# Abstract

Occasionally, people judge an action as wrong, but when asked why, they may simply assert “*it’s just wrong!*” or even “*it’s wrong, but I can’t explain why*”. Maintaining a moral judgment without supporting reasons is known as moral dumbfounding. But why are people dumbfounded? One explanation proposes that it occurs as a result of a conflict between intuitive and deliberative processes. Previous research demonstrated that a manipulation designed to lead to more intuitive thinking rather than deliberative thinking (increased cognitive load), reduced the likelihood of providing reasons, and increased dumbfounded responding. We examine if dumbfounded responding can be reduced by facilitating deliberative thinking (over intuitive thinking). Drawing on construal-level theory and the finding that distancing facilitates deliberative thinking, we predict that including a distancing manipulation in a moral dumbfounding task will increase reason-giving and reduce dumbfounded responding. We propose a pre-registered experiment (*N* = 2,400) to test this prediction.

*Keywords*:  moral dumbfounding, distancing, construal-level theory, dual-processes, reasons, intuitions

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# Space to Think: Testing the Effect of Distancing on Moral Dumbfounding – A Registered Report

*“This line of thinking is morally wrong”* (US Representative discussing role of parents/schools in raising children in the House of Representatives in 2023)

*“The Bill is unnecessary and it is poorly drafted, but above all, it is deeply wrong…”* (UK Politician discussing free speech in Universities in UK Parliament in 2021)

*“I appeal to the Minister to put this Bill on the shelves of his Department and to leave it to become surrounded by cobwebs … Artificial contraception is morally wrong.”* (Irish Politician discussing contraception in the Dáil in 1979)

People often appeal to morality to support their positions in arguments or discussions. Such more appeals are typically presented as self-evident (Skitka, 2010) and justifications or reasons not provided. In politically charged, contentious debates proponents may argue for something *“because it is the right thing to do”* while opponents may reject something *“because it is just plain wrong”*. It is not always apparent if the speakers could provide a justification if pressed. In fact, there is good reason to expect that, at least in some cases, speakers would fail to provide reasons in support of their moral position (e.g., McHugh, Zhang, et al., 2023; see also Sim, 2016).

This failure to provide reasons for a moral judgment is known as moral dumbfounding (Haidt et al., 2000; McHugh et al., 2017). While perhaps not as prevalent as initially thought (e.g., Royzman et al., 2015; see also McHugh et al., 2020), there is a growing body of evidence demonstrating that it is a robust phenomenon (e.g., in a cross-cultural study McHugh, Zhang, et al., 2023, reported rates of dumbfounded responding ranging from 9% to 36% depending on the scenario and population). Despite a growing body of empirical work demonstrating the phenomenon, it remains poorly understood, with only limited work examining the specific mechanisms that underpin dumbfounded responding.

# The Importance of Studying Dumbfounding

Since its earliest empirical demonstrations (Haidt et al., 2000), moral dumbfounding has influenced theorizing about moral judgment. It is typically cited as evidence that moral judgments can be grounded in intuitive, automatic, or emotional processes (Haidt, 2001; Prinz, 2005; Cushman et al., 2010), informing the development of both intuitionist (e.g., Haidt, 2001) and dual-process theories of moral judgment (e.g., Cushman, 2013; Byrd & Conway, 2019).

While its influence on the development of theory is apparent, the cognitive processes that lead to dumbfounding are not well accounted for within current theorizing. There have been some attempts to explain the phenomenon (e.g., Haidt, 2001; McHugh et al., 2022; Green, 2024), however, there is only limited empirical work directly testing these explanations (e.g., McHugh, McGann, et al., 2023). Furthering our understanding of moral dumbfounding is therefore critical for the advancement of theory in moral psychology. That is, a successful theory of moral psychology should be able to provide a clear account of the phenomenon. This is the first aim of the current work.

In addition to the theoretical importance of understanding moral dumbfounding, There is also a practical benefit to studying it. Understanding moral dumbfounding may lead to the development of techniques by which it can be reduced. This would be of value both because the experience of it is often morally uncomfortable, but also because dumbfounding limits effective communication and discussion. Interventions that reduce its likelihood may improve debates addressing morally charged issues in settings from schools to parliaments. Testing one such strategy is the second aim of the current research. We propose a pre-registered experiment that will (a) examine one possible explanation for moral dumbfounding and (b) test the efficacy of one strategy that might help reduce dumbfounding: psychological distancing.

# Moral Dumbfounding as a Conflict in Dual-Processes

McHugh, McGann, et al. (2023) proposed a dual-process explanation of moral dumbfounding. Dual-process approaches contrast intuitive/habitual responding, e.g., *killing is wrong!*, with deliberative responding, e.g., *sacrificing one to save five may be justified* (e.g., Bago & De Neys, 2019; Cushman, 2013; Greene, 2008). McHugh, McGann et al. (2023) argue that moral dumbfounding results from a conflict in dual-processes, that is, when a habitual/intuitive response - making a moral judgment - differs from a response that results from deliberation - providing reasons for a moral judgment (e.g., Bonner & Newell, 2010; De Neys & Glumicic, 2008).

According to this view, rates of reason-giving should be reduced in situations where deliberation is inhibited. McHugh, McGann, et al. (2023) tested this prediction demonstrating that a cognitive load manipulation (requiring participants to attend to a secondary task while responding to the moral dumbfounding protocol) led to reduced rates of reason-giving and higher rates of dumbfounding (McHugh, McGann, et al., 2023).

 The reverse prediction can also be made: under conditions where deliberative responding is more likely, rates of reason-giving should increase, and rates of dumbfounding should decrease. We draw on research on construal level theory and psychological distance (e.g., Trope & Liberman, 2010), and predict that under conditions of increased psychological distance, rates of reason-giving should increase.

# Distancing and Dumbfounding

Psychological distance is the degree to which something is removed from direct experience, or the perception that something is close or distant from the self (this could be temporal, spatial, social, or hypothetical, see Trope & Liberman, 2010). Previous research has shown that varying psychological distance impacts how people think about things (e.g., Förster et al., 2004).

Most relevant for the current research is the suggestion that manipulations intended to increase psychological distance will lead to an increase in deliberative thinking (e.g., Bar-anan et al., 2006; Evans & Stanovich, 2013; Trope & Liberman, 2010). Specifically, greater psychological distance is associated with higher-level construals, which are associated with more abstract (Bar-anan et al., 2006) and analytical or deliberative thinking (Evans & Stanovich, 2013), as well as less emotional thinking (Powers & LaBar, 2019). Conversely, decreased psychological distance is associated with lower level construals, and associated with more emotional thinking (Powers & LaBar, 2019), less abstraction (more concrete thinking), and less deliberative (more intuitive) thinking (Evans & Stanovich, 2013). Together, this suggests that increasing psychological distance should facilitate deliberative thinking (and lead to higher rates of reason-giving in a dumbfounding task).

In line with McHugh, McGann, et al. (2023), we have mapped the three responses in the dumbfounding paradigm according to their relative amount of deliberation (see Figure 1). This mapping is informed by existing theorizing (McHugh et al., 2022; Cushman, 2013) and assumes that making a moral judgment is an intuitive/habitual response, involving relatively little deliberation. In contrast, reason-giving involves greater deliberation. Applied to the available responses in the dumbfounding paradigm, providing a reason for a moral judgment (reason-giving) involves the most deliberation; providing a dumbfounded response (dumbfounding) involves the least amount of deliberation; and selecting “There is nothing wrong” (nothing-wrong) involves less deliberation than reason-giving, but more deliberation than dumbfounding (deliberation may lead participants to accept the counter-arguments, and revise their judgment rather than deliberate further to identify alternative reasons). Based on this, we predict that under conditions of increased psychological distance, rates of reason-giving will increase, while rates of nothing-wrong and dumbfounding will decrease.

**Figure 1**

*Hypothesized level of deliberation for each response in the dumbfounding paradigm*

*A diagram of different levels of progress

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*Note.* Adapted from McHugh, McGann, et al. (2023)

# The Current Research

The current research tests the prediction that manipulations designed to increase psychological distance will facilitate reason-giving in a moral dumbfounding task. We will test two different manipulations across two domains of psychological distance, social distance and temporal distance. We predict that under increased psychological distance (social/temporal distance), rates of reason-giving will be higher than in a control group or under reduced psychological distance. These findings will allow us to peer into the moral judgment process to better understand the cognitive processes that underpin moral dumbfounding, including those which may mitigate it.

# Method

## Participants and Design

The proposed study will be a 2 × 3 between-subjects design. The dependent variable is rates of reason-giving/dumbfounding (measured using the critical slide with 3 response options: 1: reason-giving; 2: nothing-wrong; 3: dumbfounded response - admission). The first independent variable is social distance with two levels: control and increased social distance. Social distance will be manipulated by asking participants to think about the moral dilemma for themselves (control) or from the perspective of another person (increased social distance). The second independent variable is temporal distance, with three levels: control, reduced temporal distance, and increased temporal distance. Temporal distance will be manipulated by varying the instructions provided to participants. In the control condition, participants will not be provided with any instructions about a time frame; in the increased temporal distance condition, participants will be asked to imagine responding in the distant future (five weeks from now); in the reduced distance condition, participants will be asked to imagine responding in the near future (two to three hours from now). In line with McHugh, McGann, et al. (2023), we will use four scenarios, and as such, the scenario will function as a third independent variable in some of our analyses.

In order to determine our target sample size, we conducted a series of power analyses for our planned analyses for large, medium, and small effect sizes. Our planned design will require three different analyses to test our hypotheses. First, we will investigate the influence of social distance on reason-giving (Analysis 1: one independent variable with 2 levels). Second, we will investigate the influence of temporal distancing on reason-giving (Analysis 2: one independent variable with 3 levels). Third, we will investigate the combined influence of social and temporal distancing on reason-giving (Analysis 3: two independent variables, 2  3 levels). We set our target power as 90% and calculated the minimum sample size required to detect large, medium, and small effects for each analysis. The minimum sample sizes required for each effect size for each analysis are detailed in Table 1.

**Table 1**

*Power analyses and sample size calculations*

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  |  | Analysis 1: Social | Analysis 2: Temporal | Analysis 3: Combined |
| Large | *V* | 0.35 | 0.25 | 0.16 |
|  | *N* | 101 | 246 | 821 |
| Medium | *V* | 0.21 | 0.15 | 0.09 |
|  | *N* | 281 | 685 | 2281 |
| Small | *V* | 0.07 | 0.05 | 0.03 |
|  | *N* | 2531 | 6162 | 20532 |

*Note.* *V* = Cramer’s V, effect size for proposed chi-squared test

Based on these analyses we set our target sample at N = 2,400. This sample size is sufficient to detect a medium effect in Analysis 3 (the combined effects of social and temporal distance). It also allows for sub-samples of *n* = 600 for each scenario, and as such the minimum number of participants at each level of the temporal distance independent variable for each scenario will be *n* = 200. Based on this target sample of *N* = 2,400 we have identified the smallest effect size that can be detected with 90% power for each analysis, for the full sample, and for the scenario-level sub-samples. These are displayed in Table 2.

**Table 2**

*Minimum detectable effect size for each analysis based on proposed sample size, for overall analysis, and for scenario level analysis*

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | Overall *N* | Overall *V* | Scenario *N* | Scenario *V* |
| Analysis 1: Social | 2,400 | 0.07 | 600 | 0.15 |
| Analysis 2: Temporal | 2,400 | 0.08 | 600 | 0.16 |
| Analysis 3: Combined | 2,400 | 0.09 | 600 | 0.18 |

*Note.* *V* = Cramer’s *V* effect size for chi-squared test

Participants will primarily be recruited through a combination of convenience and snowball sampling in the institutions each of the members of the research team. Some participants may be recruited using the SONA research participation system where available. Additional participants may be recruited through online research participation systems (e.g., Prolific / Lucid / CloudResearch / MTurk) if necessary. Data collection will take place primarily in Ireland, UK, USA, Poland and Chile (though the convenience and snowball sampling may also result in participants from other countries taking part). For the Polish and Chilean samples, the materials will be translated into Polish and Spanish respectively. The forward translation will be completed by members of the research team, with independent back translation for verification. Participant nationality and place of residence will be recorded to test for differences between the samples.

## Procedure and Materials

Data will be collected using an online survey programmed in Qualtrics (or comparable package). On agreeing to take part in the survey, participants will be randomly assigned to one of six experimental groups (reflecting the 2 × 3 design). At this point participants will be presented with an instruction page that will contain the experimental manipulation.

In the social distance control condition, participants will not receive instructions to think about the scenario/questions from a perspective other than their own. They will simply be told that the following page contains a description of a scenario and a series of questions relating to the scenario.

In the increased social distance (manipulation) condition, participants will be introduced to a student (Alex) who is currently taking a course in ethics. Participants will then be told that the next page contains a scenario and a series of questions that Alex has been asked to study and complete an assignment on. Participants will be instructed to think about the scenario and the related questions from Alex’s perspective rather than their own.

In the temporal distance control condition, participants will not receive instructions regarding responding to the scenario within any particular time frame.

In order to manipulate temporal distance, participants in both temporal distance manipulation conditions will be asked to consider the scenario as if it related to assignments as part of an ethics course, either imagining that they are completing such a course (social distance control group) or on behalf of Alex (increased social distance group). In the increased temporal distance condition, participants will be asked to imagine the deadline for this assignment is five weeks away. In the reduced temporal distance condition, this deadline is within 2-3 hours. A summary of the key differences between the different conditions is outlined in Figure 2 (see the supplementary materials for the full text of the instructions for each condition).

**Figure 2**

*Summary of the different experimental conditions*

A screenshot of a social media page

Description automatically generated

Following the experimental manipulation, participants will be presented with one of four moral scenarios (*Heinz*, *Trolley*, *Jennifer*, and *Julie and Mark*) previously used in the study of moral dumbfounding (McHugh et al., 2017), and the influences on moral dumbfounding (McHugh, McGann, et al., 2023). Participants will be asked to rate on a 7-point Likert scale how right or wrong the behavior described is (where 1 = *morally wrong*; 4 = *neutral*; 7 = *morally right*), as well as their confidence in this judgment (where 1 = *not at all confident*; 7 = *extremely confident*). Participants will then be given an opportunity to provide reasons for their judgment. Following this, participants will be presented with a series of counter-arguments, which refute commonly used justifications for rating the behavior as “wrong” (see supplementary materials for full text of scenarios and all counter-arguments).

After each counter-argument, participants will be asked if they (or Alex) view (should rate) the behavior as wrong, and if they have a reason for this judgment. Following the counter-arguments and related questions, dumbfounding will be measured using the critical slide (McHugh et al., 2017). This contains a statement defending the behavior and a question as to how the behavior could be wrong (e.g., “Jennifer’s actions did not harm anyone, or negatively affect anyone. How can there be anything wrong with what she did?”). There are three possible answer options (presented in randomized order): (a) “It’s wrong, and I can provide a valid reason” (reasons-giving); (b) “It’s wrong, but I can’t think of a reason” (dumbfounding: an admission of not having reasons); (c) “There is nothing wrong” (nothing-wrong). Participants who select (a) will be prompted to type a reason on the next page. We will also record participants’ response times, which may be used in future secondary and exploratory analyses.

Two attention check tasks will be included for all participants; these included a brief paragraph of text where instructions for the correct response were embedded within the text. The wording of the text is misleading, such that if participants skim or only read some of the text, they will likely provide an incorrect response. Participants who fail both attention checks will be excluded from the analysis. Participants will also be asked to report basic demographic information including age, gender, nationality, religion, political ideology, and place of residence.

When participants click on the survey link, they will be randomly assigned to one of the six experimental groups, and randomly presented with one of the four moral scenarios. The study duration is estimated to be less than 5 minutes.

## Analysis Plan

We will conduct a combination of chi-squared tests for independence, and multinomial logistic regressions to test each of our hypotheses. Our primary analysis for each hypothesis will be a chi-squared test for independence, where we will test for an association between the experimental condition/experimental group and response to the critical slide. We will also conduct logistic regressions to test each hypothesis, this will allow us to additionally control for other variables (e.g., scenario or participant country) in our analysis. In cases where no effects are found we will conduct follow-up equivalence tests to distinguish between absence of evidence and evidence of absence of an effect.

# Results

# Discussion

# Disclosures

All measures, manipulations, and data/participant exclusions are reported in the manuscript or its Supplementary Material.

# Open Practices

All (pilot) data, analysis scripts, and materials are available from the OSF at <https://osf.io/3fuer/?view_only=c2b02ef663fc4a12a2c3a143c21d9776>. Full data from the main study will be made available on completion of the study.

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