Cheating and Self-Confidence*

Nathan R. Adams[†]
Michael A. Kuhn[‡]
Glen R. Waddell[§]

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Abstract

Self-confidence has both instrumental and intrinsic value. Accurate beliefs about ability are important for decision-making, but this can be in tension with strongly-held preferences over those beliefs. We study this tension when maintaining accurate beliefs requires acknowledging dishonesty: do rewards obtained from cheating at a task influence self-assessed ability at that task? Our novel experiment allows us to estimate the relationship between cheating and both stated and revealed confidence at the individual level. We find that cheaters both state higher confidence in their future performance and wager substantially more on their future performance. Given the recent focus on the role of gender differences in confidence in the labor market, and the extensive literature on gender differences in dishonesty, we also consider whether there are important gender differences in whether confidence responds to success through cheating. Indeed, the relationship is stronger for men. This could be because the average overconfident man and woman are different—men substitute complete cheating for effort, whereas women complement effort with marginal cheating, as a nudge over the finish line. Our findings have implications for managerial decision making, and expand the limited literature on the follow-on effects of engaging in dishonest behavior.

Keywords: overconfidence; cheating; beliefs; experiment

JEL Classifications: J24, M12, D83

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†Washington State Institute for Public Policy, nathan.adams@wsipp.wa.gov

‡University of Oregon and CEGA mkuhn@uoregon.edu

§University of Oregon and IZA waddell@uoregon.edu

1 Introduction

What determines whether an employee puts her name forward for an important assignment, requests a promotion, or seeks an outside offer to obtain a raise? Are those the same factors that determine "softer" self-promotion embedded in language, attitude, and casual statements? In a modern workplace with team production, difficult-to-monitor inputs, and difficult-to-quantify outputs, managers often rely on employee self-promotion and initiative to make important decisions. If self-promotion is effective, and if the willingness to engage in it is influenced by factors beyond rational expectations of ability, then workers, managers, and policy-makers alike should be concerned about the potential for inequity and inefficiency in human-resource decisions. Recent work by Exley and Kessler (2019) demonstrates both of these criteria: they find that self-promotion is effective in a strategic setting, and that the willingness to do it is strongly influenced by gender.

While there is much existing work on the willingness to compete and negotiate with beliefs about ability held fixed, in this paper we focus specifically on the formation of expectations about ability.[1] Workers face many of the same informational challenges as managers when estimating their own ability—we refer to this estimation as "confidence"—and even some extra ones associated with motivated reasoning. For example, the good news-bad news effect (Eil and Rao, 2011), attribution bias (Haggag et al., 2018), correlation neglect (Enke and Zimmerman, 2018), selection neglect (Enke, 2018), and outcome bias (Baron and Hershey, 1988) each pose potential inference problems for workers mapping outcomes to beliefs. In this paper we consider the link between dishonesty and confidence—inquiring into the systematic nature through which cheating or dishonest revelation can influence either stated beliefs of ability or costly signals of ability.

Consider two employees who report having achieved the same level of productivity, with Employee A having done so honestly, but Employee B having inflated his productivity. Although they might both receive performance pay for their work, they subsequently face different signal-extraction problems in evaluating their own ability. While Employee A can attribute performance pay to her productivity quite easily, Employee B must consider that his pay may have been induced at the margin by dishonesty. If Employee B fails to do so, he will form a biased self assessment. In this way, we imagine a role for dishonesty in the endogenous development of *overconfidence*.[2]

We also examine the role that gender plays in self-assessed ability, and how managers and

[1] See Niederle and Vesterlund (2011) for a review.

[2] Heger et al. (2022) also consider the relationship between confidence and dishonesty, but in the opposite direction, finding that a lack of confidence about whether something is true creates wiggle-room for dishonesty.

outsiders should evaluate measures of confidence across gender. Three recent studies combine to suggest that—especially in occupations that are stereotypically male– -we should be particularly concerned about the development of unjustified gender differences in confidence; Bordalo et al. (2019) show that domain-specific gender stereotypes about and performance gaps in ability are important determinants of women's beliefs, Coffman et al. (2021a) show that gender stereotypes affect how strongly ability beliefs are updated in response to news, and Coffman et al. (2021b) show that women's ability beliefs react more negatively and for a longer period of time to bad feedback than men's. This is closely related to the literatures on the gender gap in willingness to negotiate (see, for example, Small et al., 2007), compete (see, for example, Niederle and Vesterlund, 2007), self-promote (Exley and Kessler, 2019), speak up (Gallus and Heikensten, 2019), and take credit (Isaksson, 2018). Descriptive evidence of a "confidence gap" typically comes in the form of survey responses. For example, a 2011 survey of British managers finds half of women managers admitting to feelings of self-doubt about their job performance and careers, while fewer than one-third of men admit to the same.[3] A review of personnel records at Hewlett-Packard found that women applied for a promotion only when they believed they met 100 percent of the qualifications listed for the job, while men were happy to apply when they thought they could meet 60 percent of the job requirements; "Underqualified and underprepared men don't think twice about leaning in. Overqualified and overprepared, too many women still hold back. Women feel confident only when they are perfect."[4]

Recent work suggests that overconfidence may play a larger role in these gender differences than previously thought. Veldhuizen (2017) finds that 48 percent of the competition gap can be explained by differences in confidence, and Kuhn and Villeval (2015) finds that underconfidence in women drives greater selection into team production.[5] Eckel and Füllbrunn (2015) finds that experimental asset markets with women traders feature smaller speculative bubbles.[6] Patterns such as these suggest that confidence differentials have real and lasting effects. Could male overconfidence come in part from differences in dishonest behavior? Dreber and Johannesson (2008), Erat and Gneezy (2012), Dato and Nieken (2014), and Kajackaite and Gneezy (2017) all find that men lie more than women, so long as they can personally benefit from the lie. Immoral behavior is often risky, of course, and prior research finds that women are more risk-averse than men (Borghans, Golsteyn, Heckman and Meijers, 2009; Charness and Gneezy, 2012; Charness, Eckel, Gneezy and Kajackaite, 2018b, to name

[3] Source: "*Ambition and Gender at Work*," Institute of Leadership & Management, 2014.

[4] Source: "*The Confidence Gap*," The Atlantic, May 2014.

[5] Veldhuizen (2017) also suggests that an additional 37 percent of the gap can be explained by the interaction between confidence and risk preferences.

[6] See Footnote 2 of Eckel and Füllbrunn (2015) for a broader review of gender and confidence in finance.

a few, while Byrnes, Miller and Schafer, 1999, Eckel and Grossman, 2008, and Croson and Gneezy, 2009 offer reviews). Thus, we anticipate that dishonesty may well be an important mechanism through which overconfidence propagates in men.

We report the results of two online experiments designed to measure the relationship between cheating and confidence. In both, subjects perform a hidden-object search task and then report their performance. In Experiment 1 (and for some subjects in Experiment 2) subjects are rewarded when they report finding more objects from a list than are actually pictured. We do not take this decision to put subjects into an impossible task lightly. When success is possible either with or without cheating, it is difficult to measure whether a specific instance of success is due to cheating. Consider common experimental designs in the real effort cheating literature like taking a test with access to an answer key or self-reporting the number of tasks completed. In these paradigms, identifying a specific instance of cheating would require data these studies do not collect: a subject's actual performance. If subjects are aware that their actual performance is observable, it compromises the externally valid act of "cheating" in the study. Without the subject's knowledge, they would be misled into publicly engaging in behavior they thought was private. Additionally, in an experiment that allows for individually identifiable success either with or without cheating within the same task, *legitimate* confidence is a confounding variable. Thus, we use an impossible task to ensure that additional monetary reward is only possible through cheating, leaving ambiguous whether all objects in the search list appear in the picture. An added benefit of this design is that it provides an excuse for cheating behavior that is unrelated to performance; if subjects suspect that an object is missing, they can label the task as unfair. With this excuse, it should be less costly to acknowledge and condition on past cheating when forming beliefs. However, to preview out results, this is not at all what we find.

After the first search task, we follow Exley and Kessler (2019) in measuring confidence in two ways. In addition to identifying a gender gap in incentivized self-promotion, they identify a gender gap in underlying self-evaluations absent any strategic motive. For this reason, and because overconfidence in the workplace can manifest in any number of ways, we analyze how both "stated" and "revealed" confidence depend on dishonest behavior. Stated confidence is meant to represent the beliefs that underlie workplace attitudes, involvement in discussions, sense of belonging, cheap talk and other "soft" factors that inevitably influence how individuals are viewed by others. Revealed confidence is meant to represent the willingness to act based on those beliefs in a situation where there exists a real risk of failure.

Revealed confidence is measured with a Gneezy and Potters (1997) style instrument; subjects can invest in their own future performance on a modified version of the initial task where it is clear that cheating is not possible. To create a situation where *accurate* beliefs have instrumental value that is in stark tension with their intrinsic value, we elicit revealed confidence in a situation where *any confidence is costly overconfidence*.[7] The only way to succeed in our revealed preference task is to avoid it, but in order for cheaters to accept that avoiding it is the best thing to do, they have to confront the fact that their past success was achieved dishonestly. We tell subjects that this task will be "similar in difficulty to the previous task" to suggest, without stating outright, that honestly succeeding at the task is either difficult or impossible. After this task, we measure stated confidence with an unincentivized self-assessment: "How well do you believe you would perform on similar hidden object tasks in the future?"

We offer four main findings from Experiment 1:

Finding 1: *Roughly half of subjects cheat, making this task well-suited for studying differences in the subsequent behavior of cheaters and non-cheaters.*

Finding 2: *Men and women are equally likely to cheat overall, but men are more likely to cheat as much as possible, and without exerting any real effort, than women.*

Finding 3: *Cheaters reveal (state) over one-third (nearly two-thirds) of a standard deviation higher confidence in their future performance than non-cheaters.*

Finding 4: *The relationship between cheating and revealed (stated) confidence is 157 percent (48 percent) larger for men than women.*

Finding 2 is of particular interest for productivity because we find that marginal cheating typically complements real effort, whereas full cheating substitutes for real effort. This suggests differences in what motivates men and women to cheat. Putting Findings 3 and 4 together, we see that while the belief distortion from cheating is larger for stated confidence, the gender gap is smaller. On net, we think gender differences in how both types of confidence relate to past dishonesty are likely important. An important corollary to Finding 4 is that a manager who observes only employee confidence should hold different beliefs about the level of dishonesty and productivity of highly-confident men and women. While 31 percent (40 percent) of revealed (stated) highly confident men are unproductive full cheaters, only 21 percent (23 percent) of revealed (stated) highly confident women are in that category. And, while another 31 percent of both revealed and stated highly confident men are marginal

[7] This also ensures that overconfidence has no strategic or self-motivational value, as suggested by Charness et al. (2018a), Schwardmann and van der Weele (2019), and Soldá et al. (2019).

cheaters who tend to also be productive, 42 percent (43 percent) of revealed (stated) highly confident women are in that category. In our study, selecting highly confident individuals for promotion or important tasks will yield cheaters at roughly the same rate by gender, but the women will be less dishonest and more productive on average.

While these associations between cheating and confidence are relevant on their own for managerial decision-making and explaining observed workplace outcomes, we also try to explore the *causal impact* of cheating. In Experiment 1, we randomly varied the *ex-ante* incentive to cheat in order to construct an instrumental variable (IV) for cheating. Our IV point estimates of the effect of cheating on confidence are generally much larger than our point estimates of the association, suggesting a causal relationship. However, the estimates are noisy due to an imprecise first stage.

We designed Experiment 2 to explore a particular causal pathway for cheating to influence confidence: are the rewards from success through cheating erroneously conflated with ability in a way that rewards from success without cheating are not? Holding fixed the *ex-ante expected* reward from success, we randomly varied the *ex-post* reward from success with cheating, and compare it to identical variation in the *ex-post* reward from success without cheating in a difference-in-differences experimental design. While the introduction of a control group creates a link between legitimate performance and confidence –which is why we avoid doing so in Experiment 1– this level effect should drop out in comparing the effect of rewards within each group. Note that this design allows individuals to misattribute the random portion of their reward to their ability even when they can succeed honestly; what we test is whether cheating adds a dimension of ambiguity to success that allows for *greater* misattribution. The main finding from Experiment 2 is that we identify little consistent misattribution of ex-post random rewards to ability. While we estimate that cheaters' stated confidence is increasing in ex-post random rewards, and that women who cheat *reduce* their revealed confidence when the reward is higher, the pattern of results is not consistent enough across our outcomes and samples to explain the clear and consistent association between cheating and confidence observed in Experiment 1. A general misattribution of success to ability rather than dishonesty is more likely.

Closely related work by Ward (2021) shows that higher levels of stated confidence exist among subjects who took a general-knowledge exam with the aid of internet search than subjects who did not. Similar to our revealed confidence task, the author informs subjects that they will not have access to internet search on a future version of the test, yet the higher confidence among those with access in the first task remains.[8] We think this complements

[8] Related, Ward et al. (2022) finds that those who share online content without reading it report higher subjective knowledge of the subject.

our work nicely by illustrating in another context that it is very hard for people to properly condition on inputs when inferring their self-confidence from output. Our focus on dishonesty adds another dimension to this problem: value judgment over the ability-independent input to success. In other words, misattribution is more than a cognitive error in our study as in Haggag et al. (2018) on attribution bias, rather it is self-deception as in Eil and Rao (2011) on the good-news bad-news effect. Our conceptual framework is closely related to Bénabou and Tirole (2011); exhibiting confidence is a (potentially) costly identity investment that protects against the threat of a past transgression to that identity. This creates a pathway for individual heterogeneity (in either identity utility or the threat that cheating poses to it) to influence self-confidence.

Our paper also contributes to an under-developed literature on the effects of dishonesty on subsequent behavior; while the determinants of cheating are well-studied in experimental economics, the behavioral consequences of cheating have, for the most part, been overlooked.[9] We can identify two other papers in the economics literature that examine the impact of cheating and/or dishonesty on subsequent behavior, both of which are experimental. Dishonesty in one participant induces more dishonesty in others (Innes and Mitra, 2013), and immoral behavior leads to feelings of guilt and increased charitable donations (Gneezy et al., 2014). The psychology literature has considered the emotional consequences of unethical behavior, but is focused on causing harm rather than cheating or dishonesty. Ruedy et al. (2013) both reviews this literature and demonstrates a link between cheating and subsequent *positive* affect. We hope that by highlighting the relationship to gender differences in the workplace, the need for work on the behavioral impacts of cheating on subsequent behavior is clear.

[9] Becker (1968) is among the earliest studies on the determinants of cheating, and forms the view that people will engage in dishonest behavior when the benefits of that behavior outweigh the costs. This precipitated literatures on the pecuniary and probabilistic determinants of cheating in the workplace: executive-compensation packages (Burns and Kedia, 2006; Efendi et al., 2007), competition and tournament incentives (Jacob and Levitt, 2003; Gilpatric, 2011; Berentsen, 2002; Kräkel, 2007; Schwieren and Weichselbaumer, 2010; Bunn et al., 1992; Charness et al., 2014), decreased deterrence (Curry and Mongrain, 2009), decreased monitoring (Nagin et al., 2002; Kerkvliet and Sigmund, 1999), team environments (Conrads et al., 2013), and productivity (Gill et al., 2013). Recent experimental work suggests that cheating may be a function of individual character—more responsive to across-individual variation in social- and self-image than to within-individual variation in motive, means, and opportunity (Mazar et al., 2008; Fischbacher and Föllmi-Heusi, 2013; Weisel and Shalvi, 2015; Kajackaite and Gneezy, 2017; Gneezy et al., 2018). Kajackaite (2018) finds that lying about random events is less costly than lying about productivity. See Abeler et al. (2019) for a broad review.

2 Experiment 1

2.1 Design

We used Amazon's Mechanical Turk (MTurk) for our study—in doing so, we expect the social distance and monitoring difficulty inherent in interacting remotely and anonymously to be conducive to cheating. The study is built around subjects' performance on a hidden-object task. Participants are shown a picture (see Page 4 of the instructions reproduced in the appendix) and asked to find items from a list of twelve potential objects. They are told that if they find nine of the twelve objects within the five minutes they are given, they will earn a reward. Likewise, they are told that if they find all twelve objects they will earn an even larger reward. However, only eight of the twelve objects on the list appear in the picture. We call these "correct" objects. Subjects self-report whether they find each object and can thereby misrepresent their true performance on the task. Below the picture, subjects can check off each object, allowing us to track which objects they reported finding.[10] We can therefore distinguish cheaters who "barely" cheated by nudging themselves over the payment threshold (i.e., from eight to nine) from cheaters who report finding several incorrect objects. Subjects can advance from the picture screen at any time during the five-minute search period. We refer to this as Task 1.

574 subjects completed the experiment, 350 (61.0 percent) of whom reported finding the eight correct objects in the picture. The reward for "finding" nine objects is randomly assigned from {\$0.10, \$0.50, \$1.50, or \$2.75}, but always made known to the subject *prior* to performing the task (this prior knowledge of reward value is the key difference between Experiment 1 and Experiment 2). We vary the reward in order to create an instrumental variable for cheating, which we discuss in depth in Section 2.2.3.[11]

After Task 1, subjects move on to a second object search task, Task 2. There are a number of important differences between Task 1 and Task 2. First, subjects are told that the new picture (see Page 12 of the instructions reproduced in the Appendix) in Task 2 will be overlaid with a grid and they will have to report the grid location of each object that they find. In this way, we signal to participants that cheating is not possible on this task. Second, subjects learn that they will not be able to advance past Task 2 before time expires. Third, subjects learn that all earnings from the Task 2 are given to the Make-a-Wish® Foundation. We separate Task 2 from personal financial gain for two reasons: 1) to limit the impact of any Task 1 income effects on subsequent investment choices, and 2) to impose external costs

[10] The order of the check-boxes was randomized across subjects.

[11] The additional reward for "finding" twelve objects is \$0.40 when the reward for nine objects is \$0.10, \$0.50 when the reward for nine is \$0.50 and \$1.50, and \$2.25 when the reward for nine is \$2.75.

of self-deception, as would exist in a firm setting.[12]

We these rules of Task 2 established, we describe a Gneezy and Potters (1997) style instrument in which subjects can invest the charity's money in their own performance on Task 2. Each participant is given a donation endowment of $2 that they can invest in any cent-increment. If they are successful (again defined as finding nine of twelve objects) their investment is tripled and given to the charity. If they are unsuccessful their investment is lost. Whatever they don't invest is also given to the charity. Assuming that subjects would like to maximize their expected donation, their investment is a revealed measure of their confidence (holding risk preference fixed). It is important to understand that because both the endowment and the investment go to the charity, differences in how much subjects like the charity, or want to behave pro-socially (conditional on a strictly positive weight on the charity) do not affect the optimal choice. Prior to investment, subjects are informed that "You will succeed at the task if you find at least 9 out of 12 objects. The task is similar in difficulty to the previous task."

Mean investment is $1.07 (S.D. = $0.75), and we use standardized investment as our "revealed confidence" measure. All subjects have to answer three comprehension questions correctly before proceeding past the description of Task 2 to ensure that they understand the investment structure. Importantly, the Gneezy and Potters (1997) task is designed to elicit risk preference and, as such, our subjects' investment should also be related to risk preference. Therefore, we elicit a stated-risk preference and use it as a control variable.[13] Following Task 2, subjects complete a brief survey.[14] In the survey, we ask subjects to provide a statement of their confidence: "How well do you believe you would perform on similar hidden object tasks in the future? Please choose a value from 0 to 10." Mean stated confidence is 6.16 (S.D. = 2.01).

2.2 Results

We break the results from Experiment 1 into three sections. First, we examine the frequency and nature of cheating. Second, we examine the relationship between cheating and confidence. Third, we exploit the varying financial incentive to cheat in the first task as an instrumental

[12] We rely on warm-glow for the personal cost of cheating, and note that if anything, cheaters should be more motivated to behave pro-socially as shown in Gneezy et al. (2014). We verify using Experiment 2 data that the same result holds when Task 2 is for personal financial gain.

[13] Subjects respond to, "In general, are you a person who likes to take risks or do you try and avoid taking risks? Please choose a value from 0 to 10." In this query, zero corresponds to "I am not at-all willing to take risks," and ten corresponds to "I am very willing to take risks." The average response is 4.74 (S.D. = 2.48), and we use standardized risk preference as a control variable. Charness et al. (2013) highlight advantages of simple, un-incentivized risk elicitation measures.

[14] Pages 14-17 of the instructions in the Appendix.

variable for the impact of cheating on confidence.

2.2.1 Prevalence of Cheating

We find frequent cheating in our MTurk study. Even though there were only eight correct objects, 48.8 percent of subjects reported finding more. Subjects also responded to the incentive to cheat fully, with 18.8 percent claiming to have found all objects. The distribution of reports among cheaters is bimodal. In an environment where there were rewards for nine and for twelve objects to be found, very few cheaters reported finding ten or eleven objects. This distinction, within the set of those who cheated, increases our own confidence that our study exhibited its intended moral framework. Clearly, if the difference between cheaters and non-cheaters was purely a realization of the ability to cheat, we would expect to see no reports of nine objects. Conditional on reporting all eight correct objects, cheating rates are much higher—of subjects who reported the eight correct objects, 73.1 percent reported finding more than eight objects. Throughout our analysis we will show the effect of either controlling for or conditioning on the number of corrector objects reported. The vast majority (89.3 percent) of subjects who report fewer than eight correct objects did not cheat. By removing or adjusting for this group, we avoid attributing a legitimate positive effect of performance on confidence to an effect of cheating on confidence.[15]

The partition of the sample into cheaters and non-cheaters is endogenous, not unlike how selection into cheating might unfold in the workplace. However, the focus of this study is specifically on cheating, and, for the moment, barring exogenous variation in cheating, we look for observable determinants of cheating that can mitigate omitted-variable bias when examining the relationship between cheating and confidence. We anticipated that risk preference, in particular, would be a strong predictor of cheating. In Column (1) of Table 1, we show estimates from a regression of an indicator variable for cheating on our measure of risk tolerance. We find that a standard-deviation increase in risk tolerance is associated with a 3.8-percentage-point increase in the likelihood of cheating. However, when we control for the number of real objects that subjects reported finding ("performance"), or restrict the sample to those who report all eight present objects, this relationship attenuates. In Column (2), we add performance, impulsivity, gender, employment status, and education as other personal characteristics that could correlate with cheating, as well as fixed effects

[15] We believe the number of present objects reported is determined prior to, or at the same time as cheating occurs. Either someone first finds all eight and then determines whether to cheat, or simply decides to check all 12 boxes, in which case there is complete co-linearity between cheating and performance. Thus, conditioning on or controlling for performance is unlikely to create an endogenous control problem.

for hour-of-day, day-of-week, week-of-month and month-of-year.[16] In so doing, the coefficient on risk is reduced by nearly 50 percent. In columns (3) and (4), we restrict the sample to subjects who reported finding the eight possible objects—the risk coefficient remains small and insignificant. Again, our prior is to appreciate this restriction, on the grounds that we are then relaxing our imposed linearity in controls, relying on essentially the "8/9" margin where we believe identification may be tightest.

Table 1: Determinants of Cheating

	Full sample		Performance = 8	
	(1)	(2)	(3)	(4)
Risk Tolerance (standardized)	0.038*	0.021	0.022	0.011
	(0.021)	(0.020)	(0.024)	(0.025)
Performance (0-8 objects)		0.160***		
		(0.019)		
Impulsivity (0-3 incorrect CRT)		-0.007		-0.005
		(0.020)		(0.025)
$\mathbb{1}$(Female)		0.004		-0.002
		(0.042)		(0.052)
$\mathbb{1}$(Employed)		0.113**		0.115*
		(0.047)		(0.066)
$\mathbb{1}$(College-educated)		0.063		0.106**
		(0.039)		(0.049)
Constant	0.489		0.731	
	(0.021)		(0.024)	
Time & Date FEs	N	Y	N	Y
Observations	574	574	350	350

Notes: *** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Robust standard errors are in parentheses. The dependent variable is an indicator for cheating, $\mathbb{1}$(Objects reported ≥ 9).

In Figure 1 we show the full distribution of the number of reported objects, separately by gender. When we consider the overall group of cheaters, we find no difference in the likelihood of cheating between men and women—48.3 percent of women cheat in our environment, and

[16] Impulsivity is measured using the standardized number of incorrect questions from the Cognitive Reflection Test (CRT, Frederick (2005)), which splits the sample into four groups: zero correct (22.7 percent), one correct (17.9 percent), two correct (20.2 percent), and three correct (39.2 percent). Employed is an indicator variable equal to one for those employed either full or part time (78.4 percent of the sample). College educated is an indicator variable equal to one for those who have obtained at least a bachelor's degree (50.4 percent of the sample).

49.3 percent of men cheat (difference: $p = 0.83$, two-tail t-test). However, men are more likely than women to report that they have found all twelve objects—23.2 percent of men, and only 14.9 percent of women report finding twelve (difference: $p = 0.01$, two-tail t-test). As individuals who report twelve have checked all the boxes, their stated performance need not inform their effort on the task at all. Indeed, individuals who cheat maximally spend an average of 48.3 seconds less on the task than individuals who cheat marginally ($p < 0.01$, two-tail t-test). In this sense, the implication would be that men are significantly more likely to *substitute* cheating for effort. On the other hand, women may be more likely to *complement* effort with cheating, wherein they first exert effort and then nudge themselves over the threshold (28.8 percent of women versus 22.8 percent of men reported finding all eight correct objects and cheated to nine, ten, or eleven, $p = 0.10$, two-tail t-test). Indeed, marginal cheaters spend significantly *more* time on the task than non-cheaters ($p = 0.02$, two-tail t-test).[17]

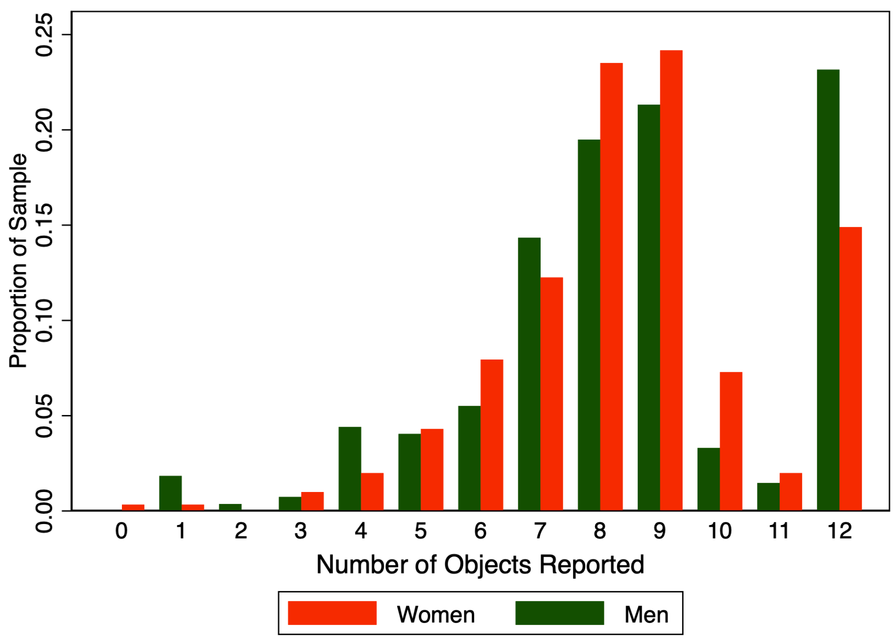

Figure 1: Objects Reported by Gender, Experiment 1

[17] 300 seconds is the time limit, and the average marginal cheater spends 279.5 seconds on the task, compared to 231.1 seconds for the average full cheater, and 265.5 seconds for the average non-cheater. Given that women are more likely to be marginal cheaters rather than full cheaters, this means that overall, women spend significantly more time on the task than men: 271.8 seconds vs. 253.7 seconds ($p < 0.01$, two-tail t-test).

2.2.2 Cheating and Confidence

First we present regression estimates of the average relationship between cheating and confidence. We standardize both revealed confidence through investment (mean = \$1.07, SD = \$0.75) and stated confidence (mean = 6.16, SD = 2.01) to use as our dependent variables. The explanatory variable of interest in an indicator variable for cheating (reporting more than eight objects found) in the first task. In columns (1)-(3) we show estimates of this relationship without any control variables or sample restrictions for men and women pooled, women only, and men only, respectively. In columns (4)-(6) we limit the sample to subjects that report all eight correct objects in Task 1 (performance = 8), where we believe our identification is not confounded by the legitimate effects of ability. This is our preferred set of estimates for considering the correlation between cheating and confidence. In columns (7)-(9) we maintain this sample restriction and add the set of control variables from Table 1 (excluding the female indicator variable), including the time and date fixed effects. These control variables move us closer to a causal interpretation of the cheating coefficient, although we note that there remains a potential for unobservable confounds. We return to this issue in Section 2.2.3. Columns (1), (4) and (7) show men and women together, columns (2), (5), and (8) show estimates specific to women, and columns (3), (6), and (9) show estimates specific to men.

Overall, we find a large and statistically significant positive correlation between cheating and both revealed and stated confidence. As expected, the estimates from the restricted sample in columns (4)-(6) are mostly smaller than the full-sample estimates in columns (1)-(3), suggesting that without this restriction our cheating coefficient may be capturing the legitimate positive correlation between finding more correct objects and confidence. The column (4) estimates show that cheating in the first task is associated with a 0.35 standard-deviation increase in revealed confidence through investment, and a 0.64 standard deviation increase in stated confidence. These estimates translate to a \$0.26 (25 percent) increase in investment in Task 2, and a 1.29 point (21 percent) increase in stated confidence. While the gender difference is not statistically significant at conventional levels ($p = 0.14$), this correlation is 2.6 times larger for men than women: for men, cheating in the first task is associated with a 0.52 standard-deviation increase in Task 2 investment ($p < 0.01$), but for women it is only associated with a 0.20 standard-deviation increase in Task 2 investment ($p = 0.18$). In fact, every single gender-specific estimate of this correlation is larger for men than women, with the difference ranging from 22 percent to 157 percent, although we can only reject the null hypothesis in the case of the full sample stated confidence estimates. The addition of control variables in columns (7)-(9) has very little impact on the estimates.[18]

[18] We obtain similar results if we use self-reported confidence in today's performance rather than future performance, or reported task difficulty as measures of stated confidence.

Table 2: Cheating and Confidence

	Full sample			Performance = 8					
	Pooled	Women	Men	Pooled	Women	Men	Pooled	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Revealed Confidence									
$\mathbb{1}$(Cheater)	0.377***	0.341***	0.417***	0.348***	0.203	0.521***	0.328***	0.249	0.445***
	(0.079)	(0.105)	(0.119)	(0.109)	(0.152)	(0.153)	(0.109)	(0.156)	(0.161)
H_0 : Women = Men		$\chi^2(1) = 0.23$			$\chi^2(1) = 2.18$			$\chi^2(1) = 0.88$	
		$p = 0.63$			$p = 0.14$			$p = 0.35$	
Panel B: Stated Confidence									
$\mathbb{1}$(Cheater)	0.775***	0.634***	0.931***	0.637***	0.519***	0.767***	0.633***	0.567***	0.714***
	(0.076)	(0.103)	(0.113)	(0.114)	(0.132)	(0.195)	(0.116)	(0.137)	(0.197)
H_0 : Women = Men		$\chi^2(1) = 3.82$			$\chi^2(1) = 1.12$			$\chi^2(1) = 0.44$	
($\mathbb{1}$(Cheater) coef.)		$p = 0.05$			$p = 0.29$			$p = 0.51$	
Time & Date FEs	N	N	N	N	N	N	Y	Y	Y
Control Variables	N	N	N	N	N	N	Y	Y	Y
Observations	574	302	272	350	183	167	350	183	167

Notes: *** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Robust standard errors are in parentheses. The dependent variable in Panel A is standardized investment in Task 2, and the dependent variable in Panel B is standardized stated confidence in future performance. The control variables in columns (7)-(9) include risk tolerance, impulsivity, employment status and whether someone is college educated. See Appendix Table A.1 for full model estimates.

Because we find substantial bunching at no investment, investing exactly half of the endowment and full investment, we also examine the relationship between cheating and the full distribution of investment. Figure 2, shows the cumulative distribution of investment by the decision to cheat in the sample restricted to those reporting eight correct objects. In Panel A, we show the investment variable itself, and in Panel B we use the residual investment from the regression specifications in columns (7)-(9) of Table 2 (excluding the cheating variable). The distributions for cheaters dominate the distributions for non-cheaters. A Kolmogorov-Smirnov test rejects the null hypothesis of equal distributions ($p < 0.01$ for both investment and residual investment). The same is true when we limit to sample to men ($p < 0.01$ for both investment and residual investment), but not with both measures when we limit the sample to women ($p = 0.50$ for investment and $p = 0.05$ for residual investment).

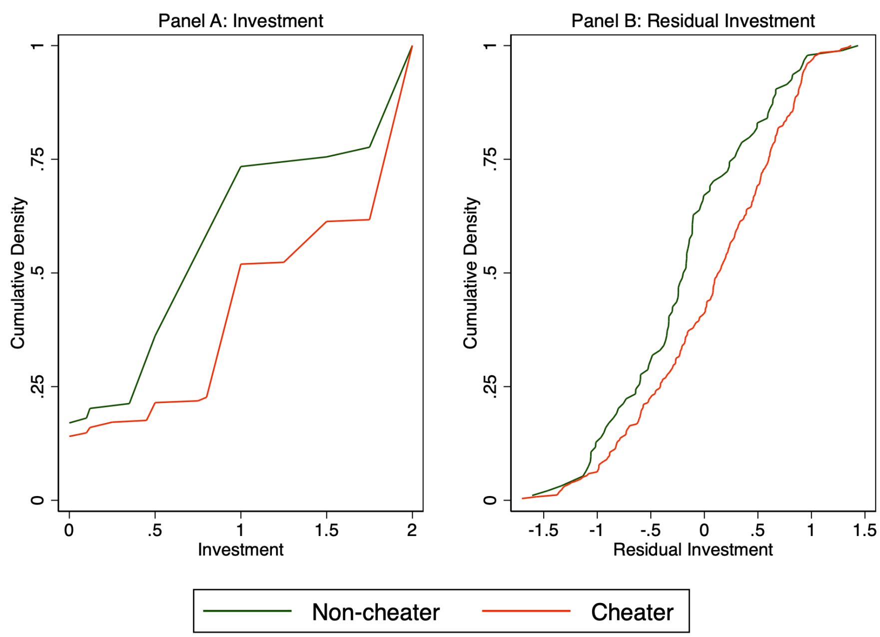

Figure 2: Distribution of Investment by Cheating, Experiment 1

What do these findings mean for a manager who observes only employee confidence (and gender), but not underlying productivity or honesty? In Table 3, we show the fraction of the full sample that cheated, separately by gender and confidence-assessment terciles. In Column (1), we show the fraction of marginal cheaters (reporting 9-to-11 objects) and in Column (2) we show the fraction of full cheaters (reporting all-twelve objects). The remaining fraction are non-cheaters. We find that the manager's inference should depend on employee gender when the employee's confidence assessment is high. Specifically, 37 percent of female workers who reveal that they are highly confident are non-cheaters, 42 percent are marginal cheaters, and 21 percent are maximum cheaters. These are similar in stated measures of confidence—

35 percent, 43 percent, and 23 percent, respectively. While a nearly identical fraction of highly revealed-confident men are non-cheaters (38 percent), fewer are marginal cheaters (31 percent, $p = 0.09$ relative to the fraction of women) and more are full cheaters (31 percent, $p = 0.12$ relative to the fraction of women). These gender differences are stronger for highly stated-confident men: 40 percent are full cheaters ($p < 0.01$ relative to the fraction of women) and 31 percent are marginal cheaters ($p = 0.05$ relative to the fraction of women. Thus, our study suggests that especially when making inference based on cheap talk, managers should be aware that reporting high levels of confidence may imply different things about the productivity and cheating behaviors of male versus female employees.

Table 3: Confidence and Propensity to Cheat

		Cheated to 9-11	Cheated to 12
		(1)	(2)
	Panel A: Revealed Confidence		
	Men ($N = 91$)	0.154	0.209
Low Confidence	Women ($N = 89$)	0.225	0.135
	Difference	$p = 0.22$	$p = 0.19$
	Men ($N = 73$)	0.329	0.151
Medium Confidence	Women ($N = 113$)	0.345	0.106
	Difference	$p = 0.82$	$p = 0.37$
	Men ($N = 108$)	0.306	0.306
High Confidence	Women ($N = 100$)	0.420	0.210
	Difference	$p = 0.09$	$p = 0.12$
	Panel B: Stated Confidence		
	Men ($N = 47$)	0.043	0.043
Low Confidence	Women ($N = 49$)	0.122	0.061
	Difference	$p = 0.16$	$p = 0.68$
	Men ($N = 93$)	0.301	0.086
Medium Confidence	Women ($N = 117$)	0.316	0.094
	Difference	$p = 0.81$	$p = 0.84$
	Men ($N = 132$)	0.311	0.402
High Confidence	Women ($N = 136$)	0.426	0.228
	Difference	$p = 0.05$	$p < 0.01$

Notes: We report the proportion of men and women within confidence terciles who cheated to 9-11 and who cheated to 12. Variables are standardized stated confidence in future performance and standardized investment in future performance.

2.2.3 Induced Cheating and Confidence

Within Experiment 1, we attempted to induce cheating by experimentally varying the marginal reward associated with cheating, and using the reward as an instrumental variable (IV). The literature is mixed on the degree to inducing cheating with monetary incentives is feasible in experiments (see Gneezy (2005) and Abeler et al. (2019)), as it can be difficult to identify the range of extrinsic incentives that will be marginal in the presence of intrinsic incentives. Indeed, it turns out that the relevance of our instrument is suspect. The only threat to IV validity that we perceive is through income. While income effects would be surprising over such a small range of earnings, this is why we elicited revealed confidence on behalf of a charity unaffected by the subject's Task 1 income. We also test directly for income effects in Experiment 2, and find no evidence thereof. We are confident in the validity of our instrument.

When we regress an indicator for cheating on the marginal incentive to cheat (report nine objects), we find a coefficient of 0.028 ($p = 0.17$, robust standard errors), suggesting that a $1-increase in the marginal incentive to cheat would only increase cheating by 2.8 percentage points.[19] While the instrument predicts cheating in the anticipated direction, the lack of precision suggests our IV estimates will be somewhat underpowered. With that caution in mind, in Table 4 we present the instrumental-variable estimates of the impact of cheating on both stated and revealed confidence, for the full sample, and then separately for men and women.

Every IV estimate is substantially larger than the corresponding OLS estimate from Table 2. Despite the weak instrument concerns, this suggests that the positive OLS estimates may be biased downwards relative to the true causal effect of cheating on confidence. This establishes a positive lower bound on this causal relationship. However, none of the IV estimates themselves are significantly different from zero, likely due to the poor first stage—especially for men. As such, we make no attempt to interpret these estimates beyond their ordinal relationship to those in the previous section. Over-identification tests using the marginal incentive to fully cheat support our claim to validity, but due to poor relevance, this test is under-powered.

[19] While there is some variation in the marginal incentive to fully cheat (report 12 objects) that is not co-linear with variation in the incentive to cheat at all (report 9 objects), full cheating is not our margin of interest here. We do use the marginal incentive to fully cheat in a limited fashion—to construct an over-identification test of the validity of our instruments—but not to generate our main estimates. We do not use control variables in the IV regressions (and thus they do not appear in the first stages) because the instrument is experimentally randomized, rather than conditionally random.

Table 4: Effect of Cheating on Confidence

	Revealed Confidence			Stated Confidence		
	Pooled	Women	Men	Pooled	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}$(Cheater)	1.604	1.688	1.198	2.241	2.073	2.528
	(1.513)	(1.503)	(3.688)	(1.641)	(1.658)	(4.284)
Constant	-0.762	-0.816	-0.546	-1.073	-1.004	-1.120
	(0.741)	(0.734)	(1.819)	(0.807)	(0.810)	(2.122)
First stage p value	0.17	0.17	0.59	0.17	0.17	0.59
Over-identification p value	0.58	0.59	0.97	0.79	0.61	0.99
Observations	574	574	302	302	272	272

Notes: Robust standard errors in parentheses. Dependent variables are standardized investment in future performance and standardized stated confidence in future performance.

3 Experiment 2

Experiment 1 leaves us with clear evidence that cheaters are overconfident in their abilities, suggestive evidence that this is causal, but without a precise causal mechanism. Specifically, because earning a bonus in Experiment 1 requires dishonesty, we cannot tell whether the erroneous attribution of success to ability operates through financial rewards. In other words, is it success itself that gives cheaters more confidence, or do cheaters infer their ability from their earnings in a way that successful non-cheaters do not? We designed Experiment 2 to rule in or out the financial rewards channel. Here, we randomize the *ex-post* rewards gained from Task 1 success. We do this both when success requires dishonesty as in Experiment 1—the "Impossible Treatment"—and when it does not—the "Possible Treatment." By differencing the effect of ex-post reward size across treatments, we can identify an effect of rewards on confidence that is specific to rewards obtained by cheating. In other words, does having cheated create extra cognitive wiggle room for random inputs to earnings to subsequently affect beliefs?

Experiment 2 plays another complementary role to Experiment 1 by allowing us to directly test for positive income effects on confidence in the Possible Treatment. If these exist, they could both confound our OLS estimates in Table 2 and violate the validity assumption of our IV estimates in Table 4.

3.1 Design

Experiment 2 is similar in design to Experiment 1, but differs in two key ways. First, subjects only know the *expected* value of cheating ($1.50) prior to Task 1. Specifically, we told subjects that, "The average bonus payment that people receive is $1.50, but it can be more or less." Subjects learn the realized reward only after the first task is performed (and the opportunity to cheat has passed). Second, we introduce a version of the task in which subjects can succeed at Task 1 without cheating. Any impact of the uncertain reward structure itself on performance and reporting behavior is differenced out using this design. As before, MTurk subjects are asked to find twelve objects in a picture, needing to find nine to earn the reward. In the impossible treatment, only eight of the listed objects are present, just as in Experiment 1. In the Possible Treatment, however, nine of the listed objects are actually present, and success is therefore possible without cheating. Subjects are randomized into one of these two treatments upon entry into the study.

We do not provide an incentive for finding all twelve objects in this version of the experiment. If a participant in either treatment reports finding nine or more objects, their realized reward is drawn randomly from the set {$0.25, $1.50, $2.75}, with equal probabilities. Following the first task, and the revelation of the reward, subjects participate in a verified investment task similar to that in Experiment 1. We make two changes to the investment task for Experiment 2. First, subjects participate for their own gain rather than for a charity. This is because the Possible treatment serves as a control group, and we do not need to rely on the charity task to guard against an income effect. Second, we increase the threshold for success in the investment task (Task 2) to ten of twelve objects. Subjects are told that finding objects will be of the same difficulty as in Task 1, but that "this time you will need to find at least 10 objects in order to succeed at the task." This was to avoid subjects in the Possible treatment being in a situation where they believed all they needed to do in Task 2 was repeat their Task 1 performance.

We identify the effect of rewards on confidence for cheaters using a difference-in-differences design. Our empirical specification is

$$\text{Confidence}_i = \alpha + \beta_1 \text{Bonus}_i + \beta_2 \mathbb{1}(\text{Impossible})_i + \beta_3 \text{Bonus}_i \times \mathbb{1}(\text{Impossible})_i + \epsilon_i \ . \quad (1)$$

The β_1 coefficient represents the causal effect of rewards among those who earn the bonus in the Possible treatment, and the β_3 coefficient represents the difference between that effect and the causal effect of rewards among those who earn the bonus in the Impossible treatment.[20]

A total of 598 subjects participated in Experiment 2, 379 in the Impossible Treatment, and 219 in the Possible Treatment. We oversampled the Impossible Treatment, anticipating a lower rate of bonus-earning when it required dishonesty. In the Impossible treatment, 41.7 percent cheated to obtain a reward with expected value \$1.50. This is slightly less than the fraction that cheated for a guaranteed reward of \$1.50 in Experiment 1 (48.8 percent). In the Possible Treatment, 71.2 percent of subjects earned a bonus, but a number of them did so by claiming to have found more than nine objects. Accounting for this, 60.7 percent of subjects reported exactly the nine correct objects and received a bonus.

We use three samples for our analysis of Experiment 2 data. For all, we limit the sample to individuals obtaining a bonus; because bonus size is randomized after it is earned, there are no selection concerns associated with narrowing our sample to this group of interest. Within this group, our first sample includes everyone receiving a bonus, such that we identify an exact causal effect of the treatment variation. This group includes subjects cheating in the Possible Treatment, and subjects cheating in the Impossible Treatment without having reported all of the correct objects. As in Experiment 1, our second sample removes everyone who does not report all of the correct objects, with the aim of removing any legitimate positive correlation between cheating and confidence. This removes 19 subjects in the Impossible Treatment who cheated without reporting all eight correct objects, and 28 subjects in the Possible Treatment who earned their bonus without reporting all nine correct objects. Our third sample then removes the 18 remaining cheaters in the Possible Treatment who reported extra objects in addition to the nine correct objects, thus making our treatment variation co-linear with cheating.

[20] An assumption underlying our interpretation is that these groups would react similarly to rewards obtained similarly. 88 percent of subjects earning a bonus in the Impossible treatment reported all correct objects; had they done so in the Possible treatment, they would have received a bonus without cheating, so their counterfactuals are strongly represented in the Possible treatment. On the other hand, only about half of the subjects who report all correct objects in the Impossible treatment cheat to earn a bonus. Thus, there are likely individuals receiving a bonus in the Possible treatment who would not receive one in the Impossible treatment. Do they react differently to rewards than others? We run a probit regression of cheating in the Impossible treatment on a variety of predictors—risk preference, impulsivity, gender, employment status, education, time of day, day of week, week of month, and month—and use the coefficients to predict the likelihood that non-cheating subjects receiving a bonus in the Possible treatment would cheat. We then regress stated and revealed confidence of those subjects on their bonus, their cheating likelihood, and the interaction. The interaction is never sizable or statistically significant: a ten percent increase in the likelihood of cheating predicts a 0.062 SD increase in investment, and 0.002 SD decrease in stated confidence ($p = 0.36$ and $p = 0.98$, respectively with robust standard errors).

3.2 Results

The relationship between cheating and confidence identified in Experiment 1 replicates, although it is smaller in magnitude: cheating in Experiment 2 is associated with a (within-treatment) 0.16 standard-deviation increase in revealed confidence through investment in Task 2 ($p = 0.07$), and a 0.32 standard-deviation increase in stated confidence ($p < 0.01$).

We present estimates of the model specified in equation 1 for revealed (Panel A) and stated (Panel B) confidence in Table 5. In columns (1) through (3), we use the sample of all subjects receiving a bonus. In columns (4)-(6) we remove all subjects that cheat without having found all of the correct objects, and in columns (7)-(9) we further remove all remaining cheaters in the Possible Treatment. Columns (1), (4) and (7) show men and women together, columns (2), (5), and (8) show estimates specific to women, and columns (3), (6), and (9) show estimates specific to men.

Considering β_1, the general effect of rewards, we find essentially no evidence that ex-post bonus size matters for confidence. The coefficient is not consistently positive or negative, and is marginally statistically significant twice (once positive, once negative). β_2, the general effect of the Impossible Treatment, is negative. This is as expected: succeeding honestly is better for confidence than succeeding dishonestly.[21]. Interestingly this estimate is never statistically significant (and is always smaller) for men, suggesting they may react to the different types of success more similarly. However, the gender difference between the coefficients is never statistically significant.

Our estimates of β_3 do not tell a consistent story. It is typically negative in the case of revealed confidence, and always positive in the case of stated confidence. For women who cheat, it may be the case that revealed confidence is *decreasing* in bonus size in a way that it is not for women who succeed; perhaps this is evidence of contrition from receiving larger ill-gotten gains leading to more accurate ability assessment. However, the size and precision of this result depends on the sample we use. It may also be the case that—for men in particular—stated confidence responds positively to bonus size in the Impossible Treatment only. This would be consistent with the financial rewards channel for attribution errors amongst cheaters, but again, the size and precision of this result depends on the sample we use. Overall, our estimates of β_3 do not suggest that this channel explains the consistent positive association between cheating and confidence in Experiment 1. More likely, the attribution error has to do with “success” itself.

[21] To be clear, this finding is different than our replication of Experiment 1 because it is about confidence conditional on success, whereas Experiment 1 is about success through cheating. Holding success fixed, justified success is, unsurprisingly, more important for confidence. The within-treatment association between cheating and confidence is positive in both treatments.

Table 5: Rewards and Confidence

	All Earning a Bonus			All Correct Objects Found			No Cheaters in Possible		
	Pooled	Women	Men	Pooled	Women	Men	Pooled	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Revealed Confidence									
Bonus - \$1.5	-0.070	0.048	-0.167	-0.049	0.107	-0.206*	-0.070	0.061	-0.193
	(0.067)	(0.081)	(0.107)	(0.071)	(0.088)	(0.109)	(0.076)	(0.091)	(0.119)
$\mathbb{1}$(Impossible)	-0.257**	-0.392***	-0.132	-0.259**	-0.334**	-0.210	-0.238**	-0.292**	-0.206
	(0.101)	(0.128)	(0.159)	(0.108)	(0.135)	(0.175)	(0.111)	(0.138)	(0.183)
(Bonus - \$1.5) $\times\mathbb{1}$(Impossible)	-0.030	-0.143	0.059	-0.090	-0.286**	0.114	-0.070	-0.240*	0.102
	(0.097)	(0.126)	(0.150)	(0.105)	(0.132)	(0.166)	(0.108)	(0.134)	(0.173)
Constant	0.229	0.348	0.126	0.236	0.299	0.188	0.214	0.258	0.184
	(0.070)	(0.087)	(0.111)	(0.075)	(0.094)	(0.118)	(0.080)	(0.099)	(0.129)
H_0 : Women = Men		$\chi^2(1) = 1.08$			$\chi^2(1) = 3.66$			$\chi^2(1) = 3.27$	
Bonus $\times\mathbb{1}$(Impossible) coef.		$p = 0.30$			$p = 0.06$			$p = 0.07$	
Panel B: Stated Confidence									
Bonus - \$1.5	0.066	0.148*	-0.043	0.043	0.076	0.000	0.011	0.062	-0.048
	(0.062)	(0.084)	(0.092)	(0.063)	(0.084)	(0.096)	(0.071)	(0.095)	(0.106)
$\mathbb{1}$(Impossible)	-0.228**	-0.333**	-0.129	-0.251**	-0.339**	-0.140	-0.202*	-0.278**	-0.108
	(0.097)	(0.129)	(0.144)	(0.100)	(0.246)	(0.148)	(0.104)	(0.139)	(0.153)
(Bonus - \$1.5) $\times\mathbb{1}$(Impossible)	0.175*	0.092	0.249*	0.145	0.090	0.176	0.177*	0.105	0.224
	(0.094)	(0.131)	(0.131)	(0.094)	(0.133)	(0.127)	(0.010)	(0.140)	(0.135)
Constant	0.248	0.198	0.335	0.296	0.261	0.345	0.247	0.200	0.312
	(0.063)	(0.082)	(0.096)	(0.065)	(0.085)	(0.104)	(0.071)	(0.093)	(0.111)
H_0 : Women = Men		$\chi^2(1) = 0.73$			$\chi^2(1) = 0.22$			$\chi^2(1) = 0.51$	
Bonus $\times\mathbb{1}$(Impossible) coef.		$p = 0.39$			$p = 0.64$			$p = 0.47$	
Observations	314	168	146	267	148	119	249	138	111

Notes: *** $\Rightarrow p < 0.01$, ** $\Rightarrow p < 0.05$, * $\Rightarrow p < 0.10$. Robust standard errors are in parentheses. The dependent variable in Panel A is standardized investment in Task 2, and the dependent variable in Panel B is standardized stated confidence in future performance. The sample in columns (4)-(6) (columns (7)-(9)) is constructed as a subset of the sample in columns (1)-(3) (columns (4)-(6)).

4 Discussion

Despite the prevalence of cheating, lying, and other forms of dishonest behavior in the workplace, we know little about the consequences of this behavior. In this paper, we examine the relationship between cheating and confidence. Using two novel experimental designs we link cheating to unjustified changes in both stated and revealed confidence. There is a strong positive relationship between cheating and willingness to invest in future performance, even when cheating is not possible in the future, and a cheap-talk measure of future ability. This is not explained by differences in risk preference, which suggests that cheaters are genuinely more confident in their future ability, and indeed this overconfidence is not benign: it proves costly in the investment task. The relationship holds in terms of both the average level of confidence people reveal, and in terms of the entire distribution of confidence.

We also identify potentially important gender differences in the relationship between cheating and confidence. For example, when we decompose cheating into full cheating and marginal cheating we find that men are more likely to cheat fully and women may be more likely to cheat marginally even though the overall cheating rates by gender are similar. We also find that the relationship between cheating and confidence is stronger for men than for women—by a margin that ranges from 22 percent to 157 percent across our measures of confidence—although we are only able to reject the null hypothesis of gender equality in one of our specifications. These two findings put together yield an important lesson for the hypothetical manager faced with reports or observations of employee confidence from our study: highly-confident men and women are different. They are both equally likely to be cheaters, but highly-confident women are more-productive cheaters in expectation. Putting more weight on signals of confidence from women would likely lead to less grossly-dishonest behavior and higher productivity. While our context is just an abstract rendering of this canonical principal-agent setting, our paper contributes to a broad experimental literature using such abstractions to try and explain why different workplace environments across occupations might lead to different gender employment, wage, and advancement gaps (e.g., Niederle and Vesterlund, 2007; Goldin, 2014; Babcock, Recalde, Vesterlund and Weingart, 2017).

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A Appendix for Online Publication

Table A.1: Cheating and Confidence - Full Model Results

	Revealed Confidence			Stated Confidence		
	Pooled	Women	Men	Pooled	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
𝟙(Cheater)	0.328***	0.249	0.445***	0.634***	0.567***	0.714***
	(0.109)	(0.156)	(0.161)	(0.116)	(0.137)	(0.197)
Risk Tolerance	0.299***	0.311***	0.331***	0.178***	0.157**	0.171**
	(0.050)	(0.076)	(0.073)	(0.047)	(0.069)	(0.079)
Impulsivity	-0.051	-0.092	-0.071	0.049	0.063	0.036
	(0.052)	(0.076)	(0.080)	(0.046)	(0.062)	(0.072)
𝟙(Employed)	-0.046	-0.018	-0.033	0.037	-0.063	0.202
	(0.117)	(0.141)	(0.224)	(0.107)	(0.135)	(0.174)
𝟙(College Educated)	-0.064	-0.291**	0.231	(0.091)	(0.125)	(0.143)
	(0.102)	(0.146)	(0.158)	(0.091)	(0.125)	(0.143)
Observations	350	183	167	350	183	167

Notes: *** ⇒ $p < 0.01$, ** ⇒ $p < 0.05$, * ⇒ $p < 0.10$. Robust standard errors are in parentheses. The dependent variable in columns (1)-(3) is standardized investment in Task 2, and the dependent variable in columns (4)-(6) is standardized stated confidence in future performance. All models feature time and date fixed effects, and are estimated on the sample of subjects reporting all eight correct objects. Columns (1)-(3) correspond to columns (7)-(9) in Table 2, Panel A, and columns (4)-(6) correspond to columns (7)-(9) in Table 2, Panel B.

A.1 Experiment Instructions

Page 1

Thank you for participating in today's study. Just for participating, you will receive a $1 thank-you payment. You will have several opportunities to earn extra money based on your performance on a some tasks.

Your choices, performance, and responses in this study will be kept anonymous. HITs will be approved based on completion, not on your choices or performance in the study.

Page 2 - Experiment 1

The task you will be doing is designed to test your attention and focus. Your task will be to find hidden objects in a picture, and the number you find will determine how much you earn. If you find 9 of the 12 objects, you will win an additional $Y. If you find all 12 objects, you will win an additional $Y.

To win the additional money, it doesn't matter which of the objects you find, so feel free to skip around if you get stuck.

Page 2 - Experiment 2

The task you will be doing is designed to test your attention and focus. Your task will be to find hidden objects in a picture, and the number you find will determine how much you earn. If you find at least 9 of the 12 objects, you will earn a bonus payment. The average bonus payout that people receive is $1.50, but it can be more or less.

To win the additional money, it doesn't matter which of the objects you find, so feel free to skip around if you get stuck.

Page 3 - Experiment 1

You will have 5 minutes to examine the picture. On the next page you will see a timer along with the picture and 12 checkboxes. Once you have completed the task you may advance, or wait until the 4 minutes are up.

Page 3 - Experiment 2

You will have 4 minutes to examine the picture. On the next page you will see a timer along with the picture and 12 checkboxes. Once you have completed the task you may advance, or wait until the 4 minutes are up.

Page 4

Please check each object as you find it. Remember, you must find at least 9 objects to earn a bonus payment.

- ☐ A horse
- ☐ An orange crayon
- ☐ A dinosaur
- ☐ A striped candy cane
- ☐ A yo-yo
- ☐ A banana
- ☐ An alligator
- ☐ A rolling pin
- ☐ A gun
- ☐ A green car
- ☐ A sailboat
- ☐ A black and yellow flag

Page 4 - Experiment 2, Possible

Please check each object as you find it. Remember, you must find at least 9 objects to earn a bonus payment.

- ☐ A banana
- ☐ A dinosaur
- ☐ An orange crayon
- ☐ A black and yellow flag
- ☐ An alligator
- ☐ A sailboat
- ☐ A horse
- ☐ A yo-yo
- ☐ A gun
- ☐ A rolling pin
- ☐ A golf tee
- ☐ A green car

Page 5

You found X objects. Click ‘yes’ to confirm or ‘no’ to change your answer.

- Yes
- No

Page 5a - If no on p.5

Enter the number of objects you found into the box below:

Page 6 - If # objects < 9

You did not find enough objects to win a bonus payment.

You will now proceed to the next task.

Page 6 - If # objects > 8

Congratulations, because you found at least 9 objects, you earned a $Y bonus payment!

These earnings are yours regardless of how you do on the next task.

You will now proceed to the next task.

Page 6 - If # objects =12, Experiment 1

Congratulations, because you found all least 12 objects, you earned a $Z bonus payment!

These earnings are yours regardless of how you do on the next task.

You will now proceed to the next task.

Page 7 - Experiment 1

We will now move on to the second part of the study. You will participate in another task of the same difficulty designed to test your attention and focus, but with a different picture and objects. There will again be 12 objects total.

You will also be working on behalf of a charity: the Make-A-Wish Foundation.

The Make-A-Wish Foundation grants the wishes of children with life-threatening medical conditions. It has been rated as highly effective by the Charity Navigator.

Page 7a - Experiment 1

First, we would like to ask you about your experience with the Make-A-Wish Foundation.

Have you heard of the Make-A-Wish foundation before today?

- Yes
- No

If you have heard of the Make-A-Wish foundation, is it a charitable organization that you trust?

- Yes
- No
- N/A

If you have heard of the Make-A-Wish foundation, have you ever donated money to it?

- Yes
- No
- N/A

Page 7 - Experiment 2

We will now move on to the second part of the study. You will participate in another task of the same difficulty designed to test your attention and focus, but with a different picture and objects. There will again be 12 objects total, but this time you will need to find at least 10 objects in order to succeed at the task.

This time, the amount you can earn depends on how much you choose to “invest" in your performance on the objects task. More detail will be provided on the next page.

Page 8 - Experiment 1

You have a charity fund of $2. You can choose how much of this fund to invest in your performance. **Whatever you DO NOT invest will be given to the charity as is. Whatever you DO invest will be TRIPLED and given to the charity if you succeed at the task. If you do not succeed at the task, your investment is lost.**

You will succeed at the task if you find at least 9 out of 12 objects. The task is similar in difficulty to the previous task.

For example, if you invest $1.50 in your performance, the remaining $0.50 in the fund is given to the charity. If you succeed, the $1.50 investment is tripled to $4.50 and given to the charity for a total donation of $5. If you do not succeed, the $1.50 investment is lost and the total charity donation is $0.50.

The table below describes 5 possible choices and outcomes, although you can choose any investment between $0 and $2.

Investment Choices and Outcomes		
Investment	Payment if Successful	Payment if Unsuccessful
$0	$2 + 3 x $0 = $2	$2
$0.12	$1.88 + 3 x $0.12 = $2.24	$1.88
$1	$1 + 3 x $1 = $4	$1
$1.50	$0.50 + 3 x $1.50 = $5	$0.50
$2	$0 + 3 x $2 = $6	$0

Before we proceed, you must pass this comprehension test to ensure that you understand the task and investment decision. You must get all the questions right in order to proceed.

Imagine that you invested $0.90 in your performance for the charity. You found 6 objects. How much would the charity earn?

- $0.00
- $0.90
- $1.10
- $2.70

Imagine instead that you invested $1.25 in your performance for the charity. You found 10 objects. How much would the charity earn?

- $0.75
- $1.25
- $3.75
- $4.50

Imagine that you were 100% sure you would succeed at the task. How much would you invest to maximize the charity's earnings?

- $0.00
- $1.00
- $2.00
- Impossible to say

Page 8 - Experiment 2

You have a personal fund of $2. You can choose how much of this fund to invest in your performance. **Whatever you DO NOT invest will be given to you as is. Whatever you DO invest will be TRIPLED if you succeed at the task. If you do not succeed at the task, your investment is lost.**

You will succeed at the task if you find at least 10 out of 12 objects. The task is similar in difficulty to the previous task.

For example, if you invest $1.50 in your performance, you will receive the remaining $0.50 in the fund no matter your performance on the task. If you succeed, the $1.50 investment is tripled to $4.50 and given to you for a total payment of $5. If you do not succeed, the $1.50 investment is lost and the total payment is $0.50.

The table below describes 5 possible choices and outcomes, although you can choose any investment between $0 and $2.

Investment Choices and Outcomes		
Investment	Payment if Successful	Payment if Unsuccessful
$0	$2 + 3 x $0 = $2	$2
$0.12	$1.88 + 3 x $0.12 = $2.24	$1.88
$1	$1 + 3 x $1 = $4	$1
$1.50	$0.50 + 3 x $1.50 = $5	$0.50
$2	$0 + 3 x $2 = $6	$0

Before we proceed, you must pass this comprehension test to ensure that you understand the task and investment decision. You must get all the questions right in order to proceed.

Imagine that you invested $0.90 in your performance. You found 6 objects. How much would you earn?

- $0.00
- $0.90
- $1.10
- $2.70

Imagine instead that you invested $1.25 in your performance. You found 10 objects. How much would you earn?

- $0.75
- $1.25
- $3.75
- $4.50

Imagine that you were 100% sure you would succeed at the task. How much would you invest to maximize your earnings?

- $0.00
- $1.00
- $2.00
- Impossible to say

Page 9 - Experiment 1

This time, the picture will be overlaid with a grid. You will report the location of the object, which we will evaluate later to figure out your exact payment. You also must remain on the page for the entire time, so you cannot avoid this task. You will have 5 minutes to complete the task.

The table below describes 5 possible choices and outcomes, although you can choose any investment between $0 and $2. It is provided for your reference only.

Investment Choices and Outcomes		
Investment	Payment if Successful	Payment if Unsuccessful
$0	$2 + 3 x $0 = $2	$2
$0.12	$1.88 + 3 x $0.12 = $2.24	$1.88
$1	$1 + 3 x $1 = $4	$1
$1.50	$0.50 + 3 x $1.50 = $5	$0.50
$2	$0 + 3 x $2 = $6	$0

REMINDER: You will only be successful if you find at least 9 objects.

Please enter the number of dollars and cents, from 0.00 through 2.00, that you will invest in your performance for the charity:

Page 9 - Experiment 2

This time, the picture will be overlaid with a grid. You will report the location of the object, which we will evaluate later to figure out your exact payment. You also must remain on the page for the entire time, so you cannot avoid this task. You will have 5 minutes to complete the task.

The table below describes 5 possible choices and outcomes, although you can choose any investment between $0 and $2. It is provided for your reference only.

Investment Choices and Outcomes		
Investment	Payment if Successful	Payment if Unsuccessful
$0	$2 + 3 x $0 = $2	$2
$0.12	$1.88 + 3 x $0.12 = $2.24	$1.88
$1	$1 + 3 x $1 = $4	$1
$1.50	$0.50 + 3 x $1.50 = $5	$0.50
$2	$0 + 3 x $2 = $6	$0

REMINDER: You will only be successful if you find at least 10 objects.

Please enter the number of dollars and cents, from 0.00 through 2.00, that you will invest in your performance:

Page 10 - Experiment 1

You have chosen to invest $W in your performance.

This means that if you find 9 or more objects, your donation to the Make-A-Wish Foundation will be $3W + (2 - W)$. If you find 8 or fewer objects, your donation will be $2 - W$.

Page 10 - Experiment 2

You have chosen to invest $W in your performance.

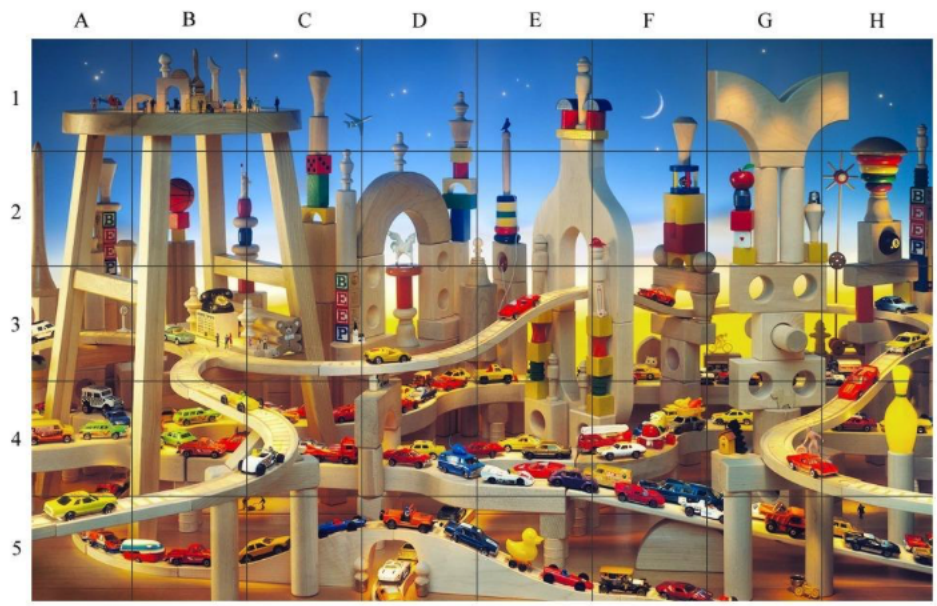
This means that if you find 10 or more objects, your total earnings will be $3W + (2 - W)$. If you find 9 or fewer objects, your total earnings will be $2 - W$.

Page 11

On the next page, you will see a timer which will automatically advance when the timer runs out. **You will not be able to advance before the timer runs out.**

Remember that this time, the picture will be overlaid with a grid. You will report the location of the object, which we will then evaluate to determine out your total payment. You will be paid the amount you earned on the previous task regardless of your performance on this task when you complete the survey.

Page 12



Write the location of the hidden object in the boxes below. For example, if you found a bird in row 4 and column D, write 4D in the box under bird.

	a duck	an elephant	a roller skate	a red apple	a black 8 ball	a helicopter
Location						

	a teddy bear	a clock	a red dice	a winged horse	a chair	a tennis racket
Location						

Page 13

You have finished the tasks, leaving only a brief survey to complete before the study is over.

As a reminder, your responses to these questions will be kept anonymous.

Page 14

What is your gender?

- Male
- Female
- Transgender
- Other (please specify)

What is the highest degree or level of school you have completed?

- Some high school
- High school graduate, diploma, or equivalent
- Some college
- Associate's degree
- Bachelor's degree
- Master's degree
- Professional degree
- Doctorate degree

What is your age?
Are you currently...?

- Employed full-time
- Employed part-time
- Out of work but not looking for work
- Out of work but looking for work
- Student
- Retired
- Unable to work

Page 15

How difficult did you find the task in today's study?

- Very difficult
- Moderately difficult
- Somewhat easy

- Very easy

During the first object search task, were you ever unsure whether you had found an object?

- Yes
- No

During the second object search task, were you ever unsure whether you had found an object?

- Yes
- No

How well do you believe you performed on today's hidden object tasks? Please choose a value from 1 to 10.

How well do you believe you would perform on similar hidden object tasks in the future? Please choose a value from 1 to 10.

If you had been given more time, do you think you would've found more of the objects on the first task?

- Yes
- No
- I found all of them

If you had been given more time, do you think you would've found more of the objects on the second task?

- Yes
- No
- I found all of them

Page 16

In general, are you a person who likes to take risks or do you try to avoid taking risks? Please choose a value from 1 to 10.

How often do you donate money to charities?

- Never
- Rarely (once or twice a year)

- Sometimes (once a month)
- Often (more than once a month)

Page 17

A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost in dollars?

If it takes 5 machines 5 minutes to make 5 widgets, how many minutes would it take 100 machines to make 100 widgets?

In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how many days would it take for the patch to cover half of the lake?