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REVENUE-SHARING TEAMS WITH REMOTE WORKERS

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ABSTRACT

Remote work policies remain controversial because of the perceived opportunity for increased shirking outside of the traditional office; a problem that is potentially exacerbated if employees work in a revenue-sharing team environment. Using a controlled experiment, where individuals are randomized to different work locations (remote or an office-like setting), we examine how remote work impacts effort choices under individual pay schemes and in revenue sharing teams. Treatments vary the number of remote workers on a team. Our results suggest that work location alone does not lead to productivity differences. However, the location of partners does impact an individual's effort levels in revenue-sharing teams. Non-remote workers reduce effort as the number of remote partners increases, and remote workers increase effort as the number of remote workers increases. These results are driven predominantly by those who are relatively less productive as individuals. Post-experiment incentivized survey evidence points to expectations of partner productivity as a contributing factor.

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Instructions to replicate the experiment are available at
https://www.dropbox.com/scl/fi/dycuvzmwt0f2g0adfg6bq/Instructions-Telecommuting.docx?rlkey=7fg84xh59gbg1p4m9mqutu1sl&dl=0

1. Introduction

The COVID-19 pandemic ushered in a structural shift in the workplace, with remote work becoming a new norm. Before the pandemic, many workers performed at least some work in a location other than the traditional office, but this constituted only 7% of all work performed. During the pandemic, more than 60% of working time was remote. More recent estimates find that this has dropped to 28%, but this appears stable and still represents a relatively large proportion of all work (Barrero et al., 2023). However, this shift is not without push-back, with famous corporate examples including Zoom and Meta recalling their employees in 2023 (Barinka, 2023; Cerullo, 2023).

There are a number of benefits associated with remote work, including decreased transportation costs, reduced employee turnover, access to a larger potential workforce, and reduced overhead expenditures (Nilles, 1975; Mokhtarian, 1991; Piskurich, 1996; Baruch, 2000; Pinsonneault and Boisvert, 2001; Bailey and Kurland, 2002). Remote work also has its challenges, with most complaints linked to productivity (Bloom et al., 2023a). One classic argument lobbied against remote work productivity is that the improved outside options available to remote workers make working less attractive. Thus far, the evolving empirical evidence of work location on productivity is mixed between lower productivity in the remote setting to limited differences, or even positive effects (Bloom et al., 2015; Dutcher, 2012; Choudhury et al., 2021; Gibbs et al., 2023; Barrero et al., 2023; Dutcher and Saral, 2022; Emanuel and Harrington, 2024; Choudhury et al., 2024).

We argue that a key reason for the continued resistance to remote work is that organizations have become increasingly reliant on group-level work and incentives, and this is where remote work is more likely to have a detrimental effect on productivity. Some studies find that reduced group-level productivity of remote workers can be tied to more isolation/siloing and higher communication costs (Gibbs et al., 2023; Yang et al., 2022; Battiston et al., 2021). Additionally, group-level incentives transform a simple individual optimization problem, "how much should I work", into a strategic choice that must now also include expectations of partner performance, "how much are my partners working for the team and how much should I work for the team," which is likely more difficult to determine in the remote setting. The literature has largely focused on the direct effect of the remote setting, but fewer have looked at this more nuanced, indirect view. In Dutcher and Saral (2022), we use an experiment to isolate the role of beliefs about partner productivity when remote and non-remote (office-based) workers are placed under a minimum-effort group compensation scheme. Despite finding that office workers and remote workers were equally productive at an individual level, both types chose to give less effort to remote teammates than office-based teammates. This reduction in effort is due to (inaccurate) beliefs of lower productivity of remote partners. The important implication is the indirect effect of beliefs - even when no productivity differences emerge between workers by location, teams may still suffer productivity losses based solely on expectations of lower partner productivity in the remote setting.

In this paper, we examine both direct and indirect effects of remote work, and we contribute to the relatively small literature examining group-level incentives in the remote setting. We do this by using an experiment to examine if work effort choices change when individuals move from an individual piece-rate payment scheme to a revenue-sharing, team-based payment scheme and if these team-level effort provisions are affected by varying numbers of remote workers (0 to 2) in the team. Using the experimental methodology allows us to causally identify the effect of the remote environment on effort. While our focus is on group production operating under a revenue-sharing compensation scheme, our results are also relevant for related incentive schemes such as profit sharing, where workers split the revenue or profit generated within their organization.

Revenue-sharing, even in the traditional office setting, is often criticized since it creates well-known incentives for free-riding behavior (Holmstrom, 1982; Alchian and Demsetz, 1972). Despite these drawbacks, group-incentive schemes are commonly employed by organizations and have even been shown to enhance worker performance (Hamilton et al., 2003; Boning et al., 2007; Jones et al., 2010; Fang, 2016). Reasons attributed to its successful implementation include peer-monitoring, worker autonomy, and other-regarding preferences such as preferences to help, preferences to cooperate, or guilt aversion (Fischbacher et al., 2001; Hamilton et al., 2003; Blasi et al., 2010; Babcock et al., 2011; Kuhn and Villeval, 2015; Cooper et al., 2021; Freeman et al., 2022). Arguably, however, the incentives for free-riding make revenue sharing more likely to be negatively impacted by remote work than other group-incentive schemes as it may reduce the efficacy of the mechanisms described above - more difficult peer-monitoring, increased incentives to free-ride, and more social distance which may lower other-regarding preferences (Mas and Moretti, 2009; Hoffman et al., 1996; Rosaz et al., 2016).

To explore the impact of remote work under revenue sharing, the real-effort experiment we designed had participants work on team production tasks and were randomly assigned to either a traditional structured office-type location or an unstructured location of their choice, in which case subjects completed the experiment remotely. The use of experiments can serve as a useful complement to studies that use observational data as it eliminates selection issues of workers into teams and workplaces. It also allows for tight control over the environment, such as the incentive scheme and availability of outside options, and it allows for direct observation of effort under randomized treatment settings, which in this case are teams with varying numbers of remote workers.

Our participants were drawn from a subject pool of undergraduate and graduate students at the University of Innsbruck (Austria) and were asked to perform a real-effort typing task where subjects had to correctly decode a string of 6 letters into a set of 6 numbers. All subjects participated initially under a piece rate individual payment scheme. Following the individual stage, the subjects were randomly matched with two partners to participate in a team stage that mimicked the individual stage in all aspects except that payment was a revenue-sharing scheme. The same wage rate used for the piece rate was paid to the cumulative output of all three members, and then this payment was divided equally among the three teammates. After the initial team stage, the participants were again re-matched with new teammates for two additional team rounds. The team rounds varied the location composition of the other two team members between two remote, one remote and one non-remote, and two non-remote partners. This design allows us to investigate how moving from teams with no remote partners to teams with all remote partners changes effort decisions. At the end of the experiment, we asked a series of questions that included incentivized belief questions regarding the productivity of partners, an incentivized risk preference measure, and demographic questions

Our findings in the paper include the following. First, using data from our piece rate setting, we add additional evidence to the comparison of individual productivity in remote versus non-remote workers, finding no significant differences in productivity by location at an average level. Second, despite cutting the marginal incentive for work by two-thirds in the revenue-sharing teams, when compared to the individual output, we do not observe free-riding in the team stages. Third, even though we do not observe free-riding, we do observe differing effort choices based on the location of partners. The non-remote participants decrease their effort as the number of remote partners in their team increases. In contrast, remote participants increase their effort with remote partners. This result is driven primarily by low-productivity types (defined by performance under the piece-rate first stage). We also explore additional findings on beliefs and gender. We demonstrate that beliefs of partner productivity remain integral to team effort choices (as in Dutcher and Saral (2022)). Our exploration of gender finds that males are much more responsive to work location than females, mirroring a similar finding from Dutcher (2012)

The remainder of this paper is structured as follows: The experimental design is laid out in Section 2 and predictions are covered in Section 3. Our findings are presented in Section 4 and Section 5 concludes.

2. Experiment Design

The experiment was designed around two types of participants: **Laboratory workers (L)** who are individuals recruited to participate in the laboratory at a pre-specified time and

Remote workers (R) who are individuals recruited to participate online at a place of their choosing within a 24 hour block of time. To avoid self-selection issues, the assignment of location to participants was random and the protocol of the experiment was conducted in exactly the same manner for laboratory and remote worker subjects. After the initial recruitment, subjects that would participate in the laboratory were instructed of the time and place to participate via e-mail. Remote subjects were sent an e-mail that directed them to a website with a link that contained an installer for the client-side of the experiment software which would connect to the university server for these subjects to participate online. The remote subjects were instructed to participate in a location of their choice and informed that they had 24 hours to complete the experiment. The participation (show-up) rate was similar for participants recruited to both locations. In what follows, we layout the timeline and details of an experimental session.

The set of experimental instructions can be found at the following link: Instructions.

All subjects participated in a series of four 8-minute rounds that gave them the option to spend their time on a paid typing task or unpaid games of tic-tac-toe. The remote subjects, by nature, could also spend their time on other outside options not available to the laboratory subjects, but neither type were explicitly informed of this. The paid typing task required subjects to decode a series of 6 random letters into a series of 6 numbers using a code that changed with each combination of letters to minimize learning effects. All subjects received the same random sequence of letters and code in each round. Prior to the set of paid rounds, all subjects participated in an unpaid practice period of the typing task and tic-tac-toe to familiarize themselves with the work interface that would remain the same for the duration of the experiment.

The first paid round was an individual round where each correctly coded set of letters paid a piece rate of 8 €-cents. The last three rounds were team rounds, where subjects were partnered with two other participants into a team of three. While the actual work performed by an individual did not change between team and individual rounds, the compensation was shifted to a revenue-sharing payment scheme in the team rounds where the team was paid 8 €-cents for the total output of all three members and this payment was split equally, reducing the marginal private payment for each paid task by nearly 67%.

Teams were purposefully minimalistic with no interaction to avoid confounds. All subjects were informed that no one in their team would ever observe any of their decisions and prior to each team round, we primed the subjects with their location and their partners' locations, which changed in each round. Partner types during the team rounds define our treatments:

1. **Mixed Partners (LR)** – each subject (remote and laboratory) was matched with one remote partner and one laboratory partner.
2. **Remote Partners (RR)** – each subject was matched with two remote partners.

3. **Laboratory Partners (LL)** – each subject was matched with two laboratory partners.

Order	Lab	Remote
1	LL, RR, LR (17)	RR, LL, LR (18)
2	LL, LR, RR (19)	RR, LR, LL (16)
3	RR, LR, LL (18)	LL, LR, RR (17)
4	LR, RR, LL (16)	LR, LL, RR (14)
Total subjects	70	65

Table 1: Treatment order and (number of subjects) in each session

The above treatments are defined for both types of subjects: remote and laboratory. Hence, we are able to analyze teams of three with the following compositions: LLL (all laboratory), LLR (two lab and one remote), LRR (one lab and two remote), and RRR (all remote).

We ran four orders of the team treatment for both types of subjects across 8 sessions, summarized in Table 1. We ensured that subjects knew the locations of the others they were matched with by giving them specific information about their teammates' locations. For example, in the two remote worker team treatment, the instructions for the lab participants specified that they would be placed into a team with two individuals participating in a location of their choice. Remote subjects in this treatment were told that they would be paired with one partner in the laboratory and one partner also participating in a location of their choice. Other treatments had similar wording, taking into account differences in a subject's perception of the location of partners based on their own location.

At the end of the four rounds, we elicited incentivized beliefs regarding the performance of their co-participants. For individual round beliefs, subjects were asked to guess the average performance of those who participated in the lab and the average performance of remote workers. For the team rounds, subjects were asked to guess the performance of their teammates. When their teammates were both from the same location, the subjects were asked to guess the average of both teammates, while in the mixed partner treatment, subjects were asked to guess the absolute performance of each teammate. Each question was incentivized using the following payoff equation paid in €-cents (based on Palfrey and Wang (2009)):

$$question\ earnings = 100 - (Actual\ Outcome - Guess)^2$$

A correct answer yielded a payment of €1. Incorrect answers were also paid, but as the distance between the respondent's answer and the correct answer increased, earnings decreased rapidly, providing strong incentives to provide accurate beliefs. Incorrect answers that would lead to negative earnings were capped at zero.

We also elicited risk preferences using a mechanism adapted from Eckel and Grossman (2008). Subjects were offered an incentivized choice between five binary 50/50 gambles where both expected value and risk are increasing in the order of gambles. Choosing a lower (higher) gamble corresponds to higher risk aversion (tolerance). The experiment ended with subjects filling out a non-incentivized survey.

The nature of the experiment required that payment be delayed for all subjects, so the payment protocol was identical for both remote workers and laboratory workers. Each subject received an e-mail Within 3 days of their participation, they were informed that their payment was ready and that they should bring their unique subject ID with them to collect payment. The experiment was incentivized to match the external labor market conditions with an average subject payment of € 13.87 – all of which was payment from the incentivized tasks for an experiment that took less than an hour to complete.

The experiment was programmed using Z-tree software (Fischbacher, 2007), and subjects were recruited through ORSEE (Greiner, 2004). The software used for the experiment made it very cumbersome to switch to an internet browser on the computer that was being used by the subject for the experiment and so the provided outside option of tic-tac-toe was mostly likely the only outside option employed on the participant's computer. The data was collected pre-Covid, in 2012, at the University of Innsbruck, Austria. The participants were recruited from a subject pool comprised of undergraduate and graduate students at the university. Subjects' anonymity was preserved through the use of a random number for identification and payment.

3. Predictions

The experiment is designed to measure productivity differences across different worker types (lab and remote) in individual work and revenue-sharing teamwork settings.

Productivity differences in the individual setting may arise because of the enhanced outside options of the remote worker. Under a traditional labor supply model, having additional and more valuable leisure outside options makes expending effort on the paid work task more costly, so the simple prediction of this model is that remote workers should be less productive than laboratory workers. In our experimental environment, the cost of effort in the laboratory is the value of time spent playing tic-tac-toe and in remote settings this could be the value of tic-tac-toe (TTT) or possibly another alternative available to the subject that generates higher utility (e.g. talking/playing on the phone, watching TV, etc.; see Dutcher et al. (2024) for a deeper discussion of controlled opportunity costs in experiments). As we are unable to observe the leisure options for the remote workers, but both remote and laboratory participants were given the same TTT outside option, we make the slightly less stark prediction: *remote workers should be no more productive than laboratory workers.*

Predicting behavior (generally) in team settings is more complex. In contrast to an individual work setting with optimization based solely on one's own cost of effort, work decisions in a team are strategic as a worker might also consider their teammates' productivity when choosing their own optimal work effort. Under revenue sharing, there are well-known incentives for free-riding (Alchian and Demsetz, 1972; Holmstrom, 1982), which may be exacerbated when working remotely as outside options are potentially more valuable. Under our design, we have reduced the private incentive of paid work by two-thirds. If a worker believes that their remote partners have highly valuable outside options and are less productive, we also have a relatively clear prediction for this setting: *a worker will reduce their effort when paired with remote workers under revenue sharing and this impact should be increasing in the number of remote partners*.

In addition to location, which we claim impacts the cost of effort through the additional outside options available to a remote worker, we must also consider ability. Revenue-sharing teams with heterogeneous ability levels (high and low ability) and limited complementarities introduce additional complications - even if team members are more cooperative and choose not to free-ride. There is ample evidence of conditional cooperation in experimental settings where free-riding is predicted that individuals will contribute to what their expectations of the average team output will be for the team (Fischbacher et al., 2001; Chaudhuri, 2011). However, revenue-sharing teams are less beneficial, pecuniarily for high-ability workers than low-ability workers, compared to a situation where they would be compensated on individual performance (Meidinger et al., 2003; Cooper et al., 2021), and this may trigger resentful behavioral responses by high-ability individuals where they reduce effort. Given our first two predictions, this could be exacerbated when paired in a team with remote teammates. In contrast, both high and low-ability workers may avoid reducing effort because of other disincentives, such as guilt from harming teammates (Bandiera et al., 2005) or other forms of group norms that foster cooperation rather than free-riding (FitzRoy and Kraft, 1986). Given these potential conflicting tensions, we leave it as an empirical exercise to determine the main impact of differing ability in this work environment.

4. Results

Table 2 provides an overview of the summary statistics for output and tic-tac-toe (TTT) play for the practice round, individual round, and the three team rounds by subject type. We also include average team output, which summarizes the output across all team treatments. The last column gives the mean difference between types. On average, we find no significant differences in overall productivity between treatments or types. One regularity that is surprising is the bump up in productivity between the individual round and team rounds ($p < 0.01$, two-

	Lab		Remote		Difference	
	mean	sd	mean	sd	mean diff.	t-stat
Practice Output	19.14	50.63	8.02	31.55	11.13	(1.54)
Individual Output	22.36	5.91	23.74	6.84	-1.38	(-1.25)
Individual TTT	4.10	7.43	2.00	4.96	2.10	(1.94)
Average Team Output	26.32	5.82	27.52	7.20	-1.19	(-1.05)
LL Output	26.59	6.15	27.09	7.89	-0.51	(-0.41)
LL TTT	2.20	7.27	1.45	4.98	0.75	(0.71)
RR Output	26.20	6.70	27.66	7.56	-1.46	(-1.19)
RR TTT	1.86	6.24	1.34	5.12	0.52	(0.53)
LR Output	26.19	5.33	27.80	7.14	-1.61	(-1.48)
LR TTT	1.80	4.59	1.52	5.80	0.28	(0.31)
Observations	70		65			

Table 2: Summary Statistics

sided t-tests for both L and R types). The individual round is always played first, so it may be an additional learning effect from the training period, which necessitates control for the period of play (round) in our regression analysis. However, the simultaneous reduction in TTT also appears to suggest that free-riding is not the chosen strategy in this environment. As an exploratory session, we also ran additional orders for 32 remote workers with the individual stage at the end. In this data, we do not find the same drop off from team to individual productivity, but we also find no evidence of free riding (26.69 for the team rounds vs. 29.06 for the individual round, $p = 0.09$).

The lack of free riding is even more surprising given that the anonymity of partners is maintained throughout the experiment, and partner performance was never explicitly revealed to the participants. However, this result is consistent with other studies that have also found a productivity increase in revenue-sharing teams with standard leisure outside options (Hamilton et al., 2003; Kuhn and Villeval, 2015; Babcock et al., 2011). This stands in contrast to the results in Croson (2001); Cooper et al. (2021); van Dijk et al. (2001) where the outside option is not a typical real leisure choice, but instead is a paid alternative to the revenue sharing team. We return to this issue in our discussion in Section 5.

To present these claims on firmer statistical ground, Table 3 provides the results of a random effects regression with standard errors clustered at the subject level. The first two specifications focus on comparing output in the individual versus team rounds, while the final two models focus on team-level behavior as the number of remote subjects increases. The primary variables of interest are *Individual Round (Ind.)* (a binary for the individual round), *Remote* (a binary equal to 1 if the subject was remote), and *# of Remote in Team* (the number of remote teammates a subject is teamed with, which ranges from 0 to 2, or LL (0) to LR (1) to RR(2)). We include

	(1) Ind. vs. Team	(2) Ind. vs. Team	(3) Team Alone	(4) Team Alone
Individual Round (Ind.)	-3.967***	-2.070***		
	(0.509)	(0.378)		
Remote	1.194	-1.124	-1.424	-3.190**
	(1.131)	(1.447)	(1.417)	(1.508)
Ind. × Remote	0.187	0.517		
	(0.663)	(0.722)		
Round		0.471**	0.484**	0.483**
		(0.196)	(0.196)	(0.197)
Remote × Round		0.517	0.479	0.519
		(0.315)	(0.305)	(0.320)
Female		0.583	1.266	0.552
		(1.034)	(0.882)	(0.568)
Tic-Tac-Toe		-0.444***	-0.478***	-0.473***
		(0.039)	(0.087)	(0.051)
Risk Preference		-0.411	-0.295	-0.434**
		(0.314)	(0.281)	(0.186)
# of Remote (R) in Team			-0.278	-0.277
			(0.187)	(0.187)
Remote × # of R in Team			0.644**	0.652**
			(0.286)	(0.291)
Ability (Ind. Output)				0.574***
				(0.083)
Constant	26.324***	26.358***	25.954***	13.901***
	(0.696)	(1.526)	(1.502)	(2.177)
Observations	540	540	467	436
Number of subjects	135	135	166	166

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Random Effects GLS regression on output.

Round to control for learning or fatigue, *Female* to control for gender, *Tic-Tac-Toe* to control for the number of TTT games played, and *Risk Preference* to control for risk, which measures risk tolerance on integer values ranging from 1 to 5, with higher risk tolerance represented by higher values of this measure. We also include two interactions of *Remote* with *Round*, *# of Remote in Team*, and *Ind* to better understand differential effects from remote subjects. The last specification also includes *Ability*, which we measure as round 1 performance (the individual stage). We define high (low) productivity types as those performing above (below or equal to) the median output, which for both remote and laboratory workers is 23 correct tasks.

The regressions largely support what is observed from the summary stats. From the first two models, we observe an increase in output when individuals move from the individual to the team rounds, and this effect does not depend on the location. When we focus on the team rounds, the coefficient on the number of Remote subjects in a team is negative for lab participants, indicating they reduce their effort as the number of remote teammates increases, but this does not quite reach statistical significance in either of the last two models. In contrast, the remote subjects have a more positive response to the number of remote workers in their team, increasing output as the number of remote teammates increases. Column 4 includes our measure of ability. We do note that because participants were given an outside option, output in the individual round is not a perfect measure of ability and perhaps more aptly referred to as high and low productivity. Nevertheless, including this measure results in the coefficient on Remote becoming negative and significant. This suggests that while the analysis of averages shows an overall consistency between remote and lab workers, we are missing important heterogeneity that is driven by productivity in the first round (or ability types).

To better understand the effects of ability, Figure 1 presents a box plot of output (top) and TTT play (bottom) across the four rounds of play, organized by treatment. In all plots, the whiskers represent the interquartile range and the line inside the box represents the median for that particular group. Examining output first (top box plots), the bump up from individual to team rounds appears strongest for low types in the lab and working remotely. The variation in output is also higher for low types in both locations. A number of low types choose zero team effort, which is never observed for high types. Turning to the outside option, tic-tac-toe (TTT) play, one clear trend that emerges is that low productivity types engage in playing TTT more than high types, although play is observed for all types and in all treatments. For low types, we see a drop off between the individual round and team rounds for many, but some increased TTT play in the team rounds. Comparing TTT play across locations, it appears as if remote players are using it less often. Of course, this does not necessarily imply that they were using this time to engage in team tasks. They had additional outside options available to them, by definition of playing remotely, that may have also been utilized.

We return to regression analysis to identify trends by high/low type. Table 4 presents the same specification used in Model 3 of Table 3, which isolates the data to the team treatments. We split the data by Low (Model 1) and High (Model 2) types. Model 1 demonstrates that the response to the number of remote subjects in a team is driven by low types. For those in the lab, they reduce their effort as the number of remote teammates increases, and this effect is more negative than the remote subjects who respond positively to the number of remote workers in their team (post-estimation testing # of Remote in Team + # of Remote in Team $\times$ Remote $= 0$ for low types yields $p = 0.028$).

Having established the core treatment results, we now turn to supplemental results on beliefs

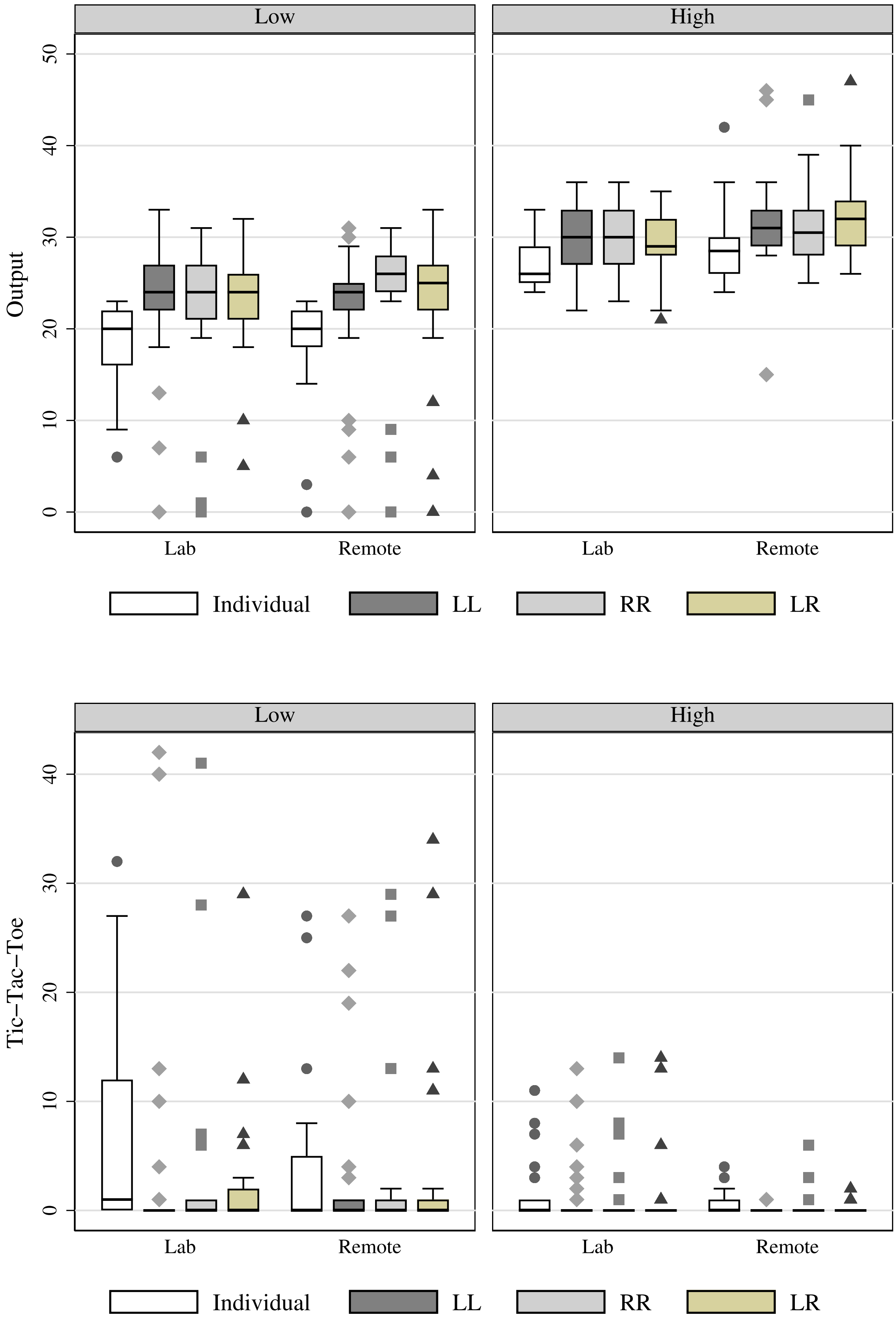

Figure 1: Boxplot of Output and TTT by Type (High/Low) and Location (Lab/Remote)

	(1) Low	(2) High
Remote	-2.643	-0.560
	(1.943)	(2.334)
# of Remote (R) in Team	-0.565*	0.038
	(0.317)	(0.120)
# of R in Team × Remote	1.039***	0.105
	(0.383)	(0.462)
Round	0.512	0.557***
	(0.334)	(0.129)
Round × Remote	0.304	0.588
	(0.435)	(0.483)
Female	1.982*	-1.746*
	(1.101)	(0.990)
Tic-Tac-Toe	-0.508***	-0.494***
	(0.055)	(0.110)
Risk Preference	-0.595*	-0.466
	(0.347)	(0.301)
Constant	24.258***	30.517***
	(2.042)	(1.302)
Observations	210	195
Number of subjects	70	65

Robust standard errors, clustered at the subject level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Random Effects GLS regression on output by High/Low Productivity Type.

based on location and gender differences. We begin with an investigation of beliefs driven by location. In our predictions in Section 3, a few reasons were provided for why beliefs may impact effort choices. These explanations generally revolve around the impact of free-riding by other group members. If workers do not want others to free-ride and also choose not to free-ride, then they may try to match the output level of the others. However, because they cannot observe their teammate's output prior to making their own effort choice, they must form beliefs about what they expect this output choice to be. This builds upon our work in Dutcher and Saral (2022) where we identify a role of beliefs by the location of partners in the minimum effort setting.

As an overview of beliefs, Figure 2 displays the correlation between beliefs over which locational types of partners are most productive and behavior. Following Dutcher and Saral (2022), we focus on two team treatments: one where there were the most remote subjects on their team (RR) and one where there were the least remote subjects on the team (LL). This

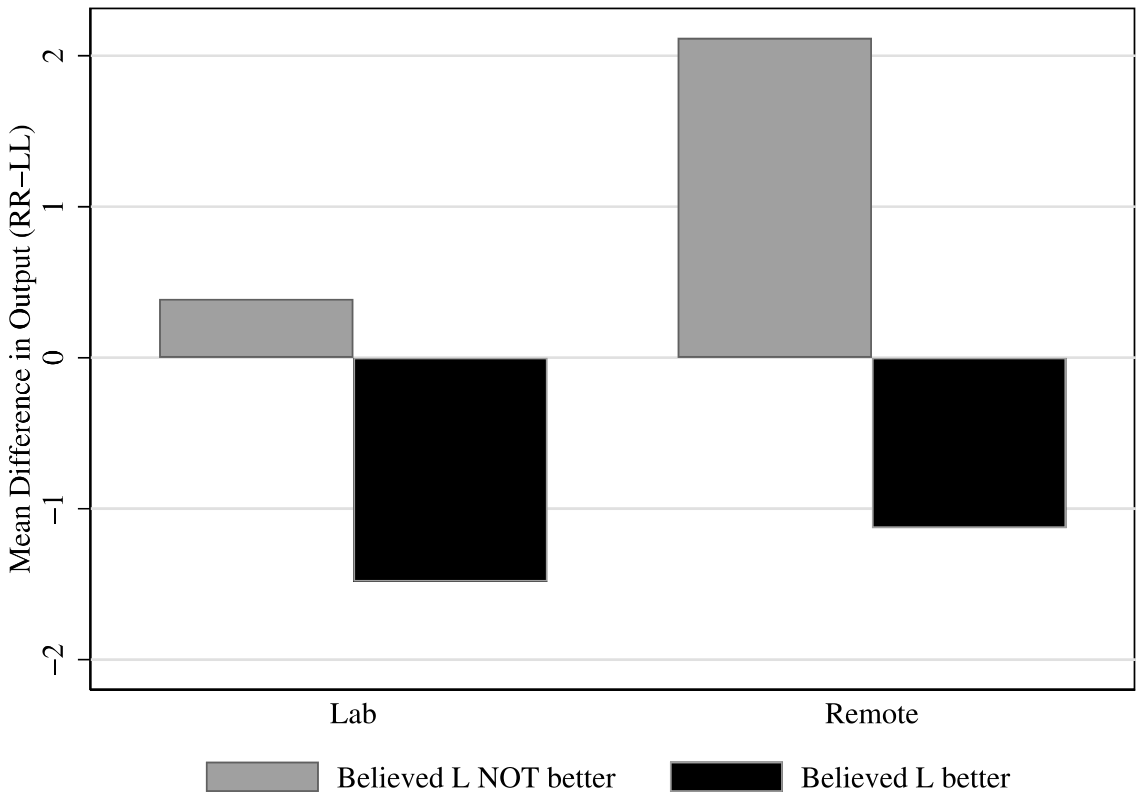

Figure 2: Correlation of beliefs with output.

provides a clean comparison because it is in these two settings where both partners are of the same type - they are either both R or both L. The vertical axis is the average difference in output when paired with two Rs versus two Ls (RR-LL), and the bars separate those who had beliefs that their laboratory (L) partners were NOT more productive (L NOT better, including those who assume no difference) versus those who held beliefs that the L partners were more productive (L better). Recall that these are incentivized beliefs based on the questions of the average output of LL partners and the average output of RR partners. Beliefs generally align with behavior given that those who believed lab teammates were better contributed more to the teams with more L subjects than those who did not hold these beliefs (two-sided t-tests show differences on the pooled sample, $p \leq 0.01$ and broken down by type: $p = 0.019$ for lab and $p \leq 0.01$ for remote). We explore this further in Table 6, which displays the results of an OLS regression that matches Figure 2 but adds additional controls. The second model differs from the first by including an interaction term of the beliefs variable with Remote. From the regression, the negative correlation between beliefs and output is confirmed for both types (post-estimation test for remote, $p < 0.01$). We find no significant differences in the responses to beliefs between remote and lab participants.

	(1)	(2)
Difference in Beliefs (RR - LL)	-2.630***	-1.902**
	(0.631)	(0.883)
Remote	0.953	1.618*
	(0.636)	(0.850)
Difference in Beliefs × Remote		-1.487
		(1.262)
Female	1.138*	1.167*
	(0.665)	(0.664)
Tic-Tac-Toe	-0.004	0.002
	(0.054)	(0.054)
Risk Preference	-0.104	-0.111
	(0.224)	(0.224)
Constant	0.512	0.206
	(1.030)	(1.061)
Observations	135	135
R-squared	0.150	0.159

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: OLS regression on Differences in Output when Partnered with RR vs. LL teammates.

Empirical regularities on gender are also worth exploring in our study. Since at least Bélanger (1999), it has been shown that females are more likely to work remotely. Barrero et al. (2023) highlights that even after the pandemic, females desire more remote work than males (4% more) and are still more likely to spend time in this work arrangement (2% more). Dutcher (2012) provides one potential explanation for why managers may allow more females to work remotely, given the results in that study are primarily due to the productivity differences between remote vs. office males. Females, on the other hand, did not respond to the differences in the work setting. These empirical regularities suggest that males and females may respond differently to the remote setting.

Figure 3 gives a distributional view of output choices by female (top row) and male (bottom row). The first column gives the individual output choices, broken down by remote and laboratory workers and the second column presents average team output across all treatments, also broken down by location. For remote participants, there are 33 females and 37 males. In the laboratory, there are 41 females and 24 males. In line with Dutcher (2012), males appear to be more responsive to the remote environment than females. Both remote and lab females

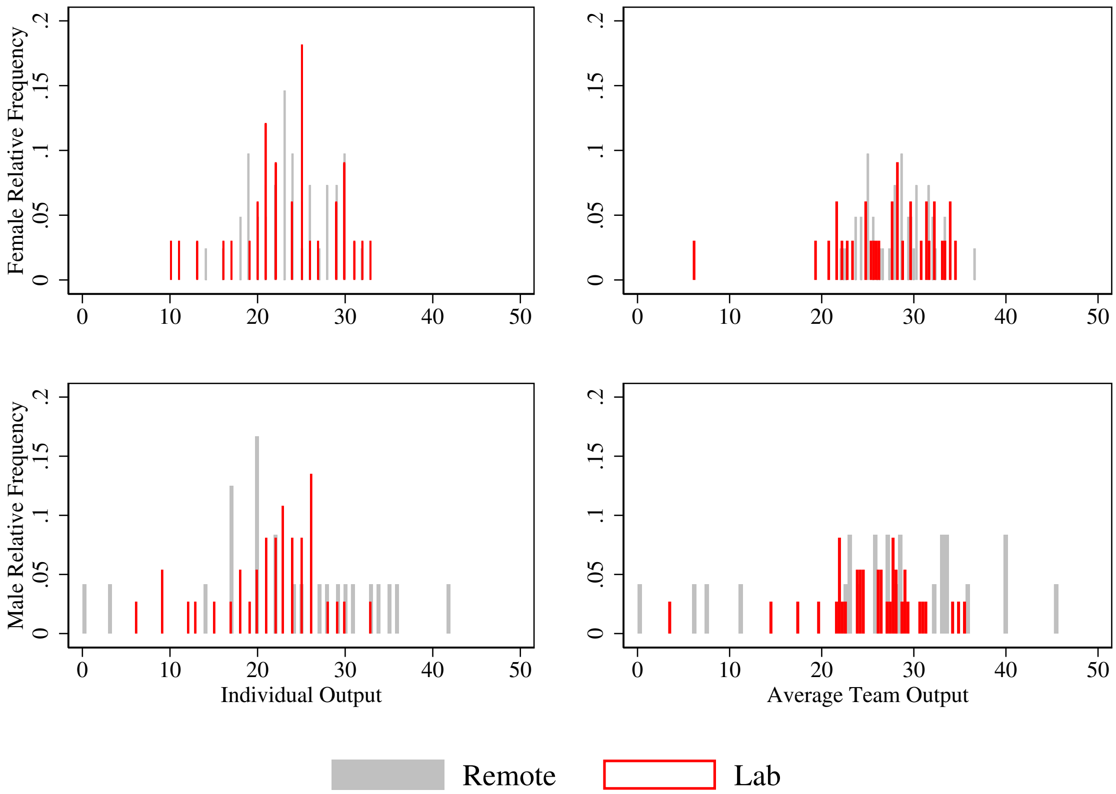

Figure 3: Gender Differences in Output

are distributionally similar, with less variation in both individual and team settings. Lab males appear similar to lab females. Remote working males, on the other hand, have a much wider variation with both higher effort and lower effort levels. To test this more formally, Table 6 replicates the specifications used in Table 4, with the exception of an additional interaction of Female and Remote dummy variables. The regressions demonstrate that remote females who are classified as a low type are better than their male counterparts, but remote females who are classified as a high type are worse than their male counterparts.

5. Conclusion

Group-level incentive plans and teams have been widely adopted by firms. A survey of Fortune 1000 companies revealed that 70% of these large firms use some form of team incentives, (Lawler and Mohrman, 2003) and a report by Deloitte emphasized that organizations are turning into a "network of teams" (Schwartz et al., 2016). The high rate of adoption may seem surprising given the well-known incentives for free-riding (Holmstrom, 1982), yet the empirical evidence appears to suggest that free-riding is not that pervasive (Hamilton et al., 2003; Boning

	(1) Low	(2) High
Remote	-5.231**	2.295
	(2.302)	(2.472)
# of Remote (R) in Team	-0.563*	0.040
	(0.316)	(0.120)
# of R in Team × Remote	1.039***	0.099
	(0.384)	(0.462)
Round	0.509	0.546***
	(0.333)	(0.131)
Round × Remote	0.305	0.605
	(0.436)	(0.486)
Female	-0.254	0.450
	(1.231)	(1.107)
Remote × Female	4.876**	-4.802**
	(2.343)	(1.959)
Tic-Tac-Toe	-0.502***	-0.453***
	(0.048)	(0.106)
Risk Preference	-0.594*	-0.634**
	(0.347)	(0.280)
Constant	25.153***	29.845***
	(2.045)	(1.323)
Observations	210	195
Number of subjects	70	65

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Random Effects GLS Regression on Output by Productivity Type, Focusing on Gender.

et al., 2007; Jones et al., 2010; Fang, 2016), possibly because of co-monitoring Freeman et al. (2010) or because revenue sharing teams trigger other pro-social preferences (Hamilton et al., 2003; Cooper et al., 2021; Freeman et al., 2022). However, with the rise of remote work, previously successful incentive schemes must be reevaluated for their effectiveness. When workers move to a remote environment, office-like monitoring is more difficult, social distance between peers is higher, and outside options are potentially more valuable.

Our study utilizes a real-effort experiment to help understand how work behavior changes under revenue sharing group-compensation when some in the team work remotely. As a baseline, we first examine work effort choices under individual incentives, and these same participants - who were randomly assigned to be either remote or non-remote (lab) - are placed into revenue-sharing teams of three with treatments varying the location of the partners they are working with. We measure effort in terms of completed typing tasks and also include an unpaid leisure option of playing tic-tac-toe.

Our first finding is that regardless of workplace location, we find a general lack of free-riding when individuals move from an individual-based piece-rate setting to a team-based revenue-sharing setting. This finding aligns with experimental evidence in similar set-ups to ours, e.g. Kuhn and Villeval (2015), but we interpret this result cautiously. One thing of note about our environment is that while we did have an outside leisure option of tic-tac-toe, it was unpaid, and the value to individuals is likely idiosyncratic and unmeasured. The structure of the experiment matches revenue-sharing workplace environments where the only paid task available to workers is the task that generates profits for others, and alternative leisure tasks, e.g., checking social media, are also available but unpaid. We find that tic-tac-toe was predominately played by non-remote low productivity/ability types, and overall tic-tac-toe play was highest in the individual round with play dropping substantially in the team rounds. The drop in leisure from the individual to the team rounds further supports that subjects did not want to free-ride in teams.

Turning to treatment differences in the 3-person team stages, we find that average output does not vary with the number of remote partners, but these averages mask the more nuanced insights driven by differing worker types. Using individual stage performance as an ability/productivity type measure, we see that low-type laboratory subjects decrease their effort as the number of remote workers in their team increases, which is counter to the positive impact seen by low-type remote workers. These responses, even though significant, are not large, representing an approximately 4% productivity decline for non-remote subjects partnered with two remote and an equal 4% productivity bump for remote subjects partnered with two remote teammates. However, the production periods were only 8 minutes, and the office workers had little else to do during the experiment but play tic-tac-toe, so we conjecture that the negative effect may be magnified in longer work periods or for workers with more attractive outside options and the positive effect may be reduced in either of those cases.

Next, aligned with the argument that indirect effects matter for effort decisions in teams, we observe that expectations of teammates' productivity are an important driver of own effort choices. Beliefs that partners will be less productive drives a negative response in one's own effort choice. This is aligned with Dutcher and Saral (2022) who find the same in a minimum effort setting. However, in contrast to the minimum effort group payment scheme where the Nash equilibrium prediction is to match the contribution of partners rather than free-ride, revenue sharing actually induces free-riding. As noted above, we find limited evidence of free-riding, thanks to the low value of the outside option, so these results are more likely arising from location-driven beliefs of partner productivity.

Last, we find that males are much more responsive to the remote environment than females.

Some caution is warranted with this result because our experiment is not explicitly designed to measure gender impacts, so we present this as an exploratory analysis to guide future research on this topic.

Our focus is on effort, which is an important aspect of a team's output, but we do not mean to imply it is the only input. For instance, increased difficulty in communication between teammates, the lack of good technologies available to them, or a "coordination tax" may negatively affect a remote team even if they are exerting high effort on the team task (Smith and Chaker, 2024). Yet, no matter the production function, effort is necessary, implying the results in this study apply broadly irrespective of the other inputs in a specific firm's production function. Existing studies have examined other determinants of output in remote vs. office teams. For instance, recent work has shown that when the task performed in the team is creative, virtual interactions reduce ideation Brucks and Levav (2022). Using a similar creative task, Dutcher and Rodet (2021) found that the number of creative ideas increases when teams work apart, but the ideas of those working face-to-face were more unique, primarily because face-to-face teams could take advantage of the team's diversity. What is left unexplored is how the task may interact with effort when workers move to a remote setting and how this may additionally interact with the payment scheme used. Future research can also examine how the remote setting affects teams that are not as minimalist as in our setting, where there may be worker complementarities, synergies, or learning in teams. Additionally, it is unclear how monitoring, including peer monitoring, may work in team settings where some or all of the team is remote.

Our findings are also relevant for hybrid work arrangements. According to Bloom et al. (2023b), hybrid work is more popular within firms than fully remote, and executives expect continued growth of the hybrid work arrangement. One reason for the popularity of this work modality may be that it does not hamper output (Choudhury et al., 2024; Angelici and Profeta, 2024; Bloom et al., 2024). Most of the work performed in these studies can be classified more as individual work as teamwork appears to be minimal. In an ideal world, the hybrid arrangement may have individuals coming into the office at the same time when teamwork is needed and working remotely when no interactions are necessary. In practice, however, there are issues with this implementation Christian (2023). Coordination issues imply that the teams will often be asynchronous, with some working remotely and some working in an office-like setting, and our mixed teams can provide evidence of how this kind of work arrangement affects effort provisions. Of course, hybrid work settings are not as sterile as our design, where hybrid arrangements attempt to build upon other important determinants of team output, such as social ties, which are known to be impactful in team production Eckel and Grossman (2005). What is left unexplored is how this kind of arrangement maintains social ties and if such an effect would alter effort provisions in mixed teams relative to what is found in our study.

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