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Cheating at the End to Avoid Regret

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Abstract

How do people behave when they face a finite series of opportunities to cheat with little or no risk of detection? In 4 experiments and a small meta-analysis, we analyzed over 25,000 cheating opportunities faced by over 2,500 people. The results suggested that the odds of cheating are almost three times higher at the end of a series than earlier. Participants could cheat in one of two ways: They could lie about the outcome of a private coin flip to get a payoff that they would otherwise not receive (Studies 1-3) or they could overbill for their work (Study 4). We manipulated the number of cheating opportunities they expected but held the actual number of opportunities constant. The data showed that the likelihood of cheating and the extent of dishonesty were both greater when people believed that they were facing a last choice. Mediation analyses suggested that anticipatory regret about passing up a chance to enrich oneself drove this cheat-at-the-end effect. We found no support for alternative explanations based on the possibility that multiple cheating opportunities depleted people's self-control, eroded their moral standards, or made them feel that they had earned the right to cheat. The data also suggested that the cheat-at-the-end effect may be limited to relatively short series of cheating opportunities (i.e., $n < 20$). Our discussion addresses the psychological and behavioral dynamics of repeated ethical choices. (228 words)

Keywords: Ethical behavior, morality, cheating, anticipatory regret, time

Cheating at the End to Avoid Regret

Life is full of opportunities to commit ethical violations for personal gain; often, these opportunities come in a series with a known end. Classes with regular exams give students only a few chances to cheat before the end of the term; consultants on short-term contracts have limited opportunities to overbill their clients; and lame-duck politicians face imminent deadlines that limit their chances of exploiting their positions to enrich themselves. In each of these examples, people face a repeated sequence of similar ethical quandaries in which they must choose whether to increase their own outcomes by cheating or to preserve a desirable self-concept by upholding their moral obligations. Although a great deal of research has examined the social-psychological factors that influence how people balance these competing concerns (e.g., Ariely, 2012; Bryan, Adams, & Monin, 2013; Shu & Effron, in press; Tenbrunsel & Messick, 2004; Wang & Murnighan, 2014), little is known about the time course of their decisions. Do people cheat at random intervals over a series of choices or do they cheat systematically at certain time points? This question is not only of theoretical interest but also of practical importance to policymakers and organizations. In the present research, we predict and test the idea that when people face a series of ethical quandaries, cheating will be most likely at the end, with anticipatory regret acting as a driving force.

Cheating, Scarcity, and Anticipatory Regret

We define ethical quandaries as situations that pit self-interested temptation against the obligation to uphold moral principles – in other words, that create a conflict between how people *want* to behave in the moment and how they feel they *should* behave (Bazerman, Tenbrunsel, & Wade-Benzoni, 1998; Tenbrunsel, Diekmann, Wade-Benzoni, & Bazerman, 2010). How people resolve these want/should conflicts will depend in part on how strong the motivating force of

temptation is relative to the inhibitory force of moral obligation (cf. Lewin, 1947). We propose that temptation is stronger at the end of a series of ethical quandaries, when no opportunities for enrichment remain, than earlier in the series, when such opportunities are still plentiful. At the beginning of the series, people can satisfy self-interest later, leading them to act more honestly as they balance temptation with moral obligation. We suggest that at the end of the series, however, the balance shifts in favor of temptation because scarcity makes the remaining opportunity more attractive (Brock & Brannon, 1992; Cialdini, 1988; Lynn, 1992; Worchel, Lee, & Adewole, 1975) without increasing the moral obligation to resist it. Said differently, when faced with an opportunity to cheat, people may ask themselves, in essence, “How would I feel if I cheated versus passed up a tangible benefit?” Cheating could cause guilt, but foregoing a benefit could spark regret. We predict that people will anticipate feeling more regret about foregoing such benefits when future opportunities to capture them are scarce, without a corresponding increase in anticipatory guilt. This process resembles how limited buying opportunities lead consumers to anticipate feeling more regret about declining to purchase an item (Abendroth & Diehl, 2006), presumably without leading them to anticipate more guilt about spending money. In the context of repeated ethical quandaries, we expect this shift in the relative strength of these anticipatory emotions to result in more cheating at the end.

Research has repeatedly shown how the need to preserve a moral self-image inhibits people from acting as unethically as they are tempted to (for reviews, see e.g., Monin & Jordan, 2009; Shu & Effron, in press; Zhong, Liljenquist, & Cain, 2009). For example, most people tend to cheat a little to satisfy their material desires, but not so much that they come to see themselves as dishonest (Bolton, Katok, & Zwick, 1998; Mazar, Amir, & Ariely, 2008; Shalvi, Handgraaf, & De Dreu, 2011); they also cheat more when doing so seems less diagnostic of their moral

character (Bryan, et al., 2013; Von Hippel, Lakin, & Shakarchi, 2005; Wiltermuth, 2011). Our theorizing and this work are mutually compatible: Both approaches acknowledge people's attempts to balance self-interested desires against the need to protect a moral self-concept by upholding moral obligations. However, our theorizing goes beyond this prior work by examining the temporal dynamic of this balancing process, as well as how anticipatory regret affects it.

Anticipatory regret influences decisions in a variety of domains (Bell, 1982; Loomes & Sugden, 1982; Miller & Taylor, 1995; Zeelenberg & Pieters, 2007), including consumer behavior (Simonson, 1992; Zeelenberg, 1999a), trust (Effron & Miller, 2010), risk-taking (Nordgren, van der Pligt, & van Harreveld, 2007; Richard, de Vries, & van der Pligt, 1998), negotiations (Larrick & Boles, 1995), and financial decision-making (Ritov, 1996). Research has examined how anticipatory guilt, a different emotion, can *inhibit* bad behavior (Cohen, Panter, & Turan, 2012; Steenhaut & Van Kenhove, 2006). In contrast, the present research examines how anticipatory regret about losing an opportunity for personal gain can *promote* unethical behavior. Lost opportunities are a particularly potent source of regret (Beike, Markman, & Karadogan, 2009; Morrison & Roese, 2011). We predict that to avoid feeling regret about passing up a scarce opportunity for personal enrichment, people will be more willing to act on that opportunity – even if it means cheating.

If scarcity sparks anticipatory regret about passing up an enrichment opportunity, then anticipatory regret should be particularly strong when scarcity is at its maximum. In the case of repeated opportunities for ethical violations, this will be at the final opportunity. Thus, we predicted that, in a series of cheating opportunities with a known end, people will be more likely to cheat on the last opportunity than they will be to cheat on earlier opportunities – a *cheat-at-the-end* effect. Cheating could also increase over the entire course of the series as the scarcity of

cheating opportunities increases, but the exact form of this increasing pattern is difficult to predict. One possibility is that each passing opportunity to cheat increases anticipatory regret about foregoing subsequent opportunities by the same amount, and thus cheating will increase linearly throughout the series. A second possibility is that the increasing scarcity of cheating opportunities does not become salient until none or almost none remain. The final cheating opportunity may also hold a particular psychological power (cf. McKenzie et al., 2014); when it is a person's "last chance" to enrich herself, anticipatory regret about not cheating may loom much larger than it did at any earlier point in the series. In this view, anticipatory regret and cheating behavior would both remain low until near or at the end of the series. Consistent with this possibility, the number of consumers redeeming a coupon spikes just before the coupon expires, apparently because anticipatory regret about foregoing a savings opportunity soars when the time remaining to redeem the coupon becomes scarcest (Inman & McAlister, 1994). Thus, our theorizing does not suggest a strong prediction about whether cheating will spike suddenly at the end of the series or increase gradually across the series. Both possibilities, however, point to heightened cheating at the very end – the point on which our hypotheses focus.¹

Alternative Explanations

Our investigation of the time course of cheating represents an important contribution to the literature on ethical decision-making, which has typically investigated "one-off" decisions (e.g., Brady & Wheeler, 1996; Flannery & May, 2000; Gunia, Wang, Huang, Wang, & Murnighan, 2012; Hegarty & Sims, 1978; Shu, Mazar, Gino, Ariely, & Bazerman, 2012; Treviño & Youngblood, 1990; White & Dooley, 1993). The few studies that have provided repeated opportunities to cheat tend not to examine the time course of cheating (e.g., Gino & Ariely,

2012; Mazar, et al., 2008). Research does suggest, however, that a variety of reasons other than anticipatory regret might also explain why people cheat more at the end.

Moral self-licensing. One line of research that has examined the time course of unethical behavior is the work on *moral self-licensing*, which indicates that making an ethical choice at Time 1 can increase people's willingness to make an unethical choice at Time 2 (e.g., Effron, Cameron, & Monin, 2009; Hofmann, Wisneski, Brandt, & Skitka, 2014; Jordan, Mullen, & Murnighan, 2011; Mazar & Zhong, 2010; Monin & Miller, 2001; Sachdeva, Iliev, & Medin, 2009). A history of virtuous behavior seems to make people feel that they have earned "moral credits" that can be "spent" to act less virtuously (Merritt, Effron, & Monin, 2010; Miller & Effron, 2010). Most relevant to the present research, passing up a chance to do something bad is sufficient to make people feel licensed to act less virtuously in the future (Effron, in press; Effron, Miller, & Monin, 2012; Effron, Monin, & Miller, 2013). Thus, moral self-licensing could account for a cheat-at-the-end effect if foregoing opportunities for dishonesty on earlier decisions helped people feel that they had earned the right to act dishonestly later. Extending previous theorizing about moral self-licensing, it is possible that people save their moral credits to spend at the very end of a series of decisions, allowing them to cheat with less threat to their moral self-concept.

Ego-depletion. Studies have also shown that cheating increases when people have exhausted their self-control (e.g., after being required to write an essay without using certain common letters; Mead, Baumeister, Gino, Schweitzer, & Ariely, 2009). Similarly, repeated struggles with the temptation to cheat could exhaust individuals' limited self-control resources, with the result – ego-depletion – reducing their ability to resist subsequent temptations (Baumeister, Vohs, & Tice, 2007; Mead, et al., 2009; Vohs & Heatherton, 2000).

Slippery slope. Research also suggests that people are more likely to approve of behavior that crosses an ethical line gradually rather than abruptly (Gino & Bazerman, 2009; Hartson & Sherman, 2012; Welsh, Ordóñez, Snyder, & Christian, 2015). This *slippery slope* account suggests that people will cheat more at the end because their moral standards have gradually eroded.

Distinguishing Among Explanations

Along with our proposed anticipatory regret mechanism, the three alternative explanations – moral self-licensing, ego-depletion, and slippery slope – all predict that when people face a limited number of cheating opportunities, they will be more likely to cheat at the end compared to earlier. However, these mechanisms do make different predictions about whether the number of *previous* or *remaining* opportunities will drive the cheat-at-the-end effect. The three alternative explanations suggest that people cheat more at the end because their *previous* cheating opportunities have made them feel that they have earned the right to cheat (moral self-licensing), exhausted their self-control (ego-depletion), or acclimated them to cheating (slippery slope). In contrast, an anticipatory regret explanation suggests that cheating increases when people perceive the number of *remaining* opportunities to be scarce. Thus, we can test anticipatory regret against the other mechanisms by holding the number of previous cheating opportunities constant and manipulating the number of remaining opportunities.

For example, imagine a contractor who has had six opportunities to overbill her clients. All four mechanisms that we have considered predict that she will be more likely to overbill on the seventh opportunity than on the previous opportunities. However, only an anticipatory regret mechanism would predict that she would be more likely to overbill if she thought that the seventh opportunity was her last than if she expected more opportunities to remain. When no

opportunities remain, scarcity is at its maximum, anticipatory regret should loom particularly large, and cheating should thus be particularly likely. The alternative mechanisms do not predict that the number of remaining opportunities will affect cheating; by the seventh opportunity, regardless of the number remaining, she has had six chances to accumulate moral credits, exhaust self-control, or acclimate to cheating.

Thus, the anticipatory regret mechanism predicts the following:

Hypothesis 1: People will be more likely to cheat when they think that no more cheating opportunities remain.

It is important to emphasize that the alternative mechanisms do not make this prediction when the number of previous cheating opportunities is held constant. Thus, for example, anticipatory regret is the only one of these mechanisms that predicts that people will be more likely to cheat on the seventh opportunity in a series if they expect seven opportunities total than if they expect ten.

If anticipatory regret indeed plays a role in this effect, then people should anticipate that passing up a last cheating opportunity would make them feel more regretful than passing up an earlier opportunity, and this regret in turn will increase their willingness to cheat at the end.

Thus:

Hypothesis 2: Anticipatory regret about passing up a last opportunity for enrichment will make people more willing to cheat when they think that no more cheating opportunities remain.

We also distinguished between the competing mechanisms in a second way. What would happen if, after what people thought was their final opportunity to cheat, they discovered an unexpected, additional series of cheating opportunities? For example, if employees snuck out of work early because they thought it was the last day before their boss returned from a trip, would

they also leave early the next day if the boss unexpectedly extended her trip by a week? If previous opportunities to cheat have eroded their moral standards, then cheating should remain high. Similarly, because additional opportunities present additional temptations rather than rest, ego depletion would also predict that cheating should remain high. If, however, anticipated regret has led people to cheat at the end, then their cheating rates should immediately drop because there are now ample opportunities to satisfy self-interest later, and thus little need to worry about regret. Therefore, cheating on the last of an expected set of opportunities should be more likely than cheating on the next, unexpected opportunity. Two of our studies presented these kinds of “windfall” cheating opportunities to test for these effects.

The Present Research

We tested our hypotheses in a series of experiments that presented participants with a sequence of cheating opportunities and manipulated how many opportunities they expected to have. Studies 1 and 2 used a paradigm developed by Bryan et al. (2013) in which participants flipped a coin multiple times in private; each flip determined whether they received a monetary payoff. We told them that lying about what they flipped would interfere with our research and urged them not to cheat – but it was impossible to verify whether they cheated. We could only observe cheating in the aggregate by examining whether significantly more than 50% of a trial’s coin flips resulted in winning outcomes. We manipulated the number of flips participants expected to complete, predicting that they would be more likely to cheat on the flip that they expected would be their last. After describing these two studies, we report a meta-analysis that includes data from prior research using the same paradigm.

Study 3 examined whether people anticipate that they will fall prey to the cheat-at-the-end effect, and directly tested the role of anticipatory regret. We hypothesized that, after reading

a description of the coin-flip task, people would indicate that they would be more willing to cheat on a given flip when it was last – and that this effect would be mediated by anticipated regret about foregoing an opportunity for enrichment.

Study 4 used a new paradigm to model the quandary that employees face when they have discretion in reporting their work time. We hired short-term research assistants whose pay depended on their self-reports of the time they had worked on each of a series of tasks. We manipulated the number of tasks they expected to complete, predicting that they would overbill us by a larger margin for a particular task when they thought it was last compared to when they did not.

Study 1

We designed Study 1 to provide an initial test of the cheat-at-the-end effect and to distinguish among different explanations for it: anticipatory regret, moral self-licensing, ego-depletion, and a slippery slope. All of Study 1's participants completed 13 trials of the coin-flip task (described above) but we manipulated whether they initially expected that Trial 7, 10, or 13 would be their last. We predicted that participants would cheat more on Trial 7 when they expected seven trials total than when they did not, and that they would cheat more on Trial 10 when they expected ten trials total than when they did not. (Our hypotheses do not make predictions about whether the manipulation will affect cheating on Trial 13. Trial 13 is last in all three conditions, but the fact that it is a surprise in two of the conditions could affect cheating).

In addition to comparing cheating on Trials 7 and 10 across conditions (a between-subjects approach), we also compared Trials 7 and 10 to earlier trials within each condition (a within-subjects approach). We expected people to cheat more on Trial 7 than on the average previous trial only when they expected Trial 7 to be last, to cheat more on Trial 10 than on the

average previous trial only when they expected Trial 10 to be last, and to cheat more on Trial 13 than average when they expected Trial 13 to be last. Comparing the last trial to the average previous trials provided a particularly conservative test of the cheat-at-the-end effect because if cheating increased gradually as the last trial approached (a pattern that, as noted, would be compatible with a cheat-at-the-end effect), elevated cheating rates on the trials just before the end would pull the average up and minimize its difference with the last trial.

As noted, support for these predictions would be consistent with an anticipatory regret mechanism instead of the alternative mechanisms. The alternative mechanisms predict that the more cheating opportunities people have faced, the more inclined they become to cheat; however, the manipulation does not vary the number of opportunities faced. For instance, the anticipatory regret explanation predicts that cheating on Trial 7 will be higher in the expect-7 condition than in the other conditions; the alternative mechanisms do not predict this because participants in all of the conditions have had six cheating opportunities before Trial 7.

We also tested these alternative mechanisms by examining cheating behavior on an unexpected series of trials, revealed only after participants had completed the trial they had thought was last. An anticipatory regret mechanism suggests that cheating rates should drop on the first of these “windfall” cheating opportunities, but they should remain high if previous opportunities have exhausted self-control resources (ego-depletion) or eroded moral standards (slippery slope).

Method

Participants. We recruited participants from Amazon.com’s Mechanical Turk service (MTurk; $N = 897$) and paid them \$.31 plus any money they reported earning from the coin flips. MTurk data have exhibited comparable reliability to data from more traditional sources

(Buhrmester, Kwang, & Gosling, 2011; Horton, Rand, & Zeckhauser, 2011; Paolacci, Chandler, & Ipeirotis, 2010). We tried to prevent people from participating at all if they had already completed this or a pilot study (Peer, Paolacci, Chandler, & Mueller, 2012), and we manually excluded 23 whose duplicate IP addresses or MTurk identifiers indicated multiple responding. We also excluded participants who had missing data ($n = 8$) or who failed an attention check (described below; $n = 19$). The final sample was 847 people (582 males, 264 females, 1 unknown gender; $M_{\text{age}} = 28.03$, $SD = 8.93$). (The direction and significance of the results were identical when we reran the analyses without excluding any participants).

Procedure. Participants were told that the study investigated psychokinesis, the ability to move objects with one's mind. To add credibility to this story, participants read that the researchers were investigating whether a Cornell University researcher's ostensible evidence for paranormal abilities would replicate (cf. Bem, 2011; Galak, LeBoeuf, Nelson, & Simmons, 2012). To avoid creating experimental demand to cheat, the directions indicated that the experimenters were skeptical about the existence of paranormal abilities. Then participants learned that they would flip a coin multiple times while trying to mentally influence each flip to land on "heads." To ensure proper motivation, they read, they would receive \$.10 every time they reported that the coin landed on a particular side. We counterbalanced between participants whether the winning side was heads or tails – a variant that did not influence our results. The directions acknowledged that it was impossible to verify what participants actually flipped, but urged them not to misreport the outcomes as "even a small amount of cheating would undermine the study." Before flipping the coin, they were reminded: "Please don't cheat." Participants then completed the coin flips and recorded the result of each one by clicking "heads" or "tails." After the study ended, they received the promised payment for the number of winning flips reported.

We assessed aggregate cheating by inspecting the proportion of participants who reported flipping heads on each trial. Assuming that psychokinesis was not operating, a proportion significantly greater than chance (50%) indicates that some participants cheated.

Manipulation and additional measures. Everyone completed 13 trials (flips). Within this set, we randomized how many trials we *said* they would complete: 7, 10, or 13 ($n_s = 217$, 423, and 207, respectively).² In the expect-7 and the expect-10 conditions, participants completed the expected number of trials and recorded their responses; then, they were informed of a surprise opportunity to complete three or six more trials, respectively. As an attention-check, we also asked them to identify which outcome paid off (response options: heads, tails, or it depended on the trial). At the end of the study, we included a moral self-concept measure that asked them to indicate the difference between their actual and ideal moral selves (Jordan, Gino, Tenbrunsel, & Leliveld, 2013); the manipulations did not affect these responses, which did not moderate the results.

Results

Overview. On each trial, participants received a score of 1 if they reported the winning flip, and 0 if they did not – a binary measure that we analyzed with logistic regression. Figure 1 shows that more than 50% of people reported winning on most trials, indicating that some people cheated. More importantly, consistent with a cheat-at-the-end effect, the most cheating occurred on Trial 7 in the expect-7 condition, and on Trial 10 in the expect-10 condition; there was also elevated cheating on Trial 13 in the expect-13 condition. The following sections formally test our specific predictions.

Cheating differences across conditions. We first examined whether the manipulation affected cheating on key trials.

Trial 7. As predicted, cheating on Trial 7 differed by condition (compare the three bars for Trial 7 in Figure 1). To test this effect's significance, we computed a logistic regression model with two dummy codes for condition: one compared the expect-10 condition (coded -1) to the expect-7 condition (coded 0), and the other compared the expect-13 condition (coded -1) to the expect-7 condition (coded 0). (We used -1 instead of +1 so that an odds ratio larger than 1 would indicate more cheating in the expect-7 condition). As predicted, more people reported the winning flip on Trial 7 in the expect-7 (66.36%) than in the expect-10 (56.26%), $OR = 1.53$, $z = 2.46$, $p = .014$, or the expect-13 condition (54.59%), $OR = 1.64$, $z = 2.47$, $p = .013$. The OR s (odds ratios) indicate that the odds of reporting winning on Trial 7 were more than 1.5 times larger when participants expected Trial 7 to be last than when they did not.

A potential concern with these analyses is that they do not account for the amount of cheating on Trials 1-6. Any condition differences in the amount of cheating before Trial 7 could conceivably drive condition differences in Trial-7 cheating. To address this possibility, we reran the analyses controlling for the total number winning outcomes reported on Trials 1-6. This variable was not a significant covariate, $p = .83$, and the analysis produced identical results as before. Thus, people cheated more on Trial 7 when they thought it was last than when they did not, even when their responses to previous trials were held constant.

Trial 10. Cheating on Trial 10 differed across conditions in much the same way (compare the three bars for Trial 10 in Figure 1). To analyze Trial 10 responses, we computed a logistic regression model with dummy codes comparing the expect-7 condition (coded -1) to the expect-10 condition (coded 0), and comparing the expect-13 condition (coded -1) to the expect-10 condition (coded 0). As predicted, a greater proportion of people reported flipping heads on Trial 10 in the expect-10 (64.01%) than the in expect-7 condition (49.77%), $OR = 1.80$, $z = 3.47$, $p =$

.001. People were slightly more likely to report flipping heads on Trial 10 in the expect-10 than in the expect-13 condition (60.87%), but this difference was not significant, $OR = 1.15$, $z = .78$, $p = .44$, as cheating on Trial 10 in the expect-13 condition was more frequent than expected. The total number of wins reported on Trials 1-9 was not a significant covariate when added to the model, $p = .34$, and including this covariate did not alter the results, suggesting that the results cannot be explained by condition differences in the amount of cheating before Trial 10. Thus, people cheated more on Trial 10 when they thought it was their last, although this difference was only significant in one of the two tests.

Cheating differences within conditions.

Trials 7 and 10. To test the cheat-at-the-end effect in another way, we examined how cheating on Trials 7 and 10 compared to cheating before these trials within each condition. Figure 1 suggests that, as predicted, people cheated more on Trial 7 than on the average of the previous six trials only in the expect-7 condition (in Figure 1, compare the black bars for Trial 7 to the black bars for previous trials). Similarly, people cheated more on Trial 10 than on the previous trials in the expect-10 condition but not in the expect-7 condition, as predicted. Unexpectedly, participants in the expect-13 condition also seem to have cheated more on Trial 10 than on the previous trials.

We tested the significance of these patterns in a multilevel logistic regression model with random intercepts. This model accounts for the fact that trial is nested within participant. We entered 9 contrasts for the first 10 trials (Trial 1 was the reference group); each contrast tested whether participants cheated more on a given trial than on the average previous trials (i.e., we used a reverse-Helmert coding scheme). The key contrasts for testing our predictions compared Trial 7 (coded 6/7) to Trials 1-6 (each coded -1/7; remaining trials coded 0), and compared Trial

10 (coded 9/10) to Trials 1-9 (each coded -1/10; remaining trials coded 0). The remaining contrasts controlled for variance among other trials. We ran this analysis separately in each condition.

The results in Table 1 show that, consistent with a cheat-at-the-end effect, more people cheated on Trial 7 than on the previous trials in the expect-7 condition, $OR = 1.55, z = 2.86, p = .004$. Also, as expected, this elevated cheating did not emerge on Trial 7 in the expect-10, $OR = 1.15, z = 1.31, p = .19$, or the expect-13 conditions, $OR = 1.13, z = .79, p = .43$.

Also consistent with a cheat-at-the-end effect, Table 1 shows that cheating was more likely on Trial 10 than on the earlier trials in the expect-10 condition, $OR = 1.51, z = 3.90, p < .001$, and no more likely on Trial 10 in the expect-7 condition, in which Trial 10 was in the middle of a surprise second series of flips $OR = .79, z = 1.64, p = .10$ (the odds ratio less than 1 indicates *less* cheating than average on Trial 10 in this condition). Contrary to expectations, however, cheating was more likely on Trial 10 than on previous trials in the expect-13 condition, $OR = 1.44, z = 2.39, p = .02$. This unexpected finding was due to the higher-than-predicted cheating on Trial 10 in the expect-13 condition, noted previously.

Trial 13. The cheat-at-the-end effect also predicts more cheating on Trial 13 than on Trials 1-12 in the expect-13 condition. We tested this prediction by running the same multilevel logistic regression in the expect-13 condition just described, except that we added reverse-Helmert codes for Trials 11-13 (each code compared the focal trial to the previous trials). Consistent with the prediction, participants were marginally more likely to report the winning outcome on Trial 13 than on the previous twelve trials, $OR = 1.29, z = 1.74, p = .08$ (the reverse-Helmert codes for Trials 11 and 12 were not significant, $ps > .76$).³

Cheating on the first surprise trial.

Trial 7 vs. Trial 8. We next examined cheating on Trial 8, the first surprise trial in the expect-7 condition. Consistent with Hypotheses 1 and 2, cheating rates dropped from Trial 7 to Trial 8 only in the expect-7 condition (see Figure 1). Specifically, people who expected only 7 trials were significantly less likely to report the winning flip on Trial 8 than on Trial 7 (49.77% vs. 66.36%, respectively), $OR = .71$, $z = 3.46$, $p = .001$ in a multilevel logistic regression model with random intercepts, in which Trial 7 was coded 1, Trial 8 was coded -1, and all other trials were coded 0. In fact, people seem to have been honest on Trial 8 in this condition: The proportion reporting the winning flip was not greater than the 50% expected by chance. Also, no difference in cheating rates emerged between Trials 7 and 8 in the two conditions in which Trial 8 was not a surprise, $ps > .55$.

Trial 10 vs. Trial 11. Finally, we examined cheating on Trial 11: the first surprise trial in the expect-10 condition. As expected, people in the expect-10 condition were significantly less likely to report the winning flip on Trial 11 than on Trial 10 (52.48% vs. 64.07%), $OR = .79$, $z = 3.38$, $p = .001$, in a multilevel logistic regression model analogous to the one just reported. Cheating rates did not differ significantly between Trials 11 and 10 in the two conditions in which Trial 11 was not a surprise, $ps > .16$.

No moderation by prior earnings. As an exploratory step, we examined whether condition differences in cheating on Trials 7 and 10 were related to the number of winning outcomes reported on previous trials. People who honestly reported earning a particularly low amount before the end of the series could be particularly likely to cheat at the end because honesty exhausted their self-control (Mead, et al., 2009) or granted them a moral license to cheat (Effron, et al., 2012; Effron, et al., 2013).

We found no evidence that the cheat-at-the-end effect depended on the number of previously reported wins. The number of winning outcomes reported on Trials 1-6 did not significantly moderate the differences in Trial 7 cheating among the expect-7, the expect-10, and the expect-13 conditions, $ps > .50$. Similarly, the number of wins reported on Trials 1-9 did not significantly moderate the difference in Trial 10 cheating between the expect-10, the expect-7, and the expect-13 conditions, $ps > .55$. These results are not consistent with the moral self-licensing and slippery-slope mechanisms. Instead, we suspect that people anticipated heightened regret about foregoing a last cheating opportunity, regardless of how much money they had earned on previous trials.

Linear vs. curvilinear increase in cheating before the end. As noted, our theorizing predicts that cheating will increase over the course of the expected number of trials, but it does not make a clear prediction about whether the increase will be gradual or come suddenly at the end. We performed exploratory analyses to test both possibilities (see Online Supplement). Significant linear effects in each condition showed that the percentage of participants reporting the winning flip increased monotonically until the last expected trial (see Figure 1). The quadratic effect was only significant in the expect-10 condition, indicating that the linear increase in cheating accelerated as the expected number of trials neared its end.

Discussion

Study 1 demonstrated the cheat-at-the-end effect (Hypothesis 1): In general, participants were more likely to take an opportunity to cheat when they thought it would be the last one than when they did not. We observed this pattern using between-subjects comparisons (e.g., participants cheated more on Trial 7 when they expected 7 trials total than when they expected 10 or 13) and within-subjects comparisons (e.g., participants cheated more on Trial 7 than on the

previous trials, but only when they thought Trial 7 was the last), and the results held when controlling for what participants reported flipping on trials before the end.

An anticipatory regret mechanism predicts that people will cheat more when they expect that fewer opportunities remain; a mechanism based on moral self-licensing, ego-depletion, or a slippery slope would instead predict that people cheat more when they have completed more trials. Because we observed more cheating when people thought that they were on the last flip than when they did not, holding constant the number of flips already completed, the data are more consistent with an anticipatory regret mechanism (Hypothesis 2) than with these other mechanisms. Moreover, the results of the surprise trials were consistent with anticipatory regret and not the other mechanisms.

One cell in our design did not conform to our predictions: We observed more cheating on the 10th trial than on previous trials in the expect-13 condition, even though it was 3 trials from the end. Given this minor but unexpected deviation from our predictions, it seemed important to replicate the cheat-at-the-end effect and to test whether we would again find heightened cheating on Trial 10 when people expected more than 10 trials. To preview Study 2's results, we replicated the cheat-at-the-end effect but found no further evidence for this unexpected finding.

Study 2

Participants in Study 2 completed 20 trials of the coin-flipping task; the instructions led them to expect that there would be 10 trials total, 20 trials total, or that the number of trials would be randomly determined. The cheat-at-the-end effect predicts more cheating on Trial 10 in the expect-10 condition than in the other two conditions. The condition in which the number of trials was ostensibly random provides a particularly conservative test of our hypothesis because people in this condition do not know whether Trial 10 is last.

Study 1 found evidence of a cheat-at-the-end effect in sequences of as many as 13 decisions; we wondered, however, whether the cheat-at-the-end effect might be eliminated if the number of expected trials were sufficiently large to satisfy participants' desire for personal enrichment before the end. Thus, an additional goal of Study 2 was to explore whether the cheat-at-the-end effect would occur after an even longer series of decisions than we included in Study 1 (i.e., on Trial 20 in the expect-20 condition).

Method

Participants. We recruited 923 MTurk participants, compensated as in Study 1. We took precautions to prevent people from signing up if they had participated in previous studies using the same paradigm. After excluding people who had used duplicate MTurk IDs or IP addresses ($n = 21$), who failed an attention-check item ($n = 24$), or who had missing data ($n = 21$), the final sample was 857 people (560 males, 295 females, 2 unknown gender; $M_{\text{age}} = 29.03$, $SD = 9.55$). Except where noted below, the results were identical in direction and significance when no people were excluded.

Procedure. Participants completed the coin-flipping task previously described. They were randomly assigned to read instructions indicating that they would perform 20, 10, or a randomly determined number of flips (respectively, the *expect-20*, *expect-10*, and *unknown-number* conditions, $ns = 290$, 285, and 282). Actually, everyone was asked to perform 20 flips (i.e., the expect-10 condition had 10 surprise trials). As in Study 2, participants could expect Trial 10 to be their final trial in only one condition. At the end of the study, participants completed Study 2's moral self-concept measure (Jordan, et al., 2013); as before, it neither responded to the manipulations nor moderated the results.

Results

Overview. Consistent with our predictions, Figure 2 shows that the most cheating occurred on Trial 10 in the expect-10 condition.

Cheating differences across conditions on Trial 10. As expected, participants were more likely to cheat on Trial 10 when they expected it to be their last than when they did not (compare the three bars for Trial 10 in Figure 2). We submitted Trial 10 cheating to a logistic regression model with dummy codes comparing the expect-10 condition (coded 0) to the expect-20 condition (coded -1), and the expect-10 condition (coded 0) to the unknown-number condition (coded -1). Results showed that more participants reported the winning flip on Trial 10 in the expect-10 (65.61%) than in the expect-20 condition (54.83%), $OR = 1.57, z = 2.64, p = .008$, or in the unknown-number condition (57.80%), $OR = 1.39, z = 1.91, p = .056$ (without excluding any participants, $p = .03$). It makes sense that the effect was somewhat weaker in the unknown-number condition; some participants in this condition may have suspected that Trial 10 might be their last. Unlike Study 1, the number of times people reported winning on Trials 1-9 was a significant covariate when added to the model: The more they reported winning before Trial 10, the higher the odds of reporting a winning flip on Trial 10, $OR = 1.10, z = 1.99, p < .046$. This finding is consistent with the slippery-slope explanation, which posits that cheating on earlier trials begets more cheating on later trials. Importantly, however, as in Study 1, the differences in Trial 10 cheating remained at the same significance level when this covariate was included, suggesting that the cheat-at-the-end effect was not due to any condition differences in the amount of cheating before Trial 10 and could not be fully explained by a slippery-slope mechanism.

Cheating differences within conditions on Trial 10. As another test of the cheat-at-the-end effect, we examined whether people were more likely to cheat on Trial 10 than on earlier trials within each condition (in Figure 2, compare the black bar in Trial 10 to the black bars for

the previous trials). As in Study 1, we used a multilevel logistic regression analysis with a random intercept and reverse-Helmert codes for Trials 1-10 included as fixed effects. We ran this analysis separately for each condition. As Table 2 shows, the results were consistent with Hypothesis 1: More people cheated on Trial 10 than on previous trials in the expect-10 condition, $OR = 1.62, z = 3.67, p < .001$. Also as expected, no such effect emerged in the expect-20, $OR = 1.06, z = .46, p = .65$, or the unknown-number conditions, $OR = 1.12, z = .91, p = .36$.

Cheating on the first surprise trial. We next examined cheating on Trial 11, the first surprise trial in the expect-10 condition. As predicted, cheating rates dropped from Trial 10 to Trial 11 in the expect-10 condition (65.61% vs. 54.05%; see Figure 2), $OR = .79, z = 2.80, p = .005$ in mixed logistic regression analysis in which Trial 11 was coded 1 and Trial 10 was coded -1. Also as predicted, there were no significant differences in cheating on these two trials in the other conditions, $ps > .44$. These results replicate Study 1's findings and support the idea that people cheat more at the end of a series because they are averse to "wasting" a final opportunity for personal gain – not because their moral standards have eroded.

Cheating on Trial 20. We next examined whether the cheat-at-the-end effect would also emerge at the end of a longer series (i.e., on Trial 20). Consistent with this possibility, we predicted more cheating on Trial 20 in the expect-20 condition than the unknown-number condition.⁴ The results, however, did not support this prediction: A logistic regression analysis showed that an approximately equivalent proportion of participants reporting a winning flip on Trial 20 in the expect-20 (52.41%, coded 1) as in the unknown-number condition (49.29%, coded 0), $OR = 1.13, z = .75, p = .46$. We found similar results when we examined cheating within the expect-20 condition, using analyses analogous to those described above (i.e., multilevel logistic regressions with random intercepts and reverse-Helmert codes for Trials 1-20

included as fixed effects). The results showed that participants were not significantly more likely to cheat on Trial 20 than on Trials 1-19 in the expect-20 condition, $OR = .94$, $z = .51$, $p = .61$. In short, our analyses indicated a robust cheat-at-the-end effect on Trial 10 but not on Trial 20.

No moderation by prior earnings. As in Study 1, exploratory analyses revealed no evidence that the number of wins reported on Trials 1-9 moderated the condition differences in cheating on Trial 10, $p > .23$.

Linear vs. curvilinear effect. Finally, we conducted exploratory analyses to examine whether cheating would increase linearly or quadratically until the last trial (see Online Supplement). As in Study 1, cheating increased linearly until Trial 10 in the expect-10 condition. There was also a significant quadratic effect of cheating in this condition, reflecting a spike in cheating on Trial 10, the last one expected (see Figure 1). Unexpectedly, there was neither a linear nor a quadratic increase in cheating from Trials 1 to 20 in the expect-20 condition. Perhaps anticipatory regret was less potent in the expect-20 condition, even as the number of cheating opportunities dwindled, because of the large total number of expected opportunities to obtain gains.

Discussion

Study 2's results provided further support for Hypotheses 1 and 2. We observed elevated cheating on Trial 10 only when participants believed it was their last chance to cheat. We also observed a drop in cheating when we surprised them with additional opportunities. Any condition differences in the amount of overall cheating cannot explain these results because our between-condition analyses controlled for cheating prior to Trial 10, and our within-condition analyses compared cheating on Trial 10 to cheating on the average of the previous trials. In addition, we did not replicate the unanticipated finding from Study 1 of elevated cheating on

Trial 10 when participants expected more than 10 trials, suggesting that it may have been an anomaly in Study 1.

We found no evidence of a cheat-at-the-end effect in the expect-20 condition, perhaps because having so many (19) previous opportunities minimized anticipatory regret. Foregoing a last chance to cheat may not have felt like a “wasted opportunity” to people in that condition. This finding suggests a boundary condition: The cheat-at-the-end effect may be most likely when the set of opportunities to cheat feels limited.

In short, the cheat-at-the-end effect appears to be relatively robust (Hypothesis 1) and to depend on the number of trials participants expect to complete (consistent with Hypothesis 2) rather than the number of trials participants have already completed.

Meta-Analysis of Coin-Flip Studies

Before turning to findings using a different cheating measure, we present a meta-analysis to better estimate the size of the cheat-at-the-end effect. Our analysis combined the data from Studies 1 and 2 (excluding the surprise trials and the unknown-number condition) with data from five previous studies ($N = 716$) that had used the coin-flip paradigm to test hypotheses unrelated to cheating at the end. Two of these studies have been published (Bryan, et al., 2013, Studies 2 and 3); here we provide new analyses. Participants in the five previous studies were recruited online from either a university-maintained subject pool (Bryan et al., 2012, Study 2, $n = 118$, plus an unpublished study, $n = 44$), a social networking website (Bryan et al., 2012, Study 3; $n = 107$), or MTurk (two other unpublished studies; $ns = 145$ and 302 , excluding two duplicate submissions and two incomplete responses).⁵ In each of the five studies, participants completed 10 coin flips; preliminary analyses indicated that, across the five studies, participants were

significantly more likely to cheat on the 10th flip than on earlier flips. The meta-analysis of all seven studies included 2,138 participants completing a total of 24,250 trials.

The probability of reporting the winning flip on the last trial was 61.46% versus 54.59% for the average of the prior trials, $OR = 1.33$, $z = 6.08$, $p < .001$, suggesting that the cheat-at-the-end effect is robust and reliable. The odds ratio (OR) indicates that the odds of reporting the winning flip on the last trial were 1/3 higher than the odds of reporting it on earlier trials, but it underestimates the number of cheaters in the sampled population because 50% of participants should have flipped the winning outcome by chance alone. The best estimate is $2\lambda - 1$, where λ is the proportion of the sample who reported heads (Dawes & Moore, 1979, as described in Clark & Desharnais, 1998). This formula estimates that 22.92% of people in the population will cheat on the last trial, compared to only 9.18% on non-last trials – an odds ratio of 2.96.⁶ In other words, our data suggest that the odds of seizing an opportunity to cheat is almost three times higher on the last trial than on the average previous trial.

Study 3

The results of Studies 1 and 2 are consistent with Hypothesis 2 – that anticipatory regret about foregoing a final opportunity to profit drives the cheat-at-the-end effect – but the use of behavioral measures did not provide an opportunity to measure underlying psychological processes directly. We also wanted to assess whether people can anticipate falling prey to the cheat-at-the-end effect. Thus, to test Hypothesis 2, Study 3 asked people to engage in a thought experiment in which they imagined doing the coin-flipping task, and then (a) to estimate how they would feel about reporting honestly or dishonestly on Trial 7 if it was either their last or not, and (b) to report their inclination to lie on this trial. We expected that participants would generally profess little inclination to lie, but that they would indicate a greater willingness to lie

when Trial 7 was their last. We measured anticipatory regret to test whether it mediated this effect. We also measured other anticipatory emotions to rule out the possibility that people cheat more at the end because they expect to feel less guilty about cheating or prouder about being honest.

Method

Participants. MTurk participants ($N = 103$) completed Study 3 online in exchange for \$.51. (An additional 3 people abandoned the study before completing it). We blocked sign-ups by participants who had completed any of our previous studies, and we found no duplicate IP addresses or MTurk IDs in the data. By *a priori* decision, we dropped 33 participants who failed at least one comprehension-check question (described below), but the direction and significance of our results did not change when we retained these participants, except where indicated (see Footnote 8). The final sample included 70 people: 37 females, 32 males, 1 of unknown sex; $M_{\text{age}} = 36.09$, $SD = 13.82$).

Procedure. Participants read the materials used in Studies 1 and 2. This time, however, they were told that they should imagine completing the task; they would not actually be flipping a coin. We then asked them to suppose that they had completed six flips, and had accurately reported three heads (the winning flip) and three tails. (We chose these parameters because flipping heads half the time is the expected outcome, and because participants in Studies 1 and 2 had reported an average of three winning flips, rounded to the nearest whole number, on their first six trials). Then we asked participants to imagine that the 7th flip turned up tails, the losing outcome. We manipulated, between participants, whether the 7th flip was their last (the expect-7 condition, $n = 37$) or whether three flips remained (the expect-10 condition, $n = 33$).

Anticipatory feelings. Participants rated how they would feel if they accurately reported that their 7th flip was tails: regretful, foolish, like they had wasted an opportunity to win money, and wishing that they had reported heads instead. We averaged their responses to form an *anticipatory regret* index ($\alpha = .83$). To test whether other anticipatory feelings might account for the cheat-at-the-end effect (and to disguise our expectation that accurate reporting would create negative feelings), we also asked participants to rate how positively they would feel if they reported tails (proud, honest, virtuous, and ethical; averaged into an *anticipatory pride* index; $\alpha = .86$), as well as how negatively and positively they would feel if they inaccurately reported heads (guilty, unethical, dishonest, regretful, and wishing that they had reported tails instead; averaged into an *anticipatory guilt* index; $\alpha = .92$; smart, rational, justified; averaged into an *anticipatory justification* index; $\alpha = .62$). All responses used the same 1-5 scale: *not at all*, *slightly*, *somewhat*, *very much*, and *extremely*. We randomized the order in which we administered the set of items asking about accurate and inaccurate reporting, as well as the order of individual items within each set.

Inclination to cheat. Participants also rated what they would report “on this particular flip” (6-point scale: *definitely*, *probably*, *or maybe heads*; *maybe*, *probably*, *or definitely tails*) and how tempted they would be to “say that [they] flipped heads” (i.e., to lie; 5-point scale: *not at all*, *slightly*, *somewhat*, *very much*, *extremely*). Their responses to these items were highly correlated in each condition ($r_s > .60$) so we standardized and averaged them after reverse-coding the first item to create an *inclination to cheat* measure.

Comprehension checks. Finally, participants responded to two comprehension-check questions: “how many flips would remain after this one” (response options: 0 to 9), and “what were you asked to imagine that you actually flipped” (heads or tails).

Within-subjects manipulation. All participants were next asked to imagine a different scenario and to complete all measures again. Specifically, people who had previously imagined that there were 7 flips total now imagined that there were 10, and vice versa. Analyses of this within-subjects manipulation produced identical conclusions to analyses of the between-subjects manipulation. For the sake of brevity, we present only the between-subjects analyses below.⁷

Results

Skewness. We first applied non-linear transformations to reduce substantial skewness in each of our measures. For each variable, we selected the transformation that best reduced skewness (see Table 3 and its note).

Mean differences. The results were as predicted (see Table 3). Participants said that they would be more willing to cheat on Trial 7 when it was the last trial than when it was not, $p = .001$, $d = .83$. Also as predicted, they indicated more anticipatory regret about telling the truth when Trial 7 was last, $p = .04$, $d = .51$. Being faced with the last trial had no significant effects on other anticipatory feelings, $ps > .13$.

Mediation analysis. Also as predicted, the effect manipulating the number of trials on willingness to cheat was significantly mediated by anticipatory regret, $b = .31$ [.03, .67] for the indirect effect and its bootstrapped, bias-corrected 95% CI computed with 5,000 resamples in regression analyses that dummy-coded condition (expect 7 = 1, expect 10 = 0) and standardized the two continuous measures (Preacher & Hayes, 2004).

To examine robustness, we next tested whether this indirect effect through anticipatory regret would remain significant when we statistically controlled for any indirect effects through the other anticipatory feelings. The results of a multiple-mediator model (Hayes, 2013; Preacher & Hayes, 2008) showed that it did remain significant, and that none of the indirect effects

through the other anticipatory feelings were significant (see Figure 3). (The other anticipatory feelings were also not significant mediators in models that tested them individually). Thus, anticipatory regret robustly and uniquely mediated people's greater willingness to cheat on Trial 7 when they expected it to be last than when they did not.⁸

Follow-Up Study

It is possible that the expectation of completing 7 flips made participants more willing to cheat on *any* trial. If this were true, it would be inconsistent with our claim that people are more likely to cheat at the end. To address this possibility, we recruited a new sample of 106 MTurk participants. We report analyses of the 59 who remained after applying Study 3's exclusion criteria, but the results were the same with no exclusions except where indicated. The procedure was identical to Study 3's except that we measured inclination to cheat on Trial 3. As in Study 3, this study contained both a between-subjects and a within-subjects manipulation of the number of flips. We report between-subjects analyses, but within-subjects analyses produced identical conclusions, except where indicated.

We began by applying the same non-linear transformations that we did in the main study (see Table 3). As predicted, expecting to complete fewer flips did not make people more willing to cheat on this trial. In fact, unexpectedly, people were *less* willing to cheat on Trial 3 in the expect-7 condition than in the expect-10 condition (respectively, raw M s = -.29 and .31, SD s = 1.17 and .47; transformed M s = .44 and .78, SD s = .40 and .60), $t(57) = 2.60$, $p = .01$, $d = .69$ for the difference in transformed means. However, this mean difference was not very robust: it was in the same direction but not significant when all participants were retained for analysis, $p = .13$, and it did not replicate in the within-subject analyses, $p = .74$.

Participants did not anticipate that declining to cheat would lead to more regret in the expect-7 than in the expect-10 condition (respectively, raw M s = 1.44 and 1.88, SD s = .49 and .1.03, transformed M s = .47 and .56, SD s = .22 and .29), $t(57) = 1.30$, $p = .20$, $d = .35$ for the difference in transformed means. The other anticipatory emotions did not respond to the manipulation either, p s > .21.

Thus, Study 3's results are not easily explained by positing that expecting fewer flips made people more willing to cheat on *any* flip before the end.

Discussion

Study 3 made two contributions. First, it showed that people anticipate the cheat-at-the-end effect, even in the context of a thought experiment. Second, it provided direct evidence that increased anticipatory regret about honesty (and not other anticipatory emotions) mediates this effect.

People's decisions in ethical quandaries depend in part on how they anticipate they would feel following cheating versus honest behavior. Study 3's results do not challenge the idea that anticipated guilt about cheating or pride about honesty shape cheating behavior. Rather, the results suggest these anticipatory emotions loom just as large regardless of whether the quandary is last in a series; it is anticipatory regret about honesty that gets magnified at the end. Thus, it appears that the temptation to cheat grows stronger at the end, whereas felt moral obligation to resist temptation does not.

Although paradigms that use hypothetical scenarios can be limited by the fact that people may not be able to accurately anticipate their feelings and behavior, this issue is less of a concern in Study 3 because (a) participants' beliefs about their cheating behavior mirrored the actual

behavior we observed in Studies 1 and 2, and (b) we argue that anticipatory feelings – which Study 3 measured directly – play a key role in the cheat-at-the-end effect.

Study 4

Our final study tested the generalizability of the cheat-at-the-end effect by using a context that simulates repeated ethical choices at work. When people are paid by the hour and monitoring is difficult (as in many service professions), they may be tempted to overstate the time they have spent on the job (i.e., to overbill). When the number of shifts to work, tasks to complete, or site visits is also fixed (as with short-term or *ad hoc* employment), people may be more likely to over-report the time they worked on their final shift, task, or visit. Study 4 simulated this kind of situation by hiring *ad hoc* research assistants to code either 7 or 10 essays, one at a time. Their pay depended on their self-reports of the time they took to code each essay; we determined whether they cheated by surreptitiously measuring the time they actually spent coding each one. Our main prediction was that people would overbill more on the seventh essay when it was last than when it was not.

Method

Participants. We recruited residents of London, England in two waves from a university-maintained subject pool to complete the study online. After the first wave ($N = 196$), a within-subjects test of our main prediction was significant, but a between-subjects test was only marginally significant. We next increased statistical power by approximately doubling the sample size in a second wave ($N = 205$), and accounting for the increased Type-I error rate by computing $p_{\text{augmented}}$ for all statistically significant tests (Sagarin, Ambler, & Lee, 2014). In each wave, we excluded data from people who had previously submitted responses (as indicated by duplicate subject IDs or IP addresses; $n = 35$), who abandoned the study before completing all

trials ($n = 37$), or who had missing data because of technical problems ($n = 2$).⁹ There was no attention check. The final sample contained 327 people (208 females, 119 males; $M_{\text{age}} = 28.59$, $SD = 10.44$). When we reran the analyses without excluding any participants, the direction and significance level of the results were identical. A small majority were students (56%); of the non-students, 66% were employed full-time, 19% part-time, and 15% were unemployed. Forty-five nationalities were represented.

Procedure. Participants logged on to a website to do “temporary research assistant work” for pay plus a chance to win a £100 gift card. Written instructions informed them about a (real) study that had produced hundreds of short essays that needed coding. The instructions explained that we were “crowdsourcing” the coding by recruiting many assistants.

Participants learned that they would be paid £0.10 per minute of work time. Ostensibly because the essays’ length and difficulty varied, we instructed participants to report how long each one took them to code. We told participants to take as long as they needed, but no longer. Moreover, we explained, “We are counting on you to be honest. It would be dishonest to say that you spent longer than you really did.” Participants were randomly assigned to code either seven (expect-7 condition) or ten essays (expect-10 condition), and they were informed of the number that they would code before they began. Coding involved reading a paragraph-long essay and rating its similarity to a target essay that we provided. Each person in the expect-7 condition saw a randomly selected subset of the ten essays seen by people in the expect-10 condition. We randomized the essays’ presentation order.

The web survey surreptitiously recorded how long participants worked on each essay: It measured the time elapsed between the moment the relevant page loaded and the moment participants advanced to the next page. After participants coded an essay, the next page asked

them to report how long the coding had taken; response options ranged from 1 to 8 minutes in 1-minute increments. We compared how long participants reported taking to how long they actually took.

To draw attention to the sequential and finite nature of the tasks, each essay was labeled with its number in the series (e.g., “1 of 10”). Right before reporting how long an essay had taken to code, participants were told how many essays remained, and right before reporting how long the final essay had taken, they were reminded that it was their last. There were no surprise trials.

Results

To create a measure of overbilling, our dependent variable, we subtracted the amount of time participants took to code each essay from the amount of time they reported taking; positive numbers indicated overbilling. Initial inspection of the data revealed 16 cases (out of the 3290 total) in which more than 8 minutes elapsed before participants finished coding an essay – the maximum time the scale allowed participants to report, and greater than 4 *SDs* above the mean. Because it is unlikely that a participant would require much longer than 8 minutes to code, and because in 12 of these 16 cases the participant *reported* taking less than 8 minutes, we suspect that these participants took a break mid-coding. To reduce the extremity of these cases, we replaced the time taken with 8 minutes. (None of these cases arose on Trial 7; only one arose on Trial 10; seven were in the expect-7 condition and nine were in the expect-10 condition).

Figure 4 shows that on average, participants overbilled on each trial in each condition. More importantly, consistent with a cheat-at-the-end effect, overbilling was highest on Trial 7 in the expect-7 condition. Within the expect-10 condition, elevated cheating can also be seen towards the end of the trials. Formal tests of these effects follow.

Cheating on Trial 7 between conditions. We first examined whether the manipulation affected cheating on Trial 7. It did: Participants overbilled significantly more on Trial 7 in the expect-7 ($M = 1.29$, $SD = 1.66$) than in the expect-10 condition ($M = .91$, $SD = 1.55$), $t(325) = 2.16$, $p = .031$, $p_{\text{augmented}} = [.054, .069]$, $d = .24$ (see Figure 4).¹⁰ This difference remained significant when we controlled for the total amount of overbilling participants had committed on the first 6 trials, $F(1, 323) = 4.79$, $p = .029$, $p_{\text{augmented}} = [.051, .067]$, indicating that the results were not due to any condition differences in cheating before Trial 7.

Cheating on Trial 7 within conditions. As an additional test of the cheat-at-the-end effect, we examined whether participants cheated more on Trial 7 than on earlier trials within each condition. Table 4 displays the results of a multilevel regression model examining cheating as a function of trial (Trial 7 coded 6/7, Trials 1-6 each coded -1/7, remaining trials coded 0). As in Studies 1 and 2, trial was nested within participant, the model allowed random intercepts, and we included reverse Helmert codes for each of the earlier trials as fixed effects. The model thus tests whether participants overbilled more on Trial 7 than on previous trials, above and beyond any differences in overbilling on Trials 1-6. As predicted, people in the expect-7 condition overbilled more on Trial 7 than on the average of the previous trials, $b = .29$, $z = 3.87$, $p < .001$, $p_{\text{augmented}} = [.050, .050]$. Also as expected, there was no such increase in overbilling on Trial 7 in the expect-10 condition, $b = .08$, $z = 1.10$, $p = .27$ (see Figure 4).

Cheating on Trial 10 within condition. Because participants only completed a tenth trial in the expect-10 condition, we could not test between-condition differences in Trial 10 cheating. However, we could test whether participants in the expect-10 condition cheated more on Trial 10 than on Trials 1-9. To do so, we added reverse-Helmert codes for Trials 8-10 to the multilevel regression model described above. As expected, participants overbilled more on Trial

10 than on the previous trials, as indicated by a significantly positive coefficient on the code comparing Trial 10 to the previous trials, $b = .16$, $z = 2.32$, $p = .02$, $p_{\text{augmented}} = [.057, .061]$ (see Figure 4).

No moderation by prior earnings or prior cheating. Consistent with Studies 1 and 2, exploratory analyses found that the condition difference in overbilling on Trial 7 was not significantly moderated by the amount of overbilling committed on Trials 1-6, $p = .29$, nor by the amount of money earned on Trials 1-6, $p = .32$.

Linear vs. curvilinear effect. As in Studies 1 and 2, we also conducted exploratory analyses to examine whether cheating increased linearly or quadratically across the series (see Online Supplement). The linear effects were significant in each condition; the quadratic effects were not (see Figure 4). Thus, in each condition, cheating increased linearly until the end.

Discussion

The main contribution of Study 4 was to conceptually replicate the cheat-at-the-end effect in a more realistic context and with an internationally diverse group of participants (i.e., citizens of 44 different nations). The key trial showed the exact same pattern observed in Studies 1-3: People overbilled more for completing the 7th task in the series when they thought it was their last than when they did not. The effect cannot be explained by positing that people overbilled more for every task they completed when they expected 7 tasks total than when they expected 10: Participants overbilled significantly more for the 7th task than for the previous tasks, but only when they thought that the 7th would be their last. Finally, the results cannot be attributed to the specific tasks that participants completed because task order was randomized.

General Discussion

The present research reveals a novel phenomenon: the cheat-at-the-end effect. People in our studies were more likely to lie for financial benefit when it was their last opportunity to obtain this benefit than when it was not (Hypothesis 1). We demonstrated this phenomenon in a scenario study, three behavioral studies with monetary stakes (employing two different paradigms), and a meta-analysis. The latter suggests that the effect was large: The best estimate was that people are almost 3 times more likely to cheat on the last trial of the coin-flip paradigm than on earlier trials.

These results shed new light on the dynamics of unethical behavior. The findings are consistent with theory and research showing that people seek to balance their obligation to follow ethical principles with their temptations to benefit themselves dishonestly (e.g., Mazar, et al., 2008; Merritt, et al., 2010; Nisan, 1991; Tenbrunsel, et al., 2010; Tsang, 2002; Zhong, et al., 2009), but our results go further by showing how this balancing act plays out over a series of repeated ethical quandaries. Rather than spreading their dishonesty randomly or evenly across a series, participants were more likely to focus their cheating at the end.

In everyday experience, people often face the same ethical temptation repeatedly: Consultants and contractors can have several opportunities for overbilling; employees can have multiple chances to come into work late undetected; students usually have a number of assignments on which they can cheat. Our results suggest that people will be more likely to take an opportunity for unethical behavior when it comes at the end of a series.

Explaining the Cheat-at-the-End Effect and Addressing Alternative Explanations

Our findings also shed light on why the cheat-at-the-end effect occurs. We have argued that cheating behavior depends in part on anticipatory regret about passing up an opportunity to enrich oneself, and that anticipatory regret looms larger when no future opportunities remain. In

support of this idea, Study 3's participants expressed more anticipatory regret about being honest when it was their last chance to enrich themselves by cheating than when it was not, and this feeling in turn predicted their willingness to cheat (Hypothesis 2).

Moral self-licensing, ego-depletion, and slippery slope. The results of our behavioral studies also favored an anticipatory regret mechanism over alternative mechanisms based on moral self-licensing (Merritt, et al., 2010; Miller & Effron, 2010), ego-depletion (Mead, et al., 2009), or a slippery slope (Gino & Bazerman, 2009; Hartson & Sherman, 2012; Welsh, et al., 2015). Each of these alternative mechanisms predicts that the more opportunities people have had to resist cheating in the past, the less likely they will be to resist cheating in the future. In contrast, our studies showed that it was not the number of previous opportunities that led to cheating at the end, but rather the expectation that the current opportunity would be the last. This finding instead supports our contention that anticipatory regret arising from scarcity drives the effect. Moreover, ego-depletion and a slippery slope both predict that cheating rates will remain high when people discover “windfall” opportunities to cheat after the opportunity they had previously thought was last; instead, consistent with our claim that people cheat when they anticipate a scarcity of future cheating opportunities, our results showed that cheating rates plummeted on a first windfall opportunity, relative to the opportunity that participants had thought would be last (Studies 1 and 2). Thus, although our results do not question that these three mechanisms can affect cheating behavior in general, none of them provides an adequate explanation for the cheat-at-the-end effect.

Fear of detection. Did participants cheat at the end to avoid getting caught and punished? If the answer is yes, the cheat-at-the-end effect would resemble the end-game effects observed in experimental economics. Research on repeated prisoners' and social dilemmas, for

instance, has shown that people make more self-serving, competitive choices as the end of their interaction approaches (e.g., Ledyard, 1995); earlier competitive choices are less likely because they invite retaliation or punishment on subsequent trials. In contrast, it does not appear that concerns about detection and punishment could have influenced our results. In Study 1 and 2's coin-flip paradigm, it was impossible to tell whether any one individual cheated. We made this feature of the paradigm salient to participants because they completed the study remotely over an Internet connection, selected the coin themselves, and flipped it without any possibility of the researchers observing them. In Study 4, in which it was possible to surreptitiously detect cheating at the individual level, the chances of detection remained constant across decisions and conditions. Thus, even if participants did believe that their cheating could be detected, it is unclear why this belief would lead them to cheat more at the end than earlier.

Although concerns about detection and punishment are unlikely to have influenced our results, it is possible that participants cheated at the end because they had previously experienced real-world situations (e.g., those resembling repeated prisoners' dilemmas) in which saving selfish behavior for the end really would allow them to escape punishment. Such situations may lead people to develop a "cheat-at-the-end heuristic" on which they then mindlessly rely even when detection is impossible. Although this possibility cannot account for the mediation by anticipatory regret observed in Study 3, our results cannot definitively rule it out, and future research should explore it in greater depth.

Diminished anticipatory guilt. Perhaps cheating increased at the end not because people anticipated more regret about foregoing an opportunity for enrichment, but rather because they anticipated less guilt about seizing that opportunity dishonestly. However, in Study 3, participants did not anticipate significantly less guilt about cheating on a last opportunity than on

a non-last opportunity, anticipatory guilt did not reliably mediate the cheat-at-the-end effect, and anticipatory regret remained a significant mediator even when a (non-significant) indirect effect through anticipatory guilt was statistically controlled. Moreover, an anticipatory guilt mechanism struggles to account for why cheating decreased on the first surprise trial after the ostensible last one in Studies 1 and 2. Whereas discovering “windfall” opportunities should diminish anticipatory regret about not cheating (by diminishing the opportunities’ scarcity), it is unclear why such windfalls would decrease anticipatory guilt. These findings do not definitively rule out the possibility that an anticipatory guilt mechanism contributes to the cheat-at-the-end effect in addition to an anticipatory regret mechanism. However, we did not find reliable evidence for anticipatory guilt; instead, the results supported the anticipatory regret mechanism.

Other alternative explanations. Another potential alternative explanation is that our participants hoped to earn a certain amount of money (e.g., by flipping heads on at least half the trials), and waited to the end to see if they would need to cheat to attain their goal. This explanation predicts that the cheat-at-the-end effect will be most pronounced among people who have earned little money before the end. However, we found no evidence of this prediction in any of our studies. Thus, this explanation cannot account for our results.

A potential concern with our interpretation of the results was that our manipulations might have produced different rates of cheating before the end. For example, if participants cheated more on Trials 1-6 when they expected seven versus ten trials total, this might account for more cheating on Trial 7 in the expect-7 versus the expect-10 condition. We address this concern in several ways. First, as noted, the results of our between-condition analyses of key trials (e.g., comparing cheating rates on Trial 7 in the expect-7 and the expect-10 condition) were virtually identical when we statistically controlled for cheating rates on all earlier trials. Second,

we kept the cheating rates before the key trial in Study 3's vignette constant across conditions. Finally, our within-condition analyses showed that people cheated more on what they believed to be the final trial relative to the trials that came before.

Theoretical Advances

The present research contributes to the literatures on moral psychology and behavioral ethics by investigating the time course of decision-making over multiple ethical temptations. Although the literature on moral self-licensing has examined choices across pairs of seemingly unrelated decisions (Merritt et al., 2010), it tends not to study longer series of identical dilemmas. The present research demonstrates how viewing a decision as the last in a series can lead to increased unethical behavior.

Our findings also contribute to the study of regret. Theory and research have posited that regret and its anticipation can have positive outcomes by motivating people to understand and fix failures to achieve their goals (Epstude & Roese, 2008; Markman, McMullen, & Elizaga, 2008; Zeelenberg, 1999b), that people make more careful decisions when they worry about feeling regret (Reb, 2008), and that people value regret for a variety of its consequences, including self-insight and maintaining social harmony (Saffrey, Summerville, & Roese, 2008). However, anticipatory regret has a dark side as well. One investigation showed that people made decisions that were riskier and more self-interested in an ultimatum game when they had reason to anticipate that they would regret any decisions that did not maximize their payoff (Zeelenberg & Beattie, 1997). Our investigation reveals another dark side: Anticipatory regret can motivate unethical behavior. Thus, whereas anticipatory guilt can *inhibit* unethical behavior (e.g., Baumeister & Newman, 1994; Steenhaut & Van Kenhove, 2006; Tangney, Stuewig, & Mashek,

2007), we show how anticipatory regret can *increase* unethical behavior – particularly at the end of a series of ethical quandaries.

Patterns of Cheating Before the End

As noted, our theorizing was agnostic about whether cheating would gradually increase across a series or spike at the end. Our hypotheses thus focused on the point at which both of these possibilities predicts high levels of cheating: the very end. However, our data allowed us to conduct exploratory analyses testing patterns of cheating before the end.

Linear vs. curvilinear effects. Overall, the results were more consistent with the possibility that cheating would gradually increase across the series rather than spike suddenly at the end (see Online Supplement). Specifically, whereas a significant quadratic effect only emerged in two out of seven conditions, cheating increased linearly in all but one condition: the expect-20 condition in Study 2, where the larger total number of expected opportunities for gain may have allowed participants to satiate their need to enrich themselves before they reached the end. The linear effect could reflect a gradual increase in anticipated regret about foregoing increasingly scarce opportunities for enrichment, or from a gradual erosion of self-control (ego-depletion) or moral standards (slippery slope). As noted, however, these latter two explanations cannot explain the drop in cheating on initial, unexpected trials after an expected last trial, or why the between-subjects manipulations affected cheating.

Cheating in the middle vs. cheating at the end. Our results differ from the results of a previous study using a procedure like our coin-flip paradigm (Touré-Tillery & Fishbach, 2012 Study 1). In that study, participants proofread ten passages; before each one, they flipped a coin, privately, to determine whether the next passage would be long or short. They could minimize their workload by reporting that the result of the coin-flip assigned them to proofread the short

passage. The results revealed heightened cheating in the middle of the series. The authors explained this result by suggesting that people view cheating in the middle as less diagnostic of moral character. Although their finding is not logically incompatible with the cheat-at-the-end effect (i.e., cheating could spike in the middle and at the very end), our participants did not show heightened cheating in the middle (see Online Supplement), and theirs did not show heightened cheating at the end. One possibility is that, in the contexts we examined, participants' concerns about regret swamped their concerns about their behavior's diagnosticity. Although future research is clearly needed to resolve the apparent discrepancy between these two sets of results, we can offer some speculations.

First, participants in Touré-Tillery and Fishbach's study interacted with an experimenter in the lab after completing the series of coin flips. The authors acknowledged that the increased salience of the experimenter towards end of the series may have increased participants' adherence to ethical standards (perhaps by making them worry slightly more about detection or about the experimenter's perception of them); this may have prevented a cheat-at-the-end effect from emerging. In contrast, our participants worked anonymously online and did not interact with anyone; detection was impossible and the experimenters' impression of them was unlikely to be of much concern. Second, anticipatory regret is likely to loom particularly large when honesty means failing to capture a tangible benefit (e.g., money) than when it means avoiding a minor hassle like proofreading a somewhat longer passage. Thus, declining to lie in Touré-Tillery and Fishbach's study may have felt less like a "wasted opportunity" for personal gain than in our studies. Finally, their participants may have habituated to proofreading passages by the time they reached the final coin flip, thereby reducing their temptation to cheat and counteracting the cheat-at-the-end effect.¹¹

Oscillation between honesty and cheating. We also tested whether people would oscillate between honest and dishonest responses on each trial – a finding that would be consistent with models of moral self-regulation (Jordan, et al., 2011; Merritt, et al., 2010; Nisan, 1991; Sachdeva, et al., 2009; Zhong, et al., 2009). In one prior study, for instance, business school students who made a series of hypothetical decisions oscillated between more- and less-ethical choices (Zhong, Ku, Lount, & Murnighan, 2010). The present studies found mixed support for this hypothesis in Studies 1 and 2, and no support in Study 4 (see Online Supplement).

Future Directions

Future research might explore individual differences that would limit the cheat-at-the-end effect. As noted, we found no evidence that our effects were moderated by a measure of individual differences in moral self-concept (Jordan, et al., 2013), but additional studies could explore other individual differences, such as compunctions about cheating or aversion to anticipated regret.

Future research might also explore additional boundary conditions of the cheat-at-the-end effect. Our results identified one: The effect did not emerge on the last of 20 coin flips in Study 2. It may be that the cheat-at-the-end effect is most pronounced when the set of opportunities to cheat is relatively small; that is, people may cheat less at the end of a set of opportunities if they feel that they have already had sufficient opportunities to do well. Determining what people interpret as “sufficient” and whether it reliably curbs the cheat-at-the-end effect is an important task for future work. Another potentially important factor is the time frame of a person’s cheating opportunities. Twenty opportunities to cheat in 10 minutes may satisfy a person’s need

for dishonest self-enrichment, but twenty opportunities in 10 months may not feel as satisfying, resulting in more cheating at the end.

Our research examined the time-course of how people balance unethical “wants” with ethical “shoulds” (Tenbrunsel, et al., 2010); future research should examine whether a similar pattern emerges when people face a series of want/should conflicts outside the ethical domain (Milkman, Rogers, & Bazerman, 2008). For example, consider someone who has been traveling on vacation for six days. If it is now the last day of her trip, compared to if it is not, she might be more tempted to do the indulgent activities she wants to do (e.g., sleep until noon, see a low-brow comedy show, and eat a box of expensive truffles from a local chocolatier) instead of the more “virtuous” activities that she “should” do (e.g., get up early, go to a high-brow ballet, and forego the special chocolates in favor of a cheaper, healthier snack). The feeling that it is her “last chance” to do what she wants before her vacation ends may increase her willingness to indulge.

People in our studies anticipated that passing up a final opportunity to enrich themselves would make them feel regretful. A contrasting prediction could have been made: that people would anticipate regret about passing up a final opportunity to feel virtuous through honesty, which would lead to less cheating at the end. We did not make this prediction because we expected our paradigms to create a conflict between wanting to benefit the self and knowing that one should be honest (Tenbrunsel et al., 2010). In other words, we thought honesty would seem more like an obligation to follow an ethical rule than an opportunity to feel virtuous. The instructions framed the experimental tasks as an opportunity to earn money, and we recruited from subject pools that people join for financial reasons. Being honest would help the researchers study psychokinesis (Studies 1-3) or analyze a previous experiment’s data (Study 4),

neither of which would seem to hold great potential for feeling virtuous. We might not have observed a cheat-at-the-end effect if honesty would have helped a worthy cause, if participants had felt a need to prove how virtuous they were (see Effron, 2014, in press), or if they had been more intrinsically motivated to be honest for another reason. Future research should examine whether framing honesty as an opportunity rather than an obligation can turn the cheat-at-the-end effect into an honesty-at-the-end effect.

Implications and Conclusions

Organizations and policymakers that want to prevent unethical behavior may only have limited resources to monitor others. The present research suggests that it may be most efficient to monitor behavior towards the end of a series of decisions that allow lying and cheating. For example, teachers might be advised to check assignments most carefully at the end of the term; it may also be prudent to take extra care when verifying employees' final expense reports before a budget is due to expire. Also, an organization that announces the implementation of stricter verification rules for expense reports on a given date might devote extra resources to scrutinizing the reports that are filed shortly before the new rules go into effect.

Beyond the conventional approach of increasing surveillance, the unknown-number condition in Study 2 suggests a means by which cheating at the end could be prevented: If people do not know the endpoint of a series of cheating opportunities, they should be less likely to cheat at the end. Employers could avoid publicizing the period in which there will be an opportunity for dishonesty; for example, if employees did not know the exact length of their supervisor's out-of-town trip, they would not realize that a particular day was their last chance to sneak out of work early.

Hiding the endpoint is not always feasible, however. In such situations, organizations might focus on applying other anti-cheating interventions closer to the end of a period when dishonesty is possible. For example, previous research suggests that highlighting the victims of cheating (Wang & Murnighan, 2011), reminding people of their ethical standards (Mazar, et al., 2008), or framing dishonest behavior as a reflection of identity (Bryan, et al., 2013) can curb cheating. Although effective in “one shot” experiments, such strategies might become less effective as people habituate to them. Thus, deploying such strategies at times of peak expected dishonesty seems ideal, and the present research helps predict when such peak periods are likely to occur.

Although more research is needed to test these implications, our results suggest that organizations and individuals who want to prevent themselves and others from acting unethically should proceed with caution as they approach the end of a series of ethical decisions. In striving to balance self-interested temptations with ethical obligations, self-interest may be more likely to win at the end.

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Footnotes

¹ Another reason why it is difficult to predict the shape of the positive association between scarcity and cheating because it is unclear whether subjective perceptions of scarcity increase linearly as opportunities to cheat decrease. Consider someone faced with 10 cheating opportunities. After each opportunity, the *number* remaining decreases by a constant amount (i.e., 1), but the *proportion* remaining decreases by a larger and larger amount (i.e., $1/10^{\text{th}}$ after the first opportunity, $1/9^{\text{th}}$ after the second, $1/8^{\text{th}}$ after the third, etc.). We know of no research examining whether subjective perceptions of scarcity are more sensitive to number or proportion.

² The expect-10 condition contains more participants because it includes two versions: one in which the winning flip was constant across trials (i.e., only heads or only tails paid off, as in the expect-7 and expect-13 conditions), and one in which the winning flip alternated between heads and tails on each trial. The Online Supplement details our motivation for including the second version. This variation had no significant effects on the cheat-at-the-end effect, and dropping the second version did not change the relevant results.

³ We did not have strong predictions about whether people would cheat more on the last surprise trial than on earlier surprise trials. Exploratory analyses showed that in the expect-7 condition, which had six surprise trials, more people reported the winning flip on Trial 13 (58.06%) than on Trials 8-12 (50.51%), $OR = 1.36$, $z = 2.03$, $p = .04$. In the expect-10 condition, which had only three surprise trials, an equivalent proportion of people reported the winning flip on Trial 13 (55.08%) as on Trials 11 and 12 (53.66%), $OR = 1.06$, $z = .48$, $p = .63$.

⁴ We did not have clear predictions about how or whether Trial 20 cheating would differ between the expect-20 and expect-10 conditions, because (a) Trial 20 was a surprise only in the expect-10 condition, and (b) it was framed as the end of a shorter series in the expect-10

condition (i.e., the last of two series of 10 trials instead of the last of one series of 20 trials).

Exploratory analyses showed that slightly but not significantly more people reported the winning outcome on Trial 20 in the expect-10 condition (58.94%) than in the expect-20 condition (52.41%), $p = .12$.

⁵ Our analyses did not include a condition from the two published studies that was intended to (and did) eliminate cheating. We also could not analyze a condition from one of the unpublished studies in which participants were not asked to report the result of each of the 10 coin flips. One of the unpublished studies collapses across a writing-task manipulation that participants completed before the coin-flip task and that did not affect the results.

⁶ The odds of cheating is by definition $p_{\text{cheat}} / (1 - p_{\text{cheat}})$, where p_{cheat} is the probability of cheating. The odds of cheating are thus .2987 on the last trial and .1011 before the last. The relevant odds ratio is calculated by dividing .2987 by .1011.

⁷ We also included exploratory measures at the end of the study that were unrelated to the main hypothesis (e.g., what participants would report flipping on the first trial when expecting to complete 10 flips total; no manipulation was used for these measures).

⁸ When we retained participants who had been unable to correctly recall the number of flips remaining or whether the most recent flip was heads or tails, the indirect effect through anticipatory guilt became significant, $b = .10$ [.003, .27]. These results provide some evidence that anticipatory guilt may play a role in explaining why people anticipated being more likely to cheat at the end although, as noted, the results were not robust when inattentive participants were excluded. More importantly, the indirect effect through anticipatory regret remained significant even when we retained inattentive participants, $b = .14$ [.02, .36], indicating that anticipatory regret continued to explain the cheat-at-the-end effect above and beyond anticipatory guilt.

⁹ These counts do not include the first 94 people who were recruited in the second wave, because a research assistant accidentally misinformed them that their payment would be capped at £5 – substantially less than the maximum they could earn by cheating in the expect-10 condition.

¹⁰ $p_{\text{augmented}}$ quantifies how a test's Type I error rate (i.e., α , usually set at .05) was affected by our decision to collect a second wave of data after obtaining a marginally significant finding in the first wave (see Participants section; Sagarin et al., 2014). Because augmenting a dataset always increases α , $p_{\text{augmented}}$ is always higher than .05. Two values of $p_{\text{augmented}}$ are computed: the smaller value estimates α based on the assumption that the second wave of data would not have been collected if the test's p -value obtained the first wave had been any higher; the larger value estimates α based on the extremely conservative assumption that the second wave would have been collected even if $p = 1.00$ in the first wave. We computed $p_{\text{augmented}}$ using the spreadsheets available at <http://www.paugmented.com>, specifying 10,000 “slices.” Our results indicate that the actual Type-I error rate did not greatly exceed .05 even with the most conservative assumption.

¹¹ None of the other studies in Touré-Tillery and Fishbach (2012) had findings in tension with ours. Another study they reported examined cheating behavior, but did not allow for a test of the cheat-at-the-end hypothesis because each participant had only one opportunity to cheat that always came before the end of the series (Study 2). Their other studies did not examine cheating behavior (i.e., Study 3 examined adherence to religious customs, and Study 4 examined accuracy in a shape-cutting task).

Figures

Figure 1

Percentage of participants in Study 1 reporting the winning flip on each trial in each condition. Percentages above 50% on a given trial suggest that some participants cheated.

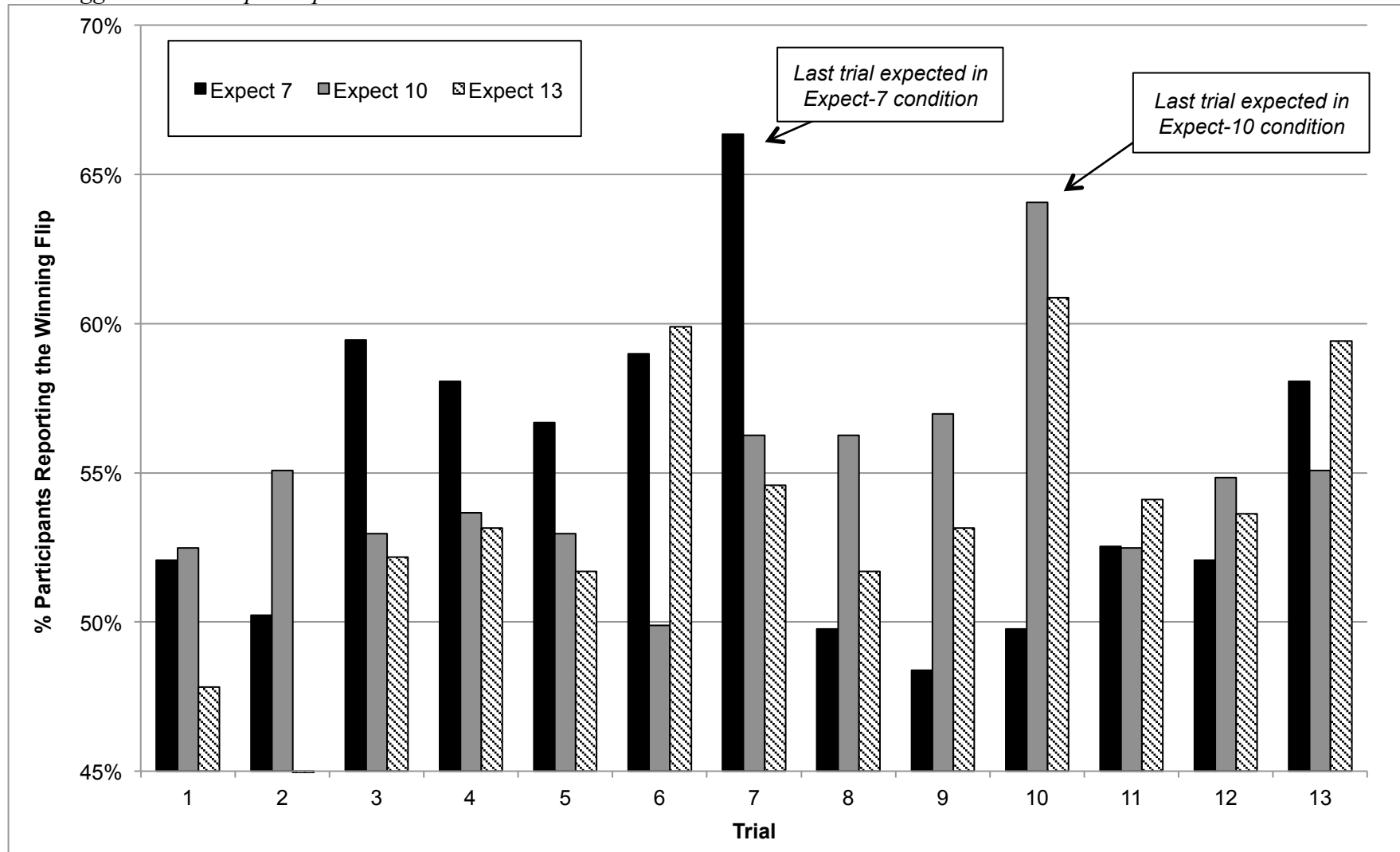


Figure 2

Percentage of participants in Study 2 reporting the winning flip on each trial in each condition. Percentages above 50% on a given trial suggest that some participants cheated.

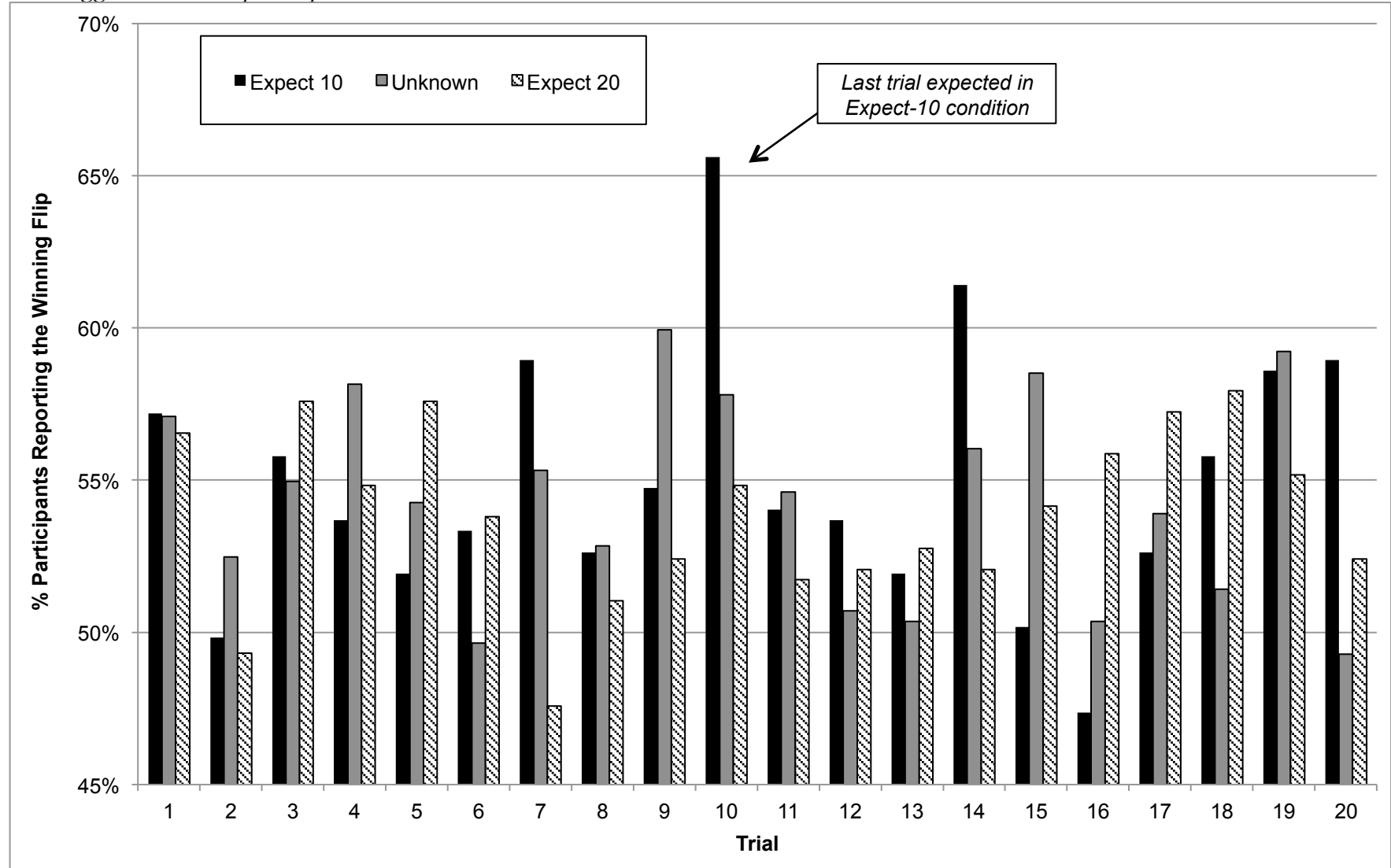


Figure 3
Multiple mediation analysis in Study 3

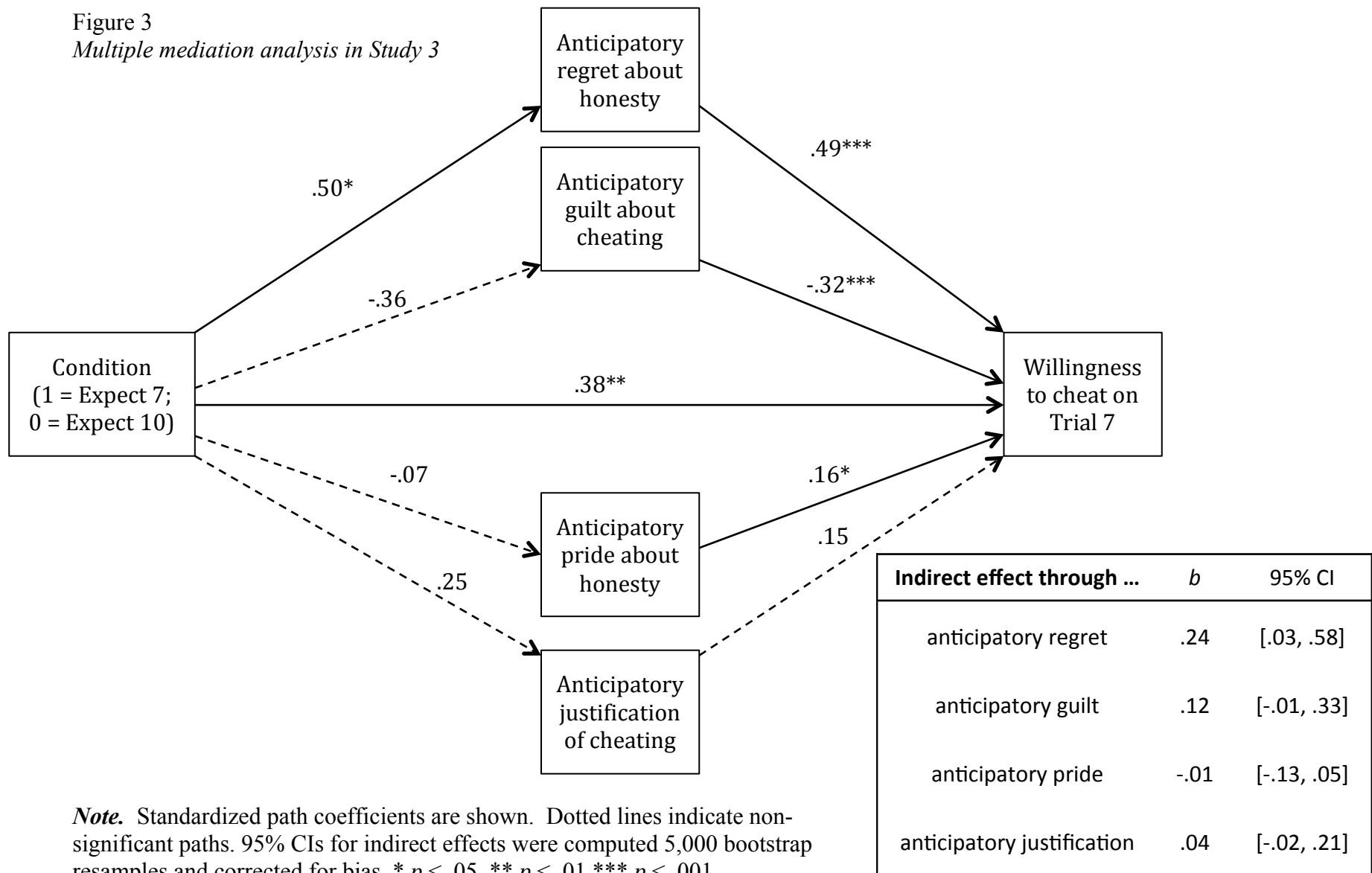
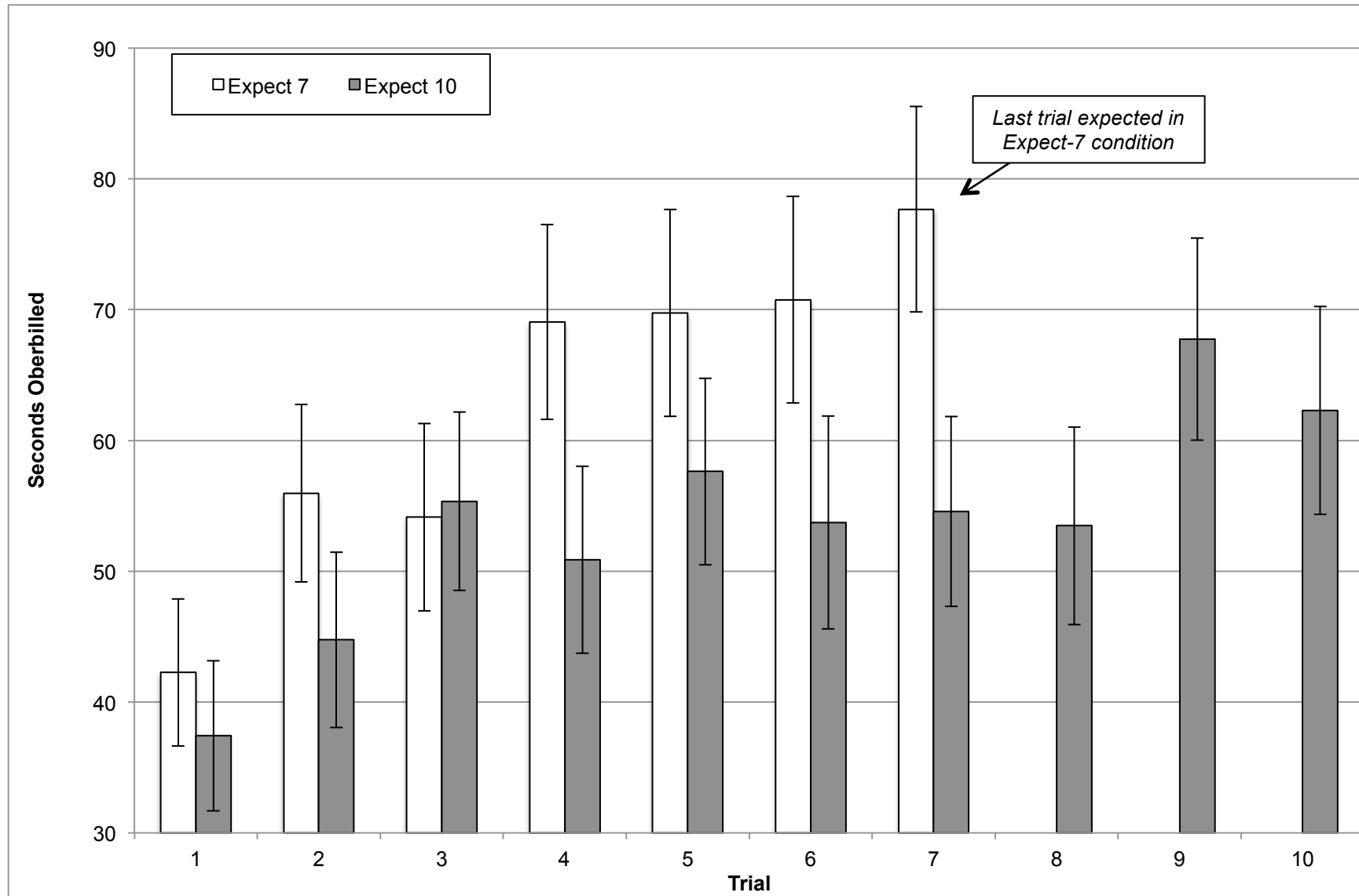


Figure 4

Average seconds overbilled in Study 4 (\pm SE) on each trial in each condition



Tables

Table 1

Fixed effects in Study 1's multilevel logistic regression analysis, by condition

Predictor	<i>Expect-7 Condition</i>				<i>Expect-10 Condition</i>				<i>Expect-13 Condition</i>			
	<i>OR</i>	<i>SE</i>	<i>z</i>	<i>p</i>	<i>OR</i>	<i>SE</i>	<i>z</i>	<i>p</i>	<i>OR</i>	<i>SE</i>	<i>z</i>	<i>p</i>
trial2	0.93	0.18	-0.38	0.70	1.11	0.15	0.76	0.45	0.89	0.18	-0.59	0.55
trial3	1.40	0.24	2.00	0.05 *	0.97	0.12	-0.28	0.78	1.27	0.22	1.37	0.17
trial4	1.18	0.19	1.05	0.29	1.01	0.11	0.06	0.96	1.22	0.20	1.21	0.23
trial5	1.07	0.16	0.45	0.65	0.98	0.11	-0.22	0.83	1.09	0.17	0.56	0.57
trial6	1.16	0.18	0.99	0.32	0.87	0.09	-1.33	0.18	1.51	0.24	2.62	0.01 **
trial7	1.55	0.24	2.86	0.00 **	1.15	0.12	1.31	0.19	1.13	0.17	0.79	0.43
trial8	0.73	0.11	-2.14	0.03 *	1.13	0.12	1.13	0.26	0.99	0.15	-0.10	0.92
trial9	0.72	0.10	-2.27	0.02 *	1.14	0.12	1.27	0.20	1.05	0.16	0.31	0.76
trial10	0.79	0.11	-1.64	0.10	1.51	0.16	3.90	0.00 **	1.44	0.22	2.39	0.02 *
constant	1.22	0.05	5.13	0.00	1.22	0.03	0.76	0.45 **	1.16	0.05	3.51	0.00 **

Note. Trial was reverse-Helmert coded so that each coefficient compares the odds of reporting the winning flip on a given trial to the odds of reporting it on the previous trials. Boldfaced text shows key hypothesis tests. Boxes show tests of whether people reported winning more on the last trial than on the previous trials. * $p < .05$. ** $p < .01$.

Table 2

Fixed effects in Study 2's multilevel logistic regression analysis, by condition

Predictor	<i>Expect-10 Condition</i>				<i>Expect-20 Condition</i>				<i>Unknown-Number Condition</i>			
	<i>OR</i>	<i>SE</i>	<i>z</i>	<i>p</i>	<i>OR</i>	<i>SE</i>	<i>z</i>	<i>p</i>	<i>OR</i>	<i>SE</i>	<i>z</i>	<i>p</i>
trial2	0.74	0.13	-1.78	0.08	0.74	0.13	-1.76	0.08	0.83	0.14	-1.11	0.27
trial3	1.10	0.16	0.63	0.53	1.21	0.18	1.30	0.19	1.01	0.15	0.05	0.96
trial4	0.98	0.14	-0.18	0.86	1.01	0.14	0.10	0.92	1.15	0.16	0.97	0.33
trial5	0.91	0.12	-0.67	0.50	1.13	0.15	0.92	0.36	0.94	0.13	-0.43	0.67
trial6	0.99	0.13	-0.11	0.91	0.94	0.12	-0.44	0.66	0.79	0.10	-1.78	0.08
trial7	1.25	0.16	1.68	0.09	0.74	0.09	-2.34	0.02 *	1.04	0.13	0.28	0.78
trial8	0.93	0.12	-0.56	0.57	0.89	0.11	-0.92	0.36	0.93	0.12	-0.55	0.58
trial9	1.02	0.13	0.18	0.86	0.95	0.12	-0.37	0.71	1.26	0.16	1.78	0.08
trial10	1.62	0.21	3.67	0.00 **	1.06	0.13	0.46	0.65	1.12	0.14	0.91	0.36
constant	1.22	0.04	6.44	0.00 **	1.17	0.03	5.29	0.00 **	1.19	0.04	5.72	0.00 **

Note. Trial was reverse-Helmert coded so that each coefficient compares the odds of reporting the winning flip on a given trial to the odds of reporting it on the previous trials. Boldfaced text shows key hypothesis tests. Box shows test of whether people reported winning more on the last trial than on the previous trials. * $p < .05$. ** $p < .01$.

Table 3

Descriptive statistics and tests of mean differences for each measure in Study 3

	Willingness to cheat	Anticipatory regret	Anticipatory guilt	Anticipatory pride	Anticipatory justification
Skewness					
Untransformed	1.52	1.87	-0.80	-0.70	1.09
Square root	0.38	1.44	-0.36	-0.40	0.81
Natural log	-0.53	1.06	0.05	-0.12	0.58
Reciprocal	-0.66	0.49	0.68	0.36	0.23
Untransformed Ms (SDs)					
Expect-7 condition	0.26 (.94)	1.69 (.83)	3.58 (1.10)	3.82 (1.04)	1.60 (.59)
Expect-10 condition	-0.29 (.79)	1.43 (.71)	3.97 (.96)	3.92 (.86)	1.58 (.82)
Transformed Ms (SDs)					
Expect-7 condition	.88 (.50)	.53 (.23)	.83 (.47)	1.01 (.37)	.57 (.24)
Expect-10 condition	.46 (.51)	.41 (.25)	1.00 (.46)	1.12 (.34)	.50 (.28)
Significance tests of transformed measures					
<i>t</i>	3.44	2.12	1.53	0.28	1.05
<i>p</i>	0.00	0.04	0.13	0.78	0.30
<i>d</i>	0.83	0.51	0.37	0.07	0.26

Note. Bold numbers indicate which transformation was applied to which variable before performing the significance tests. For each variable, we chose the transformation that produced the least skewed result. Because the transformations reduce positive skewness, we reverse-coded the two negatively skewed variables (i.e., anticipatory guilt and pride) before transforming them. After the square-root and natural-log transformation, we recoded anticipatory guilt and pride to their original direction; this recoding was not necessary following the inverse transformation, which reverses a variable's direction. Following the inverse transformation on the positively skewed variables, we recoded the values to their original direction. To avoid undefined values when applying the square-root or natural-log transformation to the willingness to cheat measure, we eliminated negative values by adding .76 to each score before transforming (the minimum score was -.756). The *t*-tests had 68 degrees of freedom.

Table 4

Fixed effects in Study 4's multilevel logistic regression analysis, by condition

Predictor	<i>Expect-7 Condition</i>					<i>Expect-10 Condition</i>				
	<i>b</i>	<i>SE</i>	<i>z</i>	<i>p</i>		<i>b</i>	<i>SE</i>	<i>z</i>	<i>p</i>	
essay2	0.23	0.10	2.42	0.02	*	0.12	0.09	1.33	0.18	
essay3	0.09	0.08	1.03	0.30		0.24	0.08	2.99	0.00	**
essay4	0.30	0.08	3.85	0.00	**	0.08	0.08	1.08	0.28	
essay5	0.24	0.08	3.13	0.00	**	0.18	0.07	2.42	0.02	*
essay6	0.21	0.07	2.83	0.01	**	0.08	0.07	1.07	0.29	
essay7	0.29	0.07	3.87	0.00	**	0.08	0.07	1.10	0.27	
constant	1.05	0.10	10.12	0.00	**	0.90	0.10	8.65	0.00	**

Note. Trial was reverse-Helmert coded so that each coefficient compares the overbilling for a given essay to overbilling for the average previous essay. Boldfaced text shows key hypothesis tests. Box shows test of whether people reported winning more on the last trial than on the previous trials. * $p < .05$. ** $p < .01$
