Vulnerability Appeals in the COVID-19 Pandemic: Insights from a National Survey Experiment

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Abstract

This study explores the impact of vulnerability appeals during the COVID-19 pandemic using a nationally representative, pre-registered survey experiment (N=4,087) conducted in mid-2021. We explore whether providing citizens with information about the vulnerability of ethnic minority and disabled citizens to COVID-19 fosters empathy and increased support for behavioural restrictions. We observe minimal statistically significant or substantive effects, although the presence of subtle effects cannot be entirely ruled out. We identify some limited indications that individuals with disabilities exhibit increased support for restrictions when exposed to information about the vulnerability of disabled people to COVID-19, but these effects are inconsistent. Therefore, our findings provide limited evidence to confirm or rule out that using vulnerability appeals alone is effective for influencing public attitudes toward behavioural restrictions. The findings point toward avenues for future research, including a closer examination of heterogeneous responses to public health messaging among population subgroups.

Keywords: COVID-19, disability, ethnicity, political psychology

1 Introduction

At the onset of the COVID-19 pandemic, governments worldwide rushed to implement new policy measures in an effort to curb the spread of the virus. These measures, ranging from stringent lockdowns and curfews to the closure of schools and businesses and mandatory use of personal protective equipment, resulted in profound disruption to the daily lives of citizens. In this critical period, policy-makers faced the urgent challenge of developing mass communication strategies that would effectively promote compliance and cooperation with disruptive public health measures.

Early in the pandemic it became clear that the health risks posed by COVID-19 varied dramatically among individuals with different health and demographic characteristics (Nasserie et al., 2021). Thus,

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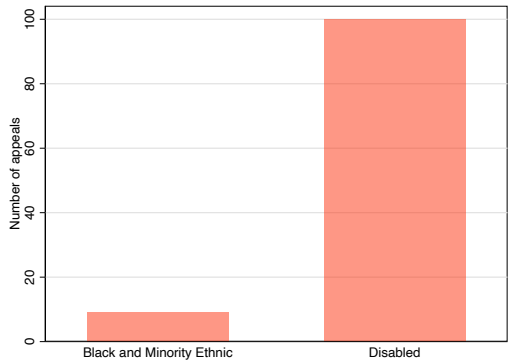

Figure 1: Number of appeals encouraging citizens to follow restrictions to protect black and minority ethnic and disabled people, made during Senedd plenary speeches and First Minister televised briefings between 1st March 2020 and 31st May 2021.

a common rhetorical strategy deployed by governments was to encourage compliance by underscoring the vulnerability of members of particular minority groups to severe illness and death from COVID-19. The logic of this strategy was straightforward: individuals should seek to stop the spread of COVID-19 to protect not only themselves, but those most vulnerable to severe health outcomes[1]. In the UK, two groups most at risk from COVID-19 were people with disabilities (PWD)[2], and people from black and ethnic minority backgrounds. Members of both these groups were significantly more likely to be hospitalised as a result of contracting the virus (Public Health Wales, 2022; Public Health England, 2021), more likely to be admitted to intensive care (Thomas et al., 2021; Office for National Statistics, 2022; Kavanagh et al., 2022), and more likely to die as a result of COVID-19 (Perera et al., 2020; Public Health England, 2020). In Wales, where this study was conducted, political elites routinely emphasised the vulnerability of these groups in public communications. As illustrated in Figure 1, a simple search of official parliamentary transcripts and governments speeches reveals that both these groups featured prominently in elite communications in the roughly fourteen months leading up to the fielding of this study. However, while such appeals were widespread, their effectiveness at motivating compliance with COVID-19 restrictions remains uncertain.

Addressing this gap, we ask: to what extent does priming the vulnerability of certain minority groups to COVID-19 promote empathy and prosocial behavior? We address this question using a nationally representative survey experiment embedded in the Welsh Election Study (N=4,087), fielded by YouGov in May 2021. Specifically, we examine whether appeals which underscore the vulnerability of disabled and minority ethnic citizens to COVID-19 promote affective concern and support for virus-curbing restrictions. Our findings suggest such appeals have, at best, a limited and small effect on attitudes when used in isolation. We find no evidence that vulnerability appeals substantially increase affective concern or support for behavioural restrictions, and we find some evidence of a backlash effect (diminished affective concern) in response to messages that cue both ethnic minorities

[1]For example, in Wales a major campaign slogan throughout the pandemic was "Protect yourself and others from coronavirus". See https://www.gov.wales/protect-yourself-others-coronavirus

[2]As Reher (2021) notes, both "disabled people" and "people with disabilities" are widely considered acceptable terminology within the disability community, though their use varies with cultural context and individual preference. We affirm the plurality of opinion within the disability community, and use both terms interchangeably throughout this paper.

and disabled people. By contrast, we find some suggestive evidence that when exposed to information about the vulnerability of disabled people in particular to COVID-19, disabled respondents report stronger support for some behavioural restrictions (we observe no statistically significant change in affective concern) than their non-disabled peers. Finally, we discuss potential limitations of our design and offer suggestions for future research.

2 The Effectiveness of COVID-19 Messaging

Existing research examining the effects of public health messaging throughout the pandemic has yielded mixed results. Messaging that emphasised the public health benefits of pandemic related restrictions appears to have been effective in some contexts, with positive effects found in the US for intention to wear masks and perceived effectiveness of masks (Carey et al., 2022), and intention to travel less (Deslatte, 2020). Furthermore, Adida et al. (2023) highlighted the substantive impact of social norms in increasing support for mask-wearing among white American Evangelicals. However, the weight of experimental survey research on the effect of public health messaging has returned null findings on a range of attitudes and intentions (e.g. Case et al., 2022; Utych, 2021; Kuipers et al., 2021). Economic frames also appear largely ineffective, either producing null effects (Deslatte, 2020), unintended reductions in support for restrictions (Carreras et al., 2021), or have failed to replicate (Knapp et al., 2023). Similarly, Favero and Pedersen (2020) found that various prosocial frames had no effect on either respondents' willingness to engage in social distancing behaviors, or their beliefs about the virus. Nevertheless, significant gaps remain in our understanding of what kinds of messages were successful in motivating compliance with COVID-19 restrictions. In particular, while messages which underscored the vulnerability of minority groups (e.g. people with disabilities, ethnic minorities) were commonplace throughout the pandemic, we do not yet know whether such messages were successful in motivating empathic concern or a willingness to abide by COVID-19 restrictions.

2.1. Stereotypes, Group Identity, and Vulnerability Appeals

Research in cognitive psychology suggests the effectiveness of vulnerability appeals is likely to vary based on the content of common group stereotypes (Fiske et al., 2002; Fiske, 2015). In the absence of specific information, people tend to rely on cognitively accessible stereotypes when forming judgments about the behavior and intentions of others (Allport et al., 1954; Fiske and Taylor, 1991). Thus, we suspect that when confronted with appeals that cue broad social categories - in our case, "people with disabilities" (PWD) and "black and ethnic minority people" - citizens are likely to respond in ways which reflect the content of common group stereotypes.

Existing work repeatedly finds that across diverse social and political contexts, PWD are stereotyped as high in "warmth" and low in "competence" (Fiske et al., 2002, 2007). In other words, while PWD are seen as friendly and agreeable, they are stigmatised as dependent and low in social status (Canton et al., 2023; Fiske et al., 2002; Nario-Redmond, 2010). Existing research has examined a range of group labels, including "disabled", "physically disabled", "blind", and "mentally retarded" [3], and

[3]Labels such as "mentally retarded" are now widely considered to be derogatory and are no longer used to refer to

find that all are similarly characterised as high in warmth and low in competence (Canton et al., 2023; Fiske et al., 2002). Importantly, these stereotypes are strongly associated with help-giving emotions, such as pity, compassion, and paternalistic concern (Goetz et al., 2010; Cuddy et al., 2007). These stereotypes are reflected in the welfare attitudes literature, in which PWD are reliably identified as among the most deserving recipients of government assistance (Coughlin, 1980; Van Oorschot, 2000, 2006).

By contrast, stereotypes assigned to ethnic minorities are more variable. On the one hand, ethnic minorities perceived as poor or migrants are often stereotyped as low in both warmth and competence (Fiske et al., 2002; Fiske, 2015), which can elicit feelings of contempt and a reluctance to extend assistance (Cuddy et al., 2007; Petersen, 2012). Similar stereotypes have been applied to more general ethnic minority cues - such as "blacks" and "Hispanics" - though these groups tend to be middling in both warmth and competence (Cuddy et al., 2009). Other more specific minority groups - such as British Indians or Asian Americans - are stereotyped as low in warmth but high in competence (Fiske, 2018). These stereotypes tend to elicit feelings of envy and intergroup threat (Cuddy et al., 2007). Importantly, the stereotypes assigned to ethnic minorities are consistently lower in warmth than those assigned to PWD, making them relatively less-likely to elicit help-giving emotions and behaviors (Fiske et al., 2002; Cuddy et al., 2007). Based on these findings, we expect respondents to report relatively more affective concern and support for behavioural restrictions in response to messages which underscore the vulnerability of disabled people as opposed to ethnic minorities.

Group stereotypes may also shape the perceived plausibility of vulnerability information. Unlike ethnicity, disability is defined by limitations in functioning resulting in part from physical or mental impairments, which can have downstream implications for other health outcomes (Krahn et al., 2015; Mitra et al., 2022; Nario-Redmond, 2019). Consequently, disability is often psychologically conflated with sickness and disease by external observers (Park et al., 2003), and individuals with specific impairments are often assumed to experience more global limitations in functioning (Nario-Redmond, 2010, 2019). Given the close conceptual association between disability and health, we may expect respondents to perceive disability status to be more directly or intrinsically related to health outcomes than other embodied characteristics, such as ethnicity. Thus, respondents may be relatively more likely to perceive information about the health vulnerability of disabled people to be convincing or plausible than appeals which emphasise the vulnerability of ethnic minority groups. These findings inform H1-H5 of our preregistered hypotheses (below). We predict that appeals which emphasise only the vulnerability of PWD will elicit more empathic concern and support for behavioural restrictions than appeals which emphasize the vulnerability of ethnic minorities, or appeals which feature both ethnic minorities and PWD.

Finally, we expect that affective concern and support for restrictions will be stronger on average among (1) disabled respondents, and (2) respondents who have close relatives with disabilities. Intuitively, disabled respondents and their close relations may report stronger support for restrictions out of an instrumental desire to mitigate the risk of severe illness to themselves and their kin. On the other hand, recent work in social psychology suggests many PWD self-consciously identify as

intellectual disability. Furthermore, it is important to note that we do not endorse such stereotypes as accurate representations of disabled people. Rather, we argue that given the prevalence of these stereotypes, they are theoretically likely to influence the interpretation of elite messaging.

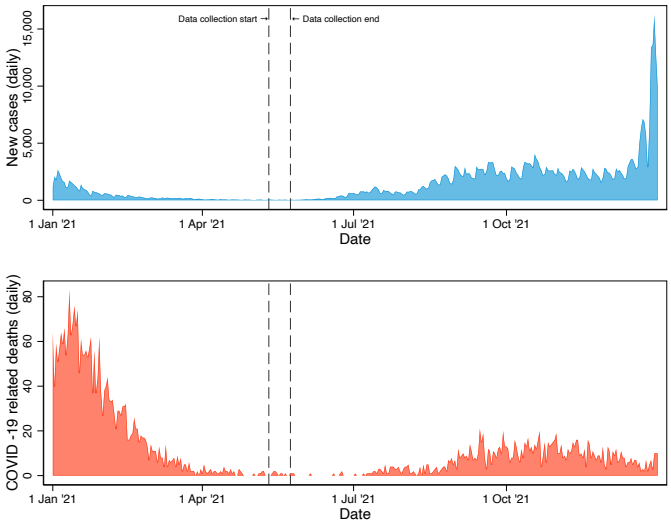

Figure 2: Daily cases (people who have had at least one positive COVID-19 test result) and deaths in Wales in 2021. (Public Health Wales, 2022)

members of a stigmatised minority group (Bogart, 2014; Nario-Redmond and Oleson, 2016), and report feelings of social and political solidarity with other PWD (Bogart et al., 2018; Dirth and Branscombe, 2019; Nario-Redmond et al., 2013). Thus, we expect that information about the vulnerability of PWD to COVID-19 will inspire feelings of group-level political solidarity, resulting in greater affective concern and support for restrictions on average among PWD and their close relations (H6)[4].

3 Experimental Design

This pre-registered experiment was embedded in a larger election study that was conducted online by YouGov between 11th May and 27th May 2021 (Wyn Jones et al., 2023). This was a time when COVID-19 cases and related deaths were at a relative low-point in Wales (see Figure 2), but a majority of government restrictions were still in place (see supplementary materials for detailed timeline). A total of 4,087 respondents were recruited from YouGov's online panel of over 1 million British adults with a sampling frame to approximate the demographic composition of the Welsh population. Additional post-stratification weights are applied to all analyses so that model estimates can be interpreted as nationally representative.[5]

The post-election survey took approximately 15 minutes to complete, with the experiment placed at the end of the questionnaire. The full questionnaire is included in the supplementary materials. Respondents were randomly assigned to one of four experimental conditions - a control condition, a disability condition, an ethnic minority condition, and a combined condition. In each condition

[4]We may expect to see similar forms of political solidarity among minority ethnic respondents in response to appeals which highlight the vulnerability of ethnic minority groups. Our sample is too ethnically homogenous to test this expectation (See Table 1 in Supplementary Materials), which should be taken up in future research

[5]Unweighted analyses yield highly similar and substantively identical results.

respondents were provided with the same factual information about the COVID-19 pandemic including the number of UK residents who had been hospitalised or died as a result of the COVID-19 pandemic. In the three treatment conditions this information was followed by a statement about how different groups – disabled people, black and minority ethnic people, and disabled *and* black and minority ethnic people – were particularly at-risk of severe illness and death. The full wording is provided in Table 1.

Assignment	Vignette Wording
Control	The COVID-19 pandemic has had a major impact on UK society. Over the course of the pandemic, approximately 450,000 Britons have been hospitalised, and over 120,000 have died.
Disability treatment	*[As above +]* Disabled people have been particularly seriously affected, with research showing that this group is significantly more likely to be seriously ill and die from COVID-19.
Ethnic minority treatment	*[As above +]* Black and minority ethnic people have been particularly seriously affected, with research showing that this group is significantly more likely to be seriously ill and die from COVID-19.
Combined treatment	*[As above +]* Disabled people and black and minority ethnic people have been particularly seriously affected, with research showing that these groups are significantly more likely to be seriously ill and die from COVID-19.

Table 1: **Experimental manipulation**

3.1. Outcome measures

After reading the treatment text, respondents were asked the extent they agreed with six different statements on a five-point scale (1-5, strongly disagree - strongly agree). The order of these statements was randomised. Three statements measured respondents' attitudes toward behavioural restrictions:

- "It should be compulsory for people to wear masks in places where lots of people are gathered (e.g. supermarkets, shopping malls, public transport)"
- "It is worth temporarily sacrificing some of our personal freedoms if it means protecting those who are most vulnerable to COVID-19"
- "There should not be another lockdown if cases of COVID-19 begin to increase again"

Three further statements measured affective responses towards those most vulnerable to COVID-19:

- "I am very concerned about those most vulnerable to COVID-19"
- "I feel anger towards people who refuse to take action to protect those most vulnerable to COVID-19"
- "I am quite moved by what can happen to those most vulnerable to COVID-19"

3.2. *Hypotheses*

We pre-registered the following hypotheses:

- **H1:** The disability condition will elicit greater anger and empathic concern than the control, ethnic minority, or mixed conditions.
- **H2:** The disability condition will elicit more prosocial action than the control, ethnic minority, or mixed conditions.
- **H3:** The mixed condition will elicit greater anger and empathic concern than the ethnic minority or control conditions.
- **H4:** The mixed condition will elicit more prosocial action than the ethnic minority or control conditions.
- **H5:** The ethnic minority condition will not elicit a significantly greater degree of anger, empathic concern, or prosocial action than the control condition.
- **H6:** Disabled people and proximate relations of disabled people will exhibit greater empathic concern, anger, and prosocial action in response to the disability condition than non-disabled respondents, and respondents without disabled proximate relations.

4 Empirical Strategy

The analysis plan for this experiment was preregistered at AsPredicted: aspredicted.org/blind. We do not deviate from the preregistration plan. To test hypotheses 1-5, we estimate the Average Treatment Effect (ATE) of exposure to the manipulations outlined above using OLS:

$$Y_i = \alpha + \beta_1 \text{Treat}_i + \epsilon \tag{1}$$

Where Y_i is respondent *i*'s attitude toward one of the six outcome variables listed above, and *Treat* is a categorical variable for treatment group. To test hypothesis 6 we compute the conditional average treatment effect (CATE) - that is, the effect of exposure to the treatment interacted with disability status, or relational proximity to PWD (in separate models). Power calculations for the estimation of the CATE are provided in Tables 7 and 8 of the supplementary materials. CATEs are computed using OLS:

$$Y_i = \alpha + \beta_1 \text{Treat}_i + \beta_2 \text{Disability} + \beta_3 (\text{Treat} * \text{Disability}) + \epsilon \tag{2}$$

In addition to calculating the ATE and CATE for each individual outcome variable, we also create two indices; the first comprised of the three pro-social behaviour items, and the second comprised of the three affective items.[6] We run the same analyses outlined above using these as outcome measures. This strategy was not included in the study's preregistration. We conduct significance tests using $p < 0.01$ thresholds in addition to $p < 0.05$ and report both confidence intervals for treatment effect estimates in all figures. To assess the precision of any null results we observe for main effects, we report equivalence bounds using a two one-sided tests approach (Lakens et al., 2018). All models include robust standard errors and sampling weights.

[6] See Supplementary Materials Section D for more information.

5 Results

Results of the models testing our first five hypotheses are presented in Figure 3. All outcome variables are standardised between 0-1 for ease of interpretation. Our first two hypotheses predicted that treatment effects would be greatest in the disability treatment across affective and prosocial behaviour outcomes. Contrary to expectations, we do not observe any effects significant at the 95% level in any of our outcome measures including both indices. For example, the disability treatment yielded an average treatment effect of $\beta = -0.010$ (p=0.485) on the compulsory masks outcome, and an effect of $\beta = -0.011$ (p=0.427) on the anger outcome. Of course, failure to reject the null is not the same as confirming the null. Here we estimate equivalence bounds using two one-sided t-tests.[7] In only one of the outcome variables - Sacrifice Freedoms - do the 90% confidence intervals fall fully within the estimated equivalence bounds meaning that we can reject the alternative hypothesis. For all other outcome variables, 90% confidence intervals fall outside of the equivalence bounds, and we therefore fail to reject the alternative hypothesis.

In hypotheses 3 and 4, we predicted that the combined treatment (disability + ethnic minority) would yield greater prosocial behaviour and empathic concern compared to the control and the ethnic minority treatment. We observe no support for these expectations. In fact, contrary to our expectations, we observe a statistically significant *negative* effect in four of our outcome variables: three at the 95% threshold (Masks compulsory $\beta = -0.037$, $p = 0.017$, Anger $\beta = -0.033$, $p = 0.035$, and the prosocial index $\beta = -0.028$, $p = 0.029$) and three at the 99% threshold (Concerned $\beta = -0.039$, $p = 0.004$, Moved $\beta = -0.038$, $p = 0.007$ and the affective index $\beta = -0.037$, $p = 0.003$). No statistically significant effects are observed for 'Sacrifice freedoms' or 'No more lockdowns', but in both cases 90% confidence intervals fall beyond equivalence bounds ([-0.0239, 0.0239] and [-0.09, 0.09] respectively) meaning that we cannot confidently rule out the presence of a small effect. Relative to the ethnic minority condition, a statistically significant difference is only observed in the 'Moved' outcome, but this is a substantively small difference. In all but one outcome - Anger - equivalence tests fail to reject the alternative hypothesis (see Table 12 of the supplementary materials).

Finally in hypothesis 5, we predicted that the ethnic minority treatment would not yield statistically significant results from the control condition in any outcome variables. While this is confirmed in four outcome variables, again contrary to our expectations we observe negative treatment effects at the 95% threshold in two outcomes: Concerned ($\beta = -0.029$, $p = 0.026$ and Anger $\beta = -0.031$, $p = 0.040$). Here, all equivalence tests fail to reject the alternative hypothesis, meaning we cannot confidently rule out a substantively small difference between these conditions.

5.1. Interactions (H6)

In hypothesis 6 we predicted that we would observe treatment heterogeneity in the disability condition among respondents who are either themselves disabled, or who have close kin with disabilities. In our sample 29.15% (N=1,176) respondents said they had a disability and 17.87% (N=721) of respondents said

[7] See Section F in Supplementary Materials for more information.

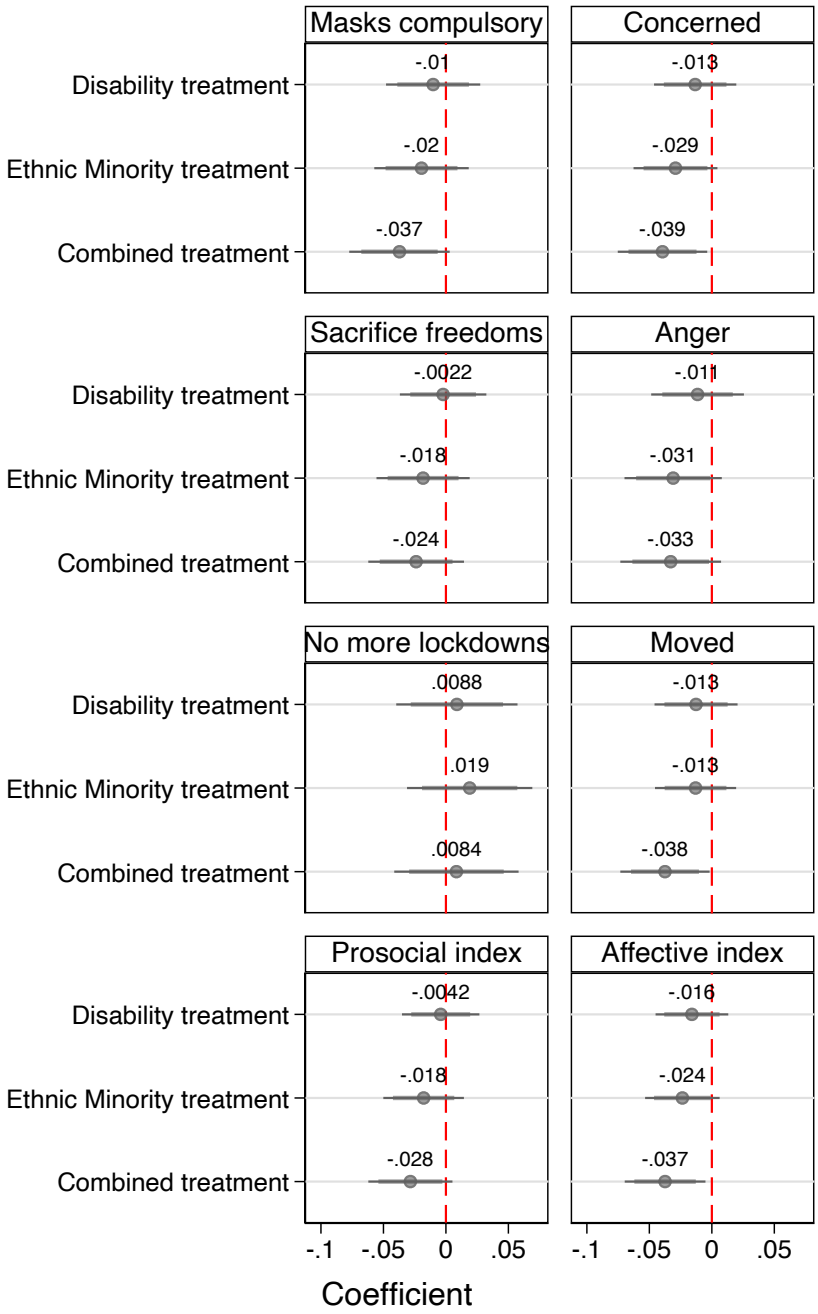

Figure 3: **Effect of vulnerability treatments on outcome measures.** Shown with 95% and 99% confidence intervals. Regression tables reported in Section E of the supplementary materials.

they were close to someone with a disability[8]. Figure 4 displays the results from our test of Hypothesis

[8]We measure disability status with a single binary item taken from the European Social Survey (ESS): "Are you hampered in your daily life activities in any way by any long standing illness or disability, infirmity, or mental health problem?" (yes/no).

6a, interacting a respondent's disability status with the disability treatment. Here, we predicted that PWD would be particularly sensitive to the disability treatment, and as a result we would observe larger effects within this group of respondents. Evidence supporting this hypothesis is mixed across outcome variables. For two of our measures of prosocial behaviour – agreeing masks should be compulsory in public places and willingness to sacrifice personal freedoms to protect others – we observe positive effects significant at the 99% threshold ($\beta = 0.10$, $p = 0.004$ and $\beta = 0.089$, $p = 0.010$ respectively).

Non-disabled respondents did not respond in a similar manner to the Disability treatment: we observe no substantive or statistically significant effects. In other words, there is some evidence that disabled respondents appear to be more sensitive to the disability treatment than their non-disabled peers, though the absence of any statistically significant effect in any of our affective measures does highlight the inconsistency of this finding. Likewise our test of Hypothesis 6b – where we interacted treatment condition with an indicator for relational proximity to PWD – produced no statistically significant effects. Results are not reported in the main body of the text for for conciseness, but complete results are available in Table 8 of the supplementary materials. In addition to these interactions, we pre-registered an interaction between respondent ideology and the treatments, though we did not pre-register any related hypotheses. Again, we observed no substantive or statistically significant effects.[9]

6 Discussion

Throughout the COVID-19 pandemic, governments aimed to motivate compliance with behavioural restrictions using messages which emphasised the vulnerability of particular minority groups to severe illness and death. Using a large, pre-registered survey experiment embedded in the 2021 Welsh Election Study, this paper has sought to evaluate the effectiveness of these kinds of appeals. We found very little support for the majority of our preregistered hypotheses. Contrary to expectations, none of our manipulations produced substantial increases in either affective concern or support for behavioural restrictions, and attitudes did not significantly vary in response to different group cues. Importantly, the results of our equivalence tests suggest we cannot rule out the presence of a substantively small effect. However, given the negative direction many of the results presented in Figure 3, in most cases these results still run contrary to our theoretical expectations.

We also found some limited, but nonetheless significant, evidence of a backlash effect, in that both affective concern and support for restrictions were significantly *diminished* in response to the combined treatment. While the exact reasons for this response are unclear, it is possible that some respondents interpreted the treatment as trying to equate the experience of social groups with different characteristics and different levels of vulnerability to COVID-19. This may have reduced the plausibility of the treatment and led some respondents to react in a contrary or hostile manner. Finally, the results of our interaction models provide some evidence that appeals which targeted PWD led to increased support for behavioural restrictions among disabled respondents. More work needs

We measure relational proximity to disability with the following item: "Are the you the family member or partner of a person with a disability, or do you currently care regularly for a person with a disability?" (yes/no/prefer not to say).

[9] See Section G of Supplementary Materials.

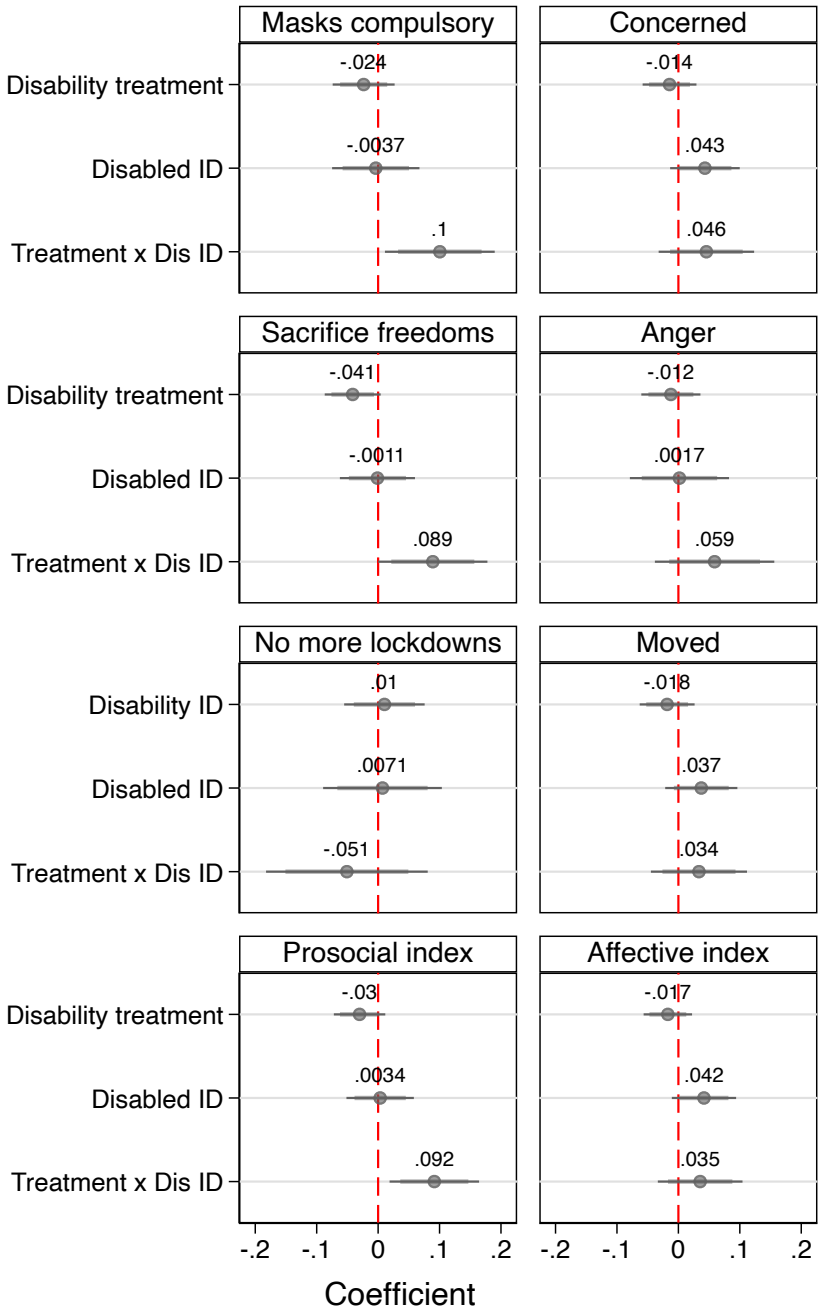

Figure 4: **Treatment interacted with Disability.** Shown with 95% and 99% confidence intervals. Regression tables reported in Section E of the supplementary materials.

to be done to clarify the psychological mechanisms underlying these results. Given that we observe no significant change in affective concern among PWD in response to the disability condition, we think it is probably more likely that this result is driven by a rational desire among PWD to mitigate their

own risk of severe adverse health outcomes, rather than a broader sense of subjective identification with PWD[10]. On the other hand, given the inconsistency of these results, we caution against drawing strong conclusions about the effects of vulnerability appeals on minority group members.

These results also have some important limitations. Firstly, we cannot rule out the possibility that our results are impacted to some extent by inattentive responding, as our design did not include a manipulation check and the treatment itself was relatively brief. Secondly, our results may have been impacted to some extent by the timing of our study. At the time of fielding, the pandemic was in its second year, daily new cases and COVID-related deaths were at a low ebb, and the Welsh Senedd had recently voted to relax a wide array of behavioural restrictions[11]. Plausibly, respondents may have found our treatments more affectively compelling if the virus were perceived as posing a more urgent existential threat. On the other hand, by 2021 there was a much greater volume of factual information about the vulnerability of social minorities to COVID-19, and this information was widely disseminated by political elites. We may not have observed substantively different responses to our treatments if the study were fielded earlier in the pandemic, when the threat posed by COVID-19 to social minorities was less clear.

It is also important to consider how our results may have varied in response to a stronger or more detailed treatment. For example, our treatments did not include statistical information describing the specific rates of severe illness and death experienced by various minority groups. We omitted these data because the groups featured in our treatments had very different rates of severe illness and death from COVID-19 by May 2021, and including accurate information of this kind would have made it difficult to draw accurate inferences about the role of group cues in particular. Nevertheless, in the absence of this kind of evidence, respondents may have been more likely to dismiss the treatments as hyperbolic or politicised, or feel they lacked the information necessary to respond in a manner commensurate to the scale of the problem. Similarly, our treatments did not include behavioural recommendations, and we did not frame our treatments as coming from a politician or government office. Both of these characteristics may plausibly have increased the strength or persuasiveness of our treatments. Finally, it is also the case that these vulnerability appeals were rarely used in isolation, and instead were frequently communicated alongside other information. Our study does not allow for us to measure the potential additive effect of these appeals when accompanied by additional information. Future research should seek to address these possibilities by (1) varying the type and volume of supporting evidence provided in vulnerability appeals, (2) including clear behavioural recommendations, and (3) varying the institutional or political source of vulnerability appeals.

Understanding how messaging shapes compliance with public health restrictions is important, both for our retrospective understanding of political behavior during moments of social upheaval and threat, and to help inform effective health policy interventions during future public health emergencies. These challenges are particularly urgent in demographically diverse societies, where messages targeted at specific groups risk alienating out-groups and producing unintended behavioural consequences. Our results indicate that while vulnerability appeals were unsuccessful in promoting affective concern or prosocial action in the full sample, there is some suggestive evidence that the

[10] For a discussion of the relationship between subjective identification and emotional reactivity to group threats, see Huddy (2013).

[11] A more detailed description of the status of COVID-19 in Wales at the time of fielding is available in the online appendix.

effect such appeals may vary to some extent among respondents with different health characteristics. Future work should seek to clarify how vulnerability appeals might be more effectively crafted and targeted to promote positive behavioural change in the mass public, and among those most at risk from public health emergencies.

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Data Availability The data, replication instructions, and codebook to replicate all analyses in this article are available at the Journal of Experimental Political Science Dataverse within the Harvard Dataverse Network, at: doi:10.7910/DVN/TLXOTB (Larner and Thorp, 2024). This research was supported by grants from the Economic and Social Research Council (Award number: ES/V009559/1)."

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Ethics Statement This research complies with all relevant ethical regulations and with APSA's Principles and Guidance for Human Subjects Research. The study was approved by Cardiff University's School Research Ethics Committee (SREC-280421-01). Informed consent was obtained from all participants. YouGov compensates participants with reward points that can be redeemed for cash.

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Supplementary Materials for Vulnerability Appeals in the COVID-19 Pandemic: Insights from a National Survey Experiment

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A Survey information

The survey experiment was embedded in wave 3 of the 2021 Welsh Election Study (Wyn Jones et al., 2023). It was carried out online by YouGov. It had a completion rate of 91.4%, with 4,087 respondents completing the questionnaire (average completion time was 14 minutes 37 seconds). Summary demographic descriptives of the survey sample are provided below in Table 1 Full information on the survey is available via the UK Data Service (Study Number: 9063).

	N	%		N	%
Gender			**Ethnicity**		
Male	1,818	44.5	Welsh/Scot./N.Irish/Eng.	3,892	95.2
Female	2,269	55.5	Irish	12	0.3
Age group			Gypsy or Irish Traveller	1	0.0
18-29	505	12.6	Any other White background	83	2.0
30-39	498	12.4	White and Black Caribbean	5	0.1
40-49	627	15.6	White and Black African	3	0.1
50-59	759	18.9	White and Asian	15	0.4
60-69	972	24.2	Any other multiple ethnic bground	9	0.2
70+	656	16.3	Indian	9	0.2
Highest Educational Qual.			Pakistani	4	0.1
None	229	5.7	Bangladeshi	1	0.0
Other	126	3.2	Chinese	4	0.1
GCSE or equiv (High School 16)	706	17.7	Any other Asian background	8	0.2
A Level or equiv (High School 18)	792	19.9	African	5	0.1
Higher below degree	346	8.7	Caribbean	3	0.1
Degree	1,215	30.5	Black/African/Caribbean	1	0.0
Don't Know	132	3.3	Arab	2	0.1
Other technical things	444	11.1	Any other ethnic group	3	0.1
Income - gross household			Prefer not to say	26	0.6
Under £15,000 per year	619	15.1	Skipped	1	0.0
£15,000 to £24,999 per year	690	16.9	**Disabled**		
£25,000 to £39,999 per year	787	19.3	Yes	916	28.9
£40,000 to £59,999 per year	565	13.8	No	2,194	69.3
£60,000+	405	9.9	Skipped	53	1.7
Don't know	271	6.6			
Prefer not to answer	744	18.2			
Skipped	6	0.1			
Geography					
Urban	2,605	63.7			
Town and Fringe	628	15.4			
Rural	851	20.8			
Uncoded	3	0.1			

Table 1: Summary demographic descriptive statistics of survey sample

B Wales as a case

The response to the COVID-19 crisis in the UK is unique in that it was/is not coordinated or led by the central UK government. As a public health crisis, responsibility for formulating a response lay with the devolved governments of the UK. Whilst the first initial few months were characterised by a largely coordinated and 'joined-up' approach to the crisis, divergence in response increased throughout 2020. Table 2 illustrates the policy response of the Welsh Government.

Table 2: Timeline of COVID restrictions in Wales

28/02/2020	First confirmed case.
23/03/2020	Schools close.
23/03/2020	Cafes, pubs, & restaurants close.
26/03/2020	Lockdown begins.
08/05/2020	Removal of once-a-day exercise limit.
09/06/2020	Masks recommended for public transport.
29/06/2020	All schools reopen.
23/10/2020	'Firebreak' lockdown begins.
9/11/2020	Firebreak ends.
20/12/2020	Wales enters full lockdown.
22/02/2021	Return to school announced for 3-7 year olds.
24/04/2021	Any six people can meet outdoors.
03/05/2021	Gyms and leisure centres can reopen.
17/05/2021	Indoor hospitality reopens and international travel resumes.
24/05/2021	Data collection ends.

More substantial divergence took place after the period outlined in Table 2, but as we only have data covering the period of May 2021, we focus on divergence that occurred until this point.The main points of divergence at this point were the timing of when schools should reopen, when face masks were recommended or made compulsory, and the social distancing rules. That said, policy divergence up until this point was limited only to differences in timing, not substantive differences in restrictions (though these followed later).

At this point in the pandemic, health outcomes in Wales were very similar to those across the other constituent countries of the UK: England, Scotland, and Northern Ireland. Public attitudes towards restrictions and government performance had began to diverge more substantially however at this point. Public opinion in Wales and Scotland remained significantly more supportive of

continuing restrictions and measures like mandatory face-masks in public spaces (see, for example, Ibbetson (2022)). Satisfaction with Welsh Government performance throughout the pandemic was also substantially higher than evaluations of the UK Government's performance (Larner et al., 2022).

C Distribution of dependent variable in control condition

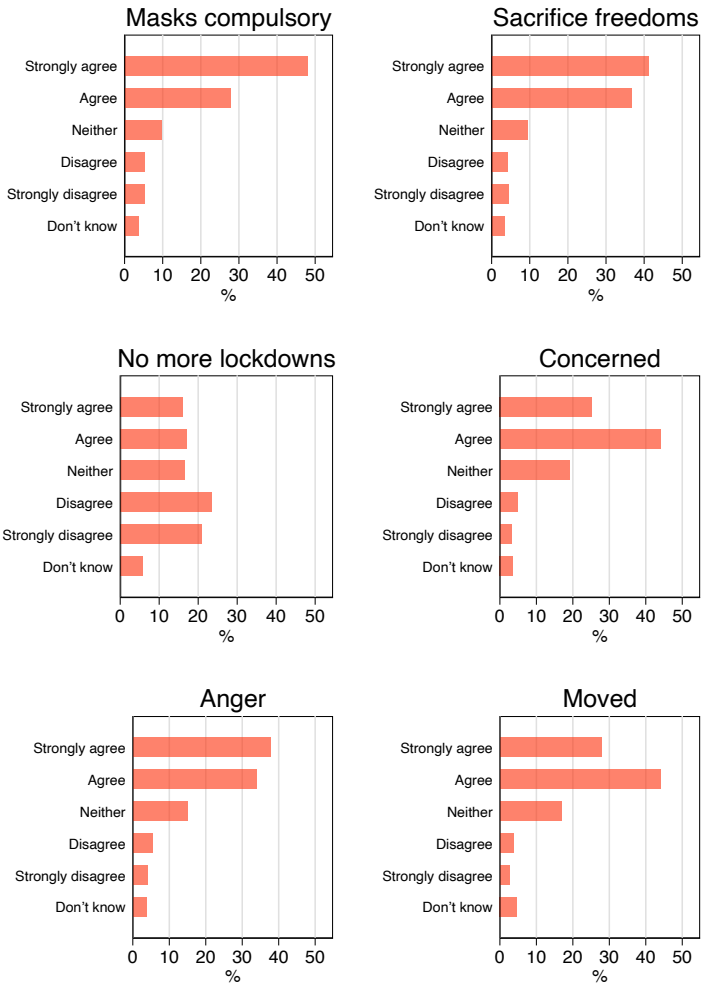

Figure 1: Distribution of responses on outcome variable in control group

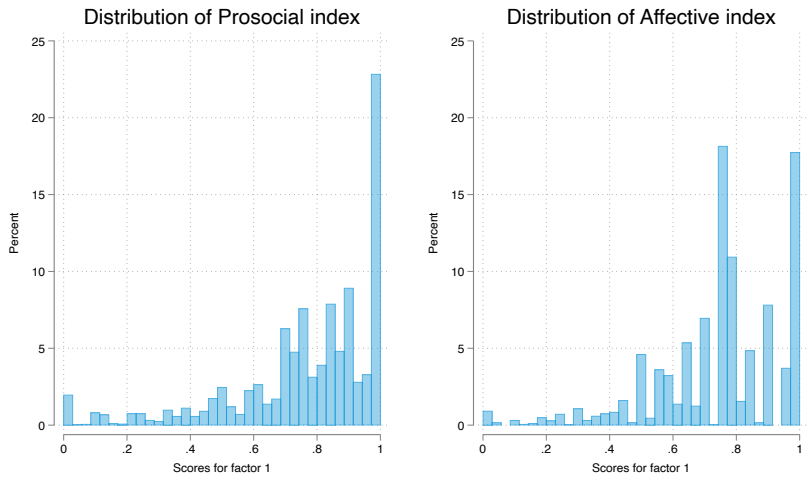

Figure 2: **Distribution of indicies**

D Collapsing outcome measures into indices

In addition to examining the effects of the treatment on each of the outcomes individually, we construct two indices: one constructed from the three items which were designed to capture willingness (or unwillingness) to engage in prosocial behaviour – Masks compulsory, Sacrifice freedoms, and No more lockdowns – and one constructed from the three items capturing affective response – Concerned, Anger, and Moved. This step of our analysis was not pre-registered. Factor loadings for the the prosocial index are as follows; Masks Compulsory = 0.7192, Sacrifice Freedoms = 0.7311, and No more lockdowns = 0.3249. Loadings for the affective index are: Concerned = 0.7974, Anger = 0.6267, Moved = 0.7797. The distribution of both indices is displayed in Figure 2, with correlations presented in Tables 3 and 4.

	Masks Compulsory	Sacrifice freedoms	No more lockdowns
Masks Compulsory	1		
Sacrifice freedoms	0.6356	1	
No more lockdowns	0.2276	0.2579	1

Table 3: **Prosocial behaviour items correlation table**

	Concerned	Anger	Moved
Concerned	1		
Anger	0.5358	1	
Moved	0.6987	0.5084	1

Table 4: **Affective items correlation table**

E Main Effects Tables (Test H1-5)

	Masks Compulsory	Sacrifice Freedoms	No lockdowns	Concerned	Anger	Moved
Control	-	-	-	-	-	-
Disability treatment	-0.010	-0.002	0.009	-0.013	-0.011	-0.013
	(0.015)	(0.013)	(0.019)	(0.013)	(0.014)	(0.013)
Ethnic Min treatment	-0.020	-0.018	0.019	-0.029**	-0.031**	-0.013
	(0.015)	(0.014)	(0.019)	(0.013)	(0.015)	(0.013)
Combined treatment	-0.037**	-0.024	0.008	-0.039***	-0.033**	-0.038***
	(0.016)	(0.015)	(0.019)	(0.014)	(0.016)	(0.014)
Constant	0.798***	0.785***	0.447***	0.737***	0.768***	0.755***
	(0.010)	(0.009)	(0.014)	(0.009)	(0.011)	(0.009)
Observations	3,993	4,002	3,909	4,000	3,998	3,969
R^2	0.002	0.001	0.000	0.004	0.003	0.003

Table 5: Effect of treatment on outcome measures. Robust standard errors in parentheses. Levels of significance: *** 99% ** 95% * 90%

E.1. Models for indices (not pre-registered)

	Prosocial index	**Affective index**
Control	-	-
Disability treatment	-0.004	-0.016
	(0.012)	(0.011)
Ethnic Min treatment	-0.018	-0.024**
	(0.013)	(0.012)
Combined treatment	-0.028**	-0.037***
	(0.013)	(0.013)
Constant	0.768***	0.753***
	(0.008)	(0.008)
Observations	3872	3941
R^2	0.002	0.004

Table 6: Effect of treatment on indices. Robust standard errors in parentheses. Levels of significance: *** 99% ** 95% * 90%

E.2. Interactions (Test H6)

	Masks Compulsory	Sacrifice Freedoms	No lockdowns	Concerned	Anger	Moved	Prosocial	Affective
Disability treatment	-0.024	-0.041**	0.010	-0.014	-0.012	-0.018	-0.030*	-0.017
	(0.020)	(0.018)	(0.025)	(0.017)	(0.019)	(0.017)	(0.016)	(0.015)
Disability ID	-0.004	-0.001	0.007	0.043**	0.002	0.037	0.003	0.042**
	(0.028)	(0.024)	(0.037)	(0.022)	(0.031)	(0.023)	(0.021)	(0.020)
Treatment x Disability ID	0.100***	0.089***	-0.051	0.046	0.059	0.034	0.092***	0.035
	(0.035)	(0.034)	(0.051)	(0.030)	(0.038)	(0.030)	(0.028)	(0.027)
Constant	0.796***	0.795***	0.453***	0.717***	0.758***	0.742***	0.771***	0.736***
	(0.013)	(0.011)	(0.019)	(0.012)	(0.014)	(0.012)	(0.011)	(0.011)
Observations	1524	1526	1489	1520	1526	1513	1480	1509
R^2	0.012	0.013	0.002	0.018	0.006	0.013	0.017	0.018
Minimal detectable effect	0.074	0.073	0.074	0.073	0.073	0.072	0.074	0.073

Table 7: **Effect of treatment, interacted with whether respondents is disabled, on outcome measures**. Robust standard errors in parentheses. Levels of significance: *** 99% ** 95% * 90%. Minimal detectable effects are calculated using the InteractionPoweR package for R at 80% power, $\alpha = 0.05$ (Baranger et al., 2023).

	Masks Compulsory	Sacrifice Freedoms	No lockdowns	Concerned	Anger	Moved	Prosocial	Affective
Disability treatment	-0.002	-0.021	0.016	-0.006	-0.006	-0.011	-0.010	-0.010
	(0.019)	(0.018)	(0.024)	(0.016)	(0.018)	(0.016)	(0.016)	(0.015)
Proximate	0.051**	0.037	0.029	0.041*	0.025	0.032	0.041**	0.046**
	(0.025)	(0.023)	(0.046)	(0.024)	(0.041)	(0.025)	(0.021)	(0.021)
Treatment x Proximate	0.012	0.029	-0.077	-0.006	0.031	-0.021	0.018	-0.016
	(0.038)	(0.034)	(0.062)	(0.034)	(0.048)	(0.039)	(0.031)	(0.032)
Constant	0.787***	0.787***	0.446***	0.723***	0.754***	0.749***	0.765***	0.741***
	(0.013)	(0.012)	(0.018)	(0.011)	(0.013)	(0.012)	(0.011)	(0.010)
Observations	1504	1505	1466	1499	1505	1492	1458	1488
R^2	0.006	0.007	0.002	0.004	0.004	0.003	0.008	0.006
Minimal detectable effect	0.075	0.074	0.075	0.074	0.074	0.075	0.075	0.075'[]

Table 8: **Effect of treatment, interacted with whether respondents is family member of someone who is disabled, on outcome measures**. Robust standard errors in parentheses. Levels of significance: *** 99% ** 95% * 90%. Minimal detectable effects are calculated using the InteractionPoweR package for R at 80% power, $\alpha = 0.05$ (Baranger et al., 2023).

F Equivalence Testing

In addition to testing for evidence of an effect, we use equivalence tests to test for the absence of a small, meaningful effect. To do so, we use a two one-sided T-test (TOST) procedure and calculate relevant equivalence bounds for all effects (Lakens et al., 2018).

The TOST procedure requires researchers to identify the smallest effect size of interest (SESOI) they can be expected to observe in their study. Lakens (2014) identifies several ways to do so, such as conducting a meta-analysis of related relevant studies, or selecting some meaningful measure of movement in the outcome variable. In the literature on framing effects, there is not a generally agrees SESOI. Recent meta-analyses of framing effects provides mixed results. For example, Amsalem and Zoizner (2022) analysis of 138 framing experiments in political science finds that framing has medium-sized effects on citizens' political attitudes and emotions (d = 0.41 & 0.47 respectively), but effects on behavior are weak (d = 0.11). However, in Gallagher and Updegraff (2011)'s meta-analysis of framing effects on public health attitudes and behaviour, they report much weaker effects across of wide range of attitudes and behaviours, but highlight the important additive nature of even small public health effects.

Recent work on COVID-19 and framing also fails to agree on a consensus of what a meaningful effect size and there is seldom SESOI's identified in pre-registrations. A meta-analysis of 57 studies on the effect of interventions on vaccine hesitancy (Huang et al., 2023) identified an overall effect size of d=0.2, with this reducing to a much smaller effect of d=0.09 for studies that used an online experimental design. Research on political attitudes and behaviours has produced similar variation in reported effect sizes. For example, Deslatte (2020) reports medium-sized effects on public health messaging reducing intentions to go shopping in the US.In contrast Kuipers et al. (2021) report much smaller (albeit not significant) effects in their study on encouraging Indonesians to pray at home during the pandemic. In identifying a SESOI they identify the minimum detectable effect size (MDES) for their sample, ranging from a 5% change in outcome variable in main treatments (approximately d=0.14) to a 9% change for subgroup analysis (approximately d=0.26). Similarly Chung et al. (2022) use power analyses to identify a MDES of 0.04 to set as their inferiority bounds in equivalence testing. Rasmussen et al. (2024) identify a SESOI based on previous work they aim to replicate, but also identify a MDES to identify small effects.

In the absence of a clear consensus identifying meaningful effects in framing studies, we follow Lakens (2014) suggestion and calculate equivalence bounds using the smallest true effect size we can statistically detect given sample size. Given the study's large sample size, this is a small effect: d = 0.09.

Outcome	Group	N	Mean	SD	Mean Difference	Reject H_0	Equivalence Bounds	Reject H_A
Masks Compulsory	Control	988	0.7981	0.2768	0.01	No	-0.025, 0.025	No
	Disability	1004	0.7879	0.2793				
Sacrifice freedoms	Control	988	0.7850	0.2476	0.002	No	-0.023, 0.023	Yes
	Disability	1007	0.7827	0.2592				
No more lockdowns	Control	971	0.4473	0.3575	-0.009	No	-0.032, 0.032	No
	Disability	981	0.4561	0.3477				
Concerned	Control	985	0.7374	0.2354	0.013	No	-0.0213 , 0.0213	No
	Disability	1005	0.7240	0.2374				
Anger	Control	988	0.7682	0.2668	0.011	No	-0.023, 0.023	No
	Disability	1008	0.7567	0.2527				
Moved	Control	974	0.7545	0.2256	0.013	No	-0.0205, 0.0205	No
	Disability	1000	0.7419	0.2308				
Prosocial Index	Control	964	0.7683	0.2255	0.004	No	-0.021 , 0.021	No
	Disability	972	0.7641	0.2309				
Affective Index	Control	970	0.7530	0.2064	0.016	No	-0.0186, 0.0186	No
	Disability	997	0.7371	0.2060				

Table 9: Descriptive statistics and equivalence testing for disability treatment

Outcome	Group	N	Mean	SD	Mean Difference	Reject H_0	Equivalence Bounds	Reject H_A
Masks Compulsory	Control	988	0.7981	0.2768	0.02	No	-0.0234, 0.0234	No
	Ethnic min treatment	1001	0.7786	0.2806				
Sacrifice freedoms	Control	988	0.7850	0.2476	0.018	No	-0.0251, 0.0251	No
	Ethnic min treatment	1002	0.7667	0.2709				
No more lockdowns	Control	971	0.4473	0.3575	-0.019	No	-0.0318 , 0.0318	No
	Ethnic min treatment	979	0.4663	0.3502				
Concerned	Control	985	0.7374	0.2354	0.029	Yes	-0.0214, 0.0214	No
	Ethnic min treatment	1006	0.7083	0.2392				
Anger	Control	988	0.7682	0.2668	0.031	Yes	-0.0243, 0.0243	No
	Ethnic min treatment	999	0.7372	0.2736				
Moved	Control	974	0.7545	0.2256	0.013	No	-0.0203, 0.0203	No
	Ethnic min treatment	995	0.7415	0.2252				
Prosocial Index	Control	964	0.7683	0.2255	0.018	No	-0.0207, 0.0207	No
	Ethnic min treatment	968	0.7504	0.2340				
Affective Index	Control	970	0.7530	0.2064	0.024	Yes	-0.0186, 0.0186	No
	Ethnic min treatment	985	0.7294	0.2072				

Table 10: Descriptive statistics and equivalence testing for ethnic minority treatment

Outcome	Group	N	Mean	SD	Mean Difference	Reject H_0	Equivalence Bounds	Reject H_A
Masks Compulsory	Control	988	0.7981	0.2768	0.037	Yes	-0.026, 0.026	No
	Combined treatment	1000	0.7609	0.3000				
Sacrifice freedoms	Control	988	0.7850	0.2476	0.024	Yes	-0.0239, 0.0239	No
	Combined treatment	1005	0.7611	0.2821				
No more lockdowns	Control	971	0.4473	0.3575	-0.008	No	-0.09, 0.09	No
	Combined treatment	978	0.4557	0.3480				
Concerned	Control	985	0.7374	0.2354	0.039	Yes	-0.022, 0.022	No
	Combined treatment	1004	0.6979	0.2523				
Anger	Control	988	0.7682	0.2668	0.033	Yes	-0.0246, 0.0246	No
	Combined treatment	1003	0.7352	0.2797				
Moved	Control	974	0.7545	0.2256	0.038	Yes	-0.0215, 0.0215	No
	Combined treatment	1000	0.7170	0.2518				
Prosocial Index	Control	964	0.7683	0.2255	0.028	Yes	-0.0213, 0.0213	No
	Combined treatment	968	0.7398	0.2481				
Affective Index	Control	970	0.7530	0.2064	0.037	Yes	-0.0193, 0.0193	No
	Combined treatment	989	0.7156	0.2229				

Table 11: Descriptive statistics and equivalence testing for combined treatment

Outcome	Group	N	Mean	SD	Mean Difference	Reject H_0	Equivalence Bounds	Reject H_A
Masks Compulsory	Ethnic min treatment	1001	0.7786	0.2806	0.018	No	-0.0261, 0.0261	No
	Combined treatment	1000	0.7609	0.3000				
Sacrifice freedoms	Ethnic min treatment	1002	0.7667	0.2709	0.006	No	-0.025, 0.025	No
	Combined treatment	1005	0.7611	0.2821				
No more lockdowns	Ethnic min treatment	979	0.4663	0.3502	0.011	No	-0.031, 0.031	No
	Combined treatment	978	0.4557	0.3480				
Concerned	Ethnic min treatment	1006	0.7083	0.2392	0.01	No	-0.022, 0.022	No
	Combined treatment	1004	0.6979	0.2523				
Anger	Ethnic min treatment	999	0.7372	0.2736	0.002	No	-0.025, 0.025	Yes
	Combined treatment	1003	0.7352	0.2797				
Moved	Ethnic min treatment	995	0.7415	0.2252	0.024	Yes	-0.021, 0.021	No
	Combined treatment	1000	0.7170	0.2518				
Prosocial Index	Ethnic min treatment	968	0.7504	0.2340	0.011	No	-0.022, 0.022	No
	Combined treatment	968	0.7398	0.2481				
Affective Index	Ethnic min treatment	985	0.7294	0.2072	0.014	No	-0.019, 0.019	No
	Combined treatment	989	0.7156	0.2229				

Table 12: Descriptive statistics and equivalence testing for differences between ethnic minority treatment and the combined treatment (H3 & H4)

G Interacting treatment with respondent ideology

In addition to the interactions presented above, we pre-registered models where we interact treatment with a respondents' ideology. Here, we focus on social values, or the so-called liberal-authoritarian scale. We select this value scale, as opposed to say left-right economic values, because of the growing body of evidence which has established a link between these values and individuals' responses to the pandemic (see, for example, Ollerenshaw (2022)). Results are presented in Figures below as per Brambor et al. (2006). We observe no statistically significant interaction effects, with very little heterogeneity at all across treatments.

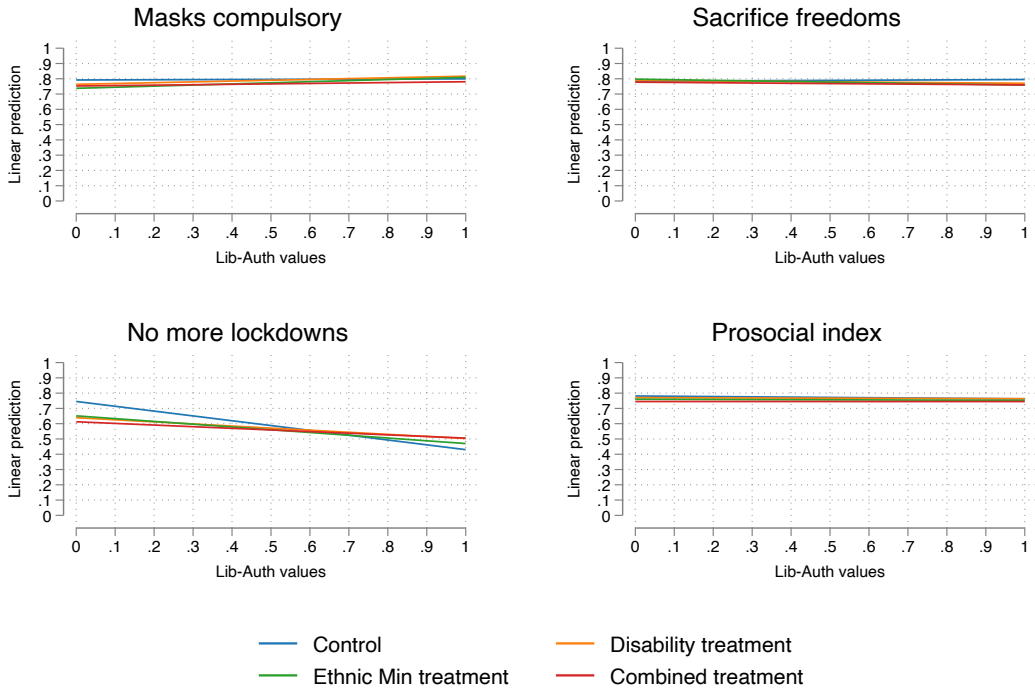

Figure 3: **Effect of treatment interacted with ideology on prosocial items + index**

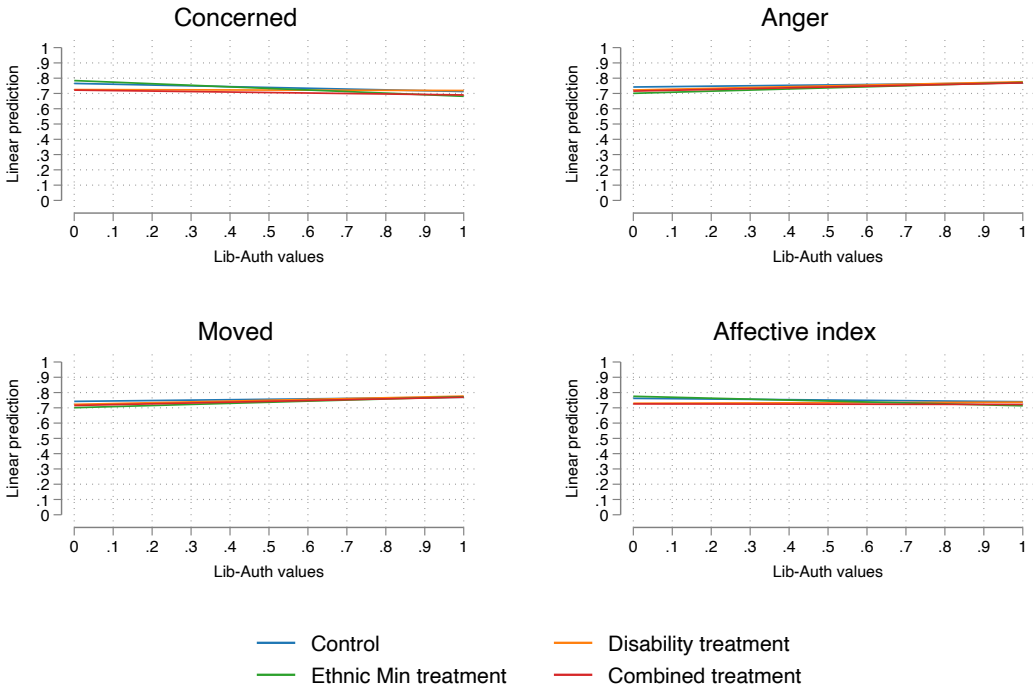

Figure 4: **Effect of treatment interacted with ideology on affective items + index**

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