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Preface

Cheating more for less: Upward social comparisons motivate the poorly compensated to cheat

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ABSTRACT

Intuitively, people should cheat more when cheating is more lucrative, but we find that the effect of performance-based pay-rates on dishonesty depends on how readily people can compare their pay-rate to that of others. In Experiment 1, participants were paid 5 cents or 25 cents per self-reported point in a trivia task, and half were aware that they could have received the alternative pay-rate. Lower pay-rates *increased* cheating when the prospect of a higher pay-rate was salient. Experiment 2 illustrates that this effect is driven by the ease with which poorly compensated participants can compare their pay to that of others who earn a higher pay-rate. Our results suggest that low pay-rates are, in and of themselves, unlikely to promote dishonesty. Instead, it is the salience of upward social comparisons that encourages the poorly compensated to cheat.

Introduction

Employee dishonesty comes in many forms – from high-powered executives who engage in insider trading to wage workers who over-report hours. While the latter may, superficially, appear less troublesome, widespread low-stakes cheating can add up to substantial losses (Mazar & Ariely, 2006). For example, the phenomenon of "inventory shrinkage" (losses partly attributable to employee dishonesty, such as the misuse of employee discounts) costs retailers billions of dollars annually. And beyond small acts of dishonesty themselves, once people take a step down an unethical road, subsequent steps gradually become easier, and the magnitude of the violations larger (Gino & Bazerman, 2009; Lifton, 1986; Milgram, 1963). Thus, understanding contextual factors that encourage cheating at low-stakes is important.

We examine how economic incentives and fairness concerns interact to influence low-stakes cheating. Some have characterized dishonesty as an economic choice, arguing that it will be more prevalent as its benefits increase, controlling for the probability and costs of getting caught (Becker, 1974). For example, teachers are more likely to inflate students' grades as the financial incentives for doing so increase (Jacob & Levitt, 2003), and several laboratory studies have observed positive relationships between lying or cheating and the magnitude of incentives (Gneezy, 2005; Gneezy, Rockenback, & Serra-Garcia, 2013). Thus, when the benefits of dishonesty are positively correlated with pay-rate, those earning higher wages may be more likely to cheat than those earning less, because they have more to gain.

Material gain undoubtedly plays an important role in unethical activity. However, there is mounting evidence that psychological factors also matter, and that dishonesty is not simply the result of economic cost/benefit analysis. For example, fairness concerns can be a better predictor of employee dishonesty than self-interest (Gino & Pierce, 2010b), suggesting that they may be important in determining the relationship between pay-rate and dishonesty. Relatedly, individuals who recall an instance of unfairness or lose a computer game for unfair reasons subsequently behave more selfishly (Zitek, Jordan, Monin, & Leach, 2010), though it is unclear whether this pattern would extend to unethical behavior.

One source of workplace unfairness, or at least an indicator of it, is differential pay-rates for similar work. While such differences often exist for reasons that people find justifiable and fair – for example, differences in job tenure – less justifiable wage gaps also exist, such as gender differences attributed to discrimination. A psychological account might therefore predict that low wage earners, upon discovering that others earn more for doing the same work, will feel a sense of unfairness, and may be more likely to behave dishonestly to level the playing field. Thus, given salient interpersonal comparisons, a psychological account of dishonesty might predict increased dishonesty among low wage-earners, even

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though they have less opportunity to profit from dishonesty than their higher-earning colleagues.

In this paper we examine how pay-rate affects dishonesty. We do so in a context in which the material benefits of dishonesty are perfectly positively correlated with pay-rate, pitting economic and psychological predictions against each other and showing when and why each prevails. We hypothesize that the relationship between pay level and dishonesty depends on how readily a person can compare themselves to others who are earning different rates of pay. When this comparison is not salient, consistent with the economic account, we predict greater cheating when it is more lucrative – i.e., at higher pay-rates. However, when people can readily compare their own rate of pay to that of others doing the same work, we predict greater cheating among those earning lower rather than higher pay-rates. The next section discusses the theoretical basis for our predictions.

Theoretical framework

Organizational behavior scholars have devoted much attention to the role of compensation in employee satisfaction and performance. Much of this work has focused on understanding the antecedents of self-reported pay satisfaction (e.g., whether actual pay level or pay relative to comparable others better predicts pay satisfaction), using hypothetical scenarios or surveys of employees (e.g., Card, Mas, Moretti, & Saez, 2012; Harris, Anseel, & Lievens, 2008; Shore, Tashchian, & Jourdan, 2006; Sweeney & McFarlin, 2005; Williams, McDaniel, & Nguyen, 2006). A number of predictors of pay satisfaction have been identified, with the difference between the amount of pay employees think they should receive and the amount they actually receive being one of the stronger predictors (Williams et al., 2006).

In addition, some research has examined the influence of pay (dis)satisfaction on actual workplace performance. Ambrose, Seabright, and Schminke (2002) examined the extent to which perceptions of distributive injustice (largely a function of the extent to which one's pay is perceived to be fair) helped to explain a wide range of self-reported organizational sabotage behaviors (e.g., aggression, incivility, vandalism). Ambrose et al. (2002, p. 960) found that perceptions of distributive injustice were positively "associated with sabotage behavior aimed at restoring equity." Pay relative to peers can also help to explain performance by NHL players (Trevor, Reilly, & Gerhart, 2012) and accident rates and on-time deliveries among truck drivers (Kepes, Delery, & Gupta, 2009). Mas (2006) found that in the months after a police union lost final arbitration to management (because the judge selected management's offer to prevail), arrest rates and average sentence length declined, and crime reports increased, although much more so for property crimes such as burglary and larceny than for more serious crimes such as murder and rape.

Perhaps most relevant to the current research is a pair of studies by Greenberg (1990, 1993) examining the influence of pay dissatisfaction on employee theft. In a study of manufacturing plant employees who either did or did not suffer a 15% pay cut, Greenberg (1990) found that rates of inventory theft were significantly higher among employees who suffered a pay cut, particularly among those employees who received a sparse, unapologetic explanation for the pay cut. In a laboratory experiment, Greenberg (1993) promised all participants that they would receive $5 for an hour of clerical work, but, once the work was done, either informed them that they would receive the promised $5 or would be under-paid ($3). Participants then paid themselves from a stack of money left on a table. Under-paid participants stole significantly more money than participants who received the promised amount, although under-paid participants still left with less than $5 on average. Both studies suggest that earning less than a salient reference wage (prior or promised earnings) can stimulate theft. This finding is consistent with behavioral decision research suggesting that people are more likely to cheat to recoup losses than they are to achieve gains (Rick & Loewenstein, 2008; cf. Schweitzer, Ordonez, & Douma, 2004).

While our discussion thus far has focused on pay compared to promised or expected benchmarks, there are many possible reference points against which one's pay may be compared (Goodman, 1974; Ordonez, Connolly, & Coughlan, 2000). Some prior work suggests that earning less than comparable others may be even more aversive than earning less than expected. For example, Austin, McGinn, and Susmilch (1980) had participants individually perform a series of tasks for a given pay-rate. Participants were then joined by a confederate, and both independently completed the rest of their tasks. The experimenters manipulated both (non-social) counterfactuals (whether participants earned less, more, or the same per task as they did when working alone), as well as social comparisons (whether participants earned less, more, or the same per task as did the confederate). Ratings of satisfaction with the task were influenced by both social comparisons and counterfactuals, but ratings of anger and fairness were only influenced by social comparisons, with disadvantageous inequality viewed as particularly unfair (cf. Loewenstein, Thompson, & Bazerman, 1989). Similarly, in a survey of pharmaceutical managers, Blau (1994, Table 1) found that pay satisfaction was more closely related to social comparisons (pay level "compared to relevant employees in similar organizations") than to counterfactuals (pay level "compared to what I earned in previous years" or the difference between what employees thought they should earn and what they actually earn).

Thus, prior work suggests that aversive social comparisons may be more likely than aversive (non-social) counterfactuals to influence pay satisfaction. In fact, aversive social comparisons regarding pay may be particularly likely to lead to unethical behavior (e.g., Rick & Loewenstein, 2008). While this precise question has not been investigated, Gino and Pierce (2009, 2010a, 2010b) have found that people are willing to engage in costly dishonesty to reduce wealth-based inequity. In their experiments, participants were randomly paired, and each partner's initial wealth endowment was orthogonally manipulated. One of the partners was then randomly assigned to solve anagrams; the other was assigned to grade the solver's work and could behave dishonestly by over- or understating the solver's score. Wealth-based inequity affected dishonesty such that poor graders dishonestly hurt wealthy solvers (by understating solvers' scores), even when they incurred a financial cost by doing so (Gino & Pierce, 2009). This work raises the intriguing possibility that aversive (upward) social comparisons based on wealth disparities stimulate retributive dishonesty (cf. Moran & Schweitzer, 2008).

Whereas Gino and Pierce (2009) manipulated initial wealth levels, we test the effect of awareness of alternative pay-rates on cheating, holding initial wealth levels constant. We do so because, in an organizational context, cheating is more likely to be a function of differences in pay-rates than of differences in initial wealth levels. Also, we examine how social comparison processes drive unethical behavior that solely benefits the self, rather than, as in Gino and Pierce (2009), unethical behavior that affects both parties. It is important to understand how pay-rates influence unethical behavior that solely benefits oneself, since many acts of dishonesty are intended to solely benefit oneself (DePaulo, Kashy, Kirkendol, Wyer, & Epstein, 1996). Finally, although Gino and Pierce's (2009) work is consistent with social comparison processes playing an important role in dishonesty, this conclusion cannot be made definitively because dishonesty was not measured in the absence of social comparison information. In *all* conditions, graders were aware of the wealth level of their solvers – making

it, at a minimum, ambiguous whether it is upward or downward social comparisons that affect dishonesty (or both). For example, poor graders tended to overstate the scores of poor solvers and to understate the scores of wealthy solvers, but without knowing the baseline tendency for a poor grader to dishonestly help or hurt his or her solver (i.e., in the absence of information about the solver's wealth), the direction of the effect is unclear. In the present studies we include conditions in which participants were unaware of pay disparities.

Overview of the present research

We conducted two incentive-compatible experiments to examine when and why people cheat more at low pay-rates than at high pay-rates. In Experiment 1, we test the basic hypothesis that the effect of performance-based pay-rates on cheating will depend on whether people are aware that others are earning a different pay-rate. When people are aware of an alternative pay-rate, we predict that cheating will be higher with a lower pay-rate, but we predict that this effect will not occur (or reverse) when people have no such awareness.

In Experiment 2, we examine two possible mechanisms underlying this pattern. People who are aware that others are earning more for the same work may be driven to cheat by at least two forces: dissatisfaction that they themselves did not earn the higher pay-rate and dissatisfaction that others are earning the higher pay-rate. In Experiment 2, we examine the relative importance of each mechanism by manipulating whether participants were aware that they could have earned a different pay-rate or are aware that others are earning a pay-rate that they themselves could have earned.

In reporting the experiments, we follow the standards outlined by Simmons, Nelson, and Simonsohn (2011). No conditions or measures were dropped from any of the experiments reported in this paper. We also provide justification for participants excluded from the analyses (14/132 in Experiment 1 and 11/183 in Experiment 2).

Experiment 1

We manipulated the level of performance-based pay, and awareness of an alternative pay-rate, in an experiment in which participants could cheat. Participants received 5 cents or 25 cents per (self-reported) correct response, either with no awareness of the alternative pay-rate (Unaware conditions) or with awareness (Aware conditions) (see Blount & Bazerman, 1996, and Bracha, Gneezy, and Loewenstein (in press), for similar methodologies applied to labor supply). We created awareness of the alternative pay-rate in the Aware conditions by flipping a coin in front of each participant to determine whether they would receive 5 cents or 25 cents per correct response. We hypothesized that participants in the Aware conditions would cheat more at the lower pay-rate than at the higher pay-rate, but that this would not occur (or be reversed) in the Unaware conditions.

Material and methods

We used an experimental cheating paradigm similar to that of Mazar, Amir, and Ariely (2008), but instead of trivia questions we used "coordination questions" (cf. Mehta, Starmer, & Sugden, 1994). Participants answered each question *as they expected a plurality of a separate group of similar respondents to answer it*, and received a point if they correctly guessed the most common response (determined by a pilot study of 23 students). For example, the most common answer to the question "Name a type of bird that has long legs" was "ostrich," and the most common answer to the question "Name a sport that requires a net" was "tennis." (Full stimuli are available from the authors upon request.) We expected that coordination questions would make it easy for people to persuade themselves that they actually deserve credit for incorrect responses (e.g., if the correct answer had come to mind even though they had written down a different answer). Recent work on "justified ethicality" by Shalvi and colleagues (e.g., Shalvi, Dana, Handgraaf, & De Dreu, 2011) suggests that people feel more justification for behaving dishonestly when previously exposed to desired counterfactuals. For example, people who are motivated to report that a flipped coin landed heads-up would be more likely to lie about the coin landing heads-up if a previous flip landed heads-up. Thus, in the context of the coordination questions, mere consideration of the correct response may help participants justify reporting an incorrect response as correct.

Participants at a private northeastern university ($N = 132$, 60% male; $M_{age} = 19$) were given a payment slip and a workbook containing 40 coordination questions grouped in four rounds of 10 questions. After recording their answers in each round, participants were given the answer key, graded their own work, and entered their score on a payment slip. Participants could therefore cheat by overstating their score. After the four rounds, participants exchanged their payment slips for a questionnaire (described below) that they completed while the experimenter prepared their payment.

The manipulations were reinforced by the payment slips, of which there were three printed versions: \$0.05 Unaware, \$0.25 Unaware, and \$0.05 vs. \$0.25 Aware (stimuli available from the authors upon request). In all versions, there were checkboxes representing each of the 11 possible scores for the given round (ranging from 0/10 to 10/10); participants checked the box corresponding to their (self-reported) score. The per-point pay-rate was indicated at the top of the pay-slip. In the Unaware versions, there was no mention of an alternative payment. The Aware payment slip contained a \$0.05 box and a \$0.25 box at the top of the slip; the experimenter checked the box corresponding to the pay-rate. Thus, there were two versions of the Aware payment slip (one in which the \$0.25 per point box was checked, and one in which the \$0.05 per point box was checked).

After submitting their pay slip, participants were given a survey that included supplementary (exploratory) measures and demographic questions. All measures are listed in the Appendix. Below, we focus our analyses on (self-reported) behavior, to test our central (behavioral) hypothesis.

Results

Fourteen subjects were excluded from the analyses because their self-reported scores were ambiguous – they checked multiple boxes on the payment slip (e.g., checking both the '4 out of 10' and '1 out of 10' boxes), leaving a sample size of 118. However, when these fourteen subjects were included (by assuming that their score was equal to the number of boxes they checked), all previously reported significant results remained significant. No observations were further than three standard deviations from the mean for any of the dependent measures, and no other observations were excluded.

Self-reported scores

We conducted a factorial ANOVA, treating the total self-reported scores across rounds as the dependent variable and payment level and awareness as independent variables. As predicted, there was a significant interaction between payment level and awareness ($F(1,114) = 5.89$, $p = .017$; Fig. 1). In the Aware conditions, participants earning 5 cents per point reported significantly higher scores than those earning 25 cents per point ($M_{5cents} = 20.2$

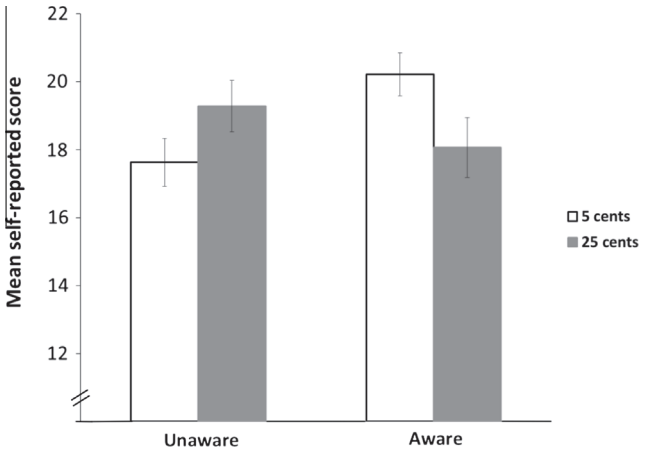

Fig. 1. Awareness of alternative pay-rates reverses the effect of performance-based pay on cheating (Experiment 1). *Note*: Error bars represent one standard error of the mean.

vs. $M_{25cents}$ = 18.1, $t(54) = 2.00$, $p = .05$). In the Unaware conditions, if anything, participants' behavior was consistent with that predicted by economic theory: participants earning 5 cents per point reported lower scores than those earning 25 cents per point, although the difference was not statistically significant (M_{5cents} = 17.6 vs. $M_{25cents}$ = 19.3, $p = .12$).

We also analyzed the data within pay-rate and across awareness levels. Participants earning 5 cents per point reported significantly higher scores when they were aware that others were earning more than when they were unaware that others were earning more (M_{Aware} = 20.2, $M_{Unaware}$ = 17.6; $t(51) = 2.64$, $p = .011$). Participants earning 25 cents per point reported slightly lower scores when they were aware that others were earning less than when they were unaware that others were earning less (M_{Aware} = 18.1, $M_{Unaware}$ = 19.3; $t(63) = 1.05$, $p = .30$).

Exit survey

The first four supplementary measures assessed participants' thoughts about the pay-rate, and are reported in Table 1. Consistent with the behavioral results, participants in the 5 cent/Aware condition were most likely to judge the payment scheme to be unreasonable and unfair, to indicate that they deserved higher payment and that they had thought about what others were earning. However, none of these measures mediated the effect of condition on self-reported scores.

Discussion

In Experiment 1, as predicted, awareness of alternative pay levels moderated the effect of pay-rate on cheating. When pay-rates were public, people cheated more when the economic incentive for doing so was lower; when pay-rates were private, people cheated slightly more when the economic incentive for doing so was higher. Note that the trend in the Unaware conditions helps to rule out income effects as an alternative account for the pattern in the Aware conditions (i.e., participants earning $0.25 per point were not working less hard in the Aware conditions because they were more financially comfortable and had a greater demand for leisure).

The realization that others are earning more for the same work is likely to be a critical driver of cheating among low-earners in the Aware condition. Indeed, because participants completed the study in the same room and at the same time, participants in the Aware condition could infer that different individuals earned different rates of pay. Moreover, because people tend to naturally compare themselves to those who are better off (Collins, 1996), participants in the 5 cent/Aware condition may have been particularly likely to compare their pay-rate to those in the room who they presumed to be earning 25 cents per point. Therefore, the primary purpose of Experiment 2 was to examine whether the greater cheating among low-earners in the Aware condition was driven primarily by awareness that others were earning more, or by awareness that one easily could have been earning more, independent of the payoffs of others.

A small procedural change (described below) in Experiment 2 also enabled us to measure levels of cheating more precisely. In Experiment 1 it is possible that the manipulations affected *actual* performance – participants earning a low pay-rate may have been motivated to try harder when made aware that they could have earned a higher pay-rate, leading them to perform better. Experiment 2 rules out this alternative explanation by measuring true performance (while still giving participants an opportunity to cheat), which makes it possible to measure differences in cheating precisely – i.e., differences between self-reported and actual scores ("overstatement scores").

Experiment 2

The purpose of Experiment 2 was to distinguish between two possible mechanisms underlying the pattern observed in Experiment 1, one involving counterfactual comparisons, and the other involving interpersonal comparisons. Experiment 2 manipulated both pay-rate (5 vs. 25 cents) and whether participants focused on what others are earning, on what they themselves could have earned, or on neither. In a Local Inequity condition, participants were aware that some participants in the room would earn 5 cents per point, whereas others in the room would earn 25 cents per point (as in the Aware condition in Experiment 1). Presumably, low-pay participants in the Local Inequity condition focused on what others were earning. In a Local Equity condition, participants were aware of the alternative pay-rate, but they were also aware that all participants in their session would receive the same pay-rate. Presumably, low-pay participants in the Local Equity condition focused on what they could have earned. We also included

Table 1
Mean survey responses by condition (Experiment 1).

	Unreasonable Pay	Unfair Pay	Deserved More	Upward Counterfactuals
5 cents				
Unaware	2.48	2.24	2.90	2.89
Aware	3.00*	3.09**	3.35	3.78*
25 cents				
Unaware	1.84	1.63	2.06	2.84
Aware	1.84	2.10	2.61	3.17

Note: The response scale for each item ranged from 1 (strongly disagree) to 5 (strongly agree). Within the 5 cents conditions, significant differences from the Unaware condition are indicated by: $^{**}p < .01$, $^{*}p < .05$. Within the 25 cents conditions, there were no significant differences from the Unaware condition.

an Unaware (control) condition, in which participants were unaware of the alternative pay-rate (as in Experiment 1).

If aversive counterfactuals are sufficient to drive low-earners to cheat, we should find that participants who earn a low pay-rate are more likely to cheat than participants who earn a high pay-rate in both the Local Equity and Local Inequity conditions. If upward social comparisons are necessary to drive low-earners to cheat, we should find that participants who earn a low pay-rate are more likely to cheat than participants who earn a high pay-rate in only the Local Inequity condition.

Material and methods

Participants at a private northeastern university ($N = 183$, 60% male; $M_{age} = 21$) arrived at a classroom in groups of 4–10. Due to the nature of the awareness manipulation (described below), it was only possible to run one condition per session; condition was varied sequentially between sessions. At the beginning of each session the experimenter explained the task and then read aloud and displayed the pay-rate information on an overhead projector. In the Unaware conditions, all participants received the same pay-rate (either 5 cents or 25 cents per point) and there was no mention of an alternative pay-rate. In the Local Equity condition, participants were told that they would all receive either \$0.05 or \$0.25 per point, determined randomly by a coin flip. Therefore, awareness of an alternative pay-rate was the only difference between the Unaware and the Local Equity conditions. In the Local Inequity condition, participants were told that some would earn \$0.05 per point, and others would earn \$0.25 per point, determined randomly by a coin flip. Therefore, unlike the Local Equity condition, in the Local Inequity condition, pay disparities existed within a session (locally).

Specifically, in the Local Inequity condition, the experimenter said: "Before I flip the coin, I am going to give each person a slip of paper with a letter on it. Half of the slips are marked **A**; the other half are marked **B**." The experimenter shuffled the slips over an overhead projector and handed each participant a slip. Participants were told: "If you received a slip marked **A** and the coin turns up heads you will receive 25 cents per point; if it turns up tails you will receive 5 cents per point. If you received a slip marked **B** and the coin turns up heads you will receive 5 cents per point; if it turns up tails you will receive 25 cents per point." Finally, the experimenter flipped a coin on the projector and circled the appropriate payment scheme on the sample pay slip displayed on the projector. To ensure the number of sessions in which participants were paid 5 vs. 25 cents was balanced, the experimenter flipped a quarter that was either double-sided heads or double-sided tails, alternating between sessions. It was not possible for participants to discern that the quarter was double-faced because they only saw the side on which the quarter landed. By physically surrounding participants with others, half of whom would receive a different pay-rate, this manipulation was designed to prompt social comparison to others earning a different pay-rate.

In the Local Equity condition, participants did not receive **A** and **B** slips and were instead told: "If the coin comes up heads, each of you will receive 25 cents per point. If it comes up tails each of you will receive 5 cents per point." The experimenter then flipped the double-faced quarter on the overhead projector, alternating heads vs. tails between sessions. Because participants were surrounded by others who received the same pay-rate, dissatisfaction with a low pay-rate was presumably driven primarily by thoughts about how easy it would have been to earn the higher pay-rate.

Next, in all three conditions, the experimenter distributed the workbooks, casually telling participants not to worry if their handwriting is messy because the workbooks would not be collected.

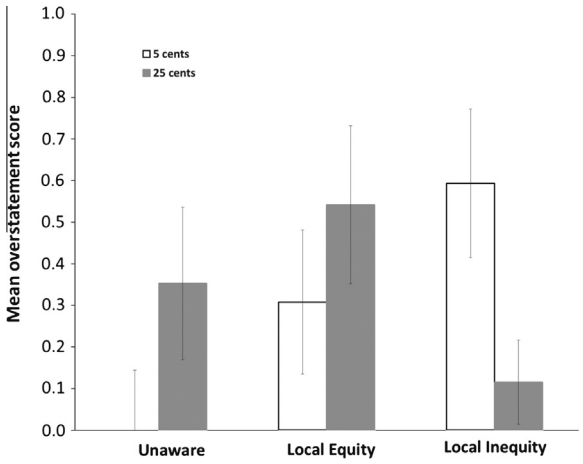

Fig. 2. Awareness of alternative pay-rates reverses the effect of performance-based pay on cheating only when upward social comparisons are made salient (Experiment 2). *Note*: Error bars represent one standard error of the mean.

This procedural change was implemented in an attempt to use workbook entries as a measure of true performance.

Unbeknownst to participants, we covertly collected their workbooks (by fishing them out of the garbage bin into which participants were instructed to throw them). Research assistants, blind to hypotheses and conditions, graded the workbooks.

After submitting their pay slip, participants were given an exit survey that included supplementary measures (listed in the Appendix) and demographic questions. Additionally, because it was not possible to prevent students who had participated in Experiment 1 from signing up for Experiment 2, a final item asked participants to report whether they had "participated in this, or a similar experiment before."

Results

Seven participants who indicated that they had participated in Experiment 1 were excluded from all analyses. Of those remaining, four took their workbooks with them (2 from the 25 cent/Unaware condition, 1 from 25 cent/Local Inequity, 1 from 25 cent/Local Equity), making it impossible to measure their overstatement; therefore, the results are restricted to the 172 participants for whom we have workbook data. No observations were further than three standard deviations from the mean for any of the dependent measures, and no other observations were excluded.

True performance

Across conditions, participants paid 25 cents per point performed significantly better than those paid 5 cents per point ($M_{25cents} = 18.3$, $M_{5cents} = 16.9$; $F(1,166) = 6.03$, $p = .015$). There was no main effect of awareness, nor was there an interaction between awareness and pay-rate on true performance.[1]

Cheating

We calculated each participant's overstatement score by subtracting their actual score (as graded by research assistants, blind to hypotheses and conditions) from their self-reported scores as indicated on the pay slips. Approximately one quarter of

[1] We could not measure true performance in Experiment 1 because participants were not informed that their workbooks would not be collected, and so may have engaged in undetectable forms of cheating such as replacing incorrect answers with correct ones after having received the answer key, or waiting until the answer key was distributed to fill in the workbook.

participants overstated their scores, and across all participants, the mean overstatement score was .32 (SD = .91).

As predicted, a factorial ANOVA revealed a significant interaction between payment level and awareness for overstatement scores ($F(2,166) = 3.66$, $p = .028$; Fig. 2). Follow-up analyses revealed that, among participants in the Local Inequity condition, those paid 5 cents per point overstated scores more than those paid 25 cents per point ($M_{5cents} = 0.59$, $M_{25cents} = 0.12$; $F(1,56) = 4.79$, $p = .033$), consistent with the Aware condition of Experiment 1. There were no significant differences in overstatement scores between those paid 5 cents vs. 25 cents in either the Unaware ($p = .14$) or Local Equity conditions ($p = .37$).

We also analyzed the data within pay-rate and across awareness levels. Consistent with Experiment 1, participants earning 5 cents per point had significantly higher overstatement scores when they were aware that others were earning more than when they were unaware that others were earning more ($M_{Local\ Inequity} = 0.59$, $M_{Unaware} = 0.00$; $t(60) = 2.57$, $p = .013$). Merely being aware of the counterfactual pay-rate, without aversive social comparisons, did not significantly increase overstatement scores among participants earning 5 cents per point ($M_{Local\ Equity} = 0.31$, $M_{Unaware} = 0.00$; $t(54) = 1.38$, $p = .17$).

An analysis of overstatement scores alone cannot shed light on whether the 5 cent/Local Inequity condition increased the proportion of participants cheating, the severity of cheating, or both. We conducted exploratory analyses to disentangle these two possible effects. Within the 5 cents conditions, the proportion of participants who cheated (i.e., who had overstatement scores >0) did not differ significantly across conditions (Fisher's exact test $p = .43$; see Table 2A). However, the proportion of participants who engaged in more serious forms of cheating (i.e., overstatement scores >1) differed significantly across conditions (Fisher's exact test $p = .015$). Specifically, there was a smaller proportion of "serious cheaters" in the Unaware condition relative to both the Local Equity (0% vs. 12%; Fisher's exact test $p = .09$) and Local Inequity conditions (0% vs. 22%; Fisher's exact test $p = .011$). It appears that the 5 cent/Local Inequity condition did not significantly increase the proportion of participants who felt they had license to cheat, but rather gave some participants (many of whom may have cheated anyway) license to overstate their scores to a greater degree.

Participants earning 25 cents per point had slightly lower overstatement scores when they were aware that others were earning less than when they were unaware that others were earning less, but this difference was not significant ($M_{Local\ Inequity} = 0.12$, $M_{Unaware} = 0.35$; $t(58) = 1.04$, $p = .30$). The proportion of participants who cheated (i.e., who had overstatement scores >0) also did not differ significantly across conditions (Fisher's exact test $p = .28$; see Table 2B). However, the proportion of participants who engaged in more serious forms of cheating (i.e., overstatement scores >1) was marginally significantly different across conditions (Fisher's exact test $p = .064$). Specifically, there was a smaller proportion of "serious cheaters" in the Local Inequity condition relative to both the Unaware (0% vs. 15%; Fisher's exact test $p = .063$) and Local Equity (0% vs. 17%; Fisher's exact test $p = .046$) conditions. These differences may have been driven by guilt among participants in the 25 cent/Local Inequity condition, a possibility we discuss further in the next subsection. It is also worth noting that merely being aware of the counterfactual pay-rate did not significantly influence overstatement scores among participants earning 25 cents per point ($M_{Local\ Equity} = 0.54$, $M_{Unaware} = 0.35$; $t(56) = 0.70$, $p = .49$).

Table 2A
Proportion of cheaters (overstatement score >0) and serious cheaters (overstatement score >1) in the 5 cent conditions (Experiment 2).

	Proportion of *P*s with overstatement score >0	Proportion of *P*s with overstatement score >1
5 cent/Unaware	20%[a]	0%[a]
5 cent/Local Equity	23%[a]	12%[ab]
5 cent/Local Inequity	34%[a]	22%[b]

Note: Within a column, proportions that do not share a common subscript differ at the .05 level (by Fisher's exact test).

Table 2B
Proportion of cheaters (overstatement score >0) and serious cheaters (overstatement score >1) in the 25 cent conditions (Experiment 2).

	Proportion of *P*s with overstatement score >0	Proportion of *P*s with overstatement score >1
25 cent/Unaware	21%[a]	15%[ab]
25 cent/Local Equity	38%[a]	17%[a]
25 cent/Local Inequity	19%[a]	0%[b]

Note: Within a column, proportions that do not share a common subscript differ at the .05 level (by Fisher's exact test).

Exit survey

An analysis of the supplementary measures collected at the end of the experiment may help to shed light on why overstatement scores differed by condition. The first five items measured thoughts about the pay-rate, and Table 3 displays the mean response to each item by condition.

Recall that overstatement scores did not differ significantly between the 5 cent/Local Equity condition and the 5 cent/Unaware condition. Similarly, there were only small differences between the two conditions on the supplementary measures. Participants in the 5 cent/Local Equity condition were marginally more likely than participants in the 5 cent/Unaware condition to think their pay-rate was unreasonable, but no other differences were significant.

Overstatement scores were significantly higher in the 5 cent/Local Inequity condition than in the 5 cent/Unaware condition, and these differences were reflected in the supplementary measures. Participants in the 5 cent/Local Inequity condition were significantly more likely than participants in the 5 cent/Unaware condition to think about how much they would have earned if their pay-rate was higher and to think about the pay-rates of other participants in the room. Indeed, as Table 3 reveals, the 5 cent/Local Inequity condition was clearly the most likely to encourage participants to engage in (presumably aversive) social comparisons. Participants in the 5 cent/Local Inequity condition were also marginally more likely than participants in the 5 cent/Unaware condition to think that their pay-rate was unreasonable and unfair.

Within the 25 cent conditions, overstatement scores in neither the 25 cent/Local Equity condition nor the 25 cent/Local Inequity condition differed significantly from overstatement scores in the 25 cent/Unaware condition. Similarly, there were no significant differences across these conditions on the supplementary measures.

Our focal survey measures (Unreasonable Pay, Unfair Pay, Deserved More, Upward Counterfactuals, and Social Comparisons) did not mediate the effect of condition on overstatement scores. This is partly because the relationship between some of the survey measures (the presumed mediators) and overstatement scores (the dependent measure) differs by condition. For example, within the Local Inequity conditions, the correlation between Unreasonable Pay perceptions and overstatement scores differed significantly between the 5 cent and 25 cent conditions (.24 vs. −.33, $z = 1.99$, $p < .05$). In the 5 cent/Local Inequity condition, perceptions that 5 cents was unreasonable did not significantly influence overstatement scores ($r = .24$, $p = .24$). However, in the 25 cent/Local Inequity condition, perceptions that 25 cents was unreasonable

Table 3
Mean survey responses by condition (Experiment 2).

	Unreasonable Pay	Unfair Pay	Deserved More	Upward Counterfactuals	Social Comparisons
5 cents					
Unaware	3.07	2.70	3.27	3.27	2.77
Local Equity	3.58†	3.04	3.56	3.73	2.65
Local Inequity	3.53†	3.16†	3.38	3.91*	3.59**
25 cents					
Unaware	2.09	2.09	2.73	3.03	2.79
Local Equity	1.83	2.04	2.63	2.88	2.92
Local Inequity	2.00	2.46	2.85	2.73	2.85

Note: The response scale for each item ranged from 1 (strongly disagree) to 5 (strongly agree). Within the 5 cents conditions, significant differences from the Unaware condition are indicated by: $^{**}p < .01$, $^{*}p < .05$, $^{\dagger}p < .10$. Within the 25 cents conditions, there were no significant differences from the Unaware condition.

actually decreased overstatement scores ($r = -.33$, $p < .10$). Participants fortunate enough to earn 25 cents per point may have felt that their pay-rate was unreasonably high (given that other participants were earning 5 times less for equal work), and the guilt associated with that perception may have curbed cheating. In general, because the supplemental measures referred to different thoughts in different conditions (e.g., how one thinks about the pay-rate of others depends on whether one is aware of alternative pay-rates in the first place, and whether one is earning more or less than others), rather than feelings whose meaning is constant across conditions (e.g., "anger" or "guilt"), it was difficult to establish mediation. Finally, in hindsight, ambiguity inherent to some of the items (e.g., references to "the" payment in the Local Equity and Local Inequity conditions, in which participants were aware of two possible payments) could have added noise and reduced our ability to detect coherent relationships.

Discussion

Experiments 1 and 2 suggest that participants earning a low pay-rate are more likely to cheat than participants earning a high pay-rate only when there is awareness of alternative pay-rates. Experiment 2 disentangled two possible reasons why awareness might increase cheating among participants earning a low pay-rate. We found that mere awareness that one could have earned more (aversive counterfactuals alone) was not sufficient to significantly increase cheating among participants earning a low pay-rate. However, awareness that other participants were earning more (upward social comparisons) significantly increased cheating among participants earning a low pay-rate. Participants who knew that other participants in the same room were earning more were most likely to cheat and most likely to report that they were thinking about the earnings of other participants, but our focal supplemental survey measures (Unreasonable Pay, Unfair Pay, Deserved More, Upward Counterfactuals, and Social Comparisons) did not mediate the effect of condition on overstatement scores.

General discussion

Two experiments suggest that reduced monetary incentives can cause people to be *more* likely to cheat when they are aware that close others are earning more. Our research suggests low pay-rates may be unlikely in and of themselves to promote cheating; rather, it is the salience of upward social comparisons that appears to matter.

This research contributes to the mounting evidence highlighting how psychological factors can in some cases trump economic ones in predicting unethical behavior. For example, Edelman and Larkin (2009) examined academics' unethical downloading of their own SSRN papers to inflate their download counts (a performance metric in some disciplines). Consistent with social comparison theory, the download counts of peer scholars strongly predicted the propensity to inflate one's own download counts. By contrast, download inflation was not affected by the benefits of engaging in it – for example, it was no more common when subjects were under review for promotion.

Our work is consistent with Akerlof and Yellen's (1988) fair-wage hypothesis (workers withdraw effort proportionately as their actual wage falls short of their fair wage) and therefore has implications for the scope of activities and wages within organizations. Nickerson and Zenger (2008) argue that wide variance in wages within organizations creates "social comparison costs" (e.g., reduced effort, lack of cooperation) that can outweigh the benefits of better performance among the highly compensated. Such costs may drive organizations to reduce their diversity of activities and wages (e.g., by outsourcing activities that would require relatively low, or even high, compensation).

Similarly, Larkin, Pierce, and Gino (2012) argue that social comparison costs can help to explain why performance-based pay is observed less frequently than the standard economic perspective (agency theory) would predict. We build on this theoretical work by showing that salient differences in levels of performance-based pay, holding the task constant, can impose important social comparison costs (namely, increased cheating). Consistency in performance-based pay-rates among employees who do the same job may be required to prevent the kind of employee dishonesty that arises from upward social comparisons.

When performance-based pay discrepancies must exist, organizations would likely benefit from keeping those discrepancies as hidden as possible. Social comparison theory suggests that people most readily compare themselves to similar others (Festinger, 1954; Wheeler, Koestner, & Driver, 1982), suggesting that pay differences will be especially salient, and particularly likely to lead to socially destructive behavior, when people in similar positions receive different levels of remuneration.

In our research, low earners did not know which individuals were earning higher pay-rates, but in many real world settings the identities of high earners is common knowledge (e.g., in public universities that publish faculty salaries). Prior research has shown that people are more vindictive toward identified perpetrators than statistical perpetrators (Small & Loewenstein, 2005). A natural extension of that finding, which could be addressed in future research, is the prediction that people respond more negatively to the awareness that identified specific others are receiving a higher pay-rate than to the awareness that other, unidentified, individuals are earning more – for example, if in the Local Inequity condition of Experiment 2, participants wore buttons indicating their pay-rate.

Consistent with research on procedural justice (Lind & Tyler, 1988), Greenberg (1990, 1993) has found that the theft-encouraging effect of pay inequity is mitigated when employees are given a rationale for the reduction and treated compassionately. In the present studies, the pay-rate was determined randomly, through a coin flip, and no rationale was given for the difference in pay-rate

between participants. Future research could explore whether the effect of awareness of alternative pay-rates is moderated by the manner in which the different pay-rates are determined. For example, determining pay-rates based on prior performance may make low earners less likely to cheat, because there is an ostensible reason for their low payment. Gino and Pierce (2010a), however, did not find that providing a rationale for wealth disparities moderated the effect of wealth-based inequity on dishonest helping and hurting.

We have focused on employee dishonesty, and in particular on the unethical behavior of wage-based earners. But our results may apply to a variety of other settings. For one, they could help to explain what many people find mysterious: why wealthy people cheat for seemingly trivial gains. Our results suggest that when wealthy people can easily compare themselves to other similar individuals who earn even more, they too may be prone to cheating. Nonetheless, the present research documents the relative importance of social comparisons in determining cheating only at relatively low incentive levels – future research should verify that social comparisons and pay-rates interact to influence cheating when pay-rates are high (for example, if the pay-rates in our experiment were increased 10-fold). It is possible that at high pay levels, the relative importance of social comparisons becomes muted.

Future research could also develop further insight into the psychological processes underlying the behavioral patterns observed here. Our work demonstrated that aversive counterfactuals alone (the Local Equity condition in Experiment 2) are not sufficient to reverse the relationship between pay-rate and cheating. However, the supplemental measures collected at the end of our experiments did not mediate the relationship between condition and cheating. Future research could more precisely measure participants' attention to others' pay-rates (e.g., by using Mouselab (Johnson, Payne, Schkade, & Bettman, 1989) to measure when and how long participants look at others' pay-rates) to gain more insight into how social comparison influences cheating.

Conclusion

This research can potentially help to explain a seemingly diverse set of unethical activities, from insider trading and bad mortgage practices within industry, to questionable research practices within academia. In each of these cases, the rewards to cheating – be they ill-gotten financial payoffs or undeserved academic credit – provide the initial impetus for behaving badly (cf. Bennett, Pierce, Snyder, & Toffel, 2013; Shleifer, 2004; Snyder, 2010). Then, once outcomes (e.g., profit, academic performance) become public, social comparison processes serve to make similar others feel unfairly disadvantaged. These feelings may produce the impression that cheating is economically necessary for survival, which provides a ready rationalization for engaging in and sustaining unethical behavior. Exacerbating this vicious cycle is the fact that individuals quickly adapt to their newly ill-gotten gains, meaning that loss aversion further encourages cheating. Thus, both social comparisons and economic imperatives create a self-reinforcing downward spiral – a 'race to the bottom' in which dishonesty begets dishonesty.

Appendix A. List of supplementary survey measures, in order of administration

A.1. Experiment 1

- I thought the payment was reasonable. (reverse-scored) [referred to as Unreasonable Pay]
- I thought the payment was unfair. [Unfair Pay]
- I thought I deserved a higher payment. [Deserved More]
- I thought about how much I would have earned had the payment been higher. [Upward Counterfactuals]
- Demographic questions.

A.2. Experiment 2

- I thought the payment was reasonable. (reverse-scored) [Unreasonable Pay]
- I thought the payment was unfair. [Unfair Pay]
- I thought I deserved a higher payment. [Deserved More]
- I thought about how much I would have earned if the pay-rate was higher. [Upward Counterfactuals]
- I thought about the payment that other participants in this room earned. [Social Comparisons]
- How much effort did you devote to performing well on the task?
- Absolute and relative morality questions (Mazar et al., 2008).
- Attention check.
  - What was the pay-rate for the other participants in this room? (25 cents per point / 5 cents per point / some participants received 25 cents per point; other participants received 5 cents per point).
- Affect Intensity Scale (Larsen, 1987).
- Demographic questions.
- Have you participated in this, or a similar experiment before?

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