Physical Fitness Hours; F – Out Family Friends; G – Social Gatherings; H – Keeping Distance; I – Relative Hand Washing; J – Hand Washing Times; K – Disinfect. **Numbers for each variable** indicate the percentage (%) of participants who answered with the corresponding response option. **Light grey shading** indicates the range of possible response options for each variable, and **dark grey cells** indicate the highest desirable response option (e.g., for *General Distancing* response option 5 indicates highest possible compliance with social distancing). **Total N** at the bottom of the table corresponds to the total number of participants who responded for each variable.

Table 2

Multiple Linear Regression for the Influence of the Interactions Between the Intervention Conditions and Distancing History on General Distancing

Condition	*b*	Std. Error	*β*	*t*	*p*	95% CI for *b* Lower	95% CI for *b* Upper
(Constant)	4.367	0.065		66.977	<.001	4.239	4.495
Letter	0.078	0.097	0.050	.801	.423	-0.113	0.269
Meaningful Activity	0.113	0.089	0.075	1.276	.202	-0.061	0.287
Economy	0.155	0.093	0.103	1.675	.094	-0.026	0.336
Information	0.259	0.089	0.174	2.925	.003	0.086	0.433
Distancing History	0.016	0.003	0.251	5.584	<.001	0.010	0.022
Int. 1	-0.005	0.004	-0.074	-1.168	.243	-0.013	0.003
Int. 2	-0.006	0.004	-0.092	-1.512	.131	-0.013	0.002
Int. 3	-0.008	0.004	-0.125	-1.967	.049†	-0.016	0.000
Int. 4	-0.012	0.004	-0.193	-3.056	.002	-0.019	-0.004

Note. Model R^2 = 0.027. Control condition is the reference category. Symbol † indicates the initially significant interaction effects that stopped being significant after the false discovery rate (FDR) correction was applied. Int. 1 = Interaction between *Letter* and *Distancing History*; Int. 2 = Interaction between *Meaningful Activity* and *Distancing History*; Int. 3 = Interaction between *Economy* and *Distancing History*; Int. 4 = Interaction between *Information* and *Distancing History*.

Table 3

Multiple Linear Regression for the Influence of the Interactions Between the Intervention Conditions and Distancing History on Going Out Times

						95% CI for *b*	
Condition	***b***	**Std. Error**	***β***	***t***	***p***	**Lower**	**Upper**
(Constant)	0.793	0.087		9.151	<.001	0.623	0.963
Letter	-0.243	0.129	-0.117	-1.881	.060	-0.497	0.010
Meaningful Activity	-0.203	0.118	-0.102	-1.725	.085	-0.434	0.028
Economy	-0.236	0.123	-0.119	-1.917	.055	-0.477	0.005
Information	-0.479	0.118	-0.243	-4.065	<.001	-0.710	-0.248
Distancing History	-0.018	0.004	-0.206	-4.560	<.001	-0.025	-0.010
Int. 1	0.008	0.006	0.094	1.482	.139	-0.003	0.020
Int. 2	0.008	0.005	0.091	1.488	.137	-0.002	0.018
Int. 3	0.008	0.005	0.100	1.564	.118	-0.002	0.019
Int. 4	0.021	0.005	0.262	4.126	<.001	0.011	0.031

Note. Model R^2 = 0.016. Control condition is the reference category. Int. 1 = Interaction between *Letter* and *Distancing History*; Int. 2 = Interaction between *Meaningful Activity* and *Distancing History*; Int. 3 = Interaction between *Economy* and *Distancing History*; Int. 4 = Interaction between *Information* and *Distancing History*.

Table 4

Multiple Linear Regression for the Influence of the Interactions Between the Intervention Conditions and Distancing History on Going Out Hours

						95% CI for ***b***	
Condition	***b***	**Std. Error**	***β***	***t***	***p***	**Lower**	**Upper**
(Constant)	0.896	0.107		8.366	<.001	0.686	1.106
Letter	-0.447	0.160	-0.175	-2.794	.005	-0.760	-0.133
Meaningful Activity	-0.290	0.146	-0.117	-1.986	.047	-0.575	-0.004
Economy	-0.241	0.152	-0.099	-1.584	.113	-0.539	0.057
Information	-0.518	0.146	-0.213	-3.553	<.001	-0.804	-0.232
Distancing History	-0.019	0.005	-0.183	-4.037	<.001	-0.029	-0.010
Int. 1	0.016	0.007	0.141	2.204	.028†	0.002	0.029
Int. 2	0.011	0.006	0.108	1.748	.081	-0.001	0.024
Int. 3	0.009	0.007	0.081	1.274	.203	-0.005	0.022
Int. 4	0.020	0.006	0.206	3.236	.001	0.008	0.033

Note. Model R^2 = 0.011. Control condition is the reference category. Symbol † indicates the initially significant interaction effects that stopped being significant after the false discovery rate (FDR) correction was applied. Int. 1 = Interaction between *Letter* and *Distancing History*; Int. 2 = Interaction between *Meaningful Activity* and *Distancing History*; Int. 3 = Interaction between *Economy* and *Distancing History*; Int. 4 = Interaction between *Information* and *Distancing History*.

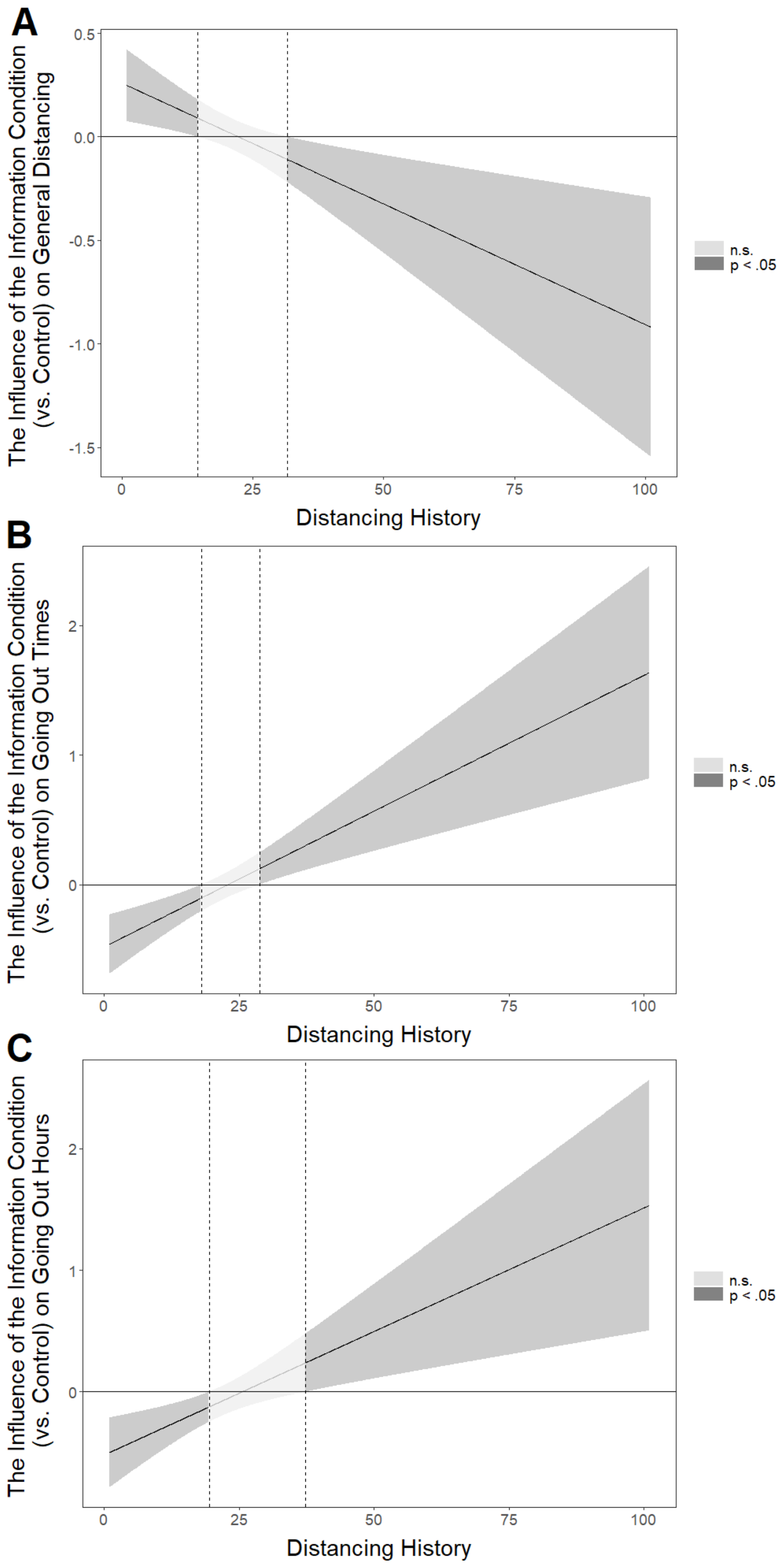

Figure 1. The influence of the information (vs. control) condition on general distancing (Panel A), going out times (Panel B), and going out hours (Panel C) at

different levels of distancing history, which corresponds to how many days before the intervention participants first started practising social distancing. Mean value of distancing history is 21.134.