Delegated Innovation Scouting when Success is Rare: A Behavioral Investigation

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Firms that engage in delegated innovation scouting face a crucial challenge: How to motivate innovation scouts to search extensively for promising innovative projects when success is uncertain and often elusive? Motivated by common industry practice, we develop a behavioral principal–agent model to analyze the optimal design of formal incentive schemes and the performance of contracts that rely on discretionary, non-contractible rewards. Our analysis shows that delegated parallel scouting confronts firms with a difficult trade-off between contractual efficiency and fairness, leading fairness-minded managers to favor superficially fair contracts that provide only weak search incentives for scouts. We test the model's key predictions in a controlled laboratory experiment that systematically varies scouts' success probabilities, contract types, and the institutional conditions for reciprocity. Our results show that managers implement superficially fair rather than efficient contracts, causing scouts to search too little, particularly when success probabilities are low. Our experiments also identify a remedy grounded in our theoretical analysis: when success is rare, generously rewarding highly successful search outcomes restores efficient search, provided that firms channel managers' fairness concerns into process and organizational design rather than into incentive design itself.

Key words: open innovation, search theory, delegation and incentives, (superficial) fairness, reciprocity

1. Introduction

Innovation scouting is a common approach firms use to identify promising innovation opportunities in their external environment (West and Bogers 2014). When engaging in innovation scouting, a firm screens a wide range of potential innovators—such as universities, research laboratories, start-ups, and even competitors—to assess whether their innovation projects could represent valuable additions to the firm's own innovation portfolio. Despite its widespread use in practice, innovation scouting has received but scant scholarly attention (some notable exceptions include Vanhaverbeke et al. 2002, Rohrbeck 2007, Lopez-Vega et al. 2016, Ehls et al. 2020).

Several factors make innovation scouting a challenging endeavor. First, reliably identifying external innovation opportunities requires specialized expertise, a deep understanding of the current (and emerging) technology landscape, and access to a broad network of potential collaborators (Arora et al. 2022). Consequently, firms rarely conduct innovation scouting directly. Instead, they often delegate this task to highly specialized *innovation scouts* whose primary responsibility is to search for external innovation opportunities (Ehls et al. 2020).

Second, it is difficult (if not impossible) for firms to verify the search activities of their scouts (Rohrbeck 2007, Ehls et al. 2020). To mitigate this information asymmetry, firms must design incentive systems that motivate scouts to search extensively for promising projects. Common incentive mechanisms include non-trivial combinations of contractible bonus payments for the discovery of promising projects and discretionary rewards (such as voluntary bonuses, promotions, collaboration opportunities, or public recognition) that are non-contractible and allocated only after observing scouting outcomes (Salo 2001, Rohrbeck 2007).

Last, in many industries, the due diligence activities required to thoroughly evaluate an external innovation opportunity can take many months, creating significant delays for scouts (Castaner et al. 2023, Popli et al. 2025). At the same time, innovation scouting often occurs in *low success probability* environments, where the likelihood of discovering promising projects is slim and most evaluated opportunities are ultimately deemed insufficiently attractive to justify investment (Drakeman et al. 2022). To use their time efficiently and increase the probability of identifying at least a few viable opportunities, scouts must therefore search for—and evaluate—many projects *in parallel*, rendering innovation scouting a complex and resource-intensive search process.[1]

These intertwined challenges make innovation scouting difficult to organize, and existing research offers little guidance. Work on innovation search has explored how firms should configure parallel search activities (see, e.g., Dahan and Mendelson 2001, Loch et al. 2001, Sommer and Loch 2004, Schlapp and Schumacher 2022), but without considering multi-project search targets in low success probability environments or accounting for the behavioral forces that shape delegated scouting. And whereas a well-established agency literature in economics offers solutions for delegation problems, from formal incentive design to reciprocity-based arrangements, it is apriori unclear if these remedies translate to an operationally rich parallel search environment, leaving open a question that is especially pointed when success is rare: How can managers incentivize scouts to search extensively, knowing they will most likely find little or nothing? This study addresses that question theoretically and experimentally.

We develop a behavioral model that captures the key trade-offs inherent in delegated innovation scouting. In our model, a manager seeks to acquire external innovation projects and delegates the costly tasks of searching for these projects and evaluating their uncertain prospects to a scout. The manager's central challenge is to design incentive schemes that motivate the scout to search for a sufficient number of projects, while anticipating that many of the evaluated opportunities will ultimately prove unworthy of investment.

[1] In practice, parallel scouting efforts are essential for maintaining a healthy innovation pipeline. Pursuing projects sequentially would quickly stagnate innovation output. For instance, if each due diligence process takes six months and only 10% of projects are ultimately deemed promising, a single scout would, on average, identify one viable project every five years. Such a pace would be incompatible with the innovation needs of most firms.

We show that a *rational*, *self-interested* manager can, in principle, design incentive schemes that induce first-best scouting behavior. Such optimal contracts generally provide little compensation for poor search outcomes but reward rare, highly successful outcomes generously. While these contracts induce efficient search, they are structurally complex and patently unfair, as they leave the scout with none of the value created. By contrast, our model predicts that a *fairness-minded* manager is more likely to offer piecewise-linear incentive schemes—contracts that are simpler and appear fairer, but that undermine the scout's incentives to search. Yet, although a *truly fair* contract would at least partially compensate the scout for search costs, our scouting environment gives managers several reasons to underweight these costs. As a result, managers may offer *superficially fair* contracts that divide the value created while paying little regard to the scout's search costs. Such contracts are particularly detrimental to scouting performance in environments with *low* probabilities of success, where providing adequate search incentives requires rewarding rare successes especially generously.

To test our key theoretical predictions, we conduct a series of controlled experiments that systematically vary the scout's likelihood of search success, the contractual structure governing the manager-scout relationship, and the institutional conditions that foster reciprocal behavior. We find substantial evidence that fairness and reciprocity concerns shape the manager-scout relationship, most notably in the observation that discretionary incentives perform comparably to formal contractual incentives. At first glance, this finding is consistent with the well-documented view that reciprocity can generate efficient and equitable outcomes in principal-agent settings (Fehr et al. 1997). However, the contracts managers actually implement are neither efficient nor fair. In fact, in low success probability environments, the observed contracts are so poorly structured that they provide only minimal incentives for scouts to search.

Our results indicate that the marked trade-off between contractual efficiency and fairness is difficult to overcome. Scouting efficiency does not improve when managers can communicate their search expectations, observe scouts' search efforts, offer upfront payments independent of search outcomes, or build relational incentives through repeated interaction. Although each of these interventions strengthens effort-based reciprocity to some extent, none is sufficient to offset the inadequate outcome-based search incentives we observe.

Because the superficially fair contracts in our data erode scouts' search incentives—especially when success is elusive—we ultimately return to the standard-theory benchmark to examine what firms can achieve. When subjected to the optimal nonlinear incentives that managers in our experiments never offer, scouts search at first-best levels. This finding suggests that firms can restore scouting efficiency by combining limited tolerance for scouting failures with generous rewards for exceptional discoveries. These bonus structures also produce a fairer division of profits. Realizing them, however, requires firms to channel managers' fairness concerns through thoughtful organizational and process design. Left to shape incentive schemes directly, those same concerns instead give rise to piecewise-linear contracts that appear fair but provide little incentive to search.

2. Related Literature

We build on and bridge two primary strands of research. The first examines innovation as a process of search; the second explores behavioral agency problems in delegation and contract design.

The innovation literature often conceptualizes innovation as a process of search, and scholars have developed a range of normative models to analyze optimal search behavior (e.g., Weitzman 1979, Levinthal 1997, Loch et al. 2001, Erat and Krishnan 2012). More recently, increasing attention has been paid to how *behavioral* biases influence innovation search. Several behavioral studies examine *sequential* search problems, centered on identifying a single "best" alternative, and investigate factors that shape whether individual searchers pursue more exploratory or exploitative strategies. These factors include, among others, the complexity of the search task (Billinger et al. 2014), the availability of performance feedback (Billinger et al. 2021), the structure of performance incentives (Ederer and Manso 2013), and access to collaboration opportunities (Sommer et al. 2020).

A related line of inquiry considers *parallel* search problems, in which *multiple searchers* simultaneously seek solutions to a given task (cf. Kornish and Ulrich 2011). The extensive experimental literature on innovation contests has shown how competition among searchers affects their search behavior (for a comprehensive review, see Dechenaux et al. 2015). In contrast, and reflecting the practical realities of innovation scouting, we focus on the search problem faced by a *single scout* who must identify *multiple* promising projects and conduct these searches *in parallel*. Moreover, because scouting tasks are typically *delegated*, we study how scouting performance emerges endogenously from the interaction between a (human) manager and a (human) scout.

Delegation, however, introduces agency problems. A large theoretical literature in economics demonstrates that principals often cannot design efficient incentives when agents' efforts are imperfectly observable or when contracts are only partially enforceable (for comprehensive reviews, see Gibbons and Roberts 2013, Aghion et al. 2014). A subsequent, influential behavioral stream has shown that reciprocity concerns can partially mitigate such inefficiencies, with *discretionary* trust-based contracts sometimes outperforming formal *incentive* schemes (Fehr et al. 1997, 2007). We contribute to the emerging experimental literature that examines the effectiveness of formal and informal contracts when agents' efforts are subject to random shocks and imperfectly transmitted to principals—two defining features of delegated innovation scouting.

For example, Rubin and Sheremeta (2016) and Davis et al. (2017) study informal contracts in classic gift-exchange settings and show that effort distortions weaken the power of effort-based reciprocity to resolve incentive problems. Corgnet and Hernán-González (2018) study formal incentive contracts in the linear-exponential-normal principal-agent model and find empirical support for theoretical predictions that effort distortions lead principals to offer higher fixed wages and lower variable performance pay. Surprisingly, despite weaker incentives in noisier environments, agents exert higher effort levels. Several of our results align with these prior findings: our data reveal strong reciprocity concerns among both managers and scouts, with evidence of effort-based reciprocity in discretionary contracts (as in Rubin and Sheremeta 2016) and outcome-based performance pay in incentive contracts (as in Corgnet and Hernán-González 2018). Other key results, however, diverge—a difference we attribute to the distinctive operational characteristics of delegated innovation scouting.

We show that scouting environments create an unfavorable trade-off between contractual efficiency and fairness. Because a scout searches in parallel for multiple projects, efficient incentives must be highly non-linear in realized outcomes,[2] whereas fair incentives are piecewise linear and too flat to motivate adequate search. Consistent with this tension, we find substantial performance losses under both contract types, and performance deteriorates further precisely when reciprocity should help. Further insights stem from our focus on low success probabilities that characterize innovation scouting in practice. We find that fixed payments remain largely unaffected by success probabilities, whereas performance incentives are too generous when success chances are high but too weak when they are low. Consequently, scouting performance suffers most when success eludes scouts. These results are not easily explained (let alone specifically predicted) by existing insights from the literature, since prior delegation studies generally model outcome stochasticity at the level of single efforts rather than compounded outcomes. In contrast, our setting involves multiple search efforts with asymmetric stochasticity, as scouting outcomes can never exceed search efforts.[3]

Finally, beyond these departures from the experimental delegation literature regarding contractual efficiency and fairness, the operational specifics of our scouting environment intertwine agency problems with judgment and decision-making biases—similar to the richer behavioral dynamics observed in buyer-supplier interactions (for an excellent overview, see Chen and Wu 2019). A recurrent finding in that stream of work is that simple contracts often perform better than predicted, partly because theoretically superior contracts are complex and thus difficult to implement (Kalkanci et al. 2011, 2014). Another consistent theme is reciprocal behavior, with many studies showing that fairness concerns can produce more efficient and equitable outcomes than theory would predict

[2] By contrast, optimal incentives are typically linear when search is sequential or when parallel search targets only a single project (Schlapp and Schumacher 2022).

[3] Whereas Rubin and Sheremeta (2016) and Corgnet and Hernán-González (2018) manipulate stochasticity dichotomously (noise vs. no noise), our design varies stochasticity between high and low levels.

(e.g., Cui et al. 2007, Katok et al. 2014, Wu and Niederhoff 2014). These insights, however, do not readily extend to delegated innovation scouting, which poses a unique tension between contractual efficiency and fairness: fairness concerns drive simple contracts, yet simple contracts undermine search incentives (cf. Davis and Leider 2018, Davis and Hyndman 2018). A key contribution of our study is to predict—and empirically confirm— that this tension is most severe when the likelihood of scouting success is low.

3. A Behavioral Model of Delegated Innovation Scouting

We develop a (behavioral) model of delegated innovation scouting to derive precise predictions about how incentive structures interact with behavioral preferences to shape scouting decisions. We first formalize the model setup and characterize a manager's first-best scouting strategy, identifying the incentive schemes that induce a rational scout to implement this strategy. We then examine how fairness concerns lead the manager to deviate markedly from these optimal incentives, thereby misaligning the scout's search behavior and causing inefficient scouting outcomes.

3.1. Model Setup

Consider a firm, represented by a manager, that seeks to acquire $n \in \mathbb{N}$ external innovation projects. Before deciding which projects to acquire (if any), the manager must search for potential opportunities and evaluate their uncertain value. In practice, these activities—collectively referred to as *innovation scouting*—are rarely performed by the manager directly. Instead, the scouting task is typically delegated to an *innovation scout*.

The scout's primary objective is to discover up to n high-value projects on the manager's behalf by conducting *parallel* project search and evaluation. At the start of the scouting process, the scout chooses the number of projects $y \in \mathbb{N}_0$ to evaluate; we denote the scouting set by $\mathbb{Y} = \{0, \dots, y\}$. The scout then evaluates, in parallel, all projects $i \in \mathbb{Y} \setminus \{0\}$ and incurs a private cost $c > 0$ for each evaluated project. Project evaluation yields a private and perfectly informative signal $s_i \in \{v, 0\}$ about the true value $V_i \in \{v, 0\}$ of project $i \in \mathbb{Y} \setminus \{0\}$, so that $\mathbb{P}(V_i = v \mid s_i = v) = 1$ and $\mathbb{P}(V_i = v \mid s_i = 0) = 0$ under Bayesian rationality. We assume $v > 0$ and that the manager and scout share a common prior belief $q \in (0,1)$ about project i having high value (i.e., $\mathbb{P}(V_i = v) = q$). After evaluating all projects $i \in \mathbb{Y} \setminus \{0\}$, the scout knows the set $\mathbb{Y}_v \subseteq \mathbb{Y} \cup \emptyset$ of high-value projects. For brevity, let $k = |\mathbb{Y}_v|$ denote the number of high-value projects discovered. In the final step, the scout credibly transmits $\mathbb{Y}_v$ to the manager.

Because the manager can acquire *at most* n projects (e.g., due to resource constraints), the manager obtains value v only for the first n high-value projects. That is, receiving k high-value projects yields total value $v_{k;n} = v \min\{k, n\}$. We assume that the manager can verify the number k of high-value projects transmitted, but not the scouting set $\mathbb{Y}$ itself. Consequently, the manager cannot directly order a specific search effort but must instead design incentives for search. The manager does so by offering an incentive scheme $\beta = (\beta_k)_{k\in\mathbb{N}_0}$, where β_k is the bonus paid to the scout for identifying k high-value projects.

3.2. Rational Innovation Scouting

To ground our work in standard economic theory, we first analyze a benchmark in which both manager and scout are rational, self-interested, and risk-neutral decision makers. That is, the scout maximizes expected utility u, which is defined as expected bonus payments net of search costs. The scout is further protected by limited liability (i.e., $\beta \geq 0$) and has a normalized outside option of zero. The manager maximizes expected profit π, defined as the expected value from acquired projects minus expected bonus payments to the scout. Anticipating the scout's optimal search strategy, the manager chooses β^* to solve:

$$\max_{\beta\geq 0}\ \pi(\beta) = \sum_{k=0}^{y^*} \big(v_{k;n} - \beta_k\big)\mathbb{P}\big(X(y^*;q)=k\big) \tag{1}$$

$$\text{s.t. } y^* \in \arg\max_{y\in\mathbb{N}_0} u(y) = \sum_{k=0}^{y} \beta_k \mathbb{P}\big(X(y;q)=k\big) - cy \tag{2}$$

$$u(y^*) \geq 0. \tag{3}$$

Here, y^* signifies the scout's optimal search strategy which, for an optimal incentive scheme β^*, simultaneously satisfies the scout's incentive compatibility and individual rationality conditions (2)–(3). We let $X(y;q)$ denote a Binomial random variable with y independent Bernoulli trials and success probability q, and we set, for any $q\in(0,1)$, $X(0;q)=0$ almost surely. To align our theoretical analyses with our experimental setup, we let $n=2$ throughout the analysis—this assumption facilitates clearer exposition without affecting the structural validity of our theoretical results.

3.2.1. Optimal Scouting. We begin our analysis by characterizing the manager's first-best search strategy, $y^{\text{fb}} \in \arg\max_{y\in\mathbb{N}_0} \pi^{\text{fb}}(y) = \sum_{k=0}^{y} v_{k;2}\mathbb{P}\big(X(y;q)=k\big) - cy$, in the absence of delegation issues. This analysis serves not only as a theoretical benchmark but, as we show in Section 3.2.2, the manager can induce the scout to implement first-best search through an appropriately designed incentive scheme. All proofs are relegated to Appendix B.

PROPOSITION 1. *Under first-best conditions, the following statements hold.*

(i) If $q < c/v$, then $y^{\text{fb}} = 0$ and $\pi^{\text{fb}} = 0$.

(ii) If $q \geq c/v$, the optimal search $y^{\text{fb}} \geq 2$ is unique and satisfies the following condition:

$$(1-q)^{y^{\text{fb}}-1}(1+q(y^{\text{fb}}-1)) < \frac{c}{qv} \leq (1-q)^{y^{\text{fb}}-2}(1+q(y^{\text{fb}}-2)). \tag{4}$$

Furthermore, y^{fb} and π^{fb} increase with v and decrease with c. Moreover, π^{fb} increases with q, and there exist thresholds $\underline{q}$ and $\overline{q}$, with $c/v < \underline{q} \leq \overline{q} < 1$, such that (a) y^{fb} increases with q if $q < \underline{q}$ and decreases with q if $q > \overline{q}$, and (b) $\mathbb{E}[X(y^{\text{fb}}(q);q)]$ increases with q if $q < \underline{q}$.

Under first-best conditions, the manager refrains from scouting when projects are insufficiently promising (i.e., $q < c/v$) but optimally evaluates at least two projects (i.e., $y^{\text{fb}} \geq 2$) otherwise. As expected, y^{fb} increases with project value v and decreases with evaluation cost c. More intricate, however, is the effect of the project success probability q—the main parameter of interest in our analysis. Although the manager's expected profit increases monotonically with q, part (a) of Proposition 1(ii) shows that y^{fb} exhibits a non-monotonic relationship with q. When success probabilities are low (i.e., $q < \underline{q}$), discovering high-value projects is very costly, and the manager hence cannot afford to evaluate many projects; in this case, higher q increases the expected returns from search, thereby prompting more scouting. In contrast, when success probabilities are high (i.e., $q > \overline{q}$), the manager reduces the number of projects evaluated as q rises, since each project is already likely to be valuable and at most $n = 2$ projects can be acquired.

3.2.2. Optimal Incentives. We next characterize the optimal incentive scheme β^* that aligns the scout's interests with the manager's objectives in delegated scouting processes. This scheme constitutes the solution to the manager's full incentive design problem (1)–(3).

PROPOSITION 2. *Under delegation, the following statements hold.*

(i) Any optimal incentive scheme β^* *satisfies* $\beta^*_0 = 0$ *and* $0 \leq \beta^*_k \leq \beta^*_{k+1}$ *for all* $k \in \mathbb{N}_0$*; the scout evaluates* $y^* = y^{\text{fb}}$ *projects, the manager's expected profit equals* $\pi^* = \pi^{\text{fb}}$*, and the scout's expected utility equals* $u^* = 0$*.*

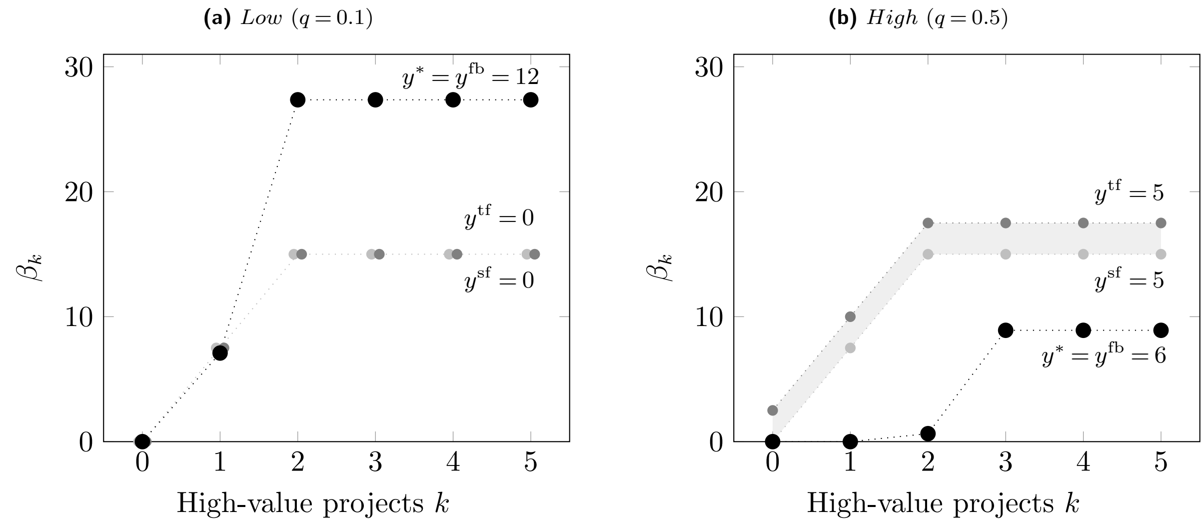
(ii) If $q < c/v$*, then* $\beta^* = 0$*.*

(iii) If $q \geq c/v$*, then* β^* *satisfies the following additional properties: (a)* $\beta^*_1 \leq c/q$*; (b)* $\beta^*_k \geq cy^*/(1-(1-q)^{y^*}) > c/q \ \forall\ k \geq \bar{k}$*, with* $2 \leq \bar{k} \leq y^*$*, if* $q > c/v$*; and (c)* $\beta^*_k = v_{k;2} \ \forall\ k \in \mathbb{N}_0$ *if* $q = c/v$*.*

Proposition 2 yields two key conceptual insights. First, the manager can always design an incentive scheme β^* that induces the scout to engage in first-best search (i.e., $y^* = y^{\text{fb}}$) while avoiding any rent extraction in expectation (i.e., $\pi^* = \pi^{\text{fb}}$ and $u^* = 0$). Hence, in theory, an optimally designed delegated scouting process entails no loss of scouting efficiency but leads to an extreme distribution of profits between manager and scout.

Second, to induce first-best search, optimal incentive schemes must possess a sophisticated structure. On the one hand, they must provide but small rewards that do not compensate for the scout's search costs whenever only few successful projects are identified, which also entails not paying the scout any bonus when scouting fails entirely (i.e., $\beta_0 = 0$). On the other hand, the manager must offer very generous bonus payments β^*_k for highly successful outcomes $k \geq \bar{k}$, and these bonuses must increase quickly when the success probability q decreases (see part (b) of Proposition 2(iii)). Put differently, optimal incentive schemes employ an intricate lottery structure to motivate the scout:

Figure 1 Contract Anatomy (Optimal ●, Superficially Fair ●, Truly Fair ●)



they offer only diminutive bonuses for easy-to-achieve outcomes while promising huge bonuses for very unlikely events. It follows that β^* is, in general, highly non-linear in k.[4]

Figure 1 (●) illustrates the pronounced non-linearity of the optimal incentive scheme β^* for two representative environments later used in our experimental studies. Specifically, we set $v = 15$ and $c = 1$, and vary the probability of identifying high-value projects between a *Low* ($q = 0.1$) and *High* ($q = 0.5$) condition. In *High*, the manager offers no bonus for discovering $k \leq 1$ high-value projects, and even for $k = 2$, the scout receives only a meager $\beta_2^* = 0.64$. Instead, substantial rewards $\beta_k^* = 8.92$ are paid only when $k \geq 3$ high-value projects are identified. In contrast, while also offering no rewards for unsuccessful search ($\beta_0^* = 0$, cf. Proposition 2(i)), the manager in the *Low* condition provides much steeper incentives: $\beta_1^* = 7.09$ for discovering one high-value project and $\beta_2^* = 27.36$ for $k \geq 2$. Comparing the two conditions highlights that incentive schemes must be considerably more generous in low success probability environments (cf. part (b) of Proposition 2(iii)).

3.3. How Fairness Concerns Distort Innovation Scouting

We have characterized the contracting and scouting decisions of rational and self-interested decision makers. Optimal incentive schemes β^* are structurally complex with pronounced non-linearities and fundamentally unfair, as they allow the manager to retain the entire profit (i.e., $\pi^* = \pi^{\text{fb}}$ and $u^* = 0$). It is thus unlikely that these incentives would naturally emerge in interactions between fairness-minded and boundedly rational (i.e., human) managers and scouts (Bolton and Chen 2018).

To formally analyze how fairness concerns affect incentive design and scouting decisions, we extend our theoretical model to incorporate behavioral preferences for fairness, and we distinguish

[4] The need for non-linear incentive schemes is a direct consequence of the parallel search regime. If the manager could instead mandate a *sequential* search process, then simply paying a constant bonus $\beta = c/q$ for each of the first two high-value projects would be optimal.

between two types of fairness concerns: *true fairness* and *superficial fairness*. A truly fair manager strives to share realized *profits* equally with the scout. In contrast, a superficially fair manager may seek only a fair division of *value created*, thereby neglecting the scout's (unobservable) search costs and inducing an unfair distribution of profits (Davis and Leider 2018).

To model these behavioral preferences, we assume that the manager experiences a disutility $\alpha \geq 0$ per squared deviation from an equal split of created value $v_{k;2}$ (superficial fairness) or of total profits (true fairness).[5] To distinguish between the two different fairness regimes, we introduce a parameter $\delta \in \{0,1\}$: $\delta = 0$ represents superficial fairness, and $\delta = 1$ signifies true fairness. The fairness-minded manager's incentive design problem can then be expressed as:

$$\max_{\beta \geq 0} \ \pi^{\mathrm{f}}(\beta; \alpha, \delta) = \sum_{k=0}^{y^{\mathrm{f}}} \big(v_{k;2} - \beta_k - \alpha(2\beta_k - v_{k;2} - \delta c y^{\mathrm{f}})^2\big)\mathbb{P}\big(X(y^{\mathrm{f}}; q) = k\big) \tag{5}$$

$$\text{s.t. } y^{\mathrm{f}} \in \arg\max_{y \in \mathbb{N}_0} u(y) = \sum_{k=0}^{y} \beta_k \mathbb{P}\big(X(y;q) = k\big) - cy \tag{6}$$

$$u(y^{\mathrm{f}}) \geq 0. \tag{7}$$

3.3.1. Optimal Scouting under Fairness Concerns. Proposition 3 and Figure 1 illustrate how true fairness (i.e., $\delta = 1$; ●) and superficial fairness (i.e., $\delta = 0$; ●) influence incentive structures, scouting decisions, and overall efficiency in delegated innovation scouting.

PROPOSITION 3. *(i) For* $\delta = 1$, $\lim_{\alpha \to \infty} \beta_k^{\mathrm{tf}} = v_{k;2}/2 + c y^{\mathrm{tf}}/2$ *for all* $k \in \mathbb{N}_0$, $\lim_{\alpha \to \infty} y^{\mathrm{tf}} \leq y^{\mathrm{fb}}$, *and* $\pi^{\mathrm{tf}} + u^{\mathrm{tf}} \leq \pi^{\mathrm{fb}}$.

(ii) For $\delta = 0$, $\lim_{\alpha \to \infty} \beta_k^{\mathrm{sf}} = v_{k;2}/2$ *for all* $k \in \mathbb{N}_0$, $\lim_{\alpha \to \infty} y^{\mathrm{sf}} \leq y^{\mathrm{fb}}$, *and* $\pi^{\mathrm{sf}} + u^{\mathrm{sf}} \leq \pi^{\mathrm{fb}}$.

(iii) If $q < 2c/v$, *then* $\lim_{\alpha \to \infty} y^{\mathrm{tf}} = \lim_{\alpha \to \infty} y^{\mathrm{sf}} = 0$. *Otherwise,* $\lim_{\alpha \to \infty} y^{\mathrm{tf}}, \lim_{\alpha \to \infty} y^{\mathrm{sf}} > 0$.

The key finding of Proposition 3 is that instead of the structurally complex (and unfair) optimal incentive scheme β^*, a fairness-minded manager offers simple, piecewise-linear incentive schemes to motivate the scout. In particular, a *truly* fair manager shares, irrespective of scouting outcomes, half of the created value $v_{k;2}$ with the scout and also reimburses the scout for half of the incurred search costs. As a result, both parties ultimately receive an equal share of total profits (i.e., $\pi^{\mathrm{tf}} = u^{\mathrm{tf}}$). In contrast, a *superficially* fair manager offers only an equal split of created value to the scout, thereby neglecting the scout's search costs and causing an unequal distribution of profits (i.e., $\pi^{\mathrm{sf}} \geq u^{\mathrm{sf}}$).

A direct consequence of offering simple, piecewise-linear incentive schemes is that the scout searches at an inefficient level (i.e., $y^{\mathrm{tf}}, y^{\mathrm{sf}} \leq y^{\mathrm{fb}}$), which naturally leads to a decline in total profits. That is, a fairness-minded manager is confronted with a pronounced trade-off between fairness and

[5] The assumptions of a self-interested scout and an equal split are for expositional clarity only; all results generalize trivially to arbitrary splits, and Appendix A.4 discusses robustness to scout fairness concerns.

scouting efficiency. Figure 1 illustrates why fair incentive schemes fail to elicit optimal scouting behavior. In *High* success probability environments, generous bonuses for weak outcomes allow the scout to earn considerable rewards with limited (and thus suboptimal) search effort. By contrast, in *Low* success probability environments that mandate steep bonus payments for unlikely search outcomes (i.e., $k \geq 2$), fair incentives are underpowered, leaving the scout with little or no incentive to search at all (Proposition 3 (iii)). Hence, the negative impact of fairness concerns on search effort is most severe in *Low* success probability environments.

3.3.2. Emergence of Discretionary Bonuses. Fairness concerns can also rationalize the emergence of another class of incentives frequently observed in practice: *discretionary bonuses* which the manager determines only *after* observing the outcomes of the scouting process and for which the scout has no contractual right to claim the rewards.

Proposition 4. *Suppose that after observing the number of high-value projects $k \in \mathbb{N}_0$, the manager decides ex-post about the scout's bonus b_k^{xp}. Then:*

(*i*) *For all $k \in \mathbb{N}_0$, $b_k^{\mathrm{xp}} = [(v_{k;2} + \delta c y^{xp})/2 - 1/(8\alpha)]^+$.*

(*ii*) *For $\delta = 1$, $\lim_{\alpha\to\infty} y^{\mathrm{xp}} = \lim_{\alpha\to\infty} y^{\mathrm{tf}} \leq y^{\mathrm{fb}}$, and for $\delta = 0$, $\lim_{\alpha\to\infty} y^{\mathrm{xp}} = \lim_{\alpha\to\infty} y^{\mathrm{sf}} \leq y^{\mathrm{fb}}$.*

Proposition 4 embeds the classic game-theoretic prediction that discretionary bonuses have no motivational power when fairness concerns are weak or incidental. Specifically, for sufficiently small α, the manager appropriates the entire value created by the scout (i.e., $b_k^{\mathrm{xp}} = 0$), and the scout, anticipating this, exerts no search effort. However, when fairness concerns are sufficiently strong (i.e., large α), discretionary bonuses become more effective: the manager voluntarily awards a positive bonus $b_k^{\mathrm{xp}} > 0$ conditional on scouting success k, which can, in turn, induce the scout to evaluate $y^{\mathrm{xp}} > 0$ projects. As fairness concerns intensify—whether *true* or *superficial*—discretionary bonuses are on par with the simple, piecewise-linear incentive schemes presented in Proposition 3. Put differently, reciprocity can (partially) substitute for formal contract enforcement, but it cannot overcome the pronounced efficiency-fairness trade-off inherent in delegated innovation scouting.

4. Overview of Experimental Studies

Having derived theoretical insights into how to manage delegated innovation scouting, we proceed to translate these results into empirically testable hypotheses. We then explain how we test these hypotheses in a series of controlled laboratory experiments.

4.1. Hypotheses

Our theoretical analysis in Section 3 reveals that delegated innovation scouting poses difficult trade-offs between scouting efficiency and fairness. Standard economic theory predicts that managers can induce first-best search efforts under *Incentive* contracts (i.e., $y^{\mathrm{in}} = y^{\mathrm{fb}}$), whereas purely

Discretionary bonus schemes lack any scouting incentives (i.e., $y^{\text{min}} = y^{\text{di}} < y^{\text{in}}$).[6] For obvious reasons, the underlying assumption of rational and self-interested decision makers is unrealistic, and we instead expect fairness concerns and reciprocity to govern the relationship between managers and scouts. That is, we predict that scouts might search even under discretionary schemes because they trust that managers will reciprocate with a bonus payment and, similarly, managers may trust that scouts searched even when they do not deliver any successful project. However, while fairness concerns and reciprocity may allow discretionary schemes to induce search efforts (i.e., $y^{\text{min}} \leq y^{\text{di}}$), and possibly as much as formal incentives (i.e., $y^{\text{di}} = y^{\text{in}}$), we posit that they erode search incentives and lead to inefficient scouting (i.e., $y < y^{\text{fb}}$).

Hypothesis 1 (Contracts and Search). $y^{\text{min}} \leq y^{\text{di}} = y^{\text{in}} < y^{\text{fb}}$.[7]

An important implication of our theoretical analysis is that fairness concerns and reciprocity create a fundamental tension in parallel search environments. Whereas basic reciprocity principles suggest that scouts might search even in one-shot relationships governed by discretionary rewards (i.e., $y^{\text{min}} < y^{\text{di}}$), scouts will find search to be worthwhile *only if* they trust that they will receive a bonus that justifies their efforts. However, our theoretical results indicate that fair bonus payments might erode precisely those incentives to search (i.e., $y^{\text{min}} = y^{\text{di}} = y^{\text{in}}$), because they fundamentally deviate from the structural properties of optimal contracts that induce first-best search under rationality and self-interest (Proposition 2). In the parallel search environment we study, such optimal contracts are highly non-linear: they offer but small bonus payments for rather unsuccessful search outcomes, including paying nothing when scouting fails entirely (i.e., $k = 0$); yet, they offer very generous bonus payments for successful scouting outcomes.

Fair contracts, in contrast, are piecewise linear and thereby mute search incentives—to a degree that depends on whether the manager's fairness concerns account for the scout's search costs, and on the success probability q (Proposition 3 and 4). Among the many contracts that can be deemed fair (e.g., those displayed in Figure 1), our analysis highlights two archetypal endpoints. On the one hand, *truly fair* contracts are predicated on a fair split of realized profits, which implies that the manager does reciprocate on the scout's unobservable search cost. Such contracts are compatible with the manager sharing more than half of the realized value $v_{k;2}$ (and even paying a positive bonus at $k = 0$) to (partially) cover the scout's search cost cy.

[6] We denote y^{min} the minimum search effort in our experiments. In Studies 3 and 5, $y^{\text{min}} = 0$, whereas in Studies 1, 2 and 4, we set $y^{\text{min}} = 1$ (consistent with, e.g., Fehr et al. 1997, 2007; please also see Appendix D.1.1).

[7] Note that although we cannot (credibly) predict the strength of fairness concerns (captured by α in our theoretical model), and the extent to which they are "superficial"(captured by δ), we state our behavioral hypotheses *in the limit* ($\alpha \to \infty$), as this allows for sharp and testable predictions.

On the other hand, managers might base their bonus payments largely on a fair split of the created value $v_{k,2}$, with inordinate attention to reimbursing the scout's search cost. Such *superficially fair* contracts, which are our main behavioral prediction, may emerge for different reasons. For instance, managers might underweight the scout's unobservable search cost because successful search outcomes are only a fraction of actual search effort, especially when the success probability q is low. They might also simply consider it unfair (to themselves) to share more than half of the created value, regardless of their belief about the scout's search efforts. Whatever the underlying mechanisms (which we explore further in our studies), superficially fair contracts sacrifice key structural properties of optimal incentive schemes.

Crucially, the effect of fair contracts on scouting efficiency depends on the success probability q. In *High* success probability environments, where it is relatively inexpensive for the scout to discover successful projects, fair contracts can still produce meaningful search incentives. In *Low* probability environments, however, they completely erode search incentives as they forgo offering substantial bonus payments for exceptional but unlikely search outcomes (Proposition 3 and 4).

HYPOTHESIS 2 (SUCCESS PROBABILITY). (a) For *High* success probabilities (i.e., $q \geq 2c/v$), $y^{\text{in}} = y^{\text{di}} > y^{\min}$; (b) for *Low* success probabilities (i.e., $q < 2c/v$), $y^{\text{in}} = y^{\text{di}} = y^{\min}$.

4.2. Roadmap to Experimental Studies

We present a series of controlled laboratory experiments designed to test our theoretical predictions. Our studies implement the task environment described in Section 3.1 in which a manager designs incentives and delegates to a scout the parallel search for promising innovation projects. In all treatments, scouts privately choose the number of projects to evaluate, $y \in \{y^{\min}, \ldots, y^{\max}\}$, with $y^{\max} \in \{20, 40\}$ (see Appendix D.1), and incur a cost of $c = 1$ for each project (unobservable to managers). Nature then determines the success of each project independently, and the manager receives a value of $v = 15$ for each high-value project delivered by the scout, for up to $n = 2$ projects, and compensates the scout. Towards our goal of understanding low success probability environments, our studies vary the prior belief about project success in a *Low* ($q = 0.1$) and a *High* ($q = 0.5$) condition. To study the underlying behavioral mechanisms and boundary conditions of the scouting performance that we predict and observe in this environment, we implement several treatments summarized in Table 1.

Study 1, as a behavioral benchmark, implements a *single* decision maker treatment to assess a possible efficiency loss caused by individual decision biases in the absence of delegation; yet, we observe that scouting performance approaches first-best levels (Proposition 1). Studies 2–4 implement the delegated setting, and systematically vary the contractual structures and communication protocols that govern the relationship between managers and scouts. The treatments manipulate

Table 1 Overview of Experimental Studies

			# Subjects		Contract Type		Search Effort		
Study	Label	Rounds	*Low*	*High*	Ex-ante	Ex-post	Observable	Recommend	Belief
1	*Single*	20	30	30	—	—	—	—	—
2	*Incentive*	20	60	60	Incentive	—	—	—	—
	Discretionary	20	58	58	—	Discretionary	—	—	—
3	*Observable*	30	48	48	—	Discretionary	Y	Y	—
	Unobservable	30	48	48	—	Discretionary	—	Y	Y
4	*Upfront*	20	64	66	Upfront	Discretionary	—	Y	—
	Repeated	20	66	60	Upfront	Discretionary	—	Y	—
5	*Automated*	30	35	—	Optimal	—	—	—	—
	Incentive2	30	60	—	Incentive	—	—	—	Y

the antecedents for reciprocal behavior, ranging from conditions that favor the *superficial* fairness predictions to conditions that align with the *true* fairness predictions (Propositions 3 and 4). Across *all* treatments, we consistently observe that managers provide piecewise-linear incentives, scouts exert insufficient search effort, and overall scouting efficiency *and* fairness decline markedly when success is elusive. Finally, Study 5 shows that scouts achieve first-best search under optimal incentive schemes β^* (Proposition 2), identifying superficially fair contracts as the central source of inefficiency in Studies 2–4 and illustrating the contract structure firms must implement to incentivize efficient search.

4.3. Implementation

We recruited participants from a subject pool associated with an experimental laboratory at a large public university, and each of a total of 839 subjects participated in one treatment only. The experiments were implemented in oTree (Chen et al. 2016). In each session, after arriving at the laboratory, participants were randomly assigned to isolated cubicles, and then read instructions which were presented on-screen in an easy to navigate format (see Appendix C).

After reading the instructions, participants in all treatments with delegation were randomly assigned to a role (manager or scout) that they kept for the duration of the experiment. At the beginning of each round, each manager was randomly matched with one scout, except in the *Repeated* treatment of Study 4, which implemented partner matching to study relational incentives. Full details on session sizes, payment procedures, and pre-registration documents are provided in Appendix D.1.

4.4. Data and Analysis

Here we briefly summarize our data and analyses; further details are provided in Appendix D.2.2.

4.4.1. Data. Across all studies and at the most granular level, in each round $t \in \{1, \ldots, T\}$ and for each pair $i \in \{1, \ldots, I\}$, we observe the manager's contract decision $\beta_{it} = (\beta_{k,it})_{k\in\{0,\ldots,5\}}$ (only under *Incentive* contracts),[8] the scout's search decision y_{it} and the resulting number of high-value projects k_{it} (which is a random draw from $X(y_{it}; q)$), and the realized bonus payment b_{it}. The realized scout utility u_{it} and manager profit π_{it} depend on y_{it}, k_{it}, and b_{it}, whereas the realized total profit $\Pi_{it} \equiv \pi_{it} + u_{it}$ depends only on y_{it} and k_{it}, but not on b_{it} (or β_{it}).

Pairs are grouped into sessions $s \in \{1, \ldots, S\}$, each implementing a single treatment. We construct session-round averages $\bar{y}_{st} = \sum_{i\in s} y_{it}/I_s$ for search (and analogously $\bar{u}_{st}$, $\bar{\pi}_{st}$, $\bar{b}_{st}$, and $\bar{\Pi}_{st}$), where I_s is the number of pairs in session s. We denote session-level averages as $\bar{y}_s = \frac{1}{T}\sum_t \bar{y}_{st}$ and treatment-level averages of session means as $\bar{y} = \frac{1}{S}\sum_s \bar{y}_s$.

4.4.2. Analysis: Aggregate Outcomes. To analyze the effect of key factors on our variables of interest (e.g., search or profit), we estimate the following regression model:

$$\bar{x}_{st} = \alpha_0 + \alpha_1 D_{1,s} + \alpha_2 D_{2,s} + \alpha_3 (D_{1,s} \times D_{2,s}) + \alpha_4 t_c + \varepsilon_{st}, \tag{8}$$

where $\bar{x}_{st}$ denotes the variable of interest; $D_{1,s}$ and $D_{2,s}$ are time-invariant treatment indicator variables (e.g., *Low* vs. *High* success probability); t_c is the mean-centered round number; and ε_{st} is an error term. Standard errors are clustered at the session level.

4.4.3. Analysis: Bonus Efficiency. To assess the incentive effect of observed bonus schedules at an aggregate level, we derive the counterfactual best-response search effort to the contracts scouts receive (*Incentive*) or anticipate (*Discretionary*), and compare them to the optimal benchmark presented in Proposition 2. For each observed contract β_{it} in *Incentive*, we calculate $y^*(\beta_{it})$ and summarize via the treatment-level average $\bar{y}^*(\beta) = \frac{1}{S}\sum_s \bar{y}^*_s(\beta)$. For *Discretionary*, we use $\bar{b}_k$—the average realized bonus at each outcome level k—as a proxy for scouts' bonus expectations, and compute $\bar{y}^*(\bar{b})$ analogously.

4.4.4. Analysis: Bonus Anatomy. To test whether observed bonus schedules fit the piecewise-linear structure predicted by fairness concerns (Propositions 3 and 4), we estimate:

$$b_{it}(k) = \sum_{k=0}^{5} \alpha_k \mathbf{1}[k_{it} = k] + \varepsilon_{it}(k), \tag{9}$$

where the estimated coefficients $\hat{\alpha}_k$ can be tested against theoretical benchmarks via linear contrasts. To compare bonus schedules across treatments, we also estimate (9) on pooled data, interacting all coefficients with a treatment indicator. We relegate these extended specifications to Appendix D.4.3. Standard errors are clustered at the session level.

[8] For practical reasons, we elicited from managers contract parameters only up to β_5 and set $\beta_k = \beta_5$ for all $k > 5$. This simplification does *not* preclude the design of optimal incentive schemes (cf. Figure 1).

5. Study 1: The No-Delegation Benchmark

Our behavioral theory predicts inefficient scouting outcomes due to undersearch because managers and scouts cannot simultaneously create contract efficiency and mitigate the fairness and reciprocity concerns their interaction invokes. That is, scouting efficiency suffers specifically due to delegation.

However, our scouting environment is also plausibly subject to individual-level biases that could lead to undersearch even absent delegation. One example is loss aversion: scouts incur search costs before receiving any returns, so scouting involves initial losses that may not be recouped. Clearly, such interim losses loom larger in *Low* than in *High* because search costs are higher and search is less likely to produce successful outcomes (see Appendix A.2 for a formal treatment). A second example is probability weighting: managers and scouts may distort the success chances that govern innovation scouting, a distortion that is naturally most consequential in low success probability environments (see Appendix A.3). Individually, either bias could entice scouts to undersearch relative to first-best levels and predict a steeper shortfall in *Low* than in *High*.

5.1. Study Design

To isolate the effect of delegation from individual-level biases, we implement a *Single* treatment in which subjects play the role of a single decision maker who not only searches but also directly keeps the value obtained from the search efforts. In all other aspects, the scouting task is identical to the delegation treatments introduced in Section 4.2. Because profits accrue to only one participant rather than being split between a manager and a scout, we halve the conversion rate to hold financial stakes constant across studies. By construction, *Single* removes any agency and fairness concerns while preserving every other feature of the scouting task; any departure from first-best predictions must hence stem from individual decision or judgment biases rather than from delegation.

5.2. Results

The search efforts of single decision makers are close to first-best levels in both *Low* (11.80 vs. 12, $p = .81$) and *High* (5.48 vs. 6, $p = .05$), enabling them to capture 66% and 95% of first-best profits, respectively. As Table EC.22 shows, the observed efficiency loss is driven chiefly by variability in search around a near-optimal average rather than by systematic undersearch.

We therefore conclude that, absent delegation, decision makers do not systematically undersearch. Consequently, if the theoretically predicted undersearch emerges in the delegated settings of Studies 2–4, we can attribute the resulting inefficiency with confidence to behavioral factors that arise specifically from delegation, such as fairness and reciprocity concerns.

6. Study 2: Contracts and the Role of Success Probability

For a first test of our main hypotheses, Study 2 varies the success probability q (*Low* vs. *High*) and the contract type (*Incentive* vs. *Discretionary*) in a 2×2 between-subject design. Under an *Incentive* contract, the manager designs an incentive scheme β that specifies, upfront, bonus payments β_k for each number k of high-value projects discovered by the scout. Managers can freely choose the structure of their contracts: no restrictions on monotonicity or linearity are imposed. In contrast, under a *Discretionary* contract, scouts first complete their search, before the manager determines a single bonus payment b based on the observed scouting outcome k.

At the beginning of each round, one manager and one scout are randomly matched. The one-shot and anonymous nature of these interactions eliminates two key antecedents of true fairness: managers cannot communicate desired search efforts, and scouts cannot signal their unobservable effort, thereby leaving payments and realized outcomes as the only channel of interaction. Study 2 thus provides but a conservative baseline for effort-based reciprocity.

6.1. Results

Table 2 presents our aggregate results, which we will study in more detail in the following.

Table 2 Study 2: Treatment Effects

		Search $\bar{y}$	Success $\bar{k}$	Bonus $\bar{b}$	Scout utility $\bar{u}$	Manager profit $\bar{\pi}$	Total profit $\bar{\Pi}$	Efficiency $\bar{\Pi}/\Pi^{\text{fb}}$	Fairness $\bar{u}/\bar{\Pi}$
High	*Incentive*	4.62	2.27	7.58	2.97	14.37	17.34	78%	17%
	Discretionary	4.11	2.01	5.44	1.32	15.60	16.92	76%	8%
	(*p-value*)	(.45)	(.38)	(.28)	(.26)	(.00)	(.80)		
Low	*Incentive*	6.43	0.59	4.36	-2.08	3.76	1.68	43%	< 0
	Discretionary	6.09	0.58	3.42	-2.67	4.41	1.74	45%	< 0
	(*p-value*)	(.34)	(.70)	(.09)	(.10)	(.01)	(.74)		

6.1.1. Scouting Efficiency vs. Fairness.

We begin our analysis in the *High* success probability environment. Under *Incentive* contracts, total profits $\bar{\Pi}$ are 78% of first-best profits (17.34 vs. 22.13, $p < .01$). Similarly, under *Discretionary* contracts, total profits are 76% of first-best profits (16.92 vs. 22.13, $p = .03$). The primary reason for the observed efficiency loss is that scouts under-search relative to the first-best benchmark, in *Incentive* (4.62 vs. 6, $p = .03$) and in *Discretionary* (4.11 vs. 6, $p = .01$), with no difference between the two treatments.

Providing direct evidence for the existence of basic reciprocity motives in our scouting environment, total profits in *Discretionary* are as high as in *Incentive*. That is, scouts do search, and managers do pay bonuses, even when standard theory predicts that scouting should collapse in *Discretionary*. But are profits distributed fairly between managers and scouts? Taking the perspective of the scout who is predicted to earn nothing under standard theory, we observe that scouts in *High* retain 17% of total profits in *Incentive* and 8% in *Discretionary*.

Turning next to the *Low* success probability environment, total profits are 43% of first-best profits under *Incentive* contracts (1.68 vs. 3.88, $p < .01$) and 45% under *Discretionary* contracts (1.74 vs. 3.88, $p < .01$). Again, this efficiency loss is driven by insufficient scouting: scouts search less than optimal in *Incentive* (6.43 vs. 12, $p < .01$) and in *Discretionary* (6.09 vs. 12, $p < .01$), with no difference between treatments. And whereas the observed search decisions and bonus payments in *Discretionary* indicate the presence of fairness motives, the ultimate distribution of profits is highly unfair: manager profits are on par with first-best predictions, but scouts, on average, lose money in *Incentive* (-2.08, $p < .01$) and in *Discretionary* (-2.67, $p < .01$). In sum, both contract types appear to suffer from a similar trade-off between contractual efficiency and fairness, but the *Low* probability environment exhibits a much larger loss of efficiency (due to stronger undersearch) *and* a much more unfair distribution of profits.

We next assess whether the observed scouting inefficiency and unfair profit distributions derive from ill-designed incentives or unreasonable responses to well-designed incentives (or, worse, both). To do so, we study managers' contract decisions (Sections 6.1.2) and scouts' contract responses (Section 6.1.3).

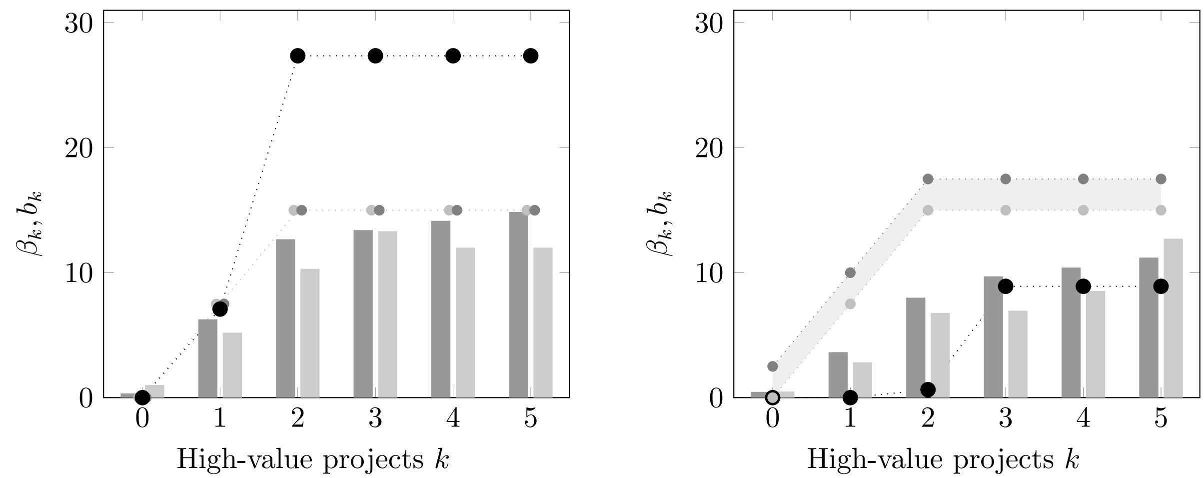
6.1.2. Bonus Efficiency and Anatomy. To assess the efficiency of managers' bonus decisions at an aggregate level, we compute the average conditionally optimal search $\bar{y}^*$ that a rational scout would choose given the manager's bonus schedule (cf. Section 4.4.3), and compare it to the optimal benchmark in Proposition 2. For *Incentive*, $\bar{y}^*(\beta)$ falls short of optimal search in *High* (4.41 vs. 6, $p < .01$), but the shortfall is substantially worse in *Low* (1.64 vs. 12, $p < .01$). Similarly, for *Discretionary*, conditionally optimal search $\bar{y}^*(\bar{b}^{\text{di}})$ is also considerably too low (*High*: 4 vs. 6; *Low*: 1 vs. 12). But why do managers fail to provide adequate incentives for search, and why do low success probabilities exacerbate the problem?

The anatomy of the observed contracts sheds more light on this question. Figure 2 displays the average *Incentive* contract parameters (■ $\bar{\beta}_k^{\text{in}}$) and *Discretionary* bonus payments (■ $\bar{b}_k^{\text{di}}$) for different search outcomes k. We test the key properties of these contracts using the econometric framework described in Appendix D.4.3, which also reports the full statistical analysis.

Our first observation is that *Incentive* contracts are structured very similar to the (implied) incentives of *Discretionary* contracts ($\bar{\beta}_k^{\text{in}} \approx \bar{b}_k^{\text{di}}$ for all k), and that both contracts exhibit a piecewise-linear structure with constantly increasing bonus payments for $k \leq 2$ and a much flatter slope for $k > 2$. Our second observation is that managers offer higher compensation in *Low* than in *High* across all outcome levels, to reimburse scouts for the more extensive search efforts required in *Low*. Our last, and most central, observation is that managers pay near-zero bonuses for unsuccessful scouting ($\bar{\beta}_0^{in} \approx \bar{b}_0^{di} \approx 0$) and never more than half of the created value otherwise ($\bar{\beta}_k^{in} \approx \bar{b}_k^{di} \leq v_{k;2}/2$).

Figure 2 **Study 2: Bonuses (Optimal ●, Truly Fair ●, Superficially Fair ●; Data: Incentive ■, Discretionary ■)**

(a) *Low* ($q = 0.1$) **(b)** *High* ($q = 0.5$)



While behaviorally appealing for their complexity reduction and fairness, the observed piece-wise linear contracts are detrimental to scouting incentives and thus scouting performance (cf. Proposition 3), for reasons that differ across uncertainty conditions. Recall that in *High* success probability environments, managers must prevent scouts from receiving attractive bonuses when they deliver only few high-value projects. However, managers in our data offer linear contracts that provide steeper-than-optimal incentives for scouts that deliver only few high-value projects, which incentivizes—rather counterintuitively—undersearch. In *Low* success probability environments, the superficially fair linear contracts in our data perform even worse, as they are incompatible with another key feature of optimal contracts (Section 3.2.2): incentivizing high search effort in *Low* requires managers to cede most of the created value for good (but unlikely) scouting outcomes. Ironically, managers might perceive such optimal contracts as unfair to themselves, which could explain why we do not observe them in the data. Finally, superficially fair contracts induce a peculiar side effect: scouts receive considerably lower average bonus payments in *Low* than in *High*, both in *Incentive* (4.36 vs. 7.58, $p < .01$) and in *Discretionary* (3.42 vs. 5.44, $p < .01$), even though they should receive higher bonus payments in *Low* to offset the higher search costs.

6.1.3. Conditional Scouting Decisions. We have thus far established that managers design piecewise-linear incentives, but that these contracts severely harm scouting efficiency in *High* and, even more so, in *Low*. This quality assessment was made at the level of conditionally (on observed contracts) optimal scouting decisions $\bar{y}^*(\beta)$ rather than observed decisions y_{it}, leaving open the question about the impact of scouts' search on the efficiency loss we observe. After all, it is possible (although certainly apriori unlikely) that scouts search significantly fewer projects than predicted by theory simply because they best respond to poorly designed contracts; i.e., $y_{it} = y^*(\beta_{it})$.

Using the contract data from *Incentive* (see Appendix D.4.4 for the full statistical analysis), we find that scouts are, in general, sensitive to the contracts they receive: they tend to search more when they should. At a more aggregate level, we also observe that average search in *High* is not different from conditional optimal search (4.62 vs. 4.55, $p = .85$). That is, the bulk of the observed performance loss in *High* appears to result from ill-designed contracts that incentivize scouts to undersearch (relative to $y^* = 6$). The data tell a different story in *Low*, where the average observed search is substantially *larger* than conditional optimal search (6.43 vs. 1.78, $p < .01$), although still considerably lower than first-best search (6.43 vs. 12, $p < .01$). Given that scouting performance is poor in *Low* despite scouts responding better than expected to their ill-designed contracts, we can conclude that poor contract design is the primary driver of the observed efficiency loss.

6.2. Study 2: Summary and Outlook

Study 2 offers broad support for Hypotheses 1 and 2 as we find evidence of fairness and reciprocity concerns in the manager-scout relationship—our fundamental behavioral assumption. Yet, overall scouting efficiency remains poor across both contract regimes, and profit distributions are truly unfair, especially in *Low*. The culprit are *superficially fair* contracts: managers appear to base bonuses on a fair split of the observable outcome (i.e., created value $v_{k;2}$), with little or no weight on the unobservable components of outcome (i.e., the scout's search cost cy).

However, even though being consistent with superficial fairness, our data leave the role of effort-based reciprocity ambiguous: the observed bonus schemes are not only consistent with managers who do not care about effort per se and merely split created value, but also with managers who do care about effort-based reciprocity, yet are either underestimating effort or unwilling to reward it beyond a (superficially) fair split of value. Study 2 alone does not allow us to resolve this ambiguity: with search effort unobservable, any bonus payment is confounded with the ensuing search outcome, which prevents us from directly measuring managers' response to effort. In addition, by design, managers cannot communicate expectations—or form adequate beliefs—about search effort, and the one-shot interactions foreclose any trust-building that might activate effort-based reciprocity. To overcome this issue, Studies 3 and 4 introduce additional institutional features that should facilitate effort-based reciprocity, thereby allowing us to shed more light on the superficial fairness mechanism and to perhaps entice more efficient and equitable scouting outcomes.

7. Study 3: Effort-Based Reciprocity

Study 2 provides evidence of basic reciprocity motives: scouts do search, and managers do pay bonuses in *Discretionary*, even though standard theory predicts that search should collapse. However, scouting outcomes are inefficient and truly unfair, especially when success probabilities are *Low*. Using the *Discretionary* setting of Study 2 as a baseline, Study 3 tests whether stronger conditions for reciprocity can overcome the superficial fairness mechanism that appears to drive inefficient scouting in Study 2. Towards that goal, we introduce scouting recommendations: before scouts engage in their search, managers can send a non-binding scouting recommendation, creating a shared reference point for effort expectations and a basis for effort-based reciprocity.

Since the concept of superficial fairness relates to managers underweighting (or even outright ignoring) the scout's search costs in their bonus decisions, Study 3 implements two treatments that enable us to study the effects of search effort more directly. First, *Unobservable* is identical to the *Discretionary* treatment of Study 2, except for the recommendation feature and the addition of a belief elicitation task: immediately before deciding on the scout's bonus payment, managers must explicitly state their belief about the scout's search effort. This allows for a clean test of whether managers condition bonus payments, independent of outcomes, on assumed search effort. The second treatment, *Observable*, informs managers about the scout's *actual* search effort before having to determine bonus payments. Since this allows managers to disentangle search effort from luck, *Observable* should facilitate effort-based reciprocity, if managers are so motivated. Yet, we also note that even with observable search efforts, outcome-based reciprocity (and with it, the detrimental effect of superficially fair bonuses) may still dominate effort-based reciprocity (Rubin and Sheremeta 2016).

The implementation of Study 3 follows Study 2, and we run both treatments (*Observable* and *Unobservable*) in a *Low* and *High* success probability condition. In addition to the variables observed in Study 2, we collect, in each period t and for each pair i, the manager's scouting recommendation r_{it} and, in *Unobservable*, the manager's belief about the scout's search effort, $\tilde{y}_{it}$.

7.1. Results

Table 3 presents our aggregate results, which we analyze in the following.

Table 3 Study 3: Treatment Effects

		Recomm. $\bar{r}$	Search $\bar{y}$	Success $\bar{k}$	Bonus $\bar{b}$	Scout utility $\bar{u}$	Manager profit $\bar{\pi}$	Total profit $\bar{\Pi}$	Efficiency $\bar{\Pi}/\Pi^{\text{fb}}$	Fairness $\bar{u}/\bar{\Pi}$
High	*Unobservable*	6.73	3.93	1.99	7.62	3.68	13.97	17.65	80%	21%
	Observable	7.87	4.63	2.35	8.19	3.56	15.02	18.57	84%	19%
	(*p-value*)	(.48)	(.04)	(.02)	(.67)	(.92)	(.33)	(.28)		
Low	*Unobservable*	11.79	6.35	0.70	4.77	-1.58	3.71	2.13	55%	< 0
	Observable	12.77	8.25	0.87	5.98	-2.27	5.42	3.15	81%	< 0
	(*p-value*)	(.45)	(.30)	(.39)	(.43)	(.46)	(.06)	(.01)		

7.1.1. Scouting Efficiency vs. Fairness. We begin our analysis in the *High* success probability environment. In *Unobservable*, similar to Study 2, total profits are 80% of first-best profits (17.65 vs. 22.13, $p < .01$) because scouts search substantially fewer projects than optimal (3.93 vs. 6, $p < .01$). Despite their insufficient search efforts, scouts still retain 21% of total profits, which is consistent with the basic reciprocity motives documented in Study 2. Interestingly, neither scouting efficiency nor distributional fairness improve significantly when search efforts become observable. In *Observable*, total profits stay at 84% of first-best profits (18.57 vs. 22.13, $p < .01$), and scouts continue to keep 19% of total profits. And although we observe a slight increase in search effort in *Observable*, this does not translate into significantly higher total profits, primarily because scouts still undersearch considerably (4.63 vs. 6, $p < .01$).

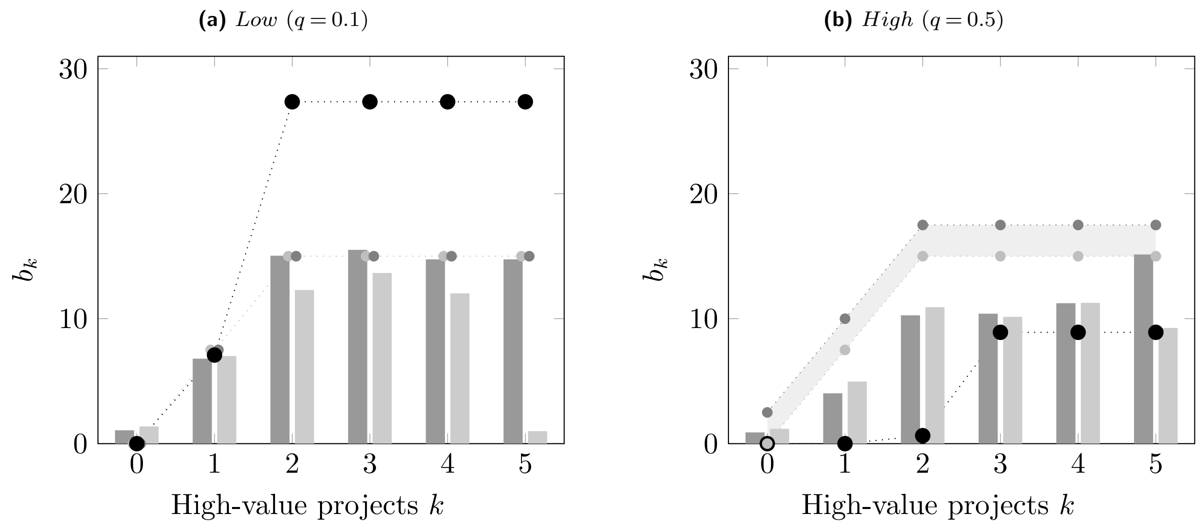
Turning to the *Low* success probability environment, we find that observable efforts have a more intricate effect on scouting outcomes. In *Unobservable*, we again find similar efficiency and fairness patterns as in Study 2: total profits are 55% of first-best profits (2.13 vs. 3.88, $p < .01$), caused by scouts searching too few projects (6.35 vs. 12, $p < .01$), and the distribution of profits remains highly unfair as scouts, on average, continue to lose money (-1.58, $p = .07$). In *Observable*, however, scouting efficiency improves significantly. Even though search efforts do not increase significantly and total profits remain below first-best profits (3.15 vs. 3.88, $p < .01$), profits increase compared to *Unobservable*.[9] Interestingly, these efficiency gains accrue solely to managers and thus reinforce the unfair distribution of profits: scouts lose even more money in *Observable*, whereas managers earn more than first-best profits.

There are two plausible explanations for our observation that observable efforts improve scouting efficiency. First, observable efforts might induce managers to offer better (e.g., steeper or more generous) outcome-based bonuses (Section 7.1.2). Second, they might entice managers to directly reward search effort y on top of scouting outcomes k (Section 7.1.3). Although related, these two reasons are decidedly distinct: the first works entirely through outcome-based rewards; the second through a reward for effort that outcomes alone would miss.

7.1.2. Bonus Efficiency and Anatomy. The premise of *Observable* is to allow managers to directly compensate scouts for their actual search costs, thereby strengthening search incentives and improving scouting efficiency. To assess whether our data support this conjecture, we first study the aggregate incentive effect of the observed bonus schemes. Specifically, for each treatment, we use average bonus payments $\bar{b}_k$ (as displayed in Figure 3) to calculate the conditionally optimal search

[9] In *High*, a small statistically significant increase in search does not translate into higher total profits, whereas in *Low*, a larger yet insignificant increase in search leads to statistically higher total profits. This happens because in *Low*, total profits are more sensitive to (small) changes in search as any additional search effort materially impacts the chance of finding at least one high-value project, which is the *only* way for a manager to obtain positive profits. In contrast, in *High*, even strong undersearch patterns allow scouts to identify high-value projects and generate profits.

Figure 3 Study 3: Bonuses (Optimal ●, Truly Fair ●, Superficially Fair ●; Data: Unobservable ■; Observable ■)



decision $\bar{y}^*(\bar{b})$. In *High*, conditionally optimal search is slightly below first-best levels, in *Observable* (4 vs. $y^{\text{fb}} = 6$) and in *Unobservable* (5 vs. $y^{\text{fb}} = 6$). In contrast, in both *Low* treatments, the conditionally optimal search is 0 (vs. $y^{\text{fb}} = 12$). As in Study 2, the bonus schedules underlying the inefficient and unfair scouting outcomes are piecewise linear and only superficially fair (Appendix D.5.3): managers pay near-zero bonuses for unsuccessful search ($\bar{b}_0 \approx 0$) and do not share more than half of the created value otherwise ($\bar{b}_k \leq v_{k;2}/2$).

If bonuses in Study 2, and by extension in *Unobservable*, were low merely because managers cannot observe effort and thus ignore search costs, then we would expect managers to pay higher bonuses when effort becomes observable. Our data clearly show that this does not happen: there is no difference in bonus payments between *Unobservable* and *Observable* at any level of k (see Figure 3 and Table EC.12). However, this aggregate finding does not provide conclusive evidence against the presence of effort-based reciprocity, as its (possibly subtle) effects may only be masked.

7.1.3. Effort-Based Reciprocity. To isolate the effect of search effort on bonus payments beyond what is explained by search outcomes alone, we estimate

$$b_{it} = \alpha_0 + \alpha_1 k_{it} + \alpha_2 (k_{it} - 2)^+ + \alpha_3 Y_{it} + \varepsilon_{it}, \tag{10}$$

where α_1 and $\alpha_1 + \alpha_2$ reflect the slope of the piecewise linear bonus schedule for $k \leq 2$ and $k > 2$, respectively. Search effort $Y_{it} \in \{y_{it}, \tilde{y}_{it}\}$ is our key variable of interest, capturing the manager's belief $\tilde{y}_{it}$ about search efforts in *Unobservable*, and the scout's actual search effort y_{it} in *Observable*. Table 4 presents the results, including (in the Δ-column) between-treatment comparisons from a pooled specification that adds a treatment dummy interacted with each regressor (Table EC.13).

Table 4 Study 3: Contract Anatomy and Effort-Based Reciprocity

	Low ($q=0.1$)				*High* ($q=0.5$)			
DV: *Bonus b* (*p-value*)	*Observable* ($Y=y$)	Δ	*Unobservable* ($Y=\tilde{y}$)	($Y=y$)	*Observable* ($Y=y$)	Δ	*Unobservable* ($Y=\tilde{y}$)	($Y=y$)
α_0	2.10	(.43)	1.61	1.16	0.21	(.92)	0.27	-0.45
	(.01)		(.01)	(.02)	(.70)		(.50)	(.09)
α_1	5.20	(.72)	5.59	6.43	5.32	(.62)	4.90	5.27
(*slope for* $k \leq 2$)	(.00)		(.00)	(.00)	(.00)		(.00)	(.00)
$\alpha_1+\alpha_2$	-0.72	(.54)	-0.01	-0.22	-0.31	(.01)	0.53	0.87
(*slope for* $k>2$)	(.54)		(.97)	(.42)	(.26)		(.01)	(.01)
α_3	0.37	(.76)	0.32	0.06	-0.12	(.38)	0.13	-0.22
(*search effort*)	(.01)		(.06)	(.16)	(.46)		(.61)	(.27)

We find that managers directly reward effort in *Low*, with no difference between *Observable* and *Unobservable*. That is, managers equally appreciate effort irrespective of whether they can observe or only infer it. In contrast, managers in *High* do not directly reward effort in either treatment. Thus, effort-based reciprocity emerges only in *Low*, consistent with the informational role of effort across environments. When success is likely in *High*, scouting outcomes are a reliable signal of effort, so conditioning bonuses on effort adds little beyond what outcomes already convey. However, when the effort-outcome link is tenuous and search might be unsuccessful despite high search efforts in *Low*, effort carries crucial information about the scout's behavior that managers seek to reward.

But why do scouts search more in *Low* when effort is observable, given that managers want to reward effort and outcomes equally in *Unobservable* and *Observable*? The reason lies in the managers' belief structures: while correct on average, their beliefs overestimate low search efforts and underestimate high ones.[10] Put differently, managers in *Unobservable* find it difficult to distinguish a hard-working scout from a lazy one. This classification issue, in turn, prevents managers from rewarding search efforts adequately, which weakens the direct effect of effort on bonus payments compared to *Observable*. Formally, we observe in Table 4 that bonus payments in *Unobservable* are much less sensitive to actual search efforts y than they are in *Observable* (0.06 vs. 0.37, $p<.01$). In other words, observing (instead of only inferring) search efforts allows managers to more directly incentivize search, which improves scouting efficiency. Yet, this improvement is only modest, and we continue to observe significant undersearch despite the opportunity for direct effort incentives and the presence of scouting recommendations.

[10] On average, managers' beliefs deviate from actual search by 0.48 in *High* and -1.07 in *Low* (both $p<.01$). But beliefs track actual search only weakly: regressing beliefs on actual search, $\tilde{y}_{it}=a_0+a_1\,y_{it}+\varepsilon_{it}$, gives a slope below one ($a_1=0.41$ in *High*, $a_1=0.25$ in *Low*; both $p<.01$). Since beliefs are right on average yet too flat, managers overestimate low search efforts and underestimate high search efforts.

7.1.4. Scouting Recommendations. We implemented scouting recommendations to enable managers and scouts to build a common understanding of what metric to reciprocate on. An initial observation is that descriptively, recommendations in *Unobservable* do not increase search relative to the corresponding *Discretionary* treatment of Study 2, neither in *High* (3.93 vs. 4.11) nor in *Low* (6.35 vs. 6.09).[11] That is, giving managers a mechanism to set search expectations does not by itself alleviate the undersearch problem, and the same pattern persists in *Observable*.

One possible explanation for the persistent undersearch is that managers simply recommend searching too few projects, but the data reject this conjecture (Table 3). In all treatments, scouting recommendations are indistinguishable from first-best scouting (all $p > .22$), which corroborates our Study 1 finding that subjects are well-calibrated in their assessment of optimal search effort. Instead, scouts simply do not comply with their recommendations: their actual search $\bar{y}$ is much lower than the scouting recommendations $\bar{r}$ they receive (all $p < .07$). This does not imply that scouts ignore their recommendations altogether, but mediation analyses (Appendix D.5.5) suggest that the impact of recommendations on search is modest at best. Moreover, observable efforts do not meaningfully affect the recommendations managers give or the extent to which scouts respond to them, neither in *High* nor in *Low*. To explain why search falls short of recommendations, we posit the same culprit as for the limited effect of observable effort: outcome-dependent, superficially fair bonuses that do not make following recommendations worthwhile.

7.2. Study 3: Summary and Outlook

By providing stronger conditions for effort-based reciprocity—via observable effort and scouting recommendations—Study 3 is designed to shed light on the mechanisms behind the superficially fair contracts we observe in Study 2, and overcome the scouting inefficiency that such contracts entail. Yet, we find that scouting inefficiencies and unfair profit distributions prevail in all treatments, particularly in *Low*. Our analysis of observed and assumed efforts shows that the culprit are, again, superficially fair contracts. Although managers reward search effort on top of mere search outcomes, effort-based reciprocity appears to operate within a superficially fair ceiling: managers refuse to pay more than half of the created value (i.e., $\bar{b}_k \leq v_{k;2}/2$), including near-zero bonuses for unsuccessful search outcomes (i.e., $\bar{b}_0 \approx 0$), and this pattern is not changed when managers can observe search effort or give scouting recommendations.

To foster better scouting outcomes, Studies 4 and 5 tackle the manager's incentive design directly. Study 4 allows managers to compensate their scouts' search costs by offering an upfront payment that scouts can keep irrespective of search outcomes. This may alleviate scouts' fairness concerns

[11] A direct statistical test with Study 2 is not feasible, owing to the temporal distance between the two studies' sessions as well as minor variations in experimental setup.

in the likely event (particularly in *Low*) that they do not find any successful projects. Study 5, in contrast, enforces an optimal incentive design to overcome the insufficient bonus payments for successful but unlikely search outcomes, which are particularly devastating in *Low*.

8. Study 4: Upfront Payments and Repeated Interaction

A key implication of Study 3 is that, even when managers can observe search efforts and provide scouting recommendations, the (weak) effort-based reciprocity we observe is not strong enough to overcome the detrimental outcome-based search incentives that superficially fair contracts entail. In Study 4, we examine two additional treatments designed to further strengthen the institutional foundations for reciprocity and thereby overcome scouting inefficiency.

The first treatment, *Upfront*, modifies the *Unobservable* setup of Study 3 by allowing managers to share an upfront payment $w \geq 0$ that scouts keep regardless of scouting outcomes. In Study 3, scouts who follow the manager's recommendation have to trust that they will be compensated for their effort ex post—even when search is unsuccessful (a likely scenario in *Low*). An upfront payment, in contrast, allows managers to lend weight to their recommendation before search takes place, thereby potentially inducing scouts to search more. Upfront payments may also mitigate scouts' concerns about interim losses from their search efforts. As in Study 3, managers in *Upfront* can still pay an additional discretionary bonus after outcomes are realized, enabling them to reciprocate when favorable outcomes are indicative of high search effort.

The second treatment, *Repeated*, pairs one manager with one scout for the entire duration of the experiment but is otherwise identical to *Upfront*. Repeated interactions may improve scouting performance for two reasons: a stable relationship may help managers and scouts better align their expectations; and it may provide additional relational incentives that foster reciprocal behavior.

Beyond their potential effect on search, the two treatments also provide a more direct window into managers' distributional preferences. Since managers rarely reward scouts for unsuccessful search in our previous studies (i.e., $b_0 \approx 0$), observing significant upfront payments would suggest that managers seek to create truly equitable profit distributions but require favorable institutional conditions to do so. That said, both our theoretical analysis and the behavioral patterns observed in Studies 2 and 3 suggest that upfront payments can also cause incentive issues. While potentially strengthening effort-based reciprocity and search, managers who already transfer part of the created value via upfront payments may, in exchange, reduce outcome-contingent bonuses, leaving bonus schedules flatter and search incentives weaker. It is apriori unclear which effect will dominate; but the substitution effect is likely most detrimental in *Low*, where the superficially fair ceiling bears down on bonus payments precisely when steep outcome-based incentives are needed.

The implementation of Study 4 is largely identical to the *Discretionary* treatment of Study 2, and we again implement both treatments (*Upfront* and *Repeated*) in *Low* and *High*. In addition to the variables observed in Studies 2 and 3, we also observe in *Upfront* and *Repeated*, in each period t and for each pair i, the upfront payment w_{it}.

8.1. Results

Table 5 presents our aggregate results, which we subsequently discuss in more detail.

Table 5 Study 4: Treatment Effects

		Recomm. $\bar{r}$	Upfront $\bar{w}$	Search $\bar{y}$	Bonus $\bar{b}$	Total pay $\bar{w}+\bar{b}$	Scout utility $\bar{u}$	Manager profit $\bar{\pi}$	Total profit $\bar{\Pi}$	Efficiency $\bar{\Pi}/\Pi^{\text{fb}}$	Fairness $\bar{u}/\bar{\Pi}$
High	*Upfront*	6.51	6.73	3.77	2.84	9.57	5.80	9.64	15.44	70%	38%
	Repeated	5.98	6.18	4.06	5.22	11.41	7.34	10.88	18.23	82%	40%
	(*p-value*)	(.35)	(.10)	(.68)	(.00)	(.06)	(.07)	(.76)	(.14)		
Low	*Upfront*	9.63	6.06	4.34	1.34	7.39	3.05	-1.89	1.16	30%	>1
	Repeated	8.70	5.11	4.95	1.68	6.79	1.84	-0.29	1.55	40%	>1
	(*p-value*)	(.38)	(.43)	(.42)	(.20)	(.77)	(.30)	(.13)	(.17)		

8.1.1. Scouting Efficiency vs. Fairness.

An initial observation is that scouts in both *Upfront* and *Repeated* search less than in any of the treatments from Studies 2 and 3, and the difference is particularly large in *Low*.[12] As a result, total profits are also lower than in the previous studies, and the efficiency loss is more severe in *Low* than in *High* across both treatments. Partner matching in *Repeated* tends to increase search and total profits over *Upfront* in both success probability conditions, though differences are not statistically significant.

The introduction of upfront payments has a striking effect on the profit distribution. Despite lower total profits, scouts now receive a substantially larger profit share and scout utility is positive in all treatments, while managers lose money in both *Low* treatments. This represents a marked departure from Studies 2 and 3, where scouts suffered from negative utilities in *Low*. Upfront payments hence act as a pure fairness-improving transfer: they redistribute profits from manager to scout but leave scouting efficiency unchanged or worse.

8.1.2. Bonus Efficiency and Anatomy.

Upfront payments can have two opposing effects. On the one hand, they financially support managers' scouting recommendations and in so doing, may induce scouts to engage in more extensive search. On the other hand, managers may partially substitute outcome-based incentives b_k with upfront payments w, thereby creating flat bonus schemes that destroy search incentives altogether.

[12] A direct statistical test with Studies 2 and 3 is not feasible, owing to the temporal distance between the studies' sessions as well as minor variations in experimental setup.

Figure 4 **Study 4: Bonuses (Optimal ●, Truly Fair ●, Superficially Fair ●; Data: Upfront, Repeated)**

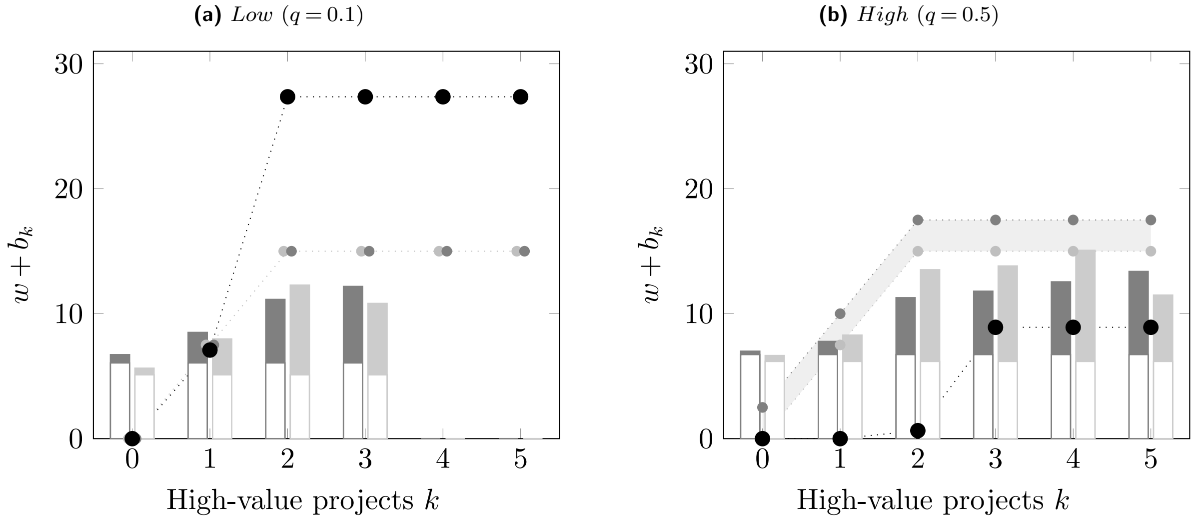


Figure 4 and the statistical analysis provided in Appendix D.6.3 reveal that substitution between w and b_k is indeed the dominant effect. Managers offer substantial upfront payments in both *Low* and *High*. As a result, and in stark contrast to theoretically optimal incentives, scouts receive considerable payments even when their search is unsuccessful (i.e., $w + b_0 \gg 0$). At the same time, however, managers offer much smaller and flatter bonuses schedules b_k than in Studies 2 and 3, which eliminate any search incentives. In fact, in *Low*, conditionally optimal search is 0 in both treatments (vs. $y^{\text{fb}} = 12$). This happens because superficially fair managers are unwilling to accompany their upfront payments with bonuses that transfer more than half of the created value to their scouts. Interestingly, in *Upfront*, the flattening of the bonus schedule is so severe that even in *High*, conditionally optimal search is 0 (vs. $y^{\text{fb}} = 6$). As Figure 4 shows, managers pay more than enough in aggregate (i.e., $w + b$), but they put too much emphasis on the upfront payment and consequently provide inadequate bonuses. We note that in *High*, repeated interactions can partially mitigate this issue: managers offer smaller upfront payments and higher bonuses, which increases the conditionally optimal search to 4 (vs. $y^{\text{fb}} = 6$) in *Repeated*.

We conjectured that the observed crowding-out of outcome-based bonuses by upfront payments could be offset by a strengthened reciprocal response—with recommendations r supported by upfront payments w creating a positive search incentive. However, this did not materialize, leaving open the question of whether w and r have any direct effect on search at all.

8.1.3. Upfront Payments and Scouting Recommendations. Managers' scouting recommendations in both treatments are approximately equal to first-best scouting in *High* (*Upfront*: 6.51 vs. 6, $p = .81$; *Repeated*: 5.98 vs. 6, $p = .63$), but fall short in *Low* (*Upfront*: 9.63 vs. 12, $p = .09$; *Repeated*: 8.70 vs. 12, $p < .01$). As in Study 3, however, scouts do not comply: they search well

Table 6 Study 4: Upfront Payments, Recommendations, and Search

DV: *Search y*	*Low* ($q = 0.1$)			*High* ($q = 0.5$)		
(*p-value*)	*Upfront*	Δ	*Repeated*	*Upfront*	Δ	*Repeated*
a_0	0.94	(.63)	1.40	0.19	(.34)	0.97
	(.04)		(.13)	(.26)		(.23)
a_1	0.22	(.43)	0.33	0.41	(.59)	0.51
(*fixed wage* w)	(.01)		(.01)	(.03)		(.01)
a_2	0.16	(.52)	0.27	0.28	(.59)	0.21
(*recommendation* r)	(.18)		(.08)	(.02)		(.11)
a_3	0.01	(.33)	-0.01	-0.03	(.62)	-0.04
($w \times r$)	(.56)		(.47)	(.08)		(.10)

below their recommendations in all treatments (all $p < .01$). So, do scouts react to higher recommendations at all, especially if these are backed up with upfront payments? To assess whether upfront payments w_{it} and scouting recommendations r_{it} motivate search y_{it}, we estimate

$$y_{it} = a_0 + a_1 w_{it} + a_2 r_{it} + a_3(w_{it} \times r_{it}) + \varepsilon_{it}. \tag{11}$$

The interaction term $w \times r$ captures whether a higher upfront payment makes recommendations more credible and therefore more effective in motivating search. Table 6 presents the results separately for *Upfront* and *Repeated*, including (in the Δ-column) between-treatment comparisons from a pooled specification that adds a treatment dummy interacted with each regressor (Table EC.20).

Both w and r do affect search positively, and the modest direct effect of recommendations is consistent with the mild compliance found in Study 3 (Appendix D.5.5). Moreover, managers who offer higher upfront payments also tend to issue more ambitious recommendations (observation-level correlations of 0.04–0.19 across treatments), suggesting the two instruments are used in tandem. However, we find no evidence that higher upfront payments amplify the effect of recommendations on search; if anything, this effect is weakly negative in *High*. Also, none of the between-treatment differences (Δ-column in Table 6) are significant, indicating that the mechanisms by which w and r affect search do not differ between *Upfront* and *Repeated*. We hence conclude that any positive effect of w and r on search is more than offset by managers reducing outcome-based bonuses when also offering upfront payments, owing to their superficial fairness concerns.

8.2. Study 4: Summary and Outlook

Study 4 examines whether boosting effort-based reciprocity through an unconditional upfront payment can substitute for outcome-based search incentives. We find that managers do offer substantial upfront payments and that they are willing to accept the risk of losses in *Low*, where even optimal search frequently yields unsuccessful outcomes. This suggests that managers have a genuine desire to compensate scouts for their search efforts and achieve a more equitable profit distribution.

However, our evidence also suggests that this desire comes at a cost: the positive reciprocity effect of upfront payments on search is more than offset by the weaker outcome-contingent bonuses it induces, leaving scouting efficiency no higher than in Studies 2 and 3. In fact, the efficiency loss is particularly severe in *Low*, where the scope for reciprocity is most limited and outcome-based incentives matter most. Intriguingly, the data from *Repeated* provide little evidence that a stable relationship between managers and scouts improves scouting efficiency: despite better conditions for synchronized expectations and relational incentives, managers and scouts continue to struggle with the same fundamental tension between upfront payments and outcome-based bonuses.

The root cause for the observed scouting inefficiency, which is most acute in *Low* success probability environments, remains the same across all studies: managers are unwilling to offer the large outcome-contingent bonuses that theory prescribes for high but unlikely search outcomes (cf. Figure 1). So far, no institutional arrangement studied was able to change this behavioral pattern. Study 5 confronts this issue directly.

9. Study 5: Scouting under Optimal Incentives

The findings of Studies 2–4 consistently show that in delegated innovation scouting, managers offer superficially fair contracts that inadequately compensate their scouts' search costs, inducing a systematic undersearch pattern whose efficiency loss is largest precisely in the low success probability environments that characterize scouting practice—even though individual search without delegation approaches first-best levels (Study 1).

Underlying this evidence is an implicit, yet untested claim: scouting inefficiency is caused by managers' contract design, because scouts would search substantially more—perhaps even close to first-best—if they faced optimally designed contracts that managers in our data effectively never offer. Put differently, will scouting efficiency improve under the optimal incentive schemes characterized by Proposition 2, or will scouts continue to undersearch even with optimal incentives?

To directly test this claim, we implement a new treatment, *Automated*, that simply enforces the optimal incentives from Proposition 2. That is, *Automated* mirrors the *Incentive* treatment but replaces human managers with a computer that always offers the optimal contract β^* to human scouts. This setup is designed to isolate scouts' response to optimal incentives from managers' reluctance to offer them. Because inadequate incentives are most consequential when success is elusive, we run *Automated* in the *Low* success probability condition. As a benchmark and to ensure statistical comparability, we also re-run the delegated *Incentive* treatment from Study 2 in *Low*, which we label *Incentive2*. As in the *Unobservable* treatment of Study 3, *Incentive2* additionally elicits the manager's belief about the scout's search effort, $\tilde{y}_{it}$, after outcomes are realized.[13]

[13] In *Incentive2*—similar to *Unobservable* (Study 3)—managers' elicited beliefs about scouts' search effort are accurate on average yet too flat, over-estimating low search efforts and under-estimating high search efforts (Appendix D.7.2).

Table 7 Study 5: Treatment Effects

		Search $\bar{y}$	Success $\bar{k}$	Bonus $\bar{b}$	Scout utility $\bar{u}$	Manager profit $\bar{\pi}$	Total profit $\bar{\Pi}$	Efficiency $\bar{\Pi}/\Pi^{\text{fb}}$	Fairness $\bar{u}/\bar{\Pi}$
Low	*Incentive2*	5.62	0.58	3.79	-1.83	3.62	1.78	46%	< 0
	Automated	12.85	1.35	12.40	-0.45	3.29	2.83	73%	< 0
	(*p-value*)	(.00)	(.00)	(.00)	(.01)	(.65)	(.07)		

9.1. Results

Table 7 presents our aggregate results. Our main observation is that the optimal contract β^* incentivizes scouts to search at the first-best level in *Automated* (12.85 vs. 12, $p = .27$), leading them to evaluate more than twice as many projects as under the inadequate incentives provided by human managers in *Incentive2*. This increase in search effort, of course, translates into markedly higher total profits. Nonetheless, total profits in *Automated* fall short of their theoretical benchmark (2.83 vs. 3.88, $p < .01$), owing mostly to variability in scouts' search decisions (see Table EC.22).

Further comparison of *Automated* and *Incentive2* yields another key insight: in low success probability environments, the extra value created through improved scouting in *Automated* accrues primarily to scouts. In fact, scout utility increases significantly, whereas manager profit remains essentially stable. This result demonstrates that both managers and scouts are (weakly) better off under optimal incentive schemes, reinforcing the conclusion that these schemes Pareto-dominate the superficially fair incentives observed in Studies 2–4.

9.2. Study 5: Summary and Outlook

Study 5 offers direct managerial implications. It shows that to overcome the scouting inefficiency caused by managers' superficial fairness concerns, firms must enforce—even if structurally complex—the implementation of optimal incentive schemes. Intriguingly, these optimal incentives also induce a fairer distribution of total profits. To ensure that managers in practice install optimal incentive schemes, firms must educate them to combine disciplined tolerance for scouting failures with generous rewards for highly successful outcomes, particularly in low success probability environments. It is equally important that managers' reciprocity concerns are addressed through thoughtful organizational and process design rather than through a simplistic, piecewise-linear incentive design that inadvertently undermines scouting performance.

10. Conclusion

Although widespread in practice, behavioral aspects of innovation search have received but scant attention in the academic literature. Existing studies have primarily examined centralized and sequential search processes (Billinger et al. 2014, Sommer et al. 2020), whereas innovation scouting typically involves delegated and parallel search, in which scouts are tasked with searching for multiple external innovation opportunities. This study therefore asks: What environmental conditions and behavioral biases shape the effectiveness of firms' innovation scouting activities?

Our theoretical analysis shows that rational, self-interested managers can, in principle, design incentive schemes that induce scouts to search at first-best levels. Yet, our experimental evidence reveals a stark deviation from this benchmark: human *managers* struggle to design incentive schemes that motivate scouts to search efficiently, and human *scouts* fail to respond optimally to the incentives they receive. Both behavioral anomalies are particularly prominent in low success probability environments—a defining feature of innovation scouting in practice. Our findings are consistent with reciprocity concerns that lead managers to offer (formal or informal) piecewise-linear contracts that result in only a *superficially* fair division of created value and largely ignore scouts' search costs. Importantly, superficially fair incentives not only mute scouting efficiency by deterring scouts from searching at an adequate level, but they also lead to fundamentally unfair profit distributions. Both effects are particularly strong and reinforcing when success is elusive.

Our results demonstrate that managers and scouts cannot naturally overcome the inherent trade-off between contractual efficiency and fairness imposed by delegated innovation scouting. What, then, can firms do to improve scouting performance? Our experiments show that scouts exert first-best search effort under theoretically optimal incentive schemes, suggesting that firms can restore scouting efficiency, and also improve distributional fairness, by implementing the non-linear, structurally complex incentives derived in this study. Whether such contracts can be implemented in practice depends on what drives the emergence of piecewise-linear incentives in our data. If complexity is the key constraint—a mechanism we briefly discuss but do not test directly—firms must educate managers on the intricate structural properties of optimal incentives, including the need for limited tolerance for scouting failures and generous rewards for successful outcomes. If, instead, fairness is the dominant driver—as our behavioral evidence suggests—firms should address reciprocity concerns through thoughtful process and organizational design rather than through superficially fair incentive structures.

Overall, because substantial performance losses persist across all treatments in which human managers design search incentives, our findings open several avenues for future research. Promising directions include exploring the boundary conditions of fairness-driven tolerance for failure, the suitability of alternative incentive structures, and additional interventions that can help managers balance fairness and efficiency in delegated search for innovation. By integrating insights from behavioral agency theory into the study of innovation search, this work advances our understanding of how human behavior shapes the efficiency of firms' boundary-spanning innovation activities.

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Online Appendix

Delegated Innovation Scouting when Success is Rare: A Behavioral Investigation

A. Model Extensions

A.1. Delegated Scouting with Piece-Wise Linear Incentive Schemes

In this section, we analyze the manager's incentive design problem (1)–(3) when the manager must use (piece-wise) linear incentive schemes that depend on only one parameter $\lambda \geq 0$. In particular, we assume $\beta_k = \lambda v_{k;2}$ for all k; that is, the scout receives a constant share λ of the total value created through the discovery of high-value projects (recall that only the first two high-value projects create any value for the manager). Proposition EC.1 characterizes the optimal linear incentive scheme and its associated properties.

Proposition EC.1. *Suppose that the manager must use a (piece-wise) linear incentive scheme of the form $\beta_k = \lambda v_{k;2}$ for all $k \in \mathbb{N}_0$, with $\lambda \geq 0$, and define $\lambda[y] = c/(vq(1-q)^{y-2}(1+q(y-2))) \geq c/(qv)$. The optimal linear incentive scheme β^l is unique, with $\lambda^l \equiv \lambda[y^l]$, and $y^l \geq 2$ satisfies the following conditions:*

$$\begin{aligned} &q(1-q)^{y^l-1}(1+q(y^l-1)-\lambda[y^l+1]) - (2-(1-q)^{y^l-1})(\lambda[y^l+1]-\lambda[y^l]) < 0 \\ &\quad \leq q(1-q)^{y^l-2}(1+q(y^l-2)-\lambda[y^l]) - (2-(1-q)^{y^l-2})(\lambda[y^l]-\lambda[y^l-1]). \end{aligned} \tag{EC.1}$$

Under an optimal linear incentive scheme β^l—compared to an optimal (non-linear) incentive scheme β^*—the scout searches less projects (i.e., $y^l \leq y^*$) and receives a higher expected utility (i.e., $u^l \geq u^*$), whereas the manager realizes lower expected profits (i.e., $\pi^l \leq \pi^*$).

It is not surprising that, in general, the manager cannot obtain first-best profits anymore if relying on only linear incentive schemes (i.e., $\pi^l \leq \pi^* = \pi^{\text{fb}}$). This happens for two reasons. First, the scout evaluates less projects than optimal (i.e., $y^l \leq y^*$), which reduces the expected total value created. Second, the scout is also able to extract rents from the manager (i.e., $u^l \geq 0$), which directly reduces the manager's return from the identified high-value projects. As a result, linear incentive schemes have a significantly negative impact on the manager's expected profits—particularly so in environments with low success probabilities (i.e., low q).

We further illustrate the consequences of relying on (piece-wise) linear incentive schemes by deriving the scouting outcomes under such an incentive scheme in our experimental conditions. In the *High* condition, the manager optimally sets $\lambda^l = 0.27$, which motivates the scout to evaluate $y^l = 4$ projects. As a result, the manager earns expected profits $\pi^l = 17.88$ and the scout receives an expected utility of $u^l = 2.50$. Total expected profits thus amount to 92% of total expected profits under an optimal incentive scheme β^*. In contrast, in the *Low* condition, the manager optimally sets $\lambda^l = 0.75$, which induces the scout to evaluate $y^l = 7$ projects. As a result, the manager earns expected profits $\pi^l = 2.49$ and the scout receives an expected utility of $u^l = 0.58$. Total expected profits thus amount to only 79% of total expected profits under an optimal incentive scheme β^*.

A.2. The Role of Loss Aversion

Delegated innovation scouting requires the scout to expend search efforts—and thus to incur search costs—before receiving any bonus payments for successful scouting outcomes from the manager. In other words, during the scouting process, the scout will inevitably face interim losses that may (or may not) be recouped depending on the stochastic scouting outcomes. A simple, yet effective way to incorporate a decision maker's aversion against such interim losses is the introduction of a loss aversion parameter $L > 1$, which increases the scout's search costs in a multiplicative fashion. With this additional parameter, the manager's incentive design problem (1)–(3) becomes:

$$\max_{\beta \geq 0} \ \pi(\beta) = \sum_{k=0}^{y^*} \big(v_{k;n} - \beta_k\big)\mathbb{P}\big(X(y^*;q) = k\big) \tag{EC.2}$$

$$\text{s.t. } y^* \in \arg\max_{y \in \mathbb{N}_0} u(y) = \sum_{k=0}^{y} \beta_k \mathbb{P}\big(X(y;q) = k\big) - Lcy \tag{EC.3}$$

$$u(y^*) \geq 0. \tag{EC.4}$$

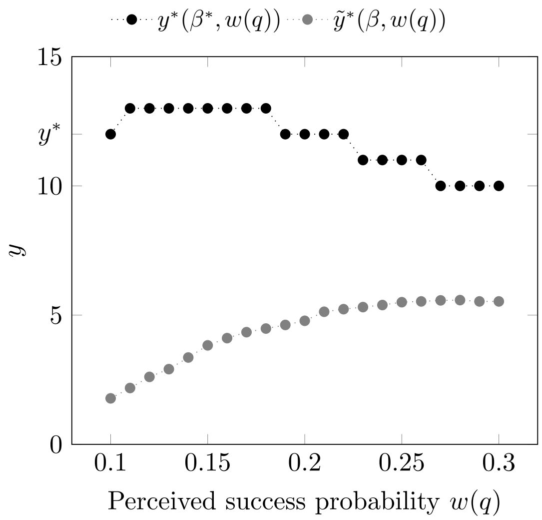
Proposition 2(i) in conjunction with Proposition 1(ii) immediately implies that $y^*(L)$ decreases with L; that is, the higher the scout's loss aversion, the less projects the scout searches in equilibrium. Furthermore, by Proposition 2(ii), if $L > qv/c$, then $y^*(L) = 0$, implying that a sufficiently strong loss aversion may result in a total collapse of the scouting process.

A.3. The Role of Probability Weighting

In this section, we provide a possible rationale for a surprising behavioral anomaly that we find in our data: Why do scouts tend to search more than is warranted by the poor contracts that they receive in *Low* but not in *High*? We argue that biased probability judgments offer a simple explanation for this observation. Specifically, a rich behavioral literature on judgment and decision making under uncertainty documents that decision makers tend to overweight small (success) probabilities like the ones in our *Low* treatment, whereas they assess medium probabilities (as in *High*) more accurately (see, e.g., Davis 2019). But even though this behavioral bias is well-established, it is not immediately obvious how it affects scouting in *Low*, as scouts' search decisions are also chiefly impacted by the incentive schemes under which they operate. Figure EC.1 sheds more light on the question of how biased probability judgments influence scouts' search behavior, as we explain next.

Denote $w(q)$ a scout's perceived success probability when the true success probability is q—in *Low*, $w(q) > q$. For our *Low* success probability environment (i.e., $q = 0.1$) and given an optimal contract β^*, we compare the optimal search of a biased scout, $y^*(\beta^*, w(q))$, with the optimal search of a properly calibrated scout, $y^*(\beta^*, q)$. Figure EC.1 (●) shows that mildly biased scouts ($w(q) < 0.19$) search more than their rational counterparts, while strongly biased scouts ($w(q) \geq 0.19$) search (weakly) less. However, one of our main empirical observations is that proposed incentive schemes in *Low* are anything but optimal. To assess how probability weighting behavior affects a scout's response to an ill-specified contract, using the data from the *Incentive* treatment of Study 2, we calculate the average optimal search decision of a biased scout (endowed with some $w(q)$) conditional on his received contract β_{it}: $\tilde{y}^*(\beta, w(q)) = \sum_{i,t} y^*(\beta_{it}, w(q))/IT$. Figure EC.1 (●)

Figure EC.1 Probability Weighting and Over-scouting (*Low*: $q = 0.1$)



shows that $\tilde{y}^*(\beta, w(q)) > \tilde{y}^*(\beta, q)$ for any reasonable $w(q) > q$; that is, for poorly designed incentive schemes, probability overweighting induces scouts to search more projects than they should.

Besides providing a plausible explanation for scouts' conditional "over-searching" behavior that we observe in *Low*, the overweighting of small success probabilities might also drive managers' poor contract decisions. When managers consider optimal contracts in *Low* as *superficially* unfair (to themselves) because these contracts include excessively high incentives β_k^* for $k \geq 2$, they effectively ignore that these incentives are unlikely to materialize under optimal search in low success probability environments—and even less so under the insufficient scouting behavior in our data. This suggests that managers place undue weight on unlikely success scenarios, which is in line with the overweighting of small success probabilities.

A.4. Fairness-Minded Scout

In our behavioral model (5)–(7), we assume that the manager is fairness-minded (as reflected by the parameter α), whereas the scout is purely self-interested. We now extend our base model to also include fairness concerns on the scout's behalf. Of course, the scout always knows the incurred search costs and thus, the concept of superficial fairness does not apply to the scout, but the scout may only care about true fairness. The manager's incentive design problem then becomes:

$$\max_{\beta \geq 0} \ \pi^{\mathrm{f}}(\beta; \alpha, \delta) = \sum_{k=0}^{y^{\mathrm{f}}} \big(v_{k;2} - \beta_k - \alpha(2\beta_k - v_{k;2} - \delta c y^{\mathrm{f}})^2\big)\mathbb{P}\big(X(y^{\mathrm{f}}; q) = k\big) \tag{EC.5}$$

$$\text{s.t. } y^{\mathrm{f}} \in \arg\max_{y \in \mathbb{N}_0} u(y) = \sum_{k=0}^{y} \big(\beta_k - \alpha(2\beta_k - v_{k;2} - cy)^2\big)\mathbb{P}\big(X(y; q) = k\big) - cy \tag{EC.6}$$

$$u(y^{\mathrm{f}}) \geq 0, \tag{EC.7}$$

and the following proposition characterizes the optimal incentive structures, scouting decisions, and the overall scouting efficiency.

Proposition EC.2. (*i*) *For* $\delta = 0$, $\lim_{\alpha\to\infty} \beta_k^{\mathrm{sf}} = v_{k;2}/2$ *for all* $k \in \mathbb{N}_0$, $\lim_{\alpha\to\infty} y^{\mathrm{sf}} = 0$, *and* $\pi^{\mathrm{sf}} + u^{\mathrm{sf}} \leq \pi^{\mathrm{fb}}$.

(ii) For $\delta = 1$, $\lim_{\alpha\to\infty} \beta_k^{\text{tf}} = v_{k;2}/2 + cy^{\text{fb}}/2$ *for all* $k \in \mathbb{N}_0$, $\lim_{\alpha\to\infty} y^{\text{tf}} = y^{\text{fb}}$, *and* $\lim_{\alpha\to\infty} \pi^{\text{tf}} = \lim_{\alpha\to\infty} u^{\text{tf}} = \pi^{\text{fb}}/2$.

The main result of Proposition EC.2 confirms the key finding of Proposition 3: irrespective of whether the scout is (or is not) fairness-minded, a fairness-minded manager always offers simple, piecewise-linear incentive schemes to motivate the scout, and those incentives cause inefficient scouting outcomes. That said, there are also two novel insights that can be taken from the proposition. First, when the manager is only *superficially* fair (i.e., $\delta = 0$), then the proposed incentives induce the fairness-minded scout to cease any scouting efforts (i.e., $\lim_{\alpha\to\infty} y^{\text{sf}} = 0$). This happens because the scout strongly values a fair split of the search costs, whereas a superficially fair manager completely ignores those costs when designing incentives. This fundamental incentive conflict between manager and scout is so strong that it cannot be overcome, and as a result, the scouting process breaks down entirely.

Second, when both manager and scout care only about a truly fair profit distribution (i.e., $\alpha \to \infty$ and $\delta = 1$), then the manager can design incentives that induce first-best search efforts and that guarantee an equal split of first-best profits between manager and scout. In other words, when manager and scout are both perfectly fairness-minded, then there is no efficiency loss in the scouting process. However, we note that if $\alpha < \infty$, then this result does not survive, in general. That is, if only a partially fair manager or scout partakes in the scouting process, then there will be a loss of scouting efficiency due to the scout searching too few projects.

B. Mathematical Proofs

Proof of Proposition 1. Under first-best conditions, the manager can directly choose how many projects y to evaluate. The manager thus solves the following optimization problem to derive her first-best scouting policy:

$$\max_{y\in\mathbb{N}_0} \ \pi^{\text{fb}}(y) = \sum_{k=0}^{y} v_{k;2}\mathbb{P}\big(X(y;q) = k\big) - cy. \tag{EC.8}$$

(i) Assume that $q < c/v$. In this case, $\pi^{\text{fb}}(0) = 0$, and it is easy to verify that $\pi^{\text{fb}}(y) > \pi^{\text{fb}}(y+1)$ for all $y \in \mathbb{N}_0$. Hence, $y^{\text{fb}} = 0$ and $\pi^{\text{fb}} = 0$.

(ii) Assume that $q \geq c/v$. As a starting point, note that $\pi^{\text{fb}}(0) = 0$, $\pi^{\text{fb}}(1) = qv - c \geq 0$, and $\pi^{\text{fb}}(2) = 2(qv - c) \geq \pi^{\text{fb}}(1)$. It follows that $y^{\text{fb}} \geq 2$. Second, by comparing $\pi^{\text{fb}}(y) - \pi^{\text{fb}}(y-1)$ to $\pi^{\text{fb}}(y+1) - \pi^{\text{fb}}(y)$ for all $y \in \mathbb{N}$, it is straightforward to verify that $\pi^{\text{fb}}(y)$ exhibits strictly decreasing differences in y for all $y \geq 2$. Hence, the manager's first-best scouting policy y^{fb} is unique and satisfies the following conditions: (a) $\pi^{\text{fb}}(y^{\text{fb}}) - \pi^{\text{fb}}(y^{\text{fb}} - 1) \geq 0$, and (b) $\pi^{\text{fb}}(y^{\text{fb}} + 1) - \pi^{\text{fb}}(y^{\text{fb}}) < 0$. Rearranging and simplifying terms leads to the optimality condition presented in (4). From that condition, it follows readily that y^{fb} increases with v and decreases with c, which also directly implies that π^{fb} increases with v and decreases with c.

Denote $y^{\text{fb}}(q)$ the manager's optimal scouting policy as a function of q, with $q \in [c/v, 1)$. It is true that $\pi^{\text{fb}}(y^{\text{fb}}(q))$ is continuous and differentiable in q almost everywhere, and that the partial derivative of $\pi^{\text{fb}}(y, q)$ with respect to q exists for all $q \in (0,1)$. In particular, $\partial\pi^{\text{fb}}(y,q)/\partial q = vy(1-q)^{y-2}(1 + q(y-2)) > 0$ for any $y \geq 1$. It follows from the envelope theorems presented in Milgrom and Segal (2002, Theorem 1 and 2) and the fact that $y^{\text{fb}} \geq 2$ that $\pi^{\text{fb}}(y^{\text{fb}}(q))$ is increasing in q.

Furthermore, we have $y^{\text{fb}}(q=c/v)=2$ and $y^{\text{fb}}(q=1)=2$. Together with the fact that $y^{\text{fb}}(q)\geq 2$ for all q, it follows that there exist thresholds $\underline{q}$ and $\overline{q}$, with $c/v<\underline{q}\leq\overline{q}<1$, such that $y^{\text{fb}}(q)$ increases (resp. decreases) with q if $q<\underline{q}$ (resp. $q>\overline{q}$). In fact, we have $y^{\text{fb}}(q)=2$ for all $q\in(c/v,1)$ if and only if $q(1-q^2)<c/v$ for all $q\in(c/v,1)$. Otherwise, there exists $q\in(c/v,1)$ such that $y^{\text{fb}}(q)>2$. Finally, $\mathbb{E}[X(y^{\text{fb}}(q);q)]=qy^{\text{fb}}(q)$, and it is now obvious that this term increases with q if $q<\underline{q}$.

Proof of Proposition 2. (i) We prove the existence of an optimal incentive scheme β^* that incentivizes the scout to engage in first-best scouting behavior (i.e., $y^*=y^{\text{fb}}$) and that guarantees the manager first-best profits (i.e., $\pi^*=\pi^{\text{fb}}$) by showing that there always exists an incentive scheme such that $u(y^{\text{fb}})=0$ and $u(y^{\text{fb}})>u(y)$ for all $y\in\mathbb{N}_0\backslash\{y^{\text{fb}}\}$. First, the manager must set $\beta_0=0$; otherwise, we would have $u(0)=\beta_0>0$, and the manager could no longer obtain first-best profits as the scout could ensure positive payments by concealing all high-value projects. Next, we argue that, in optimum, $\beta_k\leq\beta_{k+1}$ for all $k\in\mathbb{N}_0$. Suppose to the contrary that there exists $k\in\mathbb{N}_0$, with $\beta_k>\beta_{k+1}$. Then, the scout would strictly prefer to reveal k high-value projects to the manager, even if $k+1$ high-value projects were discovered. However, by setting $\beta_k=\beta_{k+1}$, the manager does not increase bonus payments, but incentivizes the scout to reveal $k+1$ high-value projects, which is strictly better for the manager. As a result, we must have $\beta_k\leq\beta_{k+1}$ for all $k\in\mathbb{N}_0$.

We finish our proof with an inductive argument to establish the existence of an incentive scheme that induces first-best search and profits. First, assume that $y^{\text{fb}}=0$. It is trivial to see that the incentive scheme $\beta^*=0$ induces $y^*=y^{\text{fb}}=0$, which also leads to $\pi^*=\pi^{\text{fb}}=0$ and $u^*=0$. Second, assume that $y^{\text{fb}}\geq 2$ (note that by Proposition 1, y^{fb} is never equal to 1). Initially, set $\beta_1\geq 0$ such that $u(1)<0$. Then, for each $k\in\{2,\dots,y^{\text{fb}}-1\}$, fix all contract parameters β_l, with $l<k$, and choose β_k such that $u(k)<0$. Note that this is always possible because, for all $l<k$, $u(l)$ does not depend on β_k. Next, for $k=y^{\text{fb}}$, fix all β_l, with $l<y^{\text{fb}}$, and set $\beta_{y^{\text{fb}}}$ such that $u(y^{\text{fb}})=0$, which is again feasible by our previous argument. Finally, for any $k>y^{\text{fb}}$, apply the same methodology as before to ensure that $u(k)<0$ for all $k>y^{\text{fb}}$. An incentive scheme β^* that is obtained via this algorithm is guaranteed to induce $y^*=y^{\text{fb}}$ and $u^*=0$, which implies first-best profits for the manager; that is, $\pi^*=\pi^{\text{fb}}$, which concludes the proof.

(ii) This result is an immediate consequence of part (i) in conjunction with Proposition 1(i).

(iii) Assume that $q\geq c/v$. We now prove parts (a)–(c) one-by-one.

Part (a): We know from Proposition 1(ii) and part (i) that $y^*\geq 2$ and $u^*=0$. By optimality, it follows immediately that $u^*=u(y^*)=0\geq u(y=1)=\beta_1^*q-c$, which proves the claim.

Part (b): Assume that $q>c/v$. From the fact that $\beta_0^*=0$ and $\beta_k^*\leq\beta_{k+1}^*$ for all $k\in\mathbb{N}_0$, it follows that $0=u(y^*)\leq\beta_{y^*}^*\left(1-(1-q)^{y^*}\right)-cy^*$, which implies that $\beta_{y^*}^*\geq cy^*/\left(1-(1-q)^{y^*}\right)>cy^*/(1-(1-qy^*))=c/q$. Now, because $y^*\geq 2$ and $\beta_k^*\leq\beta_{k+1}^*$, there always exists a $\bar{k}$, with $2\leq\bar{k}\leq y^*$, such that $\beta_k^*\geq cy^*/\left(1-(1-q)^{y^*}\right)>c/q$ for all $k\geq\bar{k}$.

Part (c): Assume that $q=c/v$. Part (i) in combination with Proposition 1(ii) implies that $y^*=2$. Hence, any optimal incentive scheme must satisfy the following conditions: (a) $u(y=0)=0$, (b) $u(y=1)\leq u(y^*=2)=0$, and (c) $u(y)<u(y^*)$ for all $y>y^*=2$. It is easy to see that condition (a) is satisfied if and only if $\beta_0=0$. Furthermore, $u(y^*=2)=\beta_2q^2+2\beta_1q(1-q)-2c$ is increasing in $\beta_1\leq v$ and $\beta_2\leq 2v$, and hence, for $q=c/v$, $u(y^*=2)=0$ if and only if $\beta_1=v$ and $\beta_2=2v$. Under this incentive scheme, we also have

$u(y=1)=\beta_1 q-c=0$ for $q=c/v$, thereby satisfying condition (b). Finally, it is straightforward to verify that condition (c) is satisfied if $\beta_0=0$, $\beta_1=v$ and $\beta_k=2v$ for all $k\geq 2$, which concludes the proof.

Proof of Proposition 3. (i) Assume $\delta=1$. Initially, note that for any $\alpha\geq 0$, (5) and (6) imply that $\pi^{\mathrm{tf}}+u^{\mathrm{tf}}=\sum_{k=0}^{y^{\mathrm{tf}}}(v_{k;2}-\alpha(2\beta_k^{\mathrm{tf}}-v_{k;2}-cy^{\mathrm{tf}})^2)\mathbb{P}(X(y^{\mathrm{tf}};q)=k)-cy^{\mathrm{tf}}\leq\sum_{k=0}^{y^{\mathrm{tf}}}v_{k;2}\mathbb{P}(X(y^{\mathrm{tf}};q)=k)-cy^{\mathrm{tf}}\leq\pi^{\mathrm{fb}}$, with the first inequality being strict if $\alpha>0$. Moreover, by (5), it follows readily that that $\lim_{\alpha\to\infty}\beta_k^{\mathrm{tf}}=v_{k;2}/2+cy^{\mathrm{tf}}/2$ for all $k\in\mathbb{N}_0$. Now, for $\alpha\to\infty$, the scout chooses y^{tf} by solving the following optimization problem: $y^{\mathrm{tf}}\in\arg\max_{y\in\mathbb{N}_0}u(y;\alpha)=\sum_{k=0}^{y}(v_{k;2}/2)\mathbb{P}(X(y;q)=k)-cy+cy^{\mathrm{tf}}/2$. Comparing this optimization problem with the manager's first-best optimization problem as given in (EC.8) immediately reveals that $\lim_{\alpha\to\infty}y^{\mathrm{tf}}\leq y^{\mathrm{fb}}$.

(ii) Assume $\delta=0$. Initially, note that for any $\alpha\geq 0$, (5) and (6) imply that $\pi^{\mathrm{sf}}+u^{\mathrm{sf}}=\sum_{k=0}^{y^{\mathrm{sf}}}(v_{k;2}-\alpha(2\beta_k^{\mathrm{sf}}-v_{k;2})^2)\mathbb{P}(X(y^{\mathrm{sf}};q)=k)-cy^{\mathrm{sf}}\leq\sum_{k=0}^{y^{\mathrm{sf}}}v_{k;2}\mathbb{P}(X(y^{\mathrm{sf}};q)=k)-cy^{\mathrm{sf}}\leq\pi^{\mathrm{fb}}$, with the first inequality being strict if $\alpha>0$. Moreover, by (5), it follows readily that that $\lim_{\alpha\to\infty}\beta_k^{\mathrm{sf}}=v_{k;2}/2$ for all $k\in\mathbb{N}_0$. Now, for $\alpha\to\infty$, the scout chooses y^{sf} by solving the following optimization problem: $y^{\mathrm{sf}}\in\arg\max_{y\in\mathbb{N}_0}u(y;\alpha)=\sum_{k=0}^{y}(v_{k;2}/2)\mathbb{P}(X(y;q)=k)-cy$. Comparing this optimization problem with the manager's first-best optimization problem as given in (EC.8) immediately reveals that $\lim_{\alpha\to\infty}y^{\mathrm{sf}}\leq y^{\mathrm{fb}}$.

(iii) By the proof of part (i), for $\delta=1$ and $\alpha\to\infty$, $y^{\mathrm{tf}}\in\arg\max_{y\in\mathbb{N}_0}u(y;\alpha)=\sum_{k=0}^{y}(v_{k;2}/2)\mathbb{P}(X(y;q)=k)-cy+cy^{\mathrm{tf}}/2$. It is readily verifiable that for $q<2c/v$, $\lim_{\alpha\to\infty}u(0;\alpha)>\lim_{\alpha\to\infty}u(y;\alpha)$ for any $y>0$, which hence implies that $\lim_{\alpha\to\infty}y^{\mathrm{tf}}=0$ if $q<2c/v$. In contrast, for $q\geq 2c/v$, we have $0\leq\lim_{\alpha\to\infty}u(0;\alpha)\leq\lim_{\alpha\to\infty}u(1;\alpha)$, implying that $\lim_{\alpha\to\infty}y^{\mathrm{tf}}>0$.

Next, assume $\delta=0$. By the proof of part (ii), for $\alpha\to\infty$, $y^{\mathrm{sf}}\in\arg\max_{y\in\mathbb{N}_0}u(y;\alpha)=\sum_{k=0}^{y}(v_{k;2}/2)\mathbb{P}(X(y;q)=k)-cy$. It is again obvious that for $q<2c/v$, $\lim_{\alpha\to\infty}u(0;\alpha)>\lim_{\alpha\to\infty}u(y;\alpha)$ for any $y>0$, leading to $\lim_{\alpha\to\infty}y^{\mathrm{sf}}=0$ if $q<2c/v$. In contrast, for $q\geq 2c/v$, we have $0\leq\lim_{\alpha\to\infty}u(0;\alpha)\leq\lim_{\alpha\to\infty}u(1;\alpha)$, implying that $\lim_{\alpha\to\infty}y^{\mathrm{sf}}>0$.

Proof of Proposition 4. (i) For any δ and $\alpha\geq 0$, upon receiving $k\in\mathbb{N}_0$ high-value projects from the scout, the manager chooses $b_k^{\mathrm{xp}}=\arg\max_{b_k\geq 0}\pi^{\mathrm{xp}}(b_k;\alpha,\delta,k)=v_{k;2}-b_k-\alpha(2b_k-v_{k;2}-\delta cy^{\mathrm{xp}})^2$. Since $\pi^{\mathrm{xp}}(b_k;\alpha,\delta,k)$ is continuously differentiable and concave in b_k, straightforward differentiation reveals that $b_k^{\mathrm{xp}}=[v_{k;2}/2+\delta cy^{\mathrm{xp}}/2-1/(8\alpha)]^+$.

(ii) First, suppose that $\delta=1$. In this case, $\lim_{\alpha\to\infty}b_k^{\mathrm{xp}}=v_{k;2}/2+cy^{\mathrm{xp}}/2$, which implies, by Proposition 3(i) and the arguments made in the associated proof, that $\lim_{\alpha\to\infty}y^{\mathrm{xp}}=\lim_{\alpha\to\infty}y^{\mathrm{tf}}\leq y^{\mathrm{fb}}$. It also follows that, in equilibrium, $\lim_{\alpha\to\infty}b_k^{\mathrm{xp}}=\lim_{\alpha\to\infty}\beta_k^{\mathrm{tf}}$.

Second, suppose that $\delta=0$. In this case, $\lim_{\alpha\to\infty}b_k^{\mathrm{xp}}=v_{k;2}/2$, which implies, by Proposition 3(ii), that $\lim_{\alpha\to\infty}b_k^{\mathrm{xp}}=\lim_{\alpha\to\infty}\beta_k^{\mathrm{sf}}$. As a result, we also have $\lim_{\alpha\to\infty}y^{\mathrm{xp}}=\lim_{\alpha\to\infty}y^{\mathrm{sf}}\leq y^{\mathrm{fb}}$.

Proof of Proposition EC.1. Consider incentive schemes of the form $\beta_k=\lambda v_{k;2}$, with $\lambda\geq 0$. For $\lambda<c/(qv)$, the scout will never evaluate any projects and the manager will then receive zero profits. In contrast, by setting $\lambda=c/(qv)$, the manager can ensure strictly positive profits and also incentivize the scout to evaluate at least $y\geq 2$ projects. Moreover, for any $\lambda\geq c/(qv)$, when the scout finds it worthwhile to evaluate $y=1$ project, it is also always worthwhile to evaluate $y=2$ projects. It follows that, in optimum, $y^l\geq 2$ and $\lambda^l\geq c/(qv)$.

It is straightforward to verify that for $y \geq 2$, $u(y)$ exhibits decreasing differences; that is, $u(y) - u(y-1) \geq u(y+1) - u(y)$ for all $y \geq 2$. It follows immediately that the scout optimally chooses y^l such that $u(y^l) - u(y^l - 1) \geq 0$ and $u(y^l + 1) - u(y^l) < 0$. Hence, if the manager wants the scout to evaluate $y \geq 2$ projects, then the minimum share $\lambda[y]$ that achieves that goal satisfies $u(y) - u(y-1) = 0$, or equivalently, $\lambda[y] = c/(vq(1-q)^{y-2}(1+q(y-2)))$.

Inserting $\lambda[y]$ into the manager's profit function (1) reduces her optimization problem (1)–(3) to simply maximizing $\pi(y) = v(1-\lambda[y])(2-(1-q)^{y-1}(2+q(y-2)))$ over $y \geq 2$. For simplicity, assume for now that $y \in \mathbb{R}$. Then, $\pi(y)$ is strictly concave in y for all y such that $\pi(y) \geq 0$ because (a) $\lambda[y]$ is convex increasing in y and (b) $f(y) = 2-(1-q)^{y-1}(2+q(y-2))$ is concave increasing in y. It follows that $\pi(y)$ exhibits decreasing differences for all $y \geq 2$ that lead to positive expected profits for the manager, and the manager thus chooses y^l such that $\pi(y^l) - \pi(y^l - 1) \geq 0$ and $\pi(y^l+1) - \pi(y^l) < 0$. Now, noting that $\pi(y^l) - \pi(y^l - 1) = vq(1-q)^{y^l-2}(1+q(y^l-2)-\lambda[y^l]) - v(2-(1-q)^{y^l-2})(\lambda[y^l] - \lambda[y^l-1])$ and applying the optimality conditions leads to (EC.1). From the fact that condition (EC.1) is, in general, different from condition (4), it follows directly that $y^l \leq y^*$, $\pi^l \leq \pi^*$, and $u^l \geq u^*$.

Proof of Proposition EC.2. (i) Assume $\delta = 0$. Initially, note that for any $\alpha \geq 0$, (EC.5) and (EC.6) imply that $\pi^{\mathrm{sf}} + u^{\mathrm{sf}} \leq \sum_{k=0}^{y^{\mathrm{sf}}} v_{k;2}\mathbb{P}(X(y^{\mathrm{sf}};q)=k) - cy^{\mathrm{sf}} \leq \pi^{\mathrm{fb}}$, with the first inequality being strict if $\alpha > 0$. Moreover, by (EC.5), it follows readily that that $\lim_{\alpha\to\infty} \beta_k^{\mathrm{sf}} = v_{k;2}/2$ for all $k \in \mathbb{N}_0$. Now, for $\alpha \to \infty$, the scout chooses y^{sf} by solving the following optimization problem: $y^{\mathrm{sf}} \in \arg\max_{y\in\mathbb{N}_0} u(y;\alpha) = \sum_{k=0}^{y}(v_{k;2}/2 - \alpha(cy)^2)\mathbb{P}(X(y;q)=k) - cy$. As a result, $\lim_{\alpha\to\infty} y^{\mathrm{sf}} = 0$.

(ii) Suppose that the manager sets $\beta_k = v_{k;2}/2 + cy^{\mathrm{fb}}/2$ for all $k \in \mathbb{N}_0$. In that case, it follows from (EC.6) that $\lim_{\alpha\to\infty} y^{\mathrm{tf}} = y^{\mathrm{fb}}$. It is also easy to verify that the manager cannot offer an alternative incentive scheme that induces $\lim_{\alpha\to\infty} y^{\mathrm{tf}} = y^{\mathrm{fb}}$ but is less expensive for the manager. Hence, if it is optimal for the manager to induce a search of y^{fb}, then the proposed incentive scheme is optimal. Yet, this is obvious because for $\alpha \to \infty$, it is optimal for the manager to ensure that $\lim_{\alpha\to\infty} \pi^{\mathrm{tf}} = \lim_{\alpha\to\infty} u^{\mathrm{tf}}$. By Proposition 1 and 2, we know that for such a scenario, it must be true that $\lim_{\alpha\to\infty} \pi^{\mathrm{tf}} = \lim_{\alpha\to\infty} u^{\mathrm{tf}} \leq \pi^{\mathrm{fb}}/2$, and it is straightforward to verify that the proposed incentive scheme satisfies this condition with equality. Hence, $\lim_{\alpha\to\infty} \beta_k^{\mathrm{tf}} = v_{k;2}/2 + cy^{\mathrm{fb}}/2$, $\lim_{\alpha\to\infty} y^{\mathrm{tf}} = y^{\mathrm{fb}}$, and $\lim_{\alpha\to\infty} \pi^{\mathrm{tf}} = \lim_{\alpha\to\infty} u^{\mathrm{tf}} = \pi^{\mathrm{fb}}/2$.

C. Experimental Materials

In addition to easy-to-navigate on-screen instructions (implemented in oTree, and administered at the beginning of each session), participants had access to printed instructions that were available to them throughout the entire duration of the experiment. In the following, we present the printed instructions for all our studies (Section C.1-C.6). In Section C.7–C.9, we display, as a representative example, the on-screen instructions, comprehension quiz, and game screens for the *Incentive-High* treatment of Study 2.

C.1. Print Instructions: Study 1 (*Single*)

Task Overview. In this experiment, you are working for a company that needs to search for successful projects. You earn an income that depends on your decision and luck.

Decision Task. At the beginning of each period: you choose how many projects to search, and incur a cost of $1 for each project you search. Each project has a 50% [10%] probability of being successful. After you have chosen projects, and luck has determined the success of the chosen projects: you earn $15 for each successful project, for up to 2 projects.

Compensation. (a) Project earnings: you earn $15 for each successful project, for up to 2 projects; (b) search cost: you incur $1 for each project you search.

Final Earnings from the Experiment. You will play 20 periods of the game described above. At the end of the experiment, the computer will randomly select 4 periods. The money you walk away with will equal your average compensation in those 4 periods (each $1 laboratory dollar equals €0.375 [€2.375]), in addition to a fixed show-up fee of €7. Since any period could count, please make your decisions carefully.

C.2. Print Instructions: Study 2 (*Incentive* and *Discretionary*)

Task Overview. In this experiment, you are working for a company that needs to search for successful projects. Half of you will assume the role of the Manager, the other half will assume the role of the Employee. The Manager has delegated decision making to the Employee, earns an income that depends on the Employee's decision and luck, and pays the Employee a bonus.

The Employee: Decision Task. At the beginning of each period: the Employee chooses how many projects to search, and incurs a cost of $1 for each project they search. Each project has a 50% [10%] probability of being successful.

The Manager: Decision Task [Treatment: *Incentive*]. After the Employee chooses projects, and luck determines the success of the chosen projects: the Manager receives all successful projects from the Employee, and earns $15 for each successful project, for up to 2 projects. Before the Employee chooses projects, and luck determines the success of the chosen projects: the Manager designs a contract that determines bonus payments to the Employee.

The Manager: Decision Task [Treatment: *Discretionary*]. After the Employee has chosen projects, and luck has determined the success of the chosen projects: the Manager receives all successful projects from the Employee, and earns $15 for each successful project, for up to 2 projects. The Manager determines a bonus payment to the Employee.

The Manager: Compensation. (a) Project earnings: the Manager earns $15 for each successful project, for up to 2 projects; (b) bonus component: the Manager pays a bonus to the Employee.

The Employee: Compensation. (a) Bonus component: the Employee receives a bonus payment from the Manager; (b) search cost: the Employee incurs $1 for each project they search.

Your Role & Your Partners. You will be randomly assigned the role either of a Manager or of an Employee, and you will keep that role for the entire duration of the experiment. You will be informed at the beginning of the experiment which role you have been assigned. At the beginning of each period, one Manager and one Employee will be randomly matched. You will interact with a different participant in this experiment in each period and you will never get to know who you are playing with.

Final Earnings from the Experiment. You will play 20 periods of the game described above. At the end of the experiment, the computer will randomly select 4 periods. The money you walk away with will equal your average compensation in those 4 periods (each $1 laboratory dollar equals €0.75 [€4.75]), in addition to a fixed show-up fee of €7. Since any period could count, please make your decisions carefully.

C.3. Print Instructions: Study 3 (*Unobservable* and *Observable*)

Task Overview. In this experiment, you are working for a company that needs to search for successful projects. Half of you will assume the role of the Manager, the other half will assume the role of the Employee. The Manager has delegated decision making to the Employee, earns an income that depends on the Employee's decision and luck, and pays the Employee a bonus.

The Employee: Decision Task [Treatment: *Unobservable*]. At the beginning of each period: the Employee receives a non-binding search recommendation from the Manager, chooses how many projects to search (the Manager does not observe this), and incurs a cost of $1 for each project they search. Each project has a 50% [10%] probability of being successful.

The Employee: Decision Task [Treatment: *Observable*]. At the beginning of each period: the Employee receives a non-binding search recommendation from the Manager, chooses how many projects to search (the Manager can observe this), and incurs a cost of $1 for each project they search. Each project has a 50% [10%] probability of being successful.

The Manager: Decision Task [Treatment: *Unobservable*]. Before the Employee chooses projects: the Manager makes a search recommendation (the Employee does not have to follow this). After the Employee has chosen projects, and luck has determined the success of the chosen projects: the Manager receives all successful projects from the Employee, and earns $15 for each successful project, for up to 2 projects. The Manager determines a bonus payment to the Employee.

The Manager: Decision Task [Treatment: *Observable*]. Before the Employee chooses projects: the Manager makes a search recommendation (the Employee does not have to follow this). After the Employee has chosen projects, and luck has determined the success of the chosen projects: the Manager observes how many projects the Employee chose to search, receives all successful projects from the Employee, and earns $15 for each successful project, for up to 2 projects. The Manager determines a bonus payment to the Employee.

The Manager: Compensation. (a) Project earnings: the Manager earns $15 for each successful project, for up to 2 projects; (b) bonus component: the Manager pays a bonus to the Employee.

The Employee: Compensation. (a) Bonus component: the Employee receives a bonus payment from the Manager; (b) search cost: the Employee incurs $1 for each project they search.

Your Role & Your Partners. You will be randomly assigned the role either of a Manager or of an Employee, and you will keep that role for the entire duration of the experiment. You will be informed at the beginning of the experiment which role you have been assigned. At the beginning of each period, one Manager and one Employee will be randomly matched. You will interact with a different participant in this experiment in each period and you will never get to know who you are playing with.

Final Earnings from the Experiment. You will play 30 periods of the game described above. The money you walk away with will equal your average compensation across those 30 periods (each $1 laboratory dollar equals €1.50 [€9.50]), in addition to a fixed show-up fee of €7. Since every period counts, please make your decisions carefully.

C.4. Print Instructions: Study 4 (*Upfront* and *Repeated*)

Task Overview. In this experiment, you are working for a company that needs to search for successful projects. Half of you will assume the role of the Manager, the other half will assume the role of the Employee. The Manager has delegated decision making to the Employee, earns an income that depends on the Employee's decision and luck, and pays the Employee a bonus.

The Employee: Decision Task. At the beginning of each period: the Employee receives a fixed wage from the Manager, receives a non-binding search recommendation from the Manager, chooses how many projects to search (the Manager does not observe this), and incurs a cost of $1 for each project they search. Each project has a 50% [10%] probability of being successful.

The Manager: Decision Task. Before the Employee chooses projects: the Manager pays the employee a fixed wage, and makes a search recommendation (the employee does not have to follow this). After the Employee has chosen projects, and luck has determined the success of the chosen projects: the Manager receives all successful projects from the Employee, and earns $15 for each successful project, for up to 2 projects. The Manager determines an additional bonus payment to the Employee.

The Manager: Compensation. (a) Project earnings: the Manager earns $15 for each successful project, for up to 2 projects; (b) fixed wage: the Manager pays a fixed wage to the Employee; (c) bonus component: the Manager pays a bonus to the Employee.

The Employee: Compensation. (a) The Employee receives a fixed wage from the Manager; (b) bonus component: the Employee receives a bonus payment from the Manager; (b) search cost: the Employee incurs $1 for each project they search.

Your Role & Your Partners [Treatment: *Upfront*]. You will be randomly assigned the role either of a Manager or of an Employee, and you will keep that role for the entire duration of the experiment. You will be informed at the beginning of the experiment which role you have been assigned. At the beginning of each period, one Manager and one Employee will be randomly matched. You will interact with a different participant in this experiment in each period and you will never get to know who you are playing with.

Your Role & Your Partners [Treatment: *Repeated*]. You will be randomly assigned the role either of a Manager or of an Employee, and you will keep that role for the entire duration of the experiment. You will be informed at the beginning of the experiment which role you have been assigned. At the beginning of the experiment, one Manager and one Employee will be randomly matched. You will interact with the same participant throughout the entire experiment but you will never get to know who you are playing with.

Final Earnings from the Experiment. You will play 20 periods of the game described above. At the end of the experiment, the computer will randomly select 4 periods. The money you walk away with will equal your average compensation in those 4 periods (each $1 laboratory dollar equals €0.75 [€4.75]), in addition to a fixed show-up fee of €7. Since any period could count, please make your decisions carefully.

C.5. Print Instructions: Study 5 (*Incentive2*)

Task Overview. In this experiment, you are working for a company that needs to search for successful projects. Half of you will assume the role of the Manager, the other half will assume the role of the Employee. The

Manager has delegated decision making to the Employee, earns an income that depends on the Employee's decision and luck, and pays the Employee a bonus.

The Employee: Decision Task. The Employee chooses how many projects to search, and incurs a cost of \$1 for each project they search. Each project has a 10% probability of being successful.

The Manager: Decision Task. After the Employee chooses projects, and luck determines the success of the chosen projects: the Manager receives all successful projects from the Employee, and earns \$15 for each successful project, for up to 2 projects. Before the Employee chooses projects, and luck determines the success of the chosen projects: the Manager designs a contract that determines bonus payments to the Employee.

The Manager: Compensation. (a) Project earnings: the Manager earns \$15 for each successful project, for up to 2 projects; (b) bonus component: the Manager pays a bonus to the Employee.

The Employee: Compensation. (a) Bonus component: the Employee receives a bonus payment from the Manager; (b) search cost: the Employee incurs \$1 for each project they search.

Your Role & Your Partners. You will be randomly assigned the role either of a Manager or of an Employee, and you will keep that role for the entire duration of the experiment. You will be informed at the beginning of the experiment which role you have been assigned. At the beginning of each period, one Manager and one Employee will be randomly matched. You will interact with a different participant in this experiment in each period and you will never get to know who you are playing with.

Final Earnings from the Experiment. You will play 30 periods of the game described above. The money you walk away with will equal your average compensation across those 30 periods (each \$1 laboratory dollar equals €4.75), in addition to a fixed show-up fee of €7. Since every period counts, please make your decisions carefully.

C.6. Print Instructions: Study 5 (*Automated*)

Task Overview. In this experiment, you are working for a company that needs to search for successful projects. You will assume the role of the Employee, and the computer will assume the role of the Manager. The Computer-Manager has delegated decision making to the Employee, earns an income that depends on the Employee's decision and luck, and pays the Employee a bonus.

The Employee (You): Decision Task. The Employee chooses how many projects to search, and incurs a cost of \$1 for each project they search. Each project has a 10% probability of being successful.

The Computer-Manager: Decision Task. After the Employee chooses projects, and luck determines the success of the chosen projects: the Computer-Manager receives all successful projects from the Employee, and earns \$15 for each successful project, for up to 2 projects. Before the Employee chooses projects, and luck determines the success of the chosen projects: the Computer-Manager designs a contract that determines bonus payments to the Employee.

The Computer-Manager: Compensation. (a) Project earnings: the Computer-Manager earns \$15 for each successful project, for up to 2 projects; (b) bonus component: the Computer-Manager pays a bonus to the Employee.

The Employee (You): Compensation. (a) Bonus component: the Employee receives a bonus payment from the Computer-Manager; (b) search cost: the Employee incurs \$1 for each project they search.

Final Earnings from the Experiment. You will play 30 periods of the game described above. The money you walk away with will equal your average compensation across those 30 periods (each $1 laboratory dollar equals €4.75), in addition to a fixed show-up fee of €7. Since every period counts, please make your decisions carefully.

C.7. On-Screen Instructions: Study 2 (*Incentive-High*)

This section displays the full set of on-screen instructions that subjects in the *Incentive-High* treatment of Study 2 had to go through before the experiment started. These instructions are representative for all other treatments and studies that we implemented.

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The Manager:

- has delegated decision making to the employee,
- earns an income that depends on the employee's decision and luck,
- pays the employee a bonus.

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The Employee: Decision task

At the beginning of each period, the employee:

- needs to choose how many projects to search,
- incurs a cost of $1 for each project they search,
- sends successful projects to the manager (each project has a 50% probability of being successful).

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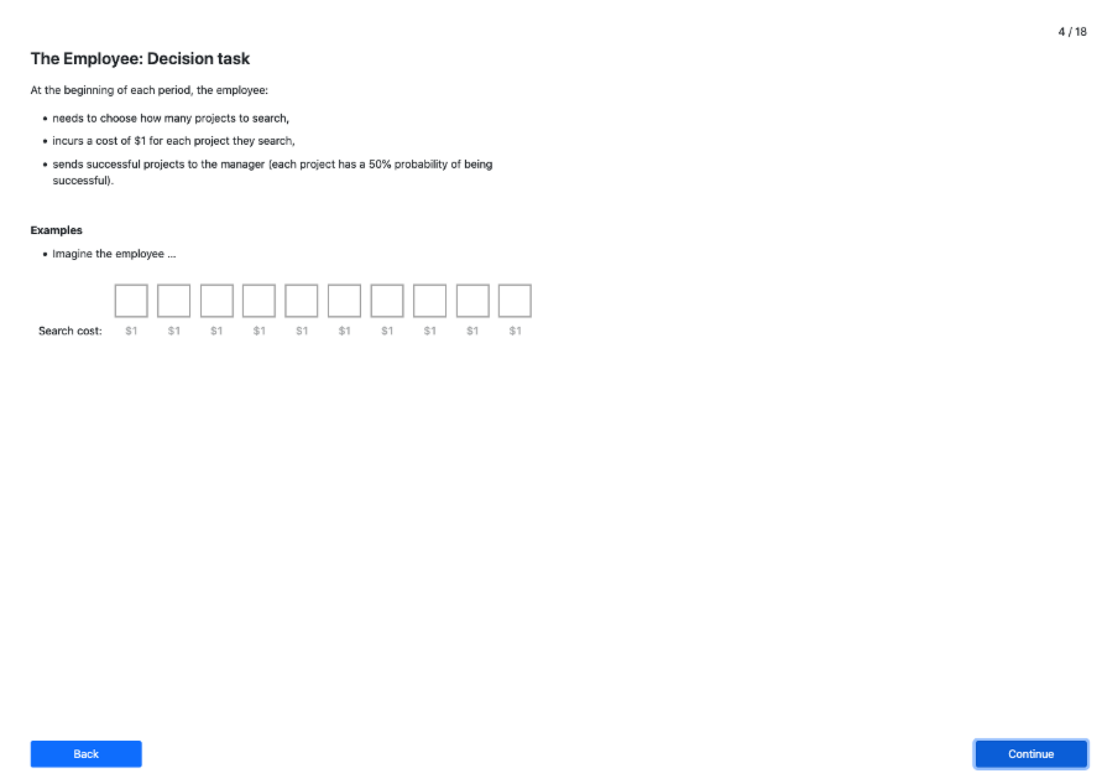

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The Employee: Decision task

At the beginning of each period, the employee:

- needs to choose how many projects to search,
- incurs a cost of $1 for each project they search,
- sends successful projects to the manager (each project has a 50% probability of being successful).

Examples

- Imagine the employee ...

Search cost: $1 $1 $1 $1 $1 $1 $1 $1 $1 $1

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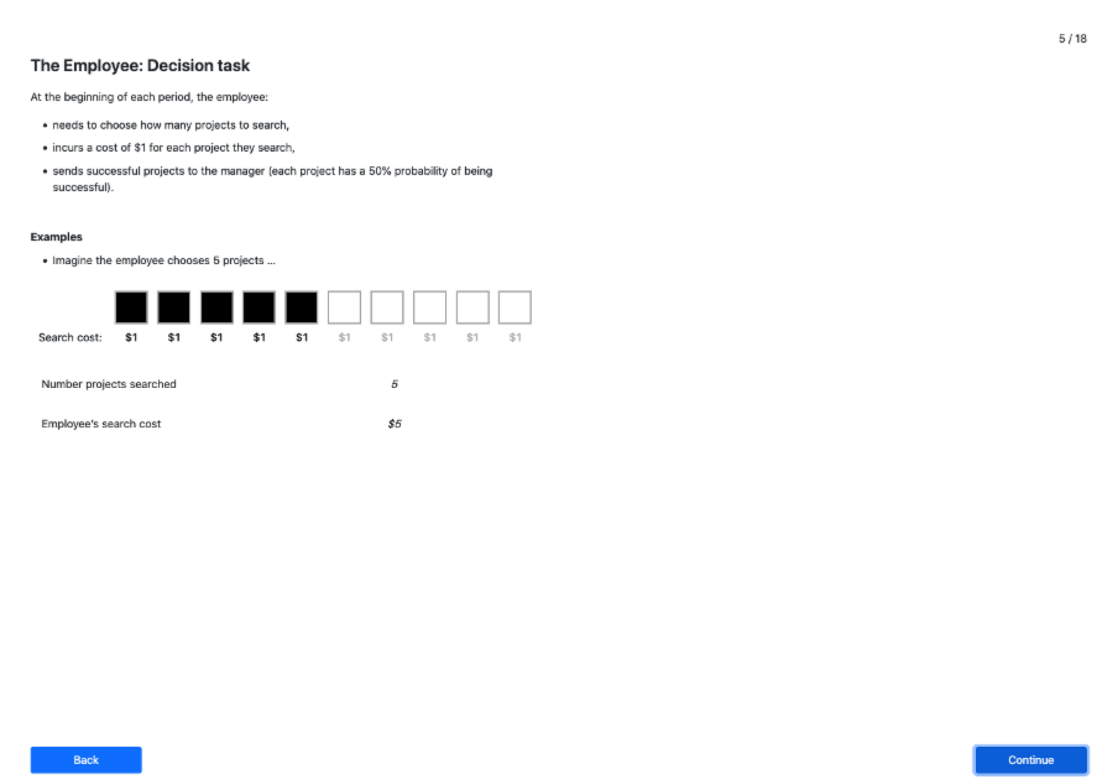

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The Employee: Decision task

At the beginning of each period, the employee:

- needs to choose how many projects to search,
- incurs a cost of $1 for each project they search,
- sends successful projects to the manager (each project has a 50% probability of being successful).

Examples

- Imagine the employee chooses 5 projects ...

Search cost: $1 $1 $1 $1 $1 $1 $1 $1 $1 $1

Number projects searched	*5*
Employee's search cost	*$5*

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The Employee: Decision task

At the beginning of each period, the employee:

- needs to choose how many projects to search,
- incurs a cost of $1 for each project they search,
- sends successful projects to the manager (each project has a 50% probability of being successful).

Examples

- Imagine the employee chooses 5 projects ...
- ... then between 0 and 5 projects can be successful.
- **Example 1:**

Search cost: **$1** **$1** **$1** **$1** **$1** $1 $1 $1 $1 $1

Number projects searched	*5*
Employee's search cost	*$5*
Number successful projects	*2*

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The Employee: Decision task

At the beginning of each period, the employee:

- needs to choose how many projects to search,
- incurs a cost of $1 for each project they search,
- sends successful projects to the manager (each project has a 50% probability of being successful).

Examples

- Imagine the employee chooses 5 projects ...
- ... then between 0 and 5 projects can be successful.
- **Example 2:**

Search cost: **$1** **$1** **$1** **$1** **$1** $1 $1 $1 $1 $1

Number projects searched	*5*
Employee's search cost	*$5*
Number successful projects	*0*

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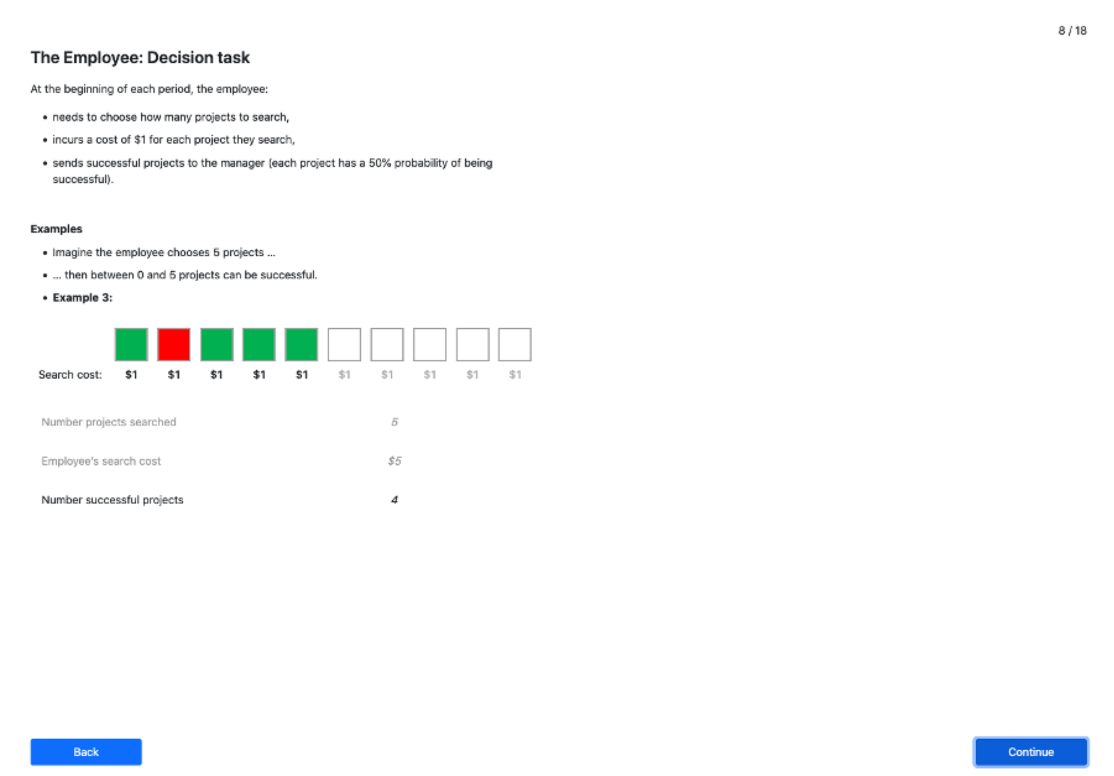
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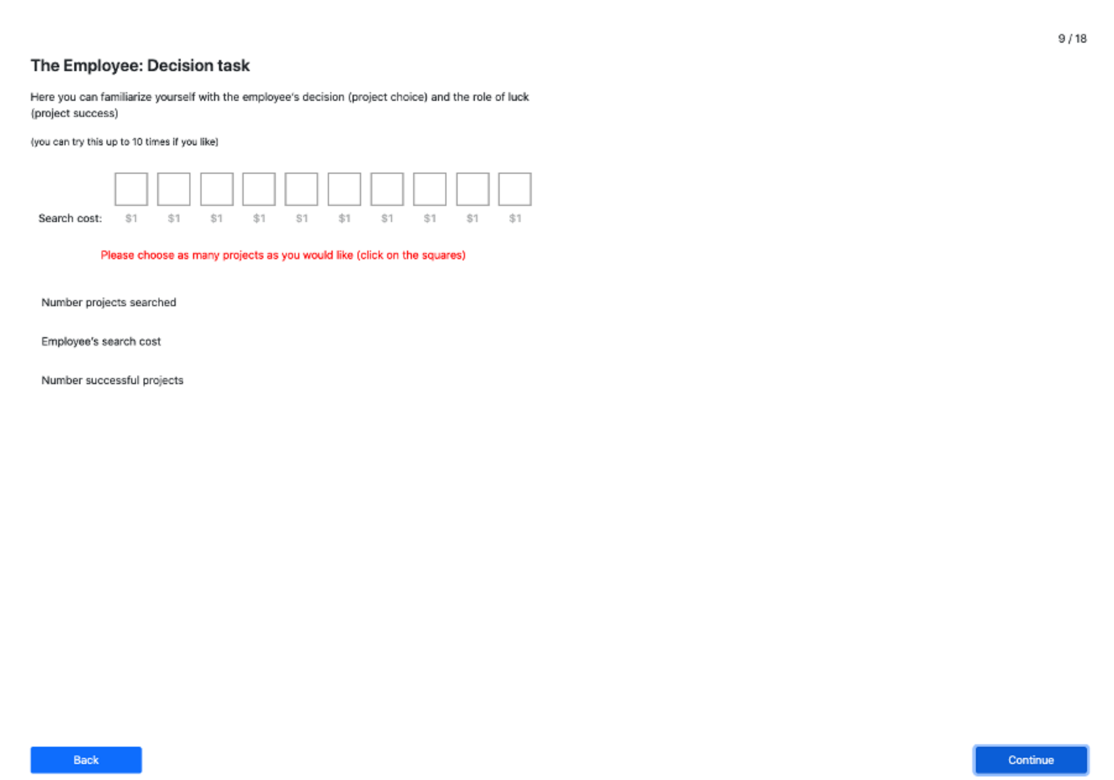
The Employee: Decision task
At the beginning of each period, the employee:
• needs to choose how many projects to search,
• incurs a cost of $1 for each project they search,
• sends successful projects to the manager (each project has a 50% probability of being successful).
Examples
• Imagine the employee chooses 5 projects ...
• ... then between 0 and 5 projects can be successful.
• Example 3:
Search cost: $1 $1 $1 $1 $1 $1 $1 $1 $1 $1
Number projects searched 5
Employee's search cost $5
Number successful projects 4
Back
Continue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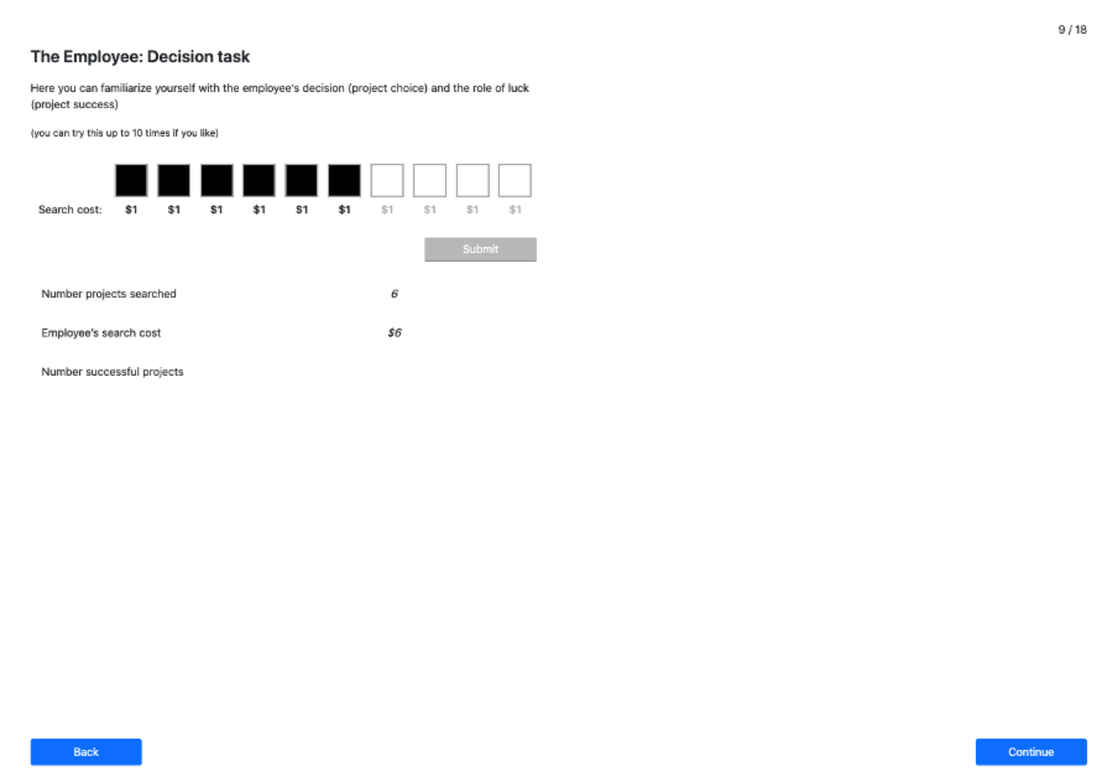
The Employee: Decision task
Here you can familiarize yourself with the employee's decision (project choice) and the role of luck (project success)
(you can try this up to 10 times if you like)
Search cost: $1 $1 $1 $1 $1 $1 $1 $1 $1 $1
Please choose as many projects as you would like (click on the squares)
Number projects searched
Employee's search cost
Number successful projects
Back
Continue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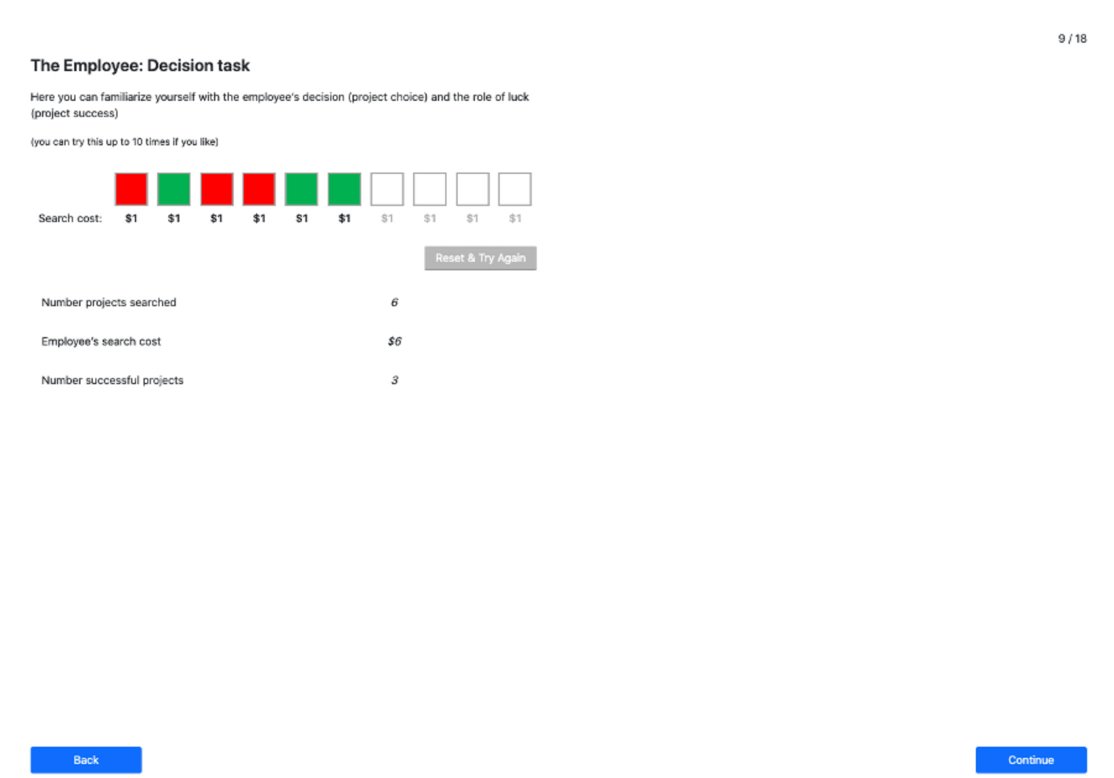
The Employee: Decision task
Here you can familiarize yourself with the employee's decision (project choice) and the role of luck (project success)
(you can try this up to 10 times if you like)
Search cost: $1 $1 $1 $1 $1 $1 $1 $1 $1 $1
Submit
Number projects searched 6
Employee's search cost $6
Number successful projects
Back
Continue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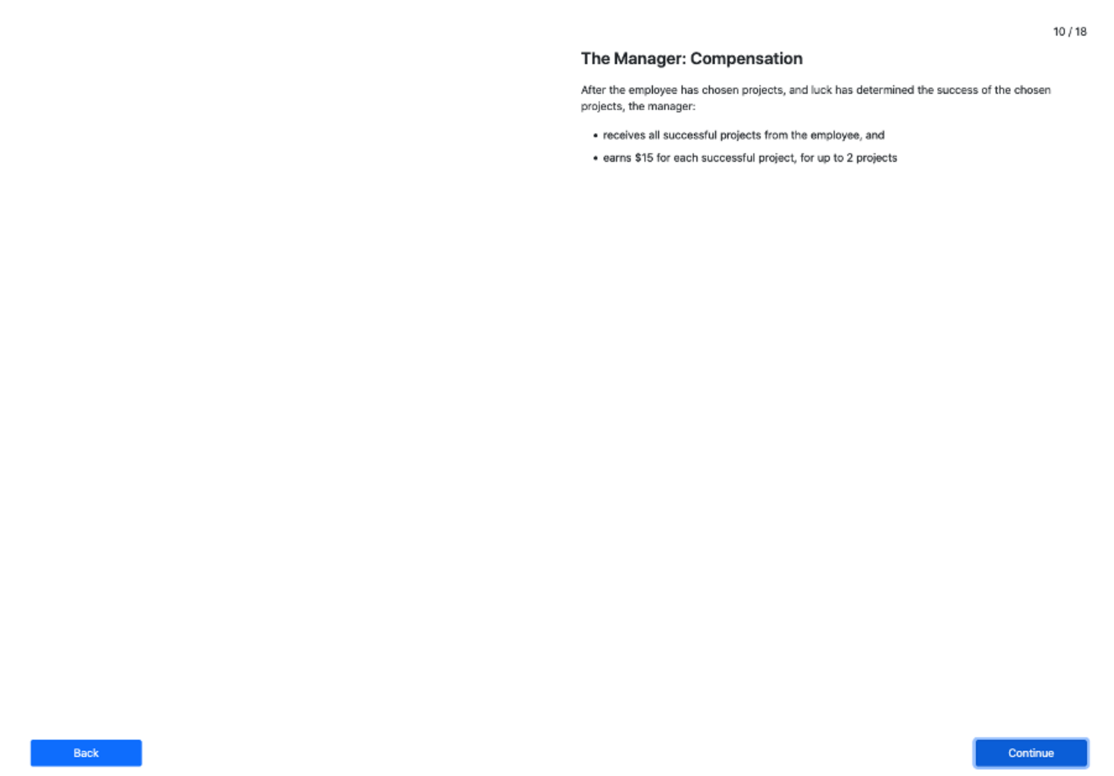
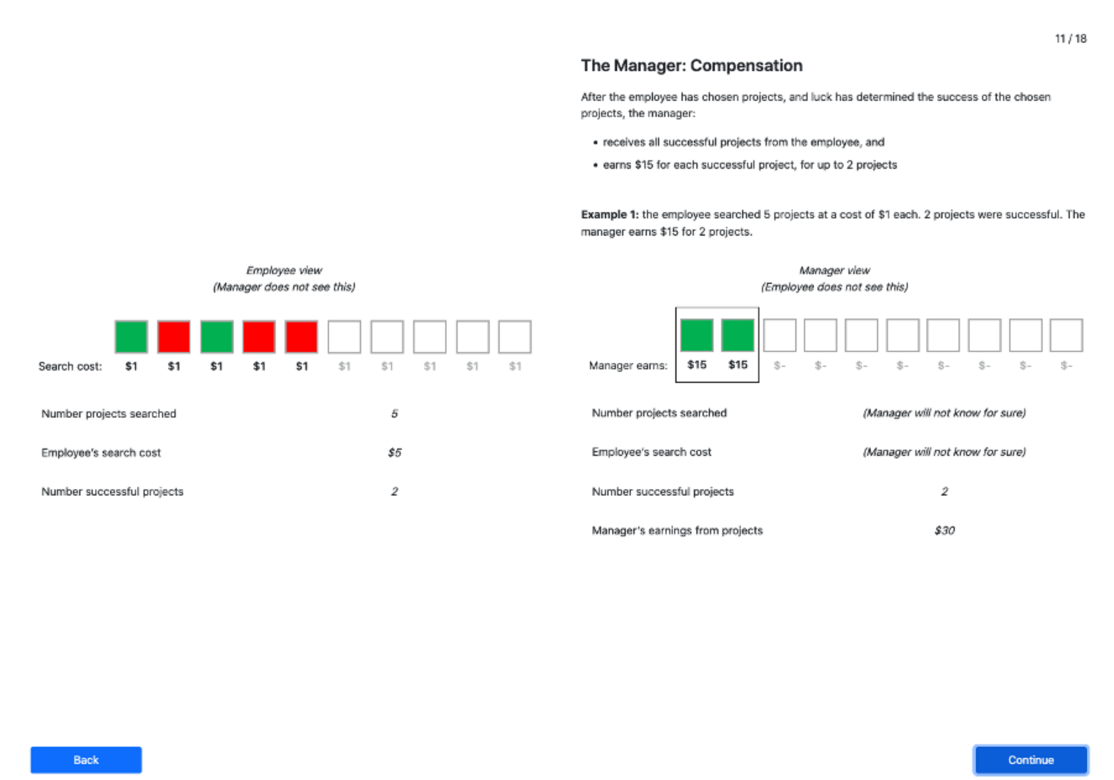
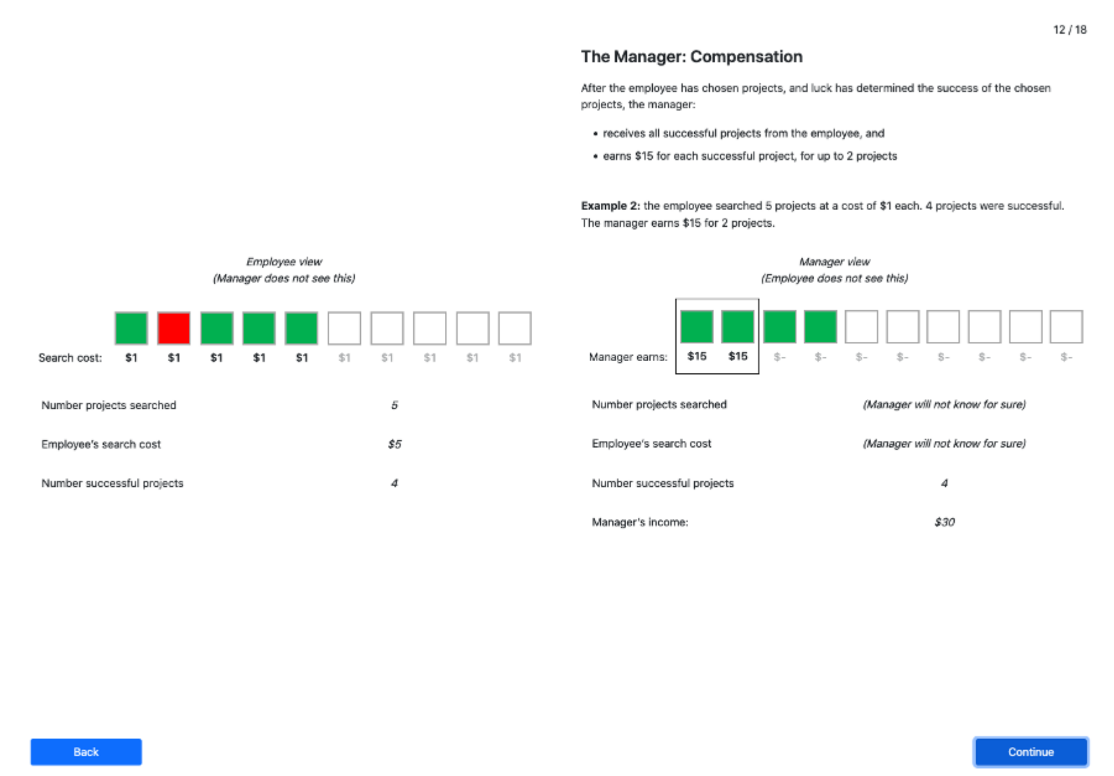
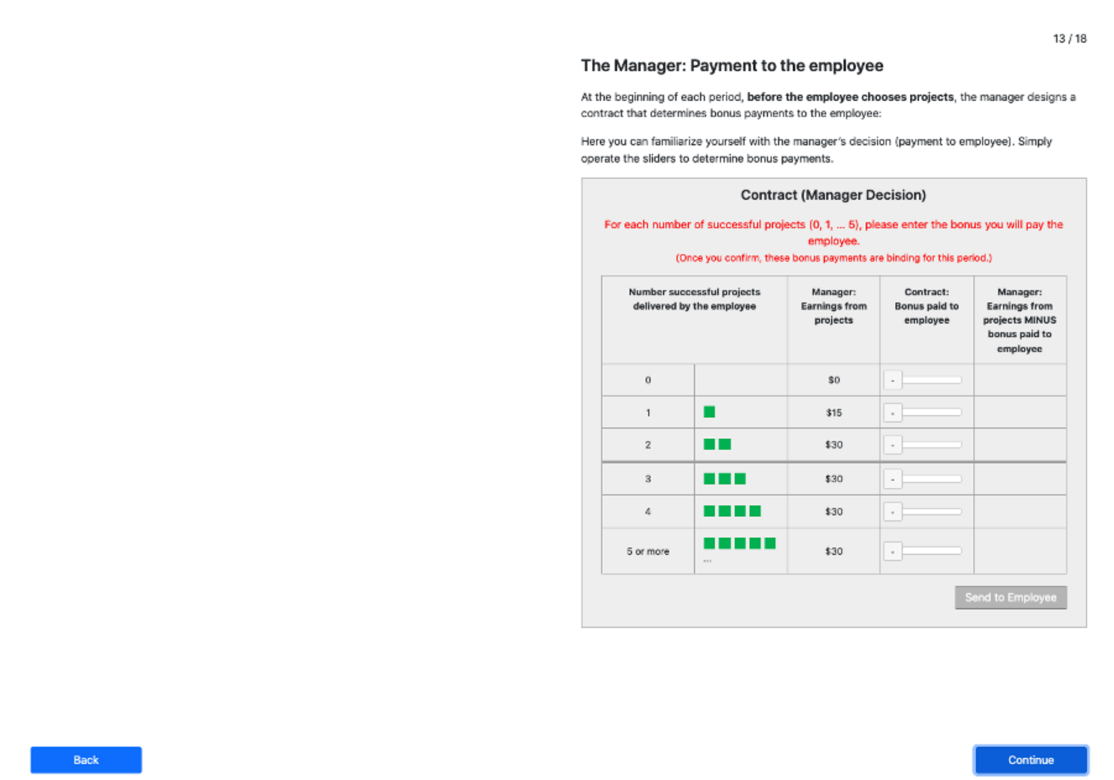
The Employee: Decision task
Here you can familiarize yourself with the employee's decision (project choice) and the role of luck (project success)
(you can try this up to 10 times if you like)
Search cost: $1 $1 $1 $1 $1 $1 $1 $1 $1 $1
Reset & Try Again
Number projects searched 6
Employee's search cost $6
Number successful projects 3
Back
Continue


10 / 18
The Manager: Compensation
After the employee has chosen projects, and luck has determined the success of the chosen projects, the manager:
• receives all successful projects from the employee, and
• earns $15 for each successful project, for up to 2 projects
Back
Continue


11 / 18
The Manager: Compensation
After the employee has chosen projects, and luck has determined the success of the chosen projects, the manager:
• receives all successful projects from the employee, and
• earns $15 for each successful project, for up to 2 projects
Example 1: the employee searched 5 projects at a cost of $1 each. 2 projects were successful. The manager earns $15 for 2 projects.
Employee view
(Manager does not see this)
Search cost: $1 $1 $1 $1 $1 $1 $1 $1 $1 $1
Number projects searched 5
Employee's search cost $5
Number successful projects 2
Manager view
(Employee does not see this)
Manager earns: $15 $15 $- $- $- $- $- $- $- $-
Number projects searched (Manager will not know for sure)
Employee's search cost (Manager will not know for sure)
Number successful projects 2
Manager's earnings from projects $30
Back
Continue


12 / 18
The Manager: Compensation
After the employee has chosen projects, and luck has determined the success of the chosen projects, the manager:
• receives all successful projects from the employee, and
• earns $15 for each successful project, for up to 2 projects
Example 2: the employee searched 5 projects at a cost of $1 each. 4 projects were successful. The manager earns $15 for 2 projects.
Employee view
(Manager does not see this)
Search cost: $1 $1 $1 $1 $1 $1 $1 $1 $1 $1
Number projects searched 5
Employee's search cost $5
Number successful projects 4
Manager view
(Employee does not see this)
Manager earns: $15 $15 $- $- $- $- $- $- $- $-
Number projects searched (Manager will not know for sure)
Employee's search cost (Manager will not know for sure)
Number successful projects 4
Manager's income: $30
Back
Continue


13 / 18
The Manager: Payment to the employee
At the beginning of each period, before the employee chooses projects, the manager designs a contract that determines bonus payments to the employee:
Here you can familiarize yourself with the manager's decision (payment to employee). Simply operate the sliders to determine bonus payments.
Contract (Manager Decision)
For each number of successful projects (0, 1, ... 5), please enter the bonus you will pay the employee.
(Once you confirm, these bonus payments are binding for this period.)
Number successful projects delivered by the employee
Manager: Earnings from projects
Contract: Bonus paid to employee
Manager: Earnings from projects MINUS bonus paid to employee
0 $0 -
1 $15 -
2 $30 -
3 $30 -
4 $30 -
5 or more $30 -
Send to Employee
Back
Continue

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The Manager: Payment to the employee

At the beginning of each period, **before the employee chooses projects**, the manager designs a contract that determines bonus payments to the employee:

Here you can familiarize yourself with the manager's decision (payment to employee). Simply operate the sliders to determine bonus payments.

Contract (Manager Decision)

For each number of successful projects (0, 1, ... 5), please enter the bonus you will pay the employee.

(Once you confirm, these bonus payments are binding for this period.)

Number successful projects delivered by the employee		Manager: Earnings from projects	Contract: Bonus paid to employee	Manager: Earnings from projects MINUS bonus paid to employee
0		$0	$10	-$10
1		$15	$23	-$8
2		$30	$12	$18
3		$30	$27	$3
4		$30	$15	$15
5 or more	...	$30	$26	$4

Send to Employee

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The Employee: Final income

The employee's final income in a period is

- the bonus payment from the manager
- MINUS the project search cost.

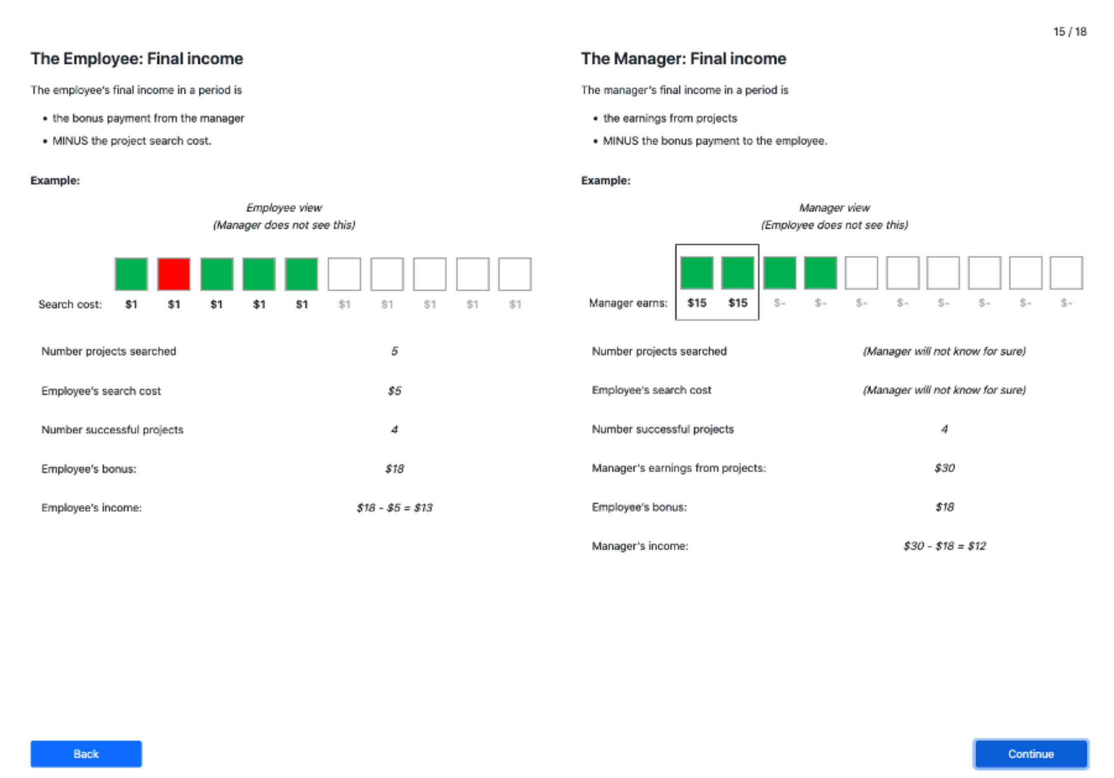
Example:

Employee view
(Manager does not see this)

Search cost:	$1	$1	$1	$1	$1	$1	$1	$1	$1	$1

Number projects searched	*5*
Employee's search cost	*$5*
Number successful projects	*4*
Employee's bonus:	*$18*
Employee's income:	*$18 - $5 = $13*

Back Continue



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The Employee: Final income

The employee's final income in a period is

- the bonus payment from the manager
- MINUS the project search cost.

Example:

Employee view
(Manager does not see this)

Search cost: $1 $1 $1 $1 $1 $1 $1 $1 $1 $1

Number projects searched	*5*
Employee's search cost	*$5*
Number successful projects	*4*
Employee's bonus:	*$18*
Employee's income:	*$18 - $5 = $13*

The Manager: Final income

The manager's final income in a period is

- the earnings from projects
- MINUS the bonus payment to the employee.

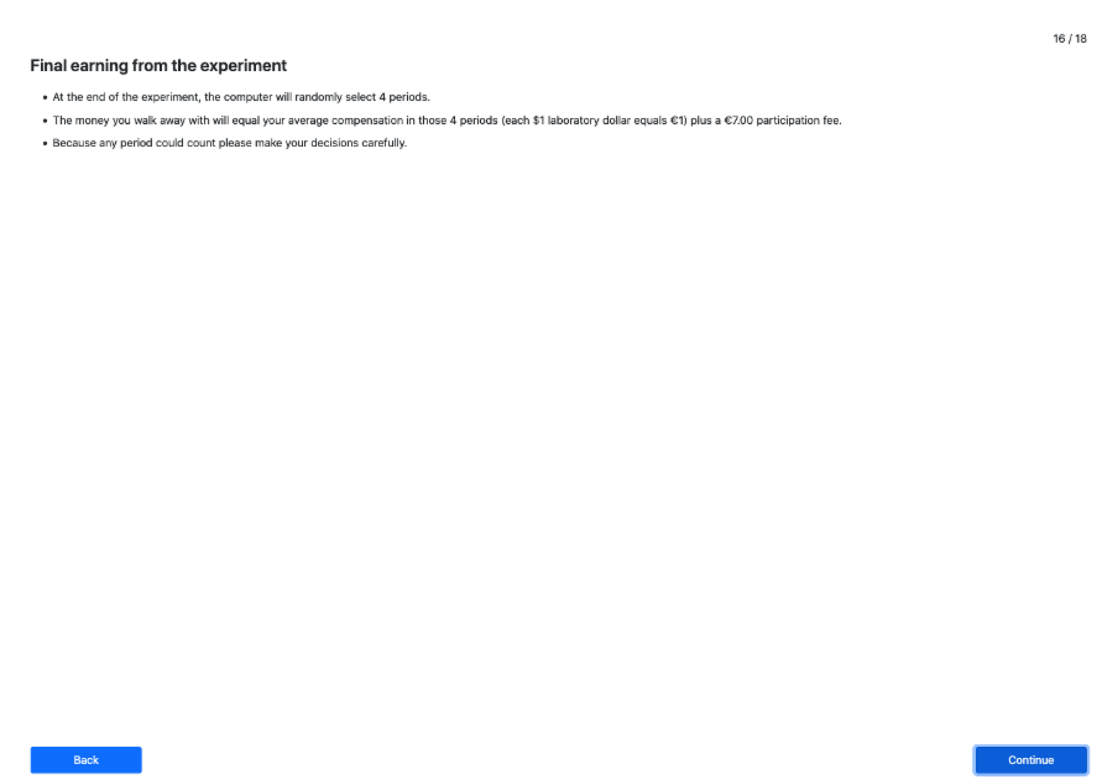
Example:

Manager view
(Employee does not see this)

Manager earns: $15 $15 $- $- $- $- $- $- $- $-

Number projects searched	*(Manager will not know for sure)*
Employee's search cost	*(Manager will not know for sure)*
Number successful projects	*4*
Manager's earnings from projects:	*$30*
Employee's bonus:	*$18*
Manager's income:	*$30 - $18 = $12*

Back Continue



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Final earning from the experiment

- At the end of the experiment, the computer will randomly select 4 periods.
- The money you walk away with will equal your average compensation in those 4 periods (each $1 laboratory dollar equals €1) plus a €7.00 participation fee.
- Because any period could count please make your decisions carefully.

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Your role

- For the entire duration of the experiment, you will assume the role either of a manager or of an employee.
- You will be informed at the beginning of the experiment which role you have been assigned.

Back

Continue

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Your group

- At the beginning of each period, one manager and one employee will be randomly matched.
- You will interact with different participants during the experiment and you will never get to know who you are playing with in a given period.

Back

Continue

C.8. Comprehension Quiz: Study 2 (*Incentive-High*)

In Study 2, after reading through the instructions, but before the actual experiment started, participants had to complete the following 8-questions comprehension quiz. All correct answers are marked.

Pre-experiment quiz

Are you going to play all periods of the experiment with the same partner?

- ( ) Yes, all periods with the same partner
- (•) No, each period with a different partner
- ( ) No, 10 periods each with the same partner

Who knows how many projects the employee searched?

- (•) Only the employee
- ( ) Only the manager
- ( ) Both

Who knows how many successful projects the employee found?

- ( ) Only the employee
- ( ) Only the manager
- (•) Both

Pre-experiment quiz

Imagine that the employee searches 7 projects. Which of the following statements is true?

- [ ] More than 7 projects could be successful
- [x] At most 7 projects could be successful
- [ ] At least 4 projects will be successful

Imagine that the employee sends 4 successful projects to the manager. Which of the following statements can then be true? (you can select multiple options)

- [x] The employee searched for exactly 4 projects, and all 4 of them were successful
- [x] The employee searched for more than 4 projects, but only 4 of them were successful
- [ ] The employee searched for 2 projects

The manager earns $15 for:

- [ ] every successful project that the employee delivers
- [x] every successful project that the employee delivers, but not more than 2 successful projects
- [ ] 2 successful projects

Pre-experiment quiz

Imagine that the manager had designed the following contract:

Contract (Manager Decision)

For each number of successful projects (0, 1, ... 5), please enter the bonus you will pay the employee.

(Once you confirm, these bonus payments are binding for this period.)

Number successful projects delivered by the employee		Manager: Earnings from projects	Contract: Bonus paid to employee	Manager: Earnings from projects MINUS bonus paid to employee
0		$0	$1	-$1
1		$15	$2	$13
2		$30	$3	$27
3		$30	$4	$26
4		$30	$5	$25
5 or more	...	$30	$6	$24

Send to Employee

Imagine that the employee chose 5 projects, and that 4 of these projects were successful. Based on the contract above, the manager pays the employee a total bonus of $5. What is the <u>employee's total income</u>?

- -5*$1 + $5 = $0
- $5
- -2*$1 + $5 = $3

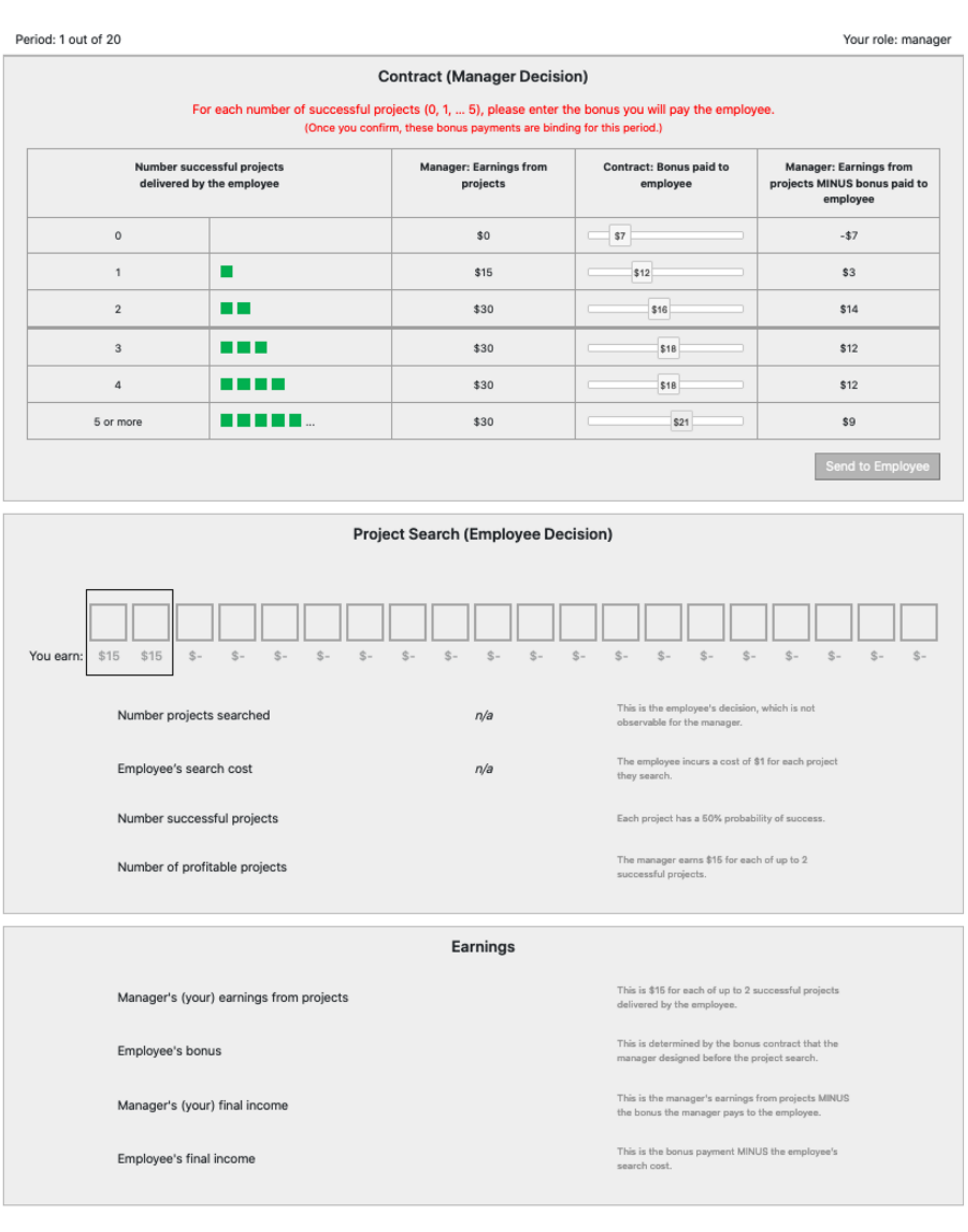
Imagine that the employee chose 5 projects, and that 4 of these projects were successful. Based on the contract above, the manager pays the employee a total bonus of $5. What is the <u>manager's total income</u>?

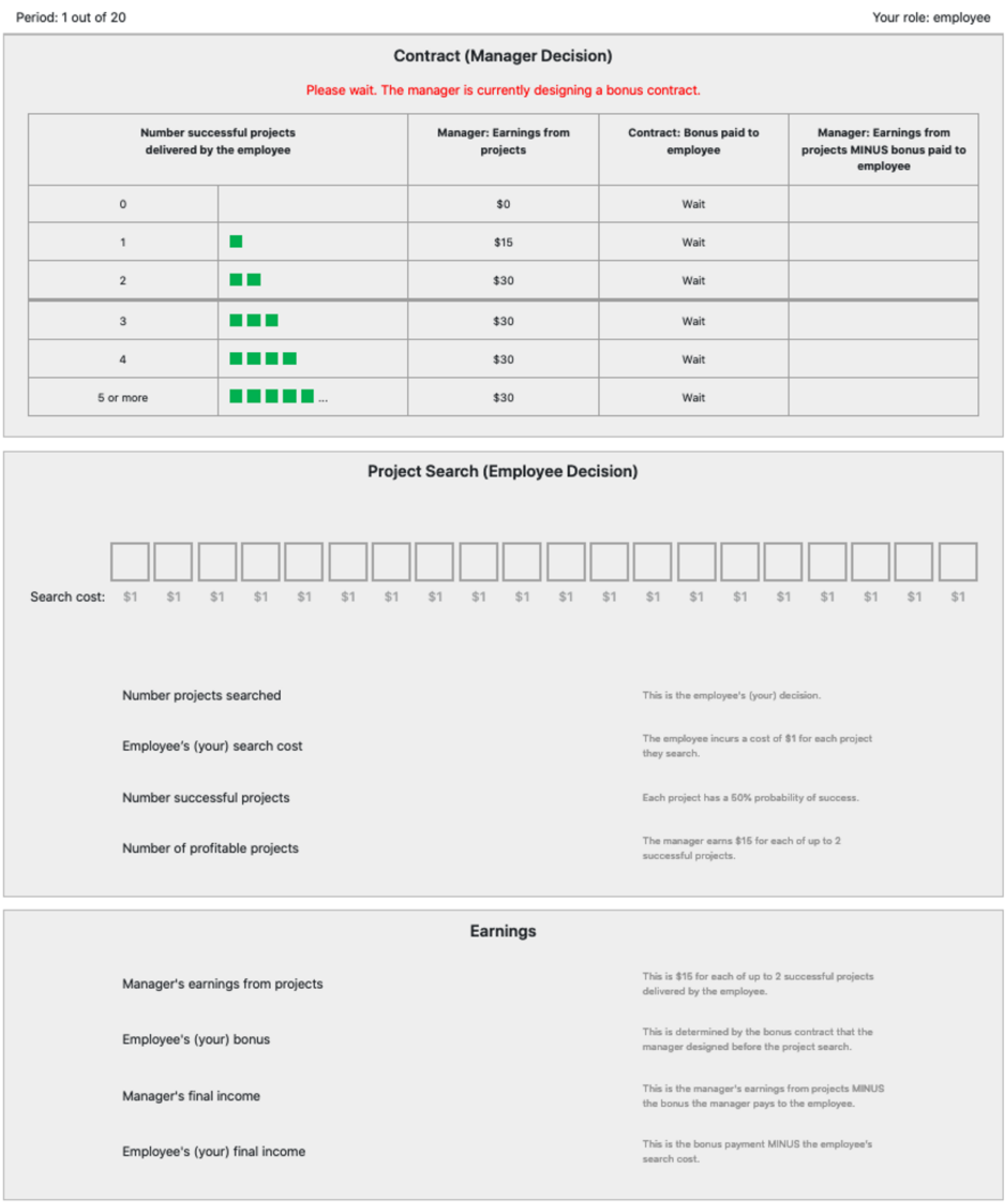
- 2 * $15 - $5 = $25
- 4 * $15 - $5 = $55
- 5 * $15 - $5 = $70

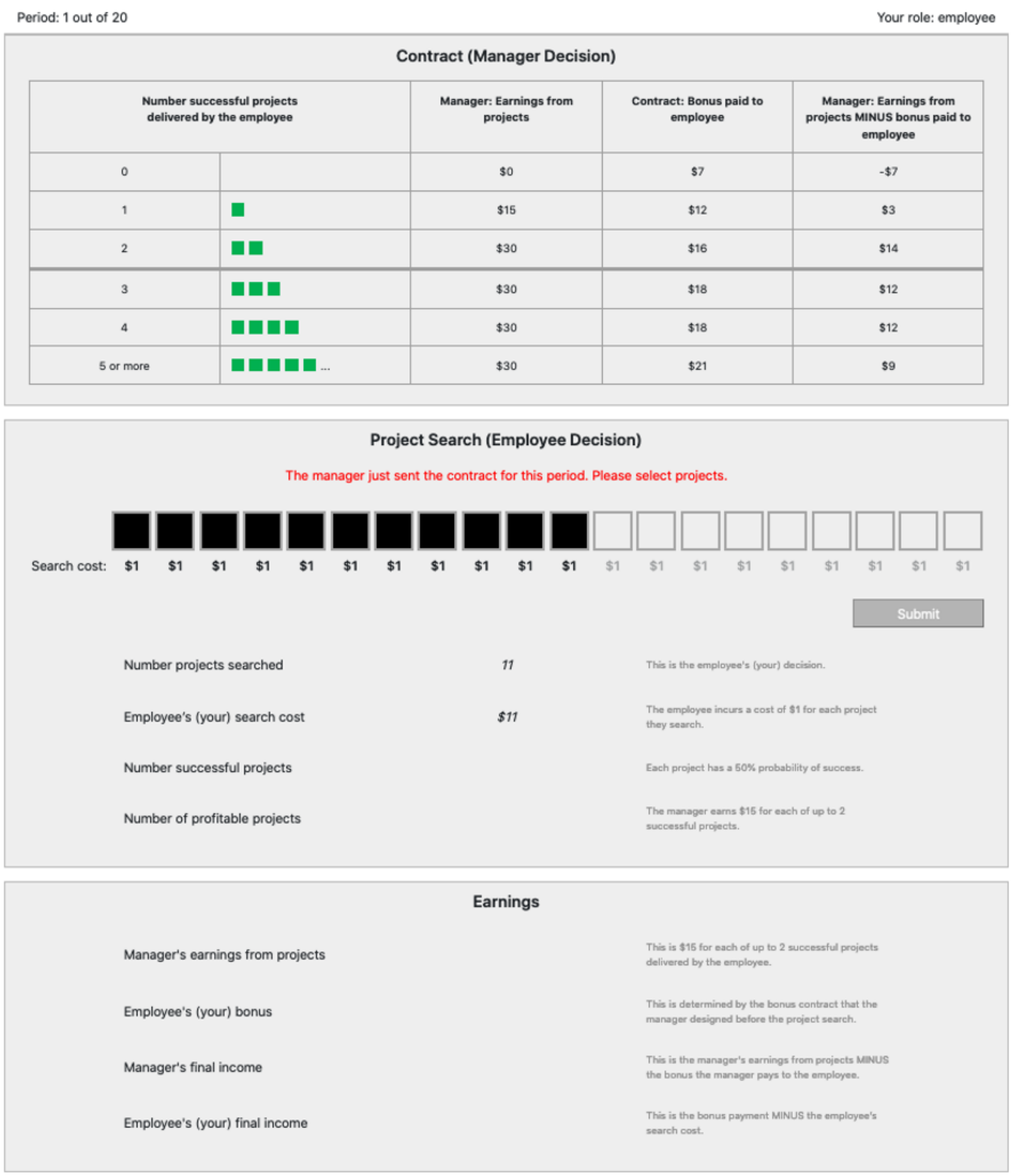
Continue

C.9. Game Screen: Study 2 (*Incentive-High*)

For easy reference, we next provide screenshots that display the evolution of the game screen during one round of the experiment in the *Incentive-High* treatment of Study 2.




Period: 1 out of 20
Your role: employee
Contract (Manager Decision)
Please wait. The manager is currently designing a bonus contract.
Number successful projects delivered by the employee
Manager: Earnings from projects
Contract: Bonus paid to employee
Manager: Earnings from projects MINUS bonus paid to employee
0
$0
Wait
1
$15
Wait
2
$30
Wait
3
$30
Wait
4
$30
Wait
5 or more
...
$30
Wait
Project Search (Employee Decision)
Search cost: $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1
Number projects searched
This is the employee's (your) decision.
Employee's (your) search cost
The employee incurs a cost of $1 for each project they search.
Number successful projects
Each project has a 50% probability of success.
Number of profitable projects
The manager earns $15 for each of up to 2 successful projects.
Earnings
Manager's earnings from projects
This is $15 for each of up to 2 successful projects delivered by the employee.
Employee's (your) bonus
This is determined by the bonus contract that the manager designed before the project search.
Manager's final income
This is the manager's earnings from projects MINUS the bonus the manager pays to the employee.
Employee's (your) final income
This is the bonus payment MINUS the employee's search cost.



Period: 1 out of 20

Your role: employee

Contract (Manager Decision)

Number successful projects delivered by the employee		Manager: Earnings from projects	Contract: Bonus paid to employee	Manager: Earnings from projects MINUS bonus paid to employee
0		$0	$7	-$7
1	■	$15	$12	$3
2	■■	$30	$16	$14
3	■■■	$30	$18	$12
4	■■■■	$30	$18	$12
5 or more	■■■■■ ...	$30	$21	$9

Project Search (Employee Decision)

The manager just sent the contract for this period. Please select projects.

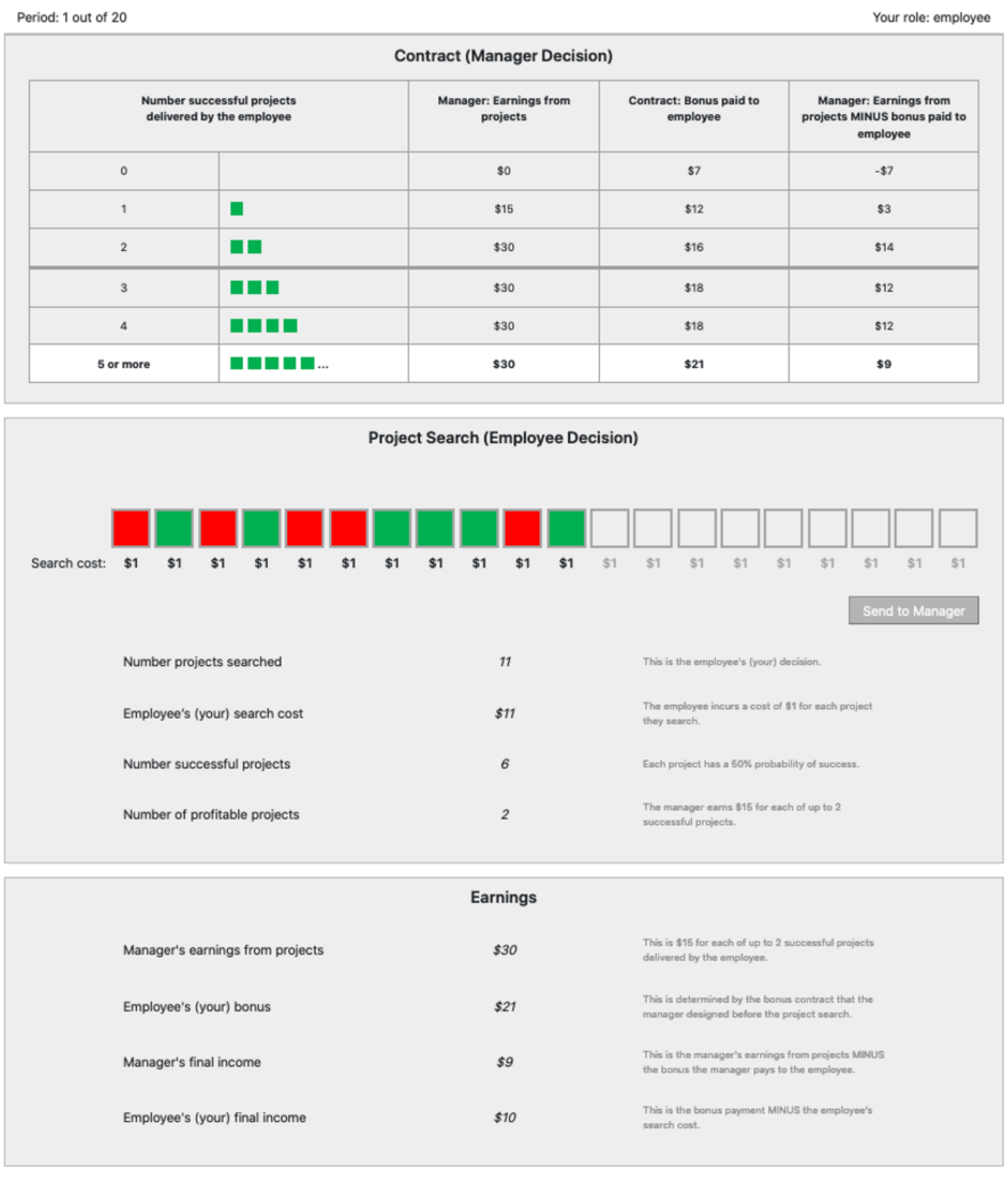
Search cost: $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1

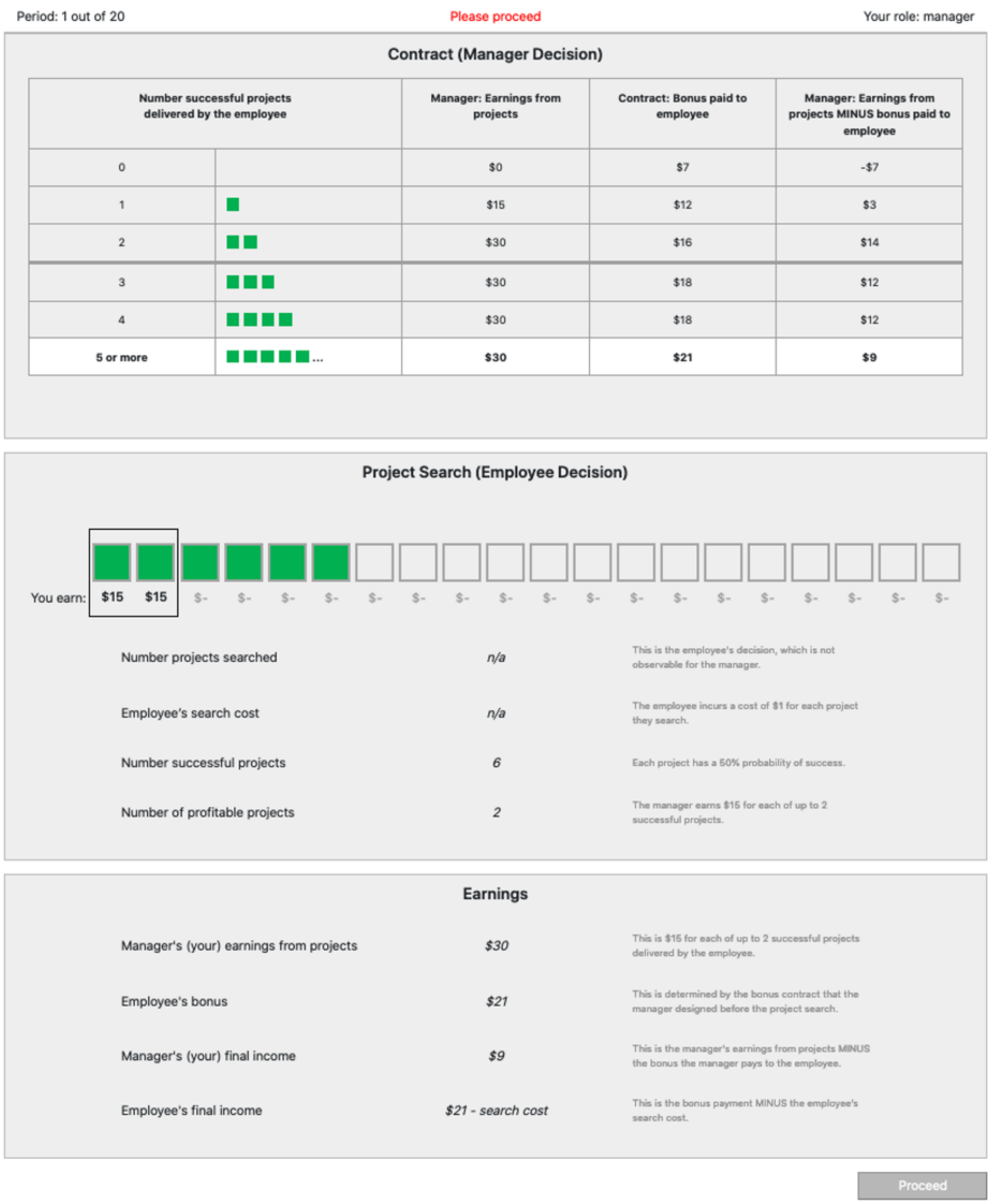
Submit

Number projects searched	*11*	This is the employee's (your) decision.
Employee's (your) search cost	*$11*	The employee incurs a cost of $1 for each project they search.
Number successful projects		Each project has a 50% probability of success.
Number of profitable projects		The manager earns $15 for each of up to 2 successful projects.

Earnings

Manager's earnings from projects	This is $15 for each of up to 2 successful projects delivered by the employee.
Employee's (your) bonus	This is determined by the bonus contract that the manager designed before the project search.
Manager's final income	This is the manager's earnings from projects MINUS the bonus the manager pays to the employee.
Employee's (your) final income	This is the bonus payment MINUS the employee's search cost.


Period: 1 out of 20
Your role: employee
Contract (Manager Decision)
Number successful projects delivered by the employee
Manager: Earnings from projects
Contract: Bonus paid to employee
Manager: Earnings from projects MINUS bonus paid to employee
0 $0 $7 -$7
1 $15 $12 $3
2 $30 $16 $14
3 $30 $18 $12
4 $30 $18 $12
5 or more ... $30 $21 $9
Project Search (Employee Decision)
Search cost: $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1 $1
Send to Manager
Number projects searched 11 This is the employee's (your) decision.
Employee's (your) search cost $11 The employee incurs a cost of $1 for each project they search.
Number successful projects 6 Each project has a 50% probability of success.
Number of profitable projects 2 The manager earns $15 for each of up to 2 successful projects.
Earnings
Manager's earnings from projects $30 This is $15 for each of up to 2 successful projects delivered by the employee.
Employee's (your) bonus $21 This is determined by the bonus contract that the manager designed before the project search.
Manager's final income $9 This is the manager's earnings from projects MINUS the bonus the manager pays to the employee.
Employee's (your) final income $10 This is the bonus payment MINUS the employee's search cost.


Period: 1 out of 20
Please proceed
Your role: manager
Contract (Manager Decision)
Number successful projects delivered by the employee
Manager: Earnings from projects
Contract: Bonus paid to employee
Manager: Earnings from projects MINUS bonus paid to employee
0 $0 $7 -$7
1 $15 $12 $3
2 $30 $16 $14
3 $30 $18 $12
4 $30 $18 $12
5 or more ... $30 $21 $9
Project Search (Employee Decision)
You earn:
$15
$15
$-
Number projects searched
n/a
This is the employee's decision, which is not observable for the manager.
Employee's search cost
n/a
The employee incurs a cost of $1 for each project they search.
Number successful projects
6
Each project has a 50% probability of success.
Number of profitable projects
2
The manager earns $15 for each of up to 2 successful projects.
Earnings
Manager's (your) earnings from projects
$30
This is $15 for each of up to 2 successful projects delivered by the employee.
Employee's bonus
$21
This is determined by the bonus contract that the manager designed before the project search.
Manager's (your) final income
$9
This is the manager's earnings from projects MINUS the bonus the manager pays to the employee.
Employee's final income
$21 - search cost
This is the bonus payment MINUS the employee's search cost.
Proceed

D. Additional Analyses

D.1. Study and Session Overview

Table EC.1 provides a detailed breakdown of all sessions across our five studies, including session sizes, minimum and maximum search efforts $y^{\min}$ and $y^{\max}$, and payment procedures. Participants were not told the session size. Cash earnings were computed at the end of the experiment by applying a predetermined exchange rate to the average payoff. Note that the delegated treatments of Study 3 were run over 30 rounds and thus consumed significantly more time than the 20-round experiments of Studies 2 and 4. To compensate participants for the prolonged experiment duration, we doubled the exchange rate in Study 3. A show-up fee of €7 was included in the earnings in all studies. The remainder of this section explains the elements of Table EC.1 in more detail: the action space (D.1.1), the payment procedure, conversion rates and resulting earnings (D.1.2–D.1.3), and the pre-registrations (D.1.4).

Table EC.1 Overview of Experimental Studies: Subjects, Session Sizes, and Earnings

Study	Treatment	q	Rounds	$y^{\min}$	$y^{\max}$	Payment	Conv. Rate	Avg. Earnings	Session Sizes
1	*Single*	*Low*	20	1	20	Avg(4)	2.375	€17.30	30
		High					0.375	€15.01	20/10
2	*Incentive*	*Low*	20	1	20	Avg(4)	4.75	€16.75	28/18/14
		High					0.75	€13.92	30/30
	Discretionary	*Low*	20	1	20		4.75	€17.47	24/24/10
		High					0.75	€13.69	28/22/8
3	*Observable*	*Low*	30	0	40	Avg(T)	9.50	€33.14	6/6/6/6/6/6/6/6
		High					1.50	€21.10	6/6/6/6/6/6/6/6
	Unobservable	*Low*	30	0	40		9.50	€26.42	6/6/6/6/6/6/6/6
		High					1.50	€20.26	6/6/6/6/6/6/6/6
4	*Upfront*	*Low*	20	1	20	Avg(4)	4.75	€17.24	30/14/12/8
		High					0.75	€12.74	34/18/14
	Repeated	*Low*	20	1	20		4.75	€16.66	30/20/10/6
		High					0.75	€13.80	32/14/8/6
5	*Automated*	*Low*	30	0	40	Avg(T)	4.75	€10.05	5/3/6/8/2/11
	Incentive2	*Low*	30	0	40		4.75	€15.71	12/6/6/6/6/6/6/6/6

D.1.1. Bounded Action Space. In all treatments, participant/scout i had to choose a search effort $y_{it} \in \{y^{\min}, \ldots, y^{\max}\}$ in each round t of the experiment. Here, the upper bound $y^{\max}$ corresponds to the number of projects displayed on the scout's decision screen (cf. Appendix C.9), and this upper bound exceeds the first-best search effort significantly. Nonetheless, we raised the upper bound from $y^{\max} = 20$ in Studies 1, 2, and 4 to $y^{\max} = 40$ in Studies 3 and 5 to verify that our empirical findings are not driven by a bounded action space. The data of Studies 3 and 5 clearly show that our choice of $y^{\max}$ has no effect on the observed scouting inefficiency.

Similarly, $y^{\min}$ denotes the minimum number of projects that scouts had to search in each treatment, with $y^{\min}$ being considerably below the first-best search effort. We reduced this lower bound from $y^{\min} = 1$ in Studies 1, 2, and 4 to $y^{\min} = 0$ in Studies 3 and 5 to analyze whether our initial assumption of non-zero search efforts had any bearing on our experimental results. Again, we find no evidence in that direction.

D.1.2. Payment Procedure. Participant payments in Studies 1, 2, and 4 were based on the average payoff over four randomly selected rounds (Avg(4)); in Studies 3 and 5, payment was based on the average payoff over all rounds (Avg(T)). We note that no payment procedure is incentive compatible without assumptions on how subjects evaluate bundles of, or gambles over, per-round payoffs (Azrieli et al. 2018). Paying based on only a single random round requires state-wise monotonicity, which guards against wealth and portfolio effects across rounds but can fail when feedback depends on subjects' own decisions or when ex ante fairness concerns are present. In contrast, paying based on all rounds instead requires the absence of complementarities across rounds and treats the sequence as a single decision problem, at the cost of admitting wealth effects. Clearly, paying based on a random subset of rounds lies between the two polar cases and requires weakened forms of both conditions. In addition, averaging over four rounds also limits the influence of a single unlucky draw, which matters in *Low*, where scouts incur search costs but rarely succeed (we discuss the handling of negative payoffs in Section D.1.3). Both employed payment procedures, Avg(4) and Avg(T), are common in the experimental literature, and our key results are qualitatively unchanged across them, suggesting that our results do not hinge on the payment procedure.

D.1.3. Payment Procedure: Handling Losses. The parallel search environment we study creates a payment challenge that is unusual in its severity and inseparable from our research question: scouting is costly and success is rare, so scouts in *Low* may accumulate negative payoffs across many rounds. Moreover, when bonuses are discretionary, scouts' realized payment depends on managers' ex post decisions, implying that even scouts who search at first-best levels may incur losses. Losses are thus a normal experience of scouts in our setting—a feature our experimental design inherits rather than induces.

Because expected payoffs are much lower in *Low*, both theoretically and because of the undersearch that we predict *and* observe, we equalized stakes across conditions through conversion rates rather than show-up fees (Table EC.1); as a result, average earnings are similar across conditions in Studies 1, 2, and 4. The same adjustment, however, also magnifies losses: with conversion rates roughly six times higher in *Low*, a given negative laboratory payoff becomes a six times larger monetary loss.

Requiring participants to bear such losses is neither permitted by the laboratories in which we ran our sessions nor standard experimental practice (Etchart-Vincent and L'Haridon 2011). The standard alternative is to absorb losses with a participation fee that exceeds the show-up fee (e.g., Rubin and Sheremeta 2016), which works when per-period payoffs are bounded such that a modest buffer suffices. Our setting admits no comparably modest buffer: a scout who searches $y^{\max}$ without ever being compensated loses many times the show-up fee, and considerably more in *Low* than in *High* given the higher conversion rate. A credible endowment would thus have to be large and vary with our main manipulation, introducing a windfall that is known to affect risk taking (Thaler and Johnson 1990, Cárdenas et al. 2014). Therefore, we instead set a participant's average payoff to zero when it was negative, so that no participant received less than the €7 show-up fee, as imposed by laboratory rules. The payment floor was applied at the end of the session and was not mentioned in the instructions, which simply stated that final earnings would equal the average payoff over the payment-relevant rounds plus the show-up fee. For affected participants, the final payment thus exceeded the amount implied by that rule. The deviation was always weakly in participants' favor, and no information disclosed during sessions would have allowed participants to infer the floor.

A participant who nonetheless anticipated the floor would face no marginal cost of search once sufficiently far below zero, and hence would be expected to search at $y^{\max}$. This concern, however, is inconsequential for two reasons. First, anticipating the floor can only raise search, whereas we predict and observe that scouts search too little. Second, the floor can be anticipated only where the payment base is trackable; that is, in Studies 3 and 5, which pay the average over all rounds; under the random-lottery procedure in Studies 1, 2, and 4, participants do not know which rounds will count. Yet, it is precisely in Studies 3 and 5 that we find no sign of such behavior: $y^{\max}$ was chosen in only 1% of search decisions in the *Low* condition of Studies 3 and 5, and the estimated round trends in search are negative in all four treatments of Study 3 (Table EC.9), precisely the opposite of what anticipation of the floor would imply.

D.1.4. Pre-registration. Prior to collecting data, we pre-registered the main research hypotheses, key dependent and independent variables, analysis plans, target sample sizes, and exclusion criteria for all studies. The full pre-registration documents are available at `https://aspredicted.org/4ZX_R83` (Study 1), `https://aspredicted.org/TQM_Y8R` (Study 2), `https://aspredicted.org/qbcc-9nhc.pdf` (Study 3), `https://aspredicted.org/8ZL_VS3` and `https://aspredicted.org/7tw7-mg6d.pdf` (Study 4), and `https://aspredicted.org/np8in7.pdf` (Study 5).

D.2. All Studies: Notes on Supporting Analyses

D.2.1. Expected Total Profit. In addition to the variables introduced in Section 4.4, we also define the *expected* total profit conditional on the observed search decision, $\Pi^*_{it} = \pi^{\text{fb}}(y_{it})$, which eliminates the impact of variability from random realizations of k_{it}, and thus allows for a clean comparison with theory predictions. We subsequently report the expected total profit for all our treatments, and note that by comparing this metric with the total profit, we can disentangle the efficiency loss caused by variability in decision-making from systematic decision bias (see Section D.8).

D.2.2. Statistical Analysis. Our data are nested because participants interact only within a fixed matching group. In most treatments, the group comprises all participants in a laboratory slot, among whom managers and scouts are randomly rematched each round. However, in Studies 3 and 5, groups of six are formed at the outset and rematching occurs only within them, and in the *Repeated* treatment of Study 4, the group is a single fixed manager–scout pair. Outcomes are independent across groups by construction, but not within: participants in the same group share a matching sequence, a common history of play, and any group-specific environmental or experimenter effects (Fréchette 2012). We therefore treat the matching group as the independent unit of observation and cluster all standard errors at that level, which is also the level at which treatment was assigned, in line with established practice in experimental operations (Leider and Lovejoy 2016, Fugger et al. 2019, Davis and Hyndman 2021, Davis et al. 2022).

All reported standard errors are cluster-robust, computed from the sandwich estimator with the finite-sample correction $S/(S-1)\cdot(N-1)/(N-K)$, where S is the number of matching groups entering the regression, N is the number of observations, and K is the number of estimated parameters including the intercept. The first factor adjusts for the finite number of clusters; the second for the degrees of freedom absorbed in estimating K parameters, and is negligible throughout, since N is large relative to K in all our specifications. Note that S varies across specifications: a regression pooled across a study draws on all of that study's matching groups, whereas one estimated within a single treatment draws only on that treatment's groups. Table EC.1 reports the composition of every group in each treatment, from which S follows for any specification. Tests of individual coefficients and of single linear contrasts are referred to a t-distribution with $S-1$ degrees of freedom.

We report two complementary types of tests, following standard convention in the literature. Between-treatment comparisons are estimated by regression, as in equation (8), with standard errors clustered at the matching-group level. Tests of a single treatment against a fixed theoretical benchmark—for example, observed total profit against the first-best level—are instead conducted by OLS on group-level means, equivalent to a t-test on the S group means with $S-1$ degrees of freedom. Note that (8) is built for between-treatment contrasts and hence offers no corresponding advantage here: the comparison is against a known constant rather than an estimated baseline, and the group mean is already the natural unit of observation.

The one exception to matching-group clustering arises in the contract regressions, where joint tests across all six outcome levels are computed under clustering at the (pair $\times$ round)-level; Appendix D.4.3 explains why.

D.3. Study 1: Supporting Analyses

Because *Single* is the only treatment in Study 1, we estimate a reduced form of the regression model presented in (8) on the pooled Study 1 data, including only a single treatment dummy for *High* (baseline: *Single-Low*) alongside the mean-centred round trend, to analyze the effect of our probability conditions on outcomes of interest (e.g., search or profit). Table EC.2 presents the results.

Table EC.2 Study 1: Aggregate Outcomes and Treatment Effect

	Search $\bar{y}$	Success $\bar{k}$	Total profit $\bar{\Pi}$	Exp. total profit $\bar{\Pi}^*(y)$
Intercept (*Single-Low*)	11.80	0.96	2.55	3.16
	(.00)	(.00)	(.00)	(.00)
High	−6.32	0.82	18.55	18.03
	(.00)	(.00)	(.00)	(.00)
$Round_c$	0.04	0.00	0.01	0.00
	(.14)	(.39)	(.83)	(.89)

Table EC.3 Study 2: Aggregate Outcomes and Treatment Effects

	Search $\bar{y}$	Success $\bar{k}$	Bonus $\bar{b}$	Scout utility $\bar{u}$	Manager profit $\bar{\pi}$	Total profit $\bar{\Pi}$	Exp. total profit $\bar{\Pi}^*(y)$
Intercept (*Incentive-Low*)	6.43	0.59	4.36	−2.08	3.76	1.68	2.24
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Discretionary	−0.35	−0.01	−0.94	−0.59	0.65	0.06	0.10
	(.34)	(.70)	(.09)	(.10)	(.01)	(.74)	(.52)
High	−1.82	1.68	3.22	5.04	10.61	15.65	15.22
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Discretionary × *High*	−0.16	−0.25	−1.20	−1.05	0.58	−0.47	−0.01
	(.83)	(.41)	(.55)	(.47)	(.14)	(.78)	(1.00)
$Round_c$	−0.01	−0.00	0.01	0.02	−0.02	0.00	−0.05
	(.81)	(.55)	(.67)	(.24)	(.69)	(.95)	(.24)

D.4. Study 2: Supporting Analyses

D.4.1. Study 2: Treatment effects. To analyze the effect of our treatments on outcomes of interest (e.g., search or profit), we estimate the regression model presented in (8) on the pooled data from Study 2, using treatment dummies for *Discretionary* and the *High* success probability condition (baseline: *Incentive-Low*). Table EC.3 presents the full statistical results, which we use as basis for the p-values in Table 2 in the main text.

D.4.2. Study 2: Learning. To assess whether outcomes change over the course of the experiment in Study 2, we estimate the following regression separately for each treatment and each dependent variable:

$$\bar{x}_{st} = \alpha_0 + \alpha_1 t_c + \varepsilon_{st}. \tag{EC.9}$$

Table EC.4 presents the results. Overall, we find no evidence that scouting efficiency improves over time. In particular, total profit changes only very mildly over rounds, but these changes are not statistically significant in any of the four treatments. Similar, scout utility and manager profits also stay the same across all rounds, implying that scouting inefficiency and unfair profit distributions persist over the course of the entire experiment.

D.4.3. Study 2: Contracts. To compare bonus payments between treatments or against a theoretical benchmark, we estimate the following regression model:

$$b_{it}(k) = \sum_{k=0}^{5} \alpha_k \mathbf{1}[k_{it} = k] + \sum_{k=0}^{5} \delta_k \left(D_s \mathbf{1}[k_{it} = k]\right) + \varepsilon_{it}(k), \tag{EC.10}$$

where the α_k coefficients identify the average bonus at each outcome level k in the baseline treatment ($D_s = 0$), and the δ_k coefficients capture the difference in bonus payments between comparison treatment ($D_s = 1$) and baseline treatment at the same search outcome k.

What the baseline represents determines what equation (EC.10) tests, and we use it in three ways. In between-treatment comparisons (e.g., between *Incentive* and *Discretionary*), the baseline is an observed

Table EC.4 Study 2: Round Trend $\hat{\alpha}_1$ by Treatment

	Search $\bar{y}$	Success $\bar{k}$	Bonus $\bar{b}$	Scout utility $\bar{u}$	Manager profit $\bar{\pi}$	Total profit $\bar{\Pi}$	Exp. total profit $\bar{\Pi}^*(y)$
Incentive-High	−0.02	0.01	−0.00	0.02	−0.04	−0.03	−0.04
	(.57)	(.39)	(.98)	(.79)	(.18)	(.57)	(.12)
Discretionary-High	−0.10	−0.03	−0.07	0.03	−0.12	−0.09	−0.17
	(.13)	(.13)	(.06)	(.39)	(.40)	(.42)	(.37)
Incentive-Low	0.15	0.01	0.13	−0.02	0.03	0.02	0.03
	(.04)	(.19)	(.01)	(.72)	(.67)	(.88)	(.15)
Discretionary-Low	−0.07	−0.00	−0.02	0.06	0.05	0.11	−0.02
	(.32)	(.93)	(.70)	(.15)	(.63)	(.32)	(.04)

treatment, so that both sets of coefficients are estimated from the data and the $\hat{\delta}_k$ measure the contract gap at each outcome level. In benchmark comparisons, the baseline is not observed but fixed at the known values of a theoretical benchmark—such as the superficially fair bonus $v_{k;2}/2$ or the optimal incentive contract $\beta^*(k)$—so that the $\hat{\delta}_k$ measure the departure of observed contracts from that benchmark. Finally, when a single treatment is estimated on its own ($D_s = 0$ throughout), the $\hat{\alpha}_k$ trace that treatment's average bonus schedule, whose shape we test against the piecewise-linear structure predicted by our theoretical fairness benchmarks (Proposition 3 and 4). All of these tests are linear contrasts of the $\hat{\alpha}_k$ coefficients. For a superficially fair contract (with $n = 2$ for the parameters of our study), for instance, we can test zero bonus at $k = 0$ ($H_0\colon \alpha_0 = 0$), a kink at $k = 2$ ($H_0\colon \alpha_1 - 2\alpha_2 + \alpha_3 = 0$), linearity of the initial slope ($H_0\colon \alpha_0 - 2\alpha_1 + \alpha_2 = 0$), flatness after the kink ($H_0\colon \alpha_k = \alpha_{k+1}$) for $\forall k \geq 2$, and equality with the superficially fair share at each outcome level ($H_0\colon \alpha_k = v_{k;2}/2$).

The dataset underlying equation (EC.10) is constructed as follows. Under *Discretionary* contracts, each (pair $i \times$ round t) contributes a single observation (k_{it}, b_{it}) at the realised outcome. Under *Incentive* contracts, each (pair $i \times$ round t) contributes six observations $\{(k, \beta_{it}(k))\}_{k=0}^{5}$, one for each outcome level in the posted contract schedule.[14] When both treatments are *Discretionary*, or when comparing against a theoretical benchmark, each (pair $i \times$ round t) contributes a single observation.

Standard errors are clustered at the session level, the level at which treatment was randomised, and all coefficient estimates and single-contrast tests reported below use this scheme. Joint tests across all six outcome levels—bonus equivalence ($H_0\colon \delta_k = 0\ \forall k$) and piecewise linearity—are the one exception: they impose more restrictions than there are sessions in a treatment, so the session-clustered variance matrix is rank-deficient and the Wald F-statistic is not computable. For these joint tests, we therefore cluster at the (pair $\times$ round)-level, which accounts for the within-contract correlation across the six k-rows contributed by each *Incentive* (pair $i \times$ round t) and leaves sufficient degrees of freedom for the test. Both schemes use the same small-sample corrected cluster-robust estimator described in Section D.2.

[14] Because *Incentive* contracts are specified only up to $k = 5$, the bonus at $k = 5$ applies to all outcomes $k \geq 5$.

Table EC.5 Study 2: Average Bonus Schedule $\hat{\alpha}_k$ by Treatment

	Low $(q=0.1)$		*High* $(q=0.5)$	
	Incentive	*Discretionary*	*Incentive*	*Discretionary*
$k=0$	0.37 (.04)	1.04 (.15)	0.46 (.20)	0.49 (.16)
$k=1$	6.24 (.00)	5.21 (.01)	3.64 (.04)	3.24 (.04)
$k=2$	12.72 (.00)	10.61 (.03)	8.00 (.01)	8.95 (.00)
$k=3$	13.32 (.00)	13.35 (.00)	9.71 (.01)	8.60 (.02)
$k=4$	13.95 (.00)	12.00 (.04)	10.41 (.01)	10.52 (.02)
$k\geq 5$	14.52 (.00)	11.00 (.04)	11.21 (.02)	11.59 (.00)

Average bonus schedules. Table EC.5 reports the $\hat{\alpha}_k$ estimates for each of the four treatments. For the *Incentive* treatments, each $\hat{\alpha}_k$ is the average contracted bonus at outcome level k across all 600 contracts (using $\beta_{it}(k)$ from the posted contract schedule, not the realized bonus payment). For the *Discretionary* treatments, $\hat{\alpha}_k$ is the average realized bonus among all (pair × round)-observations in which k projects were successful. At $k=0$, the average bonus is small throughout and indistinguishable from zero in three of the four treatments. At every outcome level $k\geq 1$, by contrast, $\hat{\alpha}_k$ is positive and statistically significant in all four treatments.

Contract anatomy tests. Table EC.6 reports the contract anatomy tests as linear contrasts of the $\hat{\alpha}_k$ coefficients. Contract linearity for $k\leq 2$ cannot be rejected in any treatment ($p\geq .05$), and contract linearity for $k>2$ likewise holds throughout ($p\geq .11$), indicating that the average bonus schedule is linear within each segment. The kink at $k=2$ is confirmed in three of four treatments (*Incentive-High*, *Discretionary-High*, and *Incentive-Low*, $p\leq .03$), but not for *Discretionary-Low* ($p=.41$), likely due to the sparsity of observations at $k\geq 3$ in that treatment. Contract flatness for $k>2$ is rejected everywhere ($p\leq .02$), confirming that the bonus schedule continues to rise across the second segment, albeit at a markedly reduced rate. The average bonus at $k=0$ is small and indistinguishable from zero in three treatments; the exception is *Incentive-Low* ($\hat{\alpha}_0=0.37$, $p=.04$), where the estimate is significant but economically negligible. Finally, observed bonuses never exceed $v_{k;2}/2$.

Between-treatment comparison: Discretionary vs. Incentive. Table EC.7 reports the $\hat{\delta}_k$ coefficients from the pooled version of equation (EC.10) with $D_s=1$ for *Discretionary.* In *High*, no individual gap is significant and the joint test is only marginally so ($F(6,1179)=2.14$, $p=.05$), providing broad support for contract equivalence between *Incentive* and *Discretionary* in the *High* probability condition. In *Low*, the joint test rejects equivalence ($F(6,1179)=5.93$, $p=.00$), but the rejection rests on a single outcome level: managers in *Discretionary* pay significantly *less* than *Incentive* contracts specify at $k=1$ ($\hat{\delta}_1=-1.04$, $p=.03$), whereas no other gap attains significance individually. The point estimates nonetheless describe a coherent pattern—more generous at $k=0$ ($\hat{\delta}_0=0.67$) and less generous at $k=1$ and $k=2$ ($\hat{\delta}_2=-2.10$)—consistent with managers in *Discretionary* partially insuring scouts against bad luck while rewarding successful outcomes less generously than *Incentive* contracts specify.

Table EC.6 Study 2: Contract Anatomy Tests

		Low $(q=0.1)$		*High* $(q=0.5)$	
		Incentive	*Discretionary*	*Incentive*	*Discretionary*
Anatomy for $k \leq 2$:					
H_0: $\alpha_0 - 2\alpha_1 + \alpha_2 = 0$	(linearity)	0.60 (.20)	1.23 (.51)	1.18 (.23)	2.97 (.05)
H_0: $\alpha_0 = 0$	(zero at $k=0$)	0.37 (.04)	1.04 (.15)	0.46 (.20)	0.49 (.16)
Slope change at $k=2$:					
H_0: $\alpha_1 - 2\alpha_2 + \alpha_3 = 0$	(kink at $k=2$)	−5.87 (.01)	−2.67 (.41)	−2.65 (.03)	−6.06 (.03)
Anatomy for $k > 2$:					
H_0: $\alpha_2 - 2\alpha_3 + \alpha_4 = 0$	(linearity)	0.03 (.28)	−4.09 (.26)	−1.02 (.17)	2.27 (.11)
H_0: $\alpha_3 - 2\alpha_4 + \alpha_5 = 0$	(linearity)	−0.06 (.77)	0.35 (.95)	0.11 (.48)	−0.86 (.72)
H_0: $\alpha_3 = \alpha_2$	(flatness)	12.95 (.00)	12.32 (.00)	9.25 (.02)	8.11 (.02)
Superficial fairness (*bonus vs.* $v_{k;2}/2$):					
H_0: $\alpha_1 = 7.5$	$(k=1)$	−1.26 (.01)	−2.29 (.02)	−3.86 (.04)	−4.26 (.03)
H_0: $\alpha_2 = 15$	$(k=2)$	−2.28 (.02)	−4.39 (.14)	−7.00 (.01)	−6.05 (.01)
H_0: $\alpha_3 = 15$	$(k=3)$	−1.68 (.05)	−1.65 (.16)	−5.29 (.02)	−6.40 (.04)
H_0: $\alpha_4 = 15$	$(k=4)$	−1.05 (.21)	−3.00 (.37)	−4.59 (.01)	−4.48 (.10)
H_0: $\alpha_5 = 15$	$(k \geq 5)$	−0.48 (.62)	−4.00 (.23)	−3.79 (.04)	−3.42 (.01)

Table EC.7 Study 2: Difference in Contract Anatomy (*Discretionary* vs. *Incentive*)

	Low $(q=0.1)$	*High* $(q=0.5)$
	$\hat{\delta}_k$	$\hat{\delta}_k$
$k=0$	0.67 (.16)	0.03 (.90)
$k=1$	−1.04 (.03)	−0.41 (.57)
$k=2$	−2.10 (.26)	0.94 (.14)
$k=3$	0.03 (.97)	−1.12 (.41)
$k=4$	−1.95 (.45)	0.11 (.94)
$k \geq 5$	−3.52 (.17)	0.38 (.32)
$F(6,1179)$	5.93 (.00)	2.14 (.05)

D.4.4. Study 2: Conditional Scouting. To assess whether scouts respond adequately to the incentives they receive, we fit to our *Incentive* data the linear regression model $y_{it} = a_0 + a_1 y^*(\beta_{it}) + \epsilon_{it}$, where $a_0 = 0$ and $a_1 = 1$ describes a fully calibrated (i.e., rational) scout. Standard errors are clustered at the session level. In *Low*, scouts are sensitive to the incentives they receive ($a_1 = 0.76$, $p < .01$) but insufficiently so ($a_1 < 1$, $p = .02$), and the intercept indicates systematic over-search ($a_0 = 5.21$, $p < .01$), which partially offsets the inadequate search incentives resulting from the under-powered contracts in our data. In *High*, the point estimates tell a qualitatively similar story ($a_0 = 2.74$, $a_1 = 0.41$), jointly implying that scouts over-search for contracts demanding a low $y^*(\beta_{it})$ and under-search for those requiring a high $y^*(\beta_{it})$.

D.5. Study 3: Supporting Analyses

D.5.1. Study 3: Treatment effects. To analyze the effect of our treatments on outcomes of interest (e.g., search or profit), we estimate the regression model presented in (8) on the pooled data from Study 3, using treatment dummies for *Observable* and the *High* success probability condition (baseline: *Unobservable-Low*). Table EC.8 presents the full statistical results, which we use as basis for the p-values in Table 3 in the main text.

Table EC.8 Study 3: Aggregate Outcomes and Treatment Effects

	Recomm. $\bar{r}$	Search $\bar{y}$	Success $\bar{k}$	Bonus $\bar{b}$	Scout utility $\bar{u}$	Manager profit $\bar{\pi}$	Total profit $\bar{\Pi}$	Exp. total profit $\bar{\Pi}^*(y)$
Intercept	11.79	6.35	0.70	4.77	−1.58	3.71	2.13	1.52
	(.00)	(.00)	(.00)	(.00)	(.07)	(.00)	(.00)	(.00)
Observable	0.98	1.89	0.17	1.21	−0.69	1.71	1.02	1.10
	(.45)	(.30)	(.39)	(.43)	(.46)	(.06)	(.01)	(.00)
High	−5.05	−2.42	1.29	2.85	5.27	10.26	15.52	16.09
	(.00)	(.12)	(.00)	(.02)	(.00)	(.00)	(.00)	(.00)
Observable × *High*	0.16	−1.19	0.20	−0.63	0.56	−0.66	−0.10	0.06
	(.94)	(.52)	(.43)	(.76)	(.72)	(.64)	(.92)	(.95)
$Round_c$	0.02	−0.06	−0.02	−0.07	−0.00	−0.09	−0.09	−0.08
	(.40)	(.00)	(.00)	(.00)	(.79)	(.00)	(.00)	(.00)

D.5.2. Study 3: Learning. To assess whether outcomes change over the course of the experiment within each treatment of Study 3, we estimate the regression (EC.9) separately for each treatment and each dependent variable. Table EC.9 presents the results. In all treatments, if anything, scouting efficiency declines over rounds! Importantly, search in *Observable* decreases more strongly than in *Unobservable*. As a result, the (modest) positive effects of *Observable* disappear as the experiment progresses. For instance, the small but significant effects of *Observable* on search (in *High*) and total profit (in *Low*) from Table EC.8 disappear when we run the regression on the second half of the data $t = 16$–30.

D.5.3. Study 3: Contracts. We apply equation (EC.10) to Study 3 using the same three types of tests as in Study 2: (i) estimation of the average bonus schedule $\hat{\alpha}_k$ separately for each treatment; (ii) contract anatomy tests based on linear contrasts of the $\hat{\alpha}_k$ coefficients; and (iii) a between-treatment comparison of *Observable* and *Unobservable* contracts via the $\hat{\delta}_k$ coefficients.

Average bonus schedules. Table EC.10 reports the $\hat{\alpha}_k$ estimates, reflecting the average realized bonus for different outcomes k. All estimates are positive and statistically significant, with the exception of $\hat{\alpha}_5$ in *Observable-Low*, owing to the sparsity of data for $k \geq 5$ in *Low*. We observe that bonus schedules in *Low* are uniformly higher than in *High*.

Table EC.9 Study 3: Round Trend $\hat{\alpha}_1$ by Treatment

	Recomm. $\bar{r}$	Search $\bar{y}$	Success $\bar{k}$	Bonus $\bar{b}$	Scout utility $\bar{u}$	Manager profit $\bar{\pi}$	Total profit $\bar{\Pi}$	Exp. total profit $\bar{\Pi}^*(y)$
Unobservable-High	-0.02	-0.03	-0.02	-0.08	-0.04	-0.09	-0.13	-0.12
	(.57)	(.13)	(.24)	(.03)	(.02)	(.28)	(.17)	(.07)
Observable-High	-0.04	-0.08	-0.05	-0.08	-0.00	-0.14	-0.14	-0.12
	(.01)	(.01)	(.01)	(.02)	(.98)	(.06)	(.07)	(.05)
Unobservable-Low	0.07	-0.05	-0.01	-0.06	-0.01	-0.07	-0.08	-0.04
	(.07)	(.37)	(.39)	(.07)	(.78)	(.08)	(.10)	(.07)
Observable-Low	0.06	-0.08	-0.01	-0.04	0.04	-0.07	-0.03	-0.03
	(.41)	(.07)	(.03)	(.29)	(.20)	(.06)	(.52)	(.02)

Table EC.10 Study 3: Average Bonus Schedule $\hat{\alpha}_k$ by Treatment

	Low $(q=0.1)$		*High* $(q=0.5)$	
	Unobservable	*Observable*	*Unobservable*	*Observable*
$k=0$	1.10 (.03)	1.22 (.01)	0.82 (.02)	1.16 (.01)
$k=1$	7.11 (.00)	7.18 (.00)	3.92 (.00)	4.93 (.00)
$k=2$	14.88 (.00)	13.72 (.00)	10.59 (.00)	10.73 (.00)
$k=3$	15.00 (.00)	15.44 (.00)	10.46 (.00)	10.58 (.00)
$k=4$	14.58 (.00)	13.80 (.01)	11.37 (.00)	11.72 (.00)
$k\geq 5$	14.54 (.00)	10.50 (.19)	14.32 (.00)	7.47 (.00)

Contract anatomy tests. Table EC.11 reports the contract anatomy tests as linear contrasts of the $\hat{\alpha}_k$ coefficients. Contract linearity for $k\leq 2$ is rejected in both *High* treatments, but cannot be rejected in the *Low* treatments. The kink at $k=2$ is confirmed (except for *Observable-Low*), and contract flatness for $k>2$ cannot be rejected in any treatment, confirming that bonus schedules rise only very mildly (if at all) after $k=2$. Consistent with the superficial fairness mechanism, and similar to Study 2, average bonus payments do not exceed $v_{k;2}/2$ in any treatment.

Between-treatment comparison: Observable vs. Unobservable. Table EC.12 reports the $\hat{\delta}_k$ coefficients from the pooled version of equation (EC.10) with $D_s=1$ for *Observable*. In *High*, no individual gap is significant except at $k\geq 5$. The joint F-test is marginally significant ($p=.04$), driven almost entirely by the $k\geq 5$ outcome level. When excluding this outcome level, managers in *High* offer bonus schedules that are statistically indistinguishable between *Observable* and *Unobservable*. In *Low*, no individual gap is even close to significance, and the joint F-test strongly fails to reject equivalence ($p=.96$). Taken together, these results indicate that managers do not adjust outcome-contingent bonuses in response to observable search effort. That is, conditional on the realized number of successful outcomes k, managers in *Observable* and *Unobservable* pay essentially identical bonuses in both success probability conditions.

Table EC.11 Study 3: Contract Anatomy Tests

		Low ($q = 0.1$)		*High* ($q = 0.5$)	
		Unobservable	*Observable*	*Unobservable*	*Observable*
Anatomy for $k \leq 2$:					
H_0: $\alpha_0 - 2\alpha_1 + \alpha_2 = 0$	(linearity)	1.77 (.29)	0.59 (.62)	3.56 (.00)	2.02 (.03)
H_0: $\alpha_0 = 0$	(zero at $k = 0$)	1.10 (.03)	1.22 (.01)	0.82 (.02)	1.16 (.01)
Slope change at $k = 2$:					
H_0: $\alpha_1 - 2\alpha_2 + \alpha_3 = 0$	(kink at $k = 2$)	−7.66 (.01)	−4.83 (.10)	−6.79 (.00)	−5.95 (.00)
Anatomy for $k > 2$:					
H_0: $\alpha_2 - 2\alpha_3 + \alpha_4 = 0$	(linearity)	−0.53 (.89)	−3.36 (.41)	1.03 (.48)	1.29 (.31)
H_0: $\alpha_3 - 2\alpha_4 + \alpha_5 = 0$	(linearity)	0.37 (.94)	−1.66 (.81)	2.04 (.30)	−5.39 (.02)
H_0: $\alpha_3 = \alpha_2$	(flatness)	0.12 (.93)	1.72 (.36)	−0.12 (.79)	−0.15 (.86)
Superficial fairness (*bonus vs.* $v_{k;2}/2$):					
H_0: $\alpha_1 = 7.5$	($k = 1$)	−0.39 (.66)	−0.32 (.75)	−3.58 (.00)	−2.57 (.03)
H_0: $\alpha_2 = 15$	($k = 2$)	−0.12 (.91)	−1.28 (.57)	−4.42 (.00)	−4.27 (.02)
H_0: $\alpha_3 = 15$	($k = 3$)	0.00 (.99)	0.44 (.87)	−4.54 (.00)	−4.42 (.02)
H_0: $\alpha_4 = 15$	($k = 4$)	−0.42 (.85)	−1.20 (.75)	−3.63 (.03)	−3.28 (.04)
H_0: $\alpha_5 = 15$	($k \geq 5$)	−0.46 (.29)	−4.50 (.55)	−0.68 (.64)	−7.53 (.00)

Table EC.12 Study 3: Difference in Contract Anatomy (*Observable* vs. *Unobservable*)

	Low ($q = 0.1$)	*High* ($q = 0.5$)
	$\hat{\delta}_k$	$\hat{\delta}_k$
$k = 0$	0.12 (.81)	0.34 (.40)
$k = 1$	0.07 (.96)	1.01 (.35)
$k = 2$	−1.16 (.62)	0.15 (.93)
$k = 3$	0.44 (.88)	0.12 (.94)
$k = 4$	−0.78 (.85)	0.35 (.85)
$k \geq 5$	−4.04 (.57)	−6.85 (.00)
$F(6, 15)$	0.22 (.96)	3.06 (.04)

D.5.4. Study 3: Bonus and Effort. The between-treatment (Δ) comparisons reported in Table 4 in the main text derive from a pooled estimation that combines the *Observable* and *Unobservable* treatments within each probability condition. We augment our contract-anatomy model with an *Observable* indicator D_s interacted with each regressor,

$$b_{it} = \alpha_0 + \alpha_1 k_{it} + \alpha_2 (k_{it} - 2)^+ + \alpha_3 e_{it} + \delta_0 D_s + \delta_1 D_s k_{it} + \delta_2 D_s (k_{it} - 2)^+ + \delta_3 D_s e_{it} + \varepsilon_{it}, \qquad \text{(EC.11)}$$

Table EC.13 Study 3: Contract Anatomy (Pooled Estimation Underlying the Δ-Comparisons)

DV: *Bonus b*	*Low* ($q=0.1$)	*High* ($q=0.5$)
Baseline schedule (Unobservable)		
α_0 *(intercept)*	1.61 (.00)	0.27 (.48)
α_1 *(slope $k \le 2$)*	5.59 (.00)	4.90 (.00)
α_2 *(kink)*	−5.60 (.00)	−4.38 (.00)
α_3 *(search effort)*	0.32 (.04)	0.13 (.59)
$\alpha_1+\alpha_2$ *(slope $k>2$)*	−0.01 (.97)	0.53 (.00)
Difference to Observable ($D_s=1$)		
δ_0 *(intercept)*	0.49 (.43)	−0.06 (.92)
δ_1 *(slope $k \le 2$)*	−0.39 (.72)	0.42 (.62)
δ_2 *(kink)*	−0.32 (.83)	−1.26 (.22)
δ_3 *(search effort)*	0.05 (.76)	−0.25 (.38)
$\delta_1+\delta_2$ *(slope $k>2$)*	−0.71 (.54)	−0.84 (.01)

where $D_s=1$ for *Observable* and $D_s=0$ for *Unobservable*, and the effort variable e_{it} is the scout's actual search y_{it} in *Observable* and the manager's elicited search belief $\tilde{y}_{it}$ in *Unobservable*, mean-centered within treatment. The α coefficients describe the *Unobservable* baseline schedule, while the δ coefficients capture the difference to *Observable*: i.e., δ_0 tests the between-treatment difference in α_0, δ_1 in α_1, δ_2 in α_2, δ_3 in α_3, and $\delta_1+\delta_2$ in the slope for $k>2$. Table EC.13 reports the full set of estimates, and the p-values reported in the Δ-column of Table 4 are taken directly from Table EC.13.

D.5.5. Study 3: Recommendations. Having established that observable search efforts improve scouting efficiency—particularly in *Low*—we ask whether this improvement operates through scouting recommendations. The natural hypothesis is a recommendation channel: observable efforts might induce managers to give higher recommendations, which in turn motivate scouts to search more. To formally test this channel (and to separate it from any effect of observable efforts on search that does not run through recommendations), we estimate the following mediation framework, separately for each success probability condition:

$$\bar{r}_{st} = \alpha_0 + \alpha_1 Observable_{1,s} + \alpha_2 t_c + \eta_{st}, \qquad \text{(EC.12)}$$

$$\bar{y}_{st} = \beta_0 + \beta_1 \bar{r}_{st} + \beta_2 Observable_{1,s} + \beta_3 (Observable_{1,s} \times \bar{r}_{st}) + \beta_4 t_c + \varepsilon_{st}, \qquad \text{(EC.13)}$$

where $\bar{r}_{st}$ and $\bar{y}_{st}$ are session-round averages of recommendation and search; $Observable_{1,s}$ is a treatment indicator; and t_c is mean-centered round. Both equations are estimated on session-round averages, with standard errors clustered at the session level. The interaction in Stage 2 lets the compliance slope differ by observability: β_1 is the slope for scouts in *Unobservable*, and $\beta_1+\beta_3$ that for scouts in *Observable*, yielding a separate indirect effect for each condition ($\hat{\alpha}_1 \times \hat{\beta}_1$ and $\hat{\alpha}_1 \times (\hat{\beta}_1+\hat{\beta}_3)$, respectively). Inference on products of coefficients uses a percentile bootstrap with session-level resampling (5,000 draws); results are reported in Table EC.14.

Table EC.14 Study 3: Observable Efforts, Recommendations, and Search (Mediation Analysis)

	Low ($q = 0.1$)		*High* ($q = 0.5$)	
	Stage 1 $\bar{r}_{st}$	Stage 2 $\bar{y}_{st}$	Stage 1 $\bar{r}_{st}$	Stage 2 $\bar{y}_{st}$
Intercept	11.79 (.00)	3.24 (.06)	6.73 (.00)	4.24 (.00)
Observable (α_1, β_2)	0.98 (.46)	1.31 (.64)	1.14 (.50)	−0.20 (.75)
Recommendation (β_1)	—	0.26 (.18)	—	−0.05 (.48)
Observable × *Recommendation* (β_3)	—	0.03 (.93)	—	0.12 (.14)
Observable Compliance ($\beta_1 + \beta_3$)	—	0.29 (.21)	—	0.08 (.12)
Round$_c$ (α_2, β_4)	0.06 (.09)	−0.08 (.02)	−0.03 (.10)	−0.06 (.00)
Mediation decomposition (5,000 session-cluster bootstrap draws)				
Indirect Unobservable ($\alpha_1 \times \beta_1$)	0.26 (.50)		−0.05 (.94)	
Indirect Observable ($\alpha_1 \times (\beta_1 + \beta_3)$)	0.28 (.65)		0.09 (.57)	
Direct (β_2)	1.31 (.64)		−0.20 (.75)	

The conjectured recommendation channel finds no support. In fact, observable efforts barely move recommendations: managers in *Observable* recommend only slightly more than managers in *Unobservable*, but the difference is insignificant in both conditions (*High*: $\hat{\alpha}_1 = 1.14$, $p = .50$; *Low*: $\hat{\alpha}_1 = 0.98$, $p = .46$). At the same time, scouts do not follow the recommendations they receive, as the compliance slope $\hat{\beta}_1$ is near zero in both conditions, and full compliance ($H_0 : \beta_1 = 1$) is strongly rejected throughout (all $p = .00$). These findings do not change with observable efforts: the interaction $\hat{\beta}_3$ is small and insignificant (*High* 0.12, $p = .14$; *Low* 0.03, $p = .93$), and the implied compliance slopes for scouts in *Observable* ($\hat{\beta}_1 + \hat{\beta}_3$) remain close to zero, with full compliance rejected for them as well. It follows that the indirect effects through recommendations are small and insignificant in every treatment (all $p > .50$). Observable efforts also have no effect on search that bypasses recommendations: the direct term $\hat{\beta}_2$ is insignificant in both conditions (*High*: -0.20, $p = .75$; *Low*: 1.31, $p = .64$). Search, however, does decline modestly over the course of the experiment, net of recommendations, in both conditions ($\hat{\beta}_4 = -0.06$, $p < .00$ in *High*; $\hat{\beta}_4 = -0.08$, $p = .02$ in *Low*). With both the direct and the indirect effects insignificant, the total effect of *Observable* on search is insignificant as well.

In short, observable efforts affect neither the recommendations managers give nor the extent to which scouts act on them. The recommendation channel hence does not account for the efficiency gain under observable efforts.

D.6. Study 4: Supporting Analyses

D.6.1. Study 4: Treatment effects. To analyze the effect of our treatments on outcomes of interest (e.g., search or profit), we estimate the regression model presented in (8) on the pooled data from Study 4, using treatment dummies for *Repeated* and the *High* success probability condition (baseline: *Upfront-Low*). Table EC.15 presents the full statistical results, which we use as basis for the p-values in Table 5 in the main text.

Table EC.15 Study 4: Aggregate Outcomes and Treatment Effects

	Recomm. $\bar{r}$	Upfront $\bar{w}$	Search $\bar{y}$	Bonus $\bar{b}$	Total pay $\bar{w}+\bar{b}$	Scout utility $\bar{u}$	Manager profit $\bar{\pi}$	Total profit $\bar{\Pi}$	Exp. total profit $\bar{\Pi}^*(y)$
Intercept	9.63	6.06	4.34	1.34	7.39	3.05	−1.89	1.16	1.74
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Repeated	−0.96	−0.54	0.60	0.32	−0.21	−0.81	1.28	0.47	0.28
	(.38)	(.43)	(.42)	(.20)	(.77)	(.30)	(.13)	(.17)	(.17)
High	−3.13	0.67	−0.57	1.50	2.18	2.74	11.53	14.28	14.09
	(.00)	(.14)	(.38)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Repeated × *High*	0.27	−0.22	−0.42	1.79	1.57	1.99	−0.95	1.04	1.08
	(.84)	(.78)	(.62)	(.00)	(.13)	(.05)	(.48)	(.33)	(.22)
$Round_c$	0.02	−0.07	−0.08	−0.05	−0.11	−0.04	−0.03	−0.07	−0.10
	(.54)	(.00)	(.00)	(.01)	(.00)	(.16)	(.38)	(.10)	(.00)

Table EC.16 Study 4: Round Trend $\hat{\alpha}_1$ by Treatment

	Recomm. $\bar{r}$	Upfront $\bar{w}$	Search $\bar{y}$	Success $\bar{k}$	Bonus $\bar{b}$	Scout utility $\bar{u}$	Manager profit $\bar{\pi}$	Total profit $\bar{\Pi}$	Exp. total profit $\bar{\Pi}^*(y)$
Upfront-High	−0.02	−0.02	−0.09	−0.04	−0.06	0.00	−0.03	−0.03	−0.18
	(.35)	(.20)	(.02)	(.04)	(.07)	(.92)	(.16)	(.19)	(.02)
Repeated-High	−0.03	−0.06	−0.06	−0.02	−0.06	−0.06	−0.01	−0.07	−0.18
	(.25)	(.03)	(.01)	(.08)	(.07)	(.09)	(.85)	(.36)	(.00)
Upfront-Low	−0.04	0.00	−0.10	−0.01	−0.07	0.03	−0.04	−0.01	−0.03
	(.57)	(.99)	(.03)	(.01)	(.04)	(.74)	(.61)	(.68)	(.01)
Repeated-Low	0.06	−0.09	−0.09	−0.01	−0.03	−0.02	−0.05	−0.07	−0.03
	(.19)	(.02)	(.00)	(.01)	(.21)	(.59)	(.36)	(.16)	(.00)

D.6.2. Study 4: Learning. To assess whether outcomes change over the course of the experiment within each treatment of Study 4, we estimate regression (EC.9) separately for each treatment and each dependent variable. Table EC.16 presents the results. If anything, scouting efficiency deteriorates over time: both search and expected total profit decline significantly in all treatments, and there is no evidence of improvement in any outcome—even in *Repeated*, where one might expect a stable relationship between manager and scout to foster learning and coordination over time.

D.6.3. Study 4: Contracts. We apply equation (EC.10) to Study 4 using the same three types of tests as in Studies 2 and 3: (i) estimation of the average total payment schedule $\hat{\alpha}_k$ for each treatment; (ii) contract anatomy tests based on linear contrasts of the $\hat{\alpha}_k$ coefficients; and (iii) a between-treatment comparison of *Upfront* and *Repeated* contracts via the $\hat{\delta}_k$ coefficients. The dependent variable throughout is the scouts' total pay $w_{it} + b_{it}$, which captures the full payment to scouts at each outcome level k, including the upfront payment.

Table EC.17 Study 4: Average Total Compensation $(w+b)$ Schedule $\hat{\alpha}_k$ by Treatment

	Low $(q=0.1)$		*High* $(q=0.5)$	
	Upfront	*Repeated*	*Upfront*	*Repeated*
$k=0$	6.08 (.00)	5.65 (.00)	5.44 (.00)	4.79 (.00)
$k=1$	9.59 (.00)	8.95 (.00)	7.43 (.00)	7.73 (.00)
$k=2$	13.18 (.00)	13.20 (.00)	12.25 (.00)	14.17 (.00)
$k=3$	13.00 (.00)	13.00 (.00)	12.71 (.00)	14.46 (.00)
$k=4$	—	—	14.39 (.00)	15.48 (.00)
$k\geq 5$	—	—	13.18 (.00)	13.69 (.00)

Average bonus schedules. Table EC.17 reports the $\hat{\alpha}_k$ estimates. All estimates are positive and highly significant across all four treatments. Total payments at $k=0$ are large—strongly rejecting the zero benchmark in all treatments ($p=.00$)—in stark contrast to the near-zero bonuses observed in Studies 2 and 3. This finding is entirely attributable to the unconditional upfront payment, which shifts $\hat{\alpha}_0$ upward by approximately $\bar{w}\approx$ 5–6 relative to prior studies. It is also noteworthy that bonus schedules in *Low* are similar to those in *High*.

Contract anatomy tests. Table EC.18 reports the contract anatomy tests as linear contrasts of the $\hat{\alpha}_k$ coefficients. The payment schedule rises from $k=0$ to $k=2$ and then flattens: contract flatness for $k>2$ cannot be rejected in any treatment ($p\geq .45$), consistent with managers ceasing to increase total payments once the value they receive is capped. In addition, the payment increment per additional success for $k\leq 2$ is smaller than in Studies 2 and 3 for three of the four treatments, providing evidence for a partial substitution effect between upfront payments and outcome-contingent bonuses. The exception is *Repeated-High*, where contract linearity for $k\leq 2$ is rejected ($p=.00$), suggesting that partner matching in *High* partially offsets this substitution effect, enabling a steeper, more curved payment schedule for $k\leq 2$.

Regarding *superficial fairness*: In *Low*, total payments exceed $v_{k;2}/2$ for $k\leq 1$ in both treatments but are statistically indistinguishable from $v_{k;2}/2$ for $k\geq 2$. In *High*, payments in *Repeated* are likewise indistinguishable from the equal-split benchmark at all outcome levels $k\geq 1$, while *Upfront* falls somewhat short at $k=2$ and $k=3$. The overall fairness pattern in Study 4 therefore differs from Studies 2 and 3, especially at $k=0$ and $k=1$, where upfront payments create a floor on total payments that pushes total payments to or above the equal-split benchmark even in the absence of successful search outcomes.

Between-treatment comparison: Upfront vs. Repeated. Table EC.19 reports the $\hat{\delta}_k$ coefficients from the pooled version of equation (EC.10) with $D_s=1$ for *Repeated*. In *Low*, no individual gap is significant and the joint F-test strongly fails to reject equivalence. That is, conditional on the search outcome k, managers in *Repeated* and *Upfront* provide essentially identical total payments. In *High*, the picture is more nuanced. Managers in *Repeated* pay significantly more at $k=2$ and $k=3$—precisely the outcome levels at which manager revenue is maximized and any additional bonus is purely redistributive—while no significant gap arises at any other outcome level. The joint F-test for *High* strongly rejects payment equivalence. The effect of *Repeated* on total payments thus appears to be concentrated at the kink (i.e., $k=2$), where managers' own marginal return to successful search outcomes drops to zero.

Table EC.18 Study 4: Contract Anatomy Tests

		Low ($q=0.1$)		*High* ($q=0.5$)	
		Upfront	*Repeated*	*Upfront*	*Repeated*
Anatomy for $k \leq 2$:					
H_0: $\alpha_0 - 2\alpha_1 + \alpha_2 = 0$	(linearity)	0.10 (.97)	0.96 (.49)	2.83 (.07)	3.49 (.00)
H_0: $\alpha_0 = 0$	(zero at $k=0$)	6.08 (.00)	5.65 (.00)	5.44 (.00)	4.79 (.00)
Slope change at $k=2$:					
H_0: $\alpha_1 - 2\alpha_2 + \alpha_3 = 0$	(kink at $k=2$)	−3.78 (.28)	−4.46 (.06)	−4.36 (.07)	−6.16 (.00)
Anatomy for $k > 2$:					
H_0: $\alpha_3 = \alpha_2$	(flatness)	−0.18 (.91)	−0.20 (.90)	0.46 (.45)	0.28 (.50)
Superficial fairness (*bonus vs.* $v_{k;2}/2$):					
H_0: $\alpha_0 = 0$	($k=0$)	6.08 (.00)	5.65 (.00)	5.44 (.00)	4.79 (.00)
H_0: $\alpha_1 = 7.5$	($k=1$)	2.09 (.06)	1.45 (.01)	−0.07 (.86)	0.23 (.59)
H_0: $\alpha_2 = 15$	($k=2$)	−1.82 (.15)	−1.80 (.25)	−2.75 (.06)	−0.83 (.23)
H_0: $\alpha_3 = 15$	($k=3$)	−2.00 (.15)	−2.00 (.35)	−2.30 (.09)	−0.54 (.49)
H_0: $\alpha_4 = 15$	($k=4$)	—	—	−0.61 (.03)	0.48 (.60)

Table EC.19 Study 4: Difference in Contract Anatomy (*Repeated* vs. *Upfront*)

	Low ($q=0.1$)	*High* ($q=0.5$)
	$\hat{\delta}_k$	$\hat{\delta}_k$
$k=0$	−0.43 (.54)	−0.65 (.26)
$k=1$	−0.64 (.43)	0.30 (.55)
$k=2$	0.02 (.99)	1.93 (.03)
$k=3$	0.00 (.99)	1.75 (.08)
$k=4$	—	1.09 (.23)
$k \geq 5$	—	0.51 (.78)
	$F(4,1296)=0.85$ (.49)	$F(6,1254)=6.10$ (.00)

D.6.4. Study 4: Upfront Payments, Recommendations, and Search. Table EC.20 reports the full pooled estimation underlying the Δ-columns in Table 6. The model augments equation (11) with a *Repeated* indicator interacted with each regressor,

$$y_{it} = a_0 + a_1 w_{it} + a_2 r_{it} + a_3(w_{it} \times r_{it}) + d_0 D_s + d_1 D_s w_{it} + d_2 D_s r_{it} + d_3 D_s(w_{it} \times r_{it}) + \varepsilon_{it}, \qquad \text{(EC.14)}$$

where $D_s = 1$ for *Repeated* and $D_s = 0$ for *Upfront*. The a coefficients describe the *Upfront* baseline; the d coefficients capture the *Repeated* difference. These are the p-values reported in the Δ-column of Table 6.

Table EC.20 Study 4: Pooled Estimation Underlying the Δ-Comparisons in Table 6

DV: *Search y*	*Low* ($q = 0.1$)	*High* ($q = 0.5$)
Baseline schedule (*Upfront*)		
a_0 *(intercept)*	0.94 (.00)	0.19 (.07)
a_1 *(upfront payment w)*	0.22 (.00)	0.41 (.00)
a_2 *(recommendation r)*	0.16 (.06)	0.28 (.00)
a_3 $(w \times r)$	0.01 (.46)	−0.03 (.00)
Difference to *Repeated* ($D_s = 1$)		
d_0 *(intercept)*	0.46 (.63)	0.78 (.34)
d_1 *(upfront payment w)*	0.11 (.43)	0.10 (.59)
d_2 *(recommendation r)*	0.11 (.52)	−0.07 (.59)
d_3 $(w \times r)$	−0.02 (.33)	−0.01 (.62)

Table EC.21 Study 5: Aggregate Outcomes and Treatment Effects

	Search $\bar{y}$	Success $\bar{k}$	Bonus $\bar{b}$	Scout utility $\bar{u}$	Manager profit $\bar{\pi}$	Total profit $\bar{\Pi}$	Exp. total profit $\bar{\Pi}^*(y)$
Intercept (*Incentive2*)	5.62	0.58	3.79	−1.83	3.62	1.78	1.69
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Automated	7.23	0.77	8.61	1.38	−0.33	1.05	0.92
	(.00)	(.00)	(.00)	(.01)	(.65)	(.07)	(.00)
$Round_c$	0.03	0.00	0.05	0.01	−0.00	0.01	0.02
	(.38)	(.47)	(.24)	(.67)	(.77)	(.78)	(.07)

D.7. Study 5: Supporting Analyses

D.7.1. Study 5: Treatment effects. Because *Automated* is run only in the *Low* success probability condition, we estimate a reduced form of the regression model presented in (8) on the pooled Study 5 data, including a single treatment dummy for *Automated* (baseline: *Incentive2*) alongside the mean-centered round trend. Table EC.21 presents the results, and is the basis of the p-values in Table 7 in the main text.

D.7.2. Study 5: Manager Beliefs. As in the *Unobservable* treatment of Study 3, *Incentive2* additionally elicits managers' beliefs $\tilde{y}_{it}$ about scouts' search effort, elicited after search and outcomes are realized. As in *Unobservable*, beliefs are right on average—deviating from actual search by only 0.36 ($p = .72$)—yet track actual search only weakly: regressing beliefs on actual search, $\tilde{y}_{it} = a_0 + a_1 y_{it} + \varepsilon_{it}$, gives a slope below one ($a_1 = 0.30$, $p = .00$). Since beliefs are right on average yet too flat, managers over-estimate low but under-estimate high search efforts, thus finding it difficult to distinguish diligent from negligent scouts.

D.8. All Studies: Decomposition of Loss in Scouting Efficiency

Table EC.22 decomposes the sources of the observed loss in scouting efficiency for all treatments, as measured by total expected profits. We first quantitatively distinguish efficiency loss that arises from agency issues

Table EC.22 Decomposition of Loss in Scouting Efficiency

	Study					Performance loss			
						vs. First-best	Search bias	Search variability	Delegation
			$\bar{y}$	$\bar{\Pi}^*(y)$	$\Pi_{\bar{y}}$	$1-\frac{\bar{\Pi}^*(y)}{\pi^{\text{fb}}}$	$\frac{\pi^{\text{fb}}-\Pi_{\bar{y}}}{\pi^{\text{fb}}-\bar{\Pi}^*(y)}$	$\frac{\Pi_{\bar{y}}-\bar{\Pi}^*(y)}{\pi^{\text{fb}}-\bar{\Pi}^*(y)}$	$\frac{\bar{\Pi}^*_{\text{sd}}(y)-\bar{\Pi}^*(y)}{\pi^{\text{fb}}-\bar{\Pi}^*(y)}$
	1	*Single*	11.80	3.16	3.87	19%	1%	99%	–
	2	*Incentive*	6.43	2.24	2.89	42%	60%	40%	56%
	2	*Discretionary*	6.09	2.34	2.77	40%	72%	28%	53%
	3	*Unobservable*	6.35	1.52	2.86	61%	43%	57%	69%
Low	3	*Observable*	8.25	2.63	3.41	32%	38%	62%	42%
	4	*Upfront*	4.34	1.74	2.09	55%	84%	16%	66%
	4	*Repeated*	4.95	2.02	2.35	48%	82%	18%	61%
	5	*Incentive2*	5.62	1.69	2.61	56%	58%	42%	67%
	5	*Automated*	12.85	2.62	3.87	32%	1%	99%	43%
	1	*Single*	5.48	21.18	22.00	4%	13%	87%	–
	2	*Incentive*	4.62	17.46	21.34	21%	17%	83%	80%
	2	*Discretionary*	4.11	17.55	20.59	21%	34%	66%	79%
High	3	*Unobservable*	3.93	17.61	20.24	20%	42%	58%	79%
	3	*Observable*	4.63	18.78	21.36	15%	23%	77%	72%
	4	*Upfront*	3.77	15.83	19.89	28%	35%	65%	85%
	4	*Repeated*	4.06	18.27	20.49	17%	42%	58%	76%

and fairness concerns (by construction absent in *Single*) from efficiency loss that might arise from non-standard preferences and/or judgment biases, by calculating the fraction of efficiency loss that results from the delegation of scouting decisions as $(\bar{\Pi}^*_{\text{sd}}(y)-\bar{\Pi}^*(y))/(\pi^{\text{fb}}-\bar{\Pi}^*(y))$, with $\bar{\Pi}^*_{\text{sd}}(y)$ denoting the expected total profit in *Single*. For the remaining efficiency loss, which is caused by behavioral anomalies that arise even in the absence of delegation, we identify the underlying mechanisms as follows.

Remaining agnostic about specific behavioral biases that chiefly impact scouting efficiency in our setting, we note that efficiency might be lower than theory predicts because scouts, on average, search too little (*search bias*) or because they exhibit *search variability*. To assess the relative impact of search bias and search variability on expected total profits, we first calculate $\Pi_{\bar{y}}=\pi^{\text{fb}}(\bar{y})$ as the counterfactual *expected* total profit from the treatment-level average scouting decision $\bar{y}$ (which eliminates *all* search variability). We can then quantify the performance loss from search bias as $(\pi^{\text{fb}}-\Pi_{\bar{y}})/(\pi^{\text{fb}}-\bar{\Pi}^*(y))$, and the performance loss from search variability as $(\Pi_{\bar{y}}-\bar{\Pi}^*(y))/(\pi^{\text{fb}}-\bar{\Pi}^*(y))$.

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