



Making it moral: Merely labeling an attitude as moral increases its strength



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HIGHLIGHTS

- Mere perceptions of moral (vs. non-moral) attitude bases were manipulated.
- Perceiving a moral basis increased attitude–intention consistency.
- Perceiving a moral basis also led to greater resistance to persuasion.
- These effects were not mediated by other established attitude strength indicators.

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ABSTRACT

Prior research has shown that self-reported moral bases of people's attitudes predict a range of important consequences, including attitude-relevant behavior and resistance in the face of social influence. Although previous studies typically rely on self-report measures of such bases, the present research tests the possibility that people can be induced to view their own attitudes as grounded in moral bases. This perception alone leads to outcomes associated with strong attitudes. In three experiments, participants were led to view their attitudes as grounded in moral or non-moral bases. Merely perceiving a moral (vs. non-moral) basis to one's attitudes led them to show greater correspondence with relevant behavioral intentions (Experiment 1) and become less susceptible to change following a persuasive message (Experiments 2 and 3). Moreover, these effects were independent of any other established indicators of attitude strength.

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1. Introduction

When people claim that an attitude of theirs is grounded in core moral beliefs, that attitude tends to be relatively consequential (Skitka, 2010; Skitka, Bauman, & Sargis, 2005). Research on attitudes' moral bases typically assesses people's beliefs regarding the link between their attitude and their morality with questions such as, "To what extent is your attitude about this topic a reflection of your core moral beliefs and convictions?" (Skitka, 2010). This work consistently shows that the more people report a moral basis for their attitudes, the more those attitudes predict relevant behavior (e.g. Bloom, 2013, Morgan, Skitka, & Wisneski, 2010, Skitka & Bauman, 2008, Skitka et al., 2005, Wright, Cullum, & Schwab, 2008) and resist being changed (Aramovich, Lytle, & Skitka, 2012; Haidt,

2001; Hornsey, Majkut, Terry, & McKimmie, 2003; Hornsey, Smith, & Begg, 2007).

Those two outcomes, attitude–behavior consistency and resistance to change, are critical consequences of *attitude strength*. The defining features of strong versus weak attitudes are that they are more resistant to persuasion, more stable over time, and have greater influence over cognition and relevant behaviors (Krosnick & Petty, 1995). To date, many measurable aspects of attitudes have been shown to index whether those attitudes will demonstrate one or more of the defining strength consequences. These indicators of strength include the extent to which an attitude is confidently held (Rucker, Tormala, Petty, & Briñol, 2014), ambivalent (Armitage & Conner, 2000), accessible (Fazio, 1995), rated as important (Eaton & Visser, 2008), based on high knowledge (Wood, Rhodes, & Biek, 1995), or resulted from considerable message-relevant thinking (Barden & Petty, 2008). Importantly, these strength indicators are not necessarily equally good at predicting strength consequences in all situations or for all people. Some strength indicators even

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interact with other indicators to predict strength consequences. For example, increased certainty increases resistance to change and stability over time when attitudes are relatively unambivalent but not when they are ambivalent (Clarkson, Tormala, & Rucker, 2008; Luttrell, Petty, & Briñol, 2016), and people engage in attitude-expressive behaviors more when certainty and importance are both high (Visser, Krosnick, & Simmons, 2003).

In short, the literature on attitude strength has distinguished between the critical consequences of strength (e.g., enhanced resistance to change) and the variables that can indicate whether these strength consequences are likely to occur (e.g., certainty). In this regard, studies have suggested that having a moral basis to one's attitude is a strength indicator akin to having more knowledge, or accessibility, or certainty. It is notable that the existing work on measured moral attitude bases has typically aimed to distinguish that construct from attitude strength, instead emphasizing that moral conviction is about the specific underlying content of an attitude rather than attitude strength per se (Skitka, 2014). However, although the measured moral basis of an attitude would most likely link to particular underlying content (e.g., having moral reasons for the attitude), measured moral conviction is nonetheless an attitude strength "indicator" given the prior research connecting it to strength outcomes of interest such as resistance to influence and attitude-behavior correspondence. But the question still remains: why would having a moral basis for one's attitude be associated with strength consequences? Is the moral content a necessary component or is the mere perception of a moral basis sufficient? That is the key question of this research.

One possibility is that morally based attitudes are different in some fundamental way (such as in content) from non-morally based attitudes and that this makes them stronger. Some scholars have suggested that morally based attitudes develop from stable, internal influences. Rozin (1999), for instance, discusses the process of moralization, noting that the adoption of new moral principles gives moral value to anything else that seems related to them. Additionally, some data demonstrate that more conservative ideology corresponds to a greater tendency to moralize attitudes (e.g., Jarudi, Kreps, & Bloom, 2008), but a recent meta-analysis suggests that people moralize attitudes that are important to any ideology, be it liberal or conservative (Skitka, Morgan, & Wisneski, 2015). Thus, moral attitudes could be more strongly based on ideology, rendering them more impactful. Other evidence also suggests stable internal origins by demonstrating a link between morality and the heritability of attitudes. Prior research has documented the heritability of a subset of attitudes such as those toward pre-marital sex and the death penalty (vs. those toward refugees and capitalism; Tesser, 1993). Building on this research, Brandt and Wetherell (2012) showed that even controlling for other attitude strength indicators such as certainty and importance, more heritable attitudes tended to be held with greater degrees of perceived moral basis.

Although this research provides some interesting insights into factors that could make morally based attitudes differ substantively from non-morally based attitudes, it does not address the questions of whether these substantive differences are responsible for the relationship between moral conviction and attitude strength consequences. For instance, does moral conviction predict resistance to persuasion *because* those attitudes have a stronger basis in heredity or ideology?

In contrast to the possibility that morally based attitudes are more consequential because they are substantively different from non-morally based attitudes, the current research explores the idea that the mere perception that attitudes have a moral basis is sufficient to render them more consequential even if there is no substantive difference. That is, a perceived moral basis could serve as an attitude strength heuristic similar to how other strength heuristics operate in the absence of substantive differences in the underlying nature of the attitude. For

example, although attitudes tend to be more consequential when they are the result of careful thinking (see Petty, Haugtvedt, & Smith, 1995), Barden and Petty (2008) showed that when people merely believed that they diligently thought about an issue, their attitude on the issue better predicted their behavior even if the perception of deep thought was created experimentally without any substantive basis to it. Following the methodology of Barden and Petty (2008), testing the unique role of perceived moral attitude bases requires randomly assigning some people to believe that their attitude is based in morality and other people not to.

Recently, Van Bavel, Packer, Haas, and Cunningham (2012) presented data suggesting that the same issue can indeed be framed in moral versus non-moral terms, which can correspond to some attitude strength indicators. Their participants evaluated a series of actions, sometimes using morally-framed questions ("how morally wrong/right it would be") and sometimes using non-morally-framed questions ("how personally bad/good you think it would be"). They found that people could switch flexibly between moral and non-moral evaluations, and people answered the morally-framed questions with more extreme and quickly-registered responses, compared to the non-moral questions (Van Bavel et al., 2012). It would thus seem that thinking along moral lines was associated with the attitude strength indicators of extremity (Abelson, 1995) and accessibility (Fazio, 1995), though this research did not examine attitude strength consequences.

Furthermore, the implications of these results for our proposal are seriously limited by the manipulation's emphasis on having participants make *different* evaluations in responding to the moral and non-moral questions. That is, participants could be drawing on different information in responding to each unique type of question, leading to different evaluations. In contrast, we propose that merely perceiving the *same* evaluations of the same object as morally based or not can produce attitude strength consequences. In particular, we aim to examine perceived morality's influence on two key attitude strength consequences: attitude-behavior correspondence and resistance to persuasion. Although Van Bavel et al.'s (2012) effects on extremity and accessibility suggest a relationship with indicators of attitude strength (though this might be due to differing considerations that informed responses to the moral versus non-moral questions), no work has yet shown that mere perceptions of a moral basis are consequential in producing differential attitude strength outcomes.

In addition to the Van Bavel et al. (2012) research, other studies have used manipulations to influence how much people perceive an *issue* to be a moral one, but our analysis is focused on labeling one's own *attitude* as morally based. That is, although some prior studies have manipulated moral vs. non-moral issue frames (Day, Fiske, Downing, & Trail, 2014), use of moral vs. non-moral persuasive arguments (Ferrari & Leippe, 1992), moral vs. non-moral judgment questions (Van Bavel et al., 2012), or reminders that others see a topic as morally relevant or not (Ben-Nun Bloom & Levitan, 2011), they do not show evidence that connecting the issue to the concept of morality makes people think that their own specific attitude is grounded in moral beliefs. Even more importantly, no prior work has examined whether manipulations of perceived moral bases can affect how consequential one's attitude is. Thus, although there is evidence in the domain of morality and attitude processes that utilized experimental manipulations, it remains to be shown that the perception of a moral attitude basis alone can lead to attitude strength outcomes such as persuasion resistance and engaging in attitude-consistent behavior.

By experimentally inducing the perception of a moral attitude basis, we are also able to better understand the role that various other attitude strength indicators might play in the relationship between perceived moral bases and attitude strength consequences. Specifically, are perceived moral attitude bases distinct from other established attitude strength indicators? As we mentioned earlier, many individual variables

such as certainty, ambivalence, and importance have been shown to predict attitude–behavior consistency and resistance to persuasion (i.e., attitude strength consequences) either alone or in combination. This posed an initial dilemma for attitude strength theorists. Because so many of these variables have been associated with the same set of attitude strength outcomes, some questioned whether all of these individual strength indicators are redundant with one another (e.g., Raden, 1985). This question prompted a slew of published factor and principle components analyses presenting evidence of latent structures underlying individual attitude strength indicators (Abelson, 1988; Alwitt & Berger, 1993; Bass & Rosen, 1969; Bassili, 1996; Erber, Hodges, & Wilson, 1995; Pomerantz, Chaiken, & Tordesillas, 1995; Prislin, 1996). Rarely did all of these indicators load on the same underlying factor, however. Thus, an alternative perspective is that most individual attitude strength variables are unique and reflect independent qualities of evaluation that can add to or interact with one another (Krosnick, Boninger, Chuang, Berent, & Camot, 1993; Krosnick & Petty, 1995; Visser, Bizer, & Krosnick, 2006).

Indeed, most prior research on morally based attitudes has taken the latter perspective, arguing that moral conviction is distinct from other attitude strength indicators (Skitka et al., 2005). As evidence for this claim, several studies have documented the effects of morally based attitudes, statistically controlling for other strength indicators like certainty, importance, etc. (e.g., Aramovich et al., 2012; Skitka et al., 2005; Wright et al., 2008). Any single study, however, can only account for a limited number of covariates, and indeed, several frequently studied attitude strength indicators such as accessibility have not been included in existing analyses.

By manipulating perceived moral bases independent of actual bases, however, we offer a stronger test of morality's unique attitude strength effects because variation in any other confounded strength indicators is controlled through random assignment. In addition, an experimental design allows us to establish any causal influence of perceiving a moral attitude basis on other attitude strength indicators, which could in turn mediate the effects of moral bases on attitude strength outcomes. Although self-reported moral attitude bases correlate with these other attitude strength indicators (e.g., Skitka et al., 2005), the particular nature of these relationships remains unclear, given the reliance on correlational methods.

In sum, the present research aims to manipulate perceived moral attitude bases to accomplish three important goals. First, manipulating moral conviction allowed us to test the unique role of perceived moral attitude bases because random assignment to conditions experimentally controls for any “objective” moral bases for participants' attitudes and any pre-developed notion of moral relevance (see Barden & Petty, 2008 for a similar rationale with respect to a different attitude strength indicator). Second, manipulating moral conviction also allowed us to test the role of perceived moral attitude bases independent of any pre-existing attitude strength-related attributes like certainty, ambivalence, etc. Finally, manipulating moral conviction allowed us to establish the causal influence of perceiving a moral basis for one's attitude. By relying on measurement-based methods in past research, it is unclear whether perceiving a moral basis for one's attitude leads to resistance to influence and attitude-consistent behavior or whether the association can be explained in other causal terms.

2. Hypotheses

The core idea tested in this research is that experimentally manipulating the perceived moral basis of an attitude would produce the attitude strength outcomes of enhanced attitude–behavior consistency (Experiment 1) and increased resistance to change in response to a persuasive message (Experiments 2 and 3). All prior studies examining these strength consequences have measured rather than manipulated the perceived moral basis of the attitude

and are thus open to the possibility that there were substantive differences in morally versus non-morally based attitudes that could account for the effects. Following prior work (e.g., Skitka et al., 2005), we also hypothesized that perceiving a moral basis constitutes a strength-related attitude attribute that is distinct from other established attitude strength indicators. As such, we expected induced perceptions of moral (vs. non-moral) attitude bases to affect the examined attitude strength outcomes (i.e., attitude–behavior consistency and resistance to persuasion) independent of any influence on other plausible attitude strength indicators.

3. Experiment 1

In our first study, we aimed to conceptually replicate previous correlational results linking perceived moral attitude bases with greater attitude–behavior consistency. However, rather than measuring people's pre-existing levels of these perceived bases, we manipulated whether people came to perceive their own attitudes as grounded in moral or non-moral bases. We predicted that leading people to perceive a moral (vs. non-moral) basis for their attitude would produce stronger attitude–intention correspondence even though there was no substantive difference in the actual basis of the attitudes.

More specifically, we hypothesized an interaction between attitude ratings and manipulated perceived moral attitude bases on behavioral intentions such that the ability of attitudes to predict relevant intentions would be greater for participants led to perceive a moral basis for their attitude, compared to participants led to perceive a non-moral basis for their attitude. In other words, even without influencing the extremity or actual basis (e.g., ideology) of the attitude itself, perceived moral basis would affect how strongly behavioral intentions follow from participants' attitudes. This interaction approach (versus, for instance, testing main effects on behavioral intentions) is common in attitude strength research, which has shown that attitude–behavior correlations can be moderated by variables like attitude certainty (e.g., Bizer, Tormala, Rucker, & Petty, 2006; Fazio & Zanna, 1978), ambivalence (e.g., Sparks, Conner, James, Shepherd, & Povey, 2001), and accessibility (e.g., Elliott, Lee, Robertson, & Innes, 2015).

In our first study, we used two different types of perceived morality manipulations in order to conceptually generalize across specific manipulations. Each of these manipulations was intended to guide some people to perceive a relatively greater moral basis for their attitudes than a control group, but the specific instantiation of the manipulation differed. The first manipulation provided false feedback about the bases of participants' attitude-relevant thoughts, and the second manipulation encouraged participants to reflect on the moral vs. non-moral bases of their attitude-relevant thoughts. We did not expect, however, for the specific type of manipulation to moderate the effect on attitude–intention correspondence.

We also measured two of the most commonly studied attitude strength indicators—ambivalence and certainty—as a first test of whether or not perceived moral bases operate independent of other established strength indicators. It remained an open question as to whether or not the manipulation of perceived moral attitude bases would affect these indicators. Certainty, for instance, has been shown to correlate with self-reported moral conviction (Aramovich et al., 2012; Skitka et al., 2005). We predicted, however, that induced perceptions of moral attitude bases would affect attitude–intention correspondence independent of any effects on these other measures, consistent with prior correlational research.

3.1. Method

3.1.1. Participants and design

One-hundred thirty-eight undergraduates at Ohio State University participated in partial fulfillment of an Introductory Psychology course requirement. The target sample size was driven by our focus on

moderating attitude–intention correlations, which tend to be large (for a meta-analysis, see Glasman & Albarracín, 2006). As a general rule, we aimed to have at least 60 people in each attitude basis condition to allow estimation of the correlation within each group. Indeed, prior published research using false feedback manipulations to moderate attitude–intention relationships have employed as few as 35 (Tormala, Clarkson, & Petty, 2006, Study 2) or 48 (Petrocelli, Clarkson, Tormala, & Hendrix, 2010, Study 2) total participants for a similar two-level false feedback manipulation.

Each participant was randomly assigned to one of the four conditions comprising the 2 (Perceived Attitude Basis: Moral vs. Non-Moral) \times 2 (Induction Type: False Feedback vs. Self-Reflection) between-subjects factorial design.¹

3.1.2. Procedure

Sessions were conducted in computer labs in groups of one to eleven participants at a time. Participants were separated by partitions. In order to develop initial attitudes toward a novel topic, participants began by reading an essay favoring the adoption of a senior comprehensive exam policy at their university. All participants were told that university officials had recently appointed a task force whose goal was to investigate the implementation of the new policy. The task force was said to have developed several detailed proposals in support of the policy, about which student feedback was being sought. Following this cover story, participants were exposed to a written message arguing in favor of the policy. The participants then provided their thoughts in response to the essay.

At this point, they received one of two inductions to manipulate the perception of a moral attitude basis. In the *false feedback induction*, participants were provided with bogus feedback regarding the basis of their views, being told that their thoughts related either to morality or to tradition, a control value. In the *self-reflection induction*, participants were told to expand on the thoughts they listed, focusing on how they related to core moral beliefs or on how they related to equality, another control value. Finally, participants completed all dependent measures and were debriefed and dismissed.

3.1.3. Independent variables

3.1.3.1. Perceived attitude basis and type of induction. Some participants were led to believe that their attitudes were based in morality whereas others were led to believe that their attitudes were based in a control value. Two types of inductions were used: false feedback and self-reflection. Each participant, however, was only exposed to one of the two types of inductions.

3.1.3.1.1. False feedback induction. After performing the thought-listing task, participants were told that the thoughts underlying their attitudes would be entered into a database and compared to the thoughts of other participants who had taken part in previous sessions. After an 8-second delay, introduced to make the cover story more plausible, participants received the feedback information. Those in the *moral basis* condition were told that their thoughts were especially based on morality, whereas those in the *non-moral basis* condition were told that their

thoughts were especially based on tradition, a control value. The critical portion of the feedback read as follows:

While your thoughts were similar to other students' on a variety of dimensions, you seem to have based your thoughts about senior comprehensive exams on morality [tradition] more than the average student.²

3.1.3.1.2. Self-reflection induction. After the thought-listing procedure, participants were asked to expand on the thoughts they provided. In the *moral basis* condition, they were told: "We would like you to describe how your thoughts about the exam policy relate to your core MORAL beliefs and convictions." That is, they were asked to establish a link between their thoughts and morality using an open-ended essay response. In the *non-moral basis* condition, participants were given the following instructions: "We would like you to describe how your thoughts about the exams relate to the value of EQUALITY." That is, they were asked to describe how their thoughts about the exams could potentially relate to their ideas about equality, a second control value, also using an open-ended essay response.³

3.1.4. Dependent measures

3.1.4.1. Attitudes. Participants rated their attitudes toward the examination policy using six 9-point semantic differential scales anchored at *positive–negative*, *good–bad*, *favorable–unfavorable*, *pro–con*, *beneficial–harmful*, and *wise–foolish*. Because these items showed good internal reliability ($\alpha = .96$), they were averaged to form a summary attitude index in which higher numbers correspond to more positive attitudes toward the policy. Notably, these questions follow standard attitude measurement formats and do not guide participants to judge the proposal as morally right or wrong (cf. Van Bavel et al., 2012).

3.1.4.2. Attitude strength indicators. Following the measures of attitudes, participants reported their degree of ambivalence and certainty with respect to their attitudes toward senior exams. For ambivalence, participants reported the extent to which they felt conflicted about the senior comprehensive exam policy on a 9-point scale anchored at "not at all" and "very much." For certainty, participants reported the extent which they were sure their attitude toward senior comprehensive exams was right on a 9-point scale anchored at "not sure at all" and "extremely sure."

3.1.4.3. Behavioral intentions. Following the measures of ambivalence and certainty, we presented three items to assess participants' behavioral intentions relevant to the senior exam policy. Participants reported how willing they would be to sign a petition in favor of the exam policy,

² Pilot testing ($N = 53$) indicated that morality and tradition are viewed as similarly important as potential attitudinal bases. That is, mean importance ratings for tradition ($M = 5.50, SD = 1.29, 7$ -point scale) and morality ($M = 5.79, SD = 1.11, 7$ -point scale) as attitude bases were not different from each other, $t(52) < 1$. In another pilot test ($N = 46$), we examined whether these perceptions of importance were similar when framed in the context of the senior comprehensive exam issue specifically. Once again, mean importance ratings did not differ between tradition ($M = 3.59, SD = 1.09$) and morality ($M = 2.57, SD = 1.07$) as attitude bases, $t(45) < 1$. This was also the case when these bases were framed as bases underlying either *positive* or *negative* attitudes toward the senior comprehensive exam policy, $p_s > .30$.

³ In another pilot test ($N = 38$), morality and equality were also viewed as similarly important as potential attitudinal bases. That is, mean importance ratings for equality ($M = 3.79, SD = 1.07, 5$ -point scale) and morality ($M = 3.84, SD = 1.00, 5$ -point scale) as attitude bases were not different from each other, $t(37) < 1$. However, in a separate pilot test ($N = 46$), when these potential attitude bases were presented specifically in the context of the senior comprehensive exam issue, participants rated equality as a *more* important basis ($M = 3.59, SD = 1.09$) than morality ($M = 2.57, SD = 1.07$), $t(45) = -5.17, p < .001$. This was also the case when these bases were framed as bases underlying *positive* attitudes, $t(45) = -4.19, p < .001$, but there was no difference when they were framed as bases underlying *negative* attitudes, $t(45) = -.36, p = .72$. Overall it seems that the equality value makes for an especially stringent control against which to test the perceived morality hypotheses.

¹ Because we were testing the correlations between novel attitudes and relevant behavioral intentions, moderated by perceived moral basis, it was necessary to ensure variance in attitudes toward the target topic. Thus, we manipulated the convincingness of the message that participants initially received so that some would have relatively more favorable attitudes toward the topic than others (Petty & Cacioppo, 1986; for a similar approach, see Fabrigar et al., 2006). Indeed, participants who read the strong message in favor of senior exams reported more positive attitudes ($M = 5.87, SD = 1.73$) than those who read the weak message ($M = 4.61, SD = 2.14$), $F(1, 130) = 13.88, p < .001, \eta_p^2 = .10$. Thus, the messages produced the intended variation in attitudes. However, the message strength manipulation did not qualify any of the reported effects, and controlling for this manipulation did not change the significance of any of the reported effects either. Thus, for clarity, we have dropped this manipulation as a factor in the reported analyses.

how willing they would be to put their names on a list of students who favor the exam policy, and which way they would vote on the exam policy, all on 9-point scales. Responses to these three questions were internally consistent ($\alpha = .96$) and were averaged to form an index of behavioral intentions whereby higher values reflected greater intentions to engage in pro-policy behaviors. Behavioral intentions are typically the best self-report predictor of actual behavior (Fishbein & Ajzen, 1975).

3.2. Results

3.2.1. Attitudes and behavioral intentions

Data were first submitted to a 2 (Perceived Attitude Basis: Moral vs. Non-Moral) \times 2 (Induction Type: False Feedback vs. Self-Reflection) analysis of variance (ANOVA) to test the effects of the manipulations on participants' initial attitudes toward senior exams. As expected, there was no main effect of perceived attitude basis ($p = .58$) or induction type ($p = .54$), nor was there an interaction ($p = .19$). Next, the data were submitted to another 2 \times 2 ANOVA, entering behavioral intentions as the dependent variable. Paralleling the attitude findings, there was no main effect of perceived attitude basis ($p = .69$) or induction type ($p = .13$), nor was there an interaction ($p = .18$).

3.2.2. Perceived morality and attitude–intention correspondence

Once again, our analytical approach to testing for attitude–behavior correspondence mirrors the method common among attitude strength research. That is, because attitudes tend to correlate with intentions to engage in relevant behaviors (e.g., increasingly positive attitudes toward senior exams are associated with increasing intentions to engage in pro-exam behaviors), we tested whether this relationship is stronger for people who perceive a moral basis for their attitudes than for people who perceive a non-moral basis for their attitudes. In other words, we tested an interaction between attitudes toward senior exams and the perceived attitude basis manipulation. However, in order to also examine whether this effect depended on the specific method used to induce the perceptions of a moral attitude basis, the data were submitted to a hierarchical multiple regression model that tested a three-way interaction predicting behavioral intentions. Attitudes toward senior exams, the attitude basis feedback manipulation, and the type of manipulation were entered in the first step of the model; the attitudes \times attitude basis, attitudes \times type of manipulation, and type of manipulation \times attitude basis interaction terms were entered in the second step; and the attitudes \times attitude basis \times type of manipulation interaction term was entered in the third step. Results are interpreted from the first step of the model in which they appear.

As expected, there was a main effect of attitudes on behavioral intentions such that more positive attitudes corresponded to greater intentions to engage in pro-policy behaviors, $B = .94$, $t(134) = 15.99$, $p < .001$, 95% CI: [.82, 1.05]. There was no main effect of the moral basis condition on intentions, $p = .17$, but there was an unexpected (though conceptually uninteresting) marginal main effect of type of manipulation: behavioral intentions were higher following a false feedback manipulation than following a self-reflection manipulation, $B = -.21$, $t(134) = -1.73$, $p = .09$, 95% CI: [–.44, .03].

Most importantly, consistent with our prediction that perceiving a moral (vs. non-moral) basis would affect the correspondence between the attitudes and relevant behavioral intentions, there was a significant interaction between attitudes and perceived moral basis, $B = .13$, $t(131) = 2.20$, $p = .03$, 95% CI: [.01, .25] (Fig. 1). Participants led to perceive a relatively moral attitude basis showed a stronger correlation between attitudes and intentions, $B = 1.06$, $t(131) = 13.09$, $p < .001$, 95% CI: [.90, 1.22], compared to participants who were told that the basis of their attitudes was either tradition or equality, $B = .80$, $t(131) = 9.45$, $p < .001$, 95% CI: [.63, .97]. No other two-way interactions were significant, $ps > .10$, and the attitude \times attitude basis interaction was not further qualified by the type of manipulation, $p = .26$.

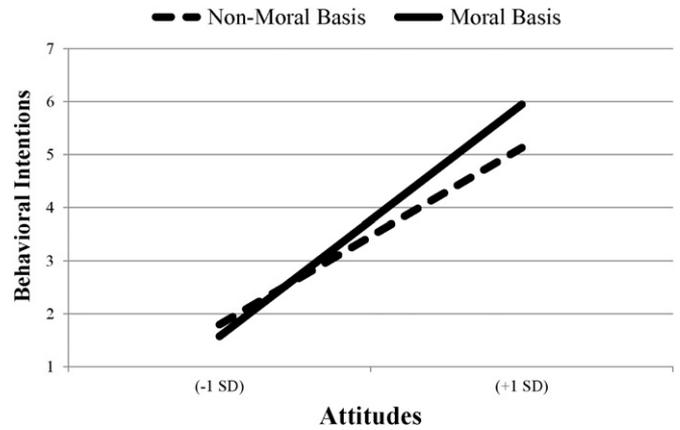


Fig. 1. Attitudes toward a senior exam policy correlate with pro-policy behavioral intentions more strongly when participants were led to perceive a moral basis for those attitudes, compared to participants who were led to perceive a non-moral basis ("tradition" or "equality") for those attitudes.

3.2.3. Other strength indicators and attitude–intention correspondence

Finally, we conducted a series of analyses to test whether other attitude strength indicators, namely ambivalence and certainty, could explain the effects of the moral basis manipulation. First, a t-test analysis revealed that mean ambivalence scores did not differ between the moral basis ($M = 4.63$, $SD = 2.23$) and non-moral basis conditions ($M = 4.69$, $SD = 2.19$), $t(136) = .17$, $p = .87$. Another t-test analysis, however, revealed that people were more sure of their attitudes toward senior exams in the moral basis ($M = 6.41$, $SD = 1.70$) than in the non-moral basis conditions ($M = 5.78$, $SD = 2.04$), $t(136) = -1.99$, $p = .05$, 95% CI: [–1.27, –.003], $d = -.34$.

To test whether the effect on certainty mediated the previously reported effect on attitude–behavior consistency, we submitted the data to a conditional process analysis using Model 15 in the PROCESS macro for SPSS (Hayes, 2013) with bootstrapping set to 10,000 iterations. The model was set up such that attitude certainty would mediate an effect of perceived moral basis on behavioral intentions. Attitude ratings were set as a moderator of both the b path (the effect of certainty on behavioral intentions) and the c path (the effect of perceived moral basis on behavioral intentions). As in the previously reported regression models, manipulation type was entered as a covariate. Results failed to support certainty as a mediator for the effect, 95% CI [–.03, .02]. Indeed, the previously reported interaction between perceived moral basis and attitudes on behavioral intentions remains significant when adding certainty as a covariate in the regression model, $B = .12$, $t(130) = 2.02$, $p = .05$, 95% CI: [.00, .23].

3.3. Discussion

Participants who were told their views had a moral basis showed greater attitude–intention correspondence than those who were told their attitudes were based in tradition or equality. This study provides the first evidence that manipulating the perceived moral basis of attitudes can influence an important attitude strength outcome. Furthermore, the study showed that the impact of perceived moral bases on attitude–intention consistency was not mediated by increases in attitude certainty, which was also affected by the manipulation.

Some readers may be struck by the lack of a main effect of the perceived attitude basis manipulation on behavioral intentions. After all, prior work on moral conviction has typically observed main effects of moral conviction on behavior and behavioral intentions. Notably, however, these past studies have tended to measure behaviors that are inherently attitude-consistent, which makes a main effect approach sensible. For example, data show that moral convictions about one's

political preferences are associated with voting (or intending to vote) in elections (Morgan et al., 2010; Skitka & Bauman, 2008). The voting measures, however, are not specific to how one votes, but simply voting in general (with the assumption that people are voting for their preferred candidate or policy positions). In our study, average attitudes were relatively neutral with some students favoring the exams and some opposing them. Thus, the behavioral intentions measures were not inherently attitude consistent (i.e., they measured exam-supportive intentions). Because of this, we would not necessarily expect an overall main effect of the manipulation on behavioral intentions. Rather, if perceived moral (vs. non-moral) bases indicate a stronger attitude, we would expect the variation in those attitudes to more strongly correlate with intentions (i.e., pro-exam people are even more likely to engage in exam-supportive behaviors and anti-exam people are even less likely to engage in exam-supportive behaviors) when people perceive a moral (vs. non-moral) basis for those attitudes.

There are some limitations to the study, however. First, some may argue that the values we used as controls were not perfect because they may have been narrower in scope than morality is. Although established accounts of values emphasize their generality and abstractness as key features (Schwartz & Bilsky, 1987, 1990), other theories that are focused on morality (e.g., *moral foundations theory*; Haidt & Graham, 2007) identify constructs like “equality” and “tradition” as individual components of morality. Thus, it may be difficult to cleanly interpret a comparison between morality and these individual values. In the next study, we employ a new control value, intended to provide an even stronger comparison condition.

Second, we did not directly measure perceived moral bases following the manipulation. Thus, Experiment 2 aims to establish whether the manipulation affects the same perceptions that were captured in prior research that measured moral bases and whether this perception mediates attitude strength consequences. Finally, although Experiment 1 examined the potential contribution of two alternative indicators of attitude strength—certainty and ambivalence—there are still other possible indicators that might account for the impact of perceived morality on attitude strength consequences. For example, perhaps attitudes based on morality are seen as more important or central to the self than those not based in morality and thus people were more willing to act on them. Thus, a goal of Experiment 2 was to assess a broader array of plausible alternative strength indicators.

4. Experiment 2

In Experiment 2, we examined resistance to persuasion as an important attitude strength outcome. To extend the results of Experiment 1, we also changed the topic to one that participants were already familiar with (recycling), allowing us to examine these effects as they related to pre-existing attitudes. We also measured a more extensive set of other attitude strength indicators that have been associated with strength outcomes in prior research. This allowed us to provide a more complete test of whether or not perceived moral basis operates through other established attitude strength indicators or whether it should be considered a separate indicator in its own right. Finally, a direct measure of perceived moral basis, drawn from the existing literature, was included to examine whether the manipulation affects the same perceptions of moral bases considered in prior correlational research.

To simplify the design, we employed only one manipulation of perceived moral bases—the false feedback induction from Experiment 1. In the previous experiment, the type of manipulation did not matter for the overall effect. Conceptually, however, the false feedback induction (i.e., simply getting feedback that your attitude-relevant thoughts reflect moral, vs. non-moral, bases) may be a stronger test of the power of “mere perception” because it does not lead participants to explicitly elaborate on the linkages between their attitudes and their actual moral beliefs. We also turned to a different control value in this study—pragmatism. This value seemed somewhat broader in scope

than those used as controls in Experiment 1 and indeed, prior research has often used pragmatism as a comparison to morality (e.g. Kreps & Monin, 2014, Van Bavel et al., 2012).

We predicted that people who come to perceive a moral basis for their attitude would show greater resistance to persuasion (i.e., less attitude change), compared to people who come to perceive a non-moral basis for their attitude. Further, we predicted that this effect would be specifically mediated by perceived moral basis and not by other established attitude strength indicators like certainty, importance, ambivalence, etc.

4.1. Method

4.1.1. Participants and design

Seventy-three undergraduates at Ohio State University participated in partial fulfillment of an Introductory Psychology course requirement. Focusing on comparing means between two groups, we aimed to collect at least 30 participants per condition (scheduling variables produced slightly higher sign-ups) to reliably estimate degree of persuasion in each. This exceeds the rule of thumb provided by Simmons, Nelson, and Simonsohn (2011). It also exceeded that of prior research that has manipulated inferences about attitude properties to test their influence on resisting persuasion (e.g., Tormala & Petty, 2002, Study 3).

Each participant was randomly assigned to one of two conditions: moral vs. practical attitude basis. Five participants (two in the *moral* basis condition; three in the *practical* basis condition) were removed from the analyses, however, because they spent less than six seconds reading the critical persuasive message. They were clear outliers; five participants spent less than 6 s on the essay screen, but otherwise the lowest amount of time spent on the screen was 20 s. Put another way, the reading time for these five participants was more than 2 standard deviations below the mean. Not surprisingly, including these participants renders the reported significant results only marginally significant. The final sample size for this study, then, is 68 participants.

4.1.2. Procedure

Like Experiment 1, sessions were conducted in computer labs in groups of one to eleven participants at a time. Participants were separated by partitions. First, participants read a brief introduction to the topic of recycling, which included background information about the process of recycling and the prevalence of it in the United States. Next, participants were asked to list the thoughts they had about recycling. At this point, participants were provided with bogus feedback regarding the basis of the thoughts behind their attitudes, being told that their thoughts related either to morality or to practicality, a control value.⁴

Upon receiving this feedback, participants reported their attitudes toward recycling along with a collection of attitude strength variables. Finally, participants received a counter-attitudinal persuasive message. Because participants entered the study with relatively positive attitudes toward recycling, the persuasive message was composed of moderately strong arguments against the benefits of recycling (284 words). After reading this message, participants reported their recycling attitudes once again. At this point, they responded to several manipulation checks and read a full debriefing.

⁴ As in Experiment 1, pilot testing ($N = 38$) indicated that morality and practicality are viewed as similarly important as potential attitudinal bases. That is, mean importance ratings for practicality ($M = 3.97$, $SD = .85$, 5-point scale) and morality ($M = 3.84$, $SD = 1.00$, 5-point scale) as attitude bases were not different from each other, $t(37) < 1$. However, in a separate pilot test ($N = 46$), when these potential attitude bases were presented specifically in the context of the topic of recycling, participants rated practicality as a more important basis ($M = 3.67$, $SD = .82$) than morality ($M = 3.22$, $SD = .87$), $t(45) = -2.53$, $p = .02$. Thus it seems that this control value is an especially stringent test of the perceived morality hypothesis.

4.1.3. Independent variable: attitude basis

As in the *false feedback induction* in the previous experiment, participants were told that the thoughts they listed were submitted to a computerized analysis that compared them to thoughts generated by other students. To provide a control value that would believably apply to participants' attitudes toward recycling, and to generalize the results of Experiment 1's manipulation, the feedback in the control condition told participants that their reactions to recycling were consistent with practicality. Using this value as a control also fits with emerging research comparing perceptions of speakers who moralize an issue to perceptions of speakers who offer a practical/utilitarian basis for their position (e.g., Kreps & Monin, 2014).

4.1.4. Dependent measures

4.1.4.1. Pre-message attitudes. Attitudes toward recycling were assessed using the same six 9-point semantic differentials used in Experiment 1. Because these items showed good internal reliability ($\alpha = .86$), they were averaged to form a summary attitude index in which higher numbers correspond to more positive attitudes toward recycling.

4.1.4.2. Attitude strength indicators. Eight different indicators of attitude strength that are fairly common in the literature were measured: extremity, certainty, ambivalence, importance, centrality, perceived accessibility, perceived elaboration, and perceived knowledge (Petty & Krosnick, 1995). The measure of evaluative extremity was computed by subtracting the attitude scores from five and taking the absolute value of the outcome. That is, extremity is indicated by the overall attitude's distance from the midpoint (i.e., 5) in either the positive or negative direction. The other attitude strength variables were assessed using a collection of measurement items adapted from prior research in the attitude strength literature (for a review of attitude strength measurement procedures, see Visser et al., 2006; Wegener, Downing, Krosnick, & Petty, 1995). See supplemental materials for the items used to assess all attitude strength indicators.

4.1.4.3. Post-message attitudes. Following the persuasive message, participants rated their attitudes toward recycling again. The measure was varied somewhat to discourage participants from simply indicating the same responses they had given not long before receiving the message. Post-message attitudes were indicated on three 9-point semantic differentials with *bad, dislike*, and *negative* at the low end (“−4”) and *good, like*, and *positive* at the high end (“+4”). These items were averaged to form an overall post-message attitude index ($\alpha = .96$). Because both pre- and post-message attitudes were reported using 9-point scales, we also computed an index of attitude change by subtracting the average pre-message attitudes from the average post-message attitudes, resulting in a measure of attitude change in the direction of the position advocated by the message.

4.1.5. Manipulation checks

4.1.5.1. Perceived moral basis. In the same section of the study where we assessed the other attitude strength indicators, we also measured how much participants thought their recycling attitudes related to their core moral beliefs. This measure was included for two reasons. First, it serves as a manipulation check to show that the feedback participants received had its intended effect. Second, it serves to link the results of the manipulation to prior research on perceived moral attitude bases. Because previous studies measured rather than manipulated moral basis, it was important to show that our manipulation affects attitude outcomes through the same perceptions of moral bases studied elsewhere in this literature. Thus, we adopted the measurement approach advocated by Skitka (2010) and simply asked participants: “To what extent is your attitude about recycling a reflection of your core moral beliefs and convictions?” This item was embedded in the list of other

attitude strength items, and participants responded on a 5-point scale anchored at “not at all” and “extremely.”

4.1.5.2. Perceived practicality basis. Participants also reported their perceived practicality basis for their recycling attitudes. It was important to test the possibility that rather than reflecting something about moral bases, any effect of the manipulation could instead be driven by instilling a perceived practicality basis in some of our participants. To this end, at the conclusion of the study, we assessed the extent to which participants perceived their attitudes about recycling to be related to practical concerns. They responded to this question on a 9-point scale anchored at “not at all” and “extremely.”

4.1.5.3. Believability check. Finally, to make sure that the morality feedback was not any more or less believable than the practicality feedback, at the end of the study, we told participants: “As a final control measure, we would like to ask you about the feedback we provided regarding your thoughts in response to recycling (i.e., when we compared your thoughts about recycling with the database of other students' thoughts).” They then responded to the following believability question on a 9-point scale anchored at “extremely inaccurate” and “extremely accurate”: “To what extent would you say that the feedback we provided was accurate?”

4.2. Results

4.2.1. Manipulation checks

Data were first submitted to t-test analyses to see whether the feedback manipulation influenced participants' perceptions of moral and practical bases for their recycling attitudes. First, participants who received feedback that their thoughts about recycling were especially reflective of morality went on to report that their recycling attitudes were more connected to their core moral beliefs ($M = 3.58, SD = .97$) than participants who were told their thoughts were especially reflective of practicality ($M = 2.94, SD = 1.08$), $t(66) = -2.53, p = .014, d = -.62, 95\% \text{ CI: } [-1.10, -.12]$. Similarly, participants who had been told that their recycling thoughts were particularly consistent with practicality concerns went on to report that their attitudes were more reflective of practicality concerns ($M = 6.64, SD = 1.96$) than those who had been told that their thoughts were consistent with moral beliefs and convictions ($M = 5.88, SD = 1.87$), $t(66) = 1.86, p = .067, d = .45, 95\% \text{ CI: } [-.03, .93]$. Importantly, participants did not report believing the feedback any more or less depending on whether it said their thoughts reflected practical concerns vs. moral beliefs and convictions, $t(66) = .83, p = .41$.

4.2.2. Pre-message attitudes and attitude strength

The data were next submitted to several t-tests to see whether the manipulation of perceived moral basis affected initial (pre-message) recycling attitudes themselves or their corresponding indications of attitude strength. First, initial attitudes toward recycling did not differ as a result of the feedback manipulation, $t(66) = .24, p > .81$.

Further, even though the feedback manipulation affected perceived moral attitude bases, there were no significant differences between morality and practicality feedback on any of the standard attitude strength indicators: certainty, ambivalence, importance, perceived knowledge, perceived elaboration, perceived accessibility, centrality, and extremity, $ps > .200$.

4.2.3. Attitude change

We tested attitude change in two ways. First, an additional t-test was conducted on attitude change scores. As predicted, the participants who had been given moral basis feedback evinced less attitude change in response to the message ($M = .92, SD = .80$) than participants who had been given practical basis feedback ($M = 1.64, SD = 1.58$), $t(66) = 2.36, p = .021, d = .58, 95\% \text{ CI: } [.08, 1.06]$. Notably, after entering the

seven attitude strength indicators as covariates, the effect of the feedback manipulation on attitude change remains significant, $F(1, 58) = 6.86, p = .011$.

Alternatively, attitude change could be treated as the effect of the manipulation on post-message attitudes, controlling for pre-message attitudes. Results of this analysis show that although pre-message attitudes are a significant predictor of post-message attitudes, $F(1, 65) = 16.83, p < .001$, there is still an additional effect of the manipulation on post-message attitudes. In particular, attitudes toward recycling are more positive in the morality feedback condition ($M = 7.56, SD = .96$) than in the practicality feedback condition ($M = 6.88, SD = 1.74$), which is a significant difference even after accounting for pre-message attitudes, $F(1, 65) = 5.41, p = .023, \eta_p^2 = .08$. That is, those led to believe that their recycling attitudes were grounded in morality were more resistant to the anti-recycling message than those led to believe that their attitudes were grounded in practicality. This result remains significant after additionally controlling for the seven measured attitude strength indicators, $F(1, 58) = 6.86, p = .011$.

4.2.4. Mediation

Although the results of the previously reported analyses demonstrated that perceived moral relevance (and not other attitude strength variables) differed between morality and practicality feedback conditions, it remained to be seen whether this difference explained the effect on attitude change. To test the role of perceived moral relevance as a mediator of the effect of attitude basis feedback on attitude change, data were submitted to tests of mediation using the PROCESS add-on for SPSS (Hayes, 2013). First, a model identifying perceived moral relevance as the mediator for the effect of feedback on attitude change scores was run. In addition to the previously reported effects of the feedback manipulation on perceived moral relevance, $t(66) = 2.54, p = .014$, and on attitude change, $t(66) = -2.36, p = .021$, perceived moral relevance was also significantly negatively related to attitude change, controlling for the effect of the manipulation, $B = -.48, t(65) = -3.39, p = .001$. Also, when entering self-reported perceived moral basis as a predictor, the manipulation of attitude basis no longer significantly influenced attitude change, $B = -.42, t(65) = -1.41, p = .163$. Most importantly, however, the indirect effect of moral vs. practical basis feedback on attitude change through perceptions of moral relevance, tested with a bootstrapping method with 10,000 iterations, was statistically significant, $B = -.30, SE = .18, 95\% CI: [-.80, -.05]$.

To test whether the manipulation affected attitude change indirectly via any of the other measured attitude strength variables, we ran an additional mediation model, entering the seven other attitude strength measures as additional independent mediators. Only perceived moral basis emerged as a significant mediator, $B = -.32, SE = .20, 95\% CI: [-.87, -.04]$. For all competing mediators, the 95% confidence interval for the indirect effect includes zero.

As before, attitude change could be tested by entering post-message attitudes as the dependent variable and entering pre-message attitudes as a covariate. Results of a mediation analysis using this approach mirror those with attitude change scores. In this analysis, perceived moral relevance was significantly positively related to post-message attitudes, controlling for pre-message attitudes and the feedback manipulation, $B = .54, t(64) = 3.77, p < .001$. Also, when entering perceived moral relevance as a predictor, the manipulation of attitude basis no longer significantly affects post-message attitudes above and beyond pre-message attitudes, $B = .37, t(64) = 1.24, p = .219$. Most importantly, however, the indirect effect of morality vs. practicality feedback on post-message attitudes, controlling for pre-message attitudes, through perceptions of moral relevance, tested with a bootstrapping method with 10,000 iterations, was statistically significant, $B = .35, SE = .20, 95\% CI: [.07, .89]$.

As before, when entering the other strength indicators as additional independent mediators, only the indirect effect of the manipulation

through perceived moral basis emerges as significant, $B = .29, SE = .17, 95\% CI: [.05, .79]$. For all other attitude strength variables, the 95% confidence interval for the indirect effect includes zero.

4.2.5. Perceived practicality basis

Given the absence of a pure control group with no mentioned basis, it is important to test for the possibility that the effects above are alternatively or additionally due to the non-moral basis condition. That is, perceiving one's attitude as based in morality may not have led to resistance as much as perceiving one's attitude as based in practicality led to susceptibility to change. Put simply, seeing one's attitude as based in practicality might open it up for influence (i.e., "changing is the practical thing to do"). Furthermore, consistent with previous research on matching effects in persuasion (e.g. Maio et al., 2014, See, Petty, & Fabrigar, 2008), it was possible that the persuasive message used arguments that unintentionally appealed to practicality concerns. If this were the case, the effect of the manipulation on attitude change could instead be due to the increased persuasive impact of practical message arguments for people who perceive their attitudes to be based on practical concerns. Therefore, we tested whether the manipulation's effect on perceived practicality basis accounted for the effect on attitude change instead of or better than perceived moral relevance.

Data were submitted to a test of dual mediation using the PROCESS add-on for SPSS, identifying a model with perceived moral basis and perceived practical basis as independent mediators of the effect of feedback on attitude change scores. When entered as simultaneous predictors, controlling for the feedback manipulation, perceived moral basis continued to demonstrate a significant negative relationship with attitude change, $B = -.46, p = .002$, but perceived practical basis did not significantly predict attitude change, $B = -.05, p = .52$. More importantly, only the indirect effect (tested with a bootstrapping method with 10,000 iterations) of feedback on attitude change via perceived moral relevance emerged as significant, $B = -.29, SE = .18, 95\% CI: [-.81, -.05]$. There was no indirect effect via perceived practicality basis, $B = .04, SE = .08, 95\% CI: [-.06, .26]$. Indeed, the indirect effect is also nonsignificant when perceived practical basis is treated as the only mediator in the model, $B = -.08, SE = .08, 95\% CI: [-.34, .02]$. Therefore, it seems that the effect of the manipulation on resistance to persuasion is due to inferences of moral basis and not due to perceptions of an attitude based on practicality.

4.3. Discussion

These results extend those of the previous experiment to another important attitude strength consequence: resistance to persuasion. Following a persuasive message, participants who were merely led to perceive that their attitudes were tied to their moral beliefs changed those attitudes less than participants led to perceive an equally important but non-moral basis for their attitudes. Self-reported moral attitude basis, and no other strength indicators, mediated this effect. We note, however, that some have criticized the use of manipulation checks as mediators (Fiedler, Schott, & Meiser, 2011), so we caution against drawing strong conclusions from these results. It is nonetheless noteworthy that only the perceived moral basis measurement (negatively) predicted attitude change whereas perceived practical basis did not, despite both measures being affected by the manipulation and serving as manipulation checks.

There still remains a possible alternative explanation for this effect. We have argued that because the perceived practical basis of one's attitudes did not mediate the effect of the manipulation on persuasion, a *matching* of a perceived practical basis to a perceived practical message explanation is unlikely. However, it is possible that if our message unintentionally focused on practical recycling arguments, a perceived moral basis would *mismatch* to this message type, leading to reduced persuasion. That is, when people were led to perceive a *moral* basis for their attitudes, they were especially resistant to persuasive arguments that

appealed to practicality. In this case, perceived moral basis could mediate the effect because such individuals were especially resistant to the use of practical (non-moral) arguments. Thus, the next experiment aims to address this mismatching alternative explanation more directly.

5. Experiment 3

The primary purpose of Experiment 3 was to replicate the preceding effects on resistance to persuasion and address the possibility that Experiment 2's results were driven by mismatching a practical message to a moral basis. To do so, we conducted a nearly identical experiment to that of Experiment 2; however, we modified the persuasive message so that it conveyed a more *moral* frame to the essay. This version of the procedure provides an even more stringent test of the hypothesis that perceiving one's attitudes as morally based leads to greater resistance to persuasion (as opposed to a mismatch effect whereby resistance arose from a mismatch to a practicality-oriented message).

The new message used in Experiment 3 was designed to contain elements that appealed to morality while still maintaining some elements that appealed to practicality (see Supplementary materials for the pilot testing of the revised persuasive message).⁵ That is, the new message was designed to match or mismatch practicality and morality equally well. Our hypothesis was that this more balanced message would replicate the pattern from Experiment 2 such that individuals with a perceived moral basis to their attitudes would show more resistance to persuasion than individuals with a perceived practical basis to their attitudes.

Finally, a potential concern about the preceding studies is that they all draw on college student samples. It is possible that such samples think of morality differently than other groups of people, so Experiment 3 was administered on Amazon's *Mechanical Turk* platform in order to increase the generalizability of the findings.

5.1. Method

5.1.1. Participants and design

An a priori power analysis was conducted based on the key effect sizes in Experiment 2 to arrive at the sample size for Experiment 3. Analyses were conducted using *G*Power* (Faul, Erdfelder, Buchner, & Lang, 2009), entering the attitude change effect size from Experiment 2. Results of this analysis conclude that the desired sample size for a two-tailed test ($\alpha = .05$) with .80 power is $N = 96$. We therefore recruited 100 U.S. participants using Amazon's *Mechanical Turk* program (54% male, 46% female; $M_{\text{age}} = 39.23$, $SD = 12.97$).

As in Experiment 2, each participant was randomly assigned to one of two conditions: moral vs. practical attitude basis. As in Experiment 2, several participants spent a relatively short time on the screen containing the persuasive message. We maintained the exclusion criterion from Experiment 2 and excluded participants who spent less than 6 s on the essay screen ($N = 6$; four in the moral basis condition and two in the practicality basis condition). Keeping these participants in the full sample, however, does not change the significance of the results. The final sample size for this study, then, is 94 participants.

5.1.2. Procedure

Like Experiment 2, participants first read a brief introduction to the topic of recycling and were then asked to list the thoughts they had about recycling, after which they were provided the same bogus feedback as in Experiment 2. Upon receiving this feedback, participants reported their attitudes toward recycling and were then presented with

⁵ Additional pilot testing revealed that people did indeed perceive that the message used in Experiment 2 appealed more to practicality concerns ($M = 5.13$, $SD = 1.09$) than it did to moral concerns ($M = 3.80$, $SD = 1.28$), $t(45) = -5.80$, $p < .001$. These pilot data also showed, however, that the new message used in Experiment 3 remedies this issue because the new message was rated as appealing equally to morality ($M = 4.94$, $SD = 1.53$) and practicality ($M = 5.34$, $SD = 1.30$), $t(49) = -1.40$, $p = .17$ (see Supplementary materials for full details on these data).

a counter-attitudinal persuasive message. Importantly, this message was similar to the one used in Experiment 2; however, it had been modified such that the arguments were re-worded or re-framed to reflect moral concerns. For example, the statement from the message used in Experiment 2 which said that "recycling may not be as beneficial as previously thought" was reworded to say that "recycling is ultimately an immoral way to deal with waste." As another example, the statement from the message in Experiment 2 which said that "the process of recycling places additional burdens on the environment" was reworded to say that recycling "unethically places burdens" (see Supplementary materials for the full message). After reading this message, participants reported their recycling attitudes once again.

5.1.3. Independent variable: attitude basis

The attitude basis manipulation was the same as the one used in Experiment 2. Minor modifications were made, however, by replacing the word "students" with "respondents" so that the feedback would make sense in the context of a Mechanical Turk study.

5.1.4. Dependent measures: attitudes

Items used to assess pre-message attitudes ($\alpha = .97$) and post-message attitudes ($\alpha = .99$) were identical to those used in Experiment 2. An index of attitude change was again computed by subtracting the average pre-message attitudes from the average post-message attitudes.

5.1.5. Believability check

To again check whether one type of feedback was more or less believable than the other, the same believability check from Experiment 2 was included at the end of the survey.

5.2. Results

Data were first submitted to a between-subjects t-test in order to see whether the manipulation affected pre-message attitudes. As in Experiments 1 and 2, initial attitudes toward recycling did not differ as a result of the feedback manipulation, $t(92) = -1.59$, $p = .12$. Another between-subjects t-test was conducted on responses to the believability check. Unlike Experiment 2, people who received the practicality feedback reported marginally greater perceived accuracy of their feedback ($M = 6.76$, $SD = 1.80$) than people who received the morality feedback ($M = 5.96$, $SD = 2.18$), $t(92) = 1.94$, $p = .06$. If the believability check is entered as a covariate in the following analyses, however, the effects remain significant.⁶

As before, we tested attitude change in two ways. First, an additional t-test was conducted on attitude change scores. As predicted, the participants who had been given moral basis feedback evinced less attitude change in response to the message ($M = .60$, $SD = .99$) than participants who had been given practical basis feedback ($M = 1.15$, $SD = 1.38$), $t(92) = 2.20$, $p = .03$, $d = .45$, 95% CI: [.04, .86].

Alternatively, attitude change could be treated as the effect of the manipulation on post-message attitudes, controlling for pre-message attitudes. Results of this analysis show that although pre-message attitudes are a significant predictor of post-message attitudes, $F(1, 91) = 88.15$, $p < .001$, there is still an additional effect of the manipulation on post-message attitudes. In particular, attitudes toward recycling are more positive in the morality feedback condition ($M = 7.73$, $SD = 1.44$) than in the practicality feedback condition ($M = 6.76$, $SD = 1.88$), which is a significant difference even after accounting for pre-message attitudes, $F(1, 91) = 5.48$, $p = .021$, $\eta_p^2 = .06$. That is, those led to believe that their recycling attitudes were grounded in morality were more resistant to the anti-recycling message than those led to believe that their attitudes were grounded in practicality.

⁶ The effect on attitude change scores remains significant after controlling for perceived feedback accuracy, $F(1, 91) = 3.88$, $p = .05$. The effect on post-message attitudes, controlling for pre-message attitudes, also remains significant, $F(1, 90) = 4.40$, $p = .04$.

5.3. Discussion

The results of Experiment 3 replicate the persuasion effects from Experiment 2 using a non-student sample. Once again, people who were led to perceive a moral basis for their recycling attitudes showed less attitude change in response to a counter-attitudinal message than people who were led to perceive a non-moral basis for their recycling attitudes. Critically, these effects replicated even when the persuasive message appealed equally to moral and practical concerns. That is, unlike Experiment 2, the message used in this study was not better matched to the practicality than the morality basis condition and thus a pure matching/mismatching interpretation does not provide a viable explanation for the results of this study.

Future research should examine a message that is more matched to the moral than the practical basis condition (rather than being equally matched) to see if there are any conditions under which matching to a moral basis would lead to more persuasion. In our view, it is possible that such a study would provide support for a matching effect, but it also seems plausible that a message matched to a moral basis would produce even more resistance than a mismatched or unmatched message. If people who have a moral basis for their attitudes hear that a speaker thinks that the *opposite* position is the more moral one (e.g., “incest is not unethical; it is a moral imperative”), people may become especially defensive, reactant, and ultimately more resistant.

6. General discussion

The current research is the first to examine the ability of a manipulation of the mere perception of moral bases to influence attitude strength outcomes. Specifically, three experiments demonstrated that simply viewing one's attitude as based in morality renders the attitude more consequential as indicated by stronger correspondence between the attitudes and relevant behavioral intentions (Experiment 1) and less attitude change following a persuasive message (Experiments 2 and 3). Notably, both Experiments 1 and 2 also went further by demonstrating that perceived moral bases should be considered as a separate indicator of attitude strength because the impact of this variable was not mediated by any other common attitude strength determinant. This finding is consistent with Skitka and colleagues' view, based on existing correlational studies, that moral bases are best conceived as a meta-cognitive construct that imbues attitudes with the ability to withstand attack and predict behavior separate from other strength indicators. As a separate construct, future research should examine whether perceived moral bases could interact with other strength indicators to affect strength outcomes.

These findings are especially remarkable, given that the control conditions in the current research involved perceiving one's attitude as grounded in important and socially-sanctioned values (i.e., tradition and equality in Experiment 1 and practicality in Experiments 2 and 3). That is, given that values-based attitudes evince greater strength than do attitudes with alternate bases (e.g. Johnson & Eagly, 1989, Ostrom & Brock, 1968), the control conditions represented stringent baselines against which to measure the effects of manipulated perceived moral basis, especially given that the control values as well as morality were rated as similarly important.

Interestingly, the manipulations of perceived moral basis did not affect the extremity of the initial (pre-message) evaluations themselves. Although this null finding might seem inconsistent with Van Bavel et al. (2012), which found an effect on extremity, recall that their analysis compared the extremity of people's responses to two completely different questions, leaving open the possibility that people drew upon different considerations to answer either question. As a concrete example, participants were asked, “How morally right/wrong would it be for you to study?” and “How personally good/bad would it be for you to study?” According to their overall results, responses to the former question were more extreme than responses to the latter. In responding to the moral question, participants might have considered how much their parents

wanted them to study (i.e., very much) but in responding to the personal version, they might have focused on the fun they would miss by studying. If so, these different considerations could explain why one question elicited more extreme responses than the other. In contrast to that paradigm, the studies we have presented manipulated the perception that one's global evaluation was or was not grounded in moral considerations. In either perceived basis condition, however, the attitude question remained the same. Additionally, it is worth noting that Van Bavel et al. (2012) studied the effects of moral construals of actions whereas the present research considered the moral construal of attitudes, which may be qualitatively different. Indeed, prior research situated more specifically in the attitudes domain has shown moral attitude bases to operate independent of attitude extremity (Brandt & Wetherell, 2012; Skitka et al., 2005; Wright et al., 2008).

The current work extends prior research on attitude strength by demonstrating the consequential impact of merely labeling an attitude as morally based. By just measuring perceived moral bases, prior attitude strength research may have captured effects of a substantive difference between morally and non-morally based attitudes (e.g., more ideological). By manipulating this perception in the absence of any substantive differences, we showed that perceived morality acts like a strength heuristic (cf. Barden & Petty, 2008). The current studies thus contribute to an emerging body of work highlighting the importance of perceived attitude qualities irrespective of their objectively measured counterparts in producing durable and impactful attitudes (e.g. Barden & Petty, 2008, Smith, Fabrigar, MacDougall, & Wiesenthal, 2008). It remains to be seen, however, whether such quick inductions of perceived morality have long-lasting effects or whether they are confined to situations in which the perception is salient.

These results also raise new and interesting questions to be explored in future research. In particular, it is worth considering the boundary conditions for this effect. When are mere perceptions of moral attitude bases most likely to produce attitude strength consequences and when might they not? From an individual difference perspective, we suggest that these results might be most likely to occur for people who value morality and are particularly inclined to view moral bases as a sign of truth and legitimacy. Similar work has shown, for example, that greater perceived accessibility increases attitude certainty only when people have the lay theory that quick reactions are a sign that the reported attitude is one's “true” attitude (vs. thinking that quick reactions imply thoughtlessness; Tormala, Clarkson, & Henderson, 2011; see also Briñol, Petty, & Tormala, 2006).

In addition, it will be important to clarify why simply perceiving a moral basis leads to attitude strength consequences. Although our goals in these studies were to examine whether mere perception was sufficient to produce the documented effects of moral conviction and to test the causal direction of the effect, subsequent research should focus on what makes the perception of a moral basis so consequential. For instance, it may be that the perception of a moral basis signals that the attitude is connected to a broad, diverse *network* of important beliefs, leading one to perceive an especially *complex* attitude. Indeed, prior research has identified attitude complexity as a reliable attitude strength indicator (Fabrigar, Petty, Smith, & Crites, 2006); however, this prior research focused on *objective* complexity. It is plausible that perceiving attitude complexity would prompt attitude strength outcomes as well through the belief that one's attitude is especially well supported. Although pilot testing did not initially support this mechanism, we invite further research on these important questions.⁷

⁷ In a pilot study ($N = 46$), we tested whether people perceived morality and the control values that we employ in our experiments as relatively “broad” or “narrow.” Specifically, we provided participants with the key terms “morality,” “tradition,” “equality,” and “practicality” and asked them to “rate how broad/abstract each of these bases for opinions is” on 7-point scales anchored at “extremely narrow” and “extremely broad.” We tested whether these ratings were significantly higher for “morality” than each control value by using a series of within-subjects *t*-tests. Although on average, ratings were higher for “morality” than the control values, these differences were never significant, $ps > .10$.

In addition to the theoretical implications, this research has clear potential for practical applications. Political figures, advocacy groups, and advertising agencies could feasibly utilize these findings to encourage strong attitudes. For example, political candidates who guide voters to view their attitudes as morally based could effectively instill relatively firm, unchanging attitudes in their constituencies. At the very least, both lay (Lakoff, 2004) and academic (Bizer & Petty, 2005) interest in issue- and attitude-framing are becoming more widespread, thus placing the current findings in the vanguard of a potentially burgeoning applied research domain.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at <http://dx.doi.org/10.1016/j.jesp.2016.04.003>.

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