Moral Luck: Mechanisms, Robustness, and Prevalence[*]

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Abstract

In many types of decisions, individuals can influence the probabilities of good or bad outcomes by their actions, but there is still a role for chance in determining final outcomes. If punishment and rewards are conditioned on such random outcomes, this violates a property of optimal incentives. It has been posited since ancient times that humans do assign punishments and rewards based on factors outside of actors' control, a tendency called "moral luck." This paper provides new evidence on the prevalence and robustness of moral luck, and on a key open question of whether moral luck is a preference or a bias. The results are from controlled experiments that can cleanly identify moral luck, but also involve real, consequential moral choices that are a matter of life and death for a third party (a mouse). We find moral luck in punishment, and show that this is at least partly due to a bias. Our findings support a causal chain in which random outcomes lead to biased judgments and incentivized beliefs about the nature of the actor, even though they contain zero information, and this in turn causes punishments to vary with outcomes. We also show that the bias is strong enough to remain in the face of an intervention that encourages deliberation. The bias is prevalent, but not universal, it is unrelated to most demographics, and is present regardless of high or low cognitive ability or education. We also find evidence that actors exhibit internalized moral luck in how they evaluate themselves based on outcomes.

In many types of decisions, individuals can influence the probabilities of good or bad outcomes by their actions, but there is still a role for chance in determining what ultimately happens. For example, driving under the influence of alcohol may increase the probability of subsequently hitting and killing a pedestrian if a pedestrian crosses the street, but the presence of a pedestrian depends on chance. Likewise, an employee can take self-interested actions that expose the employer to increased risk of a loss, but chance will ultimately determine whether the loss occurs. More generally, in meritocratic societies, individuals can have a strong work ethic and exert high effort, but due to bad luck still end up being unsuccessful. In all of these cases, realized random outcomes do not contain any additional information about the intentions or effort of the actor beyond the observed actions. If punishments and rewards do vary with such outcomes, however, this violates a property of optimal incentives, sometimes called the "informativeness" principle (Holmström, 1979; Bolton and Dewatripont, 2004). Such violations would have profound implications for the functioning of legal systems, employment relationships, democracies, and meritocratic societies, by undermining the motivating and deterrent value of rewards and punishments.

It has been posited since ancient times that there is, in fact, a human tendency to reward or punish actors for outcomes that are beyond their control (Aristotle, 1984), a phenomenon sometimes called "moral luck," but the prevalence and robustness of this phenomenon are still not fully understood, and a key open question continues to be debated, which is whether moral luck is a preference or a bias. Understanding whether moral luck is a preference or a bias is important, because if it is a preference, having punishments vary with outcomes satisfies some notion of what is appropriate or desired, which might offset the costs of providing suboptimal incentives. If the phenomenon is a bias, however, then this raises important questions about the desirability of how punishments and rewards are determined in many areas of society. It also points to a possible value of interventions to de-bias decisions. An early contributor to the debate on mechanisms was Adam Smith, who proposed that sentiments or emotions aroused by outcomes can affect perceptions of the actor, even though they contain no information about intentions, and thereby distort attributions of merit or demerit (Smith, 1790). Moral luck has been viewed as an ethical puzzle, as it seems to violate an appealing moral principle known as the "control principle," that requires that people only be held responsible for factors under their control (Kant, 1784). More modern philosophers, however, have continued to debate whether moral luck could be due to some coherent moral preference or principle that is an alternative to the control principle (e.g., Nagel, 1979; Williams, 1981; for a survey see Dana, 2021), and the question remains a current one for legal scholars (e.g., Enoch and Marmor, 2007).

This paper provides new evidence on the existence and prevalence of moral luck, and provides evidence on the question of mechanisms, indicating that it is at least partly a bias. As a first step we show evidence of moral luck in punishment behavior. Second, we show that random outcomes influence various judgments about the character of the actor, as well as incentivized beliefs about one aspect of the preferences of the actor, despite the random outcomes containing zero information. These biased judgements and beliefs are in turn correlated with punishment behavior. Third, to complete the causal chain, we exogenously vary whether punishers are provided with information about the actor's character, and show that this significantly reduces the influence of outcomes on punishment. This indicates that impact of random outcomes on beliefs about the actor is a mechanism underlying the variation of punishment with outcomes. We check robustness of the bias to an intervention that encourages deliberative rather than spontaneous decision making, and find that an influence of outcomes on beliefs and punishment remains, although the effects are somewhat smaller, indicating that the bias is relatively deep-seated and hard to remove. Interestingly, we also find that actors tend to internalize moral luck, in terms blaming themselves differently depending on random outcomes, which indicates that the phenomenon can emerge even when actions and outcomes are unobserved.

This study uses an approach that addresses some important methodological challenges to studying moral luck. With naturally occurring observational data on punishments and rewards, e.g., sentencing decisions from court cases, a problem is the difficulty of observing the roles of chance versus actions as perceived by those deciding on punishments and rewards. Without knowing how much information people have about these factors, it is difficult to establish if punishments are varying with outcomes in a way that reflects moral luck, as opposed to just inferring hidden action from outcomes.[1] One solution is to study decisions in controlled environments, where the researcher causes outcomes to vary in ways that are plausibly or explicitly due to chance (see Robbenholt, 2020; Martin and Cushman, 2016; Gurdal et al., 2013; Brownback and Kuhn, 2019), but using such artificial environments raises the difficulty of having real, consequential moral decisions. Having consequential decisions may be important for recruiting key mechanisms in a realistic way, e.g., eliciting strong emotions. Such realism is desirable for assessing how strong the phenomenon is, e.g., in the face of interventions designed to mute emotions and de-bias behavior. This study uses a framework that combines both clean identification of moral luck, with consequential moral

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[1] If actions that influence the probabilities of good or bad outcomes are unobserved, or observed only with noise, then outcomes become informative signals of the intentions and actions of the actor, and thus the informativeness principle entails varying punishments and rewards with the outcomes to some extent. Without knowing exactly what decision makers believe, it is difficult to assess if punishments and rewards vary with outcomes to the optimal degree.

choices that are a matter of life and death for a third party (a mouse).

Experiments. In a first stage of our experiments, shown in Figure 1, subjects in the role of active players make a choice between two lotteries, denoted the moral lottery and the immoral lottery, where outcomes are consequential in that they involve life or death for a third party. Specifically, the immoral lottery involves a 70% chance that a mouse dies, and a 30% chance that a mouse is instead rescued from death. The immoral lottery gives the active player $6 for themselves, regardless of the outcome for the mouse. The moral lottery, by contrast, involves only a 30% chance of death for the mouse, and a 70% chance that it is rescued, but gives the active player no money. An active player who chooses the immoral lottery thus indicates a willingness to increase the risk of death for the mouse, in order to achieve personal financial gain, whereas choice of the moral lottery reflects a willingness to sacrifice personal gain, in order to reduce likelihood of death for the mouse.

Our study uses the mouse paradigm developed in Falk and Szech (2013), where a key feature is that the population of mice used will be killed by default, in the absence of intervention through the study, and thus the scientific study can only improve welfare for the mice. The mice in question are ordinary laboratory mice, bred by a company for, e.g., medical research, but slated to be euthanized by the company to do lack of demand. If it is determined in our study that a mouse should be rescued, our research money is used to purchase one of these "surplus mice" from the company, and allow the mouse to live out the rest of its natural life in a hygienic environment with other mice.

Figure 1: Stage 1 of experiment: Active player choices and outcomes
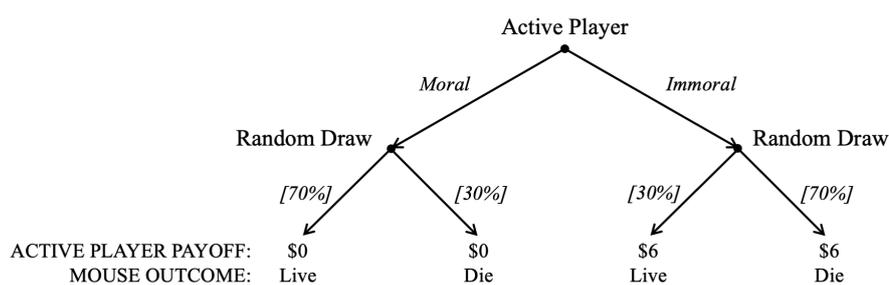
Notes: The active player first chooses one of two options, shown in the figure as *moral* or *immoral*, although more neutral, factual labels "option likely live" and "option likely die" were used with subjects. The moral choice leads to a subsequent random draw with a low probability of death for the mouse, 30%, and gives the active player no money regardless of what happens to the mouse. The immoral choice leads to a random draw with a high probability of death for the mouse, 70%, and gives the active player $6 regardless of what happens to the mouse. Note that the default for such surplus laboratory mice is to be killed, so the study is rescuing mice.

The first stage of our study also elicits traits and judgements of the active players.

Specifically, we measure an active player's "value of the life of a mouse" using a question asking how much they would need to be paid, in order to allow a mouse to die for sure. In addition, the study measures active players' judgements about, e.g., the morality of their own choice, and whether they see themselves as a good person, after learning what happens to their mouse. The active players also have an additional, "pending payment" of $12; how much of this they receive depends on the choices of spectators in stage 2 of our experiment. We use university students as active players (N=562).

In the second stage of our experiment, which was pre-registered, we recruit a large sample of US adults to participate in online experiments in the role of spectators; our main treatment, Treatment Main, has N=2,200. We explain the concept of surplus mice to spectators, and elicit their (hypothetical) value of a life of a mouse. As was pre-registered, our analysis focuses only on spectators who have more than a minimal value for mice, to eliminate those who might dislike mice and thus not view active players as facing a moral dilemma. Spectators are given an endowment of $6, and can choose how much of this to spend, in order to reduce the pending payment of an active player. As shown in Figure 2, our design matches a given spectator with a sequence of four active players, so that they see each possible combination of choice and outcome for the mouse. The order of seeing the different active players with different possible choices and outcomes is randomized across spectators, to address any possible order effects. Spectators make a choice of how much money to deduct from each of the four active players, knowing that only one of the four choices will be randomly selected to potentially be implemented. In this sense, our design is an example of the "strategy method," where subjects make choices without knowing for sure which case will be realized. Spectators knew that multiple spectators might be matched to a given active player, in which case it would be randomly determined which spectator's choice was used to determine the active player's payoff. This design allows a within-subject analysis. It can thus can speak to individual heterogeneity in a tendency to condition punishments on random outcomes, as well as the robustness of such a tendency to making the different possible choices and outcomes of actors salient to the spectator.

The study also elicited judgements of the spectators about the fourth active player they saw, e.g., in terms of morality of the choice, and whether the active player was a good person. The elicitation asks only about the final active player a spectator saw, to reduce complexity of asking about all previous active players, and to focus on the one that was discussed most recently. We can compare across spectators how choices and outcomes affect judgements and emotions, because order is randomized. We also elicit incentivized beliefs of the spectator, about how the active player answered the question about value of the life of a mouse, paying the spectator for correctly guessing the money

Figure 2: Stage 2 of experiment: Spectator punishment choices, judgements, and beliefs
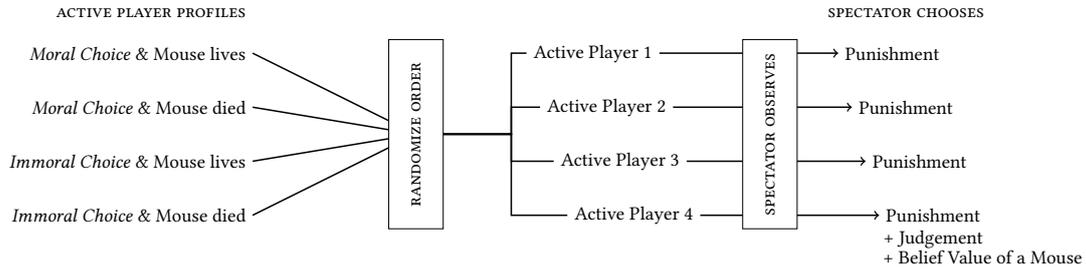
Notes: The spectators see a sequence of four different active players, with each possible combination of choice and outcome. The order is randomized across spectators. For each active player, the spectator has $6 to spend on punishment, with each dollar spent deducting two dollars from a pending $12 payoff of the active player. Spectators are asked for judgements and beliefs about the fourth active player that they see. Spectators know that one of the four active players will be randomly selected, and their punishment choice in that case will affect their payoff and potentially the payoff of the active player.

range indicated by the active player.

The rest of the study measures additional traits of the active player, and also assesses whether spectators exhibit moral luck in their judgements of hypothetical scenarios that span a range of contexts from crime, to politics, to economic interactions. Key traits that are measured include cognitive ability, captured by the cognitive reflection test (CRT) and a subset of Raven's progressive matrixes. We also ask about educational attainment. The questionnaire elicits agreement with the control principle, beliefs about the role of chance in determining outcomes like poverty in the US, and political affiliation and self-reported conservatism. Additional demographics include traits such as gender, age, and religion.

Results. Figure 3 shows our first set of results from Treatment Main, on whether there is moral luck in punishment choices. The figure shows average punishment levels by choice of the active player and outcome for the mouse, using all choices of spectators for a within-subject analysis. We see that punishments are on average significantly higher for active players who choose the immoral lottery, compared to those who choose the moral lottery, consistent with spectators sanctioning an immoral choice (OLS; s.e. clustering on spectator; $p < 0.001$). Punishments also vary significantly, however, with the outcome for the mouse, conditional on the active player's choice. For both the moral choice and the immoral choice, active players are punished significantly less if the mouse lives than if the mouse dies (OLS; s.e. clustering on spectator; $p < 0.001$, $p < 0.001$). Punishment choices thus violate the informativeness principle, in that active players are not being punished solely based on factors under their control. Results are similar and also statistically significant in a between-subject comparison, using only first choices of spectators (see Figure A1). This shows that the result is robust in the

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sense that it is not confined to within-subject contrasts. These findings raise the question whether moral luck in punishment reflects some alternative moral principle, or whether instead it is a mistake or bias.

Figure 3: Punishment levels by active player choice and outcome in Treatment Main
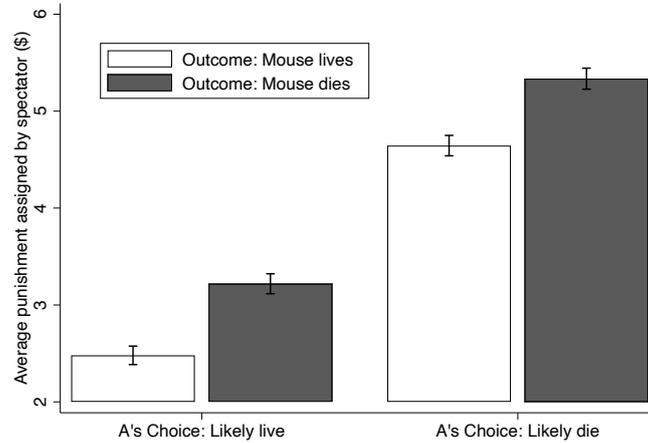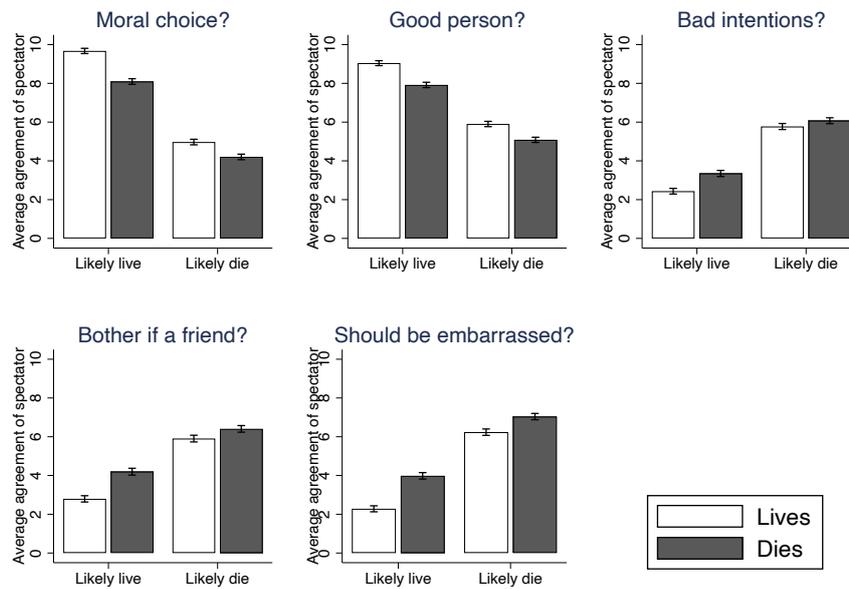

Notes: Average punishment levels for each of the four cases. "A's choice" refers to Active Player's choice. Each spectator chooses punishment for all four cases so there are four observations per spectator (within-subject comparison). Figure shows standard error bars clustering on spectator.

Figures 4 and 5 explore one possible explanation (pre-registered), which is that punishments might vary with random outcomes because these influence judgements and beliefs about the nature of the active player, despite the fact that the outcome conveys zero information above and beyond the observed choice. Figure 4 shows that for both the moral and immoral choice, the mouse dying causes spectators to judge the active player's choice as relatively more immoral, and to agree less that the active player is a good person. It also causes spectators to agree more strongly that the active player should be embarrassed, and that the active player had bad intentions. The impact of the mouse dying on each of these judgements is statistically significant, for both the moral and immoral choice, although interestingly, the effect of outcomes on most judgements is significantly stronger for the moral choice (see Table A1). Results are robust to controlling for spectator characteristics (see Table A2). We also elicited spectator beliefs about the active player's value of a mouse, because this might be a belief that is biased by whether the mouse dies, and potentially relevant for spectator punishment decisions. The belief measure also has the advantage that it can be incentivized for accuracy. Panel (a) of Figure 5 shows that the mouse dying significantly influences spectator beliefs about the active player's value of the life of a mouse (t-test; $p < 0.001$), particularly for the moral choice, where the effect is very large and individually significant (t-test; $p < 0.001$). The effect for the immoral choice is also positive, but smaller

and not statistically significant individually (t-test; $p < 0.13$), and the effect is also significantly smaller than for the moral choice (OLS; $p < 0.001$). Finding a smaller effect for the immoral choice is consistent with the results on judgements. One explanation for these findings could be that spectators find the immoral choice as relatively more informative about the nature of the active player, and also recognize that it is consistent with only a relatively narrow range of values for life of a mouse, and this acts as a constraint on how much outcomes bias judgments and beliefs.

Figure 4: Judgements by choice and outcome in Treatment Main

Notes: Average agreement levels for each of the four cases. Each spectator judges one case so there is one observation per spectator (between-subject comparison). Figure shows standard error bars.

Despite conveying no information, the random outcomes influence judgements and even incentivized beliefs about the active player. If this is a mechanism underlying moral luck in punishment, one would expect punishment choices to be explained by variation in the judgements and beliefs. Table 1 presents evidence that this is the case. Columns (1) and (5) show that spectators who have a less favorable judgements of the active player's choice or nature punish significantly more. Column (6) shows that incentivized beliefs about the active player's value of the life of a mouse are also significantly related to strength of punishment, with punishment decreasing in beliefs about how much the active player values a mouse. The results are robust to adding controls for other factors that matter for punishment choices, including choice and outcome of the active player, and spectator characteristics including own value of a mouse, income, education, gender, and educational attainment (see Table A3). These findings are consistent with the bias in perceptions of the active player, caused by the (uninformative)

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Figure 5: Spectator beliefs about the active player's value of the life of a mouse
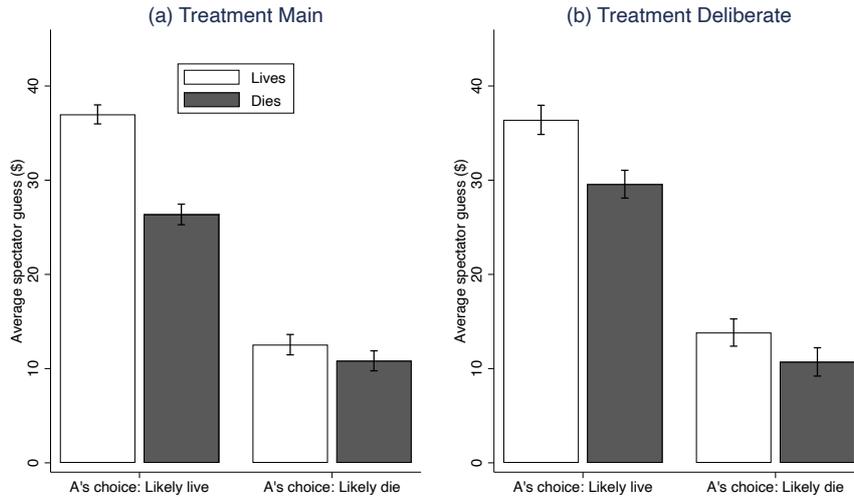
Notes: Average incentivized guess about the active player's value of the life of a mouse. Each spectator makes a guess for one case so there is one observation per spectator (between-subject comparison). Panel (a) shows results from Treatment Main, Panel (b) from Treatment Deliberation. Figure shows standard error bars.

random outcomes, being a mechanism behind why punishment varies with random outcomes, but the evidence is correlational.

To provide evidence on whether the impact of random outcomes on perceptions of the active player is a causal mechanism explaining moral luck in punishment, we conducted a second treatment, Treatment Revealed Value (N=1,000). In this treatment, spectators learned the active player's value of the life of a mouse, along with the choice and the outcome for the mouse. Each spectator was matched with four active players, two who chose the moral lottery and two who chose the immoral lottery. The active players who chose the moral lottery had different outcomes for the mouse, but had the same (high) value of a mouse, while the active players choosing the immoral lottery had different outcomes but the same (low) value of a mouse.[2] The key feature of the design is that value of a mouse was known to the spectator, and constant across active players with different outcomes, so there was no scope for random outcomes to influence beliefs. If part of the reason why punishment varies with outcomes in Treatment Main is the bias in beliefs about the active player value of a mouse, we would expect moral luck to be weaker in Treatment Revealed Value.

As shown in Figure 6, we find that punishment does, indeed, vary substantially less with the random outcome in Treatment Revealed Value compared to Treatment Main, a

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[2]The information conveyed about value of a mouse was calibrated to be line with priors conditional on choices. We used the modal guesses of spectators in Treatment Main, about values of active players choosing the moral or immoral lotteries, respectively, and selected active players with these values to use for the matching.

Table 1: Punishment choices in Treatment Main as a function of judgements and beliefs
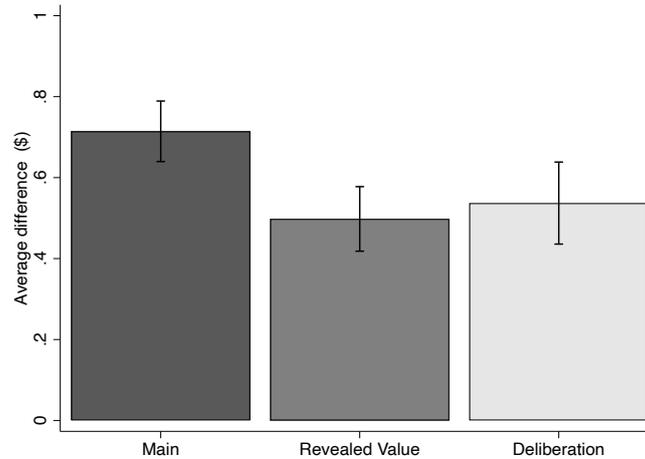
	Punishment ($)					
	(1)	(2)	(3)	(4)	(5)	(6)
Moral choice	-1.39***					
	(0.10)					
Good person		-1.26***				
		(0.11)				
Bad intentions			1.34***			
			(0.10)			
Bother if a friend				1.27***		
				(0.11)		
Embarassing					1.30***	
					(0.10)	
Belief about active player						-0.77***
						(0.10)
Constant	3.80***	3.84***	3.75***	3.77***	3.75***	4.02***
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.11)
Observations	1441	1446	1443	1446	1446	1446
Adjusted R^2	0.127	0.100	0.112	0.096	0.105	0.037

Notes: OLS regressions. The dependent variable is punishment in dollars of the fourth active player seen by the spectator. Independent variables include self-reported judgements about the active player given information about the fourth active player's choice and outcome for the mouse: Morality of active player's choice; active player is a good person; it would bother the spectator if active player was a friend; active player had bad intentions. Another independent variable is the spectator's incentivized guess about the active player's value of the life of a mouse, in dollars. All independent variables are standardized, so coefficients give the impact of a one standard deviation increase in the independent variable. Each spectator makes judgements and reports beliefs for one case so there is one observation per spectator (between-subject comparison). Robust standard errors in parentheses.

reduction of about 30 percent. This treatment difference is also statistically significant ($p < 0.05$; see Table A4). Moral luck is still, however, present and significant in Treatment Revealed Value, with punishment significantly stronger for cases when the mouse dies (t-test; $p < 0.001$). This is not unexpected, given that Treatment Revealed Value only eliminated the bias in a single aspect of how the active player is perceived, among a bundle of different types of judgments that are influenced by random outcomes and all seem to matter (correlationally) for punishment. Indeed, we find that judgements about the active player are still significantly skewed by random outcomes in Treatment Revealed Value, and these effects are not significantly different from in Treatment Main (see Table A5).

In another treatment, Treatment Deliberation, we investigate whether moral luck is robust to encouraging deliberative rather than intuitive thinking. We prime individuals to deliberate, through an essay asking about times when deliberation lead to good decisions, and intuition to bad, and also require a minimum time of 30 seconds to assign punishments, make judgements, and form beliefs. This treatment is based on previous approaches to encourage deliberative rather than intuitive decision making (Rand et al., 2012; Gino et al., 2008). If moral luck is weakened in this condition, it would suggest that violations are at least partly due to a mechanism of intuitive

Figure 6: Punishment of die minus punishment of live: Average difference by treatment

Notes: Average difference in punishment of die minus punishment of live. Each spectator chooses for all four cases so there are four observations per spectator (within-subject comparison). Figure shows standard error bars clustering on spectator.

judgements, that are swayed by salient random outcomes when decisions are made quickly and spontaneously.

Figure 6 shows that encouraging deliberation does have a directional effect of reducing moral luck, leading to less variation in punishment with random outcomes, but the difference relative to Treatment Main is not statistically significant ($p < 0.16$; Table A4). Indeed, moral luck in punishment is still significant within Treatment Deliberation ($p < 0.001$; Table A4), and we also see signs of the bias mechanisms identified in Treatment Main, albeit somewhat weaker. Panel (b) of Figure 5 shows that beliefs about the active player's value of a mouse are significantly influenced by the mouse dying in Treatment Deliberation, overall (t-test; $p < 0.02$) and for both the moral and immoral choices individually (t-tests; $p < 0.01$, $p < 0.04$), and the effect is weaker for the immoral choice like in Treatment Main, although this difference is not statistically significant in Treatment Deliberation (OLS; $p < 0.22$). Overall, the impact of the mouse dying on beliefs is directionally weaker in Treatment Deliberation compared to Treatment Main, but the difference is not statistically significant (OLS; robust s.e. clustering on spectator; $p < 0.12$). We also see that outcomes significantly influence judgements in Treatment Deliberation. As for beliefs, the effects tend to be weaker than in Treatment Main, although the difference is significantly in the case of some judgements (see Table A5). Thus, it appears that deliberation may be directionally reducing moral luck by reducing, but not eliminating, the impact of outcomes on various judgements and beliefs. The fact that moral luck in punishment and the bias mechanisms are still present and significant in Treatment Deliberation suggest that the bias is relatively

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deeply-rooted and not easily corrected.

Taken together, our results show the existence of moral luck in punishment, and are consistent with the phenomenon being at least partly a bias, with random outcomes biasing judgements and beliefs about the active player, which in turn cause punishments vary with outcomes.

Additional analysis. In additional analysis we explore some alternative explanations for why punishment might vary with outcomes, besides the bias that we identify, but find little support for these. One possible explanation is that individuals disagree with the control principle and have some other moral preference in mind that involves conditioning punishment on outcomes. We asked spectators directly whether they agree that people should only be punished for outcomes under their control, however, and find widespread agreement with this statement of the control principle, with the median individual expressing strong agreement, 9, on a scale from 0 (completely disagree) to 10 (completely agree). We also do not see a significant difference in the impact of the mouse dying on punishment, if we interact this with extent of agreement with the control principle (see Table A6; $p < 0.69$). Moral luck is also significant and strong even among those expressing complete agreement with the control principle. These findings provide little support for an alternative moral principle as a driving force behind moral luck, and accord with one of the reasons moral luck as been seen as an ethical problem, that people will tend to agree with the control principle when asked, but violate this in practice (Smith, 1790; Nagel, 1979). Another explanation for why punishment varies with outcomes could be an imperfect understanding of the role of chance in our study, due to limited cognitive ability, or inattention to information provided, especially if this inattention is skewed towards noticing outcome information more than choice information. Working against such explanations, however, is the fact that spectators were required to correctly answer comprehension questions before making their choices. We also do not see a significant difference in the impact of the mouse dying on punishment, if we interact this with extent of cognitive ability, as measured by the cognitive reflection test, or Raven's progressive matrixes, or educational attainment (see Table A6; $p < 0.71, p < 0.89, p < 0.49$). We also find that spectators were attentive to the information provided. In an incentivized question at the very end of the study we asked spectators to recall the choice and outcome for the final active player they saw, and accuracy rates are quite high, about 85 percent, and essentially identical for choices and outcomes. Thus, relatively greater inattention to information about choices than outcomes does not appear to explain moral luck.

The bias we identify raises questions about what might be the deeper mechanisms that drive the bias; in exploratory analysis, we investigate three possible mechanisms –

belief in a just world; hindsight bias or limited salience of counterfactuals; emotional impact of outcomes – and find some support for the final mechanism. The first two mechanisms would involve spectators viewing the bad outcome as more likely, if it occurs, and thereby potentially viewing the actor's choice as more immoral in that case. Belief in a just world is a type of motivated bias, such that people want to believe that bad things happen to bad people (Rubin and Peplau, 1975). Hindsight bias is a tendency for ex-post beliefs about the likelihood of an outcome to be greater than ex-ante, and has been hypothesized to reflect the fact that outcomes that occur are more salient than counterfactual outcomes (Roese and Vohn, 2012). A factor that works against these biases in our design, however, is the use of explicit probabilities. We also elicited spectator beliefs about the role of chance versus effort in determining inequality and poverty in the United States, as a proxy for belief in a just world, but find that the impact of the mouse dying on punishment is not significantly different depending on the extent of between belief in a just world (see Table A6; $p < 0.89$). The fact that we find strong moral luck in a within-subject design, where spectators make choices for all possible choices and outcomes, and counterfactuals are therefore salient, provides another indication that hindsight bias is not likely to be a key driver of the results. Lastly, we consider whether moral luck might be stronger for individuals who have stronger emotional reactions, suggesting a mechanism based on emotion. We first check whether outcomes affected emotions using a survey measure of self-reported emotions "about the fourth active player," on a scale from strongly negative to strongly positive, and find that the mouse dying significantly decreases positive emotions in the case of the moral choice (t-test; $p < 0.001$), and exacerbates negative emotions in the case of the immoral choice (t-test; $p < 0.001$). We hypothesize that spectators who experience stronger emotions in response to outcomes are those who care more about the outcome. As a candidate proxy for a trait of caring more about the outcomes, we take the spectator's own value of a mouse, and find that the impact of the mouse dying on negative emotions about the active player is, indeed, significantly stronger for spectators with a higher value of a mouse (OLS; $p < 0.002$). Turning to mechanisms for moral luck, there is a significantly stronger bias in beliefs about the active player's value of a mouse, for spectators who have a higher value themselves (OLS; $p < 0.001$), and a significantly stronger effect of the mouse dying on punishment for spectators who value a mouse more (Table A6; $p < 0.001$). One implication of these findings is that moral choices involving bad outcomes that are more emotionally upsetting may be more likely to generate moral luck. Another is that heterogeneity in moral luck may be partly explained by heterogeneity in how much punishers care about a given type of outcome.

Our within-subject design and use of a non-student sample allows us to investigate

the prevalence of moral luck as a bias, as well as have meaningful variation in demographics and other correlates to explore whether the bias varies systematically across different segments of society. We find that exhibiting moral luck, defined as punishing more on average when the mouse dies than when the mouse lives, is the modal choice pattern in Treatment Main. Specifically, if we eliminate the 9 percent of spectators who do not exhibit moral luck because they never punish at all, we find roughly 64 percent exhibit moral luck, while 36 percent exhibit zero moral luck. Thus, moral luck is prevalent but not universal. We do not find significant differences in propensity to exhibit moral luck, or magnitude of moral luck, by gender, age, income, education, or political affiliation. Thus, the bias is found for individuals from across society. As noted above, one trait that does predict strength of moral luck is the spectator's own value of a mouse, pointing to caring about the outcome as a key moderator for moral luck in punishment.

Because we elicited judgments of active players about themselves, we can also explore an intriguing, additional question, which is whether moral luck is to some extent internalized by actors. Adam Smith and others have hypothesized that moral luck is internalized in this way, and one can also find examples from literature with this theme. For example, in ancient Greek tragedy, Oedipus kills his father in a roadside conflict, and marries his mother, without knowing their identities; when he later discovers what he has done, he blinds himself, and goes into exile, even though he would presumably not have done had his vanquished opponent, and his wife, been unrelated to him. If random outcomes influence actors in how they judge themselves, and even potentially punish themselves (psychologically through feelings of guilt, or possibly through costly actions like "penance"), this would be a particularly striking form of moral luck, given that actors presumably have greater certainty about their own characters than external spectators.

We do find evidence of internalized moral luck for active players, although it differs in an interesting way from that of spectators. Specifically, active players judge their own immoral choice as significantly less immoral if the mouse lives than if the mouse dies (Table A7; $p < 0.03$). There is also suggestive evidence that actors who make the immoral choice change their view about being a good person based on the outcome, relative to a baseline assessment before their choice; the reduction in self-esteem if the mouse dies is marginally significant for individuals who have above median baseline self-image and therefore do not have a floor effect working against a reduction (Table A7; $p < 0.08$). Interestingly, however, we find an asymmetry, in that for active players there is little internalized moral luck for the moral choice. Active players view the moral choice as highly moral, regardless of the outcome, and also do not adjust their views of themselves as a good person (Table A7; $p < 0.9, p < 0.16$). These findings suggest that

actors have a conviction that the moral action clearly indicates a good character, which cannot be shaken by having the mouse die, whereas they have more malleable views about the immoral action. This could potentially be motivated, if actors want to believe they are a good person; it may be possible to convince themselves of this in all cases, except for the immoral choice with the mouse dying. At the same time, we see that actors' feelings of embarrassment vary significantly with the outcome, for the moral as well as the immoral choice (Table A7; $p = 0.01$, $p < 0.01$). This suggests that actors anticipate that others may evaluate them based on outcomes for the moral choice, even if they themselves do not do so. This asymmetry in external versus internal moral luck that we find is in line with the type of tension hypothesized to arise in meritocracies, by Sandel (2019) and others, such that individuals who have had bad luck feel unfairly judged by others. Also, good or bad luck may have lasting influences on how individuals view themselves.

Discussion.

Our findings have important implications for theories of human punishment behavior. Models of reciprocity theorize that individuals will engage in costly punishment of actions that cause harm. This can reflect a strategy of deterrence in repeated interactions, or it can arise as a heuristic or a preference for punishing those who would create harm by their actions, and manifest even in one-shot interactions (e.g., Trivers, 1971; Axelrod and Hamilton, 1981; Rabin, 1993; Levine, 1998; Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006). A complicating factor in reality, however, is that both actions and chance often play a role in determining whether harm is caused. Our findings show that punishment behavior is influenced partly by actions, consistent with reciprocity theories, but also partly by random outcomes, something that cannot be explained by traditional models of reciprocity. Furthermore, we show that a key reason that random outcomes influence punishments is by biasing judgements and beliefs about the intentions of the actor. This implies lasting reputational effects of random outcomes, which could in turn lead to longer-run consequences for punishments that are also not explained by reciprocity models, e.g., in the form of future avoidance or ostracism of actors who are viewed as bad types due to previous bad luck with outcomes. Another novel prediction from our findings is that punishments may be particularly sensitive to outcomes when there is more limited information about intentions or characters of actors. Strangers, therefore, might be more subject to moral luck in how they are evaluated, compared to individuals' whose characters and reputations are well-known to evaluators (good or bad). Our findings call for modifying traditional models of reciprocity to allow for a bias in which evaluators wrongly infer about intentions and character from random outcomes.

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The existence of moral luck in punishment also offers a new angle from which to theorize about how and why human punishment behaviors may have evolved. Evolutionary theories posit that human punishment behaviors may have played a crucial role in allowing the human species to sustain large-scale cooperation, by deterring actions that lead to harmful outcomes (e.g., Trivers, 1971; Axelrod and Hamilton, 1981; Henrich and Boyd, 2001; Fehr and Gaechter, 2002). Such theories, however, abstract away from the role of both actions and chance in determining harmful outcomes. One explanation for our finding that humans have a deep rooted tendency to condition punishment on outcomes, could be that it evolved to solve a problem of deterrence that arises in such cases, if actions are hard to observe (Gurdal et al., 2013). When actions influence the probability of harm, but are unobserved, outcomes are signals of actions, and optimal incentives involve conditioning punishment on the occurrence of harm. Our findings suggest that punishments are likely to be overly sensitive to outcomes, since they respond to outcomes even when actions are perfectly observable. But as a fast and frugal heuristic (Gigerenzer, 2004), moral luck could have been adaptive, if conditions with hard to observe actions were sufficiently frequent. One factor that may have also minimized the scope for moral luck to cause distortions in early societies, in cases when actions were observed, is the high frequency of repeated interactions. The result that having more information about actors reduces moral luck suggest that in early societies, with dense social networks and well-established reputations, distortions due to moral luck could potentially have been small, whereas in modern societies, where social networks are less dense, and there is less information about others' characters, moral luck has more scope to distort behavior.

The results in this paper complement previous empirical research on moral luck, and related concept of "outcome bias." The most common methodology has been hypothetical vignettes that vary whether an action is described as leading to more or less severe harm, and elicit moral judgements about the actor and views on appropriate punishment. Previous results are mixed, potentially due to issues of subjectivity in how subjects interpret scenario descriptions, especially interpretations of what different outcomes may signal about probabilities of harm, given that probabilities are typically implicit (for a survey and metaanlysis see, e.g., Robbenholt, 2000). Hypothetical measures also potentially encourage intuitive decision making and inattention, and likely attenuate emotional reactions, which may explain why asking subjects to decide rationally and deliberately has been found to significantly reduce moral luck in hypothetical vignettes (Gino et al., 2008), whereas in our setting with real outcomes and incentives we find persistent moral luck even with a relatively heavy-handed intervention. Previous research has found that incentivized beliefs about an actor can be influenced

by random outcomes (Brownback and Kuhn, 2019), like we do, but we complement this finding by providing the first causal evidence that belief distortions due to random outcomes can cause moral luck in punishment behavior.

The findings of our study also add to the debate on whether moral luck is explained by a preference or moral principle, or is instead a bias. Theories of preferences over outcomes posit that individuals can care about outcomes per se, e.g., disliking inequality (Fehr and Schmidt, 1999). If the harm that results from an action leads to more inequality between the actor and another individual, punishment could be motivated by a desire to reduce inequality between these individuals. In our setting, however, it is unclear that inequality aversion applies, since when harm is caused the mouse is dead. Furthermore, we show that moral luck in punishment is driven by an impact of outcomes on perceptions of the actor, so the mechanism appears to be a form of reciprocity with biased beliefs, rather than inequality-averse preferences. It is also hard to explain our findings with adherence to a moral principle, since moral luck in punishment is driven by judgements and beliefs responding to outcomes that contain zero information. Instead, moral luck appears to be a bias. This does not mean, of course, that philosophical inquiry cannot make progress on seeking a moral principle that can justify conditioning punishment on random outcomes. Our findings do not answer the question of how people should make punishment decisions from a normative point of view, rather they shed light on the positive question of how people are making such decisions.

The fact that moral luck appears to reflect a bias, and has distortionary effects on deterrence, also suggests a potential value of interventions to reduce the bias. Our results suggest that effective interventions may include providing additional information about an actor, or encouraging deliberation, although this will probably not eliminate, the bias. One challenge for such interventions, however, is that evaluators may themselves be evaluated by the wider public, if the incidents they evaluate are in the public view. To change behavior might therefore require an intervention to influence the public, not just the evaluator, which may be challenging. A recent example of such a situation could be the very public disqualification of the tennis player, Novak Djokovich from the 2020 U.S. Open. Djokovich hit a tennis ball in frustration towards the back of the court, and hurt a linesperson by hitting her in the throat. Video evidence shows that he was not looking where he was hitting, and if the path of the ball had been slightly different, no harm would have occurred. The rules of the tennis association call for disqualification for sufficiently severe recklessness, but leave it to officials to judge severity. In subsequent interviews, tennis officials agreed that Djokovich was not trying to hurt someone, and that if harm had not been caused, their decision would likely have

been different. Since harm was caused, he was disqualified, and lost the $250,000 that he had earned for reaching the fourth round of the tournament. Tennis officials might have personally felt that the occurrence of harm was relevant for the decision, or they might have had doubts, but decided that the public would not be satisfied by anything less than disqualification.

In some cases, moral luck is seemingly codified in laws or rules within organizations, requiring evaluators to exhibit moral luck, raising the question whether there is a need for policy reform. An example is differences in sentencing guidelines, or rankings of severity of the crime, for attempted murder versus "successful" murder. Because this rule applies regardless of how hard the individual tried to commit murder, it seems that the key difference is whether, due to circumstances beyond the criminal's control, the murder attempt failed, and thus it exhibits moral luck. To the extent that legal judgements need to concur with notions of justice held by the general populace, and murder is an outcome with a particularly profound emotional impact, reforming legal codes to have the same punishment for attempted and successful murder may be difficult. Another argument against reform could be that it is too costly to determine the role of chance, and simpler to just adjust punishment based on whether outcomes occur, as these can be signals of good or bad intenti. This seems contrary, however, to the notion of deliberation in legal judgements. Furthermore, in other areas of law, which involve civil rather than criminal offenses, the law is clear that severity of outcomes is not relevant for setting punishment. These seemingly contradictory ways of handling the role of chance in outcomes may reflect differences in severity of outcomes, and thus emotional reactions, and a tension between what seems rationally correct, and what feels correct.

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Online Appendix

A Additional results

Figure A1: Punishment levels in Treatment Main using only first choices for between-subject comparison
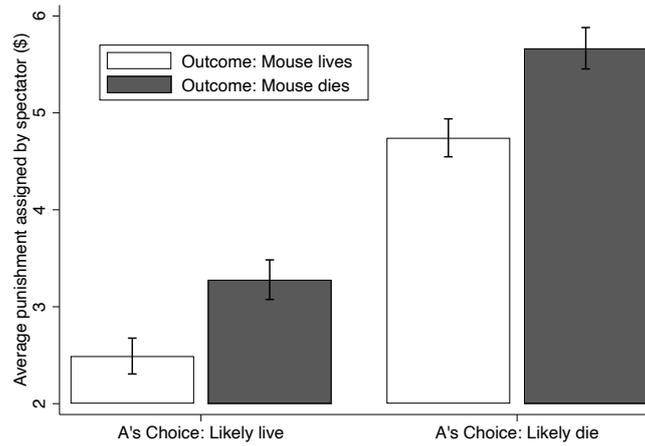
Notes: Average punishment levels for each of the four cases, with separate groups of subjects in each category. Figure shows standard error bars. Punishment is significantly higher when the mouse dies than when the mouse lives, for both the moral and immoral choice (OLS; $p < 0.004$, $p < 0.001$).

Table A1: Moral luck in judgments in Treatment Main

	Moral	Good	Embarrassing	Bother if friend	Bad intent.
	(1)	(2)	(3)	(4)	(5)
Immoral choice	-4.70***	-3.14***	3.34***	3.11***	3.95***
	(0.20)	(0.19)	(0.21)	(0.24)	(0.22)
Die	-1.58***	-1.13***	0.91***	1.40***	1.70***
	(0.18)	(0.17)	(0.20)	(0.24)	(0.22)
Immoral*Die	0.81***	0.31	-0.62**	-0.90***	-0.90***
	(0.29)	(0.27)	(0.31)	(0.34)	(0.33)
Constant	9.67***	9.04***	2.44***	2.79***	2.28***
	(0.10)	(0.11)	(0.12)	(0.16)	(0.13)
Observations	1441	1446	1443	1446	1446
Adjusted R^2	0.416	0.281	0.227	0.167	0.275

Notes: OLS regressions. Dependent variables in Columns (1) to (5) are measured on scales from 0 to 10, indicating levels of agreement with: (1) Choice was moral; (2) active player is a good person; (3) active player should be embarrassed; (4) would bother if active player were a friend; (5) active player had bad intentions. Independent variables are indicators for the immoral choice, outcome of mouse dying, and the interaction of these. Self-reported judgements refer to the fourth active player choice and outcome combination seen by the spectator, so there is one observation per spectator (between-subjects). Robust standard errors in parentheses.

Table A2: Moral luck in judgments in Treatment Main with controls

	Moral (1)	Good (2)	Embarrassing (3)	Bother if friend (4)	Bad intent. (5)
Immoral choice	-4.70***	-3.13***	3.33***	3.12***	3.92***
	(0.20)	(0.19)	(0.21)	(0.24)	(0.22)
Die	-1.62***	-1.16***	0.96***	1.44***	1.73***
	(0.19)	(0.18)	(0.20)	(0.24)	(0.22)
Immoral*Die	0.82***	0.32	-0.65**	-0.94***	-0.89***
	(0.29)	(0.27)	(0.31)	(0.34)	(0.33)
Constant	9.86***	9.31***	2.87***	2.92***	1.75***
	(0.46)	(0.44)	(0.50)	(0.58)	(0.54)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	1441	1446	1443	1446	1446
Adjusted R^2	0.422	0.290	0.244	0.176	0.284

Notes: OLS regressions. Dependent variables in Columns (1) to (5) are measured on scales from 0 to 10, indicating levels of agreement with: (1) Choice was moral; (2) active player is a good person; (3) active player should be embarrassed; (4) would bother if active player were a friend; (5) active player had bad intentions. Independent variables are indicator for the immoral choice, outcome of mouse dying, and the interaction of these. Self-reported judgements refer to the fourth active player choice and outcome combination seen by the spectator, so there is one observation per spectator (between-subjects). Controls consist of: The spectator's own value of a mouse; gender; age; income range; educational attainment. Robust standard errors in parentheses.

Table A3: Punishment choices in Treatment Main as a function of judgements and beliefs with controls

	Punishment ($)					
	(1)	(2)	(3)	(4)	(5)	(6)
Moral choice	-1.42***					
	(0.10)					
Good person		-1.28***				
		(0.10)				
Bad intentions			1.40***			
			(0.10)			
Bother if a friend				1.31***		
				(0.11)		
Embarassing					1.34***	
					(0.10)	
Belief about active player						-0.82***
						(0.11)
Constant	3.73***	3.86***	3.43***	3.61***	3.86***	3.38***
	(0.66)	(0.67)	(0.67)	(0.68)	(0.67)	(0.70)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1437	1442	1439	1442	1442	1442
Adjusted R^2	0.135	0.107	0.123	0.106	0.115	0.044

Notes: OLS regressions. The dependent variable is punishment in dollars of the fourth active player seen by the spectator. Independent variables include self-reported judgements about the active player given information about the fourth active player's choice and outcome for the mouse: Morality of active player's choice; active player is a good person; it would bother the spectator if active player was a friend; active player had bad intentions. Another independent variable is the spectator's incentivized guess about the active player's value of the life of a mouse, in dollars. All independent variables are standardized, so coefficients give the impact of a one standard deviation increase in the independent variable. Controls consist of: Dummy variables for choice of the observed (fourth) active player and outcome for the mouse, with moral choice and lives as the omitted category; the spectator's own value of a mouse; gender; age; income range; educational attainment. Self-reported judgements and beliefs refer to the fourth active player choice and outcome combination seen by the spectator, so there is one observation per spectator (between-subjects). Robust standard errors in parentheses.

Table A4: Treatment comparisons of moral luck in punishment

	Punishment ($)	
	(1)	(2)
Die	0.71***	0.71***
	(0.07)	(0.07)
T. Revealed Value	0.30**	0.29**
	(0.13)	(0.13)
T. Deliberation	-0.23*	-0.23*
	(0.14)	(0.13)
Die*T. Revealed Value	-0.22**	-0.22**
	(0.11)	(0.11)
Die*T. Deliberation	-0.18	-0.18
	(0.13)	(0.13)
Constant	3.56***	3.65***
	(0.08)	(0.34)
Controls	No	Yes
Observations	12120	12116
Adjusted R^2	0.007	0.010

Notes: OLS regressions. Dependent variable is punishment in dollars. Die is an indicator for the outcome of the mouse dying, measuring the differential punishment when the mouse dies versus when the mouse lives, averaged across the cases of moral and immoral choice. T. Revealed Value and T. Deliberation are treatment dummies, respectively. Controls include: The spectator's own value of a mouse; gender; age; income range; educational attainment. Each spectator chooses punishment for all four cases of active player choice and outcome so there are four observations per spectator (within-subject comparison). Robust standard errors in parentheses, clustering on spectator.

Table A5: Treatment comparisons of moral luck in judgements

	Moral	Good	Embarrassing	Bother if friend	Bad intent.
	(1)	(2)	(3)	(4)	(5)
Die	-1.38***	-1.12***	1.43***	1.09***	0.75***
	(0.18)	(0.16)	(0.19)	(0.18)	(0.17)
T. Revealed Value	-0.33	-0.28	0.16	0.28	0.21
	(0.22)	(0.18)	(0.22)	(0.22)	(0.20)
T. Deliberation	-0.21	-0.17	0.23	0.12	0.18
	(0.22)	(0.19)	(0.22)	(0.22)	(0.21)
Die*T. Revealed Value	0.49	0.44*	-0.23	0.04	-0.23
	(0.31)	(0.26)	(0.31)	(0.31)	(0.29)
Die*T. Deliberation	0.55*	0.52**	-0.59*	-0.18	-0.44
	(0.31)	(0.26)	(0.32)	(0.32)	(0.29)
Constant	7.47***	7.58***	4.13***	4.25***	4.00***
	(0.13)	(0.11)	(0.13)	(0.13)	(0.12)
Observations	3008	3030	3030	3030	3027
Adjusted R^2	0.025	0.021	0.028	0.021	0.007

Notes: OLS regressions. Dependent variables in Columns (1) to (5) are measured on scales from 0 to 10, indicating levels of agreement with: (1) Choice was moral; (2) active player is a good person; (3) active player should be embarrassed; (4) would bother if active player were a friend; (5) active player had bad intentions. Independent variable Die is an indicator for the outcome of mouse dying, T. Revealed Value and T. Deliberate are treatment dummies, respectively. Self-reported judgements refer to the fourth active player choice and outcome combination seen by the spectator, so there is one observation per spectator (between-subjects). Robust standard errors in parentheses.

Table A6: Moral luck in punishment choices in Treatment Main as a function of potential mechanisms

	Punishment ($)					
	punish	punish	punish	punish	punish	punish
Die	0.71***	0.72***	0.71***	0.72***	0.72***	0.51***
	(0.07)	(0.07)	(0.08)	(0.07)	(0.08)	(0.08)
Agreement control principle	-0.11					
	(0.08)					
Die*Agreement control principle	0.04					
	(0.08)					
CRT score		-0.29***				
		(0.08)				
Die*CRT score		0.02				
		(0.07)				
Raven's IQ score			-0.33***			
			(0.09)			
Die*Raven's IQ score			-0.01			
			(0.07)			
Educational attainment				-0.01		
				(0.08)		
Die*Educational attainment				0.05		
				(0.07)		
Belief in a just world					-0.11**	
					(0.05)	
Die*Belief in a just world					-0.01	
					(0.05)	
Spectator's value of a mouse						-0.01
						(0.01)
Die*Spectator's value of a mouse						0.38***
						(0.09)
Constant	3.56***	3.54***	3.57***	3.56***	3.59***	3.71***
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.21)
Observations	5784	5784	5784	5784	5772	5784
Adjusted R^2	0.008	0.011	0.014	0.007	0.009	0.009

Notes: OLS regressions. The dependent variable is punishment in dollars of the fourth active player seen by the spectator. Independent variables in include: Self-reported agreement with the control principle; CRT test score; Raven's IQ test score; spectator's own value of the life of a mouse. All independent variables are standardized, so coefficients give the impact of a one standard deviation increase in the independent variable. Each spectator makes a choice for all four cases so there are four observations per spectator (within-subjects). Robust standard errors in parentheses, clustering on spectator.

Table A7: Moral luck for active players

	Immoral Choice			Moral Choice		
	Moral	Good	Embarrassed	Moral	Good	Embarrassed
	(1)	(2)	(3)	(4)	(5)	(6)
Die	-0.72**	-0.32*	0.94***	-0.05	0.15	1.78***
	(0.32)	(0.18)	(0.36)	(0.34)	(0.10)	(0.46)
Constant	4.23***	-0.43***	2.73***	8.38***	-0.12*	0.80***
	(0.28)	(0.14)	(0.29)	(0.21)	(0.06)	(0.21)
Observations	257.00	133.00	257.00	170.00	103.00	170.00
Adjusted R^2	0.02	0.01	0.02	-0.01	0.01	0.09

Notes: OLS regressions. Dependent variables in Columns (1) to (5) are measured on scales from 0 to 10, indicating levels of agreement with: (1) and (4) Choice was moral; (2) and (5) I am a good person (difference after-before choice); (3) and (6) I would be embarrassed if a friend learned my choice. Columns (2) and (5) consider only those subjects with above median self-esteem to avoid floor effect. Die indicates whether the mouse died. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$