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Feedback Spillover Effect on Competitiveness Across Unrelated Tasks

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ABSTRACT: Employees typically work on multiple tasks that require unrelated skills and abilities. While past research strongly supports that relative performance feedback influences employee performance and effort allocation, little is known about the effect of relative performance feedback on employee competitiveness. Using a lab experiment, we study and confirm a complementary feedback spillover effect—relative performance feedback in the first task positively affects competitiveness in the unrelated second task. Furthermore, we find that the effect operates jointly and independently through belief- and taste-altering mechanisms. The results have important implications for organizations to understand both the power and the limitations of using relative performance feedback as an intervention policy in the design of accounting, control, and reporting systems.

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I. INTRODUCTION

Modern workplaces increasingly require employees to multitask across an array of tasks that involve different skills (Coviello, Ichino, and Persico 2014; Schöttner 2008). For example, employers in consultancy and high-tech industries often use job rotation schedules across projects to improve employee motivation, decrease burnout, and exploit synergies (Arya and Mittendorf 2004; Hsieh and Chao 2004). Employees undertake projects sequentially that may require a range of skills in areas such as accounting, sales, customer service, and software development. Similarly, in non-corporate settings such as hospitals, many clinically active surgeons are responsible for performing surgeries, teaching medical students, and conducting research (Schultz, Schreyoegg, and von Reitzenstein 2013). In such workplaces, relative performance feedback received in one task may spill over and have an unintended impact on effort, perseverance, and competitiveness in subsequent tasks. This can create path dependence such that outcomes achieved in one task lead to exaggerated long-term consequences in other tasks.

While feedback effects on employee performance, effort allocation, and goal setting behavior across multiple tasks have been studied widely (Bryant, Murthy, and Wheeler 2009; Buser 2016; Eriksson, Poulsen, and Villeval 2009; Hannan, McPhee, Newman, and Tafkov 2013, 2019; Hannan, Krishnan, and Newman 2008; Lourenço 2016; Wozniak, Harbaugh, and Mayr 2014), little is known about feedback effects on employee competitiveness. Filling this gap in the literature, this paper investigates whether and how relative performance feedback in the first work task spills over to affect competitiveness in the second *unrelated* work task.

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Supplemental material can be accessed by clicking the link in Appendix A.

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For accounting research, studying feedback effects on competitiveness is no less important than studying direct feedback effects on performance. Competitiveness determines, for example, how likely employees are to make individual choices such as job, award, and promotion applications, and to take risks when pursuing organizational goals (Benabou and Tirole 2002; Buser 2016; Malmendier and Tate 2005). Employee competitiveness can thus affect organizations' job turnover rates, investment portfolios, mergers, and acquisition decisions (Billett and Qian 2008; Malmendier and Tate 2005). If employee competitiveness affects projects to tender for, organizations can leverage feedback effects to increase willingness to compete for riskier yet more lucrative projects. Hence, while excessive competitiveness may sometimes be value destroying, more confident and competitive managers may create greater value for their organizations (Goel and Thakor 2008; Hirshleifer, Low, and Teoh 2012). Competitiveness may also affect prosocial behavior in workplaces. A recent study suggests a link between employee competitiveness and their (un)willingness to help colleagues (Black, Newman, Stikeleather, and Waddoups 2018). This provides another reason to better understand the determinants of competitiveness if collaborative work is crucial for firm success.

Our study contributes to the literature on feedback effects in multitask settings (e.g., Hannan et al. 2013; Hannan et al. 2019) by testing for the existence and direction of feedback spillover effects on competitiveness and identifying underlying mechanisms. We test for a complementary feedback spillover effect hypothesis, predicting that relative performance feedback in the first task will positively affect competitiveness in terms of compensation choices in the second task. We further distinguish between and test a belief-altering and a taste-altering mechanism, both of which may explain the feedback effect. While the mediating factor in the belief-altering mechanism is an individual's confidence about their task ability relative to others, in the taste-altering mechanism it is an individual's preference for social comparisons.

We conducted an experiment throughout which participants were matched in fixed groups of four. Participants worked on a visual perception task first and then a mathematical task. These two tasks were carefully pre-tested to be unrelated to each other both in terms of actual performance and perception of required abilities. In a between-subjects design, we manipulated whether participants received relative performance feedback on being in the top/bottom half of their group after the first task. In both tasks, we rewarded participants' performance with a piece rate. We elicited individuals' confidence about their performance relative to their group members both before and after the second task to test for the belief-altering mechanism. In the final task, we measured participants' competitiveness by asking them to make a series of incentivized choices between competitive pay and a piece rate to compensate for their performance in the second task. At the end of the study, we elicited individuals' demographics and economic preferences including preferences for social comparisons to test for the taste-altering mechanism.

Our results suggest a strong spillover effect of relative performance feedback from the first task on competitiveness in the second task. Consistent with the complementary feedback effect hypothesis, individuals who receive positive feedback (on being in the top half of their groups) in the first task choose competitive pay in an unrelated second task more often than those who receive negative feedback (on being in the bottom half). Importantly, the observed feedback effect on competitiveness operates through both the belief-altering and taste-altering mechanisms; combining elicited confidence and tastes for comparisons can fully explain the feedback spillover effect. We also find important heterogeneity in the feedback effect. Consistent with the previous literature, when relative performance feedback is unavailable, women choose competitive pay less often than men. We find that relative performance feedback reduces the gender difference in competitiveness. This result has important implications for the social desirability of feedback policies that accountants may utilize to design information systems and promote more gender-diverse workplaces.

Our study is most related to Hecht, Tafkov, and Towry (2012) and Black et al. (2018). Hecht et al. (2012) examine whether performance incentives in a rewarded task spill over to affect performance in an unrewarded task when these tasks are completed in close temporal proximity. Focusing instead on competitiveness, our paper complements their findings in showing that spillovers may also happen in tasks completed sequentially due to an *unrelated* feedback effect. Black et al. (2018) show that providing relative performance feedback in one task affects employees' helping behavior in an unrelated subsequent task. In their study, subjects had to complete a fixed-rate math task first and then play a prisoner's dilemma game. Subjects who received relative performance feedback cooperated significantly less than those who did not receive such feedback. The authors argued that this result was mediated by subjects' competitive mindset, a one-item self-reported measure of how competitive subjects felt toward the task. Our paper complements Black et al. (2018) by establishing a *direct* link between relative performance feedback and competitiveness, which is measured in an incentive-compatible way.

While we focus on a sequential multitask setting, our results may be generalized to other environments where multiple tasks are completed simultaneously (in close temporal proximity) or where tasks are somehow related. For example, consider a financial manager who is tasked with trading on a stock market and at the same time is managing several consultancy projects. The daily feedback they receive through the stock market may affect the decisions they make in their consultancy projects (Billett and Qian 2008). In such environments, the feedback spillover effect will be stronger given the informational value of the feedback and greater psychological closeness of the tasks (Hecht et al. 2012). In that sense, our study provides a conservative test of the feedback spillover effect on competitiveness across tasks.

Our results suggest that relative performance feedback may be detrimental to firm performance in situations where employees perform interdependent tasks that require repeated collaboration among employees. Managers may have to think twice about the provision of negative feedback to these employees since excessive competitiveness may hinder cooperation. Managers may consider avoiding relative performance feedback or focusing exclusively on positive performance feedback in these situations. Our study also implies that relative performance feedback may be helpful in settings where jobs are intrinsically uninteresting yet profitable. Firm managers may use positive feedback strategically to encourage engagement in such jobs.

II. BACKGROUND AND DEVELOPMENT OF HYPOTHESES

One of the important functions of accountants is determining how information such as relative performance feedback affects employee behavior. The existing economics and accounting literatures have overwhelmingly focused on feedback effects on performance (Buchheit, Dalton, Downen, and Pippin 2012; Gill, Kissová, Lee, and Prowse 2019; Hannan et al. 2008; Hannan et al. 2013; Hannan et al. 2019; Hecht et al. 2012; Holderness, Olsen, and Thornock 2017; Kuhnen and Tymula 2012; Loftus and Tanlu 2018; Newman and Tafkov 2014; Viator, Bagley, Barnes, and Harp 2014). For example, Hannan et al. (2013) study how relative performance feedback affects effort allocation and performance across multiple real-effort tasks. They find that feedback leads to a greater distortion of effort allocation across tasks.

Hannan et al. (2013) also identify self-reported feelings of pride (shame) and concerns for social comparisons as mediating factors for the feedback effect on performance. These two factors are conceptually similar to our constructs of confidence in relative ability and taste for social comparisons in our belief-altering and taste-altering mechanisms, respectively. However, we are interested in learning about the feedback effect on competitiveness independent of any direct feedback effect on actual task performance. As discussed in the introduction, employee competitiveness can affect, for example, their willingness to take up projects with more uncertain yet more lucrative outcomes or to self-select into jobs that pay more competitively. These are all important employee decisions that matter to firm performance other than direct feedback effects.

In what follows, we develop the complementary feedback spillover effect we test by fleshing out the underlying testable mechanisms.

Complementary Feedback Spillover Effect

Our main hypothesis predicts that, in a multitask setting, there will be a positive association between the valence of relative performance feedback in one task and competitiveness in another unrelated task. We consider two mechanisms that operationalize the complementary feedback spillover effect in our multitask setting.

The first mechanism is based on economic learning theories. Consistent with these theories, providing subjects with full objective feedback repeatedly eliminates biases in confidence beliefs and subsequent economic decisions (Moore and Cain 2007; Murad 2016; Rose and Windschitl 2008). Our design selects two unrelated tasks in an attempt to eliminate such belief updating about *specific* ability in a task. But it remains possible that the feedback effect operates through updating beliefs of *general* ability relative to the reference group and hence may carry some "informational" value: *I was better/worse than my peers in task 1 → I am more/less able than my peers in general → I am better/worse than my peers in task 2 → I become more/less competitive*.

A closely related line of experimental research about feedback effects on willingness to compete sheds light on the last logical induction. These studies show that confidence promotes choices of competitive pay; conditional on performance, confidence accounts for most of the variations in gender gap in terms of willingness to compete (Dreber, von Essen, and Ranehill 2014; Kamas and Preston 2012; van Veldhuizen 2017). This mechanism rests on the premise that, even when economic theory predicts that people should ignore feedback from an unrelated task, feedback may still enhance people's beliefs about their abilities and spill over to an unrelated task.

The second mechanism to operationalize the complementary feedback effect is based on social psychology theories in which feedback directly interacts with tastes for social comparisons. Social comparison theory posits that individuals have a drive to continually compare themselves to others in order to evaluate their own competence (Festinger 1954). Individuals want to do well relative to others and their sense of self-identity suffers when they perform worse than others (Tesser and Campbell 1980). We expect that positive feedback in the first task increases individuals' tastes for social comparisons while negative feedback has the opposite effect. In our experiment, taste for social comparisons can reflect an individual's tendency to seek further social comparisons by becoming more competitive in the second task. Unlike the informational (*cognitive*) belief-altering mechanism, the taste-altering mechanism is motivational (*affective*) in nature: *I was better/worse* *than my peers in task 1 → I enjoyed/disliked how it felt to receive positive/negative feedback → I seek more/fewer comparisons with my peers in task 2 → I become more/less competitive*.

The claim that tastes for comparisons are malleable to the environment has found some support in experiments that examine explicit behavior seeking or avoiding information on social comparisons. For instance, individuals who are self-confident in their ability are more interested in learning about their relative performance in tasks that are ego-relevant such as an IQ task, while those that lack confidence avoid receiving feedback (Burks, Carpenter, Goette, and Rustichini 2013).

On a deeper psychological level, feedback can affect employees' self-esteem (Kuhnen and Tymula 2012) through the motives of self-enhancement and self-improvement (Falk and Knell 2004; Wood and Taylor 1991). Self-enhancement is a mental strategy in which people make themselves feel better and enhance their subjective well-being via downward social comparisons. Self-improvement is an alternative mental strategy in which people commit themselves to working harder via upward social comparisons. The balance of these two motives generally affects the level of social comparisons and the amount of costly effort individuals are willing to exert in a task. In particular, Falk and Knell (2004) find evidence that highly able individuals tend to engage in upward social comparisons; that is, they compare themselves to similar others. Berger, Libby, and Webb (2018) similarly find that social comparisons become more important for individuals' performance when there is a higher chance of winning in tournaments.

The complementary feedback spillover hypothesis thus predicts the following feedback effect on competitiveness:

H1: Positive/negative feedback in the first task will spill over to increase/decrease the level of competitiveness in the second task.

In contrast to complementary feedback spillover effects, self-affirmation theories predict a compensatory feedback spillover effect where individuals who receive negative relative performance feedback report higher confidence and seek more social comparisons than those who receive positive feedback. These theories claim that individuals' primary goal is to maintain and affirm *general* integrity of the self, and not necessarily resolve the *specific* threat (Steele 1988). For example, Tsai and Xie (2017) find that people strive to compensate for lower status in one domain by obtaining higher status in another unrelated domain (for instance, by being more generous when altruism is perceived as the primary source of status).

In a multitasking environment like ours, it follows that individuals may deal with a threat to their self-image in one area by affirming their competence in another area. Specifically, when individuals' overall self-esteem is threatened by receiving negative feedback in the first task, they may attempt to restore their general image in the second unrelated task by engaging in social comparisons more often. At the cognitive level, individuals may also maintain their self-image via self-deception or "positive thinking" (Sharot 2011; Sharot, Korn, and Dolan 2011).[1] Thus, the compensatory process suggests that individuals may respond to negative feedback in the first task by reporting higher confidence in the second task with the same aim: restoring self-image. They may seek affirmative comparisons with others by choosing competitive pay. By the same compensatory process, individuals who receive positive feedback may have achieved the desired enhancement of self-esteem, and thus avoid comparisons in the second task, which could create additional threats to their self-esteem (Charness, Rustichini, and van de Ven 2018; Köszegi 2006; Schwardmann and van der Weele 2019). These compensatory feedback spillover effects mitigate our hypothesized complementary spillover effect.

As discussed above, the complementary feedback spillover effect, we predict, may operate through altering confidence about one's ability, altering tastes for social comparisons, or both. We distinguish between the belief-altering and taste-altering mechanisms and verify whether either or both can account for the feedback spillover effect on competitiveness. We thus test the following hypotheses:

H2a: (Belief-altering mechanism): Relative performance feedback effect from the first task on competitiveness in the second task will operate through altering confidence beliefs.

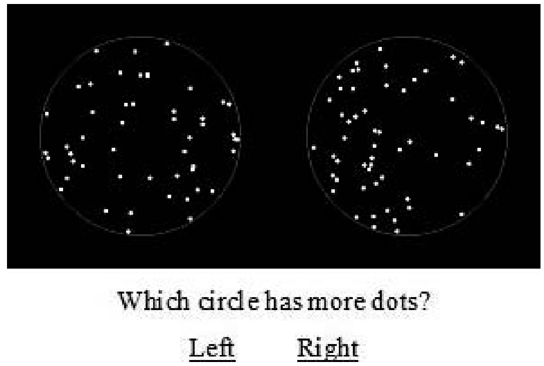
H2b: (Taste-altering mechanism): Relative performance feedback effect from the first task on competitiveness in the second task will operate through altering tastes for social comparisons.

III. EXPERIMENTAL DESIGN

To test our hypotheses, we ran a number of pilot experiments and the main experiment at a large public university in the South-East of England. In total, 42 subjects participated in the pilot experiments (male = 50 percent, age: M = 21.5, SD = 4.63).

[1] Typical positive thinking in response to feedback is that, while people incorporate desirable information into existing beliefs according to Bayes' Rule, they show an aversion to incorporating undesirable information (Köszegi 2006) and thus discount its impact (Eil and Rao 2011). Eil and Rao (2011) and Ertac (2011) show that experimental subjects incorporate feedback asymmetrically by disregarding ego-relevant negative feedback in single-task environments.

FIGURE 1
The Circle Task



One hundred fifty-two subjects participated in the main experiment (male = 55 percent; age: M = 21.9, SD = 5.51). The experiments with embedded instructions were programmed in z-Tree (Fischbacher 2007).[2] A typical session lasted around 45 minutes with an average payment of £13.70, including a £2 show-up fee and a £5 completion fee.

Pilot Experiments

The validity of our experimental design depends on sufficient dissimilarity between the two work tasks that are ideally unrelated in every aspect. In order to make an empirically grounded selection of unrelated tasks, we performed a pre-test evaluation of five different experimental tasks.[3] For each task, we measured subjects' piece rate performance, their perceptions of task difficulty on a seven-point scale, and their choices of submitting past performance to competitive pay. We also asked subjects to rate the importance of five cognitive skills for each task performance and whether they thought that performance between combinations of two tasks was related.[4]

The results of the pilot experiments are presented in Online Appendix B. Based on a within-subject analysis of the pilot data (summarized in Table B1 in Online Appendix B), we decided to use the circle task as our first task and the number-adding task as our second task. The circle task is a visual perception task, which requires subjects to see a pair of black circles with white dots in them for 1 second and to judge which circle has more dots (see Figure 1). The version of the circle task used in our experiment consisted of 60 such pairs of circles. The position of dots in every pair of circles was randomly determined, but one circle always contained 50 dots and the other 55 dots. Subjects were asked to choose the circle with more dots for every pair of circles. After the submission of each answer, a new pair was shown without telling them whether the previous answer was right or wrong. Correct answers added up to the subjects' final performance score; wrong answers were not penalized.

The number-adding task consisted of adding up five randomly generated two-digit numbers. Subjects had five minutes to complete as many summations as possible. Calculators were not allowed but subjects could use provided scratch paper. After the submission of each answer, a new problem was shown without any feedback on whether the previous answer was right or wrong. Right answers added up to the subjects' final performance score and wrong answers were not penalized.[5]

[2] All instructions and the post-experimental questionnaire are reproduced in Online Appendix A (see Appendix A for the link to the downloadable document including all Online Appendices). All experiments received favorable ethical approval from the University Ethics Committee.

[3] These included the circle task (Hollard, Massoni, and Vergnaud 2016), the counting zeros task (Abeler, Falk, Goette, and Huffman 2011), the ball-catching task (Gächter, Huang, and Sefton 2016), the slider task (Gill and Prowse 2012), and the number-adding task (single-digit variant). They were paid according to a piece rate in one randomly selected task. Piece rates were varied across the tasks to equalize expected earnings from each task. The instructions for the pilot experiments are presented in Online Appendix A2.

[4] We asked for the importance of Attention (holding attention while completing the task), Working Memory (ability to learn information and use that information for the current activity), Visual Perception (ability to see and interpret the visual information), Cognitive Flexibility (being able to consider several solutions or plans, not only the first one that comes to mind), and Numeracy Skills (ability to reason and to apply simple numerical concepts) for each of the five tasks subjects completed.

[5] Using the circle task as the first task and the number-adding task as the second task also allows us to test whether relative performance feedback affects gender differences in competitiveness in math tasks previously reported in the literature (Kamas and Preston 2012; Niederle and Vesterlund 2007). Indeed, our pilot experiments show significant gender differences in competitiveness in the number-adding task, whereas no such difference exists in the circle task.

Main Experiment

Our main experiment consisted of three incentivized parts. Subjects were asked to perform the circle and then the number-adding task in Parts 1 and 2. This sequential single-period setup was similar to that in Buser (2016) and avoided any development of endogenous norms that would carry over from one period to another (Brüggen and Moers 2007). Upon arriving at the lab, subjects were seated according to randomly allocated ID numbers they received from the experimenter. The general instructions of the experiment, including general participation rules, rules for cash payment, and consent forms, were provided in paper form and were read aloud by the experimenter. Subjects were told that they would be asked to complete several parts and that one part would be randomly selected for payment at the end of the experiment. They were also told that the specific instructions for different parts of the experiment would be shown on their computer screens before each part began.

At the beginning of a session, each subject was randomly matched into a group with three other subjects and the group remained the same throughout the session. In Part 1, all subjects worked on the first task (the circle task), and in Part 2 they worked on the second task (the number-adding task). Performance in both tasks was incentivized by piece rates: £0.20 for each correctly judged circle task and £0.50 for each correctly solved number-addition task. The chosen piece rates for the tasks aimed to equalize expected earnings from each task. Previous studies show that using sufficient piece rate incentives usually leads subjects to exert maximum performance (Eriksson et al. 2009). This allows us to study the feedback effect on competitiveness independent of its direct effect on performance.

In a between-subjects design, we manipulated whether subjects received feedback about their relative standing within their group according to their circle task performance at the end of Part 1. In the *NoFeedback* treatment, subjects did not receive any performance feedback in the circle task. In the *Feedback* treatment, subjects received relative performance feedback after the circle task, which read as "your score was one of the TOP/BOTTOM two scores of your group." A recent paper by Gill et al. (2019) shows that people are mainly affected by feedback about the top and bottom ranks ("first place loving" and "last place loathing") and do not care much about intermediary ranks. We thus chose to use coarse binary feedback of top/bottom halves without having to discard the observations from the intermediary feedback receivers.

Before each task, subjects were asked to judge, on a continuous scale from 0 percent to 100 percent, the probability that their performance score would be in the top half of scores in their group. Specifically, they were asked to assign 0 percent if they were completely certain about scoring in the bottom half of their group, assign 100 percent if they were completely certain about scoring in the top half of their group, and assign intermediary values to the degree they deemed appropriate for their uncertainty. They could indicate their choices using the slider on their screens, which could be moved from 0 to 100. The answer to this question served as the *pre-task confidence* measure. After they completed the number-adding task, they were asked the same question about whether their performance score was in the top half of their group, the answer to which served as the *post-task confidence* measure.[6] Subjects did not learn about the accuracy of their beliefs and payments until the end of the experimental session, but they knew that their accuracy was incentivized using an incentive-compatible Becker-DeGroot-Marschak mechanism (Schotter and Trevino 2014).[7]

Measuring Competitiveness

In Part 3, subjects did not have to perform a task but were asked to fill in a multiple-choice list table shown in Exhibit 1. The table comprised ten rows, and the choice in each row was between a tournament (Option A) and a piece rate (Option B) payment scheme. The chosen payment scheme in a randomly selected row would be applied to their performance in the number-adding task if Part 3 was chosen by the random incentive mechanism at the end of the experiment. The tournament option paid £1.00 per correct number addition if subjects were in the top half of their group and £0.00 if they were in the bottom half. The piece rate option had ten levels in decreasing order from £1.00 to £0.10 per correct addition. Subjects had to make a choice for every row of the table.

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[6] Our confidence elicitation method was robust to Benoît and Dubra's (2011) and Benoît, Dubra, and Moore's (2015) critique of "apparent overconfidence." To obtain more informative measures of confidence, they recommend (1) using well-defined groups that subjects can compare themselves to, (2) using well-defined performance tasks, and (3) eliciting confidence as a subjective belief distribution rather than expected rankings within the group. Our confidence elicitation tool was a straightforward one-item question and satisfied all three criteria. Criteria (3) was satisfied assuming that subjects had additive subjective probabilities on scoring in the top and bottom halves of their groups.

[7] The Becker-DeGroot-Marschak mechanism is an incentive-compatible tool to elicit subjects' valuations and beliefs. Subjects were asked to indicate the probability that they would score in the top two in their groups. Then the computer would draw a number X between 0 and 100. If subjects' reported probability in percent was above X, then subjects would be paid an additional £1 if they scored in the top two in their groups. If subjects' reported probability in percent was below X, then subjects would be paid £1 with X percent probability. In theory, this procedure implies that subjects would maximize their chances of winning the £1 if they stated their true belief of scoring in the top two. For the exact wording and explanation of this method, please see the instructions in Online Appendix A1.

EXHIBIT 1
Eliciting Preferences for Competitive Pay in Part 3

Row	A: Tournament	Your Choice	B: Piece Rate
1	You get £1.00 per correctly solved number addition if your score in Part 2 was in the TOP two scores of your group, and £0.00 if your score in Part 2 was in the BOTTOM two scores of your group.	A □ □ B	£1.00 per correct number addition
2		A □ □ B	£0.90 per correct number addition
3		A □ □ B	£0.80 per correct number addition
4		A □ □ B	£0.70 per correct number addition
5		A □ □ B	£0.60 per correct number addition
6		A □ □ B	£0.50 per correct number addition
7		A □ □ B	£0.40 per correct number addition
8		A □ □ B	£0.30 per correct number addition
9		A □ □ B	£0.20 per correct number addition
10		A □ □ B	£0.10 per correct number addition

This method, which was developed by Ifcher and Zarghamee (2016), is a fine-grained measure of competitiveness compared to Niederle and Vesterlund's (2007) simple binary choice method. Given the construction of the table, subjects were expected to choose the piece rate in the first row because it was the highest and most certain payment for any performance level. At some point, they were likely to switch from piece rate to tournament; the level of the piece rate where they made the switch was the *piece rate equivalent* (*PR-equivalent*) of competitive pay. For example, a subject switching from piece rate to tournament on the fifth row implied that they valued competitive pay equivalent to the piece rate worth between £0.50 and £0.60. For simplicity, we take the lower bound of the equivalence range as an individual measure of competitiveness.[8,9]

Post-Experimental Elicitations

After Part 3, subjects were asked to complete a questionnaire on demographic characteristics and self-reported economic preferences (general risk, general confidence, and general competitive attitudes).[10] At the end of the experiment, all subjects received information about their absolute performance scores and whether the scores were in the top or bottom half of their groups in each of the two tasks. They were also informed which part was selected for payment (in the case of Part 3, which row was selected) and their final cash payment.

[8] Our measure of competitiveness could be influenced by other factors such as feedback aversion, risk, and competitive attitudes. We control for feedback aversion by making it clear that although all subjects will be paid by piece rates in Parts 1 and 2, they would still receive relative performance feedback at the end of the experiment in both treatment conditions. Past research shows that feedback aversion affects confidence levels, risk preferences, task choices, and effort levels in a variety of decision situations (Burks et al. 2013; Niederle and Vesterlund 2007; Zeelenberg, Beattie, van der Pligt, and de Vries 1996). Thus, giving subjects feedback on their relative performance at the end of experiment, irrespective of their choices in Part 3, removes any potential effect of feedback aversion on choices in Part 3. We also explicitly measured general risk, confidence, and competitive attitudes in an end-of-study questionnaire and subsequently used them as controls when analyzing preferences.

[9] Our measure of competitiveness reflects individuals' preferences for competitive pay to compensate past performance. An alternative measure would be to conduct this part before the number-adding task, and therefore competitiveness would reflect preferences for competing in a future task. The main reason why we did not use that measure is that we believe the decision made in compensation choices is likely to affect subjects' motivation to perform. For example, a subject may choose the tournament option across the board and use that as a commitment or motivational device to work harder than if they have chosen the piece rate. Therefore, it is unclear whether the measured competitiveness reflects such motivation or preferences for competitive pay. Importantly, we note that our theory does not rely on task completion since both confidence beliefs and tastes for social comparisons would work similarly given individuals' beliefs about past or future absolute performance.

[10] The self-reported economic preferences were similar to the validated survey instruments developed by Dohmen et al. (2011), who showed that self-reported measures of six economic preferences strongly correlate with many real-life and laboratory decisions.

EXHIBIT 2
Design of the Main Experiment

	General Instructions	General participant rules; processing of payment; consent form
Part 1	Pre-task Confidence Elicitation	On a scale of 0% to 100%, indicate the probability that your score will be in the top half of your group. BDM Incentivized
	The Circle Task	Complete 60 pairs of circles; £0.20 per correct answer
	Feedback	"NoFeedback": no information about performance "Feedback": relative performance (top/bottom two in your group)
Part 2	Pre-task Confidence Elicitation	On a scale of 0% to 100%, indicate the probability that your score will be in the top half of your group.
	The Number Adding Task	Add up as many as possible five two-digit numbers in 5 minutes; £0.50 per correct answer
	Feedback	Absolute performance score of number adding task
	Post-task Confidence Elicitation	On a scale of 0% to 100%, indicate the probability that your score was in the top half of your group.
Part 3	Competitiveness	Choose between the tournament and piece rate compensation for each of the 10 levels of piece rate from £0.10 to £1.00 to be applied to Part 2 score; one of the 10 choices for payment
	Questionnaire	Demographics; general risk/confidence/competitiveness; aversion to social comparisons
	Final Feedback	Absolute performance score + relative performance (top/bottom) feedback for both tasks. Final earnings.

To additionally test for the taste-altering mechanism underlying the feedback spillover effect, we asked the following question in the end-of-study questionnaire: "We will be holding different types of experiments in the future. If you had a choice, what kind of experiments would you like to participate in?" The answers ranged from 1 "I would STRONGLY prefer participating in the experiments where my earnings depend on my relative performance compared to other participants' performance" to 5 "I would STRONGLY prefer participating in the experiments where my earnings depend on my own performance NOT compared to other participants' performance" with 3 indicating indifference. This measure is an indication of subjects' aversion to social comparisons in experiments. Exhibit 2 summarizes the design of the main experiment.

IV. RESULTS

Before the main analysis, we check whether scores in the two tasks are uncorrelated, in line with our pilot results. The Pearson's correlation coefficient between the circle task and number-adding task scores is low and insignificant ($\rho = 0.12$, $p = 0.13$), thereby supporting the internal validity of our design. We also do not observe any correlation between being in the top half of a group in the circle and number-adding tasks: the Pearson's correlation coefficient between these two measures is also low and insignificant ($\rho = 0.13$, $p = 0.12$). Hence, the feedback received from the circle task before subjects began to work on the number-adding task could be seen as good as random. We analyze our results according to three feedback "conditions": having received positive relative performance feedback in the circle task (*Top Feedback*), having received negative relative performance feedback (*Bottom Feedback*), and having received no relative performance feedback (*NoFeedback*).

Table 1 reports descriptive statistics on all variables in the experiment to check whether the randomization was balanced in terms of subjects' observable characteristics. We find no significant differences in our control variables between feedback conditions, such as pre-task confidence level in the circle task, subjects' self-reported general risk, general competitiveness, and general confidence measures ($p > 0.10$, Wilcoxon rank-sum test). Significant differences exist between feedback conditions in the circle task score, pre- and post-task confidence in the number-adding task, PR-equivalents, and self-reported aversion to social comparisons ($p < 0.10$). These are discussed in the main analysis below.

In the remainder of the main analysis, we first present non-parametric test results of treatment differences in PR-equivalents. We then conduct parametric estimations of the feedback spillover effect and the possible mechanisms operationalizing the effect and check heterogeneity in the feedback effect. In all analyses, we remove 15 subjects who demonstrate reverse-order switching by choosing the tournament in the first row and then switching to the piece rate in the later rows.

TABLE 1

Descriptive Statistics

	***NoFeedback* (n = 52) Mean (SD)**	**Bottom (n = 50) Mean (SD)**	**Top (n = 50) Mean (SD)**
Pre-Task Confidence (Circle)	53.63%	54.50%	55.86%
	(25.67)	(25.55)	(25.23)
Score (Circle)	39.90	37.82	43.40
	(4.99)	(3.95)	(2.74)
Pre-Task Confidence (Number-Adding)	59.29%	52.88%	62.30%
	(28.11)	(26.47)	(24.98)
Score (Number-Adding)	8.02	7.30	8.98
	(3.58)	(3.59)	(3.85)
Post-Task Confidence (Number-Adding)	57.51%	44.56%	65.52%
	(29.64)	(30.79)	(25.89)
PR-Equivalents	£0.42	£0.37	£0.54
	(0.31)	(0.27)	(0.28)
Aversion to Social Comparisons	3.15	3.68	2.92
	(1.38)	(1.41)	(1.30)
Risk	4.90	4.52	4.90
	(1.07)	(1.25)	(1.27)
Confidence	4.63	4.66	4.58
	(1.28)	(1.26)	(1.42)
Competitiveness	5.10	5.26	5.28
	(1.38)	(1.31)	(1.34)

General risk, confidence, and competitiveness are measured on a 1–7 Likert scale with 1 indicating "not at all" and 7 "very." When calculating PR-equivalents, we remove 14 subjects who demonstrate reverse-order switching by choosing the tournament in the first row and then switching to piece rate in the later rows. Standard deviations are in parentheses.

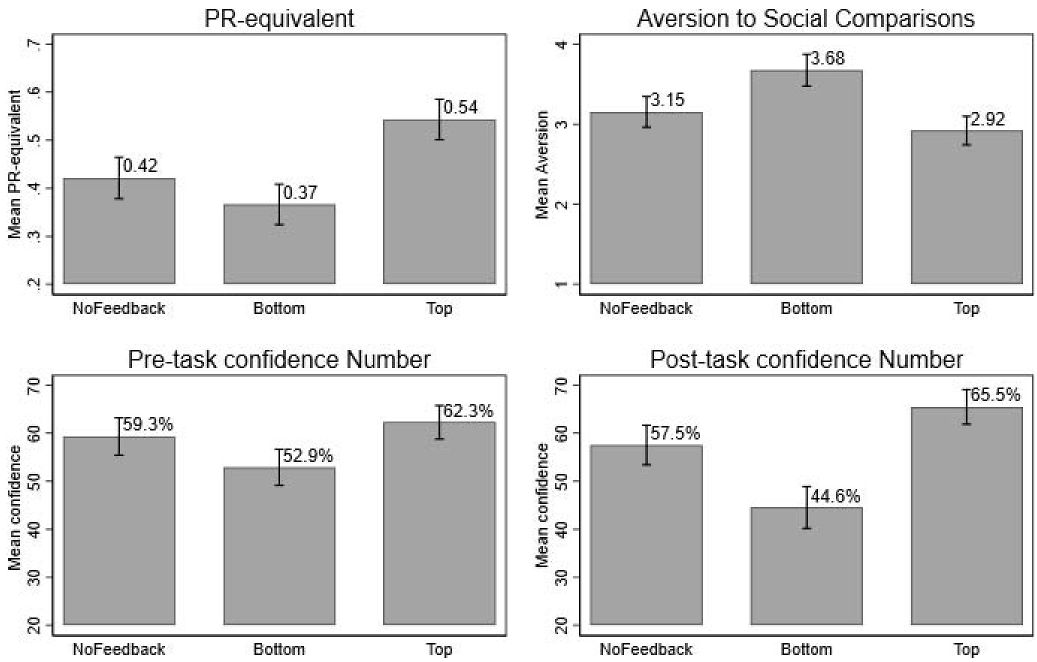
Evidence on the Feedback Spillover Effect

Figure 2 presents the means of four main variables: PR-equivalent (measuring competitiveness), pre- and post-task confidence in the number-adding task, and self-reported aversion to social comparisons. PR-equivalents differ significantly between the *Top Feedback* and *Bottom Feedback* conditions: subjects revealed £0.17 higher PR-equivalents after receiving *Top Feedback* than *Bottom Feedback* ($p = 0.01$, Wilcoxon rank-sum test). This result is consistent with the complementary feedback spillover effect (H1). The pre- and post-task confidence levels are higher in the *Top Feedback* than *Bottom Feedback* conditions: pre-task confidence has $p = 0.06$ and post-task confidence has $p < 0.01$. Aversion to social comparisons is also significantly different between the *Top Feedback* and *Bottom Feedback* conditions ($p = 0.02$).

We further examine the robustness of the feedback spillover effect on competitiveness by estimating two regression models. Model 1 controls for outcome variables that may confound the feedback effect: confidence levels in the circle task and number-adding performance scores. Model 2 additionally controls for demographics and self-reported economic preferences. The histogram of PR-equivalents reported in Figure C1 of Online Appendix C shows censors at 0 and 1; thus we estimate Tobit regressions. We also check the robustness of our results using other model specifications, such as symmetrically censored least squares and fixed effects for the number-adding task score. The results of these robustness tests are reported in Table C1 of Online Appendix C.

Table 2 reports results of the pooled sample from both treatments (upper panel) and on the sample from the Feedback treatment only (lower panel). The key variable of interest is the estimate of the *Top Feedback* versus *Bottom Feedback,* which is either calculated from post-estimation tests when both treatments are analyzed together or directly from the regressions when only the Feedback treatment is analyzed. We find strong evidence of the complementary feedback spillover effect on PR-equivalents. The estimate shows that receiving Top as opposed to Bottom relative performance feedback results in £0.14 higher PR-equivalents. We find that the spillover effect operates mainly through receiving positive feedback rather than negative feedback: those who receive *Top Feedback* have £0.09 higher PR-equivalents compared to those who receive no feedback, statistically significant at the 1 percent level. Those who receive *Bottom Feedback* have £0.05 lower PR-equivalents than those receiving no feedback, which is not statistically significant. These results parallel the "good news-bad news" effect in belief

FIGURE 2
Preliminary Evidence of Feedback Spillover Effects



Error bars show ± 1 SEM. PR-equivalent measures competitiveness elicited via subjects' choices of compensation pay. Pre- and post-task confidence measure confidence in the number-adding task. Aversion to social comparisons is elicited via subjects' answers to the question about if they would like to be invited to future experiments that pay competitively.

updating (Eil and Rao 2011). Overall, we conclude that there is strong evidence of the complementary feedback spillover effect on competitiveness (H1).

Belief-Altering or Taste-Altering Mechanism?

Is the observed complementary feedback spillover effect on competitiveness fully accounted for by variations in confidence in the number-adding task as predicted by the belief-altering mechanism? Or does the effect operate by changing subjects' tastes for social comparisons? Or both? To answer these questions, we turn to a structural estimation using a linear path analysis (Pearl 2013). We test for the causal link between feedback and PR-equivalents and the mediating effects of confidence and tastes on social comparisons. Figure 3 presents the standardized beta path coefficients between the main variables. We expect that, if belief- and taste-altering mechanisms explain the complementary feedback effect on PR-equivalents, the direct effect of feedback on PR-equivalents will be insignificant, while the indirect effect through the mediating variables of tastes (aversion to social comparisons) and beliefs (confidence) will be significant.

We test for this conjecture by estimating a path model from feedback to competitiveness with mediations through confidence, aversion to social comparisons, and actual performance. The feedback effect on PR-equivalents is significant through confidence[11] (path coefficient $= 0.32$, $p < 0.01$) and aversion to social comparisons (path coefficient $= -0.21$, $p = 0.03$).[12] The feedback effect on performance is not significant (path coefficient $= 0.10$, $p = 0.27$), whereas performance has a positive and significant direct effect on PR-equivalents (path coefficient $= 0.23$, $p = 0.03$). Importantly, the residual direct effect of feedback on PR-equivalents is not significant (path coefficient $= 0.13$, $p = 0.16$), meaning that combining the taste-altering and belief-altering mechanisms can fully account for the complementary feedback spillover effect (H2a and H2b).

[11] We use pre-task confidence, as it is a measure uncontaminated by experience in the number-adding task.
[12] Note that, in our setup, the negative coefficient means that positive feedback decreases aversion to social comparison, i.e., positive feedback increases tastes for social comparisons.

TABLE 2

Testing for the Feedback Spillover Effect

	PR-Equivalent	
	(1)	(2)
Top (versus no feedback)	0.09***	0.08***
	(0.03)	(0.02)
Bottom (versus no feedback)	−0.05	−0.03
	(0.07)	(0.07)
Pre-Task Confidence (Circle)	0.01***	0.01***
	(0.00)	(0.00)
Score (Number)	0.04***	0.04***
	(0.01)	(0.01)
Top versus Bottom	0.14	0.12
p-value	0.06	0.14
Pseudo R^2	0.21	0.27
Obs.	137	137
Feedback Treatment Only		
Top (versus bottom)	0.12**	0.14**
	(0.07)	(0.07)
Pre-Task Confidence (Circle)	0.01	0.00
	(0.01)	(0.01)
Score (Number)	0.04***	0.04***
	(0.01)	(0.01)
Pseudo R^2	0.22	0.35
Obs.	86	86
Demographics and psych. measures	No	Yes

, * Denote significance levels at 5 percent and 1 percent, respectively.
Standard errors clustered at session level are in parentheses. Demographics include gender and age. Psychological measures include general risk-taking, confidence, and competitiveness. The top versus bottom estimates and p-values in the upper panel are from post-estimation Wald tests.

Are There Heterogeneities?

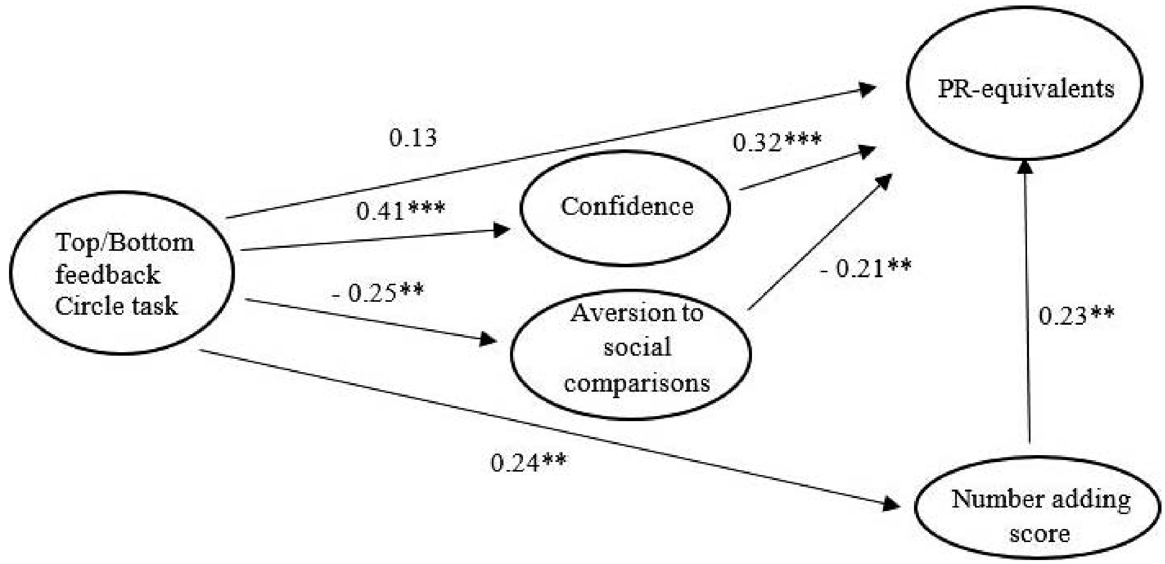
Past research shows that men and women have different behavioral responses to previous outcomes (Buser 2016; Gill and Prowse 2014). The number-adding task is often used to study gender differences in willingness to compete. We thus explore whether there are gender differences in competitiveness and whether the feedback spillover effect interacts with gender.[13]

In Table 3, we analyze gender differences in PR-equivalents controlling for other observable variables and find that gender differences in the absolute level of PR-equivalents are influenced by feedback. The regression results show that in the absence of any feedback, women's average PR-equivalent is £0.16 lower than that of men (Model 1). Importantly, the interaction of the female dummy with the Feedback treatment suggests that receiving feedback increases the average PR-equivalents of women compared to men by £0.15. Receiving feedback decreases the gender gap in competitiveness, independently of variations in controls such as actual performance levels, self-reported general risk, confidence, and competitive attitudes (Model 2).

What are the forces driving this result? We look at whether there are gender differences in the feedback effect on competitiveness. Table 4 reports the estimates separately by gender. Compared to the *NoFeedback* condition, women who receive *Top Feedback* increase PR-equivalents by £0.18. Comparing *Top Feedback* to *Bottom Feedback,* we find that men's PR-equivalents are higher when they receive positive feedback. Women's PR-equivalents are not statistically different, however. This suggests that feedback increases women's competitiveness, irrespective of the feedback type. This is an interesting result in light of previous findings that feedback helps correct confidence differences between genders and eliminate gender gaps in willingness to compete (Wozniak et al. 2014).

[13] We note that this analysis significantly decreases the power of our sample, since analyzing gender is similar to adding an additional treatment to the experiment. However, even with reduced power, the analysis reported below provides valuable information on the heterogeneous feedback spillover effect.

FIGURE 3
Path Analysis



*, **, *** Denote significance levels at 10 percent, 5 percent, and 1 percent, respectively.

RMSE = 0.03. Confidence refers to pre-task confidence in the number-adding task. Aversion to social comparisons is elicited via subjects' answers to the question about if they would like to be invited to future experiments that pay competitively. PR-equivalent measures competitiveness elicited via subjects' choices of compensation pay.

Robustness Study

We test the robustness of our results by reporting the results of a study that slightly varied the feedback structure and confidence elicitation compared to our main experiment. In real-world settings, employees almost always receive feedback about their absolute performance. In addition to absolute performance feedback, they may or may not receive feedback about their relative performance.

In the robustness study, we provided subjects with absolute performance feedback after the circle task in both the *NoFeedback* and Feedback treatments. In the Feedback treatment, we additionally provided subjects with relative performance feedback on whether they scored in the Top or Bottom half of their groups. Due to the presence of absolute performance feedback, the relative performance feedback manipulation in the robustness study might be less salient than in the main study and hence provided a stringent test of feedback spillover effects. We also elicited the confidence of subjects in a non-incentivized way, which was expected to decrease the precision of the data. Yet, non-incentivized belief elicitation had the advantage of reducing subjects' cognitive load when reporting their beliefs. We tested whether the feedback spillover effect on competitiveness is robust to these design changes.

Figure 4 presents the means and standard errors of competitiveness (PR-equivalent) and confidence across feedback conditions.[14] The means exclude subjects who received extreme absolute performance scores that would make the relative performance feedback redundant.[15] We again find evidence for the feedback spillover effect on competitiveness ($p = 0.03$, Wilcoxon rank-sum test). Consistent results from Tobit regressions of PR-equivalents are reported in Table C2 of Online Appendix C. But we do not find any statistical evidence for feedback effects on pre- and post-task confidence ($p = 0.10$ and $p = 0.37$, respectively).

V. CONCLUDING REMARKS

The informational and motivational value of feedback to change confidence, effort, risk-taking, and willingness to compete in single-task domains have been extensively studied in the literature. Feedback has been found to correct employees' biased

[14] We did not ask the survey question regarding aversion to social comparisons in this study.

[15] Thirty-four subjects who scored less than 37 or greater than 46 in circle task would always either be in the top or bottom half and hence would make the relative performance feedback ineffective. We thus exclude these subjects from the analysis. The results are robust to including these subjects.

TABLE 3

Feedback Eliminates the Gender Gap in Competitiveness

	PR-Equivalent	
	(1)	**(2)**
Female	−0.16*	−0.18*
	(0.09)	(0.09)
Female × Feedback	0.15	0.21*
	(0.08)	(0.09)
Feedback	−0.05	−0.07
	(0.06)	(0.06)
Score (Number)	0.04***	0.05***
	(0.01)	(0.01)
Obs.	137	137
Demographics and psych. measures	No	Yes

*, *** Denote significance levels at 10 percent and 1 percent, respectively.
Standard errors clustered at session level are in parentheses. Demographics include age. Psychological measures include general risk-taking, confidence, and competitiveness. We exclude subjects who demonstrate reverse-order switching.

beliefs and mitigate misalignments in competitive choices. The motivational value of feedback, which is adopted by some organizations, has also been shown to improve employee performance. Using a laboratory experiment, we investigate the effect of relative performance feedback on competitiveness in a setup where workers have to complete multiple unrelated tasks sequentially.

We contribute to the stream of research investigating policy design in multitask environments (Kachelmeier, Reichert, and Williamson 2008; Kachelmeier and Williamson 2010), as well as how social comparisons influence behavior in the workplace (Berger et al. 2018; Brüggen and Moers 2007; Hannan et al. 2013, 2019). Our study adds to this growing knowledge base by showing that relative performance feedback has a significant spillover effect on competitiveness in a multitask setting. The direction and strength of the spillover can be best explained by a complementary feedback effect operating via both a belief-altering and a taste-altering mechanism.

Our results have important implications for organizational policies. An interesting place to use feedback spillovers as a policy tool is job centers. Anecdotal evidence suggests that the psychometric and ability tests that jobseekers complete at English job centers are calibrated so that the test results are always positive. This is intended to give some positive feedback to jobseekers to improve their confidence (Malik 2013) and thus encourage more competitive and successful job searches. Our

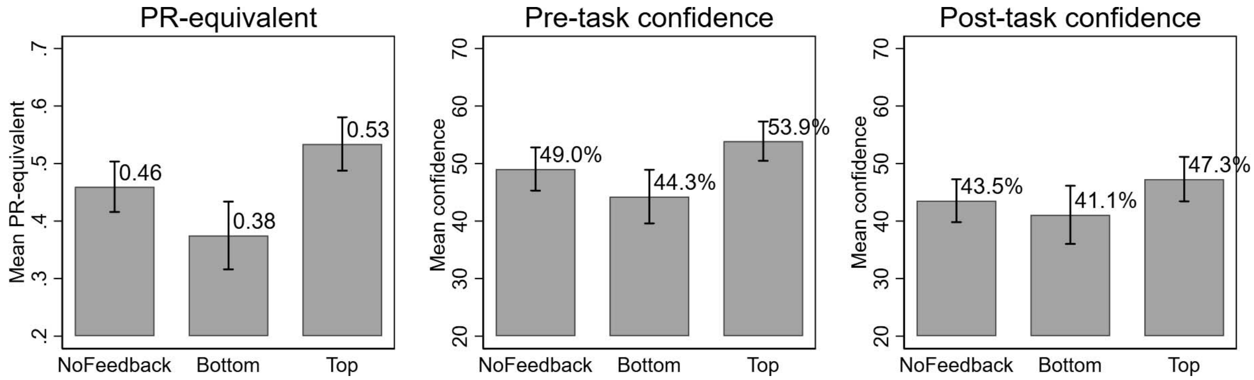
TABLE 4

Gendered Feedback Spillover Effect on Competitiveness

	PR-Equivalent	
	Men	**Women**
Top (versus no feedback)	0.02	0.18**
	(0.07)	(0.08)
Bottom (versus no feedback)	−0.14	0.11
	(0.09)	(0.08)
Top versus Bottom	0.16	0.07
p-value	0.07	0.26
Obs.	74	63
Demographics and psych. measures	Yes	Yes

*, **, *** Denote significance levels at 10 percent, 5 percent, and 1 percent, respectively.
Standard errors clustered at session level are in parentheses. Controls include the number-adding task score, demographics (age), and psychological measures (general risk-taking, confidence, and competitiveness). We exclude subjects who demonstrate reverse-order switching. The top versus bottom estimates and p-values are from post-estimation Wald tests.

FIGURE 4
Feedback Spillover Effects in the Robustness Study



Error bars show ± 1 SEM. PR-equivalent measures competitiveness elicited via subjects' choices of compensation pay. Pre- and post-task confidence measure confidence in the number-adding task.

results provide support for such policies. According to our results, managers should provide positive relative performance feedback to those who have succeeded in previous unrelated projects. Alternatively, managers should provide non-negative feedback or at least abstain from providing any negative feedback to those who have underperformed.

In practical terms, organizations should be aware of the context and timing when assigning tasks to workers. For example, providing easy tasks at the beginning may improve success in subsequent tasks that require competitive motivations, such as leading negotiations, pitching ideas to investors, working on R&D projects, or performing in sports tournaments (Rosenqvist and Skans 2015). In academic settings, schools can create environments with more positive feedback to foster students' confidence and competitiveness, both of which are predictors of students' career choices (Buser, Niederle, and Oosterbeek 2014; Schulz and Thöni 2016). This may prove especially important for encouraging students from underrepresented backgrounds, such as those from ethnic and racial minority groups and female students, to pursue competitive careers.

A novel finding of our study that requires further attention is the heterogeneous feedback effect. Previous literature has stressed that feedback helps mitigate or eliminate gender differences in willingness to compete by correcting biased beliefs of relative performance. For example, Wozniak et al. (2014) and Wozniak, Harbaugh, and Mayr (2016) show that providing relative performance feedback in number-adding tasks encourages highly skilled women to enter competitions and discourages low-skilled men. These studies show that feedback helps to close the gender gap in competitive behavior, and consequently improves economic efficiency through sorting by ability. Thus, feedback is said to be an alternative and perhaps a more efficient affirmative action policy than other policies studied in the literature (Balafoutas and Sutter 2012; Niederle, Segal, and Vesterlund 2013). In our setup, we diminish and control the informational value of feedback by assessing feedback effect on competitiveness across two different tasks and still show that feedback reduces gender differences in competitiveness.

One concern about the generalizability of our results is that all participants in our experiment were university students. If we consider that students will become employees, their behavior and preferences are of particular interest to researchers. Another concern is that lab experiments examine preferences and behavior in artificial environments with limited external validity. However, we anticipate that the observed feedback effect across two clearly dissimilar laboratory tasks could have underestimated the true extent of the potential feedback spillover effect on competitiveness in the field where boundaries between tasks are more blurred. Moreover, a recent study by Buser et al. (2014) shows that competitiveness measured in the laboratory strongly predicts students' academic track and career choices. Extrapolating their results to our experiment, our findings suggest that feedback may have a long-term effect on students' choices of competitive academic tracks. Consequently, decisions to pursue highly paid careers may have psychological roots in earlier path-contingent yet unrelated events.

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APPENDIX A

bria-52583_Online_Appendices: http://dx.doi.org/10.2308/bria-52583.s01