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# Does Momentary Outcome-Based Reflection Shape Bioethical Views? A Pre-Post Intervention Design

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## Abstract

Many bioliberals endorse broadly consequentialist frameworks in normative ethics, implying that a progressive stance on matters of bioethical controversy could stem from outcome-based reasoning. This raises an intriguing empirical prediction: encouraging outcome-based reflection could yield a shift toward bioliberal views among nonexperts as well. To evaluate this hypothesis, we identified empirical premises that underlie moral disagreements on seven divisive issues (e.g., vaccines, abortion, or genetically modified organisms). In exploratory and confirmatory experiments, we assessed whether people spontaneously engage in outcome-based reasoning by asking how their moral views change after momentarily reflecting on the underlying empirical questions. Our findings indicate that momentary reflection had no overall treatment effect on the central tendency or the dispersion in moral attitudes when compared to prereflection measures collected 1 week prior. Autoregressive models provided evidence that participants engaged in consequentialist moral reasoning, but this self-guided reflection produced neither moral “progress” (shifts in the distributions’ central tendency) nor moral “consensus”

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[Correction added on November 15, 2024, after first online publication: Co-author name ‘John Rueda’ has been corrected to ‘Jon Rueda’.]

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(reductions in their dispersion). These results imply that flexibility in people's search for empirical answers may limit the potential for outcome-based reflection to foster moral consensus.

*Keywords:* Consequentialism; Moral consensus; Moral disagreement; Moral progress; Outcome-based reasoning

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## 1. Introduction

Scholars who advocate progressive views on current debates in bioethics—or as they are sometimes labeled, *bioliberals*—endorse broadly consequentialist frameworks in normative ethics. According to this ethical theory, the moral status of an action or policy should be based on a calculus of its positive and negative consequences. A central tenet of consequentialist ethics is that empirical beliefs play a pivotal role in shaping moral judgments (e.g., Mill, 1863). Accordingly, empirical beliefs about the consequences of an action should inform one's moral attitudes toward the action in question. These empirical beliefs might be about whether the action is deemed harmful (Gray & Schein, 2016) or has the potential to save numerous lives (Engelmann & Waldmann, 2022; Hannikainen, Miller, & Cushman, 2017; Shenhav & Greene, 2010). This principle is in contrast to other ethical frameworks, such as deontological or virtue ethics, in which the role of consequentialist information is not as central, and the normative status of people's moral judgments is guided primarily by inflexible principles, such as respect for autonomy (Demaree-Cotton & Sommers, 2022), as well as the action's causal and intentional properties (Rodríguez-Arias, Rodríguez Lopez, Monasterio-Astobiza, & Hannikainen, 2020).

### 1.1. *The relation between reflection and consequentialist moral judgment*

Numerous empirical investigations have uncovered a relationship between reflection and consequentialist moral judgments about hypothetical thought experiments. For instance, a classic experiment by Paxton and colleagues (2012) found participants were more permissive, and consequentialist, in their moral judgments about an innocuous case of consensual incest after reflecting on the evolutionary origins of the incest taboo (but see Herec et al., 2022). Relatedly, Pennycook, Cheyne, Barr, Koehler, and Fugelsang (2014) found that individuals with an analytic cognitive style, compared to those with a more intuitive style, were less likely to condemn disgusting, yet victimless, crimes—a pattern of response in line with consequentialism. Finally, Royzman, Landy, and Goodwin (2014) found that reasoning not only influences the intensity of moral judgments, but also plays a role in distinguishing between moral and conventional transgressions. They found that more reflective participants tended to moralize selectively, treating only clearly harmful acts as moral transgressions, and treating contingently harmful transgressions as arbitrary societal constructions.

Could these experimental demonstrations of the relation between reasoning and consequentialist moral judgment help to explain moral disagreement on matters of real-world ethical controversy? A traditional literature in the domain of political psychology, on the existence of a “backfire effect” (Nyhan & Reifler, 2010), speaks against this prediction. Early

research indicated that encounters with uncongenial evidence can lead partisans to intensify their pre-existing worldview, instead of conceding or adjusting their beliefs *toward* the evidence (Lord, Ross, & Lepper, 1979; Nyhan & Reifler, 2010; Taber & Lodge, 2006). For instance, when provided corrective information on controversial political issues in American politics, such as the invasion of Iraq, tax cuts, and stem cell research, participants tended to strengthen their misperceptions about such facts (Nyhan & Reifler, 2010). This line of research raises the possibility that attitudes toward real-world issues might be *less* malleable by reflection than are hypothetical issues—though some recent studies have failed to replicate the backfire effect (Tappin, Pennycook, & Rand, 2020; Wood & Porter, 2019).

The relation between reasoning and consequentialist moral judgment has proved to be particularly significant when such reasoning concerns consequences, or outcomes. Hannikainen and Rosas (2019) replicated the relation between reflection and the consequentialist tendency for selective moralization by directing participants to reason about the consequences of a set of moral transgressions: Colombian and British participants evaluated both victimless crimes, such as cutting up the national flag and using it to clean one's toilet (Haidt, Koller, & Dias, 1993), and harmful or unfair acts, such as food hoarding. A brief reflection on the consequences of *harmful* behavior, such as hoarding, elevated moral condemnation of such behavior in both cultural groups. The corresponding effect of reflecting on the consequences of “impure,” yet innocuous, behavior was absent in Colombia and *negative* in the United Kingdom—replicating the tendency for consequentialist reasoning to propel *demoralization* of victimless taboos.

Other studies in experimental ethics have successfully documented the persuasive effects of consequentialist arguments on the moral imperative to eradicate global poverty (e.g., Buckland, Lindauer, Rodríguez-Arias, & Véliz, 2021) or to reduce meat consumption (e.g., Schwitzgebel, Cokelet, & Singer, 2020). Together, these studies reveal that reflection on empirical facts about consequences, namely, outcome-based reasoning, such as others' suffering, can shape people's moral outlook. Consequently, there is mixed evidence as to whether reflection, and particularly reflection on consequences, relates to consequentialist moral judgment about real-world controversies.

## 1.2. *Moral progress and moral consensus*

It may be useful to consider two variants of the hypothesis that outcome-based reasoning shapes moral judgment, each with a distinct predicted outcome. One possible outcome of outcome-based reasoning is a shift in *central tendency* of the distribution of moral attitudes (see Fig. 1A). For instance, as some utilitarian theorists have suggested (e.g., Singer, 1981), various elements of outcome-based reasoning, for example, the adoption of an impartial point of view, might propel moral “progress” on a variety of issues. Elements of this prediction can be seen in the work of Peter Singer (1981), who argues that adopting a consequentialist approach to ethics yields an expansion of the moral circle—a characteristic quality of a progressive worldview. Some empirical work has vindicated this prediction, showing that political liberals reveal a greater tendency toward consequentialist moral reasoning (Hannikainen et al., 2017; Kahane et al., 2018; Luke & Gawronski, 2021; Piazza & Sousa, 2014).

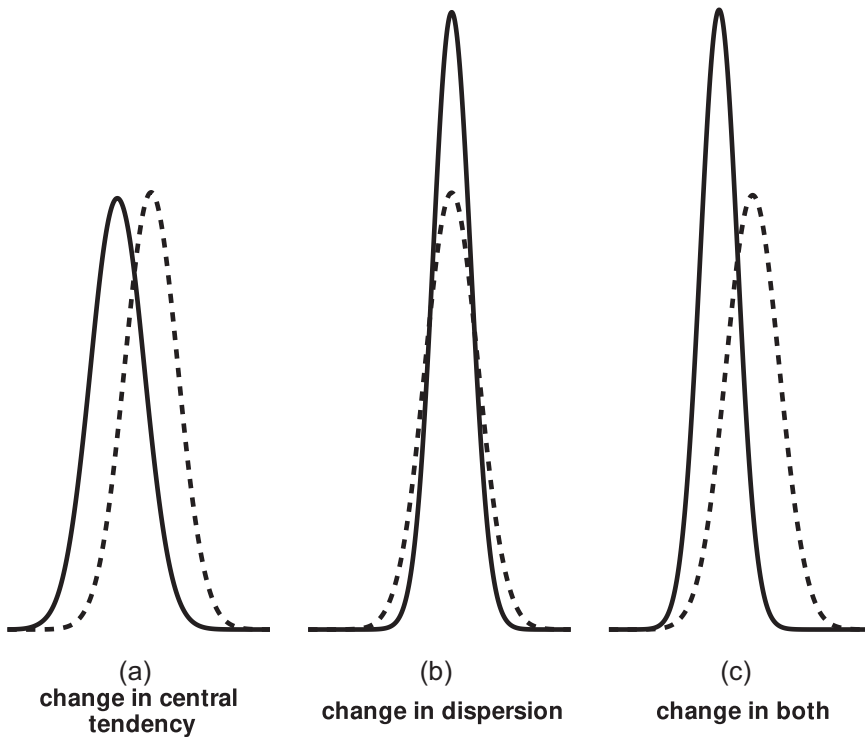


Fig. 1. Model predictions. (A) The progressive shift view predicts a change in the central tendency of moral judgment. (B) The consensus view predicts a change in the dispersion of moral judgment. (C) Panel C displays the combined occurrence of changes in central tendency and dispersion.

A second potential outcome is that outcome-based reasoning might facilitate moral *consensus*. This result would manifest as a change in the variance or *dispersion* (Fig. 1B) of the distribution of moral attitudes. Joshua Greene (2014), for example, argues that outcome-based reasoning could serve as a “common currency” to establish overarching norms for cooperation within culturally diverse communities. Insofar as outcome-based reasoning defines the moral status of an act in terms of allegedly objective matters of fact (i.e., the consequences of the act), adopting a consequentialist framework can reduce moral disagreement and bring citizens’ moral attitudes into closer alignment. However, it is also worth noting that opportunities for reflection have also been found to *exacerbate* disagreements leading to dissensus or *greater* dispersion (Drummond & Fischhoff, 2017; Kahan, 2013; Kahan et al., 2012)—as predicted by theories of identity-protective cognition. Finally, it must be noted that the progress and consensus hypotheses are not mutually exclusive, as depicted in Fig. 1C.

### 1.3. Overview

In the context of bioethics, this potential impact of outcome-based reasoning on moral judgment implies that a progressive stance on ethical controversies, such as mandatory vaccination, the limits on legal abortion, or genetically modified organisms, could stem primarily

from outcome-based reasoning. Our present research contributes to this line of research on the impact of outcome-based reasoning on moral attitudes toward real-world issues in two significant ways. First, we specifically focus on bioethical issues rather than political issues or hypothetical scenarios to address real-world concerns with immediate relevance. Second, we use an unguided and brief reflection methodology rather than a guided or structured one, which allows participants to reflect spontaneously without predefined prompts.

Thus, our present paper examines the impact of momentary outcome-based reasoning on moral disagreement about seven different issues that engender controversy in bioethics, and in the public discourse. For instance, some of such issues were the mandatoriness of vaccines, or the limits of legal abortion. Our focus in exploratory and confirmatory studies was on evaluating two distinct hypotheses: the progressive shift and the consensus hypotheses described above. To this end, we recorded participants' baseline (or Time 1) normative attitudes toward all seven issues. Then, in a second test session after a 1-week delay, participants were randomly assigned to briefly reflect on an empirical matter that underlies scholarly debate about the moral status of one of those seven issues—their target issue, to report their belief about that empirical question, and to provide a post-reflection (or Time 2) measure of their normative attitudes. For instance, in the case of the permissibility of abortion, participants were asked to reflect on the item “At what stage of human pregnancy is the fetus capable of experiencing pain?”. This aimed to prompt reflection on empirical evidence from medical and psychological research about fetuses' capacity to experience pain, and the consequences that certain practices (i.e., abortion) might have on them. The goal was to encourage reflection on the harm-related outcomes of such practices. After this mandatory and brief outcome-based reflection, we recorded their normative attitudes toward the target item.<sup>1</sup> Comparisons of Time 1 and Time 2 normative attitudes allowed us to evaluate the hypotheses that outcome-based reflection engenders moral progress and/or moral consensus. Further details about the precise experimental protocol are described in the Procedure subsections of each experiment. Study materials, data, and analysis scripts are publicly available on the *Open Science Framework* at: <https://osf.io/gwu7b/>.

## 2. Experiment 1

Experiment 1 employed a within-subjects design to examine the effect of momentary reflection on moral attitudes toward bioethical issues. Study participants initially recorded their normative attitudes toward each of the seven issues. Then, in a second experimental session 1 week apart, participants were asked to briefly reflect on a factual question (underlying one of the normative issues) and elaborate on their response in writing. After that, participants recorded their normative attitudes to the seven issues once again.

### 2.1. Materials and pre-test

We drafted 14 normative and 16 factual statements regarding 11 controversial issues in contemporary bioethics (see Table 1), and recruited a politically balanced sample of 140 U.K.

Table 1  
Models of normative attitudes toward target issues in Experiment 1

	Target Model 1	Target Model 2	Target Model 3	Target Model 4
AIC	185.56	190.71	176.58	181.71
$r^2$	.59	.59	.63	.63
Intercept	$B = -0.08$ 95% CI [-0.15, -0.01] $t = -2.26$ $p = .025$	$B = -0.13$ 95% CI [-0.15, 0.01] $t = -2.35$ $p = .08$	$B = -0.08$ 95% CI: [-0.15, -0.01] $t = -2.15$ $p = .08$	$B = -0.07$ 95% CI: [-0.14, 0.01] $t = -1.71$ $p = .13$
Time 1 Normative Belief	$B = 0.71$ 95% CI [0.61, 0.81] $t = 14.00$ $p < .001$	$B = 0.70$ 95% CI: [0.59, 0.80] $t = 13.19$ $p < .001$	$B = 0.66$ 95% CI: [0.56, 0.75] $t = 13.47$ $p < .001$	$B = 0.64$ 95% CI: [0.54, 0.74] $t = 12.32$ $p < .001$
Political Orientation	—	$B = 0.02$ 95% CI: [-0.07, 0.17] $t = 0.78$ $p = .44$	—	$B = 0.05$ 95% CI: [-0.06, 0.16] $t = 0.86$ $p = .39$
Empirical Belief	—	—	$B = 0.19$ 95% CI: [0.10, 0.28] $t = 4.04$ $p < .001$	$B = 0.19$ 95% CI: [0.10, 0.28] $t = 4.03$ $p < .001$

residents via Prolific (78 women;  $mean_{age} = 39.7$ ;  $median_{age} = 37.5$ ,  $sd_{age} = 13.6$ ;  $range_{age}$ : [18, 76]) to pre-test our materials. Each participant was presented with all 30 statements in a randomized order and asked to report their agreement or disagreement on 100-point sliding scales labeled at both extremes and intermediate tertiles.

We selected pairs of normative/factual statements on the basis of the following three criteria:

1. the strength of the fact-norm correlation;
2. the bimodality of normative attitudes (indicative of ideological conflict); and
3. the strength of the norm-political orientation correlation.

By applying these three criteria, we retained seven norm-fact pairs and reverse-scored progressive norms, so that higher scores would reflect conservative attitudes on every issue. As a result, normative attitudes correlated positively with conservatism for every issue,  $.12 < rs < .53$ . We then conformed the factual beliefs to the normative views, so that for each norm-fact pair the predicted relationship would be positive,  $.34 < rs < .77$ . To facilitate the comparison of results across issues, we applied *nonparametric*<sup>2</sup> scaling around the median in units of the interquartile range. Thus, the scaled response of participant  $i$  on issue  $j$ ,  $\underline{x}_{ij}$ , equals the difference between the response,  $x_{ij}$ , and the median response on issue  $j$ ,  $Mdn(x_j)$ , divided by the interquartile range of responses to issue  $j$ ,  $IQR(x_j)$ :

$$\underline{x}_{ij} = (x_{ij} - Mdn(x_j)) / IQR(x_j)$$

For the political orientation measure, in order to preserve the scale midpoint (representing the political center), we replaced the median value,  $Mdn(x_j)$ , with the scale midpoint (i.e., 4), so that the numerator represented the difference between the response and the scale midpoint. As a result, scaled responses reflect the number of interquartile ranges from the median (/midpoint). As such, a unit of change on the empirical belief and normative view responses approximates the magnitude of disagreement between a typically progressive (first quartile) and a typically conservative (third quartile) response on any given issue.

We fit a series of linear mixed-effect models with random effects of participants and issues. As expected, the effect of political orientation was highly significant in models of empirical beliefs ( $B = 0.22$ , 95% CI [0.13, 0.31],  $t = 4.59$ ) and normative attitudes ( $B = 0.32$ , 95% CI [0.24, 0.40]),  $t = 7.17$ ,  $ps < .001$ . Furthermore, empirical beliefs ( $B = 0.42$ , 95% CI [0.39, 0.46],  $t = 23.7$ ) and self-reported political orientation ( $B = 0.23$ , 95% CI [0.16, 0.29],  $t = 6.92$ ) each independently predicted normative attitudes, all  $ps < .001$ —indicating that normative views are shaped by both empirical beliefs and political identity (Fig. 2).

## 2.2. Participants

The politically balanced sample of 160 native English-speaking adults was recruited on Prolific to take part in the first wave of the study, out of which 146 participants also completed the second part. We did not exclude any observations; hence, the analyzed data came from 146 participants (83 female, 3 nonbinary,  $median_{age} = 31$ ,  $mean_{age} = 34.3$ ,  $sd_{age} = 13.7$ ,  $range_{age}$ : [18, 65]), of whom 65 participants reported right-wing political views.

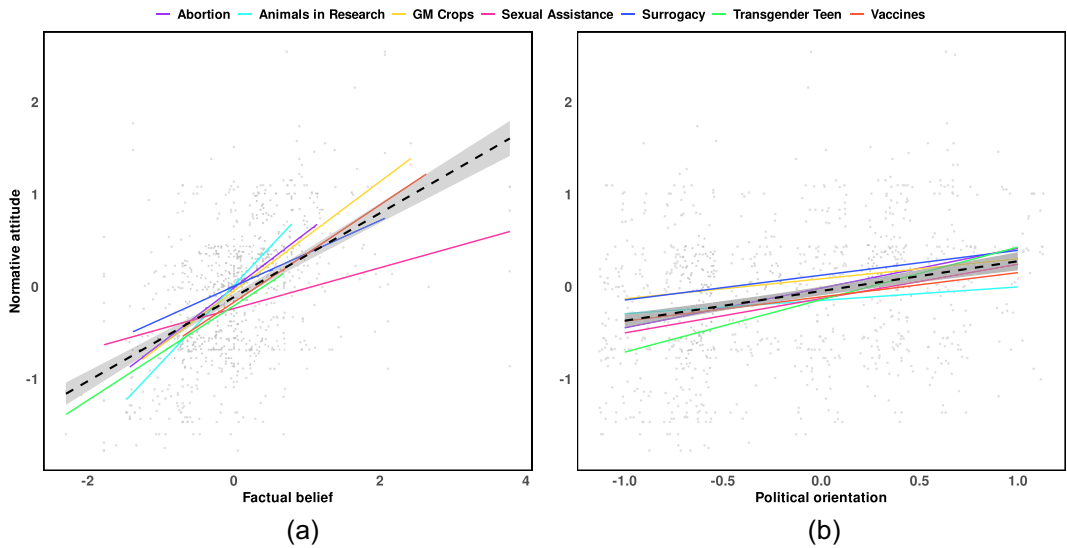


Fig. 2. Scatter plots and linear trend lines display the linear relationships between normative attitudes and (A) empirical beliefs and (B) political orientation. Dashed lines represent the relationships by issue, and the solid lines represent the aggregate relationship across issues.

The target sample size was determined using a heuristic of 150 observations per wave, aiming at a total of 300 observations across both waves. We recruited a slightly larger sample to account for the expected between-session dropout rate.

### 2.3. Procedure

The study consisted of two sessions, as depicted in Fig. 3. In the first session, participants were asked to express their normative views regarding each of the seven issues. They also answered a few basic demographic questions. After a 7-day delay, participants were invited to take part in the second session of the study. A 1-week delay was considered a trade-off between minimizing both (i) participants' recall of their preintervention responses and (ii) their likelihood of dropping out of the study (see also Helzer et al., 2017; Rehren & Sinnott-Armstrong, 2023). Participants were asked to reflect on the factual matter corresponding to one randomly drawn issue for at least 45 s. Participants were instructed to record their agreement or disagreement with the factual statement on a 100-point sliding scale, and to be prepared to explain their reasoning to others. On the following screen, participants were asked to briefly elaborate on their reasoning in writing.

Finally, participants responded to the same seven statements as in Session 1. One of the seven items constituted our *target* item (concerning the issue on which participants were asked to reflect), and we refer to the remaining six issues as *filler* items. This allowed us to check whether the manipulation selectively affected views on the target issue, or generalized to the filler issues.



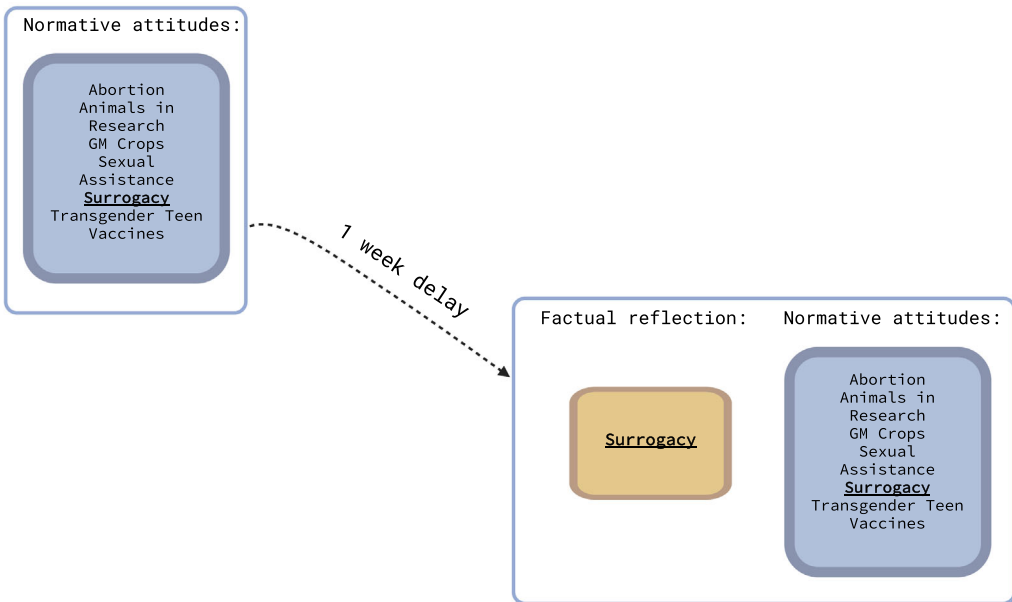


Fig. 3. Protocol in Experiment 1. The *target item* (i.e., the object of factual reflection) was selected randomly for each participant.

## 2.4. Analysis plan

### 2.4.1. Change in central tendency

The moral progress hypothesis predicts that outcome-based reflection will yield a shift in the central tendency of the distribution (as in Fig. 1A). To examine this prediction, we regressed normative attitudes at Time 2 on normative attitudes at Time 1:

$$attitude_{iT2} = a \times attitude_{iT1} + intercept$$

In this model,  $a$  refers to the stability of normative attitudes and the *intercept* describes the magnitude and direction of the shift in the central tendency of the distribution.

### 2.4.2. Change in dispersion

The moral consensus hypothesis predicts that reflection yields a shift in the dispersion of the distribution (as in Fig. 1B)—such that, for example, normative views become more similar overall. We calculated the squared deviation of every observation from the (grand) median for a given item. We then examine whether, in the aggregate, the squared dispersion measure differed between post- and pre-treatment across participants and issues:

$$dispersion = a \times time + intercept$$

Here, we investigate whether  $a$ , the effect of time (post- vs. pre-treatment) on the time-varying dispersion values, is negative or positive. A negative coefficient of time would

indicate reduced dispersion at Time 2 (relative to Time 1), and accordingly, a greater tendency toward moral consensus.

### 2.4.3. Bayesian analyses

To compare the null model (of no change in the central tendency or dispersion) to the alternative models, we calculated the corresponding Bayes factor using the *BayesFactor* package (Morey, Rouder, Jamil, & Morey, 2015) in *R* with default *ZJS* priors—as recommended in Rouder, Speckman, Sun, Morey, and Iverson (2009).

## 2.5. Results

As in the pre-test, a linear mixed-effects model treating issue as a random effect revealed that political orientation predicted normative views,  $B = 0.28$ , 95% CI [0.21, 0.35],  $t = 7.44$ ,  $p < .001$ .

### 2.5.1. Change in central tendency

Participants' normative attitudes were stable across sessions,  $B = 0.71$ ,  $p < .001$  (see Table 1). Additionally, we observed a leftward shift in normative attitudes,  $B = -0.08$ ,  $p = .025$  (as revealed by the negative intercept in Models 1 and 2). This leftward shift in normative attitudes corresponded to anecdotal evidence for the alternative over the null model (i.e., of no shift),  $BF_{10} = 2.97$ . Entering political orientation into the model did not improve model fit, whereas entering empirical beliefs at Time 2 predicted unique variance in normative attitudes and rendered the intercept nonsignificant—implying that the leftward (intercept) shift in moral attitudes from Time 1 was accounted for by participants' concurrent empirical beliefs (see Models 3 and 4), and suggesting a role for momentary outcome-based reasoning.

These effects did not generalize to control issues on which participants had not reflected (see Table 2). Critically, there was no leftward shift (in Models 1 and 2), and no relationship between participants' empirical beliefs (about the target issue) and their normative views about control issues (see Models 3 and 4).

Our final step was to compare Target and Control Models 4 by entering issue (target vs. control) as a moderator of the fixed effects of Time 1 normative belief, political orientation and Time 2 empirical belief. In doing so, we obtained evidence that (i) the leftward shift took place selectively for target issues,  $B = -0.08$ , 95% CI [-0.01, -0.15],  $t = -2.10$ ,  $p = .036$ , (ii) normative attitudes on control issues were more stable than on target issues,  $B = 0.14$ , 95% CI [0.03, 0.24],  $t = 2.62$ ,  $p = .009$ , and (iii) empirical beliefs predicted normative attitudes selectively on target issues,  $B = 0.17$ , 95% CI [0.26, 0.08],  $t = 3.61$ ,  $p < .001$ .

### 2.5.2. Change in dispersion

To examine whether momentary reflection produced changes in dispersion, we regressed the squared deviation from the median attitude (on each issue) on session as a fixed effect, with random effects of participant and issue. We observed no effect of reflection on the squared deviation from the median—whether on the target issue on which participants had reflected ( $B = -0.08$ ,  $t = -1.45$ , 95% CI [-0.19, 0.03],  $p = .15$ ), or on the control set of

Table 2  
Models of normative attitudes toward control issues in Experiment 1

	Control Model 1	Control Model 2	Control Model 3	Control Model 4
<i>AIC</i>	817.55	819.97	824.39	827.19
<i>r</i> <sup>2</sup>	.63	.63	.63	.63
(Intercept)	<i>B</i> = 0.00 95% CI [-0.03, 0.04] <i>t</i> = 0.20	<i>B</i> = 0.02 95% CI [-0.02, 0.05] <i>t</i> = 0.83	<i>B</i> = 0.00 95% CI [-0.03, 0.04] <i>t</i> = 0.21	<i>B</i> = 0.02 <i>t</i> = 0.81 95% CI: [-0.02, 0.05]
Time 1 Normative Belief	<i>p</i> = .85 <i>B</i> = 0.79 95% CI: [0.75, 0.83] <i>t</i> = 37.95 <i>p</i> < .001	<i>p</i> = .44 <i>B</i> = 0.77 95% CI: [0.73, 0.81] <i>t</i> = 36.16 <i>p</i> < .001 <i>B</i> = 0.05 95% CI: [0.01, 0.09] <i>t</i> = 2.34 <i>p</i> = .020	<i>p</i> = .84 <i>B</i> = 0.79 95% CI: [0.74, 0.83] <i>t</i> = 37.89 <i>p</i> < .001	<i>p</i> = .45 <i>B</i> = 0.77 <i>t</i> = 36.14 95% CI: [0.73, 0.81] <i>p</i> < .001 <i>B</i> = 0.05 95% CI: [0.01, 0.09] <i>t</i> = 2.26 <i>p</i> = .024 <i>B</i> = 0.02 95% CI: [-0.02, 0.05] <i>t</i> = 1.04 <i>p</i> = .30
Political Orientation	—	—	—	—
Time 2 Empirical Belief	—	—	<i>B</i> = 0.02 95% CI: [-0.01, 0.05] <i>t</i> = 1.20 <i>p</i> = .23	—

issues ( $B = -0.01$ ,  $t = -0.20$ , 95% CI  $[-0.07, 0.06]$ ,  $p = .84$ ). In Bayesian terms,<sup>3</sup> these effects corresponded to anecdotal ( $BF_{01} = 2.96$ ) and substantial ( $BF_{01} = 18.51$ ) evidence for the null, respectively.

## 2.6. Discussion

The aim of Experiment 1 was to test the impact of momentary outcome-based reasoning on moral attitudes regarding a set of real-world issues. Tentatively, the results provided support for the *progress* hypothesis: Inducing reflection may shift participants' views on various normative questions toward the characteristically liberal view (see also Luke & Gawronski, 2021). Numerically, this pattern arose on six of the seven issues (see Fig. 1). Furthermore, the manipulation appeared to selectively impact attitudes toward the target issue, and not the filler issues—providing some validation of the reflective nature of the task. However, Experiment 1 provided no support for the *consensus* hypothesis: Analyses of pre-to-post change in *dispersion* did not indicate that reflection reduced the average *deviation* from the median. Thus, while participants tended to adopt a slightly more liberal attitude selectively on the issue on which they had reflected, reflection did not appear to bring participants' attitudes closer in line.

## 3. Experiment 2

In Experiment 2, we sought to replicate the effects of momentary reflection observed in Experiment 1. To ensure the robustness and validity of our findings, we used the same material and a highly similar procedure in Experiment 2.

### 3.1. Materials

We employed the same seven norm-fact pairs as in Experiment 1.

### 3.2. Participants

We recruited a politically balanced sample of 803 native English-speaking adult residents of the United Kingdom on Prolific for the first wave of the study out of whom 651 (368 female, 5 nonbinary,  $median_{age} = 34$ ,  $mean_{age} = 36.8$ ,  $sd_{age} = 15.3$ ,  $range_{age} = [18, 79]$ ) completed the second part of the study. We did not exclude any observations, hence Experiment 2 included 651 participants. Three hundred and forty-eight participants reported right-wing political views.

Power analysis was conducted using the `pwr.f2.test` function from the `pwr` package in R (Champely et al., 2017). Assuming the minimal effect size of interest of Cohen's  $f_2 = .02$ , and power of .9, we obtained the target sample of 633 participants. We recruited a slightly larger sample to account for the expected between-session dropout rate.

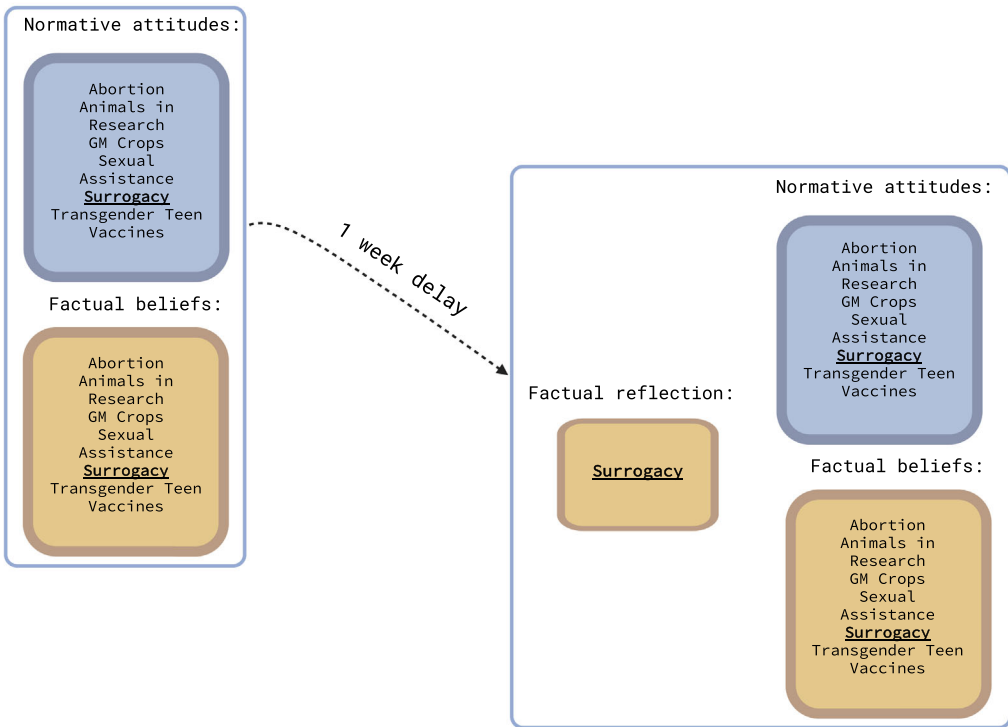


Fig. 4. Protocol in Experiment 2. The *target item* (i.e., the object of factual reflection) was selected randomly for each participant.

### 3.3. Procedure

Experiment 2 consisted of two sessions (see Fig. 4). In the first session, participants were asked to express their normative views regarding each of the seven issues. Unlike Experiment 1, in the first session of Experiment 2, participants were also asked to record their agreement or disagreement with the seven factual statements corresponding to the seven issues. They also answered a few basic demographic questions.

After a 7-day delay, participants were invited to take part in the second session of the study, and were randomly assigned to one of seven conditions. In each condition, participants were asked to reflect on the corresponding factual matter for at least 45 s. Participants were instructed to record their agreement or disagreement with the *target* factual statement on a 100-point sliding scale, and to be prepared to explain their reasoning to others. On the following screen, participants were asked to briefly elaborate on their reasoning in writing.

Finally, participants responded to the same seven normative statements as in Session 1. Unlike Experiment 1, in the second session of Experiment 2, participants were also asked to record their agreement or disagreement with the remaining six *filler* factual statements. One of the seven normative items constituted our target item (concerning the issue on which

participants were asked to reflect), and we refer to the remaining six normative statements as control items. This allowed us to assess whether the manipulation selectively affected views on the target issue, or generalized to the filler issues.

### 3.4. Analysis plan

We followed the same analysis plan that we used in Experiment 1.

### 3.5. Results

As in Study 1, normative attitudes in Session 1 were predicted by political orientation ( $B = 0.29$ , 95% CI: [0.26, 0.33],  $t = 16.10$ ,  $p < .001$ ) as well as by factual beliefs ( $B = 0.39$ , 95% CI: [0.37, 0.41],  $t = 34.67$ ,  $p < .001$ ).

#### 3.5.1. Stability and discriminant validity

The data in Study 2 also allowed us to calculate the stability and discriminant validity of our normative and factual assessments. The stability (i.e., between-session correlation) of normative attitudes ( $r = .75$ ) and factual beliefs ( $r = .69$ ) was in the range of previously reported values<sup>4</sup> (see also Models T1 and T2 in Table 3). They were also substantially higher than the within-session correlations between normative and factual measures in Session 1 ( $r = .49$ ) and in Session 2 ( $r = .48$ ), providing evidence of discriminant validity. In other words, participants treated normative and factual questions within the same session differently and, for example, did not simply *moralize* their responses to factual questions.

#### 3.5.2. Change in central tendency

We observed no overall pre-to-post change in normative attitudes,  $ps > .76$ , as revealed by the nonsignificant intercepts in Models T1 and T2. This nonsignificant effect corresponded to strong evidence for the null model (i.e., of no shift) over the alternative model,  $BF_{01} = 11.69$ . Entering factual beliefs at Time 2 predicted unique variance in normative attitudes, and partitioning this effect revealed that reflection (*change* in empirical beliefs from Time 1 to Time 2) predicted variance in normative attitudes,  $B = 0.22$ , 95% CI: [0.16, 0.27],  $t = 7.71$ ,  $p < .001$ , after controlling for both Time 1 measures and political orientation (see Model T5 in Table 3).

We observed a similar pattern of results with respect to moral attitudes toward the control issues (see Table 4). For instance, an intercept-only model revealed a nonsignificant intercept ( $B = 0.00$ , 95% CI [-0.03, 0.04],  $t = 0.15$ ,  $p = .89$ ), indicating no leftward shift (in Models C1 and C2). There was a relationship between participants' factual beliefs and the remaining normative questions (in Models C3 and C4).

#### 3.5.3. Change in dispersion

As in Experiment 1, we regressed the squared deviation from the median attitude (on each issue) on session as a fixed effect, with random effects of participant and issue. This analysis revealed no pre-to-post effect of reflection on dispersion, whether on the target issue ( $B = 0.00$ , 95% CI [-0.04, 0.05],  $t = 0.14$ ,  $p = .89$ ) or the control issues ( $B = -0.01$ , 95% CI

Table 3  
Experiment 2: Target issues

	Model T1	Model T2	Model T3	Model T4	Model T5
AIC	763.58	750.74	622.34	618.45	623.26
$r^2$	.53	.54	.62	.62	.62
(Intercept)	$B = -0.00$ 95% CI: [-0.07, 0.06] $t = -0.12$	$B = 0.01$ 95% CI: [-0.06, 0.08] $t = 0.32$	$B = -0.02$ 95% CI: [-0.09, 0.05] $t = -0.47$	$B = -0.01$ 95% CI: [-0.08, 0.07] $t = -0.14$	$B = -0.01$ 95% CI: [-0.08, 0.07] $t = -0.20$
Time 1 Normative Belief	$p = .91$ $B = 0.73$ 95% CI: [0.68, 0.79] $t = 26.27$ $p < .001$	$p = .76$ $B = 0.70$ 95% CI: [0.64, 0.75] $t = 24.31$ $p < .001$	$p = .66$ $B = 0.59$ 95% CI: [0.53, 0.65] $t = 20.08$ $p < .001$	$p = .90$ $B = 0.57$ 95% CI: [0.51, 0.63] $t = 19.33$ $p < .001$	$p = .84$ $B = 0.56$ 95% CI: [0.49, 0.62] $t = 17.62$ $p < .001$
Political Orientation	—	$B = 0.13$ 95% CI: [0.07, 0.19] $t = 4.52$ $p < .001$	—	$B = 0.09$ 95% CI: [0.04, 0.15] $t = 3.37$ $p < .001$	$B = 0.09$ 95% CI: [0.04, 0.15] $t = 3.37$ $p < .001$
Time 2 Empirical Belief	—	—	$B = 0.25$ 95% CI: [0.21, 0.30] $t = 10.61$ $p < .001$	$B = 0.24$ 95% CI: [0.19, 0.29] $t = 10.02$ $p < .001$	—
Time 1 Empirical Belief	—	—	—	—	$B = 0.26$ 95% CI: [0.21, 0.32] $t = 9.20$ $p < .001$
$\Delta$ Empirical Belief	—	—	—	—	$B = 0.22$ 95% CI: [0.16, 0.27] $t = 7.69$ $p < .001$

Table 4  
Experiment 2: Control issues

	Model C1	Model C2	Model C3	Model C4	Model C5
AIC	4053.20	4017.40	3502.07	3467.01	3475.65
$r^2$	.57	.57	.60	.61	.61
(Intercept)	$B = 0.00$ 95% CI: [-0.03, 0.04] $t = 0.15$	$B = 0.01$ 95% CI: [-0.02, 0.05] $t = 0.77$	$B = -0.00$ 95% CI: [-0.04, 0.04] $t = -0.13$	$B = 0.01$ 95% CI: [-0.03, 0.05] $t = 0.40$	$B = 0.01$ 95% CI: [-0.03, 0.05] $t = 0.39$
Time 1 Normative Belief	$p = .89$ $B = 0.74$ 95% CI: [0.72, 0.76] $t = 68.63$ $p < .001$	$p = .47$ $B = 0.72$ 95% CI: [0.70, 0.74] $t = 64.23$ $p < .001$ $B = 0.08$ 95% CI: [0.05, 0.10] $t = 6.72$ $p < .001$	$p = .90$ $B = 0.64$ 95% CI: [0.62, 0.66] $t = 52.87$ $p < .001$	$p = .70$ $B = 0.62$ 95% CI: [0.60, 0.64] $t = 50.14$ $p < .001$ $B = 0.07$ 95% CI: [0.05, 0.10] $t = 6.67$ $p < .001$	$p = .71$ $B = 0.62$ 95% CI: [0.59, 0.64] $t = 48.49$ $p < .001$ $B = 0.07$ 95% CI: [0.05, 0.10] $t = 6.68$ $p < .001$
Political Orientation	—	—	—	—	—
Time 2 Empirical Belief	—	—	$B = 0.18$ 95% CI: [0.16, 0.20] $t = 17.80$ $p < .001$	$B = 0.18$ 95% CI: [0.16, 0.20] $t = 17.68$ $p < .001$	—
Time 1 Empirical Belief	—	—	—	—	$B = 0.18$ 95% CI: [0.16, 0.20] $t = 16.24$ $p < .001$
$\Delta$ Empirical Belief	—	—	—	—	$B = 0.17$ 95% CI: [0.15, 0.20] $t = 13.71$ $p < .001$



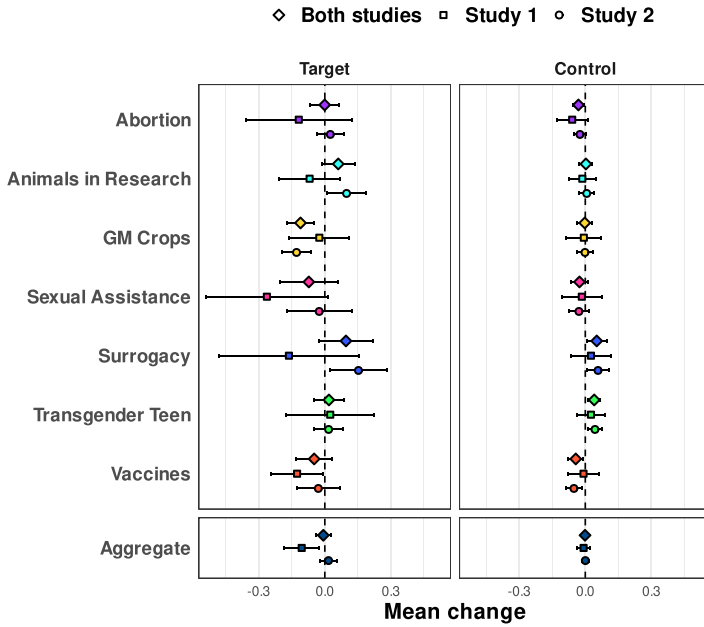


Fig. 5. Mean change in attitudes toward the seven normative statements across the two sessions of Experiments 1 and 2, comparing target (reflected) and control (unreflected) items.

$[-0.04, 0.01]$ ,  $t = -0.98$ ,  $p = .33$ ). In Bayesian terms, these effects corresponded to strong evidence (target:  $BF_{01} = 16.12$ ; control:  $BF_{01} = 24.35$ ) against the alternative hypothesis of moral consensus, according to which outcome-based reflection attenuates moral disagreement.

### 3.6. Internal meta-analysis

Finally, we estimated the meta-analytic effect sizes in our primary analyses drawing on the pooled dataset from Experiments 1 and 2 (total  $N = 748$ ). In standardized units, the effect of momentary reflection on moral attitudes toward target issues was equal to Cohen’s  $d = -0.02$ , 95% CI  $[-0.18, 0.14]$ ,  $z = -0.21$ ,  $p = .83$  (see Fig. 5). In Bayesian terms, this corresponded to strong evidence for the absence of pre-to-post change both in the central tendency ( $BF_{01} = 14.31$ ) and in the dispersion ( $BF_{01} = 14.34$ ) in moral attitudes.

With data from Study 2, we ran a further analysis to estimate the magnitude of the relationship between change in factual beliefs and normative attitude change (where change = Time 2 – Time 1). We observed a small-to-medium correlation both for target issues,  $r = .17$ , 95% CI  $[.02, .31]$ ,  $z = 2.24$ ,  $p = .025$ , and for control issues,  $r = .12$ , 95% CI  $[.07, .17]$ ,  $z = 4.61$ ,  $p < .001$ .

### 3.7. Discussion

Contrary to Experiment 1, the results of Experiment 2 revealed no change in the central tendency of participants' normative attitudes after briefly reflecting on underlying empirical matters. Thus, Experiment 2 did not replicate the findings of Experiment 1 concerning the impact of momentary outcome-based reasoning on moral attitudes toward bioethical issues (see Fig. 5). Autoregression models suggested that outcome-based reflection predicted *change* in participants' normative attitudes, though the direction of the effect was heterogeneous across participants and issues—resulting in no overall effect toward either bioliberal or bioconservative attitudes. Turning to analyses of the consensus hypothesis, we found no evidence that outcome-based reflection reduced dispersion in normative attitudes—exactly as in Experiment 1.

## 4. General discussion

In two studies, we tested the impact of momentary outcome-based reasoning on moral attitudes regarding real-world controversies in the bioethical domain. Experiment 1 suggested that briefly reflecting on the consequences of real-world bioethical practices could impact normative attitudes, with outcome-based reasoning yielding a modest shift toward progressive views. With a larger sample size, Experiment 2 did not replicate this effect—providing strong evidence for the absence of an effect. Adopting an autoregression approach, we found that people might indeed update their normative views in response to outcome-based reflection—as indicated by the significance of empirical belief *change*. Yet, shifts in normative attitudes did not occur as predicted by the progress or the consensus hypotheses. Rather, our findings suggest that shifts in people's normative attitudes occurred toward bioliberal and bioconservative conclusions equally—giving rise to the absence of an aggregate effect on the central tendency of the response distribution.

Recent replication difficulties cast doubt on the robustness of the effect of reflection on moral judgment in hypothetical cases (Herec et al., 2022). The heterogeneous effects of outcome-based reflection in our studies might prompt us to further question the influence of reflection to shift people's attitudes toward consensus or progress in the case of real-world dilemmas. On the other hand, the outcome-based reflection paradigm has previously demonstrated an impact on participants' responses to hypothetical moral dilemmas (e.g., Hannikainen & Rosas, 2019; Luke & Gawronski, 2021). In this light, the heterogeneous effects of outcome-based reflection in our studies using real-world bioethical controversies may point toward fundamental differences between hypothetical and real-world moral dilemmas, as well as differences in the cognitive processes underlying them (see Francis et al., 2016; Kneer & Hannikainen, 2022; Körner, Joffe, & Deutsch, 2019). This suggests caution in generalizing findings from the hypothetical dilemmas to real-world situations.

Previous research on moral conviction may help to illuminate this pattern of results: Studies have shown that people are reluctant to revise moral attitudes that are deeply ingrained (Aramovich et al., 2012; Heinzlmann, Höltingen, & Tran, 2021; Hornsey, Majkut, Terry, &

McKimmie, 2003; Luttrell, Petty, Briñol, & Wagner, 2016; Stanley, Dougherty, Yang, Henne, & De Brigard, 2018), and most willing to engage in outcome-based reflection on the issues they care least about (Viciano, Hannikainen, & Rodríguez-Arias, 2021). This body of research points toward a potential explanation for the discrepancy across hypothetical and real-world moral dilemmas—rooted in the idea that people’s greater moral conviction about real-world issues than about hypothetical dilemmas renders them immune to outcome-based reasoning.

In our study, participants’ search for empirical evidence was *unguided*, potentially leading to the motivated search for congenial evidence or even misinformation. Thus, future work should investigate the impact of exposure to vetted empirical information—for instance, a manipulation in which participants obtain curated information on the empirical facts relevant to the moral dilemmas under consideration. This approach might ensure that participants engage with reliable sources, and minimize the risk of misinformation shaping their moral reasoning. For instance, prompting participants to challenge their assumptions about which agents are more vulnerable in each bioethical controversy could potentially resolve disagreements (Womick et al., 2024).

Second, the mandatory period of outcome-based reasoning in our studies was less than a minute. Future research on the impact of outcome-based reasoning on moral attitudes might benefit from stronger treatments that entail a more exhaustive acquisition of novel empirical information (for instance, over the course of a semester), allowing participants to delve deeper into the empirical questions surrounding real-world bioethical controversies and potentially revealing more substantial and enduring shifts in normative attitudes. Thus, whether stronger and more sustained manipulations of outcome-based reasoning might engender moral progress or consensus remains a possibility for future research.

In sum, our research on the impact of momentary outcome-based reasoning on moral attitudes toward real-world bioethical issues provided evidence of heterogeneous effects. Despite exploratory evidence of a shift toward progressive views in response to outcome-based reflection, a larger replication attempt provided substantial evidence in favor of the null model—according to which outcome-based reflection has no overall, directional effect on bioethical attitudes. These conflicting results underscore the need for a nuanced understanding of how outcome-based reasoning influences moral attitudes about real-world issues. In future research about the impact of outcome-based reasoning, it is essential to recognize the multifaceted nature of moral reasoning and the heterogeneous effect of consequentialism on moral attitudes.

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## Notes

- 1 We are aware that people often use factual questions as a means to rationalize their moral attitudes (e.g., Ditto & Liu, 2012). In our studies, we have made our best attempt to formulate factual items for each normative issue, and have used statistical tools (such as controlling for moral attitudes at Time 1), to isolate the effect of factual reflection over and above people's expression of normative attitudes. This might be particularly difficult in some of our factual items, such as "At what age is the average teenager mature enough to decide whether to undergo such procedures?", with which we aimed to prompt reflection on how policies on teenage transitioning might undermine their capacity to make autonomous choices. We included both clear and subtle cases to ensure a representative sample of bioethics topics, even when the link to empirical consequences was not immediately obvious.
- 2 *Nonparametric* scaling (i.e., using median and IQR rather than mean and standard deviation) was implied by our expectation to observe bimodal distributions (representing polarized views). This choice not only accounted for the non-normal nature of our data but also was more sensitive to the actual position of the two expected modes.
- 3 All Bayes factors reported in this article were calculated using the BayesFactor package in R (Morey, Rouder, Jamil, & Morey, 2015). We used the default option of *ZJS* priors, as explained and defended in Rouder, Speckman, Sun, Morey, and Iverson (2009).
- 4 At the same time, the stability (between-session correlations) is not high enough to suggest people were able to recall their response from Session 1 while trying to provide consistent responses in Session 2. In a moral psychology study with a similar design (Hannikainen, Machery, & Cushman, 2018), where the two sessions were 9 years apart, the stability was only slightly lower ( $r = .67$ ) than the two coefficients reported here.

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