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Karma and honest behavior: An experimental study☆

Juliane V. Wiese

University of Warwick, Warwick Business School, Scarman Road, Coventry CV4 7AL, United Kingdom

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ABSTRACT

We examine the impact of karma beliefs on honest behavior. Previously studied strategies to promote honesty are either susceptible to rationalization or lack universality due to their religious ties. Karma, though rooted in religious contexts, is widely understood and accepted by secular society today. Its supernatural quality and simultaneous compatibility with atheism make it a widely appealing mechanism for prosocial behavior. Using two large online Prolific samples of US residents (N = 1,045 and N = 2,149), we test whether priming participants with karmic thoughts increases honesty in two anonymous online games: a coin toss where a "winning" outcome results in additional pay-out of $1, and a die roll where additional pay-out is determined according to the number on the die. We find that when there is a financial stake to winning, approximately 40% of participants lie completely in the coin toss and 11% of participants lie at least partially in the die roll. Furthermore, when partial lying is a possibility (die roll sample), we find that karmic nudges causally and significantly reduce dishonesty. Our results suggest that reminding people about karma has the potential to reduce people's propensity to cheat.

1. Introduction

In certain parts of the world, belief in karma, the idea that good deeds beget good consequences, and bad deeds beget bad consequences, has motivated important policy action. Buddhist monks in Northern Thailand began practicing "tree ordination" after a story emerged of loggers encountering bad karma after having felled a sacred bodhi tree (Morrow, 2012). By ordaining trees, these monks have preserved wildlife and forests that would otherwise have been logged. This concept has spread to neighboring countries, including Laos, Cambodia and Indonesia. While historically rooted in ancient religions dating back thousands of years, including Buddhism, Hinduism, and Jainism (Olivelle, 2023), the idea of karma has woven itself into secular Western society, evidenced by sayings like "you reap what you sow" and "what goes around comes around." Nearly 40% of Canadian nationally representative survey respondents score above the karma scale midpoint, and this figure doubles when examining beliefs of Indian respondents (White et al., 2018). Nevertheless, karma has been scarcely examined as a policy tool to increase prosocial behaviors such as honesty among the general population.

The behavioral science literature has examined numerous mechanisms and incentives to promote honesty. This includes societal (Bateson et al., 2006; Yaniv et al., 2020) and self -imposed triggers (Mazar et al., 2008; Ploner & Regner, 2013), as well as religious pressure (Shariff & Norenzayan, 2007). However, despite its widespread recognition as a concept (White et al., 2018), karma is a little-studied mechanism in the field of psychology and decision-making. While it has been linked with charitable prosocial behaviors like donating time or money to noble causes (Kulow & Kramer, 2016; Converse et al., 2012), it has yet to be studied in the context of the honesty literature. Nevertheless, karma is a psychologically motivating concept because it empowers individuals to take control of their future via their present choices. While karma has been found to motivate donations of time and money, it is possible that these behaviors were driven by a desire to make a good impression on the experimenter. Much less is understood about whether karma motivates other, less conspicuous prosocial behaviors, particularly in an anonymous setting. We aim to explore the effectiveness of a karma intervention on honesty, where the experimental design ensures that the participant's behavior cannot be observed.

Section 2 discusses how the existing literatures on criminal behavior, honesty, illusion of control, and belief in a just world create a foundation for our theory which links belief in karma with honesty. Section 3 provides an overview of the methods, hypotheses, and analytical plan, Section 4 presents and discusses the results, and Section 5 offers concluding thoughts.

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E-mail address: juliane.wiese@warwick.ac.uk.

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2. Background literature

To provide a better understanding of how belief in karma may influence an individual's decision to commit a moral crime, we turn to a stylized model of criminal behavior. According to Gary Becker's model of criminal behavior, a person will choose to commit a crime when the expected benefits of the crime exceed its expected costs (Becker, 1968). His-model accounts for the benefits and costs of crimes to individuals and society. It can also be applied when considering the costs and benefits of moral transgressions.

Becker's model has led to a rich literature on moral decision-making. As with any legal crime, an individual making a moral decision must determine whether the benefits of a moral act, perhaps to society or to one's own psyche, outweigh the costs. While cheating and dishonest behavior prevail (McCabe et al., 2001), there is an abundance of evidence which shows that people do not always cheat as much as possible (Mazar et al., 2008), in part due to the choice architecture and framing designed to elicit a psychological burden from dishonest behavior (Mazar & Hawkins, 2015; Steinel et al., 2022). This finding has been exploited to increase tax compliance by designing bureaucratic processes in such a way that the default option elicits honesty, and the dishonest choice becomes psychologically burdensome due to the action required (Behavioural Insights Team, 2011; Sunstein, 2013). Nevertheless, the sum of numerous small moral transgressions can amount to a substantial level of dishonesty. To address this, behavioral science researchers have tested nudges that incentivize honest behavior by making the cost of dishonesty more salient.

The cost of dishonesty can take numerous forms, but there are three main desires which trigger honest behavior: to avoid societal judgment through norm-breaking behavior (Bateson et al., 2006; Yaniv et al., 2020; López-Pérez, 2012), to maintain one's self-concept (Mazar et al., 2008; Ploner & Regner, 2013), and to comply with one's religious convictions (Shariff & Norenzayan, 2007). In an experiment where participants freely claim the remainder of their experimental payoff, Schitter et al. (2019) find that reducing anonymity increases honesty. In a study that examines dishonesty where lying can be either observed or unobserved, reputational concerns persist even in online experimental settings (Hermann & Brenig, 2022). In addition, participants who are exposed to an image of a set of watchful eyes pay for their drinks in an honesty box system nearly three times as much as participants exposed to a control image (Bateson et al., 2006). Honor codes, which prompt students to write a written pledge declaring that they have not cheated, are shown to be highly effective in reducing dishonest behavior (Shu et al., 2011; McCabe et al., 2002). Other experimental evidence shows that God-related priming serves as a moral guide in an anonymous economic game (Shariff & Norenzayan, 2007), although there is some mixed evidence on the positive effect of religiosity on cheating behavior (Norenzayan and Shariff, 2008).

Despite these advances in discovering strategies that curb dishonesty, individuals continue to cheat by rationalizing. Rationalizing, e.g., reducing cognitive dissonance or justifying choices post hoc, is a uniquely human psychological trait that allows us to make excuses for ourselves when we do not behave according to our principles (Yong et al., 2020; Kurzban, 2011). For example, individuals can use moral licensing to justify dishonest behavior by recalling instances in which they behaved honestly. Alternatively, a student could protect his or her self-concept despite cheating by citing reasons for which he or she was already disadvantaged compared to other students (e.g., job responsibilities, family stress, etc.). Designing an honesty-incentivising intervention that prevents such rationalization and appeals to universal concepts which do not rely on theism is an important avenue to pursue in the honesty literature. Belief in karma is a promising strategy to developing such an intervention.

Karma, as perceived in Western society today, circumvents the possibility of rationalization because of its non-theistic supernatural quality, which considers every action, whether observable or not. This prevents rationalization from taking place precisely because it is an incomprehensible and yet omniscient concept that ensures an overall balance of justice in the world, without requiring the acceptance of a theistic deity (Olivelle, 2023). It has been shown to be an effective concept in other studies that lack religious context (Converse et al., 2012; Kulow & Kramer, 2016). Furthermore, where the other honesty interventions highlight explicit costs to dishonesty (societal- or self-image), karma highlights the implicit cost of dishonesty, as individuals forego the possibility of positively influencing an uncertain future. As individuals make a present moral decision over which they have complete control, the law of karma allows them to gain improved odds of a favorable outcome for some uncontrollable event in the future by way of *karmic investment*.

Why might eliciting karmic beliefs deter subsequent dishonest behavior? The illusion of control and belief in a just world literatures can offer some insights into the psychological benefits of a belief in karma. In the illusion of control literature, individuals confound skill and chance in their experiences by believing that their skills can influence purely luck-based outcomes (Goffman, 1967; Henslin, 1967). Similarly, belief in karma allows individuals to use moral behavior to gain a sense of control over otherwise uncontrollable outcomes. This control affords a sense of stability in an otherwise seemingly incomprehensible world order.

This search for stability echoes the need for order amongst people with a belief in a just world. Historically, belief in a just world has been linked to patterns of victim blaming and justification of unjust situations, as just-world believers aim to give reason and sense to otherwise disordered and unjust events (Lerner & Simmons, 1966). Doing so protects their optimism, well-being, and life satisfaction (Correia et al., 2009; Jiang et al., 2016). Just-world beliefs and belief in karma are positively correlated with one another (White et al., 2018). Both rest on a stable concept of the world in which actions and consequences are morally aligned.

However, they are distinct in that belief in a just world tends to be backward looking and explanatory (e.g., this patient is ill because of his previous actions), whereas karma tends to be forward looking and motivating (e.g., do something good so that good karma will ensure a positive future). This difference also helps to explain why we expect a negative association between belief in karma and dishonesty, despite the existing positive associations between just-world beliefs and dishonesty (Wenzel et al., 2017), and just-world beliefs and antisocial behavior (Bègue & Muller, 2006; Sutton & Winnard, 2007; Dalbert, 2009; Hafer & Sutton, 2016). Where belief in a just world relies on a static conception of the world in which people will get what they deserve regardless of one's own actions, karma affords a greater sense of control over one's future, providing a motivation to behave morally, and therefore honestly.

Furthermore, the existing literature provides evidence that links belief in karma with other prosocial behavior. Karmic beliefs are correlated with an increased likelihood to make prosocial choices when those decisions are centered around other-gains, rather than self-gains (Kulow & Kramer, 2016). Other research has therefore examined the causal effect of karma primes on prosocial behavior. When prompted to reflect on important events that are beyond one's personal control, individuals are more likely to donate time or money to charities than when they are asked to reflect on a neutral topic (Converse et al., 2012). This suggests that making a connection between present behaviors and future outcomes, which falls into the scope of karmic beliefs, increases a willingness to give to others, perhaps in a hope to reap the benefits later. Nevertheless, the literature on karma and prosocial behavior remains scarce, and up until now, there is no existing evidence on the link between belief in karma and honesty. This study aims to fill this gap.

3. Method

3.1. Data

We report all manipulations, measures, and exclusions in this study. 1,400 US-based Prolific (www.prolific.co) users aged 18 and above were invited to constitute our coin toss sample by joining an online randomized controlled trial on individual experiences and aspirations. They were randomized evenly across control and three different karma conditions, each of which elicit karmic thinking in different ways: implicit, explicit, and implicit around a negative event. 1,395 participants completed the survey and 3 participants were excluded due to duplicate submissions. Significant delays in data collection of the negative implicit karma condition resulted in a statistically imbalanced sample (see Table A.1 in Online Appendix A). This condition is therefore included only in complementary analysis in Online Appendix A, with results consistent with the main coin toss analysis. Excluding this condition from the main coin toss analysis results in a final analysis sample of 1,045. Our sample size allows for 80% power to detect an effect size of 0.09. In addition, it is slightly larger than the simulation studies of previous research on measuring cheating behavior in a coin toss game (Moshagen & Hilbig, 2017).

Data collection of the coin toss sample's control, implicit, and explicit karma conditions took place in June and July 2021, and collection of the implicit negative karma condition in September 2021. Participants gave informed consent and were compensated $0.75 for completing the survey, and an additional $1.00 if reporting a winning coin toss in the coin toss game.

The final coin toss analysis sample of 1,045 consisted of three conditions: control, implicit, and explicit karma ($N = 374$, 336, and 335, respectively). 54% were females, 65% were ethnically White, and 45% of participants had yearly pre-tax earnings at or above $60,000. The mean age was 35. Descriptive summary statistics of the dataset are contained in the balance checks, which confirm that our sample was balanced on observable characteristics across control, implicit, and explicit treatment conditions; see Table 1.

Following analysis and submission of our results and in response to suggestions from the reviewers and the editor, we replicated our study with a new outcome variable that would allow us to obtain a more precise measurement of lying by creating the opportunity for partial lying: die rolls. 2,149 US-based Prolific users aged 18 and above, who had not already completed the coin toss experiment, were randomized into one of the three experimental arms: control, implicit, and explicit karma ($N = 776$, 704, and 669, respectively). 49% were females, 74% were ethnically White, and 50% had yearly pre-tax earnings at or above $60,000. The mean age was 40. Descriptive summary statistics of the dataset are contained in the balance checks, which confirm that our sample was balanced on observable characteristics across all three treatment conditions; see Table 2.

Data collection took place in January 2023. Participants gave informed consent and were compensated $0.75 for completing the survey, as well as a bonus payment depending on their reported die roll outcome.

Table 1
Balance checks on observable characteristics of experimental arms of coin toss sample included in the main analysis.

	Control (0)	Implicit Karma (1)	Explicit Karma (2)	(0) vs. (1), p-value	(0) vs. (2), p-value	(1) vs. (2), p-value
Female	0.54	0.60	0.56	0.122	0.701	0.256
Age	35.19	35.08	34.41	0.910	0.398	0.488
Income at/above $60,000	0.50	0.48	0.53	0.713	0.453	0.273
Undergraduate+	0.73	0.71	0.76	0.636	0.499	0.268
White	0.64	0.66	0.64	0.717	0.936	0.666
Religion (std. factor)	0.02	−0.03	−0.07	0.521	0.257	0.627

Table 2
Balance checks on observable characteristics of experimental arms of die roll sample included in the main analysis.

	Control (0)	Implicit Karm (1)	Explicit Karma (2)	(0) vs. (1), p-value	(0) vs. (2), p-value	(1) vs. (2), p-value
Female	0.48	0.51	0.51	0.186	0.203	0.974
Age	39.37	39.41	40.78	0.961	0.068	0.077
Income at/above $60,000	0.49	0.53	0.50	0.137	0.921	0.182
Undergraduate+	0.66	0.69	0.67	0.243	0.633	0.510
White	0.74	0.74	0.75	0.956	0.624	0.670
Religion (std. factor)	−0.01	−0.01	0.02	0.894	0.516	0.613

3.2. Experimental design

After giving informed consent to join the study, participants were immediately randomized into one of the conditions. The control group was asked to write no more than 150 words about their daily routine, starting with the morning and briefly going through the day. To subtly elicit the idea of karma, we turned to an established karma intervention which prompts participants to consider desirable but uncontrollable future events in their lives. In doing so, participants associate a positive outcome in the future with virtuous behavior in the present. This has been shown to increase willingness to donate time and money to charity by 16 percentage points and $0.21, respectively (Converse et al., 2012). To adapt the intervention to an online context where participants could not ask the researcher for clarification, we implemented a modified version of the original karma intervention which elaborates slightly on how participants might think about these future events and guides them through their thinking with question prompts that were not included in the original materials. The core content of the message remains the same as in the original experiment, which emphasized that the outcome should be important to the individual and currently unknown. We refer to this condition as the implicit karma group:

> *Throughout life, we are often waiting to learn about the outcome of an uncertain event that is outside of our control. This could include, for example, news from a loved one, the result of a university, scholarship, or job application, or the result of a medical exam.* ***Please write no more than 150 words about an ongoing event in your life that is uncertain, that you have limited control over, and that you wish to go a certain way. Responses can be bulleted and organized under the following headers:***
>
> *Future event you are hopeful for but can't control,*
>
> *Things you can do to influence the event,*
>
> *Why you believe this event hasn't happened yet.*

The second prompt, "things you can do to influence the event," was intended to guide participants' thinking towards unrelated good actions that, via karma, would positively impact the outcome in question. The aim of this prompt was to subtly encourage consideration of how good karma might be espoused by prosocial action without resulting in an over-estimation of the potential karma effect size. While there is some concern that this prompt would lead participants to consider concrete actions that can directly impact the outcome, downplaying the potential impact of karma, this would result in an under-estimation of the effects of the karma intervention, suggesting that the real effect size is larger than estimated in this paper. In addition, there were concerns that the implicit karma prompt would be too subtle to drive individuals' thoughts towards karma-driven prosocial behavior. Therefore, the explicit karma group was presented with a modified version of the

passage and prompts above, which more explicitly evoke the idea that good deeds can favorably influence the outcome of an otherwise uncontrollable event:

> *Throughout life, we are often waiting to learn about the outcome of an uncertain event that is outside of our control. This could include, for example, news from a loved one, the result of a university, scholarship, or job application, or the result of a medical exam. Some people believe that we can influence the outcome of uncertain events by doing good deeds, as moral actions are rewarded in the future.* ***Please write no more than 150 words about an ongoing event in your life that is uncertain, that you have limited control over, and that you wish to go a certain way. Responses can be bulleted and organised under the following headers:***
>
> *Future event you are hopeful for but can't control,*
>
> *Things you can do to influence the event,*
>
> *Why you believe this event hasn't happened yet,*
>
> *What good deed you think you can do to influence the event.*

In addition, the explicit karma group completed the 3-question karma scale (discussed in Section 3.3.2) as part of the writing task, prior to advancing to the next stage of the experiment.

3.3. Outcome measures

3.3.1. Primary outcome

After completing the writing task, participants in the coin toss experiment were informed that due to surplus budget, a limited number of participants would receive a bonus payment of $1.00. To be eligible, participants were asked to toss a coin, and to indicate whether their coin toss matches the specified outcome on their screen (randomized as "heads" for half of the sample). Although the true outcome of any one toss was only ever known to the participant, this measure of honesty is advantageous because it allows the participant to behave comfortably without fear of other-judgment, while the investigator can infer dishonesty by comparing outcomes at the aggregate level to the known probability distribution of coin toss outcomes (Moshagen & Hilbig, 2017). To compare behavior in scenarios where participants do and don't have something to gain by winning, participants were prompted to conduct three practice tosses, report their outcomes, and then conduct and report one final toss for the real potential winnings.

In our follow-up experiment, the primary coin toss outcome was replaced with a die roll. Participants were asked to roll a die three times for practice (i.e.: no payment) and report the outcomes of their die rolls. They were then asked to roll the die a fourth and final time, where the outcome would be indicative of their bonus payoff: a roll of 1 corresponds to a bonus of $0.10, a roll of 2 to $0.20, 3 to $0.30, 4 to $0.40, 5 to $0.50, and 6 to $0. This outcome is a modified version of the cheating paradigm developed by Fischbacher and Föllmi-Heusi (2013). Our pay structure is reduced by a factor of 10 for budget purposes, given that the literature shows that there is no effect of stakes on deceptive behavior (Mazar et al., 2008; Fischbacher & Föllmi-Heusi, 2013). In addition, consistent with the coin toss outcomes, we asked participants to roll three practice dies before the final "real" die. This allows us to examine whether there is any evidence of an illicit sense of deservingness amongst participants who are lucky in their first (but unpaid) die roll.

A priori, each outcome should appear 16.67% of the time in a large enough sample. While our study design does not allow us to identify the lying status of any one individual, we are able to infer dishonesty by comparing outcomes at the aggregate level to the known probability distribution of die rolls. This design improves upon the coin toss outcome because it allows for a more precise measure of dishonesty by detecting partial lying, *i.e.*: people who lie to slightly increase their payoff without necessarily lying the maximal amount. The other outcome measures and controls remain identical to the previous analysis.

3.3.2. Secondary outcome

To measure belief in karma, participants were asked to respond to the following three statements on a 5-point Likert scale, chosen from an established karma scale (White et al., 2018), including only prompts that capture the concept of karma within one's current lifetime:

> *When people are met with misfortune, they have brought it upon themselves by previous behavior in their life,*
>
> *When people experience good fortune, they have brought it upon themselves by previous behavior in their life,*
>
> *In life, everyone eventually gets what they deserve based on their deeds.*

3.3.3. Additional controls

Participants then completed a post-experiment questionnaire, which elicited their gender, age, pre-tax household income, ethnicity, education level, region, and religiosity. Religiosity was measured via three statements which capture the role of religion in one's life on a 5-point Likert scale, marking the extent to which they are not at all true or completely true:

> *Religion is a very important part of my life,*
>
> *Religion influences how I live my life,*
>
> *I would describe myself as very religious.*

Online Appendix B contains complete experimental instructions.

3.4. Analytical plan and hypotheses

3.4.1. Prevalence of cheating

To begin, we investigate whether, on average across all treatments, people cheat when given the opportunity to earn extra money. For each population of coin tossers and die rollers, we separately estimate the proportion of dishonest individuals following the calculation proposed by Moshagen and Hilbig (2017):

$$d = \frac{p(win) - p}{1 - p}, \tag{1}$$

where p(win) is the observed winning rate and p is the baseline probability of winning, in this case 0.5 for coin tosses or 0.167 for each die roll outcome. We then conduct paired t-tests for each pair of outcomes to compare the coin toss outcomes amongst practice tosses and between each of the three practice tosses and the "real" toss, in which a winning outcome would result in additional payment.

Our first hypothesis is consistent with previous findings in the literature, which is that people cheat when they stand to gain something (McCabe et al., 2001). Given that our experimental design allows us to collect baseline data on honest behavior, we predict that when participants are not incentivized to lie in the first three practice tosses, they will behave honestly, but when given the opportunity to earn more money via a simple and unidentifiable lie during the real coin toss, that they will cheat.

> *H1: People will cheat for financial gain when given the opportunity.*

3.4.2. Causal effects of karma treatments on belief in karma

Though not the primary outcome, belief in karma gives a preliminary indication of the treatments' potential effectiveness in influencing behaviors via karmic thinking. As a manipulation check, we assume cardinality in the belief in karma variable. Using an Ordinary Least Squares (OLS) regression with robust standard errors, we estimate the treatment effects on karmic beliefs and present the results in Table 4:

$$Y_i = \alpha + \beta T_i + \gamma X_i + \varepsilon, \quad (2)$$

where Y_i represents the standardized factor for belief in karma; T_i represents a vector of treatment assignment dummies (control, implicit, or explicit karma); X_i represents the vector of covariates, including female (vs. male) dummy, age, age-squared, $60,000+ annual income (vs. below $60,000 annual income) dummy, undergraduate education or more (vs. below undergraduate) dummy, white (vs. non-white) dummy, a standardized religiosity factor, dummy variables flagging if the participant "can't choose" their response to the religiosity and karma questions, and ε represents the error term.

3.4.3. Causal effects of karma treatments on coin toss cheating

Next, we examine our main outcome of interest: cheating behavior. We assume that when reporting their final coin toss outcome participants have a latent propensity to report the outcome of their coin toss honestly. In addition, we assume that this latent propensity is a linear function of their treatment assignment T, and demographic characteristics γ, the full list of which can be found in Table 5. To estimate the probability that a subject is dishonest, we run a probit regression in the form of Eq. (3):

$$Z_i^* = \beta T_i + \gamma X_i + u, \quad (3)$$

where β and γ are parameter vectors representing the treatment effect and the matrix of covariates described in Eq. (2), and u signifies an unobservable stochastic component. The covariates included in vector X_i include: female (vs. male) dummy, age, age-squared, $60,000+ annual income (vs. below $60,000 annual income) dummy, undergraduate education or more (vs. below undergraduate) dummy, white (vs. non-white) dummy, a standardized religiosity factor, and dummy variables flagging if the participant "can't choose" their response to the religiosity questions. Belief in karma is not included as a control variable because it was collected at different points in time depending on the treatment assignment. We cannot directly observe the latent propensity to report honestly, but observe only the self-reported outcome Z, where $Z = 1$ if a participant reports winning the coin toss and $Z = 0$ if a participant reports losing the coin toss. Assuming that the stochastic component has a standard normal distribution, or $u \sim N(0, 1)$, leads to the probit model of the form $\Pr(Z = 1 \mid T, \gamma) = \phi(\beta T + \gamma X)$ where $\phi(\bullet)$ is the standard normal cumulative distribution function. Using this structure the parameter vectors $\beta$and γ are estimated using maximum likelihood estimation. Because the probit model coefficients cannot be interpreted intuitively due to their nonlinear nature, we report the marginal effects of the model instead, as shown in Table 5.

Belief in karma is prevalent in both religious and non-religious groups and there is already a demonstrable link between belief in karma and other prosocial behavior (Kulow & Kramer, 2016; Converse et al., 2012). Our hypothesis, *H2*, predicts that when asked to report the outcome of a coin toss, individuals who are prompted with a karma-inducing writing task will, on average, report their outcome more honestly than those who are given a neutral writing task about their daily routine.

> *H2: Participants primed with karma are more likely to honestly report the outcome of their coin toss than participants in the neutral condition.*

Nevertheless, we include two different karma treatments in our experiment: one which subtly triggers wanting thoughts about a future event, and the other which additionally explicitly prompts participants to state whether they believe that good and bad fortune is brought about by previous behaviors. The purpose for including the explicit karma treatment is to examine whether the subtle treatment, originally used by Converse et al. (2012), is too subtle to drive individuals' decision-making when faced with the tempting and easy choice to lie for additional survey earnings. The existing literature suggests that explicit references to supernatural agents or forces can alter behavior (Bering & Parker, 2006), and even reduce cheating (Bering et al., 2005). Based on the concern that the implicit karma treatment is too subtle, and the existing evidence which supports a link between explicit supernatural references and prosocial behavior, we develop our third hypothesis, *H3*.

> *H3: The explicit karma treatment more effectively reduces cheating behavior than the implicit karma treatment, which triggers karmic thinking more subtly.*

3.4.4. Die roll cheating

Following the analysis of Fischbacher and Föllmi-Heusi (2013), we use a one-sided binomial test to observe whether the observed percentage of each die roll outcome is significantly different from 100%/6. Based on the findings of Fischbacher and Föllmi-Heusi (2013), we predict that we will find evidence of partial lying (*H4*) and that the karma treatments will reduce this partial lying to some extent (*H5*).

> *H4: There will be evidence for partial and full lying in the die roll experiment, with higher than expected frequencies of reported* 4s *and* 5s.
> *H5: The extent of partial and full lying in the die roll experiment will be lower in the karma treatments than in the control group.*

Finally, we run an OLS regression, Eq. (4), to explore whether treatment conditions and/or higher earnings in the practice rolls are predictive of higher earnings in the real roll.

$$R_i = \alpha + \beta T_i + \gamma X_i + \varepsilon, \quad (4)$$

where R_i represents the outcome of the real die roll; T_i represents a vector of treatment assignment dummies (control, implicit karma, or explicit); X_i represents the vector of covariates, including female (vs. male) dummy, age, age-squared, $60,000+ annual income (vs. below $60,000 annual income) dummy, undergraduate education or more (vs. below undergraduate) dummy, white (vs. non-white) dummy, a standardized religiosity factor, dummy variables flagging if the participant "can't choose" their response to the religiosity questions, practice roll outcomes; and ε represents the error term.

3.4.5. Experimenter demand effect

To check for an experimenter demand effect in which participants may have figured out the purpose of the study, we examine whether treatment assignment predicts use of the words "honest," "honesty," or "karma" when asked what the experiment was about. In the coin toss sample, neither implicit nor explicit karma treatment assignments are predictive of describing the experiment using these words, $\beta = -0.04$, [95% C.I.: −0.10, 0.01], $p = 0.145$ and $\beta = -0.01$, [95% C.I.: −0.07, 0.05], $p = 0.663$, respectively; see Table C.1 in Online Appendix C. In the die roll sample, we find that both implicit and explicit karma treatments decrease the likelihood of mentioning these words in the experimental description, $\beta = -0.08$, [95% C.I.: −0.12, −0.04], $p < 0.001$ and $\beta = -0.08$, [95% C.I.: −0.12, −0.04], $p < 0.001$, respectively; see Table C.1.

While we cannot explain the surprising directionality of this effect, we examine whether suspecting the true purpose of the study impacts reported coin toss or die roll outcomes. We find that suspecting the purpose of the study is not significantly associated with the likelihood of reporting a winning (coin toss) or higher-earning (die roll) outcome, $\beta = -0.06$, [95% C.I.: −0.13, 0.01], $p = 0.107$ for the coin toss sample; $\beta = -0.14$, [95% C.I.: −0.30, 0.02], $p = 0.087$ for the die roll sample; see Table C.2. Thus, if there was an experimenter demand effect in this study, which is theoretically possible given closeness in timing of the karma and honesty tasks, its impact on participants' reported coin toss and die roll outcomes is limited. Finally, after reading all written responses to the writing prompts, only two participants in the coin toss sample were identified as having evidently not engaged with the writing prompt. All other participants, including all those in the die roll sample,

wrote sincere responses either about their daily routine, or in response to the karma prompts, including, but not limited to, hopes for reassuring medical results, growing a family, and job opportunities. Excluding the observations that did not engage properly does not change our primary findings; see discussion in Online Appendix C and Table C.3.

4. Results and discussion

4.1. Prevalence of cheating

We examine the extent to which people cheat when doing so cannot be observed, and whether these patterns change when cheating is incentivized. In our study, three unincentivized practice coin tosses yielded success rates of 50.0%, 45.7%, and 50.1%, whereas the incentivized coin toss yielded a success rate of 69.0%. We assume that participants who win would not lie and claim a loss. Therefore we conclude that no cheating behavior occurred in practice tosses 1 and 2, and using Eq. (1) that very limited cheating took place in practice toss 3 (0.501–0.5) / 0.5 = 0.2%. In contrast, the winning rate of 69.0% in the incentivized coin toss yields a dishonesty rate of (0.69 – 0.5) / 0.5 = 38.0% of participants. To contextualize this figure, other studies using coin flips to measure honesty present evidence of dishonesty rates of 17.3% (Zettler et al., 2015), 49.0% (Houser et al., 2012), and 70.8% (Bucciol & Piovesan, 2011), although the latter figure results from an experimental sample of children aged between 5 and 15. In observing lying when dishonesty is observable and unobservable, Hermann and Brenig (2022) report a dishonesty rate of 39.9% when lying is observable, and of (0.8224 – 0.5) / 0.5 = 64.5% when it is unobservable. The proportion of cheaters in our coin toss experiment, where lying is unobservable, compares more closely to the cheating rate where lying is observable in Herman & Brenig's work. Perhaps some participants worried that their lying could somehow be observed after all, and were therefore less likely to cheat.

We conduct paired t-tests of each combination of winning coin toss outcomes (practice 1, practice 2, practice 3, and real toss) to understand whether the winning rates differ significantly from one another depending on the stakes of the coin toss. Practice toss outcomes one, two, and three do not statistically differ from one another, practice tosses 1 and 2: $p = 0.061$, practice tosses 2 and 3: $p = 0.053$, practice tosses 1 and 3: $p = 0.936$. However, the success rate of the real coin toss differs significantly from all three practice tosses ($p < 0.001$ in all three comparisons). This suggests that participants have a greater tendency to cheat in the real coin toss outcome, when they have something to gain from lying. This trend holds true both in the pooled sample (Fig. 1), and across all three treatment groups (Fig. 2). Even participants in the karma treatment groups follow a similar trend.

Turning to the die roll sample, we estimate the extent of cheating in each die roll. Table 3 summarises the percentage of participants who claim each die outcome, as well as the estimated percentage of people who have lied about each outcome (in italics). We also report the results of a one-sided binomial test, which determines whether each outcome is significantly different from 100%/6. While the evidence for dishonesty in the practice rolls is spurious, a clear pattern emerges in the incentivized "real" die roll: Outcomes of 4 and 5 occur significantly more frequently than expected, providing support for *H4*. This suggests that both partial and full cheating have taken place in the die roll sample. Furthermore, Table D.1 in Online Appendix D shows that the reported die rolls for the "real" (incentivized) die roll are always significantly different ($p < 0.05$) compared to the reported die rolls for each of the practice rolls. This is true for each die roll outcome and for each paired comparison. As in the coin toss sample, the die roll sample shows a consistently dishonest behavior when participants stand something to gain from lying.

4.2. Karmic beliefs

Table 4 presents the predictive factors of self-reported karma beliefs. Where previous literature has not found a significant association between belief in karma and gender (White et al., 2018), our results suggest that females are significantly less likely to report a belief in karma than males, $\beta = -0.16$, [95% C.I.: −0.28, −0.04], $p = 0.007$ in the coin toss sample and $\beta = -0.10$, [95% C.I.: −0.18, −0.02], $p = 0.017$ in the die roll sample. Unsurprisingly, religiosity is strongly predictive of belief in karma, $\beta = 0.27$, [95% C.I.: 0.19, 0.35], $p < 0.001$ in the coin toss sample and $\beta = 0.24$, [95% C.I.: 0.19, 0.30], $p < 0.001$. This serves as a sense-check that our data is reflective of people's actual beliefs.

We find weak evidence that the explicit karma treatment impacts karma beliefs. Estimates for the explicit karma treatment effect should be interpreted with caution, as participants in this condition were asked to report their karma beliefs alongside the reflective writing prompt as part of their treatment, whereas participants in the control and implicit karma treatments were elicited their karma beliefs at the end of the experiment, during the demographics survey. In the coin toss sample, the explicit karma treatment increases average belief in karma, compared to the control conditions, $\beta = 0.22$, [95% C.I.: 0.078, 0.35], $p = 0.002$. However, this relationship does not hold in the die roll sample, $\beta = 0.023$, [95% C.I.: −0.074, 0.119], $p = 0.643$, nor in the implicit karma treatment of each sample; see Table 4 for more details and

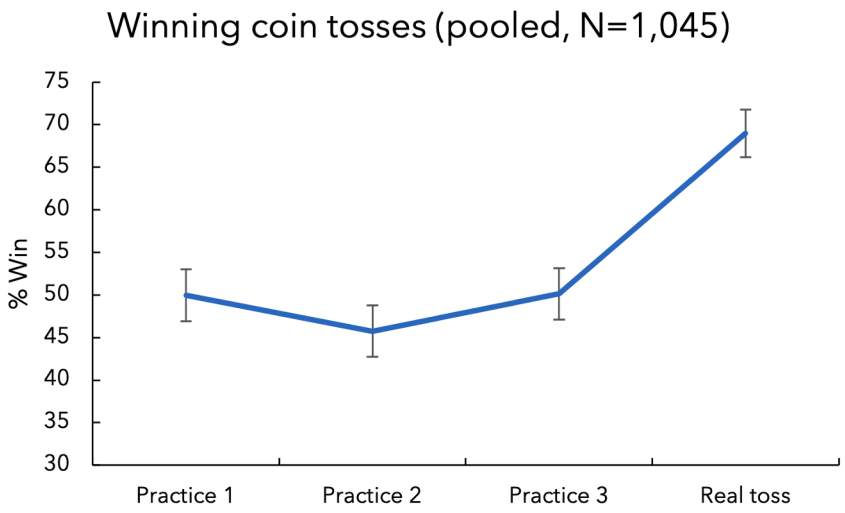


Fig. 1. Percentage of participants winning coin tosses over time, across 3 practice tosses and the final real toss, pooled sample. The standard error bars represent 95% confidence intervals.

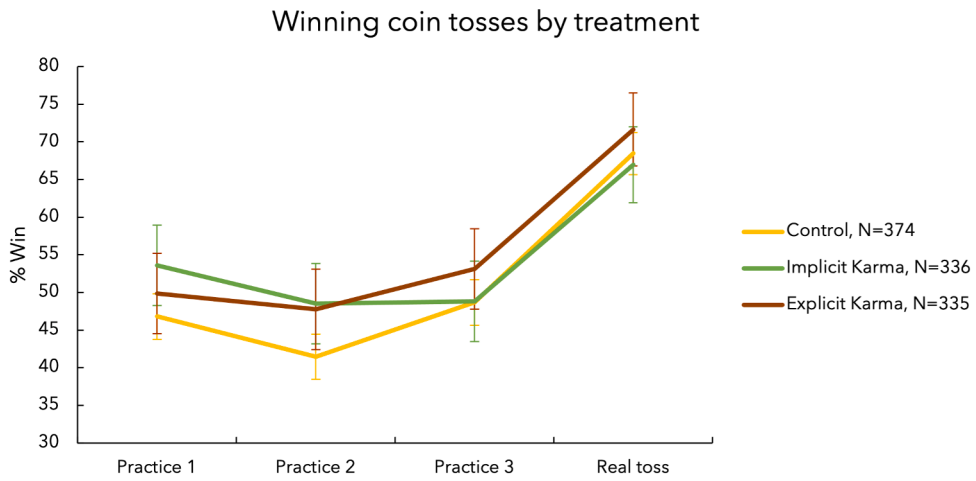


Fig. 2. Percentage of participants winning coin tosses over time, across 3 practice tosses and the final real toss, by treatment group. The standard error bars represent 95% confidence intervals.

Table 3
Summary of die roll outcomes across pooled sample and individual treatments. The first line of each row captures the percent of participants who reported the corresponding payoff. The second line (in italic) captures the estimated percent of participants who have lied about the corresponding payoff. This is calculated using Eq. (1).

		Share of subjects (in percent) who reported corresponding payoff; one-sided binomial tests that it is smaller (larger) than 100%/6. *(+) = 5%-level, **(++) = 1%-level, ***(+++) = 0.1% level					
		0	1	2	3	4	5
Pooled, N = 2,149	**Practice roll 1**	11.5***	9.3***	18.1+	21.9+++	22.2+++	17.0
		6.2	*8.9*	*1.7*	*6.2*	*6.6*	*0.4*
	Practice roll 2	13.7***	11.1***	20.7+++	18.3+	17.9	18.3+
		3.6	*6.7*	*4.8*	*1.9*	*1.4*	*1.9*
	Practice roll 3	15.2*	15.2*	14.8*	17.7	16.8	20.1+++
		1.8	*1.8*	*2.3*	*1.2*	*0.1*	*4.1*
	Real roll	7.4***	6.7***	10.7***	15.1*	28.0+++	32.2+++
		11.2	*12.0*	*7.2*	*1.9*	*13.6*	*18.6*
Control, N = 776	**Practice roll 1**	11.5***	9.3***	18.8	21.0+++	21.9+++	17.5
		6.2	*8.9*	*2.5*	*5.2*	*6.2*	*1.0*
	Practice roll 2	13.1++	11.6+++	20.6**	18.8	16.9	18.9
		4.3	*6.1*	*4.7*	*2.5*	*0.2*	*2.6*
	Practice roll 3	14.9	15.1	15.9	18.0	15.5	20.6++
		2.2	*1.9*	*1.0*	*1.6*	*1.4*	*4.7*
	Real roll	6.4***	5.3***	10.1***	14.3*	28.2+++	35.7+++
		12.4	*13.7*	*7.9*	*2.9*	*13.8*	*22.8*
Implicit karma, N = 704	**Practice roll 1**	10.9***	8.4***	19.9+	22.4+++	20.9+++	17.5
		7.0	*10.0*	*3.8*	*6.8*	*5.0*	*1.0*
	Practice roll 2	13.4**	11.6***	20.7++	17.0	18.8	18.5
		4.0	*6.1*	*4.8*	*0.4*	*2.5*	*2.2*
	Practice roll 3	15.1	17.5	11.9***	18.3	17.9	19.3+
		1.9	*1.0*	*5.8*	*1.9*	*1.4*	*3.1*
	Real roll	6.8***	7.8***	11.5***	15.5	27.3+++	31.0+++
		11.9	*10.7*	*6.2*	*1.4*	*12.7*	*17.2*
Explicit karma, N = 669	**Practice roll 1**	12.3***	10.3***	15.2	22.3+++	24.1+++	15.8
		5.3	*7.7*	*1.8*	*6.7*	*8.9*	*1.1*
	Practice roll 2	14.6	9.9***	20.8++	19.1	18.1	17.5
		2.5	*8.2*	*4.9*	*2.9*	*1.7*	*1.0*
	Practice roll 3	15.7	13.2**	16.7	16.7	17.3	20.3++
		1.2	*4.0*	*0*	*0*	*0.7*	*4.3*
	Real roll	9.0***	7.0***	10.6***	15.5	28.4+++	29.4+++
		9.2	*11.6*	*7.3*	*1.4*	*14.0*	*15.2*

Table E.1 in Online Appendix E for parsimonious models of these regressions. With this weak evidence for increased karma beliefs in mind, we examine whether the karma treatments can increase honest behavior.

4.3. Treatment effect on cheating behavior

To test for the treatment effects of the karma intervention on the probability of winning the coin toss, Column 1 in Table 5 reports the marginal effects of the probit model that regresses the treatment variable on the real coin toss outcome. We find no statistically significant effect of implicit and explicit karma treatment groups on coin toss winning outcomes $\beta = -0.02$, [95% C.I.: −0.09, 0.05], $p = 0.604$ and $\beta = 0.03$, [95% C.I.: −0.04, 0.10], $p = 0.368$, respectively. This suggests that none of the karma treatments have a significant effect on honest reporting of the coin toss.

Nevertheless, when examining the karma treatment effects on honesty in the die roll outcome, we do find significant effects of both

Table 4
OLS regression of karma treatments on standardized belief in karma with a mean of 0 and a standard deviation of 1. We correct for heteroskedasticity in our standard errors using the command *hc3* in STATA.

	(1) OLS Standardized karma factor Coin toss	(2) OLS Standardized karma factor Die roll
Implicit Karma	0.114	−0.0418
	(0.0731)	*(0.0485)*
Explicit Karma	0.215**	0.0228
	(0.0695)	*(0.0493)*
Female	−0.162**	−0.0964*
	(0.0601)	*(0.0405)*
Age	0.0100	0.00952
	(0.0134)	*(0.00840)*
Age sq.	−0.000122	−8.21e-05
	(0.000147)	*(9.28e-05)*
$60,000+	0.0264	−0.00838
	(0.0633)	*(0.0422)*
Undergraduate or above	0.00195	−0.0402
	(0.0681)	*(0.0458)*
White	0.0937	−0.00754
	(0.0660)	*(0.0491)*
Std. religion factor	0.271***	0.244***
	(0.0404)	*(0.0293)*
Coin toss/Die roll, practice and real controls	YES	YES
Religion and karma "can't choose" dummies	YES	YES
Constant	−0.316	−0.188
	(0.274)	*(0.195)*
Observations	1045	2149
R-squared	0.180	0.156

Standard errors in parentheses.
* $p<0.05$,.
** $p<0.01$,.
*** $p<0.001$.

Table 5
The effect of karmic nudges on individuals reporting winning coin tosses (1) and die rolls (2). Marginal effects obtained from running the probit model in column (1) are reported instead of coefficients. We correct for heteroskedasticity in our standard errors using the command *hc3* in STATA.

	(1) Probit Winning in the real toss	(2) OLS Winning in real die roll
Implicit Karma	−0.0178	−0.185*
	(0.0343)	*(0.0787)*
Explicit Karma	0.0302	−0.221**
	(0.0335)	*(0.0807)*
Female	−0.0600*	−0.149*
	(0.0286)	*(0.0668)*
Age	−0.0110	−2.69e-05
	(0.00648)	*(0.0140)*
Age sq.	7.52e-05	−8.06e-05
	(7.51e-05)	*(0.000154)*
$60,000+	−0.0546	0.0371
	(0.0301)	*(0.0691)*
Undergraduate or above	0.0590	0.0575
	(0.0339)	*(0.0743)*
White	0.0156	0.0491
	(0.0309)	*(0.0787)*
Std. religion factor	3.45e-05	−0.000599
	(0.0186)	*(0.0449)*
Win Practice 1	0.143***	0.152***
	(0.0277)	*(0.0225)*
Win Practice 2	−0.0246	0.0613**
	(0.0281)	*(0.0204)*
Win Practice 3	−0.0335	0.0177
	(0.0285)	*(0.0190)*
Religion "can't choose" dummies	YES	YES
Constant	–	3.095
Observations	1045	2149
R-squared	–	0.0463

treatments. Both the implicit and explicit karma treatments significantly reduce the reported die roll outcomes (where a higher roll, up to 5, results in a higher payout), $\beta = -0.19$, [95% C.I.: −0.34, −0.03], $p = 0.019$ and $\beta = -0.22$, [95% C.I.: −0.38, −0.063], $p = 0.006$, respectively; see Column 2 in Table 5. The parsimonious models can be found in Table F.1 in Online Appendix F. We find a stronger effect from the explicit karma treatment. These results support *H3* and *H5*. The fact that we find no significant effect in the coin toss experiment, but do find one in the die roll sample suggests that the coin toss cheating paradigm simply does not offer sufficient variation in dishonest behavior to detect the impact of the karma interventions. By allowing for partial cheating in the die roll experiment, we enable participants to cheat "a little bit" and can therefore estimate the intensive effects of the karma interventions at reducing the extent of cheating.

Our finding that karma interventions do not reduce dishonesty in the coin toss sample (i.e., at the extensive margin of lying) but do reduce dishonesty in the die roll sample (i.e., at the intensive margin of lying) contributes a more nuanced understanding of the impacts of a wanting exercise on honest behavior. While reflecting on an uncontrollable and important future event may reduce the extent of someone's lying, we do not find evidence to suggest that it will prevent someone from lying entirely. Despite the existing literature linking the wanting intervention with an activation of karma-related concepts (Converse et al., 2012), we do not find strong evidence that the interventions increase belief in karma. It is plausible that while an individual's fundamental beliefs about karma might not change, triggering the idea of karma in a writing exercise could nevertheless elicit more honest behavior. In order to better understand the mechanisms, this explanation requires further investigation in future research.

Our results show an interesting pattern with regards to two of the covariates included in the model: gender and the outcome of the first practice toss. In addition to being less likely to report a belief in karma, women are significantly less likely to win the final coin toss than men, $\beta = -0.06$, [95% C.I.: −0.12, −0.0040], $p = 0.036$ in the coin toss and $\beta = -0.15$, [95% C.I.: −0.28, −0.018, $p = 0.026$ in the die roll samples. This suggests that male participants are more likely to lie about the outcome of their final coin toss. While the evidence on differences in cheating behavior by gender is mixed (Karabenick & Srull, 1978; Graham et al., 1994; Jacobson et al., 1970), most studies discover more cheating behavior in males than in females (Baird, 1980; Cochran et al., 1998; Davis et al., 1992; Hetherington & Feldman, 1964; Jensen et al., 2002), with some evidence suggesting that males are simply more willing to admit to their cheating behavior than females (Whitley, 1998).

In addition, participants who won their first practice toss are significantly more likely to win the final toss, compared to participants who did not win their first practice toss, $\beta = 0.14$, [95% C.I.: 0.09, 0.20], $p < 0.001$ in the coin toss and $\beta = 0.15$, [95% C.I.: 0.11, 0.20], $p < 0.001$ in the die roll samples, see Table 5. This effect size is nearly three times the impact of being female on the likelihood of winning the final coin toss. Because the proportion of winners in the first practice toss aligns perfectly with the statistical prediction of 50.0%, there is strong evidence to suggest that most players reported this outcome honestly. However, the outcome of the final real coin toss should be statistically unrelated to the outcome of the first practice toss, as they are entirely independent events. While we do not have any conclusive explanations for this result, a possible explanation could be that participants who got lucky in their first practice toss but not in the incentivized final toss felt a sense of illicit deservingness that justified their dishonesty in claiming a win in the incentivized toss. Further research is required to investigate whether participants engage in a psychological "transfer of outcomes" from one scenario to another, to justify their dishonesty and thereby maintain their self-concept. This presents a novel form of rationalization of one's dishonest behaviors, which can help an individual avoid both external and internal forms of judgment.

5. Conclusion

This paper contributes to the dishonesty literature by providing empirical evidence on the effectiveness of karmic nudging at reducing cheating in an online, anonymous, incentivized experiment. In line with the previously established literature where karma-related messages effectively promote charitable behavior (Kulow & Kramer, 2016), our study demonstrates that karmic nudges can, in some contexts, trigger more honest behavior at the intensive margin. This study is therefore one of the first to provide evidence around the relationship between karma-related messages and honesty.

Nevertheless, there are some limitations to this work that could help to explain our results, and which could guide future research on this subject. Thinking about karma has been shown to effectively reduce selfish behavior amongst karma believers (White et al., 2019). Our population was sampled from a pool of US-based residents, where karma is not central to prevalent religious beliefs. It would be interesting and relevant to examine the relationship between karma and honesty in other cultural contexts where karmic beliefs are more prevalent, notably throughout Asia. Where the policy implications of our findings are limited in a Western context, the use of karma-related messages in Asia has the potential to be a highly effective tool in policymaking to motivate individuals to do the right thing, even at the extensive margin, where we thus far have found no effects. For example, by priming citizens to think about a culturally relevant spiritual concept which motivates good actions, i.e. karma, policymakers could reduce littering by posting karma-related messages on signs in natural areas, increase tax compliance by modifying tax forms to prompt citizens to consider their future when reporting earned income, and encourage volunteer work by making appeals for public engagement that highlight the good that comes to others and oneself from engaging in such activities. Based on the optimistic results of our findings on the impact of karma on honest behavior, future research could examine such specific, implementable, and scalable policy mechanisms that encourage other prosocial behaviors.

Finally, our finding that winners of the first unincentivized round are significantly more likely to win the final incentivized round, even though the two rounds are statistically independent events, demands further attention. While this experiment does not yield a conclusive explanation, it would be interesting to explore whether a sense of illicit deservingness is used by individuals to justify their immoral behaviors.

Declaration of Competing Interest

None.

Data availability

Data will be made available on request.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.socec.2023.102018. Analysis and data files can be found at https://osf.io/6347y/.

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