The Effect of Incentives in Non-Routine Analytical Team Tasks*

Florian Englmaier† Stefan Grimm‡ Dominik Grothe§
David Schindler¶ Simeon Schudy‖

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Abstract

Despite the prevalence of non-routine analytical team tasks in modern economies, little is understood regarding how incentives influence performance in these tasks. In a series of field experiments involving more than 5,000 participants, we investigate how incentives alter behavior in teams working on such a task. We document a positive effect of bonus incentives on performance, even among teams with strong intrinsic motivation. Bonuses also transform team organization by enhancing the demand for leadership. Exogenously increasing teams' demand for leadership results in performance improvements comparable to those seen with bonus incentives, rendering it as a likely mediator of incentive effects.

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†florian.englmaier@econ.lmu.de, LMU Munich, Department of Economics & Organizations Research Group (ORG) & CEPR & CESifo.

‡stefan.grimm@econ.lmu.de, LMU Munich, Department of Economics.

§dominik.grothe@econ.lmu.de, LMU Munich, Department of Economics.

¶d.schindler@tilburguniversity.edu, Tilburg University, Department of Economics & CESifo.

‖simeon.schudy@uni-ulm.de, Ulm University, Institute of Economics & CESifo.

1 Introduction

Until the 1970s, a major share of the workforce performed predominantly manual and repetitive routine tasks with little need for coordination in teams. Since then, the work environment has rapidly changed. Nowadays, work is frequently organized in teams (see, e.g., Bandiera et al., 2013), and a large share of the workforce performs tasks that require a greater amount of cognitive effort compared to physical labor.

Examples include teams of IT professionals, specialist doctors, and management consultants. These teams often face a series of novel and complex problems and need to gather, evaluate, and recombine information to succeed, frequently in a limited amount of time. Autor et al. (2003) analyze task input in the US economy using four broad task categories: routine manual tasks (e.g., sorting or repetitive assembly), routine analytical and interactive tasks (e.g., repetitive customer service), non-routine manual tasks (e.g., truck driving), and non-routine analytical and interpersonal tasks (e.g., forming and testing hypotheses). They document a strong increase in the latter category between 1970 and 2000. Autor and Price (2013) reaffirm the importance of these tasks in later years.

Given their pervasiveness in modern economies and their importance for innovation and growth, understanding the determinants of performance in these tasks is crucial. One core question is how monetary incentives affect team performance in such cognitively demanding, interactive, and diverse tasks. While there is well-identified evidence about the behavioral effects of monetary incentives on performance in mechanical and repetitive routine tasks such as fruit picking, tea plucking, tree planting, sales, or production (see, e.g., Bandiera et al., 2005, 2013; Delfgaauw et al., 2015; Englmaier et al., 2017; Erev et al., 1993; Friebel et al., 2017; Hossain and List, 2012; Jayaraman et al., 2016; Lazear, 2000; Shearer, 2004), evidence on the effects of bonus incentives is scarce for non-routine analytical tasks where teams collaboratively solve complex problems.[1]

[1] This study focuses on performance-related bonus payments that firms may use as part of their annual incentive plans. The 2021 CAP-WorldatWork Incentive Pay Practices Survey (`https://worldatwork.org/resources/research/incentive-pay-practices`) indicates that both short- and long-term incentives are prevalent among a variety of companies from different sectors (>90% of which use short-term incentives and >50% use long-term ones) with, on average, 76% of firms using annual incentive plans. However, the use of different annual incentive pay components varies substantially across firms and levels, rendering the question of whether bonus incentives work in non-routine tasks crucial from a practitioner's perspective. For a more general discussion on the use of performance-related bonus payments as part of compensation in firms, see also Prendergast (1999), Lazear (2000), Oyer (2000), Lazear and Oyer (2013), Moynahan (1980), and Churchill et al. (1993). For theoretical motivations to use simple binary payment schemes, see, e.g., Fehr et al. (2007), Larkin and Leider (2012), Herweg et al. (2010), and Ulbricht (2016).

The efficacy of incentives may substantially differ in non-routine analytical team tasks for several reasons. First, they are often performed by people who enjoy their challenging nature and are intrinsically motivated (see, e.g., Autor and Handel, 2013; Delfgaauw and Dur, 2010; Friebel and Giannetti, 2009).[2] In turn, extrinsic incentives could negatively affect team performance by crowding out workers' intrinsic motivation (e.g., Deci et al., 1999; Eckartz et al., 2012; Gerhart and Fang, 2015; Hennessey and Amabile, 2010). Bénabou and Tirole (2003) provide a theoretical framework formalizing arguments for crowding out based on the idea that incentives may alter workers' perception of the task or their own ability. For example, they may infer from the existence of incentives that the task is less enjoyable than expected or that incentives are likely implemented for less able or less intrinsically motivated workers.[3] Further, as non-routine tasks are generally multidimensional, incentives may lead to crowding out due to a substitution of effort (Holmstrom and Milgrom, 1991). As these tasks require information acquisition, information recombination, and creative thinking, there is thus room for performance incentives to discourage activities not included in the relevant performance measure, such as the autonomous exploration of new and original approaches (e.g., Amabile, 1996; Azoulay et al., 2011; Ederer and Manso, 2013; McCullers, 1978; McGraw, 1978).

Second, the efficacy of incentives may differ as output could be a noisier function of effort than in routine tasks. In particular, optimal team production in non-routine tasks likely requires more coordination of individual efforts than in routine team tasks, potentially reducing the efficacy of any incentives that do not specifically stimulate such coordination. In a similar spirit, incentives may be less effective in non-routine tasks as workers may possess less knowledge about the production function or because these tasks are typically found in fields for which employees may already have large incentives to perform well (due to intrinsic motivation, status, recognition, or career concerns).

[2] Intrinsic motivation may stem from direct task utility (and thus reflect lower levels of or lower marginal effort costs), or from benefits beyond the production outcome such as additional utility due to self- or social signaling motives (Bénabou and Tirole, 2003, 2006), or from greater goals attached to the activity (such as job mission; see, e.g., Cassar, 2019). We do not consider greater goals or job missions to be necessary in all non-routine team tasks. However, we believe that both direct task utility and benefits beyond the production outcome are often relevant in non-routine analytical team tasks. Even without greater goals, their challenging nature renders these tasks interesting, and by performing well, agents can signal their ability (to themselves and others).

[3] As such, incentive effects may also interact with whether the task is perceived as interesting (Takahashi et al., 2016).

Third, incentives may be less effective in team settings as free riding could be present. The output produced by some workers can be misattributed to the work of others, and, additionally, team incentives reward the overall team output instead of individual contributions (Holmstrom, 1982). Fourth, salient incentives may also alter team organization (Englmaier et al., 2017). Particularly in non-routine tasks, incentives may create a demand for efficient leadership that enables teams to solve complex problems in a more coordinated manner. The variety of reasons for why incentives may work differently in non-routine analytical tasks is mirrored in substantial heterogeneity in experts' expectations about the efficacy of incentives, underscoring the need for clean empirical evidence on how incentives alter behavior in teams collaboratively performing non-routine analytical tasks.[4]

This study exploits a unique field setting to measure the effects of bonus incentives for behavior in teams collaboratively performing a non-routine analytical task. We study the performance of teams in a real-life escape game in which they have to solve a series of cognitively demanding quests to succeed (usually by escaping a room within a given time limit using a key or a numeric code). The task provides an excellent environment to study our research question as it encompasses several elements that are prevalent in many other non-routine analytical and interactive team tasks: teams face a series of complex and novel problems, need to collect and recombine information, and must solve analytical and cognitively demanding quests that require thinking outside the box. The task is also interactive since members of each team have to collaborate with each other, discuss possible actions, and develop ideas jointly. At the same time, real-life escape games allow for an objective measurement of joint team performance (time spent until completion) as well as for exogenous variation in incentives for a large number of teams.

Our setting is particularly flexible, allowing us to vary the incentive structure for over 700 teams (3,308 participants) under otherwise equal conditions and to replicate the main findings in a second, distinct sample of presumably less intrinsically motivated teams (268 teams, 804 participants). Further, it enables us to identify potential mechanisms behind the effects of bonus incentives by running an additional field experiment (281 teams, 1,273

[4]For instance, we document in an additional survey with HR experts that the range of predictions of incentive efficacy varies strongly. While the median HR expert expects 40 out of 100 teams to improve when facing incentives, 20% of them believe that between 0 and 20 teams will improve, while another 20% believe that 60–100 teams will improve (see Appendix Figure A.8 for the full distribution and Appendix Section A.16 for more details on the survey).

participants), hence substantially advancing the literature on the effects of incentives in collaboratively solved non-routine team tasks.

To identify the causal effects of incentives on behavior, we first conducted a series of field experiments with strongly intrinsically motivated teams (which were regular participants in escape games at *ExitTheRoom* (ETR), a firm we partnered with) who were unaware of taking part in an experiment.[5] We implemented a between-subject design, in which teams were randomly assigned to either a treatment or a control condition. For the main treatment, we offered a team bonus (of approximately €10 per participant) if the team completed the task within 45 minutes (the regular pre-specified upper limit for completing the task was 60 minutes). In the control condition, no incentives were provided.

We find that bonus incentives significantly and substantially increase performance. Teams in the incentive treatment are more than twice as likely to complete the task within 45 minutes. Moreover, in line with the idea that non-routine tasks feature an important noisy component in how effort translates into performance, bonus incentives not only induce a local effect around the threshold for receiving the bonus but also improve performance over a significant part of the distribution of finishing times.[6]

We then leverage the advantages of our setting and study in depth the most important aspects through which bonuses alter behavior in teams. To investigate the role of potential crowding out of intrinsic motivation, we use a three-pronged approach. First, Bénabou and Tirole (2003) argue that incentives may alter workers' perceptions and thereby crowd out their intrinsic motivation to exert effort and perform well. Indeed, it seems plausible that bonus incentives can serve as negative signals about the task or

[5]Harrison and List (2004) classify this approach as a natural field experiment. The study was approved by the Department of Economics' IRB at LMU Munich (Project 2015-11) and excluded customer teams with minors. In the general booking process, customers also gave written consent that data obtained at ETR could be shared with third parties for research purposes.

[6]Many non-routine tasks may feature a noisy production function or (low) effort elasticity, which may, in turn, reduce bunching around bonus thresholds or performance goals. In contrast, bunching can occur in routine tasks, where the relationship between effort provision and outcomes is more deterministic and oftentimes precise, and (real time) feedback about performance is available (see, e.g., Hossain and List, 2012; Allen et al., 2017; Kuhn and Yu, 2021). However, routine tasks may also not exhibit bunching resulting from strategic responses to incentives, e.g., when feedback is noisy, only provided on an aggregate level, or with delay. For instance, Friebel et al. (2017) do not find differences in the distributions of the percentage of sales (as a percentage of the target) between their treatment and control teams, indicating that their incentive condition did not result in bunching (we thank the authors for reporting these additional results to us). The latter aspects, as well as a potential lag of continuous outcome variables, may explain why several other field experiments related to bonus incentives in routine tasks (see Table A.1) do not report bunching.

a worker's type in our setting. Still, the results from our main treatment do not indicate substantial crowding out among strongly intrinsically motivated teams. However, our main treatment combines the bonus payment with a rather ambitious performance threshold (45 minutes), which could be interpreted as a positive signal about workers' ability. Further, this ambitious performance threshold itself could cause performance improvements (independent of the bonus incentive).

To test for such countervailing effects, we implement two additional treatment conditions. We first combine the bonus with a less ambitious performance threshold (60 minutes) and thus provide additional room for crowding out due to incentives. The second condition provides the ambitious (45 minutes) threshold as a reference point, signaling excellent performance but no monetary reward. The results from these treatments reveal that the observed performance improvements clearly result from the monetary reward provided and do not depend on which reference point they were combined with.[7] Hence, it is unlikely that the existence of the bonus incentive strongly crowded out teams' intrinsic motivation to solve the task quickly.[8]

Second, in the spirit of List (2003, 2004a,b, 2006), we contrast the findings from our natural field experiment with evidence from a second sample of 268 student teams (804 participants) who were paid to perform the same task as part of an economic experiment. These teams were likely less intrinsically motivated as they did not self-select into the task.[9] We find that despite potentially lower intrinsic motivation, bonus incentives similarly improve performance. Akin to the results from the field experiment, incentives more than double the fraction of teams that manage to solve the task within 45 minutes. As the incentive effect is of similar size, our findings suggest that the efficacy of the bonus incentive does not substantially interact with teams' intrinsic motivation.

Third, our setting furthermore offers us the opportunity to shed light on potential crowding out due to substitution in the spirit of Holmstrom and Milgrom (1991). Teams

[7] The latter findings also complement recent research on non-monetary means of increasing performance (for a review of this literature, see Levitt and Neckermann, 2014), in particular work referring to workers' awareness of relative performance (see, e.g., Blanes i Vidal and Nossol, 2011; Azmat and Iriberri, 2010; Barankay, 2010, 2012). Our finding, however, does not rule out that salient performance goals may further increase team performance, as observed, for example, in laboratory (Corgnet et al., 2015) and field experiments (Gosnell et al., 2020).

[8] Note that surveys among customer teams confirm that their main goal is to achieve success together and not to stay in the room as long as possible, independent of whether or not a bonus is offered (see also Table A.23).

[9] According to Harrison and List (2004), the student sample can be considered a framed field experiment as students are non-standard subjects in the context of real-life escape games.

could request external help when they were stuck by asking for (up to five) hints from ETR staff, which were not relevant for bonus payment eligibility. Interestingly, we find that incentives do not significantly reduce the willingness of teams to explore original solutions among likely more intrinsically motivated customer teams, who self-select into the task. However, we observe an increase in hint taking due to incentives among the presumably less intrinsically motivated student teams, who were paid by us to perform the task. Thus, our result highlights an important trade-off regarding substitutional crowding out when teams are not intrinsically motivated to explore on their own.[10]

As a next step, we shed more light on the mechanisms through which incentives operate. To better understand the role of teams' knowledge regarding the production function and potential stake size effects, we exploit natural variation in team size and experience among teams. We find that the efficacy of incentives does not substantially depend on team size, but incentives are more effective among experienced customer teams. This suggests that awareness of how effort translates into performance enhances the positive incentive effect.

Further, to study the role of team organization in more detail, we collect additional survey data among student teams. The surveys reveal an increased demand for leadership among treated teams and thus suggest that leadership is an important channel through which performance effects may come about.

To uncover the causal role of leadership demand, we then implemented an additional natural field experiment with 281 teams (1,273 participants) in the exact same setting. In this experiment, we exogenously varied the demand for leadership by nudging (or not nudging) teams to pick a leader. The experiment reveals a substantial positive effect of leadership demand on team performance. The findings are consistent with the idea that incentives may indeed enhance performance by encouraging team members to seek leadership and take initiative in coordinating and motivating others. As such, we conjecture that the impact of incentives goes beyond merely increasing individual effort; rather, they appear to provide the impetus for teams to endogenously adopt more structured forms of leadership.

Our field experiments, encompassing more than 5,000 participants, offer valuable insights for researchers as well as practitioners involved in designing incentive schemes for

[10]This interpretation is also in line with findings from additional customer surveys that indicate a strong relationship between own hint-taking behavior and image concerns regarding the latter (see Section 3.3.3 and Appendix Figure A.7).

non-routine analytical team tasks. In particular, we address a prevalent concern among many practitioners of whether monetary incentives impair team performance in tasks that are non-routine and require thinking outside the box. This concern has been widely propagated in public discourse, notably by best-selling author Daniel Pink through a TED Talk with over 19 million views and his popular book *Drive* (Pink, 2009, 2011). Our results alleviate these concerns in the context of teams collaborate on a rich and diverse non-routine analytical task. We provide novel and robust evidence that bonus incentives can be a viable instrument to increase performance in such tasks.

To put our findings in perspective, we briefly compare the incentive effects observed in our setting to other field experiments in the literature. In our natural field experiment, the difference in finishing time between treated and control teams amounts to about 0.44 standard deviations. In other work, for routine tasks, performance pay has been shown to improve performance with varying effect sizes (Bandiera et al., 2021). Effects range from 0 (Delfgaauw et al., 2020) to 0.90 standard deviations (Hossain and List, 2012).[11] Negative effects of incentives have rarely been observed in routine work environments and mostly when pay was low or when performing a routine task could signal prosocial behavior, such as in Hossain and Li (2014), who study the limits of crowding out in a routine data entry task. The authors find that low wages (as compared to no wages) only crowd out participation when a task is framed as a prosocial act but not when it is presented in a work frame or when crowding out does not occur conditional on participation. Complementing previous work, our findings thus suggest that monetary incentives can provide strong motivations to perform well.

Regarding field experiments involving tasks that are less routine in nature, our work draws parallels to research on incentives for teachers and health practitioners. For both professions, typical tasks require cognitive rather than physical effort and may involve (at least sometimes) novel and unknown problems. As such, we may consider these settings non-routine and analytical in nature (although it remains unclear if and to what extent complementarities exist). Studies on incentive pay for teachers yield overall mixed results (see, e.g., Fryer et al., 2022) and range from zero effects (Behrman et al., 2015) to 0.31 standard deviations (List et al., 2018, see also Appendix Table A.1). Evidence regarding

[11] See also Appendix Table A.1 and the discussion regarding the retail sector and other settings in Delfgaauw et al. (2020).

incentive pay for health workers is less abundant (Miller and Babiarz, 2014), and observed effects sizes are smaller (see Appendix Section A.1).

Regarding other non-routine tasks, our work contributes to the literature on incentives for idea creation (Gibbs et al., 2017) and creativity (e.g., Bradler et al., 2019; Charness and Grieco, 2019; Gibbs et al., 2017; Laske and Schroeder, 2017; Ramm et al., 2013). These studies also indicate mostly positive incentive effects but almost exclusively measure individual production instead of joint team production (i.e., in some of these studies, workers may face team incentives but work on individual tasks).[12] One rare exception is a small-scale laboratory experiment by Ramm et al. (2013), who investigate the effects of incentives on the performance of two paired individuals in a creative insight problem, in which the subjects are supposed to solve the candle problem of Duncker (1945). The study finds no effects of tournament incentives on performance in pairs, but it remains unclear whether this null effect is robust as the authors achieve rather low statistical power.[13] Our work substantially advances this literature by focusing on a collaboratively solved complex team task and allows for cleanly testing whether and why incentives improve performance. Such settings provide room for incentives to improve team performance by not only by increasing workers' effort but also creating a demand for better organizational and leadership structures within teams, which causes additional performance improvements.

The rest of this paper is organized as follows. Section 2 presents the field setting and the experimental design. Section 3 provides the main results with respect to performance improvements and potential crowding out. Section 4 discusses potential mechanisms that shape the efficacy of incentives, and Section 5 provides a more general discussion of our findings. Section 6 concludes.

[12]Bradler et al. (2019), Charness and Grieco (2019), and Laske and Schroeder (2017) study individual production. In Gibbs et al. (2017), team production is potentially possible, but submitted ideas have fewer than two authors, on average.

[13]Ramm et al. (2013) also study individual performance in the candle problem and find no negative incentive effects, whereas Kleine (2021) shows that piece-rate incentives increase the time needed to solve that task.

2 Experimental design and hypotheses

2.1 The field setting

We partner with the company ETR,[14] a provider of real-life escape games. In these games, teams have to solve, in a real setting, a series of quests that are cognitively demanding, non-routine, and interactive in order to succeed (usually by escaping from a room within a given time limit). Real-life escape games have become increasingly popular over the last few years and can now be found in almost all major cities around the world. Often, the task is embedded in a story (e.g., to find a cure for a disease or to defuse a bomb), which is also reflected in the room's design and how the information is presented. The task itself consists of a series of quests in which teams have to find cues, combine information, and think outside the box. They make unusual use of objects and exchange and develop innovative and creative ideas to complete the task within a given time limit. If a team manages to complete the task before the allotted time (one hour) expires, they win. However, if time runs out before the team solves all quests, they lose.

A typical escape room usually features several items, such as desks, shelves, telephones, and books. These items may include information needed to eventually complete the task. Typically, not all items will contain helpful information, and part of the task is determining which ones are useful for solving the quests. To illustrate a typical quest in a real-life escape game, we provide a fictitious example.[15] Suppose the participants have found and opened a locked box that contains a megaphone. Apart from being used as a speaker, the megaphone can also play three distinct types of alarm sounds. Among the many other items in the room, there is a volume unit (VU) meter in one corner of the room. To open a padlock on a box containing additional information, the participants will need a three-digit code. The solution to this quest is to play the three types of alarms on the megaphone and write down the corresponding readings from the VU meter to obtain the correct combination for the padlock.

The teams at ETR solve quests similar to this fictitious example. The tasks at ETR may further include finding hidden information in pictures, constructing a flashlight out of several parts, or identifying and solving rebus (word picture) puzzles (see also Erat and Gneezy, 2016; Kachelmaier et al., 2008).

[14] See `https://www.exittheroom.de/munich`.

[15] Our partner ETR asked us to not present an actual example from their rooms.

We conducted our experiments at an ETR facility in Munich. The location offers three rooms with different themes and background stories.[16] Teams face a time limit of 60 minutes and can see the remaining time on a large screen in their room. A task will be declared as completed if the team manages to escape from the room (or defuse the bomb) within 60 minutes. If they do not manage to do so within 60 minutes, the task is declared incomplete and the activity ends; if they get stuck, they can request hints via radio from the ETR staff. As they can only ask for up to five hints, a team needs to state explicitly that they want to receive a hint. The hints never contain the direct solution to a quest but only provide vague clues regarding the next required step.

ETR provides a rich setting with many aspects of modern non-routine analytical team tasks. First, finding clues and information very much matches the research activity that is often necessary before collaborative team work begins. Second, combining the discovered information is not trivial and requires the ability to solve complex problems. Subjects are required to process stimuli in a way that transcends the usual thinking patterns or are required to use objects in unusual ways. Third, to complete the task, subjects must effectively cooperate as a team. As in other non-routine team tasks, team members are supposed to provide additional angles to solve the problem at hand, and substantial synergy effects of different approaches to problem solving will enable a team to complete the task more quickly.

Fourth, participants, who self-select into the task, have a strong motivation to succeed as they have spent a non-negligible amount of money to perform the task (participants pay between €79 for two-person groups and €119 for six-person groups for the activity). We interpret the fact that many teams opt to write their names and finishing times on the walls of the entrance area of ETR as evidence for a strong motivation to finish quickly. Especially when teams are driven by the challenge of solving puzzles and derive enjoyment from making progress in the task, succeeding as fast as possible is clearly desirable.[17] Most importantly and objectively, teams never know how many intermediate

[16] *Zombie Apocalypse* requires teams to find the correct mix of liquids before time runs out (the anti-zombie potion). In *The Bomb*, teams must find a bomb as well as a code to defuse it. In *Madness*, teams need to find the correct code to open a door so as to escape (ironically) before a mad researcher experiments on them. We refrain from presenting the regression specifications with room fixed effects in the main text but provide these specifications in the Appendix. Adding room fixed effects does not change our results (see Appendix Tables A.2 and A.21).

[17] This is also corroborated by additional results from surveys among customer teams confirming that the main goal of teams is to achieve success together (see Appendix Table A.23).

quests are left to complete the task in its entirety. Hence, if a team wants to complete the task, the team has a strong incentive to succeed quickly. Finally, the team task is both difficult and non-routine in nature. This is corroborated by the fact that a substantial fraction of teams fail to finish in 60 minutes (33% of customer teams and 52% of student teams) without incentives, and even a substantial fraction of teams with experienced team members (28% in the field experiment and 50% in the framed field experiment) fail to do so either.[18]

The properties of these tasks are defining features of a broad class of modern jobs. Deming and Kahn (2018) find that many modern jobs require both cognitive skills (such as problem solving, research, and analytical and critical thinking) and social skills (such as communication, teamwork, and collaboration). Further, employers routinely list teamwork, collaboration, and communication skills as among the most valuable yet hard-to-find qualities of workers (Deming, 2017; Casner-Lotto and Barrington, 2006; Jerald, 2009). Akin to the skills required in our escape game, employers who were asked which attributes they seek on a candidate's resume in the National Association of Colleges and Employers Survey (NACE, 2015) rank leadership skills, ability to work in a team, problem-solving skills, strong work ethic, and analytical and quantitative skills among the top 6.

While these features therefore render escape rooms as an excellent framework for studying the effect of incentives on team performance, the setting is also extremely flexible. Our collaboration with ETR allows us to implement different incentives for more than 700 teams of customers and to also study whether incentives increase performance in a sample of presumably less motivated and exogenously formed teams of student participants (268 teams). The setting's considerable flexibility also enables us to delve into potential mechanisms through which incentives operate (by surveying student teams and conducting an additional natural field experiment that sheds light on the important role of the demand for leadership; see Section 4).

[18] In the field experiment, 48% of customer teams have at least one experienced team member, while among the student sample, 36% of teams have at least one. With incentives, still more than 15% of experienced teams fail to finish the task in 60 minutes in the field experiment and about 40% in the framed field experiment.

2.2 Hypotheses

As customer teams are strongly intrinsically motivated to succeed in the team challenge, there is room for potential motivational crowding out. The theoretical framework outlined in Bénabou and Tirole (2003) formalizes the idea that workers facing incentives may have a distorted perception of their own ability or the task's nature. For example, they may believe that the task is less enjoyable than expected if it needs to be incentivized or that incentives are likely implemented for less intrinsically motivated teams. As such, incentives may increase or decrease performance among intrinsically motivated teams. An increase in performance would mirror the mostly affirmative findings of incentive effects in routine tasks (see, e.g., Bandiera et al., 2005, 2013; Delfgaauw et al., 2015; Englmaier et al., 2017; Erev et al., 1993; Friebel et al., 2017; Hossain and List, 2012; Jayaraman et al., 2016; Lazear, 2000; Shearer, 2004), whereas a decrease could substantiate the widely promoted perception that monetary incentives impair team performance when tasks are non-routine and require thinking outside the box (Pink, 2009, 2011). We thus test the following non-directional hypothesis:

Hypothesis 1 *Providing bonus incentives does not affect team performance in the non-routine task.*

Following Bénabou and Tirole (2003), a bonus for extraordinary performance also contains a possible positive signal about a team's ability (due to the ambitious performance goal to which the bonus is tied). Hence, if positive performance effects are observed after the introduction of a bonus, these effects can be caused by the positive team ability signal instead of the reward the bonus provides. Similarly, if crowding out is observed, the actual extent of motivational crowding out due to monetary rewards may be underestimated (due to the compensating effects of the positive signal). The ensuing conjecture is presented in Hypothesis 2.

Hypothesis 2 *Bonuses with less ambitious performance thresholds lead to more crowding out, while introducing an ambitious reference point (indicating extraordinary performance) without offering a monetary reward improves performance.*

The framework by Bénabou and Tirole (2003) also implies that a team's level of intrinsic motivation should mediate incentive effects. For highly intrinsically motivated teams,

we expect that apart from causing direct positive incentive effects, extrinsic rewards may reduce intrinsic motivation, whereas for weakly intrinsically motivated teams, such motivational crowding out is less likely. This reasoning implies Hypothesis 3.

Hypothesis 3 *Teams' intrinsic motivation affects the efficacy of incentives.*

In addition to motivational crowding out, incentives may also result in substitutional crowding out (i.e., in a reduction of effort in non-incentivized dimensions; Holmstrom and Milgrom, 1991). In particular, bonus incentives for quickly completing a task may alter teams' intrinsic motivation to explore original solutions and instead make them rely more on external help. In fact, previous research has suggested that performance-based financial incentives may affect workers' willingness to explore in an experimentation task (see, e.g., Ederer and Manso, 2013). In our setting, incentives for speed may reduce teams' effort to explore original solutions (i.e., trying out different approaches on their own and instead asking for hints), particularly when they fail to quickly find the solution themselves.[19] Hypothesis 4 summarizes these arguments.

Hypothesis 4 *With bonus incentives, teams are less likely to explore original solutions.*

To better understand the roots and causes of our findings, we investigate two particular mechanisms at play. First, independent of crowding out effects on performance, team members' understanding of how effort maps into performance likely affects whether incentives eventually alter outcomes. This seems particularly relevant in non-routine team work, where subtasks can differ starkly from one another and the inputs by multiple team members aggregate into outputs in a very specific manner. We thus expect the following:

Hypothesis 5 *Understanding the production function enhances the performance effects of incentives.*

Second, it has been shown that salient incentives may alter team organization (Englmaier et al., 2017), and in non-routine tasks, such changes may require efficient leadership. If teams are motivated by the opportunity of receiving an additional bonus payment, incentives may also result in an increased demand for leadership. As leadership has been attributed importance in business, management, economics, and politics (Antonakis

[19] This intuition is also in line with additional survey evidence (see Section 2.5.2) revealing that hints are used to solve difficult puzzles but are perceived as less creative and less original by teams taking few hints.

et al., 2021), it appears that it is a likely candidate to improve outcomes in non-routine team tasks. Hence, we hypothesize the following:

Hypothesis 6 *Bonus incentives induce demand for leadership, leading to better performance.*

2.3 Experimental treatments, outcome measures, and hypotheses tests

We conduct the main field experiment with 3,308 customers (722 teams) of ETR Munich and implemented a between-subject design. To test Hypothesis 1, our main treatments included 487 teams randomly allocated to either the control condition or a bonus incentive condition. In the bonus condition, *Bonus45* (249 teams), a team received a monetary team bonus if they completed the task in less than 45 minutes.[20] In the *Control* condition (238 teams), teams were not offered any bonus.

We collect observable information related to team performance and team characteristics, which include time needed to complete the task, number and timing of requested hints, team size, the team's gender and age composition,[21] team language (German or English), experience with escape games, and whether the customers came as a private

[20]The bonus amounted, on average, to approximately €10 per team member. Teams in the field experiment received a bonus of €50 (for the entire team of between two and eight members, with about five members, on average). To keep the per-person incentives constant in the student sample with three team members (described in Section 2.4.2), the student teams received a bonus of €30. The treatment intervention (i.e., the bonus announcement) was always implemented by the experimenter present on site. For that purpose, they announced the possibility for the team to earn a bonus and had the teams sign a form (see Appendix A.5) indicating that they understood the conditions for receiving the bonus. The bonus incentive was described as a special offer, and no team questioned that statement. The experimenter also collected the data. To preserve the natural field experiment, we always ensured that the experimenters blended in with the ETR staff. To study the role of potential loss aversion akin to Hossain and List (2012), we framed the bonus as either a gain (125 teams) or a loss (124 teams). In *Gain45*, each team was informed that they would receive the bonus if they managed to complete the task in less than 45 minutes. In *Loss45*, each team received the bonus in cash up front, kept it during their time in the room, and were informed that they would have to return the money if they did not complete the task in less than 45 minutes. We do not identify major differences across these two conditions and thus pool these treatments in the main text. Additional analyses for these two subtreatments are provided in Appendix Section A.7.4.

[21]Again, note that to preserve the natural field experiment, we did not interfere with ETR's standard procedures. Thus we did not explicitly elicit participants' ages. Instead, we estimated each participant's age based on appearance to be either 1) below 18 years, 2) between 18 and 25 years, 3) between 26 and 35 years, 4) between 36 and 50 years, or 5) 51 years or older. As requested by the IRB, teams with minors were not included in the study.

group or were part of a company team-building event.[22] Our primary outcome variable is team performance, which we measure by i) whether or not teams complete the task in 45 minutes and ii) the time left upon completing the task. Comparing the *Bonus45* with the *Control* condition allows us to estimate the causal effect of bonus incentives on these objective performance measures.

Notably, the *Bonus45* condition includes an ambitious performance threshold (solving the task within 45 minutes rather than in 60 minutes), which may serve as a positive signal for intrinsically motivated workers. To test Hypothesis 2, we implement two additional experimental treatments. In *Bonus60* (88 teams), we provided the same monetary bonus but did not include the ambitious performance threshold. Instead, the bonus referred to the reference point of 60 minutes (akin to the *Control* condition).[23] That is, teams received the bonus if they completed the task within 60 minutes.[24] In the second additional treatment (*Reference Point*, 147 teams), we explicitly mentioned the 45 minutes as a salient reference point before the team started working on the task but did not pay any bonus.[25] The performance in *Bonus60* as compared to *Control* allows for an additional, even stronger test regarding potential motivational crowding in the spirit of Bénabou and Tirole (2003). Differences in performance between *Reference Point* and *Control* further reveal whether referring to an ambitious reference point increases the performance of the teams even if a monetary bonus is absent.

To test Hypothesis 3, we exploit the unique opportunity to replicate our (*Bonus45* and *Control*) conditions in a framed field experiment in the exact same setting with different teams that are conceivably less intrinsically motivated. For this purpose, we randomly allocated 804 student participants from the subject pool of the social sciences laboratory at LMU Munich (MELESSA) into 268 teams. The teams of three students were assigned to treatments *Control* (88) and *Bonus45* (180).[26] Importantly, these participants did not self-select into the escape challenge and were paid to perform the task as part of an economic

[22] ETR staff regularly ask teams whether they have ever participated in an escape game and whether the nature of the group is private or a team-building event irrespective of our experiment.

[23] Note that in *Control*, roughly 10% of the teams completed the task within 45 minutes, whereas roughly 67% did so within 60 minutes.

[24] Akin to the main treatment, we implemented *Bonus60* in two subtreatments, *Gain60* (42 teams) and *Loss60* (46 teams). Since treatment differences are again minor, we pool the data in our analysis.

[25] We said, "In order for you to judge what constitutes a good performance in terms of remaining time: If you make it in 45 minutes or less, that is a very good result."

[26] Akin to our analyses regarding the natural field experiment, we also pool the two subtreatments *Gain45* (90) and *Loss45* (90) for the student teams. Appendix Section A.8 provides additional results on the framing of incentives.

experiment, which we interpret as implying that they have lower intrinsic motivation.[27] Naturally, both samples differ along a host of dimensions other than intrinsic motivation (e.g., exogenous versus endogenous team formation, age, or educational background). However, it does not seem obvious to what extent these other differences are likely candidates to explain differential reactions to incentives when testing Hypothesis 3.[28]

To test Hypothesis 4, we use teams' hint taking as a proxy for whether they explore original solutions. If the bonus (i.e., an incentive for fast completion) reduces teams' effort to try out different approaches, it should become more likely that teams use hints when facing incentives. To test whether knowledge about the production function enhances positive incentive effects (Hypothesis 5), we rely on variation in team members' experience with escape challenges.

To test Hypothesis 6, we use a two-step procedure. First, we compare student teams' demand for leadership between the *Bonus45* and the *Control* condition based on a post-experimental questionnaire. Second, to identify the causal role of an increased demand for leadership, we ran an additional natural field experiment in the exact same setting. In this experiment, we randomly assigned 1,273 regular customers in 281 teams to one of two experimental conditions: *Control-L* and *Leadership*. As in our *Control* conditions reported earlier, participants in *Control-L* did not experience any intervention. In *Leadership*, ETR staff highlighted the importance of leadership to succeed in the task and encouraged teams to select a leader from their own group (for the exact wording, see Section 2.4.3).

[27] This experiment allowed us to also collect additional data on teams' task perception and team organization (discussed in Sections 3 and 4).

[28] The intuition that student teams are less intrinsically motivated is also in line with Result 4 in Section 3.3.3, which shows that student teams in particular are willing to give up developing original solutions by using more hints when incentivized. Further, other observable characteristics—i.e., dimensions in which student teams may have differed from customer teams, such as cognitive ability (proxied by math and overall grades), relative importance of receiving a monetary reward (proxied by students' income), or task-related abilities (proxied by their field of studies)—do not significantly interact with incentives (see Appendix Table A.11).

2.4 Procedures

2.4.1 Natural field experiment (customer sample)

We conducted the field experiment with ETR customers during regular opening hours from Monday to Friday.[29] We implemented the field experiment's main treatments (*Bonus45* and *Control*) in November and December 2015 and from January to May 2017. In the second phase of data collection, we further ran the additional treatments *Bonus60* and *Reference Point*. We randomized on a daily level to avoid treatment spillovers between different teams on site (as participants from one slot could potentially encounter participants arriving early for the next slot, and overhear, e.g., the possibility of earning money). Further, we avoided selection into treatment by not announcing treatments ex ante and randomly assigning treatments to days after most booking slots had already been filled.[30]

Upon arrival, ETR staff welcomed teams of customers as usual, and customers signed ETR's terms and conditions, including its data privacy policy. The staff then explained the rules of the game, and afterwards the teams were shown to their room and began working on the task. In the natural field experiments, teams were not informed that they were taking part in an experiment. The only difference between the treatment conditions and the control was that in the bonus conditions, the bonuses were announced as a special offer to reward successful teams, while in the reference point treatment, the finishing time of 45 minutes was mentioned saliently before the team started working on the task.

2.4.2 Framed field experiment (student sample)

For the framed field experiment, we invited student participants from MELESSA. Between March and June 2016 and January and May 2017, 804 participants (268 groups) took part in the experiment. To avoid selection into the sample based on interest in the task, we recruited these participants using a neutrally framed invitation text that did not explicitly state what activity they could expect. The invitation email informed potential participants that the experiment consisted of two parts, of which only the first part would be conducted on the premises of MELESSA, whereas the second part would occur out-

[29] ETR offers time slots from Monday through Friday from 3:45 p.m. to 9:45 p.m., and Saturday and Sunday from 11:15 a.m. to 9:45 p.m., with the different rooms shifted by 15 minutes to avoid overlaps and congregations of teams in the hallway.

[30] All slots in November and December 2015 were fully booked before treatment assignment. According to the provider, fewer than 5% of their bookings are made on the day of an event after the first time slot has ended.

side of the laboratory (without mentioning the escape game). They were further informed that their earnings from the first part would depend on the decisions they made and the second part would include an activity with a participation fee that would be covered by the experimenters.[31]

Upon arriving at the laboratory, the participants were informed about their upcoming participation in an escape game. They had the option to opt out of the experiment, but no one did so. In the first part of the experiment, i.e., on the premises of MELESSA, we elicited the same control variables as for the customer sample (age, gender, and potential experience with escape games). In addition, the participants took part in three short experimental tasks and answered several surveys. As the main focus of this paper is to analyze the robustness of the incentive effects across the two samples, we relegate the discussion of the results from these additional tasks to a future paper.[32] After completing the laboratory part, the experimenters guided the participants to the ETR facility, which is located a 10-minute walk (0.4 mile/650 meters) away from the laboratory. At ETR, each participant was randomly allocated to a team of three members, received the same explanations from ETR staff that were given in the field experiment, and, depending on the treatment, was informed about the possibility of earning a bonus.

For the student sample, we randomized the treatments on the session level (stratifying on rooms), as we made sure that student teams in different sessions on a given day did not encounter each other at the ETR facility. During the performance of the task, the same information about team performance as in the field experiment was collected. Once participants completed the task, they answered questions about the team's behavior and organization, as well as their perception of the task individually, on separate tablet computers. At the end, we paid the earnings individually in cash. In addition to the participation fee for ETR, which we covered (given the regular price, this corresponds to roughly €25 per person), participants earned €7.53 on average, with payments ranging from €3.50 to €87.[33]

[31] Appendix Section A.6 provides a translation of the invitation's text.

[32] These tasks included an elicitation of the willingness to pay for an ETR voucher, an experimental measure of loss aversion (based on Gächter et al., 2022), and a word creation task (developed by Eckartz et al., 2012). The participants also answered questionnaires regarding creativity (Gough, 1979), competitiveness (Helmreich and Spence, 1978), status (Mujcic and Frijters, 2013), a big-five inventory (Gosling et al., 2003), risk preferences (Dohmen et al., 2011), and standard demographics. On average, the subjects spent roughly 30 minutes completing the experimental tasks and questionnaires.

[33] In one of the laboratory tasks, the student participants further had the chance to win an ETR voucher worth roughly €100. Twenty-six participants actually won a voucher, implying an average additional

2.4.3 Additional natural field experiment (leadership)

Between January and March 2018, 1,273 additional regular customers in 281 teams were assigned to one of two experimental conditions: *Control-L* and *Leadership*. As before, we randomized on a daily level to avoid treatment spillovers between different teams on site. Participants were not informed that they were taking part in an experiment. The only difference between the conditions was that in *Leadership*, ETR staff highlighted the importance of leadership to succeed in the task and encouraged them to select a leader according to a short standardized script: "One piece of advice before you begin: a good team needs a good leader. Past experience has shown that less successful teams often wanted to have been better led. Thus, choose one of you to take the lead and consistently motivate/coordinate the team."[34]

2.5 Additional surveys

2.5.1 Student sample

To not interfere with the standard procedures at ETR, we could not run extensive surveys with their customer participants of our natural field experiments. However, we asked the student participants from the framed field experiment ($n = 804$) to what extent they agree that the team task exhibits various characteristics (using a seven-point Likert scale): does the task require logical thinking, thinking outside the box, creative thinking, for participants to be concentrated, high effort, and mathematical thinking? Furthermore, we asked whether the task encompassed mostly easy exercises or to what extent the problems were challenging (both on the same Likert scale).

In addition, we conducted two post-experimental questionnaires to analyze potential mechanisms through which the treatment effect could operate. In questionnaire 1, we asked participants to agree or disagree (on a seven-point Likert scale) with 19 statements that might capture aspects of team motivation and organization. In questionnaire 2 (which was conducted for a subsample of 375 student participants), we used an additional

earning from this task of roughly €3.23. Adding up all these earnings assuming market prices as valuations, the participants, on average, earned an equivalent of €35.76 for an experiment lasting two hours.

[34] The treatment *Leadership* consisted of two subtreatments that differed only by whether the last sentence stressed the word "motivate" or "coordinate." Since the effects of stressing different leadership functions are not the focus of this paper, see Englmaier et al. (2021) for details.

set of 12 questions based on the concept of team work quality by Hoegl and Gemuenden (2001).[35]

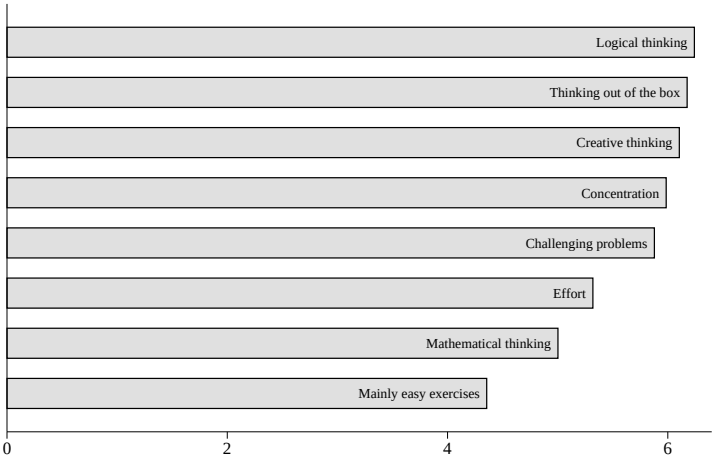
2.5.2 Additional ETR customers

To identify how teams' goals are potentially shifted when teams face incentives as well as how teams perceive hint taking, we ran additional surveys with 201 customers performing the team challenge at ETR Munich in January 2023.[36] Before participating in the escape challenge, survey participants were asked to rank eight potential goals they may pursue in the challenge from most (rank 1) to least (rank 8) important. Half were asked to rank goals for a hypothetical scenario in which they had the opportunity to win a team bonus of €50 if they completed the task in 45 minutes ("bonus" condition, $n = 100$). The other half was randomly assigned to a "no bonus" condition ($n = 101$); i.e., they ranked the goals without any bonus being mentioned. After participating in the escape challenge, survey participants had to evaluate by how much they agree with seven statements about hint taking.

2.5.3 HR experts

To estimate the ability of our study to shift priors about the effectiveness of incentives, in March 2023, we asked 400 participants from a pool of HR experts by survey provider Cint for their priors on the effectiveness of incentives in non-routine analytical team tasks.[37] Slightly more than half ($n = 203$) were asked about the effectiveness of bonus incentives in escape challenges. We explicitly informed these experts about the nature of the task at hand and asked them to guess how many out of 100 teams i) would become faster, ii) would become slower, and iii) would neither, once they received the opportunity to earn a bonus. The remaining ($n = 197$) HR experts reported the same numbers for abstract non-routine analytical team tasks (without mentioning escape games). Comparing the assessment of HR experts across tasks allows us to discuss the external validity of our setting.

[35] All questions are presented in Table 9, where we discuss the results.
[36] Appendix Section A.15 describes the survey in more detail.
[37] Appendix Section A.16 describes the survey in more detail.



Notes: The figure shows mean answers of $N = 804$ student participants to eight questions concerning the task's attributes. Answers were given on a seven-point Likert scale.

Figure 1: Task perception

3 Results

3.1 Task perception and randomization

We have previously argued that real-life escape games encompass many features of modern non-routine analytical tasks as teams face novel and challenging problems that require cognitive effort, analytical thinking, and thinking outside the box rather than easy repetitive chores. Figure 1 shows the mean answers of our post-experimental survey with student participants (see Section 2.5). Participants strongly agree that the task involves logical thinking, thinking outside the box, and creative thinking, in particular as compared to mathematical thinking and easy exercises (signed-rank tests reject that the ratings have the same underlying distribution; all p-values < 0.01 except for thinking outside the box versus logical thinking, $p = 0.16$, and thinking outside the box versus creative thinking, $p = 0.02$).

Table 1 provides an overview of the properties of the sample in the main treatments of the natural field experiment with ETR customers. The table highlights that our randomization was successful, based on observables such as the share of men, group size, experience, whether teams were taking part in a private or company event, and whether the team was English speaking. The only characteristic that differs significantly across treatments is the distribution of participants over the age categories guessed by our re-

Table 1: Sample size and characteristics

	Control (n=238)	*Bonus45 (n=249)*
Share of men	0.52 (0.29) [0,1]	0.51 (0.29) [0,1]
Group size	4.53 (1.18) [2,7]	4.71 (1.05) [2,8]
Experience	0.48 (0.50) [0,1]	0.48 (0.50) [0,1]
Private	0.69 (0.46) [0,1]	0.63 (0.48) [0,1]
English speaking	0.12 (0.32) [0,1]	0.08 (0.28) [0,1]
Age category $\in$ {18-25;26-35;36-50;51+}	{0.29;0.45;0.21;0.05}	{0.18;0.42;0.33;0.07}***

Notes: All variables except age category represent means on the group level. Experience denotes teams that have at least one member who experienced an escape game before. Private denotes whether a team is composed of private members (1) or whether the team belongs to a team-building event (0). Standard deviations and minimum and maximum values are in parentheses; (std.err.)[min, max]. Age category displays fractions of participants in the respective age category. Stars indicate significant differences to *Control* (using χ^2 tests for frequencies and Mann-Whitney tests for distributions), and *** p<0.01, ** p<0.05, * p<0.1.

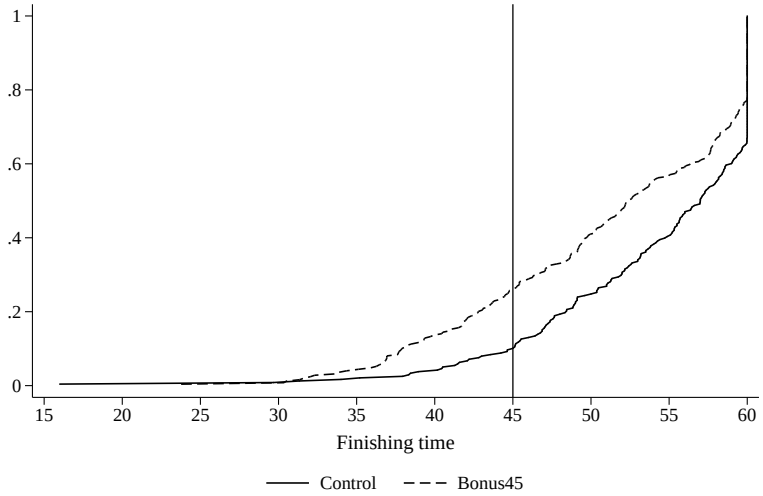
search assistants (χ^2 test, p-value < 0.01).[38] We therefore provide results from both the regression specifications without controls and the regression specifications in which we control for the estimated age ranges (and other observables).

3.2 Bonus incentives and team performance

We now turn to our primary research question—whether providing bonus incentives improves performance. As previously mentioned, our objective outcome measure of performance is whether teams manage to complete the task within 45 minutes and more generally how much time teams need to complete the task.

Figure 2 shows the cumulative distribution of finishing times with and without bonus incentives in the field experiment, with the vertical line marking the time limit for receiving the bonus. The figure indicates that bonus incentives induce teams to complete the task faster. In line with the idea that non-routine tasks are characterized by a noisy process that translates effort into performance, we observe differences over a large part of the support of the distribution rather than merely around the 45-minute threshold. In *Control*, only 10% of the teams manage to finish within 45 minutes, whereas in the bonus treatments more than twice as many teams (26.1%) do so (χ^2 test, p-value < 0.01). The remaining time upon completion also differs significantly between *Bonus45* and *Control* (p-value < 0.01, Mann-Whitney test). In *Bonus45*, teams are about three minutes faster than in *Control*, on average. The positive effect of bonuses on performance is also re-

[38] This does not change when adjusting for multiple hypothesis testing (MHT) according to List et al. (2019).



Notes: The figure shows the cumulative distributions of finishing times with and without bonus incentives. The vertical line marks the time limit for the bonus.

Figure 2: Finishing times in *Bonus45* and *Control* in the field experiment

flected in the fraction of teams finishing the task within 60 minutes. With bonuses, 77% of the teams finish the task before the 60 minutes expire, whereas in *Control* this fraction amounts to only 67% (χ^2 test, p-value $= 0.01$). Adjusting p-values for MHT as suggested in List et al. (2019) yields similar results. For further details, see also Appendix Table A.7 and Appendix Section A.12.1.

In addition to our non-parametric tests, we provide regression analyses that allow us to control for observable team characteristics (gender composition of the team, team size, experience with escape games, private versus team building, English speaking, and the estimated age of team members). Table 2 presents the results from a series of probit regressions that estimate the probability of completing the task within 45 minutes. We cluster standard errors at the day level (at which we varied the treatment) throughout.

Column (1) includes only a dummy variable for the bonus treatment *Bonus45*. Bonus incentives are estimated to increase the probability of completing the task in less than 45 minutes by 16.5 percentage points. This effect is substantial and equivalent to expanding the team size from four to six members. We add observable team characteristics in Column (2),[39] fixed effects for the ETR staff members on duty in Column (3), and week fixed effects in Column (4). Across all specifications, the coefficients of the bonus treatments

[39]From the set of characteristics in these and the following analyses, group size, experience with escape games, and the share of men in a team have a positive effect on performance, whereas English-speaking groups perform slightly worse. For more details, see Table 8, Column (1).

Table 2: Probit regressions: Completed in less than 45 minutes

	Probit (ME): Completed in less than 45 minutes			
	(1)	(2)	(3)	(4)
Bonus45	0.165***	0.164***	0.188***	0.151***
	(0.024)	(0.022)	(0.025)	(0.041)
Fraction of control teams completing the task in less than 45 min	0.10	0.10	0.10	0.10
Control variables	No	Yes	Yes	Yes
Staff fixed effects	No	No	Yes	Yes
Week fixed effects	No	No	No	Yes
Observations	487	487	487	487

Notes: The table displays average marginal effects from probit regressions of whether a team completed the task within 45 minutes on our treatment indicators (with *Control* as the base category). Control variables added from Column (2) onward include team size, share of men in a team, a dummy for whether someone in the team has been to an escape game before, dummies for median age category of the team, a dummy for whether the group speaks German, and a dummy for private teams (opposed to company team-building events). Staff fixed effects control for ETR employees present on site and week fixed effects for the week of data collection. All models include the full sample, including weeks that perfectly predict failure to receive the bonus (Appendix Table A.4 in Appendix Section A.7 reports regressions from a sample, excluding weeks without variation in the outcome variable). Robust standard errors clustered at the day level are reported in parentheses, and *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

are positive and highly significant, indicating that paying bonuses to teams completing a non-routine task strongly enhances their performance. In Appendix Table A.5, we also estimate the effects of bonuses on the time remaining upon completing the task, which confirms both the results from the non-parametric tests on the remaining time as well as the results from the probit models in Table 2.

Since the incentive only rewards completion of the task within the first 45 minutes, it should become ineffective for the last 15 minutes. In addition, if incentives crowd out intrinsic motivation to exert effort, we should see a decrease in performance after 45 minutes compared to *Control*. To investigate these conjectures in more detail, we run a Cox proportional hazard model, where we define the hazard as completing the task. If our prior was true, we should observe the treatment to have a strong effect on the hazard in the first 45 minutes, and no or even a negative effect in the last 15 minutes, conditional on covariates.

Table 3 shows the hazard ratios using our usual set of controls and employing cluster-robust standard errors. Columns (1) to (3) estimate the effect on the hazard rate for the first 45 minutes, while Columns (4) to (6) focus on the last 15 minutes. In Columns (1) and (4) we present the baseline effect of the treatment without any covariates, which are

Table 3: Influence of main bonus treatment on hazard rates

	Cox proportional hazard model: Finishing the task					
	First 45 min			Last 15 min		
	(1)	(2)	(3)	(4)	(5)	(6)
Bonus45	2.853***	2.947***	2.914***	1.178	1.251	0.841
	(0.446)	(0.474)	(0.844)	(0.189)	(0.248)	(0.180)
p-value for prop. haz. assumption	0.830	0.748	1.000	0.800	0.686	0.995
Control variables	No	Yes	Yes	No	Yes	Yes
Staff fixed effects	No	No	Yes	No	No	Yes
Week fixed effects	No	No	Yes	No	No	Yes
Observations	487	487	487	398	398	398

Notes: The table shows hazard ratios from a Cox proportional hazard regression of time elapsed until a team has completed the task on our treatment indicator *Bonus45*. All models include control variables, staff, and week fixed effects as in Table 2. Robust standard errors clustered at the day level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1. Significant coefficients imply that the null hypothesis of equal hazards (i.e., ratio = 1) can be rejected. The proportional hazard assumption is tested against the null that the relative hazard between the two treatment groups is constant over time.

added in Columns (2) and (5), respectively. Columns (3) and (6) also include week and staff fixed effects.

The treatment clearly increases the hazard rate of completing the task in the first 45 minutes. All coefficients are significantly different from 1 and are large in magnitude. Adding controls and fixed effects does not change the estimates by much, and the p-values of the proportional hazard assumption test do not indicate any reason to doubt our specification. However, in the last 15 minutes (Columns (4) to (6)), the effect has almost completely vanished. The coefficient on our treatment ranges closely around one and is not significantly different from one in any specification. Again, the proportional hazard assumption cannot be rejected. Thus, our data reflect two important aspects. First, the treatment indeed increases the likelihood of completing the task in the first 45 minutes but much less so in the last 15 minutes. Second, incentives are unlikely to have caused strong feelings of disappointment leading to substantially worse performance after teams failed to achieve the threshold relevant for the bonus payment in our setting. We conclude the following:

Result 1 *Bonus incentives increase team performance in the non-routine task.*

3.3 Potential crowding out of intrinsic motivation

Importantly, the results from our field experiment demonstrate that bonus incentives substantially improve team performance among teams with strong intrinsic motivation. As such, the monetary reward of the bonus appears to outweigh potential negative effects due to the crowding out of intrinsic motivation. However, in *Bonus45* the bonus incentive was tied to an ambitious performance threshold (45 minutes) that only 10% of teams in *Control* could achieve. Hence, it is crucial to investigate whether bonuses also work when they are not coupled with ambitious performance thresholds (see Hypothesis 2).

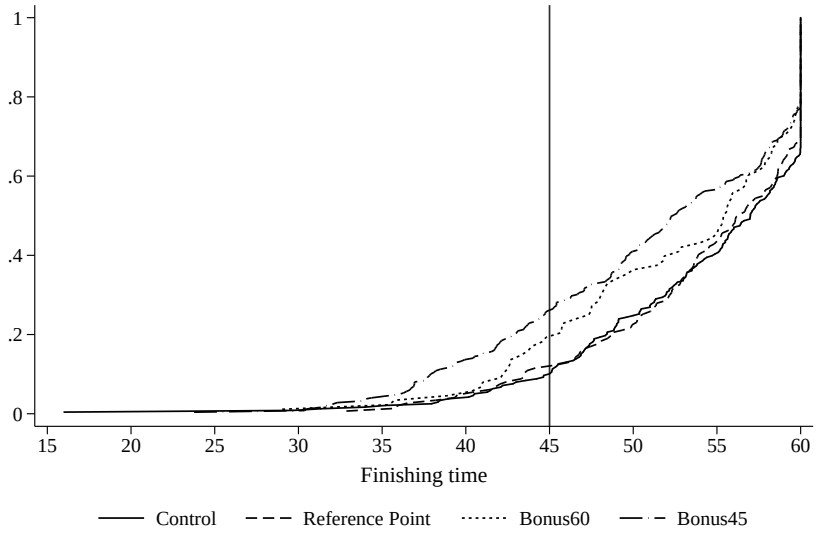
Furthermore, we aim to explore the robustness of incentive effects among a sample of less intrinsically motivated teams. Doing so allows us to go beyond merely analyzing the potential "net effect" of incentives and potential crowding out. In particular, observing similar effect sizes among differently intrinsically motivated teams would likely suggest that the net effect aligns with the "pure" positive effect of bonus incentives (see Hypothesis 3). Finally, we seek to uncover whether crowding out can be observed in the form of substitution of (multidimensional) effort by shedding light on teams' exploration behavior (i.e., hint taking; see Hypothesis 4).

3.3.1 Ambitious performance thresholds and incentives

To understand whether ambitious performance thresholds countervailed a potential crowding out of intrinsic motivation or independently caused positive performance effects, we refer to Figure 3. This figure displays the cumulative distribution of finishing times in conditions *Control*, *Reference Point*, *Bonus60*, and *Bonus45*. It suggests that monetary rewards reduce the amount of time teams need to finish the task, even when coupled with a less ambitious performance goal of 60 minutes (*Bonus60* versus *Control*, Mann-Whitney test, p-value $= 0.05$; *Bonus45* versus *Control*, Mann-Whitney test, p-value < 0.01, with *Bonus45* versus *Bonus60*, Mann-Whitney test, p-value $= 0.24$). Further, we do not observe that the ambitious reference point independently improves performance as the cumulative distribution of remaining times in *Reference Point* almost perfectly overlaps with the cumulative distribution function in *Control* (Mann-Whitney test, p-value $= 0.78$).[40]

For completeness, we provide a regression analysis for the full sample of ETR customer teams in Table 4. We regress the probability of finishing within 45 minutes on the

[40]The results point in a similar direction when adjusting for MHT following the approach suggested in List et al. (2019) (see Appendix A.12.1 for details).



Notes: The figure shows the cumulative distribution of finishing times of *Bonus45* (pooled), *Bonus60* (pooled), *Reference Point*, and *Control*. The vertical line marks the time limit for the bonus in the *Bonus45* condition.

Figure 3: Finishing times for all treatments in the field experiment

Table 4: Probit regressions: Completed in less than 45 minutes (all treatments)

	Probit (ME): Completed in less than 45 minutes			
	(1)	(2)	(3)	(4)
Bonus45	0.160***	0.157***	0.164***	0.108***
	(0.023)	(0.022)	(0.026)	(0.035)
Bonus60	0.105**	0.102***	0.105***	0.127**
	(0.041)	(0.038)	(0.039)	(0.051)
Reference Point	0.025	0.023	0.011	0.020
	(0.032)	(0.035)	(0.039)	(0.039)
Bonus45 = Bonus60	[0.151]	[0.095]	[0.120]	[0.752]
Bonus45 = Reference Point	[0.000]	[0.000]	[0.000]	[0.073]
Bonus60 = Reference Point	[0.066]	[0.059]	[0.033]	[0.024]
Fraction of control teams completing the task in less than 45 min	0.10	0.10	0.10	0.10
Control variables	No	Yes	Yes	Yes
Staff fixed effects	No	No	Yes	Yes
Week fixed effects	No	No	No	Yes
Observations	722	722	722	722

Notes: The table shows average marginal effects from probit regressions of whether a team completed the task within 45 minutes on our treatment indicators *Bonus45* (pooled), *Bonus60* (pooled), and *Reference Point* with *Control* being the base category. All models include control variables, staff, and week fixed effects as in Table 2. Robust standard errors clustered at the day level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

three treatment indicators *Reference Point*, *Bonus60*, and *Bonus45*. Column (1) includes only the treatment dummies, Column (2) adds our set of control variables, Column (3) adds staff fixed effects, and Column (4) adds week fixed effects. The regressions show that monetary incentives significantly increase the probability of finishing within 45 minutes, whereas the reference treatment does not.[41] It also becomes apparent that this finding is robust to adding covariates and fixed effects.

Moreover, a post-estimation Wald test rejects the equality of coefficients of *Bonus60* and *Reference Point* in all specifications (Columns (1) to (4), p-values < 0.1). Similarly, the coefficient of *Bonus45* is significantly larger than the coefficient of *Reference Point* in all specifications (p-value $= 0.07$ in Column (4), p-value < 0.01 in all other specifications). Equality of coefficients of *Bonus60* and *Bonus45* can only be rejected for one of the specifications (Column (2), p-value $= 0.095$). We summarize this finding in Result 2:

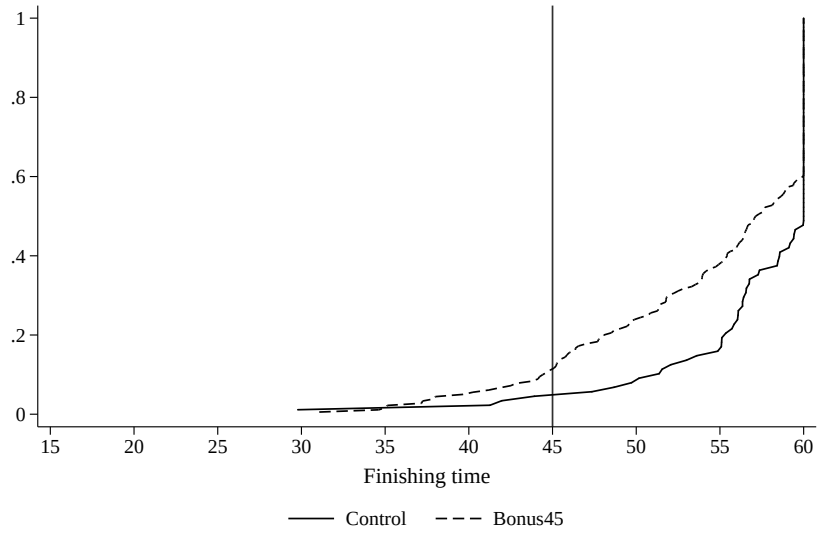
Result 2 *Bonuses with less ambitious performance thresholds do not lead to additional motivational crowding out. Introducing an ambitious reference point (indicating extraordinary performance) alone is not sufficient to induce a performance shift.*

3.3.2 Incentive effects among less intrinsically motivated teams: Results from the framed field experiment

To test whether the performance-enhancing effect of bonus incentives is also present in teams other than the self-selected customer sample, we turn to our student sample. Student participants may react differently to bonus incentives than the teams from our natural field experiment for several reasons. Most importantly, the process by which the sample is drawn is different across the two experiments. While regular ETR customers self-select into the task and are likely to be intrinsically motivated to perform well, student teams from the laboratory subject pool are assigned the task, do not pay for it (but instead are paid to perform it as part of an economic experiment), and hence are less likely to be intrinsically motivated.[42]

[41] Appendix Table A.6 confirms these findings for remaining time as the dependent variable.

[42] As discussed in Section 2.3, ETR customer teams were also formed endogenously and varied in size, whereas we randomly assigned students to teams of three participants. Further, student teams differ along observable dimensions, such as age, gender, and experience with the task. They are, on average, younger (23.03 years), slightly less likely to be male (44%), and less experienced in escape games (36% of the student teams had at least one member with escape game experience). As shown in Appendix Table A.11, these characteristics (apart from experience) do not significantly relate to teams' probability of receiving the bonus.



Notes: The figure shows the cumulative distributions of finishing times with and without bonus incentives in the framed field experiment. The vertical line at 45 minutes marks the time limit for the bonus.

Figure 4: Finishing times in *Bonus45* and *Control* in the framed field experiment (student sample)

Across both treatments, student teams do not differ significantly in any observed characteristic. The average share of men in *Bonus45* (0.43) is not significantly different to *Control* (0.45) (Mann-Whitney test, p-value $= 0.31$) and neither is the share of teams with at least one experienced member (0.36 versus 0.36, χ^2 test, p-value $= 0.90$) or teams' average age (22.96 versus 23.18, Mann-Whitney test, p-value $= 0.72$). Nevertheless, we control for team characteristics in our regression analyses.

Analogously to the analysis in the customer sample, we study the treatment effects on team performance by analyzing the fraction of the teams completing the task within 45 and 60 minutes, respectively, as well as the remaining times of teams in general, and among successful teams. Figure 4 shows the performance of teams in the framed field experiment, serving as the student sample counterpart to Figure 2. While student teams perform, on average, substantially worse than the ETR customer teams, the bonus incentives prove to be similarly effective for the student teams.[43]

[43]Given the differences in completion rates at 45 minutes in the *Control* condition across student and customer teams, we provide further analyses assessing the treatment effects "by the minute" using a Cox proportional hazard model, which additionally controls for team characteristics, staff, and week fixed effects. Appendix Figures A.1 and A.2 reveal that students' conditional likelihood of success remains low until the 50-minute mark in *Control* and then sharply increases. In contrast, for customer teams in *Control*, we find a gradual increase from minute 35 onward, indicating a richer heterogeneity among customer

Again, the fraction of teams finishing within 45 minutes is more than twice as large when teams face bonus incentives. In the incentive treatments, 11% of teams manage to complete the task within 45 minutes whereas only 5% do so in *Control* (χ^2 test, p-value $= 0.08$). The fraction of teams finishing the task within 60 minutes is also significantly larger under bonus incentives. With bonuses, 60% of the teams finish the task before the 60 minutes expire, whereas in *Control* this fraction amounts to 48% (χ^2 test, p-value $= 0.06$). Further, with bonus incentives, teams are, on average, about three minutes faster than in *Control*, and Mann-Whitney tests reject that finishing times in the control condition come from the same underlying distribution as finishing times under bonus incentives (Mann-Whitney test, p-values < 0.01).[44] These results are also robust to adjusting p-values for MHT as suggested in List et al. (2019) (see Appendix Section A.12.2 for more details).

In addition to the non-parametric tests, we run regressions analogously to the analysis for the customer sample. As before, we control for the share of men in a team, average age, and experience with escape games.[45] The first four columns of Table 5 report the results from probit regressions on the probability of completing the task within 45 minutes. Column (1) only uses the treatment dummy and shows that bonus incentives significantly increase the probability of completing the task within 45 minutes. The positive effect of the bonus incentives is robust to controlling for background characteristics (Column (2)), for staff fixed effects (Column (3)), and week fixed effects (Column (4)). Overall, the probit regression results reinforce our non-parametric findings: offering bonuses increases team performance.

Column (5) reports results from a linear regression, in which we pool both samples and test for the interaction of incentives and the specific sample. The results show no differential effect of incentives for the customer versus the student sample. Furthermore, for the student sample, the positive effect of bonus incentives is reflected qualitatively

teams' performance. With incentives (*Bonus45*), the hazard rates both among student and customer teams steadily increase from the 35-minute mark onward. This is in line with the idea that teams provide more effort early on (in the hope of receiving the bonus payment) and do not completely slack after the 45-minute mark has passed (see also the analyses in Table 3). Hence, incentives increase the likelihood of finishing early in both samples, and their efficacy does not seem to strongly depend on the underlying heterogeneity in teams' performances without incentives.

[44] Appendix Table A.8 summarizes these findings and provides further details with respect to the framing of incentives.

[45] In contrast to the ETR customer sample, all teams speak German and consist of three team members. Hence, we do not need to control for language or group size.

Table 5: Probit regressions: Completed in less than 45 minutes (student sample)

	Student sample				Pooled
	Probit (ME): Completed in less than 45 minutes				OLS
	(1)	(2)	(3)	(4)	(5)
Bonus45	0.075*	0.073*	0.075*	0.079**	0.086***
	(0.042)	(0.041)	(0.039)	(0.037)	(0.030)
Field					0.290*
					(0.151)
Bouns45 × *Field*					0.083
					(0.059)
Fraction of control (student) teams completing the task in less than 45 min	0.05	0.05	0.05	0.05	0.05
Control variables	No	Yes	Yes	Yes	Yes
Staff fixed effects	No	No	Yes	Yes	Yes
Week fixed effects	No	No	No	Yes	Yes
Observations	268	268	268	268	755

Notes: The first four columns show average marginal effects from probit regressions of whether a team completed the task within 45 minutes on our treatment indicators (with *Control* as the base category). Column (5) reports coefficients from a linear regression including both the student and the customer sample. Control variables added from Column (2) onward include the share of men in a team, a dummy for whether someone in the team has been to an escape game before, and average age of the team. Staff fixed effects control for ETR employees present on site, and week fixed effects control for the week of data collection. All models include the full sample, including weeks that perfectly predict failure to receive the bonus (Appendix Table A.10 in Appendix Section A.8 reports regressions from the student sample, excluding weeks without variation in the outcome variable). Robust standard errors clustered at the session level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

in the analyses of the time remaining (see Appendix Table A.9). These results emphasize that a crowding out of intrinsic motivation does not seem to strongly distort the pure effect of incentives.[46] We summarize these findings as follows:

Result 3 *Incentives are similarly effective among teams that self-selected into the task (customer teams) and teams assigned to the task by us (student teams).*

[46] As previously discussed, we do not find it obvious to what extent any sample differences in characteristics other than intrinsic motivation would affect performance. Given that we do not observe differences in treatment effects across the samples, any differences in other (un)observable characteristics between the groups could only influence the result if they exactly canceled out the effects introduced by differences in intrinsic motivation, which appears unlikely. Additionally, as Appendix Table A.11 shows, no other observed characteristics interact with the performance effect among the student participants.

Table 6: Hints requested in the field experiment and the framed field experiment

	Control	*Bonus45*
Within 60 minutes		
Field experiment (487 groups)	2.92 (1.55)	3.10 (1.34)
Framed field experiment (268 groups)	3.74 (1.04)	4.11 (0.98)***
Within 45 minutes		
Field experiment (487 groups)	1.97 (1.22)	2.36 (1.15)***
Framed field experiment (268 groups)	2.33 (0.93)	3.17 (1.04)***

Notes: This table summarizes the mean number of hints taken across treatments in the field experiment and the framed field experiment (standard deviations in parentheses). Stars indicate significant differences from *Control* (using Mann-Whitney tests), and *** p<0.01, ** p<0.05, * p<0.1. Teams in the framed field experiment take more hints within 60 minutes (*Control*: p-value < 0.01, *Bonus45*: p-value < 0.01) and within 45 minutes (*Control*: p-value $= 0.013$, *Bonus45*: p-value < 0.01). p-values of non-parametric comparisons between *Gain45* and *Loss45* are larger than 0.10 for both experiments.

3.3.3 Bonus incentives and team willingness to explore

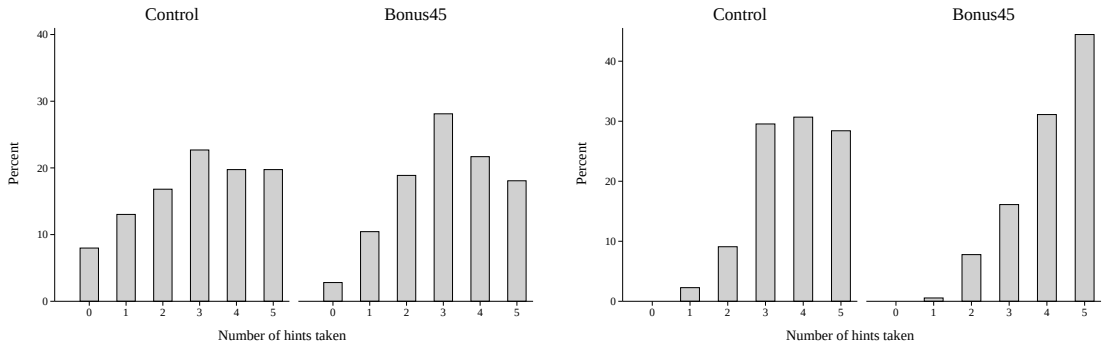
To test Hypothesis 4, we next analyze how many out of the five possible hints teams request under the different treatment conditions as well as whether they are more likely to take hints earlier in the presence of incentives.[47]

Table 6 shows the number of hints taken across samples and treatments. For teams that self-selected into the task (customer sample), we do not find a statistically significant difference in the number of hints taken within 60 minutes. These teams take, on average, about three hints in both the bonus treatment and the control condition. In contrast, for teams confronted by us with the task (the student sample), we observe (economically and statistically) significant increases in hint taking in the bonus treatments as compared to *Control*, suggesting that incentives reduce these student teams' willingness to explore original solutions.[48]

To capture potential heterogeneity across teams, we report the fractions of teams requesting zero, one, two, three, four, or five hints for the customer sample in panel (a) and for the student sample in panel (b) of Figure 5. The figure reinforces our earlier

[47] Appendix Section A.11 provides additional evidence that the increase in hint taking in the framed field experiment is unlikely due to increased importance of risk aversion when incentives are in place.

[48] Note that a similar picture arises if we "standardize" the task's length to account for different completion times by customer and student teams. We convert the time the hint was taken as a fraction of the total game time (either actual time of completion or 60 minutes in case teams did not complete the task). Appendix Figures A.3 and A.4 plot the average fraction of hints taken conditional on the share of time elapsed in the customer and student sample across treatments. The figures show that incentives leave hint taking among customer teams virtually unchanged, whereas student teams seem to use more hints when facing incentives after around 20% of the standardized length of the game has passed.



(a) Customer sample (487 groups)

(b) Student sample (268 groups)

Notes: The figure shows histograms of hints taken across samples. Panel (a) depicts the fractions of customer teams choosing 0, 1, 2, 3, 4, or 5 hints in *Control* (left graph) and *Bonus45* (right graph). Panel (b) shows the fractions of student teams.

Figure 5: Hints requested across samples and treatments

findings: bonus incentives have, if at all, a minor effect on the number of hints taken in the customer sample. These teams' willingness to explore original solutions fails to differ statistically significantly across treatments (χ^2 test, p-value=0.11). Panel (b) of Figure 5 depicts the same histogram for the framed field experiment with student participants. It becomes apparent that teams that did not self-select into the task are much more likely to take hints when facing incentives (χ^2 test, p-value=0.029). Roughly 75% of these teams take four or five hints when facing incentives, as compared to 59% doing so in *Control*. Ordinary least squares regression analyses for hint taking including additional controls (see Table 7, Columns (1), and (3)) confirm these results.[49]

Focusing only on hints taken within the first 45 minutes, non-parametric tests indicate significant differences across treatments for both samples, but again, the effect is much stronger for student teams that we assigned to the non-routine task (customers: χ^2 test, p-value$<$0.01; students: χ^2 test, p-value$<$0.01). Regression analyses using additional controls and fixed effects imply that these teams take, on average, 0.808 more hints within the first 45 minutes when facing incentives, whereas customer teams take, on average, only 0.186 more hints (Columns (2) and (4) of Table 7). Hence, the non-parametric results for the student sample remains largely unchanged, whereas the positive effect ob-

[49] An ordered probit regression yields qualitatively similar results; see Appendix Table A.13.

Table 7: OLS regressions: Number of hints requested

	OLS: Number of hints requested					
	Field experiment		Framed field experiment		Combined	
	within 60 min	within 45 min	within 60 min	within 45 min	within 60 min	within 45 min
	(1)	(2)	(3)	(4)	(5)	(6)
Bonus45	0.098	0.186	0.343**	0.808***	0.357***	0.829***
	(0.183)	(0.134)	(0.136)	(0.122)	(0.117)	(0.119)
Field					-2.589***	-1.917***
					(0.603)	(0.385)
Bonus45 x Field					-0.297	-0.674***
					(0.217)	(0.182)
Constant	4.037***	1.770***	5.391***	4.236***	4.994***	3.363***
	(0.442)	(0.469)	(0.650)	(0.698)	(0.439)	(0.416)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Staff FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	487	487	268	268	755	755

Notes: The table shows coefficients from OLS regressions of the number of hints requested within 60 or 45 minutes regressed on our treatment indicator *Bonus45* (pooled). The sample in Columns (1) and (2) is restricted to the (natural) field experiment and in Columns (3) and (4) to the framed field experiment. Columns (5) and (6) include both samples. *Field* is a dummy equal to one for the (natural) field experiment. Controls and fixed effects (FE) are identical to previous tables. Robust standard errors clustered at the day (for the field experiment) or session (for the framed field experiment) level are reported in parentheses, and *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

served in our non-parametric analyses becomes small and statistically insignificant for the customer sample.

In Columns (5) and (6), we pool the data from the two samples and study whether there is a significant difference in the reaction to the bonus incentive (in terms of hint taking) in the customer as compared to the student sample. While students in the incentive condition do not generally react substantially differently to the incentive by taking more hints (see Column (5)), bonus incentives indeed substantially increase their hint-taking behavior as long as the bonus threshold can still be achieved (i.e., within the first 45 minutes; see Column (6)).

Overall, our results align with the conclusion that intrinsic motivation and incentives interact complexly when teams can choose whether or not to explore original and innovative solutions on their own.[50] Incentives increase the hint-taking behavior of teams that did not self-select into the task, indicating a substitution of effort due to incentives,

[50]These findings complement recent work on incentive effects in meaningful routine tasks (Kosfeld et al., 2017).

in line with the multitasking framework by Holmstrom and Milgrom (1991). However, such substitution is much less prevalent among intrinsically motivated customer teams, aligning with the idea that these teams may derive utility from progressing on their own and hence take fewer hints.

To understand whether this idea is reflected in teams' perceptions, we turn to our additional survey among ETR customers and analyze how teams' perceptions differ conditional on their own hint-taking behavior. While both teams that take few (less than three) or many hints (three or more) similarly agree that hints are used to solve difficult puzzles (χ^2 test, p-value$= 0.71$), we observe that teams taking few hints perceive hint taking more negatively, particularly as less creative (χ^2 test, p-value < 0.01), less original (χ^2 test, p-value < 0.01), and less fun (χ^2 test, p-value < 0.01).[51]

An alternative explanation for reduced substitution among intrinsically motivated teams (as compared to hired teams) can be found in the framework of Bénabou and Tirole (2003). Here, strongly intrinsically motivated teams may wish to compensate potential "negative news" about their ability due to incentives and thus not substitute exploration effort for hints when incentives are present. However, this should likely result in less hint taking among teams in the bonus condition as compared to *Control* (which we do not observe). Further, among the intrinsically motivated customer teams, we see no significant differences in the number of hints taken when bonuses are combined with more ambitious (as compared to less ambitious) performance thresholds (3.09 hints in *Bonus45* versus 3.26 hints in *Bonus60*, χ^2 test, p-value=0.84), rendering compensating behavior unlikely.

We summarize our findings in Result 4.

Result 4 *As long as the bonus can still be achieved (i.e., within the first 45 minutes), incentives increase hint taking by teams hired to perform the task (student teams). This effect is much smaller and statistically insignificant among teams that chose to perform the task (customer teams).*

4 Mechanisms

Our results have shown that incentives causally and unambiguously improve team performance but have not yet established how they improve performance. We aim to provide

[51] For further details on the survey, see Appendix Section A.15.

insights on likely mechanisms through two distinct avenues. First, to better understand what distinguishes teams that do respond to incentives from those that do not, we discuss whether any particular observable team features interact with the observed efficacy of incentives. Second, we investigate how incentives affect behavior, particularly team organization. Post-experimental survey responses identify increased demand of leadership as a potential channel, which we subsequently investigate using our additional natural field experiment.[52]

4.1 When do incentives work?

We first investigate whether the efficacy of incentives for solving the task within 45 minutes interacts with customer teams' observable characteristics in Table 8.[53] The results do not contain significant interactions with the teams' gender share (Column (2)), team size (Column (3)), teams' language (Column (6)), or whether teams participated as part of a company event (Column (5)). This suggests that bonus incentives appear to be similarly effective for teams of different size and levels of diversity.

We further investigate whether teams with experienced team members react differently to incentives than inexperienced teams (Column (4)). Experienced members possess more knowledge about how team effort translates into team success, which could enhance the effects of incentives. We find a positive, economically and statistically significant interaction of bonus incentives and experience. Our estimates imply that the positive bonus effect is about 1.5 times larger for experienced teams. This suggests that a good understanding of the production function is crucial in this setting for harnessing the benefits from incentives.

The latter is also reflected in teams' remaining times, where the bonus tends to be more effective for experienced teams, though not at conventional significance levels (p-value = 0.10, see Column (4) in Appendix Table A.3). For remaining times, we also find that a higher share of men relates positively to performance but decreases the effectiveness of incentives (possibly due to ceiling effects). Similarly, when studying the efficacy of incentives across predicted performance quintiles (based on observable team charac-

[52] Additionally, in Appendix Section A.14, we provide a broader discussion of the dimensions along which incentives may change behavior within teams, including even more additional surveys and an additional laboratory experiment.

[53] Appendix Table A.3 provides results for teams' remaining times.

Table 8: Linear probability model: Completed in less than 45 minutes

	OLS: Completed in less than 45 minutes					
	(1)	(2)	(3)	(4)	(5)	(6)
Bonus45	0.172***	0.200***	0.023	0.120**	0.130**	0.169***
	(0.050)	(0.071)	(0.122)	(0.057)	(0.056)	(0.047)
Share of men	0.102*	0.130***	0.102*	0.100*	0.105*	0.103*
	(0.055)	(0.048)	(0.055)	(0.054)	(0.056)	(0.058)
Group size	0.056***	0.056***	0.042**	0.057***	0.055***	0.056***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Experience	0.125***	0.126***	0.126***	0.058*	0.124***	0.125***
	(0.031)	(0.031)	(0.032)	(0.032)	(0.031)	(0.031)
Private	0.040	0.039	0.039	0.036	-0.001	0.039
	(0.041)	(0.042)	(0.042)	(0.041)	(0.049)	(0.041)
English	-0.115*	-0.117*	-0.113*	-0.114*	-0.117*	-0.129***
speaking	(0.060)	(0.062)	(0.062)	(0.060)	(0.059)	(0.044)
Bonus45 ...						
... × Share of men		-0.055				
		(0.128)				
... × Group size			0.031			
			(0.025)			
... × Experience				0.132**		
				(0.051)		
... × Private					0.077	
					(0.056)	
... × English						0.027
speaking						(0.139)
Constant	-0.177	-0.192	-0.109	-0.179	-0.163	-0.172
	(0.132)	(0.133)	(0.142)	(0.132)	(0.133)	(0.138)
Staff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	487	487	487	487	487	487

Notes: The table shows coefficients from a linear probability model. The dependent variable is a dummy for finishing within 45 minutes. All models include staff and week fixed effects as in Table 2. Robust standard errors clustered at the day level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

teristics), we find weaker incentive effects for teams predicted to perform very well (see Appendix Figure A.5). This result aligns with the notion that the efficacy of incentives can be weaker for teams that already exert high levels of effort.

Notably, we do find robust, positive, and significant incentive effects among all other quintiles. Finally, and akin to the analyses regarding the probability of finishing within 45 minutes, we find that the efficacy of incentives for improving remaining times does not significantly differ for the number of team members, whether the team is English or German speaking, or whether the team challenge was booked by a company or private team. We summarize these findings in Result 5:

Result 5 *The effect of bonus incentives is larger for teams with experienced team members.*

4.2 Performance and team organization

Table 9 shows the results from questionnaires 1 and 2, reporting uncorrected p-values, as well as MHT-adjusted p-values with 31 outcomes following List et al. (2019). The upper panel shows that overall, incentives do not strongly affect agreement with the statements we provided. However, teams appear to be notably more stressed when facing incentives than teams in *Control* (Mann-Whitney test, p-value < 0.01).[54] At the same time, similar to teams in *Control*, treated teams strongly agree with the statement "I would like to participate in a similar task again" (Mann-Whitney test, p-value $= 0.88/0.99$), suggesting that incentives cause positive rather than negative stress among the team members. Second, participants in the incentive treatment tend to agree more with the statement that "one team member was dominant in leading the team" (Mann-Whitney test, p-value $= 0.03/0.40$) as well as with the statement "I was dominant in leading the team" (Mann-Whitney test, p-value $= 0.05/0.52$). However, both of these statements lack statistical significance when adjusted for MHT.

The results from questionnaire 2 in the lower panel of Table 9 mirror the answers from questionnaire 1. Teams facing incentives wish for more leadership (Mann-Whitney test, p-value < 0.01) and tend to report that teams were better led (Mann-Whitney test, p-value$= 0.04/0.40$). However, the latter fails to reach conventional significance levels when adjusting for MHT. Overall, both questionnaires suggest that incentives may

[54] We are agnostic about whether this increase in stress levels is a direct result of incentives or a byproduct of increased effort levels.

Table 9: Answers to post-experiment questionnaires

	Control	*Bonus45*	p-value / MHT adjusted
Questionnaire 1 (n=804)			
(1) "The team was very stressed."	3.57	4.13***/†††	0.000 / 0.000
(2) "One person was dominant in leading the team."	2.60	2.86**	0.028 / 0.396
(3) "We wrote down all numbers we found."	5.64	5.50**	0.044 / 0.991
(4) "I was dominant in leading the team."	2.64	2.87**	0.053 / 0.520
(5) "We first searched for clues before combining them."	4.58	4.39	0.107 / 0.899
(6) "We exchanged many ideas within the team."	5.87	5.74	0.119 / 0.904
(7) "When we got stuck, we let as many team members try as possible."	5.43	5.28	0.143 / 0.914
(8) "The team was very motivated."	6.14	6.27	0.221 / 0.881
(9) "We communicated a lot."	5.78	5.88	0.227 / 0.982
(10) "All team members exerted effort."	6.24	6.37	0.242 / 0.850
(11) "Our notes were helpful for finding the solution."	5.50	5.43	0.413 / 0.999
(12) "I was able to present all my ideas to the group."	5.95	5.93	0.406 / 0.991
(13) "We were well coordinated within the group."	5.73	5.80	0.606 / 0.997
(14) "I was too focused on my own part."	2.88	2.83	0.763 / 1
(15) "We made our decisions collectively."	5.51	5.58	0.867 / .999
(16) "I would like to perform a similar task again."	6.30	6.28	0.876 / 0.985
(17) "Our individual skill sets complemented each other well."	5.65	5.68	0.891 / 0.998
(18) "We had a good atmosphere in the team."	6.30	6.37	0.929 / 0.992
(19) "All team members contributed equally."	5.97	6.00	0.956 / 0.999
Questionnaire 2 (n=375)			
(1) "To what extent did you want someone to take the lead?"	2.67	3.32***/†††	0.000 / 0.009
(2) "How well was the team led?"	3.85	4.21**	0.036 / 0.400
(3) "How deeply did you think about the problems?"	6.00	5.79	0.111 / 0.553
(4) "To what extent did you follow ideas that were not promising?"	5.02	4.79	0.173 / 0.772
(5) "To what extent did you develop a team spirit?"	5.54	5.80	0.168 / 0.760
(6) "How well were individual tasks and joint strategy coordinated?"	3.28	3.51	0.183 / 0.914
(7) "How well did you leverage team members' individual potential?"	5.14	4.94	0.217 / 0.890
(8) "How much did you help each other when someone was stuck?"	5.70	5.58	0.217 / 0.994
(9) "How intensely did you search the room for clues?"	6.31	6.22	0.515 / 0.994
(10) "How much effort did all the team members exert?"	5.98	5.96	0.600 / 0.908
(11) "How much did you communicate about procedures?"	5.30	5.35	0.883 / 1
(12) "How willing were team members to accept the help of others?"	5.80	5.85	0.892 / 1

Notes: This table reports answers to our post-experiment questionnaires from the framed field experiment by treatment (*Control* and *Bonus45*) and p-values of the differences between the treatments. The scale ranges from not at all agreeing to the statement (=1) to completely agreeing (=7) in questionnaire 1 and from very little (=1) to very much (=7) in questionnaire 2. Stars indicate significant differences from *Control* using Mann-Whitney tests, and *** p<0.01, ** p<0.05, * p<0.1. Daggers indicate significant differences when adjusting for MHT (concerning 31 outcomes) according to List et al. (2019), where ††† <0.01, †† <0.05, † <0.10.

change the way teams are organized, indicating that incentives may lead to an endogenous emergence of (a demand for) team leaders. This inference is also supported by an alternative approach to adjust for MHT, where principal component factor analyses is used for dimensionality reduction, following the Kaiser-Guttman rule (see Loehlin and Beaujean, 2016). We apply this method separately for questionnaires 1 and 2 in Appendix Table A.12. For questionnaire 1, the analysis retains five factors. We name these factors "general team collaboration" (factor 1), "team cohesion" (factor 2), "dominance" (factor 3), "documentation" (factor 4), and "intensity" (factor 5).[55] We find that general team collaboration does not significantly differ across treatments (Mann-Whitney test: p-value=0.76) and neither does dominance (Mann-Whitney test: p-value=0.11). However, incentives tend to increase team cohesion (Mann-Whitney test: p-value=0.07) and intensity (Mann-Whitney test: p-value$<$0.01) but decrease documentation (Mann-Whitney test: p-value=0.02).

Regarding questionnaire 2, we retain three factors that we term as "cooperative" (factor 1), "leadership" (factor 2), and "struggling" (factor 3).[56] Cooperative behavior (factor 1) does not significantly differ across treatment conditions (Mann-Whitney test: p-value= 0.34). Leadership (factor 2) is significantly more pronounced with incentives (Mann-Whitney test: p-value$<$0.01). Struggling in teams (factor 3) tends to be lower with incentives but statistically insignificantly so (Mann-Whitney test: p-value=0.26). Overall, both analysis indicate that incentives appear to change team organization and stimulate the demand for, and the emergence of, leadership.

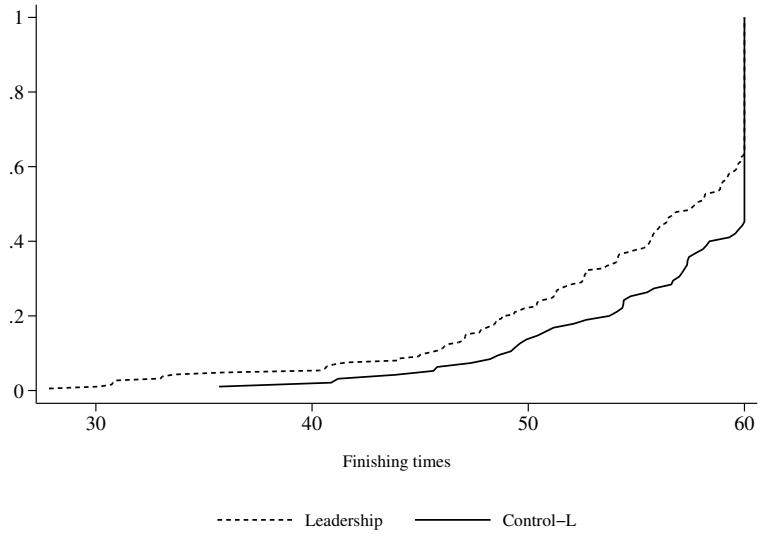
4.3 The causal effect of leadership

To investigate the causal demand of an increased demand for leadership, we ran an additional natural field experiment in which teams where either randomly encouraged to choose a leader (*Leadership*) or not (*Control-L*; see also Section 2.4.3). Figure 6 shows the cumulative distribution functions of finishing times across both conditions. Teams in the *Leadership* treatment condition clearly perform better than those in the *Control-L* condition. Specifically, in *Leadership* 63% of teams finish the task within the time limit of

[55] Items from questionnaire 1 that load heavy on factor 1 are (5) (6), (7), (9), (13), (15), and (18). Items loading heavy on factor 2 are (8), (10), (12), (16), (17), and (19). Items loading heavy on factor 3 are (2) and (4), those loading heavy on factor 4 are (3) and (11), and those loading heavy on factor 5 are (1) and (14).

[56] Items that load high on factor 1 are (1, negatively), (5), (7), (8), (10), (11), and (12). Items that load high on factor 2 are (2) and (6), and items that load high on factor 3 are (3), (4), and (9).

Figure 6: Leadership: Cumulative distribution functions of finishing time



Notes: The figure shows the cumulative distribution of finishing times for teams in *Leadership* and *Control-L*.

60 minutes, whereas only around 44% do so in *Control-L* (Pearson χ^2 test: $p < 0.01$). In addition to being more likely to complete the task, teams that were encouraged to choose a leader also solve the task faster (average remaining times: 3 minutes and 10 seconds in *Control-L* and 5 minutes and 29 seconds in *Leadership*; Mann-Whitney test: $p < 0.01$).

These non-parametric results are confirmed by a series of probit regressions, where we incrementally introduce additional control variables and fixed effects as in Table 2. In Table 10, we estimate the average marginal effect of *Leadership* on the probability of completing the task within 60 minutes. As before, we cluster standard errors at the daily level, which also corresponds to the level of random treatment assignment. In all specifications, we find that exogenously shifting the demand for *Leadership* significantly increases teams' probability to succeed within 60 minutes. The estimated average marginal effect amounts to an increase of 17 percentage points as compared to *Control-L*, implying a relative increase in the fraction of successful teams by about 38%.

In Appendix Table A.20, we present the analyses for the remaining time. The implied average marginal effects show that raising awareness of the importance of leadership demand unambiguously increases the remaining time upon task completion by, on aver-

Table 10: Probit regressions: Leadership, completed in less than 60 minutes

	Completed within 60 minutes			
	(1)	(2)	(3)	(4)
Leadership	0.182***	0.187***	0.185***	0.168***
	(0.051)	(0.052)	(0.065)	(0.051)
Fraction of teams in *Control-L* completing the task in 60 minutes	0.442	0.442	0.442	0.442
Controls	No	Yes	Yes	Yes
Staff FE	No	No	Yes	Yes
Week FE	No	No	No	Yes
Observations	281	281	281	281

Notes: The table displays average marginal effects from probit regressions of whether a team completed the task within 60 minutes on our *Leadership* indicator (with *Control-L* as the base category). Each column indicates whether team controls (group size, share of men, experience with escape games, median age, language spoken, private versus team-building events, actively taken walkie-talkie), staff, and week fixed effects are included. Standard errors in parentheses are clustered at the daily level, with significance levels * = $p < 0.10$, ** = $p < 0.05$, and *** = $p < 0.01$.

age, 2 minutes and 48 seconds.[57] These findings, coupled with the survey evidence that incentives increased the demand for leadership, show that the resulting emergence of leadership mediates the positive effects of incentives on performance. We summarize our findings in Result 6:

Result 6 *Bonus incentives induce demand for leadership. Exogenously shifting the demand for leadership results in substantial performance improvements.*

5 Discussion

Our results demonstrate that bonus incentives have sizable positive effects on team performance in both the natural and the framed field experiments. Building upon important work by Maniadis et al. (2014), we investigate how much our findings should update our

[57] Note that the magnitudes are hardly comparable with the results presented in Tables 2 and A.5, as incentives targeted task completion after 45 minutes, whereas the leadership intervention only targeted completion at the 60-minute mark. The cleanest comparison for the case of incentives would be to regress the remaining times or the likelihood of completion in 60 minutes on the *Bonus60* treatment. Doing so in the full specification results in a marginal effect of an additional 2 minutes and 44 seconds of remaining time and a 12.5 percentage point increase in completion probability. The latter effect is somewhat lower, albeit not significantly so, than the effect of leadership; however, teams in the different control groups exhibited varying levels of success (0.442 in *Control-L* versus 0.67 in *Control*). This suggests that leadership possibly had a larger scope to improve performance on the extensive margin. Therefore, the emergence of leadership seems to have a comparable potential for improving performance to that of offering bonus incentives.

Table 11: Post-study probabilities

Achieved power for...	χ^2 tests on success dummy (45 & 60 mins) in framed field	χ^2 tests on success dummy (45 mins) in nat. field	χ^2 tests on success dummy (60 mins, nat. field) and t-tests on remaining time (field and framed field)
	(1)	(2)	(3)
	0.45	0.70	0.95
Prior probability	Posterior	Posterior	Posterior
0.05	0.32	0.42	0.50
0.1	0.50	0.61	0.68
0.2	0.69	0.78	0.83
0.4	0.86	0.90	0.93
0.6	0.93	0.95	0.97
0.8	0.97	0.98	0.99
0.9	0.99	0.99	0.99

Notes: This table reports PSPs (Maniadis et al., 2014) for different combinations of prior probabilities and achieved power. The levels of power in Columns (1)–(3) correspond to the achieved power in terms of statistical tests (t-tests and χ^2 tests) for our primary outcomes. We achieve a power of about 0.95 for t-tests on the remaining time in the natural and framed field experiment, as wells as for the χ^2 tests of whether the team received the bonus in the natural field experiment. Our achieved power for χ^2 tests of whether teams complete the task within 45 minutes amounts to 0.7 in the field experiment. In the framed field experiment, achieved power for the χ^2 tests of whether the team completes the task within 45 or 60 minutes, respectively, amounts to 0.45.

beliefs that incentives truly increase performance in our task. To do so, we calculate post-study probabilities (PSPs) conditional on different priors. PSP $= (1-\beta)\pi/[(1-\beta)+\alpha(1-\pi)]$, where π denotes the probability of a given prior and $(1-\beta)$ denotes the study's statistical power. Intuitively, the PSP reflects the posterior probability that our null hypothesis (no incentive effects) is false.

The results are displayed in Table 11, where the rows display increasing priors and the columns reflect different levels of power. Column (1) shows posteriors given a statistical power of $(1-\beta) = 0.45$. This corresponds to the achieved power of our binary measures to complete the task within 45 or 60 minutes from our framed field experiment with the student sample. The posteriors indicate that even with moderate power, we should drastically update our beliefs upward. Starting from priors as low as $\pi = 0.10$, which indicate a strong disbelief in any effect, the posteriors reflect equal probabilities of both outcomes (PSP$= 0.50$). Priors of $\pi > 0.10$ yield posteriors strongly in favor of our result.

Column (2) shows posteriors for a power of $(1-\beta) = 0.7$, which corresponds to our binary outcome variable on succeeding within 45 minutes for the natural field experiment. Column (3) reports posteriors for a power of $(1-\beta) = 0.95$, which we achieve

for our binary outcome variable on succeeding in 60 minutes in the natural field experiment, as well as for t-tests on the remaining time in both the framed and the natural field experiment. Both columns show that even moderate to high disbelief converts into posteriors strongly favoring an effect to exist.

To establish a realistic prior, we turn to our survey with HR experts. On average, these experts believed that 40.38% of teams would improve in performance, 23.33% of teams would decline, and outcomes for 36.29% of teams would remain unchanged. As Table 11 shows, a prior of approximately 0.4 (believing a positive effect is less likely than a coin flip) in all cases enables posteriors close to believing a true effect to exist.[58] These calculations emphasize the strong updating that decision makers should undergo as they learn about the results from our study.[59]

Our series of large-scale field experiments constitutes, to the best of our knowledge, the first systematic investigation into bonus incentive effects in non-routine analytical and collaboratively solved team tasks. To discuss the external validity of our results, we consider it useful to draw on the SANS conditions introduced in List (2020): selection, attrition, naturalness, and scaling.[60] Our two main samples reported in this paper consist of actual ETR customers, as well as students, who conceivably differ along several dimensions.[61] As our documented treatment effects carry over to participants from both samples, this seems to indicate that selection is not a primary concern. Additionally, university students are likely (on average) similar to workers in many non-routine, analytical team work environments, as these frequently require higher levels of education.

[58] As HR experts in the survey could have believed that improving teams became substantially faster, whereas declining teams only moderately slower, we also asked for the number of minutes teams would be expected to be faster/slower (conditional on being faster/slower). The small difference (48 seconds) between the two is not statistically significant; Wilcoxon signed-rank test $p = 0.25$.

[59] In addition to HR experts, in Appendix Section A.14, we describe a survey with two samples: a hand-curated list of academics working in personnel economics and respondents from the Economic Science Association's "ESA discuss," a mailing list for academic experimental economists. We asked both samples if they believed incentives influence performance in non-routine analytical team tasks. Over 80% reported that incentives have at least some positive effect. A 0.4 prior for HR experts therefore seems to be a lower bound among relevant samples, pushing the posterior potentially even closer toward certainty.

[60] For similar applications of this approach, see also Holz et al. (2023), Goldszmidt et al. (2020), and Fehr et al. (2022).

[61] As we do not collect background information about customers apart from age, we can only assume that not all ETR participants are university educated (and are different along the many margins that typically correlate with this). In light of comparatively low rates of university attendance in Germany of below 30%, we deem this assumption reasonable. Any differences in characteristics may be in addition, or give rise, to differences in preferences, constraints, and beliefs (e.g., differing levels of intrinsic motivation for the task).

We also do not consider attrition to be a major concern as none of the participants opted out from our framed field experiment and participants were unaware of being studied in the natural field experiment (and hence selective attrition could not occur in the latter either).

In terms of scaling, it is worth noting that stakes in our setting are substantially lower than typical bonuses paid in firms. On the other hand, our results in Table 8 and Appendix Table A.3 do not show a significant interaction between incentives and team size, suggesting that, at least locally, the incentive's size is less important. As such, we would expect to observe, if anything, larger effects when applying our interventions in various work environments.[62]

In terms of naturalness, we concede that our task indeed is only one example of a non-routine analytical team task. Given the vast number of work environments that fall under this broad classification, other jobs may contain additional idiosyncratic features that could influence the presence of the effect we detect. But importantly, our task, and all other non-routine, analytical team tasks, share three features: they 1) are non-routine, 2) require analytical thinking, and 3) are conducted in teams. Building our experiment around these commonalities ensures that our analysis covers the essence of this class of tasks. This assertion is corroborated by our survey among HR experts, whom we either ask about their expectations regarding the efficacy of incentives for team performance in an escape challenge or in a neutrally framed non-routine task.

Across both settings, HR experts believe incentives to be similarly effective (see also Appendix Table A.24). They predict that 41.37% of teams will improve for abstract non-routine tasks versus 40.38% for escape challenges (p-value $= 0.66$, Mann-Whitney test). Furthermore, 21.48% versus 23.33% of teams are predicted to perform worse (p-value $= 0.41$, Mann-Whitney test) and 37.15% versus 36.29% similarly (p-value $= 0.80$, Mann-Whitney test). While we argue, based on these insights, that additional idiosyncratic features of other tasks should not constitute a major threat to external validity per se, we wish to discuss idiosyncratic features of our task one by one.

First, ETR customers choose to perform the team challenge and are willing to incur costs to do so. This suggests that they are likely to receive some utility from performing the task (e.g., they are motivated by the challenge of solving puzzles and tackling

[62] As we observe consistent effects of incentives across both samples (which may have very different costs and benefits), the use of incentives seems to be scalable to a large number of cases that vary along similar dimensions.

different angles of the complex task), which may not generally hold for the choice of an occupation. However, many employees working on non-routine analytical team tasks (e.g., teams of IT specialists or specialist doctors) have also self-selected into their occupation and incurred substantial costs (e.g., in terms of education) to be able to perform challenging non-routine tasks in their job.[63] Naturally, self-selection into work environments with non-routine tasks will likely become less important as current labor market trends continue, with many jobs expected to transform and include more non-routine team elements in the future. Importantly, as we find very similar effects of incentives on teams' finishing times across both of our samples, it seems that this particular feature (i.e., interest in performing the task) is not crucial to the effectiveness of our bonus treatment.

Second, non-routine analytical team tasks are diverse in nature. Intrinsic motivation to perform these tasks (e.g., in business or academia) may stem not only from making progress in and eventually completing them but also from the salient greater goals that team success can deliver. As the escape game does not feature such greater goals, it is worthwhile to discuss its implications for external validity in more detail. One could argue that the lack of such goals reduces external validity, as the effectiveness of incentives may hinge on workers' motivation. However, since we find that incentives increase performance, both for people who value performing the task (customer sample) and those being assigned to complete it (student sample), it is unlikely that a lack of intrinsic motivation (due to a lack greater goals) affects our main findings. Further, our results highlight that the positive incentive effects mainly stem from improved organization and more structured leadership, benefits of which should extend to teams performing tasks with greater goals. Finally, we consider our finding as broadly applicable, as many workers perform non-routine tasks in occupations that do not necessarily serve greater goals.

[63] An intrinsic desire for being able to perform non-routine analytical jobs has been long recognized and leveraged by recruiters. One notable example are some of Google's recruiting campaigns, which featured signs placed at Harvard Square and across Silicon Valley. These signs were not initially revealed to be associated with Google but instead challenged passersby to solve a complicated math problem. The correct answer led to a website that posed yet another puzzle. Eventually, the determined problem solver arrived at an official Google recruiting website that asked them to submit their resume. See `https://www.npr.org/templates/story/story.php?storyId=3916173&t=1534099719379`. Further, escape challenges are also used in the context of hiring, where employers can use team-based approaches to screen future employees' skills to work in non-routine tasks (`https://www.eseibusinessschool.com/experimental-escape-room-recruitment-event-esei-tradler/`).

Third, one could argue that in some environments, more than one single solution to a complex problem may exist, while in our setting there is only one. We agree that some non-routine tasks may feature open solutions. However, we do not perceive it as a threat to external validity for two reasons. First, many complex problems of interest arguably have only a single (optimal) solution, but there are multiple ways of arriving at that solution, both in the workplace as well as in our setting. More specifically, we think of incentives as means to motivate the worker to produce the best possible solution in a given amount of time (by identifying the main problems to be solved and coming up with a solution). For example, consider a team of IT specialists confronted with a complex task in which they have to develop a platform that fulfills predefined requirements within a specific time frame. To this end, team members have to identify the main constraints and develop tailored solutions. While there may be several new platforms that the team can develop, most likely only one of them will be optimal given the employer's demands (e.g., in terms of specifications or expected sales). Thus, even if several platforms can be developed, the employer will want to incentivize the team to find the optimal solution and not an inferior one. Second, while in our setting the optimal solution is known to the creators of the escape challenge, it is unknown to the participating teams. Throughout the task, teams may not know if there exists only one solution to each subproblem or if picking one out of a number of possible solutions will let them advance in the task.

Fourth, the proximity of our subjects to their team members may alleviate potential free rider concerns typical in regular office settings. In the absence of free riding, we could thus estimate inflated incentive effects. However, given that the task requires mainly cognitive effort, the observability of co-workers' effort provision is limited in our setting as well. Furthermore, if the utility from completing the task quickly without contributing was lower than in a comparable work setting, we should observe differences in performance effects among highly intrinsically motivated (customer sample) and presumably less intrinsically motivated teams (student sample). However, the incentives increase performance in both samples to a similar degree.

Finally, we would like to note that while our task lasts much longer than usual tasks in laboratory experiments, incentives in work environments are frequently designed to stimulate effort over long periods, such as weeks, months, or years. We deem the question of how to optimally design incentives over such time spans as very important, but clearly, our experiment was not designed to investigate the long-run effects of bonus incentives.

Instead, we study the general effectiveness of bonus incentives in collaboratively solved non-routine analytical team tasks in light of widespread claims of "if-then rewards" being ineffective in such modern tasks (Pink, 2009, 2011, in the nomenclature of List, 2020, we thus view the findings as WAVE1 insights). Hence, while we do provide robust evidence in a controlled field setting and from two distinct samples that incentives do improve team performance, more replications will need to be completed to understand if the size of the result applies to other non-routine tasks and occupational environments.

6 Conclusion

According to Autor et al. (2003) and Autor and Price (2013), non-routine, cognitively demanding, interactive tasks are becoming increasingly important in the economy. At the same time, we know relatively little about how incentives affect performance in these tasks. We provide a comprehensive analysis of incentive effects in a non-routine, cognitively demanding team-based task in a large-scale field experiment. The experiment allows us to study the causal effect of bonus incentives on the performance and exploratory behavior of teams. In collaboration with our partner, we implemented a natural field experiment with more than 700 teams. We find an economically and statistically significant positive effect of incentives on performance: Teams are more than twice as likely to complete the task within 45 minutes under the incentive condition than under the control condition, and the difference in finishing time between treated and control teams amounts to about 0.44 standard deviations observed in control.

Our comprehensive approach further allowed us to isolate important channels through which incentives may operate in collaboratively solved non-routine analytical team tasks. First, as these tasks are often performed by intrinsically motivated teams, we studied whether incentives lead to crowding out. Following the framework of Bénabou and Tirole (2003), in which crowding out occurs because incentives are perceived as negative signals about the task or teams' ability, we studied the efficacy of bonuses among teams that were intrinsically motivated to succeed in the task at hand. We varied whether bonuses were coupled with less or more ambitious performance goals and find a substantial improvement in teams' performance in both conditions. Thus, we document a robust net positive effect of bonus incentives, rendering the likelihood of crowding out as per Bénabou and Tirole (2003) unlikely.

Further, and in line with the latter interpretation, we find that bonus incentives lead to similar performance improvements among intrinsically motivated (customer) teams that self-selected into the task and less intrinsically motivated (student) teams that were assigned to perform the task. However, our experiments still document an important trade-off related to crowding out in the form of substitution of effort (Holmstrom and Milgrom, 1991). Particularly among teams that we assigned to perform the task, we find a tendency toward reduced independent problem-solving and an increased reliance on hints.[64]

Second, in contrast to routine tasks, in which the relationship between effort is often deterministic, non-routine analytical team tasks are characterized by a noisier relationship between effort and performance. As such, teams' productivity may depend on how individual efforts are combined and teams' understanding of the production function may shape the efficacy of incentives. In line with this idea, we find that incentives are most effective for experienced teams, thus making understanding of the production function a crucial mediator for the efficacy of incentives in non-routine tasks.[65] Other team-specific factors that could contribute to the efficacy of incentives (e.g., team size) turn out to be less important. Further, we document that incentives induce important changes in team organization and increase teams' demand for leadership. As such, incentives may not only fulfill their required function to increase performance but also provide additional benefits beyond this by fostering more structured leadership within teams, which can causally improve team performance.

Finally, we find that teams in the incentive condition reported to be significantly more stressed. Although, in our setting, we did not observe that increased stress levels reduced teams' willingness to perform similar tasks again, in general firms may worry that increased stress may result (in the long run) in costly turnover. Overall, our findings thus emphasize robust positive effects of bonus incentives but also highlight important trade-

[64]There are several reasons to believe that hints are not responsible for the observed differences in performance. First, an increase in performance will mechanically make subjects request hints earlier since they reach difficult stages earlier. Second, in our natural field experiment, overall hint-taking behavior is not significantly different across treatments. Third, when studying at what point in time teams achieve an intermediate step early in the task, and how many hints teams have taken before reaching that step, we observe significantly better performance by teams facing incentives but no significant differences in hint taking (see Appendix Table A.14).

[65]The latter finding also challenges the idea that incentives enhance learning about the essentials of the production function, i.e., how combinations of different kinds of effort (e.g., searching, deliberating, combining information) map into performance.

offs between employee production and turnover as well as regarding potential crowding out in the form of substitution (in our setting exploration versus hint taking), particularly when teams are less intrinsically motivated to explore on their own.

Taken together, our results raise several interesting questions for future research. As our findings only provide an initial glimpse at the incentive effects in these kinds of tasks, systematically varying incentive structures within teams could create additional insights into the functioning of non-routine team work. A very interesting, but particularly challenging, question that remains is to empirically find the optimal incentive mechanism for performance in non-routine analytical team tasks. This requires varying different types of incentives (tournaments, bonuses, etc.) and their extent simultaneously, ideally on a set of non-routine tasks of different nature. While clearly beyond the scope of this study, it is certainly a very interesting and relevant avenue for future research. Looking beyond the question of incentives, the setting of a real-life escape game may further be used to study other important questions such as goal setting, non-monetary rewards and recognition, the effects of team composition, team organization, and team motivation. Studies in this setting are in principle easily replicable, many treatment variations are implementable, and large sample sizes are feasible.

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A Supplementary appendix

A.1 Incentive effects in other field experiments

Table A.1 presents observed effect sizes from a selection of field experiments primarily based on Ogundeji et al. (2016), Bandiera et al. (2021), and Fryer et al. (2022), augmented by recently and prominently published studies. The aim of the table is not to provide a comprehensive overview but to illustrate the heterogeneity in effect sizes observed within and across different task categories. The table reports only published field experiments in which some real effort was incentivized with a monetary reward and for which effect sizes in standard deviations were reported in the original study or in one of the three overview studies.

Table A.1 also provides an overview of potential bunching at performance levels above the incentivized performance threshold. Although bunching might be expected particularly in routine tasks (where the relationship between effort provision and outcomes is often deterministic), only one of the studies explicitly reports bunching (Hossain and List, 2012). However, a closer inspection of studies involving routine tasks reveals that strategic effort provision often appears difficult, as performance feedback is either provided with delay, noisy, or performance targets are specified in relative terms. The table further shows that bunching is not reported in studies involving non-routine tasks (as expected, due to the noisier relationship between effort and outcomes).

Table A.1: Summary of studies and effect sizes

Reference	Outcome	Incentive	Effect size	Reports bunching
Panel A. Retail, production, and service provision				
Delfgaauw et al. (2015)	Average number of products per customer	Elimination tournament with a prize of 1.25%–6.25% of monthly gross earnings	0.20	No
Friebel et al. (2017)	Sales	Team bonus of up to €300 per month conditional on pre-existing sales targets	0.33	No
Fehr and Goette (2007)*	Revenues per four-week period	25% increase in commission rate	0.35	No
Boly et al. (2011)*	Exam-grading accuracy	Monetary penalties for mistakes	0.36	No
Bandiera et al. (2005)*	Kilograms of fruit picked per hour	Constant piece rate (vs. decreasing piece rate with average productivity of group of workers).	0.86	No
Hossain and List (2012)*	Inspected units per hour	Roughly 20% increase in pay for meeting productivity target	0.90	Yes
Panel B. Student performance				
Gneezy et al. (2019)	Mathematics test score (25 questions taken from PISA test), Shanghai	RMB 90 (take away RMB 3.6 for each incorrect answer)	-0.09	No
Angrist et al. (2009)*	First-year GPA	Monetary bonuses (scholarships) for meeting GPA targets. Higher bonus (up to $5,000) for higher targets.	-0.03	No
Levitt et al. (2016)	Test score in low-stakes tests (low incentive)	$10 incentive for improving their baseline score from the prior testing session	[-0.008; 0.009]	No
Angrist and Lavy (2009)*	Matriculation exam performance	Increasing monetary bonuses (up to ca. $1,500) for taking any matriculation test, passing any matriculation test, and completing all matriculation requirements.	0.02	No
Levitt et al. (2016)	Test score in low-stakes tests (high incentive)	$20 incentive for improving their baseline score from the prior testing session	[0.07; 0.15]	No
Gneezy et al. (2019)	Mathematics test score (25 questions taken from PISA test), US	$25 (take away $1 for each incorrect answer)	[0.24; 0.28]	No
List et al. (2018)	Test score (knowledge related to ISAT)	$90 if the student improves their score on the test relative to the baseline test	0.30	No
Panel C. Incentives for teachers				
Fryer (2017)**	Meta-analyses (US experimental studies): Math scores (pooled across years)	–	0.02	meta study
Pham et al. (2021)**	Meta-analyses (US experimental and non-experimental studies): Math scores (pooled across years)	–	0.005	meta study
Pham et al. (2021)**	Meta-analyses (Non-US experimental and non-experimental studies): Math scores (pooled across years)	–	0.22	No
Fryer (2013)**	Standardized math performance (pooled across years)	Bonus up to $3,000 for every union-represented teacher	-0.03	No
Glazerman et al. (2009)**	Standardized math performance	Incentives based on Teacher Advancement Program (TAP, Chicago)	-0.004	No
Marsh et al. (2011)**	Standardized math performance	School-wide performance bonuses (New York)	-0.002	No
Springer et al. (2012)**	Standardized math performance (Round Rock)	$5,400 bonus for teachers in team's who ranked among top third (within grade level)	-0.01	No
Springer et al. (2011)**	Standardized math performance	bonuses of up to $15,000 per year (for test score gains on Tennessee Comprehensive Assessment Program (TCAP)	0.003	No
Mbiti et al. (2019)**	Math scores (Tanzania)	TZS 5,000 (ŨS$ 3) bonus per student passing a standardized test	0.006	No
Gilligan et al. (2022)**	Math scores (Uganda)	"Pay-for-percentile" incentives (Barlevy and Neal, 2012)	0.02	No
Duflo et al. (2012)**	Standardized math performance (India)	Incentives for teacher attendance and monitoring	0.02	No
Behrman et al. (2015)	Student performance (math test, Mexico)	10% and 15% of the annual salary of a teacher in a federal high school	[0.01; 0.04]	No
Speroni et al. (2020)	Student test score (math and reading, years 1–4 pooled)	Bonuses for principals and teacher of an average yearly cost of $100 per student	[0.03; 0.04]	No
Mbiti et al. (2023)	Student test score (year 1)	"Pay-for-percentile" and ''levels" schemes with multiple (curricular-based) thresholds (3.5% of the annual net salary)	[0.06; 0.22]	No
Loyalka et al. (2019)**	Math scores (pooled incentive treatments, China)	"Pay-for-percentile" incentives Barlevy and Neal (2012)	0.07	No
Barrera-Osorio and Raju (2017)**	Student score on government exam (pooled subjects and incentive treatments, Pakistan)	Bonus from 10%–15% of annual basic salary	0.08	No
Fryer et al. (2022)	ThinkLink scores (knowledge related to ISAT)	"Pay-for-percentile" (pooled), expected value of reward was $4,000 (equal to 8% of the average teacher salary)	0.10	No
Muralidharan and Sundararaman (2011)**	Standardized math performance (India)	Average bonus calibrated ̃3% typical annual salary	0.20	No
Brownback and Sadoff (2020)**	Student score in final exams (multiple postsecondary departments), Indiana community college	Loss-framed incentives of $50 per student who passed an objective, externally designed course exam	0.20	No
List et al. (2018)	Student test score (knowledge related to ISAT)	$90 bonus incentive for tutors working with tier two students on ISAT preparation	0.31	No
Panel D. Health professionals				
Bardach et al. (2013)***	Cardiovascular risk reduction (US)	Up to $200/patient or $100,000/clinic, paid to clinic	-0.12	No
Twardella and Brenner (2007)***	Smoking cessation (Germany)	€130 per successful cessation, paid to general practitioners	0.05	No
Salize et al. (2009)***	Smoking cessation (Germany)	€130 per successful cessation, paid to general practitioners	0.06	No
An et al. (2008)***	Quitline referrals (US)	$5,000 for 50 quitline referrals, paid to clinic	0.06	No
Basinga et al. (2011)***	Provision and quality of maternal and child health care (Rwanda)	$0.09–$4.59 per unit of healthcare service, paid to health center	0.09	No

Notes: This table presents observed effect sizes from a selection of field experiments primarily based on Bandiera et al. (2021)*, Fryer et al. (2022)**, and Ogundeji et al. (2016)***, augmented by recently and prominently published studies. The aim of the table is not to provide a comprehensive overview but to illustrate the heterogeneity in effect sizes observed within and across different task categories. The table reports only published field experiments in which some real effort was incentivized with a monetary reward and for which effect sizes in standard deviations were reported in the original study or in one of the three overview studies. The last column indicates whether the authors explicitly reported on performance levels bunching slightly above the relevant performance threshold.

A.2 Room fixed effects (natural and framed field experiment)

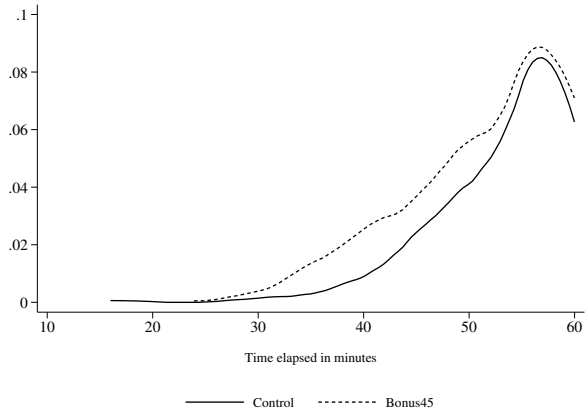
Table A.2: Main treatment probit and GLM regressions, including room fixed effects

	Field experiment		Framed field experiment	
	Probit (ME)	GLM	Probit (ME)	GLM
	(1)	(2)	(3)	(4)
Bonus45	0.150***	0.266**	0.076**	0.655***
	(0.041)	(0.113)	(0.036)	(0.215)
Constant		3.706***		3.896***
		(0.488)		(0.834)
Fraction of control teams completing the task in less than 45 min	0.10		0.05	
Control variables	Yes	Yes	Yes	Yes
Staff fixed effects	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes
Room fixed effects	Yes	Yes	Yes	Yes
Observations	487	487	268	268

Notes: The table shows average marginal effects from probit regressions of whether a team completed the task within 45 minutes (Columns (1) and (3)) and coefficients of GLM regressions on the remaining time (Columns (2) and (4)) for the customer and the student sample. The specifications are as in Column (4) of Table 2, Column (4) of Table A.5, Column (4) of Table 5, and Column (4) of A.9. However, they also include room fixed effects. Robust standard errors clustered at the day (field experiment) and session (framed field experiment) level are reported in parentheses, and *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

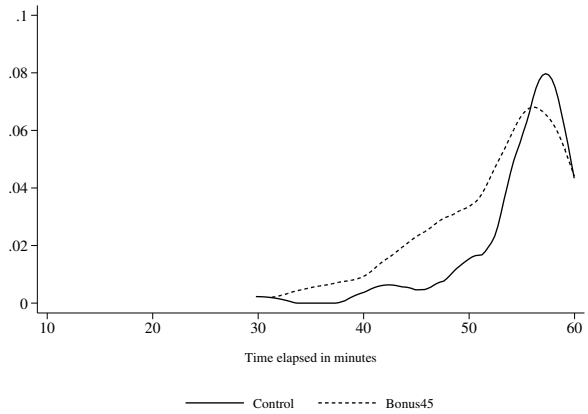
A.3 Hazard rates for the natural and framed field experiment

Figure A.1: Customer sample: Hazard rates (completion) across time



Notes: The figure shows the hazard rates for customer teams in *Bonus45* and *Control*.

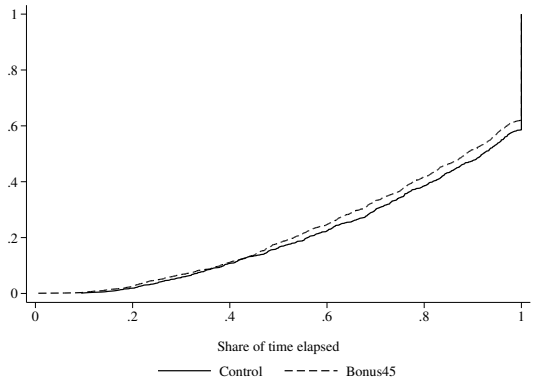
Figure A.2: Student sample: Hazard rates (completion) across time



Notes: The figure shows the hazard rates for student teams in *Bonus45* and *Control*.

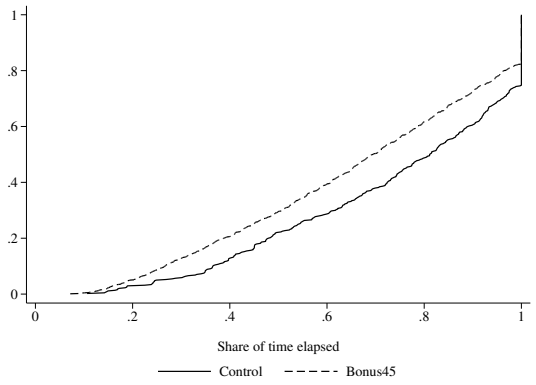
A.4 Hint taking (standardized length of the challenge)

Figure A.3: Customer sample: Fraction of hints taken by time spent in the task



Notes: The figure shows the average fraction of hints taken for customer teams in *Bonus45* and *Control* conditional on the challenge's standardized length. To standardize the length, we focus on the share of time elapsed relative to a team's total time in the challenge (which equals the time it took the team to complete the task, or 60 minutes if it did not complete the task).

Figure A.4: Student sample: Fraction of hints taken by time spent in the task



Notes: The figure shows the average fraction of hints taken for student teams in *Bonus45* and *Control* conditional on the challenge's standardized length. To standardize the length, we focus on the share of time elapsed relative to a team's total time in the challenge (which equals the time it took the team to complete the task, or 60 minutes if it did not complete the task).

A.5 Treatment form for bonus treatments

Bonus treatment teams had to sign a form, indicating that they understood the treatment procedures. For teams in the loss frame, the form further included the obligation to give back the money in case the team did not qualify for the bonus. Only one member of each team signed the form, and the forms differed between the customer and student sample only in the amount of the bonus mentioned (€50 for the customer sample and €30 for the student sample). Similarly, the forms of *Bonus45* and *Bonus60* only differed in the time set for receiving the bonus.

The form for *Gain45* said "As usual, you have one hour in total to escape from the room. Furthermore, we have a special offer for you today: If you escape from the room within 45 minutes, you will receive €50." The form for *Loss45* said "As usual, you have one hour in total to escape from the room. Furthermore, we have a special offer for you today: You now receive €50. If you do not escape from the room within 45 minutes, you will lose the €50."

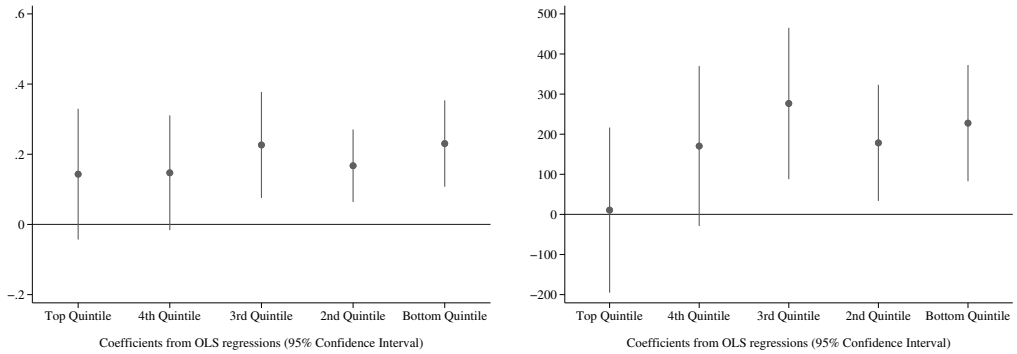
A.6 Text of the invitation to laboratory participants

We added the following paragraph to the standard invitation for student participants in the framed field experiment: "Notice: This experiment consists of two parts, of which only the first part will be conducted on the premises of the MELESSA laboratory. In Part 1 you will be paid for the decisions you make. Part 2 will take place outside of the laboratory. You will take part in an activity with a participation fee. Your compensation in Part 2 will be that the experimenters will pay the participation fee of the activity for you."

A.7 Additional analyses for the field experiment

A.7.1 Bonus incentives and team characteristics

Table A.3 shows the results for how teams' remaining times are affected by incentives. Column (1) includes no interactions and uses the same variables and fixed effects as Column (4) in Table A.5. In Columns (2) to (6), we add interactions with observable team characteristics. The findings from these models show that incentives are more effective for experienced teams (p-value $= 0.10$ for the interaction term in Column (4)). Further,



Notes: Panel A shows the effect of incentives on the residualized probability of completing the task within 45 minutes by quintiles. Teams were assigned to quintiles based on their predicted performance using observable team characteristics (and how predictive these were in *Control*). Panel B, in the same fashion, shows the effect of incentives on the residualized remaining time by quintiles.

Figure A.5: Incentive effects by quintiles

teams with more men have higher remaining times (2), but incentives are less effective for them (in line with the idea of potential ceiling effects). Similar to the analyses regarding the probability to finish the task within 45 minutes, other team characteristics do not significantly alter the efficacy of the bonus incentive.

Finally, we shed light on whether the efficacy of incentives differs for teams expected to perform well based on observable characteristics. To do so, we first estimate how observable team characteristics affect their remaining times in *Control*. Based on the obtained coefficients, we then predict for each teams in *Bonus45* and *Control* their performance, and sort all teams into the respective quintiles. We build the residualized completion probability and remaining time by subtracting the predicted performance from actual performance. In a second step, we estimate the treatment effect for the residualized probability to solve the task within 45 minutes (Panel A of Figure A.5) and teams' remaining times (Panel B of Figure A.5) for each quintile.

Both panels show that incentives do not statistically significantly improve outcomes for teams in the top quintile, who may already be exerting a lot of effort and thus have less scope for improvement. Notably, incentives are effective for all other quintiles. For these, we observe strong and statistically significant effects.[66]

[66] p-values for the fourth quintile are $p = 0.093$ for the residualized remaining time and $p = 0.077$ for the residualized completion probability.

Table A.3: GLM regressions: Remaining time

	GLM: Remaining time in seconds					
	(1)	(2)	(3)	(4)	(5)	(6)
Bonus45	0.257**	0.569***	0.612	0.154	0.256*	0.276**
	(0.116)	(0.209)	(0.482)	(0.127)	(0.155)	(0.125)
Share of men	0.513***	0.867***	0.513***	0.509***	0.513***	0.507***
	(0.134)	(0.159)	(0.132)	(0.130)	(0.134)	(0.137)
Group Size	0.286***	0.287***	0.327***	0.288***	0.286***	0.287***
	(0.048)	(0.047)	(0.052)	(0.048)	(0.048)	(0.048)
Experience	0.336***	0.343***	0.334***	0.186	0.336***	0.340***
	(0.086)	(0.086)	(0.086)	(0.121)	(0.087)	(0.084)
Private	0.197**	0.195*	0.196**	0.188**	0.195	0.198**
	(0.098)	(0.104)	(0.098)	(0.095)	(0.162)	(0.098)
English speaking	-0.333	-0.352	-0.333	-0.347	-0.334	-0.160
	(0.240)	(0.235)	(0.236)	(0.237)	(0.241)	(0.201)
Bonus45 ...						
... × Share of men		-0.562**				
		(0.256)				
... × Group Size			-0.072			
			(0.086)			
... × Experience				0.244		
				(0.148)		
... × Private					0.003	
					(0.177)	
... × English speaking						-0.281
						(0.460)
Constant	4.136***	3.929***	3.942***	4.162***	4.137***	4.073***
	(0.387)	(0.356)	(0.369)	(0.384)	(0.401)	(0.373)
Staff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	487	487	487	487	487	487

Notes: The table shows coefficients from a GLM with log link. The dependent variable is the remaining time in seconds. All models include staff and week fixed effects as in Table 2. Robust standard errors clustered at the day level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

A.7.2 Probability of completing the task within 45 minutes (field experiment)

Table A.4 reports the results for the regression Columns (1)–(5) of Table 2, excluding those weeks where we do not observe variation in the outcome variable. These results confirm our previous findings.

Table A.4: Main treatment probit regressions: Excluding weeks with no variation in the outcome variable

	Probit (ME): Completed in less than 45 minutes			
	(1)	(2)	(3)	(4)
Bonus45	0.150***	0.151***	0.183***	0.163***
	(0.026)	(0.024)	(0.027)	(0.045)
Fraction of control teams completing the task in less than 45 min	0.11	0.11	0.11	0.11
Control variables	No	Yes	Yes	Yes
Staff fixed effects	No	No	Yes	Yes
Week fixed effects	No	No	No	Yes
Observations	451	451	451	451

Notes: The table reports average marginal effects from probit regressions of whether a team completed the task within 45 minutes on our treatment indicator. All models include control variables, staff, and week fixed effects as in Table 2. All models exclude weeks that perfectly predict failure to receive the bonus. Robust standard errors clustered at the day level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

A.7.3 Regression analysis for remaining time as dependent variable (field experiment)

We also estimate the effects of bonuses on the remaining time in seconds. Because our outcome measure is strongly right skewed and contains many zeros (as there is no time left for those not finishing the task at all), we estimate a GLM regression with a log link, again employing cluster-robust standard errors in Table A.5. Column (1) starts out with our baseline specification, which includes a dummy for the incentive treatments (pooled) only. Bonus incentives significantly increase performance (measured by the remaining time). Analogously to our analysis in Table 2, we add the set of observable controls in Column (2). In Column (3) we add staff fixed effects. Column (4) presents the results from an estimation that also includes week fixed effects.

Table A.5: GLM regressions: Remaining time

	GLM: Remaining time in seconds			
	(1)	(2)	(3)	(4)
Bonus45	0.432***	0.447***	0.406***	0.257**
	(0.088)	(0.096)	(0.094)	(0.116)
Constant	5.842***	4.041***	4.251***	3.803***
	(0.082)	(0.393)	(0.359)	(0.403)
Control variables	No	Yes	Yes	Yes
Staff fixed effects	No	No	Yes	Yes
Week fixed effects	No	No	No	Yes
Observations	487	487	487	487

Notes: The table shows coefficients from a GLM regression with a log link of the remaining time on our treatment indicator. All models include control variables, staff, and week fixed effects as in Table 2. Robust standard errors clustered at the day level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

Table A.6: GLM regressions: Remaining time (all treatments)

	GLM: Remaining time in seconds			
	(1)	(2)	(3)	(4)
Bonus45	0.432***	0.436***	0.376***	0.244**
	(0.088)	(0.093)	(0.092)	(0.102)
Bonus60	0.233*	0.267**	0.392***	0.449***
	(0.131)	(0.114)	(0.126)	(0.134)
Reference Point	0.002	-0.001	0.102	0.131
	(0.106)	(0.108)	(0.114)	(0.086)
Constant	5.842***	4.044***	4.225***	3.713***
	(0.081)	(0.317)	(0.310)	(0.329)
Control variables	No	Yes	Yes	Yes
Staff fixed effects	No	No	Yes	Yes
Week fixed effects	No	No	No	Yes
Observations	722	722	722	722

Notes: The table shows coefficients from a GLM regression with a log link of the remaining time on our treatment indicators (with *Control* being the base category). All models include control variables, staff, and week fixed effects as in Table 2. Robust standard errors clustered at the day level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

Analogously to the probit regressions reported in Table 4, we also run GLM specifications with the remaining time as the dependent variable for the full set of incentive treatments. The results in Table A.6 confirm our findings that incentives that include rewards increase performance, whereas only mentioning the reference performance does not.

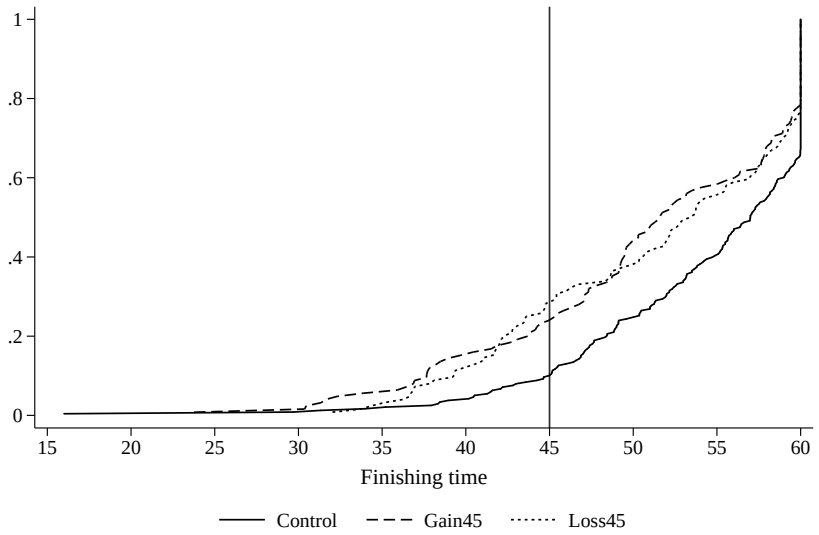
A.7.4 Framing of bonus incentives (field experiment)

As explained in Section 2, for roughly one-half of the teams in *Bonus45*, we framed the bonus incentives as gains, while the other half faced a loss frame. Participants arrived at the facility not expecting any payment at all, and therefore both frames have the same absolute distance from a reference point of zero.[67] Figure A.6 shows the cumulative distributions of finishing times for both frames separately.

We find that the framing of the bonus appears to be of minor importance for team performance. A Mann-Whitney test fails to reject the null hypothesis that the finishing times for the two framings come from the same underlying distribution (p-value $= 0.70$). Also, the fraction of teams completing the task within 45 minutes does not differ significantly (in *Gain45*, 24% of teams finish within 45 minutes; in *Loss45* 28% of teams do so, χ^2 test, p-value $= 0.45$).

Further, the fraction of teams completing the task in 60 minutes (78% in *Gain45* and 77% in *Loss45*) does not differ significantly (χ^2 test, p-value $= 0.85$), and no statistically significant differences are observed for the remaining times across frames. In *Gain45*, teams have, on average, 36 seconds more left than those in *Loss45*, and the successful teams in *Gain45* have, on average, 37 seconds more left than those in *Loss45* (Mann-Whitney test, p-value $= 0.71$). Table A.7 summarizes these different performance measures, and Table A.17 in Section A.12 highlights that the observed incentive effect is robust to controlling for MHT using procedures recommended in List et al. (2019).

[67] It seems unlikely that participants were forming any other reference point than zero. Payment for the activity was usually made weeks in advance through the company's website and should therefore not affect reference points when entering the facility at a much later date.



Notes: The figure shows the cumulative distribution of finishing times with bonus incentives framed as either gains, losses, or without bonuses. The vertical line marks the time limit for the bonus.

Figure A.6: Finishing times in *Gain45*, *Loss45*, and *Control* in the field experiment

Table A.7: Task performance for main treatments

	Control	*Bonus45*	*Gain45*	*Loss45*
Fraction of teams completing task in 45 min	0.10	0.26***	0.24***	0.28***
Fraction of teams completing task in 60 min	0.67	0.77**	0.78**	0.77*
Mean remaining time (in sec)	344.55	530.82***	548.57***	512.92***
Mean remaining time (in sec) if completed	515.75	688.40***	706.92***	669.49***

Notes: This table summarizes key variables and their differences across our three treatments *Control*, *Gain45*, and *Loss45* as well as the pooled bonus incentive treatment (*Bonus45*). Stars indicate significant differences from *Control* (using χ^2 tests for frequencies and Mann-Whitney tests for distributions), and *** p<0.01, ** p<0.05, * p<0.1. See Appendix Table A.17 for MHT-adjusted *p*-values according to List et al. (2019).

A.8 Additional analyses for the framed field experiment

A.8.1 Overview of performance across treatments (framed field experiment)

Table A.8 provides an overview of the fraction of teams finishing the task within 45 (60) minutes as well as the remaining times across treatments.

Table A.8: Task performance for main treatments (student sample)

	Control	*Bonus45*	*Gain45*	*Loss45*
Fraction of teams completing task in 45 min	0.05	0.11*	0.13**	0.09
Fraction of teams completing task in 60 min	0.48	0.60*	0.54	0.66**
Mean remaining time (in sec)	169.90	327.97***	321.28*	334.67***
Mean remaining time (in sec) if completed	355.98	546.62***	590.10**	510.51***

Notes: This table summarizes key variables and their differences across our three treatments *Control*, *Gain45*, and *Loss45* as well as the combined *Bonus45* (pooled) for the student sample. Stars indicate significant differences from *Control* (using χ^2 tests for frequencies and Mann-Whitney tests for distributions), and *** p<0.01, ** p<0.05, * p<0.1. P-values of non-parametric comparisons between *Gain45* and *Loss45* exceed 0.10 for all four performance measures.

A.8.2 Regression analysis for remaining time as the dependent variable (framed field experiment)

Table A.9 shows results from GLM regressions on the remaining time. Column (1) shows a positive and statistically significant effect of the bonus treatment on remaining times. The coefficient and its standard error remain roughly unchanged with the addition of controls and fixed effects. Column (5) shows the regression on the non-pooled framing treatments. The coefficients for both frames are highly significant, but the equality of coefficients of *Gain45* and *Loss45* cannot be rejected (p-value $= 0.88$).

A.8.3 Probability of completing the task in 45 minutes (framed field experiment)

Table A.10 reports the results for the regression Columns (1)–(5) of Table 5, excluding those weeks where we do not observe variation in the outcome variable. The results confirm our previous findings.

Table A.9: GLM regressions: Remaining time (student sample)

	GLM: Remaining time in seconds				
	(1)	(2)	(3)	(4)	(5)
Bonus45	0.658***	0.673***	0.664***	0.661***	
	(0.216)	(0.217)	(0.210)	(0.213)	
Gain45					0.676***
					(0.238)
Loss45					0.647***
					(0.226)
Constant	5.135***	3.816***	4.039***	3.684***	3.690***
	(0.195)	(0.678)	(0.723)	(0.894)	(0.889)
Control variables	No	Yes	Yes	Yes	Yes
Staff fixed effects	No	No	Yes	Yes	Yes
Week fixed effects	No	No	No	Yes	Yes
Observations	268	268	268	268	268

Notes: The table shows coefficients from a GLM regression with a log link of the remaining time on our treatment indicators (with *Control* being the base category). All models include control variables, staff, and week fixed effects as in Table 5. Robust standard errors clustered at the session level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

Table A.10: Main treatment probit regressions: Excluding weeks with no variation in the outcome variable (student sample)

	Probit (ME): Completed in less than 45 minutes			
	(1)	(2)	(3)	(4)
Bonus45	0.107*	0.097*	0.104**	0.111**
	(0.055)	(0.054)	(0.052)	(0.051)
Fraction of control teams completing the task in less than 45 min	0.06	0.06	0.06	0.06
Control variables	No	Yes	Yes	Yes
Staff fixed effects	No	No	Yes	Yes
Week fixed effects	No	No	No	Yes
Observations	191	191	191	191

Notes: The table reports average marginal effects from probit regressions of whether a team completed the task within 45 minutes on our treatment indicators (with *Control* as the base category). All models include control variables, staff, and week fixed effects as in Table 5. All models exclude weeks that perfectly predict failure to receive the bonus. Robust standard errors clustered at the session level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

A.8.4 Heterogeneity analysis (framed field experiment)

Table A.11: OLS regressions: Completed in less than 45 minutes

	OLS: Completed in less than 45 minutes							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bonus45	0.084**	0.024	0.061	0.120	0.091	0.048	0.004	0.070
	(0.035)	(0.303)	(0.063)	(0.211)	(0.126)	(0.111)	(0.067)	(0.046)
Age	0.003	0.001	0.003	0.003	0.003	0.003	0.003	0.003
	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Gender ratio	-0.005	-0.005	-0.039	-0.005	-0.005	-0.004	0.003	-0.004
	(0.066)	(0.066)	(0.080)	(0.066)	(0.067)	(0.066)	(0.067)	(0.066)
Overall grade	-0.047	-0.047	-0.045	-0.034	-0.046	-0.047	-0.053	-0.044
	(0.067)	(0.067)	(0.066)	(0.083)	(0.066)	(0.067)	(0.066)	(0.068)
Math grade	-0.028	-0.028	-0.029	-0.028	-0.026	-0.028	-0.023	-0.027
	(0.033)	(0.032)	(0.033)	(0.033)	(0.041)	(0.033)	(0.033)	(0.033)
Income	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Hard science	0.090	0.090	0.092	0.090	0.090	0.091	-0.007	0.088
	(0.067)	(0.067)	(0.067)	(0.067)	(0.069)	(0.067)	(0.102)	(0.067)
Experience	0.063*	0.063*	0.064*	0.063	0.063*	0.062	0.061	0.039
	(0.037)	(0.037)	(0.037)	(0.038)	(0.038)	(0.038)	(0.037)	(0.047)
Bonus45 ...								
... x Age		0.003						
		(0.013)						
... x Gender ratio			0.050					
			(0.117)					
... x Overall grade				-0.018				
				(0.105)				
... x Math grade					-0.003			
					(0.053)			
... x Income						0.000		
						(0.000)		
... x Hard science							0.144	
							(0.124)	
... x Experience								0.037
								(0.072)
Constant	0.248	0.054	0.031	-0.007	0.014	0.041	0.075	0.023
	(0.263)	(0.214)	(0.241)	(0.256)	(0.239)	(0.241)	(0.243)	(0.239)
Staff fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	268	268	268	268	268	268	268	268

Notes: The table displays coefficients from OLS regressions of whether a team completed the task within 45 minutes on our treatment indicators (with *Control* as the base category), including staff and week fixed effects. Robust standard errors clustered at the session level are in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

A.8.5 Factor analyses (questionnaires in framed field experiment)

Table A.12: Factor analyses

	Control	Bonus45	p-values
Questionnaire 1			
Factor 1 (*Collaboration*)	-0.0015	0.0007	0.7631
Factor 2 (*Team Cohesion*)	-0.0958	0.0469	0.0715
Factor 3 (*Dominance*)	-0.0834	0.0408	0.1056
Factor 4 (*Documentation*)	0.0853	-0.0417	0.0155
Factor 5 (*Intensity*)	-0.1478	0.0723	0.0066
Observations	264	540	804
Questionnaire 2			
Factor 1 (*Cooperative*)	0.0311	-0.0146	0.3406
Factor 2 (*Leadership*)	-0.2244	0.1054	0.0038
Factor 3 (*Struggling*)	0.0960	-0.0451	0.2572
Observations	117	249	366

Notes: This table reports means of factors based on factor analyses of two questionnaires as part of the framed field experiment. For questionnaire 1, five factors survived the factor analyses, while three factors survived the analyses for questionnaire 2. Items from questionnaire 1 that load heavy on factor 1 are (5) (6), (7), (9), (13), (15), and (18). Items loading heavy on factor 2 are (8), (10), (12), (16), (17), and (19). Items loading heavy on factor 3 are (2) and (4). Items loading heavy on factor 4 are (3) and (11). Items loading heavy on factor 5 are (1) and (14). Items from questionnaire 2 that load high on factor 1 are (1, negatively), (5), (7), (8), (10), (11), and (12). Items that load high on factor 2 are (2) and (6). Items that load high on factor 3 are (3), (4), and (9). Numbers in parentheses refer to the questions in Table 9. The last column contains p-values from Mann-Whitney tests.

A.9 Ordered probit regressions for natural and framed field experiment: Hint taking

Table A.13: Ordered probit regressions: Number of hints requested

	Ordered probit: Number of hints requested					
	Field experiment		Framed field experiment		Combined	
	within 60 min	within 45 min	within 60 min	within 45 min	within 60 min	within 45 min
	(1)	(2)	(3)	(4)	(5)	(6)
Bonus45	0.086	0.190	0.395***	0.933***	0.368***	0.884***
	(0.148)	(0.129)	(0.148)	(0.147)	(0.116)	(0.125)
Field					-2.260***	-2.012***
					(0.663)	(0.396)
Bonus45 x Field					-0.306	-0.723***
					(0.193)	(0.186)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Staff FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	487	487	268	268	755	755

Notes: The table shows coefficients from an ordered probit model of the number of hints requested within 60 or 45 minutes regressed on our treatment indicator *Bonus45* (pooled). The sample in Columns (1) and (2) is restricted to the (natural) field experiment and in Columns (3) and (4) to the framed field experiment. Columns (5) and (6) include both samples. *Field* is a dummy equal to one for the (natural) field experiment. Controls and fixed effects (FE) are identical to previous tables. Robust standard errors clustered at the day (for the field experiment) or session (for the framed field experiment) level are reported in parentheses, and *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

A.10 Hint taking at a specific step in the task

We have argued that it is unlikely that hint-taking behavior alone can explain the observed performance increase of the customer teams facing incentives. In what follows, we provide some additional evidence on the relationship between hint taking and performance in our experiment. When doing so, we must deal with two opposing effects. First, from a theoretical perspective, worse teams are more likely to use hints (which is also reflected in the positive correlation between finishing times and the number of hints taken). Second, faster teams are more likely to take hints earlier on as they are likely to reach a difficult quest faster than slower teams. That is, if incentives make (worse) teams faster, these teams may also mechanically take more hints and this effect accumulates over time.

Table A.14: Ordered probit regressions: Number of hints taken when entering last room (field experiment)

	Ordered probit: Number of hints taken				
	(1)	(2)	(3)	(4)	(5)
Bonus45	-0.018	0.012	0.113	0.050	0.134
	(0.115)	(0.113)	(0.084)	(0.110)	(0.137)
Control variables	No	Yes	Yes	Yes	Yes
Staff fixed effects	No	No	Yes	Yes	Yes
Week fixed effects	No	No	No	Yes	Yes
Room fixed effects	No	No	No	No	Yes
Observations	461	461	461	461	461

Notes: The table shows coefficients from an ordered probit model. The dependent variable is the number of hints taken at the intermediate step of entering the last room. All models include control variables, staff, and week fixed effects as in Table 2. Robust standard errors clustered at the day level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

To particularly reduce the impact of the second effect, we collected information on the time at which a subsample of 461 out of the 487 teams reached a specific intermediate step, and compare the number of hints taken at that specific step. This allows us to control the number of quests solved and to relate fixed progress in the task to hints taken. We focus on the point in time at which teams entered the last room of their specific task (*Zombie Apocalypse*, *The Bomb*, *Madness*), as they reach this step rather early in the escape game, on average. Teams facing incentives complete this step after 22 minutes on average, whereas teams in the control condition need, on average, 24 minutes (Mann-Whitney test, p-value$= 0.02$). Hence, teams facing the incentive condition also outperform control teams early in the task.

In Table A.14 we report results from ordered probit models to study whether teams facing incentives take more hints before the intermediate step. All five specifications reveal that team incentives do not significantly affect the number of hints taken, and none of the marginal effects of moving from one category to another (e.g., from one to two hints) turns out to be statistically significant.

In contrast to the customer teams, we have shown that student teams (assigned to the task by us) took more hints when facing incentives, on average. Repeating the analysis on reaching the intermediate step for the student sample shows that students facing incentives reached the intermediate step significantly earlier (on average, they entered the last room after 31 minutes in *Control* and after 27 minutes when facing incentives;

Table A.15: Ordered probit regressions: Number of hints taken when entering last room (framed field experiment)

	Ordered probit: Number of hints taken				
	(1)	(2)	(3)	(4)	(5)
Bonus45	0.244**	0.235*	0.285**	0.306***	0.361**
	(0.122)	(0.123)	(0.119)	(0.117)	(0.154)
Control variables	No	Yes	Yes	Yes	Yes
Staff fixed effects	No	No	Yes	Yes	Yes
Week fixed effects	No	No	No	Yes	Yes
Room fixed effects	No	No	No	No	Yes
Observations	267	267	267	267	267

Notes: The table shows coefficients from an ordered probit model. The dependent variable is the number of hints taken at the intermediate step of entering the last room. All models include control variables, staff, and week fixed effects as in Table 5. Robust standard errors clustered at the session level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

Mann-Whitney test, p-value$= 0.004$), but they also took significantly more hints before reaching this step (see Table A.15).

A.11 Hint taking and risk aversion

One might be concerned that original solutions may be perceived as riskier, particularly when incentives are at play. To reduce exposure to such risks, participants from the student sample (who may have different levels of risk aversion compared to customers) simply request more hints under incentives, thus mechanically inducing the difference in requested hints across treatment conditions. However, the data from our framed field experiment allow us to test whether heterogeneity in the willingness to take risks is decisive for hint taking and whether incentives interact with the willingness to take risks.

Using our measure for risk-taking in general (Dohmen et al., 2010), we regress the number of hints taken (within 60 and 45 minutes) on the incentive condition, whether the teams' propensity to take risk lies above or below the median, and the interaction between these two explanatory variables. Table A.16 shows that both below-median risk-taking and the interaction term do not significantly affect hint-taking behavior. Columns (2) and (4) show the same results but include additional controls as well as host and week fixed effects. All columns show that risk preferences appear to play a minor role in terms of magnitude and significance (compared to the treatment) and do not interact signif-

Table A.16: OLS regressions: Number of hints requested

	Number of hints requested within			
	60 mins		45 mins	
	(1)	(2)	(3)	(4)
Bonus45	0.407**	0.366**	0.877***	0.853***
	(0.189)	(0.174)	(0.170)	(0.151)
Below-median willingness to take risks	0.125	0.121	0.224	0.236
	(0.223)	(0.211)	(0.210)	(0.227)
Bonus45 x	-0.081	-0.048	-0.076	-0.095
Below-median willingness to take risks	(0.269)	(0.262)	(0.265)	(0.282)
Constant	3.686***	4.706***	2.235***	3.109***
	(0.163)	(0.717)	(0.141)	(0.650)
Control variables	No	Yes	No	Yes
Staff fixed effects	No	Yes	No	Yes
Week fixed effects	No	Yes	No	Yes
Observations	268	268	268	268

Notes: The table shows coefficients from OLS regressions of the number of hints requested in the framed field experiment within 60 or 45 minutes regressed on our treatment indicator *Bonus45* (pooled), whether the team's propensity to take risk in general lies above or below the median, and the interaction of those variables. Controls and fixed effects are identical to previous tables. Robust standard errors clustered at the session level are reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

icantly with incentives. Hence, we deem it unlikely that greater risk aversion coupled with bonus incentives leads to fewer original solutions in our setting.

A.12 Multiple hypotheses testing (adjusted p-values)

A.12.1 Field experiment

Table A.17 presents MHT-adjusted p-values according to Theorem 3.1 in List et al. (2019), by simultaneously testing for differences in multiple outcomes and treatments (where appropriate). For the pooled treatment effect (*Bonus45* vs. *Control*), we correct for multiple outcomes. For the effects of *Gain45* and *Loss45*, we correct for multiple outcomes and treatments and perform all pairwise comparisons simultaneously. The pooled treatment effect is still significant at the 1% level for all four outcome variables. Both *Gain45* and *Loss45* significantly increase the fraction of teams completing the task within 45 minutes and significantly reduce unconditional and conditional remaining times. Only the fraction of teams finishing the task within 60 minutes in *Gain45* (vs. *Control*, p-value$= 0.11$) and *Loss45* (vs. *Control*, p-value$= 0.14$) fails to differ significantly at the 10% level when performing 12 tests simultaneously. Outcomes in *Gain45* and *Loss45* treatments do not differ.

Table A.17: Field experiment: MHT-adjusted p-values according to List et al. (2019) (referring to Table A.7)

Outcome	*Control* vs. *Bonus45*	*Control* vs. *Gain45*	*Control* vs. *Loss45*	*Gain45* vs. *Loss45*
Fraction completing in 45 min	0.0003	0.0073	0.0003	0.7773
Fraction completing in 60 min	0.0083	0.1050	0.1443	0.8523
Mean remaining time (in sec)	0.0003	0.0003	0.0080	0.8367
Mean r. time (in sec) if completed	0.0010	0.0173	0.0523	0.8343

Notes: This table shows MHT-adjusted p-values according to List et al. (2019) for comparisons of *Control* versus the pooled bonus incentive treatment (*Bonus45*, corrected for multiple outcomes), as well as *Control* vs. *Gain45*, *Control* vs. *Loss45*, and *Gain45* vs. *Loss45*, all adjusted for multiple outcomes and treatments in testing for all pairwise comparisons.

Table A.18: Field experiment: MHT-adjusted p-values according to List et al. (2019)

Outcome	*Control* vs. *Bonus45*	*Control* vs. *Bonus60*	*Control* vs. *Reference Point*
Fraction completing in 45 min	0.0003	0.2030	0.8943
Fraction completing in 60 min	0.0543	0.2203	0.9080
Mean remaining time (in sec)	0.0003	0.3570	0.9850
Mean r. time (in sec) if completed	0.0003	0.8717	0.9260

Notes: This table shows MHT-adjusted p-values according to List et al. (2019) for comparisons of *Control* vs. *Bonus45*, *Control* vs. *Bonus60*, and *Control* vs. *Reference Point*, adjusted for multiple outcomes and treatments.

Table A.18 relates to Table 4 and presents MHT-adjusted p-values by simultaneously testing for differences in multiple outcomes and treatments (*Bonus45*, *Bonus60*, and *Reference Point* to *Control*). Our main treatment *Bonus45* is still significant at conventional levels. The increase in the fraction of teams finishing the task (in 45 or 60 minutes) in *Bonus60* and the reduction in the remaining times is too small to reach significance at conventional levels when adjusting p-values conservatively for 12 simultaneous tests. However, even these adjusted p-values are substantially smaller than the p-values for the *Reference Point* treatment, which has essentially no effect on the four outcome variables. Hence, our conclusion remains that we do not observe any performance effects solely due to introducing reference points.

A.12.2 Framed field experiment

Table A.19 refers to Table A.8 and shows MHT-adjusted p-values according to Theorem 3.1 in List et al. (2019), by simultaneously testing for differences in multiple outcomes and

Table A.19: Framed field experiment: MHT-adjusted p-values according to List et al. (2019) (referring to Table A.8)

Outcome	*Control* vs. *Bonus45*	*Control* vs. *Gain45*	*Control* vs. *Loss45*	*Gain45* vs. *Loss45*
Fraction completing in 45 min	0.0830	0.2163	0.6720	0.6687
Fraction completing in 60 min	0.0520	0.5837	0.0883	0.4430
Mean remaining time (in sec)	0.0023	0.0807	0.0107	0.8353
Mean r. time (in sec) if completed	0.0320	0.0547	0.2123	0.6913

Notes: This table shows MHT-adjusted p-values according to List et al. (2019) for comparisons of *Control* vs. the pooled bonus incentive treatment (*Bonus45*; corrected for multiple outcomes), as well as *Control* vs. *Gain45*, *Control* vs. *Loss45*, and *Gain45* vs. *Loss45*, all adjusted for multiple outcomes and treatments in testing for all pairwise comparisons.

treatments (where appropriate) for the framed field experiment. After adjusting p-values for testing on multiple outcomes, the pooled treatment effect is still significant at conventional levels for all four outcome variables. Further, the remaining times significantly differ between *Gain45* and *Control* and *Loss45* and *Control* when correcting for testing on multiple outcomes and all pairwise comparisons simultaneously.

A.13 Additional leadership analyses

Table A.20: GLM: Leadership, remaining time

	Remaining time in seconds			
	(1)	(2)	(3)	(4)
Leadership	0.542***	0.550***	0.544***	0.598***
	(0.167)	(0.164)	(0.191)	(0.190)
Mean in control	191.1	191.1	191.1	191.1
Controls	No	Yes	Yes	Yes
Staff FE	No	No	Yes	Yes
Week FE	No	No	No	Yes
Observations	281	281	281	281

Notes: The table displays coefficients from GLM regressions with a log link of remaining time on the *Leadership* indicator (with *Control-L* as the base category). Each column indicates whether team controls (group size, share of men, experience with escape games, median age, language spoken, private versus team-building events, actively taken walkie-talkie), staff, and week fixed effects are included. Standard errors in parentheses are clustered at the daily level, with significance levels * = $p < 0.10$, ** = $p < 0.05$, and *** = $p < 0.01$.

Table A.21: Team performance (completion and remaining time with room fixed effects)

	Completed within 60 minutes				Remaining time in seconds			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Leadership	0.137***	0.137***	0.125**	0.105**	0.378***	0.354***	0.298**	0.321**
	(0.045)	(0.047)	(0.058)	(0.043)	(0.144)	(0.126)	(0.143)	(0.161)
Mean in control	0.442	0.442	0.442	0.442	191.1	191.1	191.1	191.1
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Staff FE	No	No	Yes	Yes	No	No	Yes	Yes
Week FE	No	No	No	Yes	No	No	No	Yes
Observations	281	281	281	281	281	281	281	281

Notes: The table displays average marginal effects from probit regressions of whether a team completed the task within 60 minutes (Columns (1)–(4)) and coefficients from GLM regressions with a log link of remaining time (Columns (5)–(8)) on the *Leadership* indicator (with *Control-L* as the base category). All columns include room fixed effects. Each column indicates whether team controls (group size, share of men, experience with escape games, median age, language spoken, private versus team-building events, actively taken walkie-talkie), staff, and week fixed effects are included. Standard errors in parentheses are clustered at the daily level, with significance levels * = $p < 0.10$, ** = $p < 0.05$, and *** = $p < 0.01$.

A.14 Incentives and effort dimensions

A.14.1 Expert survey

In addition to highlighting (the demand for) leadership as a central mechanism of how incentives improve team performance, we also explore which effort dimensions may be affected by incentives in non-routine team tasks. Based on numerous comments in seminars, workshops, and conference presentations, we compiled a list of 10 potentially important effort dimensions (see Table A.22) through which incentives may impact team performance. We then recruited experts with knowledge of behavioral and experimental economics, as well as personnel and organizational economics, to participate in an online survey to consider the relative importance of incentives for each of these dimensions.[68]

We contacted 104 academic economists whom we identified as working on the role of incentives in the workplace, being broadly concerned with studying the effects of (financial) incentives, or contributing to the field of personnel economics (if we deemed their work relevant to our study). In January 2020, these experts received an email containing a link inviting them to fill in the survey (henceforth the expert sample). A few days later, we also sent the invitation to the ESA's discussion mailing list (ESA discuss) using a different link and thus generating results from a second sample consisting mostly of

[68]The survey's entire design, timing, and intended analysis was pre-registered. For details, see `https://aspredicted.org/hc8r7.pdf`.

Table A.22: Survey results

Statement	Average rank		# of wins in pair wise comparisons	
With incentives, ...	Experts	ESA	Experts	ESA
...teams communicate more (or less).	3.54	4.52	9	7
...teams share information better (or worse) among members.	4.00	4.92	8	6
...teams select the most skilled person for a specific problem.	4.68	4.38	7	8
...team members are more (or less) likely to take the initiative to lead the team.	4.68	5.40	6	4
...team members spend more (or less) time working collaboratively on a specific problem (as opposed to individually).	5.25	5.51	4	2
...teams are more (or less) likely to give in to distractions.	5.50	4.54	2	8
...teams select the most confident person for a specific problem.	5.57	5.57	4	3
...teams allocate more (or less) time on information search relative to problem solving.	5.93	5.28	3	4
...teams allocate effort more (or less) unevenly across stages of the task.	6.00	6.02	1	1
...teams think more (or less) outside the box.	7.25	6.57	0	0
Observations	28	65	28	65

Notes: This table reports how our sample of experts and the sample of respondents on the ESA discuss mailing list rank the different dimensions of team production that incentives can affect. Average rank reports the average rank assigned to a statement (from 1 to 10) across all respondents within the respective sample (i.e., the lower the average rank, the more important respondents deem this dimension). # of wins in pairwise comparisons indicates how many other statements will lose in a pairwise comparison (round-robin tournament) in the respective sample (i.e., the higher the number, the more important respondents deem this dimension).

researchers active in behavioral and experimental economics (henceforth the ESA sample).

Survey participants could rank the 10 possible effort dimensions from most to least affected by incentives and add additional dimensions, if they wished so.[69] The survey assessed the relative importance of the 10 different effort dimensions. It also included questions on respondents' beliefs about the effectiveness of incentives (and their framing) on task performance, their familiarity with our paper (and some related research), their experience in conducting experiments on incentives, and their academic seniority.

We received 39 responses from the expert sample and 121 from the ESA sample. In line with our pre-registration, we eliminated respondents who took less than 60 seconds, suggesting they did not fill in the survey carefully. We also removed those who did not rank all dimensions, leaving us with 28 responses from the expert sample and 65 from the ESA sample.

[69]None of the respondents recommended any additional effort dimension for consideration.

Table A.22 shows the 10 statements and their average rank of each statement across our two samples as well as the number of wins of each statement in pairwise comparisons with the other statements. As the results show, respondents in both samples strongly agreed on the relative importance of the three statements listed at the top: "With incentives, teams communicate more (or less)," "With incentives, teams share information better (or worse) among members," and "With incentives, teams select the most skilled person for a specific problem." In both samples, these three dimensions rank among the top 4 and win in at least 6 pairwise comparisons.

For dimensions that experts rank as the top 4–6, there is somewhat less consensus. While both experts and ESA members agree to some extent that incentives affect the likelihood of team members taking the initiative—as expressed by the statement "With incentives, team members are more (or less) likely to take the initiative and lead the team" (rank 4 for experts and rank 5 for the ESA sample)—experts consider incentive effects on joint problem solving ("With incentives, team members spend more (or less) time working jointly") and concentration ("With incentives, teams are more (or less) likely to give in to distractions") as relatively more important than ESA respondents do.

In contrast, ESA respondents consider incentive effects for concentration and for how time is spent ("With incentives, teams allocate more (or less) time on information search relative to problem solving") as relatively more relevant than the effects of incentives on joint problem solving. Finally, respondents in both samples consider the role of incentives relatively unimportant for effort provision across time ("With incentives, teams allocate effort more (or less) unevenly across stages of the task"). They also do not expect that "with incentives, teams allocate more (or less) time on information search relative to problem solving."

A.14.2 Additional laboratory experiment: Description

As part of the survey pre-registration, we performed a small-scale laboratory experiment with a non-routine team task that mimicked the real-life escape room challenge. This task was tailored to test how incentives affect the three effort dimensions survey respondents had ranked as most important (skill-to-task matching, information sharing, and communication).

Our laboratory experiment is based on a board game version of a real-life escape game. The board game resembles similar features as our field setting but allows us to

alter some subtasks to explicitly study the causal effects of team incentives on the three effort dimensions. We first test if incentives causally affected whether teams assign the most skilled team member to a specific subtask (skill-to-task matching). Next, we investigate the causal effect of incentives on the likelihood of team members sharing relevant information (information sharing) to facilitate task completion. Finally, we study the causal effect of incentives on communication.

As participants arrived at the laboratory, they were randomized into teams of three, and each team was guided to a separate room to perform the task (with treatments being randomized across these rooms as well). In each room, one experimenter welcomed the participants and explained the general procedures, before each participant underwent a cognitive skill test (Raven's progressive matrices) on a computer tablet at a separate workstation. After completing the test, each participant received their own test score as private information, but no participant was informed about their team members' performance on the test. Then, all three participants were guided to stand around a large table in the middle of the room to start the board game escape challenge.

The board game escape challenge was framed as a secret mission in which participants needed to gain access to the palace of a fictitious country's (part I), find some secret information in the palace (part II), and escape (part III), all within 60 minutes. Each part contained several subparts (e.g., part I.2 denotes subpart 2 of part I).

Participants were guided by a tablet computer placed in the middle of the table. The tablet displayed the time left to solve the escape challenge and electronically recorded task solutions entered by the team. It also displayed hints to help teams make progress at pre-specified times (i.e., all teams received the exact same hints at the exact same time, a feature adapted from the original board game our team challenge is based on). To take notes, each participant received a pen and paper, and was equipped with an identical decoding sheet. Further, each team member received an envelope containing a text with information about the layout of the leader's office in the palace. While this text mostly contained entertaining but useless information, it also included, uniquely for each team member, some information that could aid in solving part II.2. Participants were explicitly told that they were not allowed to share this information at that stage but were not explicitly informed that combining this information could help to solve part II.2 much faster.

After participants indicated that they were ready to start the experiment, a 60-minute clock was started on the tablet computer, and the team received an envelope containing the materials for part I.1. These materials included a name tag with an empty field at the bottom titled "personal code," an invitation letter to the palace opening containing the information to "bring your personal code," a solution sheet displaying a matrix of numbers, several keys, and a white paper strip with small dots and stripes on both sides. At this stage, the tablet computer asked participants to enter their personal code, which could be found by combining the dots and stripes shown on both sides of the paper strip. The resulting pattern could then be decoded (using the decoding sheet distributed initially) to obtain the personal code.[70]

After completing this part, subjects advanced to part I.2 and then to part II.1. We designed parts I.2 and II.1 to be similar yet challenging to subjects. The materials for part I.2 consisted of five different flags, an invitation card reminding subjects not to speak (if communication was prohibited in part I.2), a text of the country's national anthem, and a note from the country's leader, saying that the combination of the country's flag and the personal code would yield the solution to part I.2.

To arrive at the solution, participants had to study the anthem's text to identify the correct flag.[71] They could then use the solution sheet from part I.1 to identify the correct four-digit number needed to solve the quests in part I.2. Using the keys handed out in part I.1 (which bore single-digit numbers), subjects needed to select the four keys (in the right order) on the tablet computer to end part I.[72] After they managed to do so, the experimenter distributed materials for part II.1.

In part II.1, participants received information cards for five different fictitious enemy countries (with a map of each country and some basic info such as GDP and strength of armed forces), a solution sheet containing a matrix that would yield two of the four correct keys to terminate part II, and a speech by the leader describing the country he considered to be the greatest enemy (containing a reminder not to speak should verbal communication be prohibited in part II.1). Selecting the greatest enemy country could

[70] Each time participants failed to enter the correct code, three minutes were subtracted from the available time.

[71] Each time participants chose the wrong flag, three minutes were subtracted from the available time.

[72] If participants failed to enter the correct key code, one minute was subtracted from the available time.

be achieved by combining clues from the speech with the information on the country information cards and then using the matrix on the solution sheet.[73]

Verbal communication was randomly prohibited in either part I.2 or part II.1, and this was announced only at the beginning of the respective part. The communication ban was implemented by the experimenter under the threat of exclusion, and after the respective subpart was solved, the experimenter also immediately announced that the team could communicate again. In half of all sessions, the contents of part II.1 and part I.2 were exchanged to avoid order effects. This exogenous variation of the availability of verbal communication was introduced to allow for an analysis of the effects of incentives on performance through communication in a difference-in-differences analysis.[74]

In part II.2, subjects could share the information distributed before the experiment started. Importantly, the information provided was sufficient but not necessary to arrive at the correct solution. Alternatively, subjects could also not share the information and use the materials provided to work on the part's solution. By comparing how much information was shared across treatments with and without incentives, this subpart allows us to determine the causal effect of incentives on information sharing.

The materials for part II.2 were a picture of the leader's office, instructions to "count the golden eagles" displayed there, and a sheet translating Roman into Arabic numerals. Participants could simply search for all golden eagles in the picture, but they could also arrive at the solution by sharing the information they received before the experiment. Two of the three participants received information about the number of golden eagles in certain parts of the room at the beginning of the experiment. When combined, this information yielded the total number of golden eagles. This number, translated into Roman numerals, yielded the last two keys, as all keys (in addition to single-digit Arabic numbers) also each bear a Roman numeral. Entering all four keys on the tablet computer ended part II.[75]

For part III, subjects were explicitly asked to select a team member for an individual task requiring logical reasoning. They were not reminded of their cognitive skill test

[73] Each time participants chose the wrong enemy country, three minutes were subtracted from the available time.

[74] As we do not find that incentives significantly affect the extent of communication reported by our participants, we refrain from including such a difference-in-differences analysis. Further, we do not find any indication that incentives significantly affect the difference in times needed to solve the subtasks in part II.1 and part I.2 with (versus without) communication (p-value$= 0.30$, Mann-Whitney test).

[75] Each time participants entered a wrong key code, one minute was subtracted from the available time.

results obtained before the experiment and not made aware of a possible correlation between the ability to perform in the individual task of part III and this test. However, they could themselves take the initiative and discuss the results if they so wished. By comparing whether teams are more likely to choose the team member with the highest score with rather than without incentives conditions, we can estimate the causal effect of incentives on skills-to-task matching.

After the team decided for a member, this member was guided to a secluded desk, where they received the respective materials and instructions. The individual task required them to sort eight picture cards (with pictures on both sides) into a 2×4 matrix based on a number of logical statements accompanying the instructions (e.g., "the green flower pot can never be next to the green portrait"). By combining all statements, only one possible solution for arranging the picture cards remained.[76] Meanwhile, the remaining two group members worked on a variety of diverse tasks. They needed to detect a pattern in a sequence of numbers and continue the sequence, find an object hidden in a stereoscopic image, arrange keys in a specific fashion so they form the shape of a number, and use a key to follow a drawn path on a paper slip to unveil some letters. The solutions to these four tasks yielded the four keys to end part III and thus the game, while the solution to the individual task done by the third team member yielded the order in which the keys had to be entered.[77]

After participants entered the correct four keys (or if the 60 minutes expired, whichever occurred first), the task ended and participants filled in a short survey, including a question on the extent of communication within the team as well as general demographics such as age, gender, and experience with escape room (board) games. If participants were assigned to a bonus condition and managed to (did not manage to) complete the task within 45 minutes, they received (kept) the bonus payment in *BGGain45* (*BGLoss45*). Otherwise they did not receive the bonus (or handed it back in *BGLoss45*). All participants also received the participation fee and were subsequently dismissed from the laboratory.

Our power calculations for the additional laboratory experiment were based on our findings in the framed field experiment (student sample) and on assumptions about the data-generating process and performances in the respective subtasks of the additional laboratory experiment. A sample of 120 groups (with 40 groups in *BGGain45*, 40 in

[76] Each time the participant entered a wrong solution, one minute was subtracted from the available time.

[77] Each time participants entered a wrong key code, one minute was subtracted from the available time.

BGLoss45, and 40 in *BGControl*) would have allowed us to identify pooled incentive effect sizes of about 0.547 standard deviations in two-sample t-tests with statistical power of 80% at the 5% significance level. That is, if we observed similar finishing times and variances as in the framed field experiments, we could identify effects of incentives (pooled) on the remaining time that are larger than 3 minutes and 13 seconds. As in our framed field experiment, power was expected to be lower for binary outcomes such as finishing within 60 or 45 minutes. Using a χ^2 test, we could identify effect sizes larger than 17 to 27 percentage points, depending on the fraction of subjects finishing the task in *BGControl* within 45 or 60 minutes.

Following these calculations, we recruited 381 participants to form 127 teams consisting of three members each. Due to technical issues with the experimental software, we had to discard three observations. In these sessions, subjects were not acoustically made aware of a hint being displayed, distorting their progress in the game relative to other participants. We removed another five sessions by one particular research assistant as they did not administer the treatment correctly in at least one session and were the only research assistant (out of 10) to receive participants' complaints about not having properly delivered the instructions. This left us with 119 observations.

Akin to the framed field experiment, we assigned roughly two-thirds of teams to the incentive treatment (36 to *BGGain45*, 37 to *BGLoss45*), and roughly one-third to *BGControl* (46). To avoid time trends in the data from affecting our results, we ran three sessions concurrently whenever possible, to have each treatment present at any same time and day. Due to no-shows of participants, some slots featured fewer sessions.

The main aim of the additional laboratory experiment was to study whether incentives causally affect the three effort dimensions considered as most important by our survey respondents: skill-to-task matching, information sharing, and communication. To do so, we discuss below whether bonus incentives alter the quality of skill-to-task matching (i.e., the likelihood of selecting the person with the highest cognitive test score in part III). Similarly, we study whether incentives affect the number of team members sharing information in part II.2 (the "counting eagles" subtask) and whether team members' report different levels of communication in the incentive condition (team members were individually asked at the end of the experiment to what extent they agree with the statement "We communicated a lot" on a seven-point Likert scale, ranging from "fully disagree" to "fully agree"). As we do not observe any substantial treatment effects for these

outcome variables, we refrain from reporting additional robustness checks (see also our pre-registration).

A.14.3 Additional laboratory experiment: Results

Following several delays due to COVID-19, we implemented the laboratory experiment in Munich and Tilburg in August and September 2021 (under the locally applicable COVID-19 restrictions).[78] The prevailing COVID-19 regulations affected our experiment in terms of recruitment possibilities, physical distancing, and hygiene measures. All of these may have negatively influenced finishing times and difficulty as compared to the real-life escape games in our field experiments (which were conducted before the pandemic).

The fraction of teams solving the task within 60 minutes in the laboratory task amounts to only 35% (*BGIncentive45*: 33%, *BGControl*: 39%, χ^2 test p-value $= 0.49$), which is substantially lower than in our natural field experiment (72%) and our framed field experiment (56%). Focusing on primary outcomes that were directly or indirectly incentivized by the bonus condition (i.e., remaining times and task completion within the bonus target), we nevertheless observe a tendency that teams perform better in the bonus condition: teams' average remaining times amount to 203 seconds in *BGIncentive45* versus 174 seconds in *BGControl*. Incentives tend to also increase the fraction of teams solving the task within the incentive target of 45 minutes (*BGIncentive45*: 7%, *BGControl*: 2%).

Due to substantial noise in the data, these tentative results fail to be statistically significant (Mann-Whitney test for remaining times: p-value $= 0.81$; χ^2 test for fraction of teams completing the task within 45 minutes: p-value $= 0.26$). However, incentives do statistically significantly improve remaining times among teams that finish the task (617 seconds remaining in *BGIncentive45* versus 444 seconds in *BGControl*, Mann-Whitney test, p-value $= 0.088$), indicating that the bonus incentive is particularly effective among teams that are also more likely to achieve the bonus target.

Focusing on how incentives affect the three effort dimensions our survey respondents considered most important, we cannot reject that teams share information similarly with and without incentives (on average, 1.73 members share information in *BGIncentive45* (std. dev.: 1.47) versus 1.72 members do so in *BGControl* (std. dev.: 1.46), Mann-Whitney test, p-value $= 0.97$)). Similarly, incentives do not seem to alter the extent of communi-

[78] For details on our pre-registration, see also `https://www.socialscienceregistry.org/trials/8073`.

cation as reported by teams (seven-point Likert scale; mean (std. dev.) in *BGIncentive45*: 5.60 (1.28) versus 5.62 (1.39) in *BGControl*, Mann-Whitney test, p-value $= 0.58$). Finally, we observe a suggestively large yet not statistically significant difference in the likelihood that teams select the most skilled person for the logical reasoning task (84% in *BGIncentive45* versus 77% in *BGControl*, χ^2 test, p-value $= 0.40$).

Our analyses on experts' expectations provides additional guidance on interesting avenues for future research in terms of better understanding how incentives may affect different effort dimensions in non-routine tasks. Our surveys identified which effort dimensions experts consider relatively more important and thus suggest which dimensions future research may focus on in more detail. Our laboratory experiment complements this approach by showing that incentive effects do not necessarily coincide with experts' expectations. Among the top three dimensions, we could only find suggestive evidence for one dimension (skill-to-task matching).

A.15 Additional customer surveys on goals and hint taking

To identify how teams' goals are potentially shifted when teams face incentives as well as how teams perceive hint taking, we ran additional surveys with 201 customers performing the team challenge at ETR Munich in January 2023.[79]

Before participating in the escape challenge, survey participants were asked to rank eight potential goals they may pursue in the challenge from most (rank 1) to least (rank 8) important. Half were asked to rank goals for a hypothetical scenario in which they had the opportunity to win a team bonus of €50 if they completed the task within 45 minutes ("bonus" condition, $n = 100$). The other half was randomly assigned to a "no bonus" condition ($n = 101$); i.e., they ranked the goals without any bonus being mentioned.

Table A.23 summarizes our findings. As can be seen, teams care about being successful in a challenging task, uphold a good atmosphere within the team, and get out of the room as quickly as possible. They also consider taking no hints as a potential goal, whereas getting to know team members, competition within teams, or staying in the room for long are considered the least important. Interestingly, bonus incentives offered for performance do not strongly affect how goals are ranked. The only statistically significant difference exists for the goal of solving more tasks than a team member

[79] The survey was pre-registered at AsPredicted (#117067), `https://aspredicted.org/ZKKNCS`.

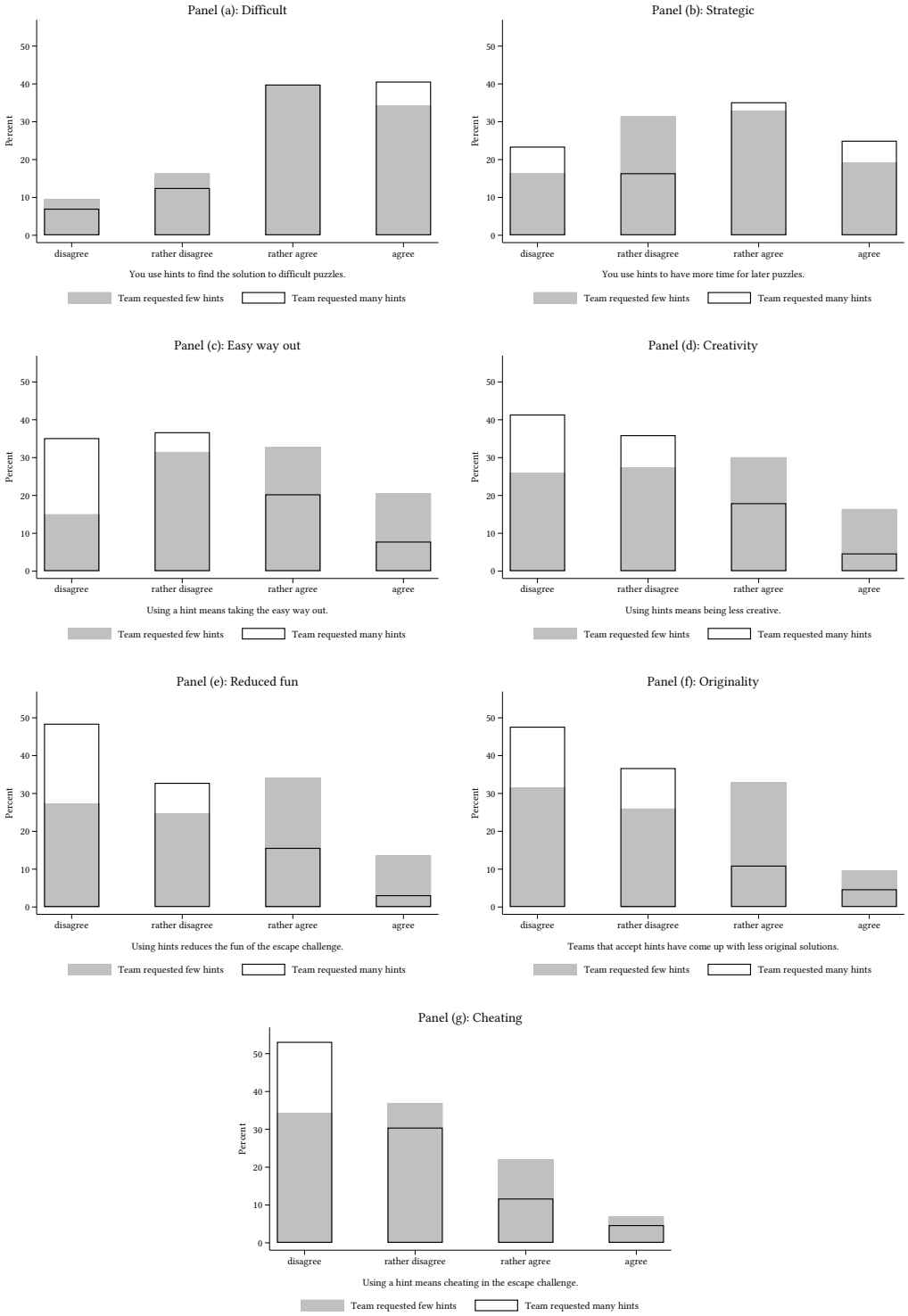
Table A.23: Goals of participating in an escape challenge

Statement	Average Rank		Wilcoxon rank sum test (p)	# of wins in pairwise comparisons	
I want to...	Bonus	No bonus		Bonus	No bonus
...achieve success together.	2.66	2.56	0.71	7	7
...create a good atmosphere in the team.	3.18	3.51	0.24	6	5
...face a challenge.	3.38	3.42	0.86	5	6
...get out of the room as quickly as possible.	3.98	4.32	0.28	4	4
...take no hints.	4.56	4.73	0.51	3	3
...get to know my team members better.	5.52	5.33	0.51	2	2
...solve more tasks than my team members.	6.16	5.61	0.10	1	1
...stay in the room as long as possible.	6.56	6.51	0.84	0	0
Observations	100	101	201	100	101

Notes: This table reports how customers of ETR rank different goals of participating in an escape challenge. Customers in *Bonus* were asked to rank these goals when a bonus incentive is in place. Average rank reports the average rank assigned to a statement (from 1 to 8) across all respondents within the respective sample (i.e., the lower the rank, the more important respondents deem this dimension). # of wins in pairwise comparisons indicates how many other statements will lose in a pairwise comparison (round-robin tournament) in the respective sample (i.e., the higher the number, the more important respondents deem this dimension).

(Mann-Whitney test, $p = 0.095$), which seems to be more important when there is no incentive scheme in place. Furthermore, while there is no statistically significant difference between the two conditions for the goals of creating a good atmosphere in the team and facing a challenge, the ordering slightly differs.

After participating in the escape challenge, survey participants had to evaluate by how much they agree with seven statements about hint taking. Figure A.7 summarizes our findings. To capture potential image concerns, the figure shows histograms of responses split by the number of hints taken by these teams. We define teams with less than three hints as those taking few hints and those with three or more hints as teams taking many hints. Both teams taking many hints and teams taking few hints agree that hints are used to find the solution to difficult puzzles (χ^2 test: p-value = 0.71), and they only have small disagreements over using hints to have more time for later puzzles (χ^2 test: p-value = 0.08). Teams that take many hints tend to perceive hint taking less often as the easy way out (χ^2 test: p-value < 0.01), but absolute differences are again small. Clearly, teams using few hints are more likely to agree that hint taking reduces fun (χ^2 test: p-value < 0.01), is less creative (χ^2 test: p-value < 0.01), reduces originality (χ^2 test: p-value < 0.01), and can be considered cheating (χ^2 test: p-value = 0.05). As such, it becomes clear that teams may refrain from hint taking if they have an intrinsic motiva-



Notes: The figure shows histograms of survey answers on the perceptions on hint taking for teams that took many hints (three or more) and team that took few hints (two or less). For each of the seven statements, subjects had to evaluate whether they disagree or agree with the respective statement on a four-point Likert scale.

Figure A.7: Perceptions about hint taking

tion to explore on their own or perceive taking hints as negative signals about their own creativity or integrity.

A.16 Survey with HR experts

To quantify a reasonable prior of the effectiveness of incentives in non-routine analytical team tasks, in February 2023, we surveyed 400 participants from a pool of HR experts, who were responsible for making hiring decisions in their jobs.[80] The sample was provided by survey provider Cint. To compare expectations about non-routine and routine tasks, we randomly assigned about half of these experts (n=197) to a condition in which expectations about non-routine analytical team tasks in general were elicited. We explained to these participants that non-routine analytical tasks require problem solving, intuition, or creativity and are often found in occupations that encompass executive or managerial functions, technical, or creative occupations (e.g., lawyers, medical and engineering professions, designers, and managers), while routine tasks were explained as those that can also be specified to be performed by a machine and are typically found in occupations with medium educational requirements (e.g., accounting, secretarial tasks, industrial production, monitoring).

To study how expectations about non-routine tasks in general differ from expectations in the context of escape challenges, we assigned 99 HR experts to a condition in which they were explicitly asked about the effectiveness of bonus incentives in escape challenges (i.e., in addition to the task description mentioned above, they learned about the specifics of the setting). Finally, to elicit informed expectations regarding our particular setting, we provided 104 HR experts (out of the 203 who were assigned to state expectations about escape games) with team' average remaining times in our control condition (these teams had, on average, six minutes remaining) before eliciting experts' expectations about the incentive effect in the escape challenge.

In all conditions, HR experts had to indicate how many out of 100 teams would i) become faster, ii) slower, and iii) do neither, once they receive the opportunity to earn a bonus. Additionally, as participants could have believed that improving teams became substantially faster, whereas declining teams only moderately slower, we also asked for the number of minutes teams would be expected to become faster/slower (conditional on becoming faster/slower), allowing us to calculate the average expected change in performance (in minutes). Translated survey instructions can be found below.

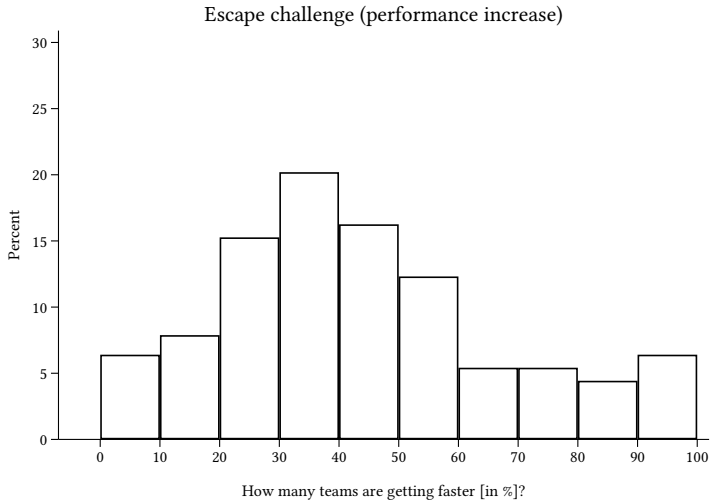
[80] The survey was pre-registered as AsPredicted (#122060), `https://aspredicted.org/1SW29C`.

Table A.24 summarizes the results from the HR expert survey. Regarding the 197 HR experts who formed expectations about abstract tasks, we find that the average expected improvement due to incentives amounts to 3.22 minutes in non-routine tasks and to 4.13 minutes in routine tasks and differs statistically significantly (Wilcoxon signed-rank test, p$= 0.01$). These experts are also more optimistic regarding the fraction of teams that improve with incentives (Wilcoxon signed-rank test, p$= 0.02$). Further, we find that experts' expectations about performance improvements in escape challenges are similar to expectations about non-routine tasks more generally (Mann-Whitney test, abstract versus no info: $p = 0.99$, abstract versus info: $p = 0.35$, pooled: $p = 0.56$). Finally, the survey revealed substantial heterogeneity in expectations within and across experts. On average, experts expect performance improvements for 39%–42% of teams in the escape challenges. While the median HR expert expects 40 out of 100 teams to improve when facing incentives, 20% of them believe that 0–20 teams will improve, while another 20% believe that 60–100 teams will improve (see also Figure A.8.)

Table A.24: Expected effect sizes

	Fraction of teams			Improvement
	faster in %	slower in %	same in %	in minutes
Abstract (n=197)				
Non-routine task	41.37	21.48	37.15	3.22
Routine task	44.55	20.23	35.22	4.13
Escape challenge (n=203)				
Escape (no info, n=99)	42.05	22.18	35.77	3.77
Escape (info, n=104)	38.80	24.42	36.78	1.97
Escape (pooled)	40.38	23.33	36.29	2.84

Notes: This table reports means of survey answers on how many teams are getting faster or slower or are not affected by a bonus incentive. It also reports the overall expected improvement (average reduction in finishing times).



Notes: The figure shows histograms of survey answers on how many teams they expect to become faster in an escape challenge, when there is a bonus incentive in place.

Figure A.8: Expected performance increase

Translated instructions

(text in square brackets only visible to participants in respective treatment condition)

Welcome!

For this survey, we want to collect your assessments of the effects of financial incentives in various team tasks. To this end, we will first provide some definitions:

Routine tasks:
Any type of task that can be specified to be performed by a machine (for example: adding multiple numbers). Routine tasks are typical of many occupations with intermediate educational requirements, for example, accounting, secretarial tasks, industrial production, or supervision.

Non-routine tasks:
Any type of task that requires problem solving, intuition, persuasion, or creativity. These tasks are often found in occupations involving managerial, technical, or creative tasks, for example, lawyers, medical and engineering occupations, designers, and managers.

[**Abstract**: For the following questions, imagine a non-routine work environment in which workers in a team must complete a series of complex tasks. All tasks must be

successfully completed within one hour (= 60 minutes). There is also a possibility that not all tasks will be successfully completed after the time has elapsed].

[**Escape**: For the following questions, imagine an Escape Game as an example of a non-routine task. In Escape Games, teams must solve a series of complex tasks to escape from a room. To do this, teams must find various clues, combine information, and think around corners. All tasks must be successfully completed within one hour (= 60 minutes). There is also a possibility that not all tasks will be successfully completed after the time has elapsed].

In addition to the usual reward, there is a consideration to introduce a bonus for the whole team, which the team will receive if the tasks are successfully completed after 45 minutes already.

[**Escape Info**: Assume that teams that are not offered a bonus will, on average, have successfully completed all tasks about 6 minutes before time expires].