Social Science & Medicine xxx (xxxx) xxx

Contents lists available at ScienceDirect

Social Science & Medicine

journal homepage: http://www.elsevier.com/locate/socscimed

Short communication

Motivating social distancing during the COVID-19 pandemic: An online experiment

Peter D. Lunn [a,b,*,1], Shane Timmons [a,1], Cameron A. Belton [a], Martina Barjaková [a], Hannah Julienne [a], Ciarán Lavin [a]

[a] Economic and Social Research Institute (ESRI), Whitaker Square, Sir John Rogerson's Quay, Dublin, Ireland
[b] Department of Economics, Trinity College Dublin, Ireland

ARTICLE INFO

Keywords:
Covid-19
Social distancing
Behavioral science
Identifiable victim
Psychology
Behavioral public policy

ABSTRACT

Rationale: Maintaining social distance during the COVID-19 pandemic can save lives. We therefore set out to test communication strategies to promote social distancing.
Objective: We aimed to test two novel public health messages against a control message. The first was designed to exploit the "identifiable victim" effect by highlighting the risk of transmission to identifiable vulnerable persons. The second sought to counteract intuitive underestimation of exponential transmission.
Method: In total, 500 Irish adults undertook a pre-registered, online experiment. They were randomly assigned to a control group or one of two treatment groups. The control group viewed a current poster that encouraged a 2-m separation between people. The two treatment groups saw posters of similar design, but with narrative messages describing how an individual had infected a specific vulnerable person or multiple other people. Later questions measured intentions to undertake three specific types of social interaction over the coming days and the stated acceptability of three other types of social interaction. Pilot work had identified these six behaviors as "marginal" – people were unsure whether they were advisable.
Results: Participants in the treatment conditions were more cautious about undertaking the behaviors and less accepting of them. This positive effect occurred despite participants rating the treatment posters as likely to be less effective and memorable than the control poster.
Conclusions: Messages that invoke thoughts of infecting vulnerable people or large numbers of people can motivate social distancing and, hence, help to limit the spread of COVID-19. Stated public evaluations (obtained via focus groups or surveys) may underestimate the actual effectiveness of such emotional messages.

1. Introduction

"Social distancing", reducing social interactions with others, may save millions of lives during the COVID-19 pandemic (Ferguson et al., 2020; Greenstone and Nigam, 2020). Governments worldwide have introduced social distancing measures, but compliance is vital (Anderson et al., 2020). While governments, public bodies, private organizations, and individuals can all communicate how and why people should maintain their distance from one another, some messages will work better than others. We describe an online experiment that tested different communication strategies that aimed to motivate social distancing by increasing caution with social interactions. The experiment was commissioned by Ireland's Department of Health for its National Public Health Emergency Team.

2. Background and hypotheses

Tasked with providing evidence to improve communications on social distancing, we looked to the behavioral science literature for helpful findings. We identified and tested two strategies: (i) highlighting the possibility that someone infects identifiable, vulnerable persons and (ii) highlighting the possibility that someone causes large numbers of other people to be infected.

In general, people are more likely to make sacrifices to help specific victims who are identified, relative to victims described merely statistically (Jenni and Loewenstein, 1997; Lee and Feeley, 2016). This effect

* Corresponding author. Economic and Social Research Institute (ESRI), Whitaker Square, Sir John Rogerson's Quay, Dublin, Ireland.
E-mail address: pete.lunn@esri.ie (P.D. Lunn).
[1] Co-lead author.

https://doi.org/10.1016/j.socscimed.2020.113478
Received in revised form 23 October 2020; Accepted 25 October 2020
Available online 27 October 2020

occurs even when a specific victim is referred to but remains anonymous, as if merely thinking about a specific person induces stronger caring emotions (Small and Loewenstein, 2003). Hence, we set out to test messages that highlight specific persons vulnerable to the coronavirus: an elderly person, a healthcare worker, etc.

Our second approach related to the exponential transmission of the virus. Exponential growth means more than increasing fast or accelerating – it is multiplicative, feeding on itself. People do not find this intuitive and greatly underestimate exponential growth (Wagenaar and Sagaria, 1975). Because of this "exponential growth bias", an individual may not realize how many onward infections could be prevented if they avoid infection. By stressing the risk of exponential transmission, communication might induce more cautious health behavior (Witte, 1992). Given this possibility, we also set out to test messages that highlight how one person's behavior could harm many others.

Both strategies centered on the potential for causing serious harm to others rather than protecting oneself, because early analyses of the pandemic identified the challenge as a collective action problem requiring coordinated behavior undertaken for the common good (Lunn et al., 2020). Hence, self-preservation would be unlikely to generate the extent of behavior change necessary from a societal perspective, especially among younger adults at low risk of serious harm. This analysis was supported by empirical evidence from contemporaneous tracking data in Ireland (Amárach Public Opinion Surveys) and some initial attempts to test persuasive messaging in other countries (Jordan et al., 2020). On average, individuals were substantially less worried about their own health than about the impact of prolonged restrictions and, especially, the health of family and friends.

The two strategies identified in the behavioral literature informed two campaign posters tested in the experiment. We refer to them as "identifiable person" (IP) and "transmission rate" (TR) treatments. Responses of the two treatment groups were compared to a control group who saw a purely informational poster based on a design employed by Ireland's public health authorities. The purpose of the posters was to encourage people to stay at least 2-m apart. This exact distance was already highly publicized at the time of the study, which was undertaken in the final week of March 2020. The aim was to reinforce the message and promote compliance.

Ideally, we would randomly assign participants to see a poster, then observe their subsequent behavior. Given practical restrictions, doing so was unfeasible. Instead, we used rating scales to measure intentions and attitudes following exposure to the posters. This is not ideal because, as well as possible differences between intentions and actions (Sheeran, 2002), such rating scales can produce ceiling effects, whereby the large majority of respondents report maximally "good" intentions and attitudes, as other experiments on COVID-19 communications have found (Barari et al., 2020; Everett et al., 2020).

To counter this problem, during the design phase we asked focus groups to describe activities where it was unclear whether the behavior was appropriate, given prevailing social distancing guidelines. Their responses informed our choice of outcome variables. The study took place at the end of March 2020 when infections in Ireland were rising rapidly and individuals were being asked to limit all social interaction and maintain a 2-m distance from one another, but before rules explicitly prevented social interaction between different households (bar exceptional circumstances). The focus groups identified behaviors that some individuals deemed acceptable but others not. We refer to these as "marginal behaviors". Some were relevant for all participants, such as meeting friends and relatives outdoors. We asked participants about their intentions to undertake these behaviors "over the next few days". Other marginal behaviors applied to only some participants, such as allowing children from different households to play together. We asked participants to judge the acceptability of these behaviors. Hence, while the precise behaviors selected as our outcome variables were specific to Ireland during the early stages of the pandemic, the method used to generate them and the experiment's findings are applicable to marginal behaviors in other places at other stages of the pandemic.

We hypothesized that the IP and TR posters would motivate social distancing by making people more cautious about and less accepting of marginal behaviors, relative to the control group, with differences also possible between the two treatment conditions. In line with best practice in reproducible science, we pre-registered formal hypotheses online (https://osf.io/r9hzs/).

3. Method

The study was conducted in line with institutional ethics policy. The sample ($N = 500$) was recruited by a market research agency from a national online panel. Recruitment ensured that broad socio-demographic subgroups were adequately represented. This approach provided a rapid experimental sample sufficient for relative comparison of outcomes across randomly assigned conditions rather than for absolute population-level measurement, which was not a feature of this study. The final sample was closely representative for gender, working status and urban-rural location, but somewhat older than the national population and with similarly lower educational attainment (Table 1) – characteristics that are strongly correlated among Irish adults. Participants were paid €5 for undertaking the 15-min study, programmed using Gorilla Experiment Builder (Anwyl-Irvine et al., 2020).

The experiment was embedded in a more general survey about COVID-19. All participants encountered the same questions before being randomized to see one of the three posters, to which they were asked to attend. Figs. 1–3 display the Control, IP and TR posters, respectively. The control poster had already been designed by the public health authorities and consisted of four panels depicting social distancing in four different social settings. The treatment posters also contained four panels, each with an image of people not maintaining social distance, with text-bubbles that foretold stories of chains of infection. (The design was inspired by a poster campaign by Baltimore City Health Department: https://health.baltimorecity.gov/sites/default/files/COVID-SocialDistancing.jpg). The IP poster depicted individuals who don't realize they have the virus, spreading it to an identifiable vulnerable person. The TR poster showed individuals unwittingly spreading the virus to multiple others. On each treatment poster, two messages leveraged counterfactuals (e.g. "if they had sat further apart, she would have been okay"), which help people identify causal relationships (Byrne, 2016). The other two partially left open the outcome to leverage implications the participant might infer (e.g. "he's asthmatic"), which facilitates memory (Brewer, 1977). A final line at the bottom of each poster summarized the overall message.

We did not equalize all aspects of the posters except message content, as one would in a study devoted solely to isolating a specific mechanism. This decision was made purposefully, in conjunction with policymakers, to maximize the research impact in the emergency response to COVID-19. The logic was as follows. Evidence was sought to improve concurrent public health messages, so the control condition needed to be a real-

Table 1
Participant socio-demographic characteristics.

		n	%	Population %[a]
Gender	Men	254	51	49
	Women	246	49	51
Age	Under 40 years	166	33	38
	40–59 years	154	31	36
	60 years +	180	36	26
Education	Degree or above	167	33	42
	Below degree	333	67	58
Employment	Employed	260	52	51
	Not employed	240	48	49
Urban-Rural	Urban	317	63	61
	Rural	183	37	39

[a] Source: Central Statistics Office (www.cso.ie).

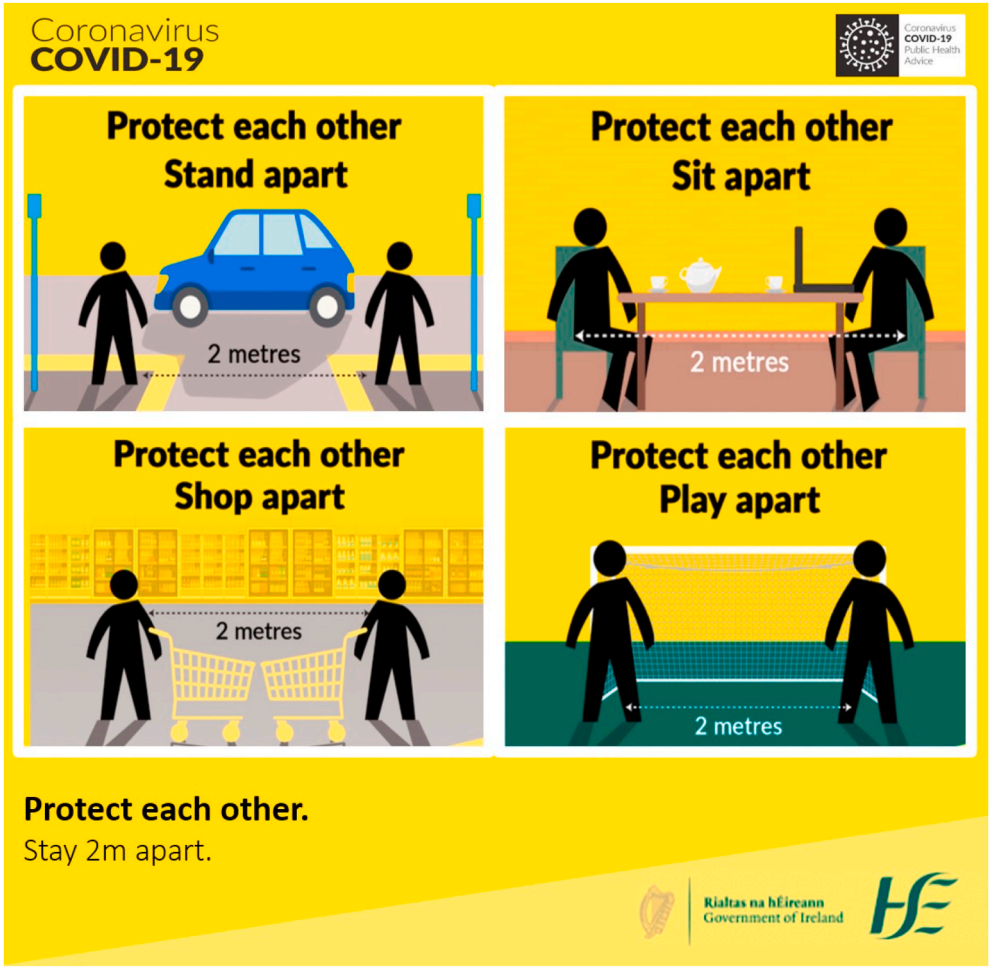

Fig. 1. Control poster.

world benchmark. The cartoon poster was in use and representative of multiple communications. However, using faceless, figurative cartoons for the treatment posters would undermine messages emphasizing impact on real people. Hence, we could not directly compare the desired treatment against the real-world control without introducing other changes. One possibility was a third treatment arm with the cartoon poster adapted to include real people, but this would have reduced statistical power substantially. From the policymakers' perspective, this strategy appeared wasteful, since it involved using a quarter of the sample to test a poster expected to be less effective than the current one. Such trade-offs between precise isolation of a potential mechanism and testing policy improvements are common when applying behavioral science to policy. While sometimes a pure mechanism experiment may be beneficial in the long run, there are also times when it is inexpedient (Ludwig et al., 2011). In this case, the decision was made to stick with two treatment arms, but to use rather bland, ordinary-looking stock photographs in the design. Thus, while it would remain possible that an observed effect was due to superficial differences, this was judged unlikely. Furthermore, by asking participants initially to rate the posters, superficial impact or attractiveness could be checked. In this way, statistical power focused on testing improvements to concurrent communications to support an emergency response.

After viewing the poster, participants were asked how effective and how memorable they thought it would be (on 7-point scales from "Not at all" to "Extremely"). They were also asked which of the four panels might be most persuasive. These questions were designed to measure participants' intuitions about the messaging, but also disguised the primary study aim, which centered on later responses. We refer to these initial responses as the "Evaluation" responses. In what was then signaled to be a separate stage, we asked participants about their plans "for the next few days". First, participants responded to three "Intentions" questions regarding marginal behaviors. They were asked to rate how likely they were to (i) visit a friend or relative in their home, (ii) meet up with friends or relatives in the open air, and (iii) go for a walk in their neighborhood (on 7-point scales from "Highly unlikely" to "Highly likely"). (Note that, all behaviors, including meeting others in the open air, were considered to be potentially risky at the time of the study.) The aim was to combine these responses into a score for how cautious participants intended to be. Next, they responded to three "Acceptability" questions about marginal behaviors others might undertake: whether it was okay to (i) travel by public transport, (ii) allow their children to play outside with friends, and (iii) travel to a parent's house for tea and a chat (on 7-point scales from "Definitely not okay" to "Definitely okay"). Responses were designed to be combined into scores for participants' caution about behavior generally. The precise question wordings for all main response variables are reproduced in Supplemental Material.

4. Results

Randomization was effective. Standard χ^2 comparisons for differences in assignment to condition (control, IP, TR) by gender, age, educational attainment, residential location, working status, and nationality were all non-significant ($ps > 0.20$).

The use of focus groups to identify "marginal behaviors" did produce variation in ratings, although responses to the rating scales were still highly skewed, with clustering at maximum and/or minimum values.

Fig. 2. Identifiable Person (IP) poster.

Given these response distributions, we initially combined and transformed the response scales into three categories (Low, Medium, and High) for analysis by ordinal logistic regression (OLR). Although this process was undertaken before any analysis by condition, one concern might be that the resulting *p*-values would depend on the category boundaries chosen. We therefore also report binary tests of proportion that test simply whether the treatment posters were associated with an increase in the proportion of participants who responded at the highest level of caution, which was the modal response. This test involves no selection of category boundaries. In Supplemental Material, we present multiple additional robustness checks.

Looking first at the Evaluation responses, participants judged all three posters to be effective and memorable, but thought the control poster would be more effective and more memorable than the two treatment posters. Fig. 4 displays this difference. Since the large majority of responses were 5 or above, lower responses are pooled for comparison. Participants were significantly more likely to give a maximum score to the control poster over the treatment posters (pooled, $p < 0.05$ for both scores) and to the control over IP poster ($p < 0.05$ for both scores), but the difference between the control and TR poster was short of significance ($p > 0.10$ for both scores). Across all three categories, OLR models show that the difference in perceived effectiveness between the control and the treatment posters was marginally statistically significant ($p = 0.06$, Model 1, Table 2), while that between the control and IP was significant ($p < 0.05$, Model 2, Table 2; control versus TR, $p > 0.20$). Differences in perceived memorability between the control and treatment posters were all significant (pooled, $p < 0.01$, Model 3, Table 2; control versus IP, $p < 0.01$, control versus TR, $p < 0.05$, Model 4, Table 2). Alternative categorizations of both variables give closely similar results (Table S1 in the Supplemental Material).

In line with the pre-registered plan, before assessing differences in the primary outcome variables across conditions, we examined the consistency of responses. Full correlation matrices for Intentions and Acceptability items are provided in Supplemental Material. Regarding the Intentions items, the visiting and meeting responses had a modest and highly significant correlation, but the walking response was not consistent with the other two. Therefore, we did not include this item in the score for Intentions, but instead analyzed it separately. The composite score for Intentions was the mean of responses to the remaining two items related to visiting and meeting friends and family (Cronbach's $\alpha = 0.51$). The correlations among Acceptability items were more consistent (public transport and children playing together, $r = 0.45$; public transport and tea with parent, $r = 0.20$; children playing together and tea with parent, $r = 0.34$; $p < 0.001$ in each case), so all three were averaged to make the score (Cronbach's $\alpha = 0.56$). For both scores, participants were categorized as expressing Low, Medium, and High caution, but the skew of the response distributions was such that the High caution category included only participants who assigned the most cautious scores possible ("definitely will not" for Intentions; "completely unacceptable" for Acceptability).

Both treatment conditions resulted in increased proportions of individuals expressing high caution (Fig. 5). For Intentions, 65% of participants in the treatment conditions (63% IP; 66% TR) assigned the most cautious scores possible, compared to 56% in the control condition.

Fig. 3. Transmission Rate (TR) poster.

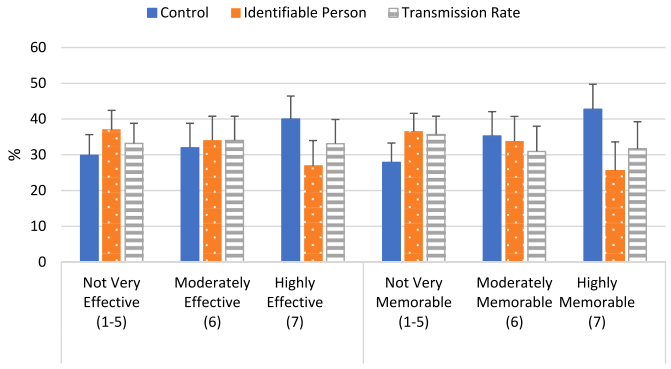

Fig. 4. Participant evaluations of posters. Participants rated how effective they thought the poster would be (left) and how memorable (right). Proportions of responses on the 1–7 Likert scales (in parentheses) are given for each of the three conditions (control, IP, and TR posters). Error bars are standard errors.

For Acceptability, the figures were 41% (41% IP; 41% TR) and 34%. Both differences between the treatment and control conditions are statistically significant: Intentions, $p < 0.05$ (control versus IP, $p < 0.10$; control versus TR, $p < 0.05$); Acceptability, $p < 0.05$ (control versus IP, $p < 0.10$; control versus TR, $p < 0.10$). Across all categories, the overall increase in caution in the treatment conditions (pooled) versus the control condition is borderline statistically significant for Intentions ($p = 0.05$, Model 5, Table 3) and significant for Acceptability ($p < 0.05$, Model 6, Table 3). Concerning Intentions, the effects for control versus IP and TR treatments are more marginal (IP, $p = 0.10$; TR, $p < 0.05$ for High versus Medium/Low caution, $p > 0.50$ for High/Medium versus Low caution, Model 7, Table 3). Concerning Acceptability the effects are clearer (control versus IP, $p < 0.05$; versus TR, $p < 0.10$, Model 8, Table 3). Robustness checks using alternative categorizations of the scores confirm the significance of these effects (Supplemental Material, Table S4). No differences between the two treatment conditions are statistically significant.

All effects survive controlling for gender, age, residential location, educational attainment, working status, and urban-rural location.

Table 2
Ordinal logistic regressions (OLR) for perceived effectiveness (Models 1 and 2) and memorability (Models 3 and 4) of posters (Low-Medium-High).

Poster	(1) Effectiveness (OLR)	(2) Effectiveness (OLR)	(3) Memorability (OLR)	(4) Memorability (OLR)
Treatment	−.336*		−.504***	
	(.176)		(.178)	
Identifiable Person		−.450**		−.561***
		(.217)		(.204)
Transmission Rate		−.223		−.447**
		(.203)		(.205)
Log likelihood	−538.963	−538.338	−518.906	−518.758
N	500	500	500	500

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

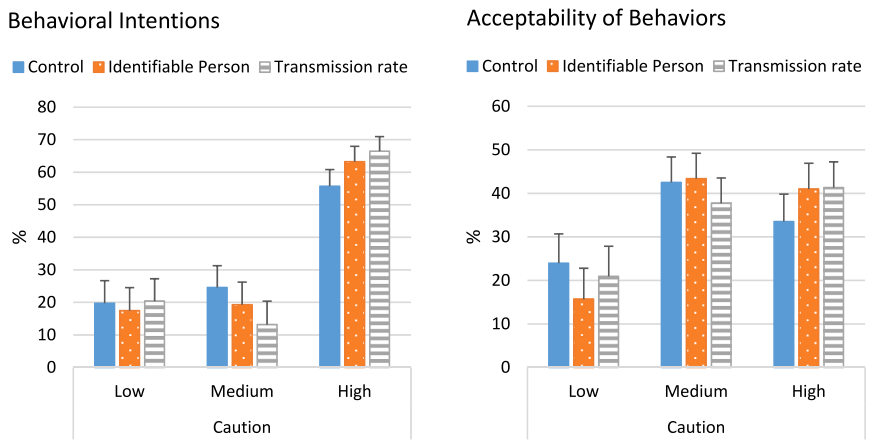

Fig. 5. Caution with respect to "marginal behaviors". Composite scores for behaviors participants intended to undertake and the acceptability of behaviors are categorized into Low, Medium and High levels of caution with respect to social distancing. Proportions in each category are given for each of the three conditions (control, IP, and TR posters). Error bars are standard errors.

Table 3
Ordinal logistic regressions for caution (Low-Medium-High) with respect to behavioral Intentions (Models 5 and 6) and Acceptability of behaviors (Models 7 and 8).

Poster	(5) Intentions (OLR)	(6) Intentions (GOLR)[a]		(7) Acceptability (OLR)	(8) Acceptability (OLR)
		L v M/H	L/M v H		
Treatment	.301*			.331**	
	(.186)			(.177)	
Identifiable Person		.276	.276		.380**
		(.217)	(.217)		(.204)
Transmission Rate		.019	.435**		.281*
		(.258)	(.224)		(.205)
Log likelihood	−463.607	−461.184		−526.180	−526.064
N	500	500		500	500

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.
[a] Generalized OLR fitted, because independent variable TR fails Brant test of proportional odds assumption ($p<0.05$; $p>0.05$ for other models).

Interactions of the effects with gender and age are not statistically significant. We did not pre-register any predictions regarding differences in responses by socio-demographic subgroup. An exploratory analysis (Supplemental Material, Table S5) finds that adults under the age of 40 were less cautious than older adults, especially in relation to their own behavioral intentions (Nivette et al., 2020). Some differences also emerged between urban and rural dwellers.

Differences in intentions to take a local walk displayed the same directional effects. In both treatment conditions, 37% of participants said that they would definitely take a local walk, versus 40% in the control condition, while 18% would definitely not take a walk, versus 11% in the control condition. Overall, the differences were borderline statistically significant (Supplemental Material).

One possibility is that different responses to the control and treatment posters were generated not by the content, but by the time spent viewing it, given the greater amount of text on the control posters. Median viewing times were longer for the treatment posters (53 s, versus 30 s for the control poster). However, viewing time was not significantly related to the outcome variables and when it was included as a control variable in the statistical models, the main effects persisted (Table S3 in the Supplemental Material).

5. Discussion

This experiment found that two communication strategies informed by behavioral science promoted greater caution about social distancing. Posters that emphasized how one person's behavior might lead to the infection of an identifiable, vulnerable person, or substantial numbers of other people, increased caution, as measured by stated intentions for behaviors and assessments of acceptable behaviors (Fig. 5).

We conclude that the study generates supportive evidence for communications that emphasize the impact of noncompliance on identifiable people and the potential to cause multiple infections. Our findings are supported by other recent evidence on messages that highlight risks to others and the immediate effects of present behavior (Jordan et al., 2020; Pfattheicher et al., 2020; Sobol et al., 2020). In simple terms, effective messages do not tell people only *what* to do to prevent being infected, but *why* it is vital for society that they avoid infection. The implications are not for messages only from public health authorities, but from all kinds of organizations and individuals seeking to promote social distancing.

5.1. Limitations

When evaluating the strength of this evidence for policy and practice, two factors stand out. First, the study measured intentions and attitudes about behavior, not behavior itself. Imperfect correlations between intentions, attitudes and actions mean there is no guarantee that the messages we found to be effective will alter behavior. Furthermore, while measurement via rating scales allowed rapid investigation of responses to public health messages, the resulting distributions of responses made measurement of clear effect sizes difficult. However, the evidence does imply that the treatment messages are better candidates for promoting good behavior, since they do drive intentions and attitudes in the right direction. Second, participants' judgments about the effectiveness of the posters were the opposite of

what we found, making some alternative interpretations of our data unlikely. Our results were clearly not due to the superficial attractiveness of the treatment posters. Moreover, the opposing effects suggest that responses are unlikely to be explained by participants trying to please the experimenters, or trying to be perceived as morally superior (Timmons et al., 2020). Rather, participants' relative dislike of messages that nevertheless increased their caution is consistent with the possibility that the posters made them feel uncomfortable about the possible consequences of their actions. This discrepancy between participants' intuitions and empirical observations matches other research on appeals to moral values (Everett et al., 2020).

6. Implications and conclusions

This pattern has a further policy implication. There are circumstances where testing campaigns via focus groups may backfire, perhaps especially where a message makes people feel uncomfortable or guilty. Individuals may want to believe that their behavior results from rational information processing, not emotions, despite evidence to the contrary (Lerner et al., 2015); they may favor pure information, when it is actually less effective.

Conducting rapid behavioral research during a crisis is challenging, particularly regarding the reliability and validity of outcome variables. Our strategy identified marginal behaviors and created a "caution" score from intentions and judgments. As the situation evolves, behaviors that might be considered marginal will change and have already done so. However, this approach mitigated ceiling effects and generated variation in measurable outcomes of interest. Despite the evolving nature of the COVID-19 pandemic, it is possible to use behavioral science to generate rapid evidence for policy and practice (Lunn et al., 2020).

Credit Author Statement

Peter D. Lunn: Conceptualisation, Methodology, Formal Analysis, Investigation, Resources, Data Curation, Writing – Original Draft, Writing – Review & Editing, Visualisation, Supervision, Project Administration, Funding Acquisition. Shane Timmons: Conceptualisation, Methodology, Investigation, Resources, Data Curation, Writing – Original Draft, Writing – Review & Editing, Visualisation. Cameron A. Belton: Methodology, Software, Investigation, Writing – Review & Editing. Martina Barjaková: Methodology, Investigation, Writing – Review & Editing. Hannah Julienne: Methodology, Investigation, Writing – Review & Editing. Ciarán Lavin: Methodology, Investigation, Writing – Review & Editing.

Acknowledgements

The authors thank the Department of Health, Ireland for support and members of the Behavioural Change Subgroup of the National Public Health Emergency Team (NPHET) for encouragement and feedback. We are grateful also to Alan Barrett and Helen Russell for comments on an initial draft.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.socscimed.2020.113478.

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