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# Are we at all liberal at heart? High-powered tests find no effect of intuitive thinking on moral foundations

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## ABSTRACT

Two opposing views define the debate on the moral principles underlying human behavior. One side argues a central role for five moral foundations (care, fairness, loyalty, authority, sanctity), while the other argues that two of these (care, fairness) capture the essence of human moral concerns. In an experiment comparing these two views, Wright and Baril (2011) found that conservatives under cognitive load devalue loyalty, authority and sanctity, and become more liberal. Their finding of common intuitive concern with care and fairness supports the two-foundation perspective. In two high-powered preregistered experiments ( $N = 3275$ ), we used time-pressure to induce intuitive thinking and tested Wright and Baril's finding that "we are all liberals at heart." Although the manipulations worked as intended, Study 1 failed to identify an effect on the moral foundations questionnaire (MFQ). We conjectured that familiarity with standard survey items may weaken intuition manipulations by eliciting stable opinions. In Study 2, we therefore used not only the MFQ but also novel moral foundations vignettes. Study 2 failed to find an effect of time-pressure on either questionnaire type. An internal Bayesian meta-analysis indicated strong evidence against an effect of intuitive thinking on moral foundations.

Which principles are foundational to human morality? Do liberals and conservatives intuitively rely on the same moral foundations? Are moral judgments stable or do they vary with contextual details? Here, we review the current literature on these questions and provide experimental evidence to help answer them. Moral foundations theory views moral judgments as intuitive (Haidt, 2007; Haidt et al., 2009), and criticizes previous definitions that restrict the study of moral attitudes to the role of analytic thought and to the issues of care and fairness (see Kohlberg, 1969). Instead, the theory argues that at least five different evolutionarily acquired moral foundations guide human behavior, as measured by the moral foundations questionnaire (MFQ).

The care/harm dimension is one of these foundations, defined as the tendency to nurture infants and to protect the weak. The fairness/cheating dimension pertains to group members, involving a sensitivity towards issues of fairness and the detection of cheaters who weaken group cohesion. On the other hand, the loyalty/betrayal dimension relates to promotion of loyalty towards in-group members and to avoidance of betraying them. The authority/subversion dimension is about defending authority in a hierarchical social structure against

subversions of the system. Finally, the sanctity/degradation foundation of morality involves concern for physical and spiritual cleanliness as well as valuation of sanctity and devaluation of material aspirations.

Graham et al. (2009) argue that care and fairness dimensions constitute the individualizing foundations pertaining to personal rights and that the other three dimensions of morality constitute the binding foundations that tend to strengthen group attachment and weaken freeriding. Individualizing and binding foundations correlate well with ideological differences: while liberals primarily view care and fairness dimensions as founding morality, conservatives tend to equally value all five foundations (Graham et al., 2009). As such, considerable attention has been given to the causal link between these foundations and ideological differences. Although early theoretical and empirical work implied that moral foundations determine political ideology (Day et al., 2014; Feinberg & Willer, 2013; Haidt, 2012), more recent work suggests that causality works in the opposite direction, thereby problematizing the foundational status of the five moral domains (Ciuk, 2018; Smith et al., 2017). Evidence on the causality question is not yet clear and open to alternative interpretations, as we discuss below (see

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Proposal for Experiment 2).

Another prominent—and more established—debate in the literature on moral foundations is the question of whether and how any of these five foundations are more fundamental than the others. Arguing that all humans rely on the five moral foundations as a common heritage of evolution, Haidt and Kesebir (2010) associate the aforementioned differences between liberals and conservatives with the late emergence of the Enlightenment culture that narrowed the definition of morality among liberals through the mental repression of binding foundations. Evidence on the *reactive liberal hypothesis* might be interpreted as supporting this perspective (Nail et al., 2009). According to this hypothesis, liberals behave like conservatives when they rely on their intuitions. For example, liberals tend to provide personal attributions (e.g., about victims) that are similar to conservatives when elicited under cognitive load (Skitka et al., 2002). Similarly, Van de Vyver et al. (2016) measured moral foundations and prejudice in a representative sample six weeks before and a month after the 2005 suicide bombings in London, and found an association between the timing of the bombings and increase in loyalty and decrease in fairness foundations. These effects on moral foundations were stronger among liberals than among conservatives, which can explain the increasing prejudice among liberals towards Muslims and immigrants. Keeping in mind that terror attacks have psychological effects akin to mortality salience manipulations (Landau et al., 2004), which in turn can be compared to cognitive load manipulations (Trémolière et al., 2012; Trémolière et al., 2014), one can argue that terror attacks affect moral foundations by inducing intuitive thought. In short, the study by Van de Vyver et al. (2016) might indicate that liberals become more conservative when thinking intuitively. Using implicit measurement techniques, Graham (2010) identified a larger discrepancy among liberals than among conservatives between their measurements of explicit and implicit moral foundations. These findings suggest that liberals do have binding moral foundations, yet they tend to repress these intuitions using cognitive effort.

A contrasting argument is provided by the *conservatism-as-motivated-social-cognition account* of Jost et al. (2003). Accordingly, conservatives value resistance to change and opposition to equality to cope with uncertainty and existential threat, thereby using the binding foundations as a palliative (see also Jost, 2012). From this perspective, although everyone relies on the two basic moral foundations of care and fairness, conservatives enhance the value they give to binding foundations through cognitive effort. Hence, according to this account, and in contrast to the reactive liberal hypothesis, the value that conservatives attach to the binding foundations is expected to decrease when their cognitive capacity is curtailed. There is some evidence suggesting that people value individualizing more, and binding less when their higher level core values become salient—for example, when they think in an abstract mindset (Napier & Luguri, 2013).

In the only study that directly compares these two theoretical approaches, Wright and Baril (2011) found no main effect of cognitive load or ego depletion on moral foundations (in comparison to a control group). When the two manipulation conditions were merged, however, conservatives who were cognitively taxed were found to significantly devalue the binding foundations. This finding supports Jost's (2012) argument that conservatives enhance their binding foundations through mental effort, and suggests that everyone primarily values care and fairness foundations at intuitive baselines. Nevertheless, Wright and Baril's (2011) study is restricted to a single experiment with low statistical power. Another limitation of Wright and Baril's (2011) study is their merger of the two experimental manipulation conditions (load and ego depletion) for making inferences, and it is unclear whether their control condition differs from each of the intuition conditions separately. In another study, cognitive load instead had a significant main (positive) effect on the care and authority foundation scores without an interaction effect with political ideology (Van Berkel et al., 2015). Contrary to both findings, in a study that activated analytical

rather than intuitive thought, Yilmaz and Saribay (2017a) found a significant increase in the care and fairness foundations, and contrary to Wright and Baril's finding, did not find an interaction with political ideology.

In Experiment 1, we attempted to disentangle these findings. In particular, building on Yilmaz and Saribay (2017a), we predicted that activating intuitive thinking will in general (i.e., independent of political ideology) lower scores on the individualizing foundations of care and fairness, and contrary to Wright and Baril (2011), we predicted that intuitive thinking will in general increase MFQ scores on the binding foundations of loyalty, authority, and sanctity.

However, our findings in Experiment 1 motivated us to consider a second debate in the literature that pertains to the question whether scores on these survey items are resistant to experimental manipulations (Ciuk, 2018; Deppe et al., 2015; Talhelm, 2018; Talhelm et al., 2015; Yilmaz et al., 2016; Yilmaz & Saribay, 2016, 2017a, 2017b). Although they did not directly measure moral foundations, Deppe et al. (2015) reported no effect of analytic thought manipulation on moral and other attitude items. In a study that was otherwise similar to Deppe et al., Yilmaz and Saribay (2017b) made a conceptual distinction between contextualized and stable opinions with regard to the dependent variable of moral attitudes (see also Talhelm, 2018; Talhelm et al., 2015; Yilmaz & Saribay, 2016). In two separate experiments, Yilmaz and Saribay (2017b) showed that analytical thought enhances liberal attitudes in contextualized scenarios but not in stable opinions (survey items). Hence, cognitive process manipulation seems to affect attitudes about novel scenarios that are contextualized as newspaper stories, while having no effect on items about which people may have already formed stable opinions.

The MFQ consists of abstract and general descriptions of morally relevant situations (e.g., "Whether or not someone acted unfairly"), and a large number of studies have extensively used these items. These two features of the experimental environment may dampen the differential roles of intuitive and analytical thinking in the process of moral attitude elicitation. In other words, the MFQ scores may reflect previously deliberated and automatized answers to specific survey items or general personal principles. On the other hand, numerous studies succeeded in finding an effect of contextual variables on moral foundations (i.e., Ciuk, 2018; Napier & Luguri, 2013; Wright & Baril, 2011; Yilmaz et al., 2016). In a recent example of this phenomenon, Eriksson et al. (2019) found that specifying the agents responsible for the acts described in the MFQ items—otherwise undefined in the standard scale—created a gap between the responses of conservatives and liberals.

The moral foundations vignettes are less likely to suffer from these methodological problems (Clifford et al., 2015). Originally developed to provide moral violation scenarios based on the moral foundations theory, these vignettes measure attitudes about contextually specific and novel scenarios that are morally relevant and include descriptions of specific moral agents, unlike MFQ ("You see a professor giving a bad grade to a student just because he dislikes him."). To the best of our knowledge, the moral foundations vignettes have not previously been used under cognitive manipulations. These vignettes allow us to test whether the effect of cognitive manipulations depends on the measurement of moral foundations as survey items (i.e., involving more stable opinions) or as novel contextual scenarios (i.e., including the process of opinion formation).

In the first experiment presented here, we provide a preregistered conceptual replication of Wright and Baril's finding (2011) as well as a test of our own hypotheses on the link between cognitive style and moral attitudes, using a high-powered test of the effect of time-pressure on the moral foundations questionnaire (as compared to a control condition). As the second experiment, we propose a test of the boundary condition for identifying the effects of cognitive style on moral attitudes. In Experiment 2, we use the moral foundations questionnaire to repeat the test in Experiment 1, and in addition, we use the moral foundations vignettes to test whether the inducement of intuitive

thinking by time-pressure is effective only in novel contextualized situations.

## 1. Experiment 1

### 1.1. Method

#### 1.1.1. Overview

Experiment 1 uses a 2 (within-subjects moral foundation: individualizing vs. binding) by 2 (between-subjects cognitive manipulation: intuition vs. control) mixed design. The experiment was preregistered at the Open Science Framework (OSF). The preregistration document, the experimental materials, the dataset and the analysis code, can be found at <https://osf.io/pdw4q/>. We report all measures, manipulations and exclusions.

#### 1.1.2. Power analysis

The sample size was determined before data collection. Since results in the literature are mixed, we assumed a very small interaction effect size ( $f = 0.05$ ), and we set alpha at 0.05 and power at 0.80. Based on a large, multi-cultural and publicly available dataset (the Many Labs 2 project) on moral foundations in WEIRD (Western, educated, industrialized, rich, democratic) societies (Klein et al., 2018), we estimated the correlation between individualizing and binding foundations to be 0.45. Using G\*Power software (Faul et al., 2009), we computed the required sample to be at least 866 to detect a very small interaction effect in a mixed ANOVA.

#### 1.1.3. Participants

We recruited US residents from Amazon Mechanical Turk (MTurk) and conducted our experiment using the Qualtrics survey software. We collected data from 909 participants. Excluding 50 incomplete or duplicate submissions, our dataset consists of 869 complete and authentic observations.

#### 1.1.4. Materials and procedure

**1.1.4.1. Procedure.** After passing a security question designed to exclude bots and having provided informed consent, participants were randomly assigned either to the time-pressure condition (TP) intended to induce intuitive thinking or to the control condition (C). Participants completed each item in Part 1 of the moral foundations questionnaire (MFQ) either under 3 s time-pressure (TP; the intuition condition) or without any time limits (C; the control condition). They then completed manipulation check questions and a brief survey on political ideology.

**1.1.4.2. Moral foundations questionnaire.** The 30-item MFQ consists of five subscales (care, fairness, loyalty, authority, sanctity), elicited in two parts (Graham et al., 2011). We intended to limit the number of items asked under TP since acclimation by repeated exposure to the manipulation may weaken its effectiveness. Given that both the five-factor (care, fairness, authority, loyalty, sanctity) and the two-factor (individualizing, binding) structures of the relevance subscale (i.e., Part 1) of MFQ fit data well (Doğruyol, Alper, & Yilmaz, 2019), we only used Part 1 of MFQ (15 items). Using a six-point Likert scale ranging from 0 (“not at all relevant”) to 5 (“extremely relevant”), Part 1 of MFQ elicits self-reported moral relevance of various considerations to individual decisions. A total of 16 items, including an attention check, were randomly presented on consecutive screens. As standard in the literature, the scores for individualizing foundations were calculated by averaging scores obtained from the care and fairness subscales, whereas scores for binding foundations were calculated by averaging the scores obtained from the loyalty, authority and sanctity subscales ( $\alpha_{\text{individualizing}} = 0.79$ ,  $\alpha_{\text{binding}} = 0.86$ ;  $\alpha_{\text{care}} = 0.70$ ,  $\alpha_{\text{fairness}} = 0.68$ ,  $\alpha_{\text{authority}} = 0.65$ ,  $\alpha_{\text{loyalty}} = 0.75$ ,  $\alpha_{\text{sanctity}} = 0.71$ ).

**1.1.4.3. Time-limit manipulations.** Recent literature on time-pressure manipulations suggests that time-limits longer than 5 s may not be short enough to induce spontaneous responses, effectively resulting in low-deliberation conditions (Myrseth & Wollbrant, 2017). Therefore, we adopt a time-limit below the threshold proposed for considering a decision as intuitive (Myrseth & Wollbrant, 2017). To ensure that participants had a chance to read each item, the decision tools (multiple-choice buttons, prompts, and for the TP condition, a timer) appeared 2 s after an item was first displayed. Answers to each MFQ item in the TP condition were elicited under 3 s time-pressure with prompts to “Be quick!” and a visible timer counting up from zero. In the C condition, there were no time-limits (or visible timers) and decisions were elicited with prompts to “Carefully consider” the decision. Compliance with time-limits was incentivized to strengthen the cognitive manipulations (Isler et al., 2018). In particular, participants in TP were told that one of their 15 answers would be randomly selected at the end of the experiment and that they would be paid additional money equivalent to the participation fee only if their response time for this question was below 3 s. Those in the C condition were told that they would earn this additional amount if they have made a decision for this randomly chosen question. These instructions were provided before the elicitation of the MFQ began.

**1.1.4.4. Manipulation checks.** After the elicitation of MFQ, participants rated their agreement with two statements on a 5-point Likert scale (1 = “strongly disagree,” 5 = “strongly agree”), which were designed to measure differences (1) in opportunities for deliberation and (2) in the tendency for spontaneous reactions: 1) “I did not have time to think through my decision,” and 2) “I decided based on my gut reactions.” We employ two separate tests that our intuition manipulation has worked. First, as a behavioral manipulation check, we test for differences in average response times across the 15 MFQ questions between TP and C. Second, as a cognitive manipulation check, we test the differences in average ratings for these two questions between TP and C.

**1.1.4.5. Survey.** Participants finally completed a brief survey, including the elicitation of orientation (1 = “liberal,” 7 = “conservative”) on social ( $M = 3.46$ ,  $SD = 1.79$ ) and economic conservatism ( $M = 3.98$ ,  $SD = 1.79$ ) on the same screen, to allow participants to clearly distinguish between the two aspects of political ideology.

**1.1.4.6. Attention check.** An attention check item (“This is an attention check please choose 4”) was presented together with MFQ. 98.9% of those in the TP condition and 98.4% of those in the C condition passed the attention check. Given the very low numbers of inattentive participants ( $n = 12$ ), we do not make exclusions in our analysis based on this criterion. Our findings are robust to the exclusion of these participants from analysis.

**1.1.4.7. Time-limit compliance.** Based on response times (RT) from the appearance of the scales and the clock on the decision screen to the submission of the decision on an item, we categorized each submission made in the TP condition as compliant if  $RT \leq 3$  s. Average compliance across all 15 items in the TP condition was 80.5%. Given that exclusions based on this criterion can annul randomization and bias analysis (Tinghög et al., 2013), we do not exclude non-compliant participants from analysis.

**1.1.4.8. Data exclusions.** As described in the preregistration form, the first screen of the experiment was designed to exclude bots. The task asked for the second letter in an underlined, italicized word-image (“*MTURK*”), and failure resulted in exclusion from further participation. In addition, participants who have not completed all parts of the experiment within 15 min were excluded from the analysis. We also excluded multiple submissions by the same individual (identified by unique MTurk Worker ID) except for the initial submission if this initial

submission was complete and if it did not coincide in time with another submission by the same participant. Among those who passed the initial screen, a total of 50 participants were excluded from analysis.

1.1.5. Statistical tests

We first present the manipulation checks. As confirmatory analysis, a 2 by 2 mixed ANOVA was conducted to test for the main effects of cognitive manipulation and moral foundation as well as their interaction. We then provide secondary analysis based on our preregistered analysis plan, testing whether the intuition manipulation affected any of the five MFQ foundations viewed separately and whether the intuition manipulation interacted with self-reported social conservatism in predicting changes in MFQ scores. As exploratory analysis, which was not part of the preregistration, we check whether economic conservatism moderates the effect of intuition manipulation.

1.2. Results and discussion

1.2.1. Manipulation checks

We use the average of response times (in seconds) across MFQ items as an index of behavioral manipulation. Results of an independent samples *t*-test indicated that participants in the time-pressure condition ( $M = 2.47, SD = 1.36; 95\% CI [2.34, 2.59]$ ) were significantly faster as compared to participants in the control condition ( $M = 4.68, SD = 5.02; 95\% CI [4.20, 5.15]$ ),  $t(867) = 8.88, p < .001, d = 0.60$ . As an index of cognitive manipulation, the scores on the two manipulation check questions were averaged. The scores were higher in TP ( $M = 3.52, SD = 0.81; 95\% CI [3.44, 3.59]$ ) as compared C ( $M = 2.79, SD = 0.73; 95\% CI [2.72, 2.86]$ ),  $t(867) = 13.90, p < .001, d = 0.94$ . Therefore, behavioral and cognitive tests indicate that the intuition manipulation worked as intended.

1.2.2. Confirmatory analyses

Each participant has two moral foundations scores, one for individualizing and one for binding foundations. The 2 (between-subject cognitive manipulation: TP vs. C) by 2 (within-subject moral foundation: individualizing vs. binding) mixed ANOVA on moral foundation scores was conducted as confirmatory analysis. There was a main effect of moral foundation on foundation scores,  $F(1, 867) = 826.03, p < .001, \eta_p^2 = 0.488$ . Specifically, the binding foundations scores ( $M = 3.74, SD = 0.80; 95\% CI [3.69, 3.80]$ ) were significantly higher than the individualizing foundations scores ( $M = 2.60, SD = 1.01; 95\% CI [2.54, 2.67]$ ). Testing our hypotheses, there was neither a main effect of time-pressure manipulation,  $F(1, 867) = 1.12, p = .290, \eta_p^2 = 0.001$ , nor an interaction between manipulation and foundation,  $F(1, 867) = 0.01, p = .916, \eta_p^2 < 0.001$ .

1.2.3. Secondary analyses

We report separate independent samples *t*-tests of the experimental manipulation for each of the five moral foundations. The effect of manipulation was not significant for any of the five moral foundations. However, there was a trending effect of intuition on the decreased endorsement of care in the hypothesized direction. The results of the analyses are presented in Table 1.

**Table 1**  
Effect of intuition manipulation on the five moral foundations.

	Control		Time-pressure		t-test		
	M	SD	M	SD	t	d	p
Care	3.76	0.85	3.65	0.97	1.76	0.119	0.079
Fairness	3.78	0.81	3.78	0.93	0.01	< 0.001	0.994
Loyalty	2.63	1.10	2.64	1.20	0.06	0.004	0.956
Authority	2.70	1.00	2.67	1.03	0.49	0.034	0.621
Sanctity	2.55	1.28	2.44	1.33	1.22	0.083	0.224

**Table 2**  
Interaction of cognitive manipulation with social conservatism in Experiment 1.

	Individualizing		Binding	
	B	t	B	t
Time-pressure	0.00	0.01	-0.10	0.76
Social conservatism	-0.06***	3.33	0.27***	11.28
Interaction	-0.02	0.52	0.02	0.47
Constant	3.99***	53.09	1.71***	17.76
R <sup>2</sup>	0.028		0.240	

Robust standard errors.

\*\*\*  $p < .001$ .

In two interaction regressions (see Table 2), we next study whether social conservatism ratings moderated the intuition manipulation. Social conservatism predicted lower levels of individualizing foundations ( $B = -0.06, p < .001; 95\% CI [-0.10, -0.03]$ ) and higher levels of binding foundations ( $B = 0.27, p < .001; 95\% CI [0.22, 0.31]$ ). However, in contrast to the findings of Wright and Baril (2011), neither the main effect of manipulation nor its interaction with social conservatism was significant.

1.2.4. Exploratory analyses

Similar to social conservatism, economic conservatism was associated with lower individualizing foundations ( $B = -0.07, p < .001; 95\% CI [-0.11, -0.03]$ ) and higher binding foundations scores (economic:  $B = 0.19, p < .001; 95\% CI [0.14, 0.24]$ ), and the main effect of manipulation as well as its interaction with economic conservatism were not statistically significant (see Table 3).

Overall, we failed to find any effect of the intuition manipulation on moral foundations or its moderation by social or economic conservatism. Conservatism, in general, was found to be significantly associated with both the individualizing and the binding foundation scores. For the next experiment, we propose to test the differential effect of intuition manipulation on stable versus contextualized moral attitudes.

2. Experiment 2

Experiment 1 failed to identify an effect of cognitive process manipulations on MFQ. We use Experiment 2 not only to retest this effect but also to reconsider recent criticisms of MFQ. In particular, we suggest that experimental manipulations are more likely to show an effect on moral foundations if measured using novel contextualized scenarios and that this methodological phenomenon may have been mistakenly interpreted as a problem with the validity of MFQ per se.

It has recently been argued that political ideology determines moral foundations, rather than the other way around (Ciuk, 2018; Smith et al., 2017). Although the argument is in principle plausible, the current evidence does not allow clear identification of such an inverse causal link. Smith et al. (2017) conduct longitudinal analyses of the

**Table 3**  
Interaction of cognitive manipulation with economic conservatism in Experiment 1.

	Individualizing		Binding	
	B	t	B	t
Time-pressure	-0.02	0.15	-0.08	0.47
Economic conservatism	-0.07***	3.75	0.19***	7.76
Interaction	-0.01	0.18	0.00	0.00
Constant	4.04***	47.05	1.88***	17.23
R <sup>2</sup>	0.028		0.116	

Robust standard errors.

\*\*\*  $p < .001$ .

correlation between foundations and ideology, which are fraught with difficulties that preclude causal inference (e.g., structural equation modeling, using different versions of the MFQ across time, etc.). On the other hand, their less restrictive interpretation that moral foundations are “highly responsive to context: more a state than a trait” seems justified. Similarly, Ciuk's (2018) experimental design is difficult to interpret as isolating the causal effect of ideology. This experiment assigns various party and ideology cues as preface to the MFQ items (e.g., “Democrats feel,” “Conservatives think”), which can also be read as isolating the contextual impact of intergroup bias (e.g., involvement of specific moral agents) on survey responses rather than the causal effect of experimental manipulation of ideology. Hence, these criticisms about the validity of MFQ may instead be reflecting the contextualization of the MFQ items—an idea that we tested in Experiment 2. Specifically, extending our initial set of hypotheses on the role of intuitive thinking on moral foundations, we inquired whether the effect of intuitive thinking on moral foundations depends on the measurement of stable or contextualized attitudes.

Talhelm et al. (2015) initially made the distinction between stable and contextualized opinions, which was later replicated in a non-Western sample (Talhelm, 2018). Their study manipulated cultural thought style (holistic vs. analytic) and found an effect on contextual newspaper stories measuring social conservative attitudes. However, the same manipulation failed to find an effect on standard survey items. A similar observation was made in a study that employed analytic thought training (Yilmaz and Saribay, 2017b). In other words, these studies found analytic thought manipulations to affect contextualized but not stable social conservative attitudes. Consistent with these findings, Schein (2020) recently argued that changes in moral behavior can be better understood when moral problems are contextualized. An analogous boundary condition can therefore apply to the moral foundations questionnaire given its reliance on general descriptions and its widespread use, both of which will tend to elicit automatic responses to previously deliberated opinions. Anecdotal evidence from Experiment 1 supports this idea. Numerous participants stated remembering the items from past surveys in an open-ended suspicion probe question, and one participant even explicitly suggested that experimental manipulations would not work for this reason: “idk but ive answered those exact questions 100's of times. i do not need to think about them any longer to answer them [sic]”. We concur that the reason why we failed to replicate Wright and Baril's (2011) finding or find evidence for our hypotheses maybe because responses to the standard MFQ have already been formed. For these reasons, in addition to the MFQ items we used in Experiment 1, we studied the effect of intuitive thinking on the moral foundations vignettes (MFV) that consist of novel contextualized scenarios involving moral agents (Clifford et al., 2015).

In Experiment 2, each participant completed either the MFQ or the MFV items, either under time-pressure or without time-limits. Overall, we predicted that the cognitive manipulation of intuitive thinking affects the MFV but not the MFQ scores. Consequently, we expected our initial hypotheses (regarding the negative effect on individualizing and positive effect on binding foundations scores of intuitive thinking) to hold when measuring attitudes in novel contextualized scenarios (MFV) but not when measuring stable opinions (MFQ).

## 2.1. Method

### 2.1.1. Overview

Experiment 2 has a 2 (within-subjects moral foundation: individualizing vs. binding) by 2 (between-subjects cognitive manipulation: intuition vs. control) by 2 (between-subjects questionnaire type: MFQ vs. MFV) mixed design. The copy of the approved prior version of the manuscript, the experimental materials, the dataset, and the analysis code can be found at the OSF study site (<https://osf.io/pdw4q/>). We report all measures, manipulations and exclusions.

### 2.1.2. Power analysis

The sample size was determined before data collection. As a conservative estimate of the total sample size required, we calculated and simply doubled the sample size needed to detect a very small interaction effect size ( $f = 0.05$ ) between the cognitive manipulation and the moral foundation for each questionnaire type. For Experiment 2, we increased power to 0.90. Using an alpha of 0.05, we computed the required sample to be at least 1158 to detect an interaction effect in a 2 by 2 mixed ANOVA in G\*Power software (Faul et al., 2009). Considering potential attrition, we planned to recruit 1200 participants for each questionnaire type (i.e., 2400 participants in total).

### 2.1.3. Participants

We recruited US residents from MTurk who had not participated in Experiment 1. We analyze data from 2406 participants with unique and complete submissions.

### 2.1.4. Materials and procedure

**2.1.4.1. Procedure.** Experiment 2 employed the materials and procedures as in Experiment 1 except the addition of MFV items and the minor revisions detailed below. Participants completed either the MFQ ( $\alpha_{\text{individualizing}} = 0.76$ ,  $\alpha_{\text{binding}} = 0.82$ ;  $\alpha_{\text{care}} = 0.63$ ,  $\alpha_{\text{fairness}} = 0.66$ ,  $\alpha_{\text{authority}} = 0.60$ ,  $\alpha_{\text{loyalty}} = 0.65$ ,  $\alpha_{\text{sanctity}} = 0.67$ ) or the MFV items, either under 3 s TP per item or without any time limits (C).

**2.1.4.2. Moral foundations vignettes.** MFV was developed based on the MFQ structure to provide moral violation scenarios (Clifford et al., 2015). It uses a five-point Likert scale ranging from 0 (“not at all wrong”) to 4 (“extremely wrong”) to elicit evaluations of moral wrongness for the behaviors described in these scenarios. The authors recommend the use of any of the 90 items (out of 132) that passed their classification requirements of high factor loadings and no cross-loadings. Since we use 15 items in the MFQ condition, we also selected 15 MFV items from among the 90 recommended items, choosing three items for each of the five moral foundations ( $\alpha_{\text{individualizing}} = 0.69$ ,  $\alpha_{\text{binding}} = 0.76$ ;  $\alpha_{\text{care}} = 0.55$ ,  $\alpha_{\text{fairness}} = 0.65$ ,  $\alpha_{\text{authority}} = 0.66$ ,  $\alpha_{\text{loyalty}} = 0.66$ ,  $\alpha_{\text{sanctity}} = 0.74$ ). To select items for each moral foundation, we simply identified the items with the three highest factor loadings in Table 2 of Clifford et al. (2015).

**2.1.4.3. Survey.** As in Experiment 1, two questions were used to measure social ( $M = 3.59$ ,  $SD = 1.79$ ) and economic conservatism ( $M = 3.98$ ,  $SD = 1.75$ ). The survey in Experiment 2 additionally measured familiarity with the moral foundation items in two questions. The first question asked participants whether they recognized the items from previous experiments they completed on MTurk. Those who said “Yes” were then asked an additional question about how many such experiments they have completed.

**2.1.4.4. Attention check.** An attention check item, the same as in Experiment 1, was presented with the moral foundation items. Twenty-one participants who received MFQ and twenty-six participants who received MFV failed the attention check ( $n = 47$ ). Given that most participants were attentive (98.0%), we include them in the data analysis. The results are robust to their exclusion.

**2.1.4.5. Time-limit compliance.** As in Experiment 1, we categorized each item submitted in the TP conditions as compliant if item RT was less than 3 s, and we kept non-compliant participants in the analysis in order not to jeopardize random assignment. Average compliance was 73.8% for the MFQ items and 58.9% for the MFV items, consistent with the assumption that MFQ is more familiar than the MFV (see Manipulation checks).

**2.1.4.6. Data exclusions.** Experiment 2 used the same initial screen as Experiment 1, designed to exclude bots. As planned, we excluded the

duplicate ( $n = 44$ ) and the incomplete ( $n = 49$ ) submissions as well as participants without valid MTurk IDs ( $n = 30$ ) from the analyses.

### 2.1.5. Statistical tests

We first present the manipulation checks. To test our hypotheses, we used a three-way mixed ANOVA model on the moral foundation (individualizing vs. binding), cognitive manipulation (TP vs. C), questionnaire type (MFQ vs. MFV), and their interactions. Because the original scales in MFQ (six-point scale about moral relevancy) and MFV (five-point scale about moral wrongness) are different, we arithmetically rescaled MFV to make it comparable to the MFQ scale. As secondary analysis based on the registered proposal, we separately analyzed each foundation component using independent samples  $t$ -tests and tested whether social conservatism moderates the effect of intuition manipulation. Finally, we explored moderation of the experimental manipulation by economic conservatism.

## 2.2. Results and discussion

### 2.2.1. Manipulation checks

As in Experiment 1, we use average RTs on moral foundation items to test for evidence of behavioral manipulation. Independent samples  $t$ -tests on MFQ items revealed that participants in the time-pressure condition ( $M = 2.93$ ,  $SD = 3.20$ ; 95% CI [2.68, 3.19]) were faster than those in the control condition ( $M = 5.13$ ,  $SD = 4.88$ ; 95% CI [4.74, 5.52]),  $t(1200) = 9.26$ ,  $p < .001$ ,  $d = 0.53$ . Results were similar for MFV, where participants were faster in TP ( $M = 3.54$ ,  $SD = 2.29$ ; 95% CI [3.36, 3.72]) as compared to C ( $M = 6.88$ ,  $SD = 6.80$ ; 95% CI [6.32, 7.43]),  $t(1202) = 11.60$ ,  $p < .001$ ,  $d = 0.67$ . The composite scores of cognitive manipulation, calculated as the average of the two self-reported manipulation check questions, were also significantly different—higher in TP ( $M = 3.54$ ,  $SD = 0.89$ ; 95% CI [3.47, 3.61]) than in C for MFQ ( $M = 2.79$ ,  $SD = 0.76$ ; 95% CI [2.73, 2.85]),  $t(1,200) = 15.71$ ,  $p < .001$ ,  $d = 0.91$ , and higher in TP ( $M = 3.61$ ,  $SD = 0.86$ ; 95% CI [3.54, 3.67]) than in C for MFV ( $M = 2.86$ ,  $SD = 0.78$ ; 95% CI [2.80, 2.92]),  $t(1,202) = 15.67$ ,  $p < .001$ ,  $d = 0.91$ .

The survey questions on item familiarity can be used to check our assumption that MFV is less familiar than MFQ. Only 11.1% of participants in the MFV condition reported familiarity with the questionnaire items, as compared with 31.6% of participants in the MFQ condition—a statistically significant difference,  $\chi^2(1, n = 2406) = 151.68$ ,  $p < .001$ . Among those who were familiar with the MFQ, 29.7% reported answering the items only once before, 45.8% reported previously answering them “2 to 5 times”, 17.4% reported answering them “6 to 20 times” and 7.1% reported answering them “more than 20 times”. These checks indicate that the cognitive process manipulations were effective and that the MFV items were less familiar than the MFQ items.

### 2.2.2. Confirmatory analyses

The three-way mixed ANOVA indicated a main effect of questionnaire type,  $F(1, 2402) = 9.44$ ,  $p = .002$ ,  $\eta_p^2 = 0.007$ , and a main effect of moral foundation,  $F(1, 2402) = 2322.56$ ,  $p < .001$ ,  $\eta_p^2 = 0.492$ . Specifically, overall scores were slightly higher in MFV ( $M = 3.24$ ,  $SD = 0.66$ ; 95% CI [3.21, 3.28]) than MFQ ( $M = 3.16$ ,  $SD = 0.69$ ; 95% CI [3.12, 3.20]), and the individualizing scores ( $M = 3.77$ ,  $SD = 0.77$ ; 95% CI [3.74, 3.80]) were substantially higher than the binding scores ( $M = 2.82$ ,  $SD = 0.86$ ; 95% CI [2.79, 2.86]). The interaction between questionnaire type and moral foundation was not significant,  $F(1, 2402) = 0.06$ ,  $p = .814$ ,  $\eta_p^2 < 0.001$ . Testing our hypotheses, there was no main effect of cognitive manipulation,  $F(1, 2402) = 0.27$ ,  $p = .603$ ,  $\eta_p^2 < 0.001$ , no interaction between cognitive manipulation and moral foundations,  $F(1, 2402) < 0.01$ ,  $p = .953$ ,  $\eta_p^2 < 0.001$ , and no three-way interaction,  $F(1, 2402) = 0.24$ ,  $p = .622$ ,  $\eta_p^2 < 0.001$ .

**Table 4**

Effect of intuition manipulation on the five moral foundations.

		Control		Time-pressure		t-Test		
		M	SD	M	SD	t	d	p
Care	MFQ	3.68	0.88	3.72	0.89	0.86	0.050	0.389
	MFV	3.76	0.90	3.79	0.94	0.55	0.032	0.585
Fairness	MFQ	3.76	0.84	3.76	0.94	0.04	0.003	0.965
	MFV	3.85	0.89	3.83	0.85	0.40	0.023	0.692
Loyalty	MFQ	2.83	1.06	2.90	1.08	1.09	0.063	0.274
	MFV	2.18	1.12	2.11	1.13	1.03	0.059	0.306
Authority	MFQ	2.81	0.95	2.87	1.03	1.06	0.061	0.291
	MFV	2.72	0.94	2.62	0.99	1.64	0.095	0.101
Sanctity	MFQ	2.63	1.18	2.63	1.24	0.003	< 0.001	0.998
	MFV	3.72	1.07	3.84	1.10	1.92	0.111	0.056

### 2.2.3. Secondary analyses

As in Experiment 2, we tested the effect of the cognitive manipulation on the five moral foundations separately for each questionnaire type (Table 4). None of the independent samples  $t$ -tests revealed significant results for any of the moral foundations. Still, consistent with our hypotheses, there was a trending effect of time-limits on the sanctity score in MFV but not in MFQ. Considering the multiple hypotheses tests that can inflate type I error, and consistent with the results of Experiment 1, we infer failure to find any effect of cognitive manipulations on moral foundations.

Next, we estimated four linear regression models with cognitive manipulation, social conservatism, and their interaction in predicting moral foundations separately for each questionnaire type (Table 5). As in Experiment 1, social conservatism predicted lower levels of individualizing foundation ( $B_{MFQ} = -0.09$ ,  $p < .001$ ; 95% CI [-0.13, -0.06];  $B_{MFV} = -0.05$ ,  $p = .007$ ; 95% CI [-0.08, -0.01]) and higher levels of binding foundation ( $B_{MFQ} = 0.21$ ,  $p < .001$ ; 95% CI [0.18, 0.25];  $B_{MFV} = 0.12$ ,  $p < .001$ ; 95% CI [0.08, 0.15]). However, neither cognitive manipulation nor its interaction with social conservatism significantly predicted the individualizing or binding foundations.

### 2.2.4. Exploratory analyses

Economic conservatism was associated with lower levels of individualizing foundation ( $B_{MFQ} = -0.10$ ,  $p < .001$ ; 95% CI [-0.13, -0.06];  $B_{MFV} = -0.04$ ,  $p = .022$ ; 95% CI [-0.08, -0.01]) and higher levels of binding foundation ( $B_{MFQ} = 0.17$ ,  $p < .001$ ; 95% CI [0.13, 0.21];  $B_{MFV} = 0.06$ ,  $p < .001$ ; 95% CI [0.03, 0.10]). However, cognitive manipulation and its interaction with economic conservatism were not significant (Table 6).

Overall, we again failed to find an effect of intuition manipulation or its moderation by social or economic conservatism on moral foundations either as measured via MFQ and MFV. Conservatism, in general, was significantly and consistently associated with both the individualizing and the binding foundations across MFQ and MFV.

## 3. Internal meta-analysis

To summarize our findings on the standard moral foundations questionnaire, we provide an internal meta-analysis (Goh et al., 2016) of 2071 participants across the two experiments who completed Part 1 of the MFQ. We estimate a two-way ANOVA for each main moral foundation on the MFQ using study (Experiment 1 vs. Experiment 2) and cognitive manipulation (TP vs. C) as between-subjects factors.

The ANOVA model of the individualizing scores in MFQ indicated no effect of time-pressure,  $F(1, 2067) = 0.22$ ,  $p = .636$ ,  $\eta_p^2 < 0.001$ , no effect of study,  $F(1, 2067) = 0.13$ ,  $p = .721$ ,  $\eta_p^2 < 0.001$ , and no interaction effect,  $F(1, 2067) = 1.15$ ,  $p = .285$ ,  $\eta_p^2 < 0.001$ . On the binding scores in MFQ, the two-way ANOVA showed neither an effect of time-pressure,  $F(1, 2067) < 0.01$ ,  $p = .971$ ,  $\eta_p^2 < 0.001$ , nor an interaction effect between time-pressure and study,  $F(1, 2067) = 1.07$ ,

**Table 5**  
Interaction of cognitive manipulation with social conservatism in Experiment 2.

	MFQ				MFV			
	Individualizing		Binding		Individualizing		Binding	
	B	t	B	t	B	t	B	t
Time-pressure	0.06	0.57	-0.09	0.76	-0.09	1.04	-0.09	0.94
Social conservatism	-0.09***	4.96	0.21***	10.84	-0.05**	2.70	0.12***	7.02
Interaction	-0.01	0.26	0.03	1.03	0.02	0.94	0.04	1.54
Constant	4.05***	58.56	1.99**	24.63	3.99***	60.64	2.44***	33.87
R <sup>2</sup>	0.048		0.196		0.008		0.094	

Robust standard errors.

\*\*  $p < .01$ .

\*\*\*  $p < .001$ .

$p = .300$ ,  $\eta_p^2 < 0.001$ . However, the main effect of study on the binding foundations was significant,  $F(1, 2067) = 16.39$ ,  $p < .001$ ,  $\eta_p^2 = 0.008$ , because the binding scores in MFQ were slightly higher in Experiment 2 ( $M = 2.78$ ,  $SD = 0.92$ ; 95% CI [2.73, 2.83]) than in Experiment 1 ( $M = 2.60$ ,  $SD = 1.01$ ; 95% CI [2.54, 2.67]).

Finally, we complement the conventional null hypothesis testing provided above with a Bayesian analysis. We conduct one-way Bayesian ANOVAs using JASP (Version 0.13; JASP Team, 2020) to quantify the evidence against the main effect of cognitive manipulation on moral foundations (Rouder et al., 2012; Wagenmakers, Marsman, et al., 2018). We report Bayes factors that measure the strength of evidence in favor of the null hypothesis (i.e.,  $BF_{01}$ ), where  $BF_{01} > 1$  indicates evidence against the main effect of time-pressure (Wagenmakers, Love, et al., 2018). Using the MFQ scores across the two experiments,  $BF_{01}$  was 19.32 for the individualizing foundation and 20.05 for the binding foundation. Similarly, using the MFV scores in Experiment 2,  $BF_{01}$  was 15.38 for the individualizing foundation and 14.90 for the binding foundation. These results indicate strong evidence against any effect of the cognitive manipulations.

#### 4. Discussion

Here, in two preregistered large-sample experiments, we failed to find any reliable effect of intuitive thinking on moral foundations. In Experiment 1, answering MFQ items under time-pressure led to no difference compared to the control condition. We argued that familiarity with the MFQ items may make it difficult to elicit spontaneous reactions. Consequently, we also elicited answers to the more novel, contextualized MFV items in Experiment 2. However, the results of Experiment 2 also indicated no effect of time-pressure on either MFQ or MFV. These findings are inconsistent with studies showing that cognitive style affects moral judgments (Van Berkel et al., 2015; Yilmaz & Saribay, 2017a), and in particular, with the claim that conservatives

become more liberal when thinking intuitively (Wright & Baril, 2011).

##### 4.1. Implications

The scientific quest for the intuitive foundations of human morality is ongoing. As detailed in our Introduction, there are currently two contrasting perspectives in the literature. On the one hand, Haidt and Kesebir (2010) argue that all humans share five distinct moral foundations. On the other hand, Jost (2012) argues that only two moral foundations are common to us all. In the first direct comparison of these two views, Wright and Baril (2011) tested whether cognitive resource depletion or cognitive load—as compared to a control group— influences moral foundations, but they failed to find a main effect. However, when the two cognitive manipulation conditions were pooled, a decrease in the binding foundation scores was identified on conservatives as compared to liberals. We attempted to replicate this finding in two large-sample experiments but found neither a main effect of intuitive thinking nor its interaction with political ideology.

Another ongoing debate in the literature is whether moral foundations questionnaire items are resistant to experimental manipulations (Deppe et al., 2015; Talhelm, 2018; Talhelm et al., 2015; Yilmaz & Saribay, 2017b). While Deppe et al. (2015) reported no effect of analytic thought manipulations on various moral attitude items, Yilmaz and Saribay (2017b) conceptually and experimentally distinguished between contextualized and stable opinions. Therefore, we here compared the frequently used MFQ items with the less familiar MFV items. Still, we failed to find evidence for our hypothesis that the effect of intuitive thinking on moral foundations depends on whether questionnaire items are novel and contextualized.

##### 4.2. Failure of the hypothesis tests

Null results can have various explanations. First, statistical tests may

**Table 6**  
Interaction of cognitive manipulation with economic conservatism in Experiment 2.

	MFQ				MFV			
	Individualizing		Binding		Individualizing		Binding	
	B	t	B	t	B	t	B	t
Time-pressure	-0.02	0.16	-0.08	0.55	-0.09	0.86	-0.09	0.76
Economic conservatism	-0.10***	5.12	0.17***	7.81	-0.04*	2.30	0.06***	3.49
Interaction	0.01	0.41	0.03	0.87	0.02	0.85	0.02	0.91
Constant	4.11***	52.09	2.08***	21.33	3.98**	53.07	2.61***	30.99
R <sup>2</sup>	0.042		0.122		0.006		0.030	

Robust standard errors.

\*  $p < .05$ .

\*\*  $p < .01$ .

\*\*\*  $p < .001$ .

be inadequately powered. Second, experimental manipulations may be weak. Third, the dependent variable may not be sensitive enough, or it may suffer from floor or ceiling effects. Fourth and finally, the effect under investigation may be spurious. Considering the first possibility, the total sample size in our study ( $N = 3275$ ) was more than seventeen times the sample size of the original study by Wright and Baril ( $N = 187$ ). Against the second possibility, our preregistered checks provided evidence that manipulations worked as intended. Allowing us to disregard the third possibility, we failed to find evidence for our hypotheses on two different measures of moral foundations, and we did not encounter any ceiling or floor effects.

Although we cannot conclusively rule out the first three possibilities, the fourth possibility—that the effect may be spurious—seems somewhat more likely than the others for two reasons. First, the claim of Wright and Baril (2011) is based on a single experiment with low statistical power. Similarly, Wright and Baril (2011) rely on the pooling of two otherwise independent experimental manipulations to identify an effect, and it is not clear whether there are meaningful differences between the experimental and control conditions when viewed separately. Second, other findings in the literature conflict with the finding of Wright and Baril (2011)—such as an increase in the authority foundation with cognitive load (Van Berkel et al., 2015), and increase in the care and fairness foundations with analytic thinking but without interaction with political ideology (Yilmaz & Saribay, 2017a). Hence, our results, as well as the findings in the literature, when viewed together, suggest that the underlying effect put forward by the original finding, may be difficult to replicate.

#### 4.3. Limitations and future directions

Even though we used tests powerful enough to detect small effect sizes across multiple dependent variables, our study suffers from various limitations as well. First, our findings may be limited in their generalizability because both experiments recruited participants from the same participant pool (i.e., MTurk). Second and relatedly, despite being a well-established method frequently used in the literature to activate intuitive thinking (e.g., Isler et al., 2018), time-pressure arguably may have had limited effect on participants on MTurk, many of whom are motivated by income and who may therefore routinely complete studies at a fast rate (Mason & Suri, 2012). Our data do not clearly support this interpretation. As compared with the control conditions, average completion times of survey items in the time-pressure conditions were 47.2% faster in Experiment 1 (2.47 vs. 4.68 s), 42.9% faster for MFQ (2.93 vs. 5.13) and 48.5% faster for MFV (3.54 vs. 6.88) in Experiment 2. Even in absolute terms, average response times in the time-pressure conditions were well within the intuitive response duration used in the literature (e.g., within 4 s in Myrseth & Wollbrant, 2017). Nevertheless, it may be the case that the control conditions did not sufficiently promote reliance on deliberation or avoidance of intuitive thinking. Hence, future studies on moral foundations can benefit from forced time-delay conditions in place of control conditions. Alternative experimental methods for activating intuitive and reflective thinking (such as reflection training; e.g., Isler et al., 2020) or designs for comparing them (such as within-subjects comparisons of time-pressured and delayed decisions; e.g., Clifford et al., 2020; Yilmaz & Isler, 2019) should also be considered.

#### 4.4. Conclusion

Our study was designed to reconcile the mixed findings on the effect of intuitive thinking on moral foundations by testing the role of questionnaire items in moderating the identification of this effect. Neither of our high-powered experiments indicated evidence for any such effect, and our internal Bayesian meta-analysis provided strong evidence against it, suggesting that previous findings may be spurious. To achieve more conclusive generalizations, the robustness of our findings

should be tested in different populations using improved methods.

#### Open practices

The experiments in this article earned Preregistered, Open Data and Open Materials badges. The preregistration documents, the datasets, the experimental materials, and the analysis codes are available at <https://osf.io/pdw4q/>.

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