// NO.26-030 | 07/2026

DISCUSSION PAPER

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# When Goliath Shares with David: The Effects of Asymmetry on Collaboration

# When Goliath Shares with David: The Effects of Asymmetry on Collaboration*

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July 13, 2026

**Abstract**

Partners in R&D alliances and joint ventures are rarely symmetric, yet whether asymmetry helps or hurts collaboration is not fully understood. We study how asymmetry in competitors' information endowments affects collaborative knowledge exchange. A model of word-of-mouth communication predicts a clean separation: Initiating the knowledge exchange requires that the informational advantage sacrificed be sufficiently small, while the continuation incentive is independent of the initial endowment (and asymmetry). In a laboratory experiment, we confirm the model's comparative-statics predictions and establish three results. First, initiation rates decline as initial asymmetry increases, holding the better-endowed partner's initial endowment constant. Second, conditional on initiation, the initial asymmetry does not predict defection later in collaboration. Third, extreme asymmetry reduces match length and joint surplus because initiation fails, not because continuation breaks down. The initiation-continuation separation implies that governance mechanisms targeting the formation margin (such as pilot projects, phased disclosure agreements, or credible commitments to reciprocate) can expand the range of asymmetric partnerships that succeed, while continuation requires no special intervention.

**Keywords:** asymmetric partnerships, cooperation, experimental economics, information sharing, joint ventures, knowledge diffusion, open innovation, R&D collaboration, strategic alliances

**JEL Codes:** C92, D83, L24, M21, O31

*Funding by the Center and Laboratory for Behavioral Operations and Economics (CLBOE) at the University of Texas at Dallas is gratefully acknowledged. Institutional Review Board approval has been obtained at the University of Texas at Dallas (MR 14-127). The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

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# 1 Introduction

Partners in R&D collaborations and strategic alliances rarely enter on equal terms. A small biotech firm with a novel compound teams up with a large pharmaceutical company that has regulatory expertise and distribution infrastructure; an entrant with a disruptive technology seeks a joint venture with the incumbent that dominates the market. Such asymmetric partnerships can be highly productive when they create complementarities (Teece, 1986; Kogut, 1988), yet many never get off the ground: At formation, partners weigh the resources a collaboration would bring against the risk that a stronger partner misappropriates what they contribute (Katila et al., 2008). Despite decades of research on alliance governance and management (Gulati, 1998; Dyer and Singh, 1998), the mechanism through which partner asymmetry affects whether collaboration *starts* (as opposed to whether it *survives*) is not well understood (Khanna et al., 1998; Cohen and Levinthal, 1990). The distinction matters: If asymmetry primarily threatens formation rather than continuation, then the conventional focus on alliance management may be addressing the wrong margin.

We study this question in the context of information sharing between competitors. Competitors routinely share proprietary information despite the competitive costs, in settings ranging from steel minimills (von Hippel, 1987; Schrader, 1991) and R&D laboratories (Bouty, 2000) to financial markets (Crawford et al., 2017; Botelho, 2018).[1] Stein (2008) develops a model of word-of-mouth communication between competing firms in which a *sharing equilibrium* emerges when the expected benefits from reciprocal exchange exceed the competitive cost. Ganglmair et al. (2020) extend and then implement this model in a laboratory experiment and confirm that expectations of reciprocity are the primary driver of sharing: A player's beliefs about a rival's *intentions* (of sharing) matter more than the rival's *ability* (to generate new information). These results establish the behavioral regularities of the information-sharing game, but the question of asymmetric initial endowments remains unanswered.

[1] Historical evidence of collective invention through knowledge sharing is documented in the iron industry (Allen, 1983), steam engineering (Nuvolari, 2004), and mechanical manufacturing (Thomson, 2009).

Stein's (2008) model already contains the key theoretical mechanism. In the network extension of his model, a player who has accumulated $k$ signals from a prior exchange decides whether to start a new exchange with a third player. The decision to *initiate* depends on $k$, where a larger stock makes the first sharing act more costly, while the decision to *continue* an existing exchange does not. Above a critical threshold $k^*$, the player prefers not to initiate at all. We reframe this logic for a bilateral exchange in which one player ($A$) starts with an initial stock of $k \geq 1$ ideas and the other ($B$) starts with fewer or no ideas: The *initiation condition* depends on the initial asymmetry (captured by $k$), while the *continuation condition* is independent thereof. The initiation-continuation separation thus generates sharp predictions about where asymmetry should bite and where it should not.

We test the predictions of the asymmetric-endowment model using data from a laboratory experiment. Our design exogenously varies the initial stocks of ideas across treatments. For treatments with low initial asymmetry, the model predicts higher initiation rates than for treatments with higher asymmetry.[2] The theoretical prediction is that asymmetry operates through one margin (initiation) rather than the other (continuation), but in field data on R&D alliances, these margins are confounded: Partnerships that fail to initiate are typically unobserved, and the endowment gap is endogenous to partner selection. The experiment varies the gap exogenously and records the full sequence of decisions: initiation, round-by-round continuation, and directly elicited beliefs. The laboratory setting thus allows us to cleanly separate the two margins.

Our experimental results confirm the initiation prediction and reveal the resilience of the collaboration mechanism. We document three main findings. First, initiation rates decrease as the initial asymmetry increases. In fact, they decline by 30 percentage points (from 94.7% to 64.4%) once the endowment gap exceeds the theoretical threshold $k^*$ but are robust to gaps below it. A classification of subjects by their persistent propensity to share shows that the gap deters initiation selectively: Subjects who are generally reluc-

[2] These treatments were conducted as part of the same experiment described in Ganglmair et al. (2020), but the asymmetric-endowment treatments were not included in that paper. The present analysis is therefore based on a new, previously unpublished set of treatments that test distinct predictions.

tant to share respond to the gap (initiating at only 37% in the extreme-gap treatment), whereas subjects with a high sharing propensity initiate at rates of about 90% or more regardless of the size of the gap. Second, conditional on initiation, the initial endowment asymmetry does not predict defection from collaborative information sharing. Continuation rates are uniformly high regardless of the initial asymmetry, and in a discrete-time hazard model at the round level, the initial gap is insignificant. Instead, the deciding player's expectations of reciprocity are the dominant predictor of continuation. The binding constraint on collaboration is therefore the initiation decision, not the continuation decision. Third, extreme asymmetry reduces joint surplus because initiation fails, not because continuation breaks down. A simulation that varies the share of high-asymmetry partnerships in a population shows that the initiation barrier alone costs up to 16.1% of potential collaborative output, even though, conditional on initiation, both partnership types are equally productive.

Our paper contributes to the literature in a number of ways. First, we contribute to the literature on strategic alliances and R&D joint ventures. A foundational rationale for cooperative R&D is that it can internalize cross-firm spillovers that non-cooperative arrangements leave on the table (d'Aspremont and Jacquemin, 1988; Grossman and Shapiro, 1986). Subsequent work studies the determinants of alliance formation (Hernán et al., 2003; Gulati, 1998) and the role of partner characteristics in alliance success (Kogut, 1988; Hagedoorn and Schakenraad, 1994), and views alliances between competitors as races to learn in which asymmetric learning shifts relative bargaining power (Hamel, 1991). A recurring empirical finding is that partner diversity has a non-monotone effect on alliance performance: Sampson (2007) documents an inverted-U relationship between technological diversity and innovation output; she attributes the downturn at high diversity to limits on absorptive capacity, but the mechanism is inferred from innovation outcomes rather than observed in the collaboration process. We provide evidence for a complementary mechanism. Collaboration is resilient to moderate asymmetry because the binding constraint is initiation, not continuation. Once initiated, matches are equally durable across all levels of asymmetry. Extreme asymmetry harms alliances at the formation stage: It preserves the willingness to keep sharing but prevents the better-endowed partner from initiating the exchange. The initiation rate in our high-asymmetry treatment, where the gap exceeds the model's threshold, is 30 percentage points lower than in its within-group control. The inverted-U thus reflects a formation constraint, not a management failure.

Second, we contribute to the experimental literature on cooperation in asymmetric settings. In public goods games, asymmetric endowments generally reduce contributions relative to symmetric groups (Cherry et al., 2005). Work in evolutionary game theory finds that heterogeneity in players' endowments or network position can promote the emergence of cooperation, with the mechanism operating through spatial structure (Santos et al., 2008; Perc and Szolnoki, 2008). Meickmann (2023) studies knowledge sharing combined with R&D investment in a duopoly with symmetric endowments, comparing endogenous and exogenously assigned sharing arrangements, and finds behavioral spillovers between sharing and investment decisions. The closest experimental predecessor is Roelofs et al. (2017), who study technology sharing between asymmetric firms (leaders and laggards that differ in initial knowledge) in a simultaneous-move design. Our setting differs from these contributions in that the interaction is sequential and involves bilateral information exchange rather than simultaneous contributions to a common pool or a one-shot sharing decision. The sequential structure separates initiation from continuation, which explains why asymmetry is costly at one margin and irrelevant at the other.

Third, we show where the expectations channel operates when endowments are asymmetric. That expectations of reciprocity drive information sharing between competitors is the central result in Ganglmair et al. (2020); in that design, however, a player's beliefs about the rival's intentions move together with the rival's ability to generate new ideas, which varies across treatments. Our design holds abilities constant and varies only the initial endowments, so treatment differences in beliefs cannot reflect differences in the partner's capacity to reciprocate. We establish that player $A$'s expectation of reciprocity is the strongest predictor of initiation, extending the expectations result of Ganglmair et al. (2020) to a margin their design did not isolate, and we show that expectations track the initiation margin: Below the threshold $k^*$, player $A$ remains optimistic despite a larger gap and initiation holds up; above the threshold, optimism falls and initiation collapses. The channel is one-sided: Player $B$ does not condition its sharing on the size of the gap (the costliness of player $A$'s sacrifice), although the absolute level of player $A$'s initial contribution to the partnership does matter to player $B$. This one-sidedness explains why player $A$'s optimism sustains initiation below the threshold but cannot rescue collaborations where the cost of sharing is prohibitive. The pattern is consistent with the finding in Roelofs et al. (2017) that leaders share more than theory predicts when they are not compensated, and with evidence from trust games that first movers hold optimistic beliefs about the reciprocation of their costly actions (Sapienza et al., 2013).

These contributions have practical implications for partnerships between unequal firms, such as alliances between market leaders and entrants. Our results characterize when such partnerships succeed: Collaboration is robust to asymmetry, provided the endowment gap remains below a critical threshold above which initiation breaks down. Because the binding constraint is the first move, governance mechanisms that lower the cost of initiation (pilot projects, phased disclosure agreements, or credible commitments to reciprocate) can expand the range of viable partnerships, while continuation requires no special intervention. The same logic extends to knowledge sharing within organizations: A senior employee paired with a junior colleague faces an analogous initiation barrier that can constrain internal knowledge flows.

The paper is structured as follows: In Section 2, we summarize the model and reframe it for asymmetric endowments. Section 3 describes the experimental design and formulates our hypotheses derived from the theoretical model. Section 4 presents the experimental results on initiation, continuation, and collaboration survival. Section 5 explores the expectations mechanism and joint surplus. Section 6 discusses managerial implications and concludes.

# 2 Model

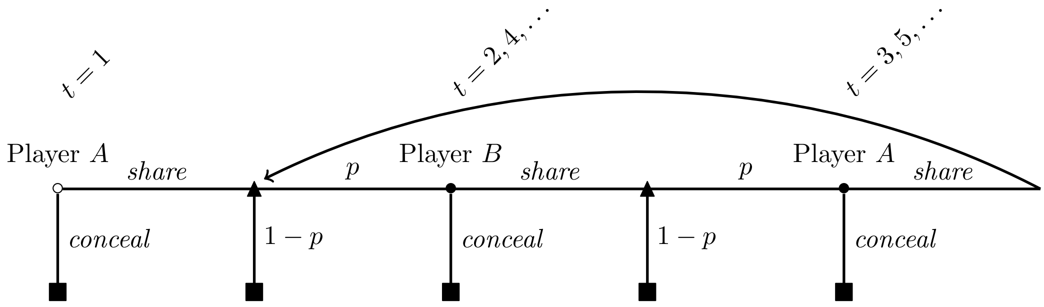
Our theoretical framework formalizes a tension at the core of inter-firm knowledge exchange. The knowledge-based view of the firm holds that proprietary knowledge is a primary source of competitive advantage (Grant, 1996; Kogut and Zander, 1992), which implies that sharing knowledge with a competitor is inherently costly. At the same time, the relational view argues that repeated, reciprocal knowledge exchange can generate relational rents that exceed what either party could achieve alone (Dyer and Singh, 1998). The central question is whether and when the expected relational rents are large enough to overcome the appropriation hazard. Our model captures this trade-off in a sequential exchange game between two players with asymmetric knowledge stocks. The critical feature is that the trade-off plays out differently at two distinct margins: the decision to *initiate* an exchange, which requires the better-endowed party to bear a first-mover cost that increases with the knowledge gap, and the decision to *continue* an existing exchange, which depends only on the expected reciprocity of the partner and is independent of the initial asymmetry. This separation implies that asymmetric partnerships are hard to form but, once formed, can be surprisingly durable, a pattern our experiment confirms.

This section presents a condensed version of the model of word-of-mouth communication in Stein (2008) and Ganglmair et al. (2020). We highlight the mechanics of the asymmetry of the players and summarize the main results relevant for our experimental design.

## 2.1 Baseline Setting

Two competing players, $A$ moving in all odd rounds and $B$ moving in all even rounds, alternate in deciding whether to share newly generated ideas. In each round $t \geq 1$, the active player generates (at no cost) a new idea with exogenous probability $p$ and decides whether to share it with the other player. Sharing an idea adds one unit to the other player $j$'s stock of ideas, $n_j$. If either player fails to generate a new idea (termination *by chance*) or the active player decides not to share (termination *by choice*), the interaction

**Figure 1:** Timing of Decisions




ends. We depict the timing of these decisions in Figure 1. Circles represent players' decisions (with player $A$ starting the game in $t = 1$), triangles stand for moves of nature, and squares indicate a terminal path. This game has an indeterminate horizon: as long as players generate new ideas and share them, the exchange (and accumulation) of ideas continues.

Each player's payoffs depend on the total number of ideas they accumulate, $n_i$, and on the informational advantage (when player $i$ has more ideas than player $j$). More concretely, the payoffs $U_i$ are a weighted sum of the value of a player's stock of ideas and the value of the informational advantage (if it exists):

$$U_i = (1 - \theta)\, v(n_i) + \theta \max \left\{ v(n_i) - v(n_j),\ 0 \right\}. \tag{1}$$

A parameterization with $v(n) = 1 - \beta^n$, where $\beta \in (0, 1)$, provides an illustrative product-market interpretation of this payoff structure (see Stein, 2008). Let $\beta^{n_i}$ denote the marginal costs of production of player $i$. Costs decrease with the number of ideas $n_i$ at a diminishing marginal rate (i.e., with diminishing marginal value of ideas). The parameter $\theta$ captures the degree of competition and thus the relevance of the informational advantage. For instance, consider the polar case with $\theta = 0$. In this scenario, having more ideas than the other player is irrelevant, as the entire payoff stems from monopoly profits (with unit demand and a price normalized to 1). In the other extreme, with $\theta = 1$, the payoffs are equal to the marginal-cost difference $v(n_i) - v(n_j) = \beta^{n_j} - \beta^{n_i}$, the outcome from a Bertrand price competition model with asymmetric costs.

In the baseline setting with symmetric information sharing (where all players always share exactly one idea, including player $A$ in $t = 1$), when player $i$ reaches round $t$, it has accumulated $t$ ideas (some generated and some shared by player $j$). If, in that round $t$, it conceals the last generated idea (so that the exchange ends), its payoffs are

$$conceal_{it} = (1-\theta)\left(1-\beta^{t}\right)+\theta\left(\beta^{t-1}-\beta^{t}\right). \tag{2}$$

For its expected payoff from sharing the most recently generated idea in $t$, it forms expectations about player $j$'s next-round behavior, that is, about the probability of receiving feedback from player $j$ in $t+1$. Suppose player $i$ is pessimistic and anticipates player $j$ will not share in $t+1$. It thus expects the game to end in $t+1$ with $t$ ideas for player $i$. The payoffs from sharing in $t$ are then

$$share_{it}(\text{ends in } t+1) = (1-\theta)\left(1-\beta^{t}\right).$$

Alternatively, suppose player $i$ is optimistic and anticipates player $j$ will always share when it generates a new idea in any future round $t+1$ and later. Moreover, assume player $i$ will also share in all future rounds $t+2$ and later. In this scenario, the game continues as long as players generate new ideas. In this sharing outcome, player $i$'s payoffs are

$$share_{it}(\text{continues}) = (1-\theta)\sum_{m=0}^{\infty} p^{m}(1-p)\left(1-\beta^{t+m}\right). \tag{3}$$

In any round $t$, player $i$ thus decides to share as long as $share_{it} \geq conceal_{it}$.

This game has multiple pure-strategy equilibria (see Ganglmair et al., 2020). There is always a *non-sharing equilibrium* in which player $i$ decides to conceal, and the game ends. Suppose player $i$ is pessimistic; then $share_{it}(\text{ends in } t+1) < conceal_{it}$. This is true for any $t$, and in a non-sharing equilibrium, player $i$ will conceal its very first idea in $t = 1$.

In a *sharing equilibrium*, both players must be willing to share in all $t$. A necessary condition for this equilibrium is $share_{it}(\text{continues}) \geq conceal_{it}$ or, after some manipulation (using the model's parameterization),

$$p\beta \geq \theta.$$

This implies that, as long as the probability of generating new ideas, $p$, is high enough or the degree of competition, $\theta$, is small enough, both players are willing to share newly generated ideas when they are optimistic.[3] When $\theta$ is large, competition is intense, and the incentive to share is low as the informational advantage dominates the payoffs.[4] On the other hand, when $\theta$ is small, the size of the stock has a larger weight in the player's payoffs, and the cooperation gains from that larger stock dominate. The same holds for higher values of $p$, which generate a larger expected stock of ideas.

The sharing condition $p\beta \geq \theta$ is independent of $t$ and symmetric (that is, independent of the player's identity $i$ or $j$). It applies to an accumulation process with an increment of one in every round. Once we allow player $A$ to have more than one initial idea, the sharing condition no longer applies in $t = 1$, but applies to all future rounds $t' > 1$, conditional on the exchange reaching that round. We therefore refer to $p\beta \geq \theta$ as the *continuation condition*.

**Condition 1** (Continuation)**.** *Conditional on reaching $t'$, the players share newly generated ideas in $t \geq t'$ only if*

$$\phi \equiv p\beta - \theta \geq 0. \tag{4}$$

## 2.2 Asymmetric Information Sharing

In the network extension of his model, Stein (2008) considers a player who has accumulated $k$ signals from a prior exchange and must decide whether to initiate a new exchange with a third player. We frame this as a bilateral setting: Player $A$ starts with an initial

[3] And because both players are always willing to share, their beliefs about the other player's behavior are consistent with this equilibrium. Following Ganglmair et al. (2020), we use payoff dominance as an equilibrium selection criterion and argue that, conditional on the exchange having been initiated, relaxing the continuation condition results in the sharing equilibrium being selected more often.

[4] Ganglmair et al. (2020) find that higher competition reduces sharing rates in the experimental laboratory, consistent with this comparative static.

stock of $k \geq 1$ ideas, whereas player $B$ starts with $k_B = 0$. Player $A$ moves first (in $t = 1$) and decides whether to share its initial stock of ideas with $B$. If player $A$ shares, then from $t = 2$ onward the game proceeds exactly as before: The active player generates a new idea with probability $p$ and decides whether to share it. From then on, the *continuation condition* in (4) applies.

What changes with $k > 1$ is the *initiation* decision. If player $A$ conceals its initial $k$ ideas, the game ends and $A$'s payoffs are

$$conceal_{i1|k} = (1-\theta)\left(1-\beta^k\right) + \theta\left(\beta^0 - \beta^k\right) = 1 - \beta^k. \tag{5}$$

If, instead, player $A$ shares (and expects player $B$ to share in all subsequent rounds), then player $A$'s payoffs are

$$share_{i1|k} = (1-\theta)\sum_{m=0}^{\infty} p^m (1-p)\left(1-\beta^{k+m}\right) = (1-\theta)\left[1 - \frac{(1-p)\,\beta^k}{1-p\beta}\right]. \tag{6}$$

Player $A$ then shares in $t = 1$ if $share_{i1|k} \geq conceal_{i1|k}$. After some manipulation, we obtain the *initiation condition* summarized as follows:

**Condition 2** (Initiation)**.** *In $t = 1$, player $A$ initiates the exchange only if*

$$\psi(k) \equiv \frac{(1-\beta)\,p\beta^k}{(1-\beta p) - (1-p)\,\beta^k} - \theta \geq 0. \tag{7}$$

This $\psi(k)$ denotes the expected net payoff from sharing in $t = 1$ and decreases in $A$'s initial endowment $k$. A larger $k$ makes initiation more costly because player $A$ gives up a larger informational advantage. For $k = 1$, the *initiation condition* (7) reduces to the *continuation condition* (4), meaning that the decision to initiate is equivalent to the decision to continue. For $k > 1$, however,

$$\psi(k) < \psi(1) = \phi,$$

and the initiation decision is the binding constraint for collaboration. If the condition for initiating the exchange at $t = 1$ holds, then the condition for continuing the exchange will also hold. Conversely, if the continuation condition (4) holds, then the initiation condition is satisfied, and player $A$ will share in $t = 1$ as long as the initial stock of ideas (that is, the initial information advantage) is not too large:

**Proposition 1.** *Suppose the continuation condition (4) holds. Then there exists a $k^* \geq 1$ such that the initiation condition (7) is satisfied and player $A$ initiates the exchange for $k \leq k^*$, with*

$$k^* = \frac{\log\left(\dfrac{\theta\,(1-\beta p)}{(1-\beta)\,p + \theta\,(1-p)}\right)}{\log \beta}. \tag{8}$$

In our experimental implementation of this game, we will hold $p$, $\beta$, and $\theta$ constant (so as to satisfy the continuation condition) and vary $k$ to trigger (theoretical) violations of the initiation condition. As the initial asymmetry between the two players (captured by $k$) increases, the expected net payoff $\psi(k)$ from sharing in $t = 1$ decreases, and player $A$ is less likely to initiate the exchange. Once initiated, the exchange is expected to continue in the same way for any $k$ as the continuation condition is not a function of $k$. In the next section, we first introduce the empirical design and then formulate our hypotheses derived from this model.

# 3 Experimental Design

## 3.1 The Game in the Laboratory

We conducted the experiment at the Center and Laboratory for Behavioral Operations and Economics (CLBOE) at the University of Texas at Dallas, implementing the model from Section 2 in z-Tree (Fischbacher, 2007). A total of 118 subjects (undergraduate and graduate students) participated across five sessions, one treatment per session. In each session, subjects were randomly divided into two matching groups of equal size (12 subjects each, except one group of 10) and group membership was kept anonymous. Subjects were matched only with members of their own group following a round-robin protocol across 11 periods (9 periods for the group of 10). Each session lasted approximately 80 to 120 minutes. Subjects received a \$5 show-up fee plus their accumulated earnings from the experiment; total payments ranged from \$12.82 to \$27.53, averaging \$19.95.[5]

During the experiment, two subjects were matched as player $A$ and player $B$ and interacted over multiple rounds. In the experiment, we refer to player $A$ as fund manager/FM $A$ and to player $B$ as fund manager/FM $B$. In this paper, we will use the term "player" to refer to the subjects' role. In each round, the active player received a new idea with some probability and decided whether to share it with their matched partner. If the active player failed to generate an idea (determined by a computerized random draw) or chose not to share, the match ended. If the player shared, the match continued to the next round with the roles alternating. At the end of each match, subjects were informed about their earnings and were rematched with a different partner within their matching group, following a round-robin protocol.

Subjects observed the payoff-relevant parameters of their treatment and were informed about the number of initial ideas held by each player ($k_A$ and $k_B$). Before making their sharing decision, subjects reported their expectation (belief) about whether their partner would share in the subsequent round.[6]

## 3.2 Additional Subject Characteristics

At the beginning of each session, subjects completed a risk-preference task (Holt and Laury, 2002). From this task, we derive a *risk aversion* measure ranging from 1 to 10, with higher values indicating greater risk aversion.[7] After the main experiment, subjects completed an exit survey that included two items adapted from the World Values

[5]The first part of each session consisted of a Holt and Laury (2002) risk-preference task. The main experiment was preceded by detailed printed instructions and a comprehension quiz. The treatments analyzed in this paper were part of a larger experiment described in Ganglmair et al. (2020) (with a focus on treatments with asymmetric success probabilities and varying degrees of competition). See Ganglmair et al. (2020) for complete procedural details, including subject recruitment, the subject pool, and session logistics; see the Online Appendix for experimental instructions.

[6]See Ganglmair et al. (2020:fn 18) for why not incentivizing this belief elicitation should not generate a bias in the reported beliefs.

[7]Risk-aversion categories are consistent with those reported in Holt and Laury (2002). The majority of subjects are risk-averse: 76% chose the safe lottery at least five times, and the median subject chose it six times, implying a risk-aversion coefficient between 0.41 and 0.68 in a CRRA expected-utility framework.

Survey: a *fairness* item ("Do you think that most people would try to take advantage of you if they got a chance, or would they try to be fair?") and a *trustworthiness* item ("Generally speaking, would you say that most people can be trusted, or that you need to be very careful in dealing with people?"). Both are measured on a 10-point scale, with higher values indicating greater perceived fairness or trustworthiness, respectively. The survey also recorded each subject's *gender*. We include these four characteristics (i.e., risk aversion, fairness, trustworthiness, and gender) as subject-level controls in the regression analyses reported below.[8]

## 3.3 Treatments

We focus on four treatments that vary the initial stocks of ideas $k_A$ and $k_B$. We also include one baseline treatment. We label each treatment by its endowment pair [$k_A$-$k_B$]. All treatments share the same parameter values with symmetric success probabilities $p = 90\%$, diminishing returns parameter $\beta = 3/4$, and competition parameter $\theta = 3/8$.[9] For these parameter values, the critical threshold from Proposition 1 is $k^* = 2.67$. In Table 1, we summarize our treatments and indicate the status of the continuation condition (4) (satisfied in all treatments) and the initiation condition (7).

As our baseline treatment, Treatment [1-0] has an initial stock of ideas of $k_A = 1$ for player $A$ and $k_B = 0$ for player $B$. The continuation condition applies to all $t \geq 1$. In treatments [2-0] and [3-0], we increase player $A$'s initial stock of ideas and, by holding player $B$'s fixed at $k_B = 0$, we increase the asymmetry between the two players.

- *Treatment [2-0]* ($k_A = 2$, $k_B = 0$): The initiation condition (7) holds and the model predicts player $A$ will share its initial stock of ideas and thus initiate the exchange.

[8] Table A.1 shows that the subject pools are comparable across treatments. Risk aversion, fairness, and trustworthiness are balanced (joint MANOVA $p = 0.68$), and the gender composition differs marginally across sessions ($\chi^2$ test, $p = 0.08$), which the subject-level controls absorb.

[9] We also include a payoff multiplier of 400 for the baseline treatment and 290.91 for the other treatments to directly translate experimental outcomes into earnings; the baseline multiplier is larger because the [1-0] payoff range is smaller. This multiplier is strategy-neutral.

**Table 1:** Calibration and Treatments

| | | Initial stock $k_B$ for player $B$ | |
|---|---|---|---|
| | | *Treatment* | *Control* |
| | | $k_B = 0$ | $k_B = k_A - 1$ |
| **Initial stock $k_A$ for player $A$** | $k_A = 1$ | $\psi(1) = \phi > 0$ | — |
| | $k_A = 2$ | $\psi(2) > 0$ | $\psi(1) = \phi > 0$ |
| | $k_A = 3$ | $\psi(3) < 0$ | $\psi(1) = \phi > 0$ |

*Notes:* Calibration parameters are $p = 90\%$, $\beta = 3/4$, and $\theta = 3/8$. The critical threshold is $k^* = 2.67$. The continuation condition $\phi > 0$ is satisfied in all treatments for both players.

– *Treatment [3-0]* ($k_A = 3,\ k_B = 0$): The initiation condition is violated, and the model predicts player $A$ will not share its initial stock of ideas and thus not initiate the exchange.

Proposition 1 implies that, as $k$ increases (and $k_B = 0$), player $A$ is less likely to initiate the exchange in $t = 1$. However, comparing treatments [1-0], [2-0], and [3-0] will yield a confounded answer. By keeping $k_B = 0$, we increase both the asymmetry between the two players and the number of ideas with which the exchange is initiated. The latter also means that the first-round payoffs at stake (a scale effect) are higher. To isolate the effect of asymmetry from scale, we compare our two main treatments with two control treatments that keep player $A$'s initial stock constant while varying player $B$'s initial stock and thus the initial asymmetry.

– *Treatment [2-1]* ($k_A = 2,\ k_B = 1$): Player $A$'s initial advantage is $k_A - k_B = 1$. The initiation condition (7) applies with the gap in place of $k$ (Appendix A.2); it holds and is equal to the continuation condition (4). The model predicts player $A$ will share its initial stock of ideas and thus initiate the exchange.

– *Treatment [3-2]* ($k_A = 3,\ k_B = 2$): Player $A$'s initial advantage is $k_A - k_B = 1$. The initiation condition (7) holds and is equal to the continuation condition (4). The model predicts player $A$ will share its initial stock of ideas and thus initiate the exchange.

Comparing [2-1] with [2-0], we increase the initial asymmetry from 1 to 2 while keeping player $A$'s payoff level constant; comparing [3-2] with [3-0], we increase the initial asymmetry from 1 to 3. In all five treatments, the continuation condition (4) is satisfied for both players. That means, conditional on initiation in $t = 1$, both players have an incentive to continue sharing in all subsequent rounds.

### 3.4 Predictions

Our first two hypotheses follow directly from our model discussion in Section 2. We state them in terms of the initial "gap" of ideas. When player $B$ starts with $k_B = 0$ (as in treatments [2-0] and [3-0]), player $A$'s initial stock of ideas is the same as the gap.

**Hypothesis 1** (Initiation)**.** *Player $A$ is more likely to share its initial ideas in Round 1 when the initial gap is $k \leq k^*$, and less likely when $k > k^*$. More specifically:*

*(a) Player $A$'s sharing in Round 1 of treatment [2-0] is not different from its sharing in treatment [2-1].*

*(b) Player $A$ shares less in Round 1 of treatment [3-0] than in treatment [3-2].*

This first hypothesis follows directly from Proposition 1. The second hypothesis is a corollary of the continuation condition (4): Once the exchange has been initiated, the incentives to share no longer depend on $k$ and are the same across all treatments.

**Hypothesis 2** (Continuation)**.** *Conditional on player $A$ sharing in Round 1, the sharing behavior in all subsequent rounds ($t \geq 2$) does not depend on the initial gap $k$.*

## 4 Results

### 4.1 Summary of the Laboratory Sessions

Table 2 presents summary statistics for the five treatments. We include the baseline [1-0] for comparison. The average number of rounds played is highest in treatment [3-2] (7.64 rounds) and lowest in treatment [3-0] (5.07 rounds). The baseline [1-0] averages

**Table 2:** Summary Statistics

| | Treatment $[k_A\text{-}k_B]$ | | | | |
|---|---|---|---|---|---|
| | 1-0 | 2-0 | 2-1 | 3-0 | 3-2 |
| Subjects | 24 | 22 | 24 | 24 | 24 |
| Matches | 132 | 111 | 132 | 132 | 132 |
| Average # of rounds | 5.63 | 5.94 | 5.14 | 5.07 | 7.64 |
| Average earnings (in $) per match … | | | | | |
| …for all subjects | 1.57 | 1.24 | 1.24 | 1.29 | 1.54 |
| …for player $A$ | 1.58 | 1.36 | 1.28 | 1.59 | 1.55 |
| …for player $B$ | 1.57 | 1.13 | 1.20 | 0.99 | 1.53 |
| Percentage of matches terminated by chance … | | | | | |
| …because player $A$ has failed | 27% | 23% | 20% | 27% | 37% |
| …because player $B$ has failed | 27% | 24% | 22% | 23% | 36% |
| Percentage of matches terminated by choice … | | | | | |
| …by player $A$ | 21% | 32% | 37% | 44% | 16% |
| …by player $B$ | 25% | 21% | 20% | 7% | 11% |

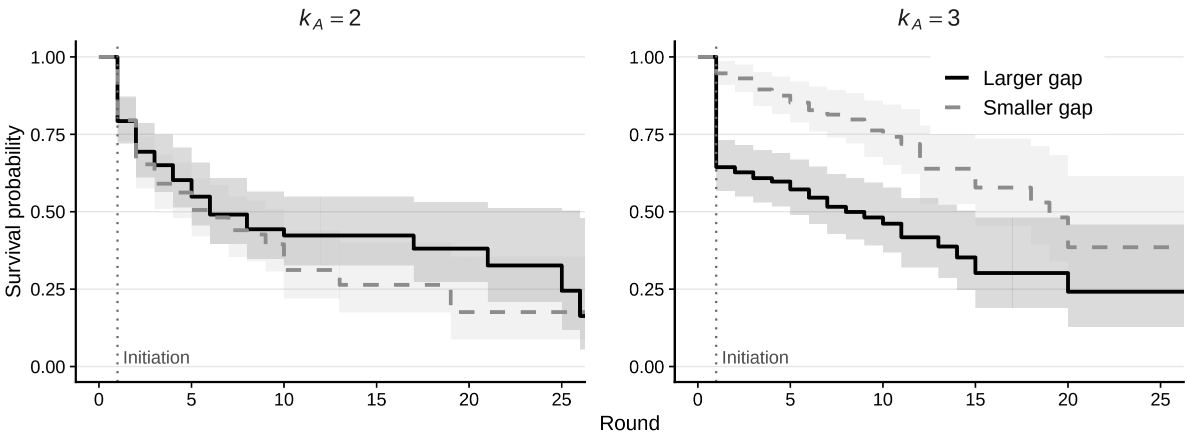
*Notes:* This table provides basic summary statistics for five treatments of the experiment. Treatment [1-0] is the baseline with symmetric information sharing ($k_A = 1$, $k_B = 0$) originally reported in Ganglmair et al. (2020); treatments [2-1], [2-0], [3-2], and [3-0] vary the initial stocks of ideas $k_A$ and $k_B$. Treatment labels indicate $[k_A\text{-}k_B]$. All treatments use $p_A = p_B = 90\%$, $\theta = 3/8$, and $\beta = 3/4$. We list the number of subjects per treatment; the number of matches; the average number of rounds per match; the average earnings per match (in U.S. dollars); and the percentage of matches terminated by chance or by choice. For the baseline treatment [1-0], the payoff multiplier is 400; for the other treatments, it is 290.91. Rescaled by 290.91/400, average earnings per match in [1-0] are 1.14 (all subjects), 1.15 (player $A$), and 1.14 (player $B$), comparable to the other treatments.

5.63 rounds, placing it between the two $k_A = 2$ treatments: [2-1] (5.14 rounds) and [2-0] (5.94 rounds). Matches are terminated by player $A$'s choice more often in treatment [3-0] (44%) than in treatment [3-2] (16%), a difference driven largely by player $A$ not initiating in Round 1 (35.6% of matches in [3-0] compared to 5.3% in [3-2]) and consistent with player $A$ being less willing to initiate when the endowment gap is large. In the baseline [1-0], termination by player $A$ (21%) and player $B$ (25%) is roughly balanced, whereas in the other four treatments, player $A$ terminates more frequently than player $B$.

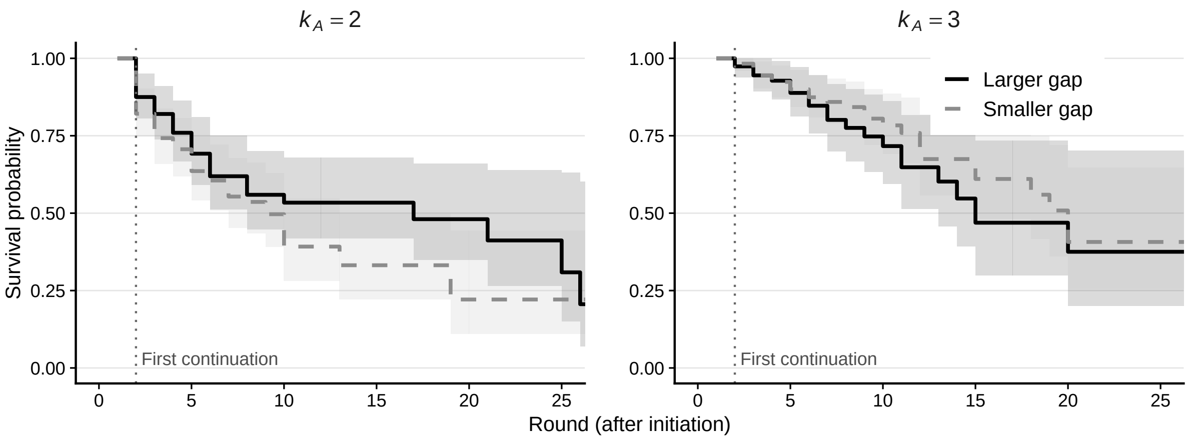
To examine subjects' decision making across the game continuum, Figure 2 presents Kaplan-Meier survival curves for the four asymmetric treatments, with chance termination treated as right-censoring.[10] For $k_A = 3$, the survival curves diverge sharply: Matches

[10] A match ends by chance when the active player fails to generate a new idea, which occurs with probability $1 - p = 10\%$ in each round, regardless of sharing behavior. Treating these terminations as censored events allows the survival function to estimate the probability of continued collaboration in the absence of the exogenous stopping rule.

**Figure 2:** Kaplan-Meier Survival Probabilities by Treatment



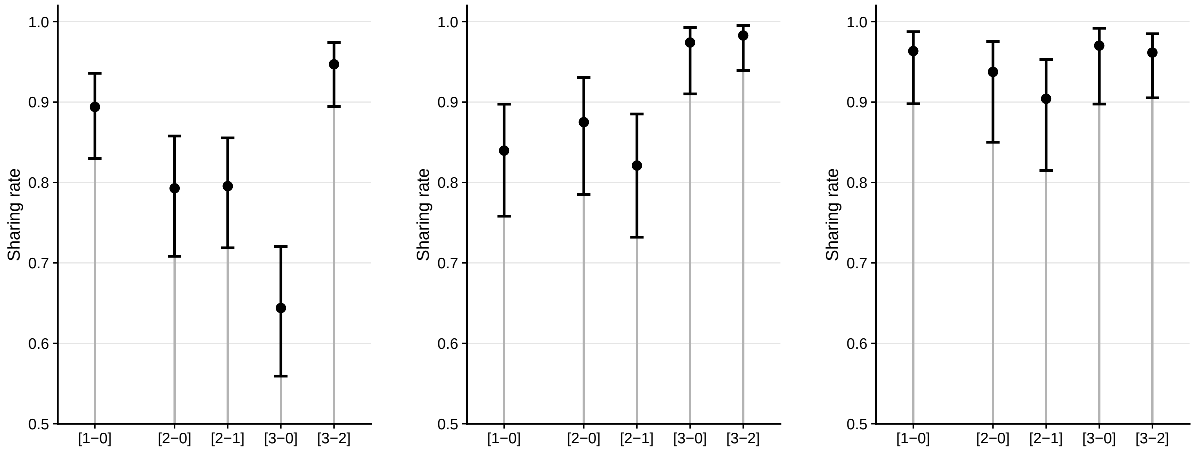

**(a)** Kaplan-Meier Survival Probabilities




**(b)** Survival Probabilities, Conditional on Round-1 Survival

*Notes:* Each curve plots the probability that a match survives (neither player defects) through a given round. Shaded bands are pointwise 95% confidence intervals. The top panel (a) shows unconditional Kaplan-Meier survival probabilities; the bottom panel (b) is conditional on initiation in Round 1. The left panels show the $k_A = 2$ treatments; the right panels show the $k_A = 3$ treatments. Within each panel, the darker series corresponds to the treatment with the larger endowment gap (treatments [2-0] and [3-0]); the lighter series corresponds to the treatment with the smaller gap (treatments [2-1] and [3-2]). Matches terminated by chance (i.e., a player fails to generate a new idea, which occurs with probability $1 - p = 10\%$ in each round) are treated as right-censored.

**Figure 3:** Sharing Rates by Treatment and Round




**(a)** Round 1 (Player $A$) **(b)** Round 2 (Player $B$) **(c)** Round 3 (Player $A$)

*Notes:* Dots show treatment means. Whiskers show (asymmetric) 95% Wilson score confidence intervals. Panel (a) shows player $A$'s Round 1 sharing rate (initiation). Panels (b) and (c) show continuation sharing rates in Rounds 2 and 3, conditional on the match surviving to that round.

in the larger-gap treatment [3-0] drop well below those in the smaller-gap treatment [3-2], driven by a large initial drop at Round 1. For $k_A = 2$, the two curves track each other closely throughout; a log-rank test (Section 4.3) detects no difference in match survival between treatments [2-0] and [2-1].

Conditional on player $A$ sharing in Round 1, the survival curves within each $k_A$ overlap: The endowment gap that sharply separates the unconditional survival curves has no bearing on survival once collaboration is initiated. This pattern is consistent with the theoretical prediction that the continuation condition depends on the idea-generation probability $p$ and the degree of competition $\theta$ but not on the endowment gap.

## 4.2 Initiation

Figure 3 reports sharing rates for player $A$ in Rounds 1 and 3, and for player $B$ in Round 2.[11] In treatments [2-1] and [2-0], player $A$'s sharing rates are nearly identical: 79.6% and 79.3%, with a difference that is not statistically significant ($p = 0.873$; an equivalence test bounds the difference at 16 percentage points, Table A.2), consistent with

[11] Table A.2 also reports player $A$'s average sharing rate in Round 1 for each treatment, along with pairwise $t$-tests.

**Table 3:** Probit Regression Results for Initiation (Hypothesis 1)

| | Dependent variable = 1 if player $A$ shares in Round 1; = 0 otherwise | | | | | |
|---|---|---|---|---|---|---|
| | (I) | | (II) | | (III) | |
| | Probit | ME | Probit | ME | Probit | ME |
| Expectations $\alpha_B$ | 0.0252*** | 0.0050*** | | | 0.0265*** | 0.0047*** |
| | (0.0036) | (0.0005) | | | (0.0038) | (0.0006) |
| Endowment Gap | | | −0.5066*** | −0.1257*** | −0.6300*** | −0.1104*** |
| | | | (0.1553) | (0.0347) | (0.1575) | (0.0291) |
| Initial Ideas | | | 0.1239 | 0.0308 | 0.1806 | 0.0317 |
| | | | (0.1627) | (0.0401) | (0.1578) | (0.0279) |
| Subject controls | ✓ | | ✓ | | ✓ | |
| Observations | 639 | | 639 | | 639 | |
| pseudo $R^2$ | 0.2585 | | 0.0673 | | 0.3277 | |
| Log-likelihood | −226.665 | | −285.121 | | −205.505 | |

*Notes:* We report probit results for treatments [2-1], [2-0], [3-2], and [3-0] in support of Hypothesis 1. We also add the baseline treatment [1-0]. The dependent variable is a dummy variable = 1 if player $A$ shares in Round 1 and = 0 otherwise. *Expectations* $\alpha_B$ is player $A$'s expectation that player $B$ will share a new idea in Round 2; *Endowment Gap* is the difference $k_A - k_B$; and *Initial Ideas* is player $A$'s initial stock $k_A$. All specifications include subject-level controls (Holt-Laury risk preference, gender, fairness attitudes, trust attitudes), not reported. Reported marginal effects (ME) are average marginal effects. Standard errors clustered at the subject level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Hypothesis 1(a). In the $k_A = 3$ treatments, the difference is large and highly significant: Player $A$ shares at a rate of 94.7% in treatment [3-2] but only 64.4% in treatment [3-0] ($p < 0.01$; Hypothesis 1(b)).[12] Taken together, these results confirm Hypothesis 1: A larger endowment gap reduces the willingness to initiate, and it does so only once the gap exceeds the theoretical threshold. The model's point prediction for treatment [3-0] is starker than the data: With the initiation condition violated, no player $A$ should share, yet 64.4% do. The model is deliberately simple, and we read its value in the comparative statics it identifies (the sharp drop in initiation once the gap exceeds the threshold) rather than in its point predictions of behavior.

Table 3 presents probit regression results for the initiation decision. The depen-

[12]These initiation results are robust to the most conservative feasible inference. Treatment is assigned at the session level, and the finest independent unit of observation is the matching group, of which there are two per treatment. A restricted wild cluster bootstrap at the matching-group level (10 clusters, Webb six-point weights) yields $p = 0.056$ for the [3-0] vs. [3-2] difference and $p = 0.885$ for the [2-0] vs. [2-1] difference, consistent with Hypothesis 1 (Cameron et al., 2008; MacKinnon and Webb, 2018). With only two matching groups per treatment, bootstrap $p$-values below 0.05 are nearly impossible to obtain by construction.

dent variable is a dummy equal to one if player $A$ shares in Round 1. We include three explanatory variables: player $A$'s expectations that player $B$ will share in Round 2 (*Expectations* $\alpha_B$) (Ganglmair et al., 2020), the endowment gap (*Endowment Gap*, $k_A - k_B$), and the number of player $A$'s initial ideas (*Initial Ideas*, $k_A$). All specifications include subject-level controls (risk preference, gender, fairness, and trust attitudes), and standard errors are clustered at the subject level to account for repeated observations within subjects across periods. The pooled probit provides a parametric summary of the within-$k_A$ comparisons that form the primary tests of Hypothesis 1. By entering the endowment gap and the number of initial ideas as separate regressors, the specification controls for the scale effect that the within-$k_A$ design eliminates by construction.

The results show that expectations and the endowment gap (that is, the initial asymmetry between the two players) are highly significant. Player $A$'s expectation of reciprocity by player $B$ is a strong predictor of initiation (see, e.g., Ganglmair et al., 2020), with a marginal effect of 4.7 percentage points per 10-percentage-point increase in the expected sharing probability ($p < 0.01$). As stated in Proposition 1 and captured in Hypothesis 1, the endowment gap has a negative and significant marginal effect. The treatment effect of the gap is the estimate in column (II), which includes only the design variables: An additional unit of $k_A - k_B$ reduces the probability of sharing by 12.6 percentage points ($p < 0.01$).[13] Column (III) adds player $A$'s expectations, a variable the treatment itself may move; the gap's effect net of this belief channel remains at 11.0 percentage points ($p < 0.01$), so the gap operates only partly through expectations. Lastly, the number of initial ideas, $k_A$, has a positive but insignificant marginal effect. Controlling for the gap, a higher baseline stock increases the expected value of the collaboration as a whole (that is, a scale effect).[14]

[13]For simplicity, the model assumes that player $B$ always has 0 initial ideas. We experiment with treatments in which player $B$ has more than 0 ideas so that we have appropriate comparison groups for our increasingly asymmetric treatment groups. Appendix A.2 shows that the initiation condition (7) extends to $k_B > 0$ with the gap $k_A - k_B$ in place of $k$, so the threshold $k^*$ applies to the gap.

[14]These results are robust to modeling subject-level heterogeneity directly: A random-effects probit with a subject-level random intercept (Table 5) yields the same sign and significance pattern for all three variables. They are also robust to controlling for experience: Adding the match number (1 through 11) to the specifications leaves the gap coefficient unchanged, and the experience term itself is insignificant ($p \geq 0.18$).

## 4.3 Continuation

### 4.3.1 Simple Mean Comparisons

Hypothesis 2 predicts that sharing in rounds $t \geq 2$ does not depend on the initial stock of ideas. Figure 3, Panels (b) and (c), report average sharing rates for player $B$ in Round 2 and player $A$ in Round 3.[15] As a benchmark, the figure includes the baseline treatment [1-0], where player $B$'s Round 2 sharing rate is 84.0% and player $A$'s Round 3 sharing rate is 96.3%. The results support our core prediction. None of the pairwise differences within $k_A$ groups are statistically significant. In Round 2, player $B$'s sharing rate ranges from 82.1% (treatment [2-1]) to 98.3% (treatment [3-2]), but the within-$k_A$ differences ([2-1] vs. [2-0] ($p = 0.267$) and [3-2] vs. [3-0] ($p = 0.518$)) are small and insignificant. Similarly, in Round 3, player $A$'s sharing rate is uniformly high across all treatments (90.4% to 97.0%) with no significant differences between groups. Comparisons with the baseline treatment [1-0] confirm this pattern for Round 3: The sharing rates in the asymmetric treatments are not significantly different from the baseline rate of 96.3%. In Round 2, however, player $B$'s sharing rates in the $k_A = 3$ treatments are 11 to 13 percentage points higher than in the baseline, although only the difference for [3-2] is marginally significant at the subject level ($p = 0.055$; [3-0]: $p = 0.104$). The pattern is consistent with player $B$ responding to the absolute level of player $A$'s initial contribution rather than to the costliness of the sacrifice, a distinction we establish in Table 6.

These findings support the model's prediction that the continuation incentive $\phi$ is independent of the initial endowment gap: all within-group comparisons of sharing rates are insignificant. Equivalence tests sharpen this null (Lakens, 2017): In the $k_A = 3$ treatments, the Round-2 difference between [3-0] and [3-2] is significantly smaller than 10 percentage points in either direction (TOST $p < 0.01$; the 90% confidence interval rules out differences larger than 5.8 points), and the Round-3 difference is significantly smaller than 15 points. The $k_A = 2$ comparisons are less precisely estimated, with equivalence bounds of 17 to 20 percentage points (Tables A.3 and A.4).

[15] Table A.3 reports average sharing rates and pairwise $t$-tests for Round 2 (player $B$) and Table A.4 reports those results for Round 3 (player $A$).

### 4.3.2 Regression Results from Survival Models

Returning to the Kaplan-Meier survival probabilities in Figure 2, we can trace round-level behavior over the entire course of a match. In the $k_A = 2$ panel, the curves for treatments [2-0] and [2-1] are nearly indistinguishable. A log-rank test cannot reject equality ($\chi^2 = 0.87$, $p = 0.35$).[16] In the $k_A = 3$ panel, the curves diverge sharply. Treatment [3-2] (gap = 1) maintains substantially higher survival than treatment [3-0] (gap = 3) at every horizon, and the log-rank test rejects equality ($\chi^2 = 22.5$, $p < 0.001$). The sharp early drop in the [3-0] curve reflects the initiation failures documented in Table A.2. That is, many matches in this treatment end in Round 1 because player $A$ chooses not to share. However, if we condition on initiation, the results converge ($\chi^2 = 0.37$, $p = 0.54$; conditioning on $t \geq 2$) and we fail to reject equality. Thus, as long as the game reaches Round 2 (player $A$ shares in Round 1), we see no evidence that continuation depends on the initial stock of ideas (Hypothesis 2).[17]

The treatment-level comparisons and survival curves document aggregate patterns but cannot distinguish between the initiation margin and continuation dynamics. To isolate the continuation margin, we estimate a discrete-time hazard model at the round level. The unit of observation is a player-round decision within a match that was initiated (player $A$ shared in Round 1). The dependent variable is defection: an indicator equal to one if the deciding player stops sharing at that round. Chance terminations are right-censored. We use the complementary log-log link, which corresponds to a grouped-duration proportional hazards model.[18]

Table 4 reports the estimates. All specifications include subject-level controls for the deciding player, and standard errors are clustered at the subject level. The sample consists of 3,118 round-level observations from 474 initiated matches that survived to

[16] Log-rank tests treat the matches as independent survival times, although each subject contributes five to six matches. We therefore read these tests as descriptive. The discrete-time hazard model below, with standard errors clustered at the subject level, carries the formal inference on continuation.

[17] Conditioning on initiation raises a potential selection concern, particularly in treatment [3-0] where only 64% of player $A$ subjects initiate. We address this in Section 5.2.

[18] The complementary log-log link arises naturally from grouping a continuous-time proportional hazards model into discrete intervals. It is preferred over the logit link when the underlying process is continuous but observed at discrete intervals (see Jenkins, 1995).

**Table 4:** Discrete-Time Hazard Model: Round-Level Defection

| | Dependent variable: Defection (0/1) | | |
|---|---|---|---|
| | (I) | (II) | (III) |
| Endowment Gap | 0.0518 | −0.0446 | −0.0359 |
| | (0.1957) | (0.2011) | (0.1999) |
| Initial Ideas | −0.4481*** | −0.4432*** | −0.4518*** |
| | (0.1640) | (0.1695) | (0.1708) |
| Round | −0.0057 | 0.0256 | 0.0288* |
| | (0.0170) | (0.0159) | (0.0166) |
| Expectations $\alpha_j$ | | −0.0494*** | −0.0496*** |
| | | (0.0040) | (0.0039) |
| Partner Expectations (lag) | | | −0.0046 |
| | | | (0.0050) |
| Subject controls | ✓ | ✓ | ✓ |
| Observations | 3118 | 3118 | 3118 |
| Log-likelihood | −666.99 | −495.94 | −495.27 |

*Notes:* We report estimates from a discrete-time proportional hazards model for treatments [1-0], [2-1], [2-0], [3-2], and [3-0]. The unit of observation is a player-round decision within initiated matches (player $A$ shared in Round 1). The sample conditions on initiation and begins at Round 2. The dependent variable equals one if the deciding player defects. Chance terminations are right-censored. All models use the complementary log-log link. *Endowment Gap* is $k_A - k_B$; *Initial Ideas* is player $A$'s initial stock $k_A$; *Expectations* $\alpha_j$ is the deciding player's expectation of their partner's sharing, elicited at the current round; *Partner Expect. (lag)* is the partner's most recently stated expectation, carried forward. All specifications include subject-level controls for the deciding player (Holt-Laury risk preference, gender, fairness attitudes, trust attitudes), not reported. Standard errors clustered at the subject level in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

at least Round 2.[19] Column (I) includes only the endowment gap, initial ideas, and the round number. The gap is insignificant, while initial ideas substantially reduce the hazard of defection ($p < 0.01$). Payoffs are strictly increasing in the number of ideas, so the negative coefficient on initial ideas is consistent with a scale effect on defection: A larger initial stock may make the payoff scale salient to players from an earlier round. The round variable is insignificant, indicating no baseline duration dependence in the defection hazard. This is the central result: Conditional on initiation, the endowment gap does not predict defection at the round level. The continuation condition is independent of the gap, exactly in line with our prediction in Hypothesis 2.

Column (II) adds the deciding player's current-round expectation of their partner's

[19] Of the 521 matches in which player $A$ shared in Round 1, 47 were terminated by the random-termination mechanism before Round 2 and contribute no continuation observations.

sharing. The coefficient is large, negative, and highly significant ($p < 0.001$): Each percentage-point increase in the player's expectation about their partner reduces the hazard of defection. Including expectations substantially improves the model fit (log-likelihood improves from $-667$ to $-496$). This confirms that round-level beliefs about reciprocity are the dominant predictor of continuation decisions (Ganglmair et al., 2020). The gap remains insignificant.

Column (III) includes the partner's most recently stated expectation as a proxy for the partner's cooperative disposition. Players do not observe their partners' expectations; rather, they must anticipate a partner's type based only on whether the partner shared in previous rounds. Under a rational expectations framework, this information *should* be sufficient to determine a partner's type. However, as we document in the following section, it is not clear that subjects can reliably determine a partner's type, despite a relatively clear division of types in the data. Here, the coefficient on the partner's lagged expectation is negative but insignificant ($p = 0.36$), and the other estimates remain stable, suggesting that inferring a partner's type does not significantly affect sharing behavior. Across all specifications, the endowment gap is insignificant, and its economic magnitude is small relative to the expectations effect. The continuation condition tracks beliefs, not the structural asymmetry of the initial endowment.

# 5 Expectations and Joint Surplus

## 5.1 Player Type and Expectations

Players' anticipation of each other's disposition toward sharing within a given match raises the broader question of a player's general disposition toward sharing, or "type". The pooled probit in Table 3 treats each subject-period observation as drawn from a common latent index, with clustering only correcting the standard errors for within-subject correlation. This addresses inference but does not model the source of that correlation. If subjects differ persistently in their willingness to initiate (i.e., some are inherently cooperative, others inherently reluctant, independent of the treatment variables), the pooled probit conflates this individual heterogeneity with the structural effects of expectations, the endowment gap, and the initial stock. The random-effects probit adds a subject-level random intercept to separate the two: It explicitly models the persistent individual component, so the coefficients on the structural variables reflect what drives initiation within a subject type rather than across a heterogeneous pool of individuals.

The random-effects probit in Table 5 confirms that the sign and significance of all three structural variables are unchanged. Expectations remain the dominant predictor of initiation ($p < 0.001$), the endowment gap is negative and highly significant ($p < 0.001$), and the number of initial ideas is positive but insignificant. The random-effects coefficients are larger in magnitude than the pooled estimates, as expected when unobserved heterogeneity attenuates the pooled probit, but the qualitative conclusions are identical. However, the variance decomposition reveals the scale of individual heterogeneity: $\hat{\sigma}_u$ ranges from 1.31 to 1.60 across specifications, and the intra-class correlation $\rho$ (the share of latent variance attributable to persistent subject-level differences) lies between 0.63 and 0.72. Most of the variation in initiation decisions is driven by who the subject is, not just what treatment they face.

The BLUPs (best linear unbiased predictions) extracted from the RE probit give us a subject-level estimate of each individual's persistent deviation from the population-average propensity to initiate, after controlling for expectations, the gap, and initial ideas. We use these to classify subjects as high type ($\hat{u}_i \geq 0$) or low type ($\hat{u}_i < 0$) and then decompose the treatment effects from the main analysis by type. The plot in Figure 4(a) shows that the distribution of types has a clear break near zero, with a mass of always-sharers whose BLUPs are shrunk toward zero and a dispersed tail of reluctant sharers with large negative BLUPs. The initiation-by-type figure (Figure 4(c)) reveals that the treatment effects on initiation are concentrated among low types: In treatment [3-0], low types initiate at only 37%, whereas high types initiate at rates of about 90% or more in every treatment, including [3-0]. This decomposition sharpens the paper's main result: The endowment gap does not reduce initiation across the board; it selectively deters those already reluctant to share.

**Table 5:** Random-Effects Probit Regression Results for Initiation

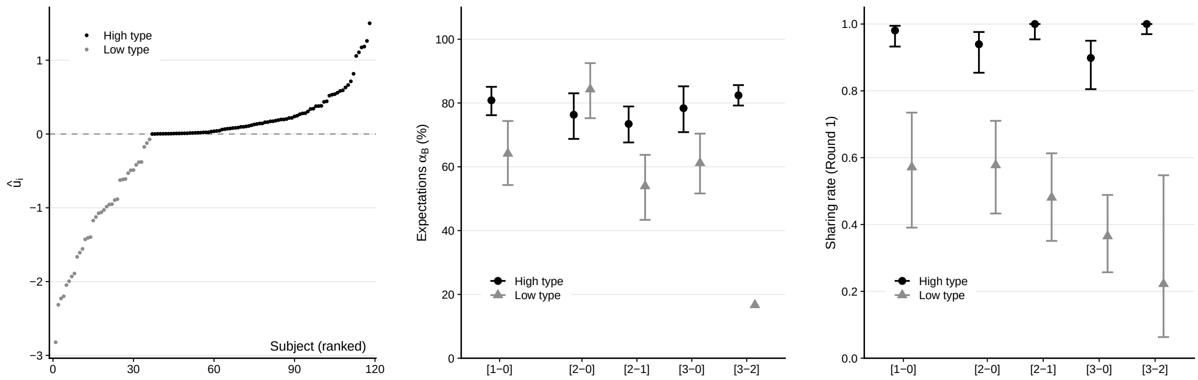
| | Dependent variable = 1 if player $A$ shares in Round 1; = 0 otherwise | | | | | |
|---|---|---|---|---|---|---|
| | (I) | | (II) | | (III) | |
| | Probit | ME | Probit | ME | Probit | ME |
| Expectations $\alpha_B$ | 3.8207*** | 0.4050*** | | | 3.6595*** | 0.3920*** |
| | (0.4468) | (0.0580) | | | (0.4311) | (0.0482) |
| Endowment Gap | | | −1.2266*** | −0.1437*** | −1.0860*** | −0.1163*** |
| | | | (0.2835) | (0.0330) | (0.2795) | (0.0288) |
| Initial Ideas | | | 0.4040 | 0.0473 | 0.4008 | 0.0429 |
| | | | (0.2962) | (0.0368) | (0.3013) | (0.0324) |
| Subject controls | ✓ | | ✓ | | ✓ | |
| $\hat{\sigma}_u$ | 1.5520 | | 1.6027 | | 1.3123 | |
| $\hat{\rho}$ | 0.7066 | | 0.7198 | | 0.6327 | |
| Observations | 639 | | 639 | | 639 | |
| Subjects | 118 | | 118 | | 118 | |
| Log-likelihood | −180.821 | | −224.406 | | −171.164 | |

*Notes:* We report random-effects probit results (with a subject-level random intercept) for treatments [1-0], [2-1], [2-0], [3-2], and [3-0], estimated by maximum likelihood (Laplace approximation). The dependent variable is a dummy variable = 1 if player $A$ shares in Round 1 and = 0 otherwise. *Expectations* $\alpha_B$ is player $A$'s expectation that player $B$ will share a new idea in Round 2, rescaled to [0,1] (dividing by 100) for numerical stability (a unit increase corresponds to moving from 0% to 100% expected sharing); *Endowment Gap* is the difference $k_A - k_B$; and *Initial Ideas* is player $A$'s initial stock $k_A$. All specifications include subject-level controls (Holt-Laury risk preference, gender, fairness attitudes, trust attitudes), not reported. Reported marginal effects (ME) are average marginal effects, with standard errors computed by the delta method. Model-based maximum-likelihood standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Expectations by type reveal how optimism interacts with the initiation margin (Figure 4(b), together with the belief dynamics in Figure 5). Below the threshold, the optimism gap between the high-asymmetry treatment and its control is a low-type phenomenon: Among the subjects least inclined to share, Round-1 expectations in [2-0] exceed those in [2-1] by 26.6 percentage points ($p < 0.05$), whereas high types hold similar beliefs in the two treatments (a difference of 2.9 points, $p = 0.77$). Indeed, [2-0] is the only treatment in which low types are, if anything, more optimistic than high types. Reluctant players initiate only when they expect reciprocity, so the low types who share despite the larger sacrifice are precisely the optimists among them. One interpretation is that these players view costly sharing as a signal of their good intentions.

Above the threshold, aggregate Round-1 expectations in [3-0] fall short of those

**Figure 4:** Types of Player $A$




**(a)** Player $A$ Types **(b)** Expectations by Type **(c)** Initiation by Type

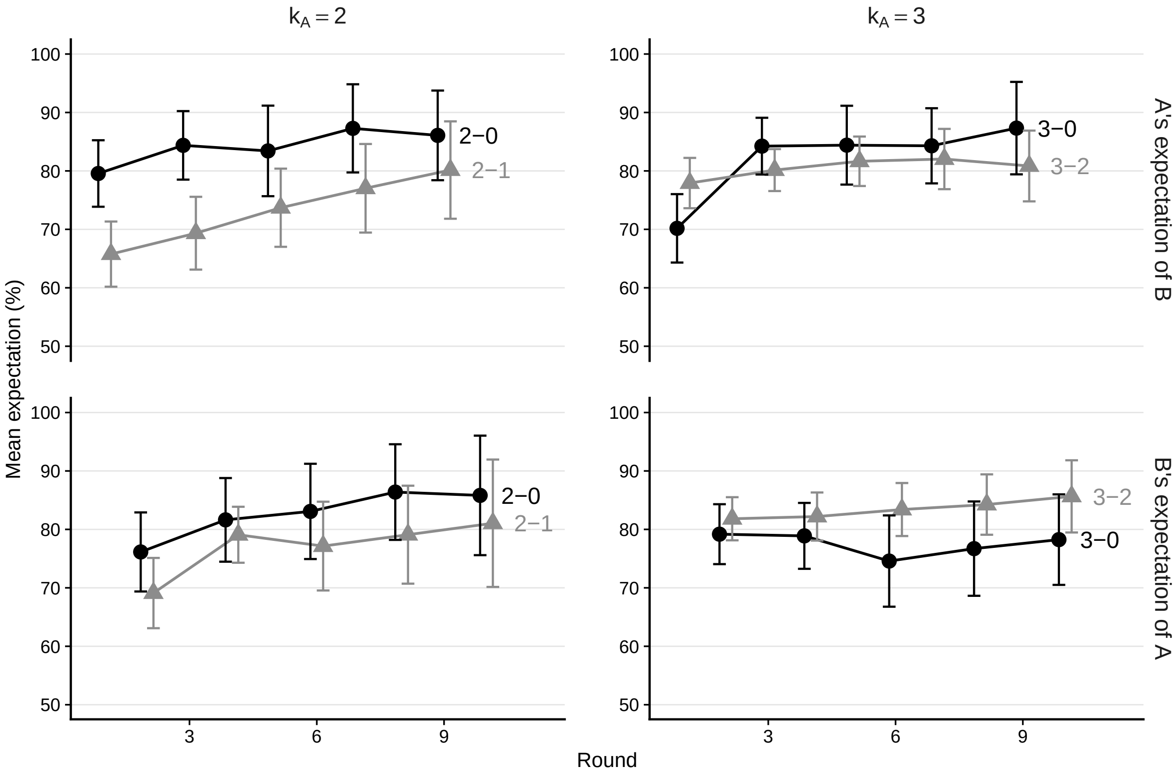
*Notes:* Subject types are classified using best linear unbiased predictions (BLUPs) of the subject-level random intercept from the random-effects probit in Table 5 (Column III). Subjects with a positive random intercept are classified as high type; subjects with a negative intercept as low type. Panel (a) plots the ranked intercepts. Panel (b) plots player $A$'s Round 1 expectations by type (95% bootstrap percentile intervals with 2,000 replicates); panel (c) plots player $A$'s initiation decision by type (asymmetric 95% Wilson score confidence intervals). The number of subjects per type and treatment ranges from 5 to 23, except for the low-type cell in treatment [3-2], which contains a single subject.

in [3-2] by 8.8 points ($p = 0.20$). This difference reflects composition rather than within-type differences: High types hold similar beliefs in the two treatments (a difference of 4.3 points, $p = 0.53$), but 23 of the 24 player-$A$ subjects in [3-2] are high types, whereas [3-0] retains 11 low types (of 24) whose expectations lie 16 points below those of their high-type counterparts. Where initiation is theoretically doomed, the reluctant types are pessimistic, and initiation rests on the optimism of the high types.

Whatever signal an initiating player $A$ intends to send, player $B$ does not appear to receive it.[20] In the $k_A = 2$ group, the gap in player $B$'s expectations between treatments [2-0] and [2-1] is small and insignificant at every round (largest difference: 7.3 percentage points at Round 8, $p = 0.23$). In the $k_A = 3$ group, treatment [3-2] subjects hold slightly higher expectations than treatment [3-0] subjects, but the difference is at most marginally significant (Round 6: 8.8 percentage points, $p = 0.06$). Player $B$'s expectations are thus sufficiently similar across treatments that they do not suggest a signal

[20] This may be a key difference between real-world collaboration and the interaction we allow in the experiment. In the experiment, partners were anonymous, and no direct communication was possible. Indirect signals may have been too noisy or too difficult to interpret. A practical implication of this research is that overcoming a large endowment gap will require significant communication alongside intentional behavior.

**Figure 5:** Expectations of Reciprocity over Rounds, by Treatment




*Notes:* Each point is the mean elicited expectation (in percent) that the rival will share a new idea in the next round, plotted by treatment and round. The top row shows player $A$'s expectations of player $B$ ($\alpha_B$, elicited before odd rounds); the bottom row shows player $B$'s expectations of player $A$ ($\alpha_A$, elicited before even rounds). Columns separate the $k_A = 2$ treatments (left) from the $k_A = 3$ treatments (right). Error bars are 95% confidence intervals. Within each panel, the darker series corresponds to the treatment with the larger endowment gap. Sample sizes decline over rounds because matches end when a player defects or fails to generate a new idea; means in later rounds are therefore conditional on the match having survived to that point.

of intent has been reliably transmitted between parties, a result supported by the probit results in Table 6, where we see that the endowment gap is an insignificant predictor of player $B$'s sharing.

## 5.2 Selection in the Continuation Sample

The BLUP-type classification addresses a potential selection concern in the continuation analysis. In treatment [3-0], only 64% of player $A$ subjects initiate, and the BLUP analysis shows that the subjects who do not initiate are disproportionately low types. The continuation sample in this treatment is therefore selected on initiation propensity. To assess whether this selection threatens the independence-of-gap result, we add player $A$'s

**Table 6:** Probit Regression Results for Round 2 Continuation by Player $B$

| | Dependent variable = 1 if player $B$ shares in Round 2; = 0 otherwise | | | | | |
|---|---|---|---|---|---|---|
| | (I) | | (II) | | (III) | |
| | Probit | ME | Probit | ME | Probit | ME |
| Expectations $\alpha_A$ | 0.0238*** | 0.0032*** | | | 0.0231*** | 0.0030*** |
| | (0.0038) | (0.0006) | | | (0.0037) | (0.0005) |
| Endowment Gap | | | 0.0476 | 0.0076 | 0.0380 | 0.0049 |
| | | | (0.1668) | (0.0268) | (0.1997) | (0.0258) |
| Initial Ideas | | | 0.4488*** | 0.0720** | 0.3739** | 0.0486** |
| | | | (0.1539) | (0.0282) | (0.1648) | (0.0227) |
| Subject controls | ✓ | | ✓ | | ✓ | |
| Observations | 474 | | 474 | | 474 | |
| pseudo $R^2$ | 0.2597 | | 0.1014 | | 0.2898 | |
| Log-likelihood | −115.044 | | −139.647 | | −110.375 | |

*Notes:* The sample is restricted to initiated matches that survived to Round 2 ($N = 474$). The dependent variable is an indicator for player $B$ sharing in Round 2. *Expectations* $\alpha_A$ is player $B$'s elicited expectation of player $A$'s sharing in Round 3; *Endowment Gap* is $k_A - k_B$; *Initial Ideas* is player $A$'s initial stock $k_A$. All specifications include player $B$'s subject-level controls (Holt-Laury risk preference, gender, fairness attitudes, trust attitudes; not reported). Clustered standard errors at the player $B$ subject level in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

BLUP-defined type to the discrete-time hazard model from Table 4. The results (Table A.7) show that type does predict continuation (high types defect less, $p < 0.05$), but the endowment gap remains insignificant ($p > 0.62$) across all specifications. Because the RE probit includes the gap as a regressor, the BLUPs capture residual subject heterogeneity net of treatment effects; empirically, the subject-level BLUPs are uncorrelated with the gap ($\rho = 0.03$, $p = 0.73$). In the treatments with an endowment gap of one idea, where selection into continuation is minimal, and both types initiate ([1-0], [2-1], and [3-2]), low types defect at 12.9% compared to 5.4% for high types. The selection in treatment [3-0] removes subjects who defect at higher rates in other treatments, biasing the test conservatively. If anything, the [3-0] continuation sample over-represents cooperative types, which makes it harder, not easier, for the gap to appear insignificant.[21]

This selection is a direct consequence of the model's initiation prediction. Proposition 1 predicts that player $A$ will not initiate when the endowment gap exceeds the critical

[21] A generated-regressor concern arises because the BLUPs are estimated, not observed (see Pagan, 1984). However, this affects inference on the BLUP coefficient, not on the gap, and understated standard errors on a control variable do not inflate the standard error on the endowment gap.

threshold $k^*$, and the gap deters sharing among the subjects with the weakest propensity to share. The relevant test for Hypothesis 2 is whether continuation behavior differs conditional on the game reaching Round 2, and the hazard estimates in Table A.7 show that it does not. The separation between the initiation and continuation margins (i.e., the central prediction of the model) holds even under the most demanding comparison.

### 5.3 Joint Surplus

Table 7 reports joint surplus regressions by treatment. Matches in which collaboration is sustained longer generate a higher joint surplus on average. However, sustaining long-term collaboration requires initiation, and we find that the size of the endowment gap significantly reduces the joint surplus. The gap-size effect[22] is about $-8\%$, meaning that the joint surplus decreases by 8% for every additional idea separating the two players. Thus, the harder it is to initiate collaboration, the less surplus is expected from such collaboration. We also see that initiating collaboration with more initial ideas is associated with a larger joint surplus. This is largely mechanical. Joint payoffs scale with the number of ideas, and initiating collaboration with more ideas yields, on average, more total ideas.

The surplus regression quantifies the per-match cost of the endowment gap, but it does not address the aggregate implications across a population of potential partnerships that vary in knowledge asymmetry. Consider a market in which the better-endowed firm in every potential partnership holds $k_A = 3$ ideas and the less-endowed partner's initial stock varies. Some partnerships have a less-endowed partner with $k_B = 2$ (a moderate gap, analogous to treatment [3-2]), and the remainder have a partner with $k_B = 0$ (a large gap, analogous to treatment [3-0]). Because both partnership types share the same endowment for the better-endowed firm, any difference in collaborative output is driven by the initiation barrier rather than scale. For each partnership type, we compute the fraction of its potential surplus (conditional on initiation) that is actually realized under (experimentally) observed initiation rates. In [3-2]-type partnerships, where 94.7% of

[22]Calculated as the coefficient on the endowment gap divided by the mean joint surplus reported in the table.

**Table 7:** Joint Surplus Regressions

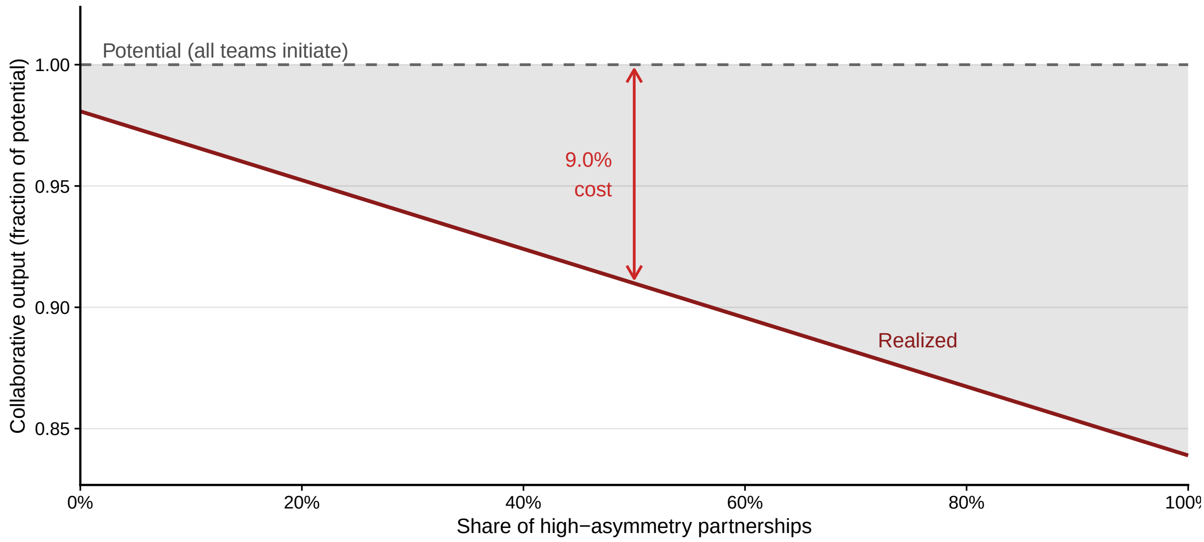
| | Dependent: Joint surplus | |
|---|---|---|
| | (I) | (II) |
| Endowment Gap | $-21.0660^{***}$ | $-21.6256^{***}$ |
| | (6.1558) | (6.2123) |
| Initial Ideas | $44.2864^{***}$ | $36.9492^{***}$ |
| | (10.6976) | (6.3200) |
| Subject controls | ✓ | ✓ |
| Baseline included | | ✓ |
| Mean surplus | 266.07 | 258.30 |
| Gap effect size | -7.9% | -8.4% |
| Observations | 507 | 639 |
| $R^2$ | 0.1101 | 0.1032 |

*Notes:* We report OLS estimation results with joint surplus as the dependent variable, measured in experimental points (100 points correspond to US$1). The unit of observation is the match. *Endowment Gap* is $k_A - k_B$; *Initial Ideas* is player $A$'s initial stock $k_A$. The sample for Column (I) is treatments [2-0], [2-1], [3-0], and [3-2]. For Column (II), we add the baseline treatment [1-0] (and rescale the joint surplus by 290.91/400 to make point values comparable across treatments). All specifications include subject-level controls (Holt-Laury risk preference, gender, fairness attitudes, trust attitudes; not reported). Two-way clustered standard errors at the player $A \times$ player $B$ level in parentheses. $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

better-endowed players initiate, partnerships realize 98.1% of their potential. In [3-0]-type partnerships, where only 64.4% initiate, only 83.9% is realized. We then vary the share of high-asymmetry partnerships and compute the weighted average.

Figure 6 plots the result. The dashed line at 1.0 represents the counterfactual in which all partnerships initiate, regardless of asymmetry. The solid line shows the fraction of potential output actually realized. The shaded region between the two lines represents the initiation barrier cost. When half of potential partnerships are high-asymmetry, 9.0% of potential collaborative output is lost to failed initiation. At 75%, the cost rises to 12.6%. In the limiting case where all partnerships face a large gap, 16.1% of potential output is forgone because the better-endowed firm declines to share (95% confidence interval: 9.8% to 22.2%, from a cluster bootstrap that resamples subjects). These losses are entirely on the initiation margin: Conditional on initiation, collaboration is equally productive in both partnership types (the hazard estimates in Table 4 show no significant difference in continuation behavior across these treatments). The same logic also applies

**Figure 6:** The Cost of the Initiation Barrier




*Notes:* The figure plots the fraction of potential collaborative output realized across a population of partnerships that vary in knowledge asymmetry. In every partnership, the better-endowed firm holds $k_A = 3$ ideas; the less-endowed partner holds $k_B = 2$ (low asymmetry, treatment [3-2]) or $k_B = 0$ (high asymmetry, treatment [3-0]). Each partnership type is normalized by its own potential (mean surplus conditional on initiation), so potential is flat at 1.0. The solid line shows the weighted average of realized output under observed initiation rates. The shaded region is the cost of the initiation barrier. A cluster bootstrap resampling player-$A$ subjects (2,000 replications) yields a 95% confidence interval of [9.8%, 22.2%] for the limiting-case cost at a high-asymmetry share of one.

within organizations, where teams whose members differ in expertise or experience face an analogous initiation barrier that can constrain internal knowledge flows.

# 6 Discussion and Conclusion

This paper studies how asymmetry in competitors' initial information endowments affects collaborative knowledge exchange. A model of word-of-mouth communication between competitors predicts a clean separation between the decision to *initiate* collaboration and the decision to *continue* it: The initiation condition depends on the size of the initial endowment and deteriorates as the endowment grows, while the continuation condition is structurally independent of the initial endowment. In a laboratory experiment with five treatments spanning symmetric and asymmetric endowments, we find that both predictions hold.

Our central finding is that collaboration is resilient to moderate asymmetry. The binding constraint is the initiation decision, and initiation persists even when endowment gaps fall below a critical threshold derived from the model. Once initiated, collaboration is equally durable across all treatments: Continuation rates do not vary with the initial asymmetry, and in a round-level hazard model conditional on initiation, the endowment gap does not predict defection, while expectations of reciprocity are the dominant predictor. Extreme asymmetry is harmful not because it undermines the willingness to continue sharing but because it prevents the better-endowed player from initiating the exchange in the first place. The initiation barrier is moreover selective: It deters the players least inclined to share, while players with a high sharing propensity initiate at high rates regardless of the gap. The expectations channel reinforces this pattern. Below the threshold, the better-endowed player remains optimistic about reciprocity even when the gap widens, and initiation holds up; above the threshold, optimism falls and cannot rescue collaborations where the cost of sharing is prohibitive.

Our results carry implications for the management of R&D joint ventures and strategic alliances. The first implication concerns partner selection. Managers evaluating potential collaborators often treat knowledge gaps as a risk factor: Alliances form less readily both when partners' technological bases barely overlap and when they overlap almost completely, since too little overlap leaves nothing to combine and too much leaves nothing to learn (Mowery et al., 1998). Ventures likewise screen capable partners on the risk that those capabilities will be used to appropriate rather than create value (Diestre and Rajagopalan, 2012). Our findings show that moderate asymmetry is not, by itself, an obstacle to productive collaboration: Once initiated, the exchange is equally durable and productive, regardless of the initial gap. The relevant screening question is not whether partners are symmetric (they rarely are) but whether the knowledge gap falls above the threshold at which the better-endowed partner declines to share. The model makes this threshold precise, and the experiment confirms it behaviorally. In our calibration, the threshold lies between a gap of two and three ideas, and the better-endowed player initiates at rates between 79% and 95% when the gap is at most two ideas but at only 64% when the gap is three. The threshold depends on the intensity of (product-market) competition and on the probability of generating new knowledge, both of which are, in principle, assessable from industry data and firm-level R&D productivity.

The second implication concerns the design of governance structures that lower the initiation barrier. Because the binding constraint on collaboration is the better-endowed partner's willingness to make the first move, mechanisms that reduce the perceived cost of initiation are likely to be most effective. Pilot projects, phased disclosure agreements, jointly funded exploratory stages, and memoranda of understanding all function as initiation-cost reducers: They shrink the effective knowledge stock that the better-endowed partner must sacrifice up front, in the spirit of starting relationships small (Watson, 1999; Kogut, 1991), of narrowing alliance scope when partners are direct competitors (Oxley and Sampson, 2004), and of selectively revealing knowledge (Anton and Yao, 2002; Alexy et al., 2013). Our evidence on the expectations channel reinforces this point. The better-endowed partner's subjective expectation of reciprocity is the strongest predictor of initiation (an average marginal effect of 4.7 percentage points per 10-point increase), and initiation survives below the threshold because the better-endowed partner remains optimistic even when the gap widens. Governance structures that credibly signal the less-endowed partner's commitment to reciprocate, such as equity stakes, co-investment requirements, or exclusivity agreements, could raise these expectations and expand the range of viable partnerships (Williamson, 1983; Parkhe, 1993). We have not tested these instruments in the experiment. What the experiment establishes is where they must operate: Collaboration fails at the first move, so the instruments with leverage are those that lower the cost of that first move. Continuation, by contrast, requires no special governance intervention: Conditional on initiation, the endowment gap does not predict defection.

The third implication concerns a perception gap between the two sides of the partnership. Below the threshold, the better-endowed players who are generally most reluctant to share are the most optimistic about reciprocity precisely where their sacrifice is largest, consistent with viewing their own initiation as a costly signal of cooperative intent. The less-endowed partner, however, does not condition its reciprocation on the costliness of that sacrifice. The endowment gap is insignificant in the less-endowed player's sharing decision, while the absolute level of the better-endowed player's initial contribution does predict reciprocation. The better-endowed partner acts as if it is sending a signal; the less-endowed partner does not appear to receive it. If the better-endowed partner expects its costly first move to be recognized and reciprocated but the less-endowed partner does not interpret it that way, disappointment and premature withdrawal may follow. Alliance managers can mitigate this perception gap by making the costliness of the first move explicit, for example, by documenting the competitive value of the shared knowledge in a joint steering committee, so that both sides share a common understanding of what initiation entails.

The final implication is a cautionary one. Extreme asymmetry creates an initiation barrier that governance mechanisms may not overcome. When the knowledge gap exceeds the critical threshold, the better-endowed partner expects to sacrifice a substantial informational advantage with insufficient prospect of reciprocal return, and the collaboration does not get off the ground. In the treatments in which the better-endowed player starts with three ideas, the joint surplus in the extreme-gap treatment falls significantly short of that in the moderate-gap treatment, driven almost entirely by the difference in initiation rates. Across a population of potential partnerships, our simulation puts the aggregate cost of the initiation barrier at 9% of potential collaborative output when half of the partnerships face a large knowledge gap, rising to 16.1% when all do. These costs suggest an upper bound on the degree of partner heterogeneity that alliances can absorb, consistent with the declining region of the inverted-U relationship between diversity and alliance performance documented by Sampson (2007). In practice, the bound may be even tighter: A partner with no prior knowledge base may also lack the absorptive capacity to assimilate external knowledge (Cohen and Levinthal, 1990), compounding the initiation barrier with a utilization barrier. Partnerships that span very large knowledge gaps may therefore require not only initiation-cost-reducing governance but also capacity-building investments in the less-endowed partner before productive collaboration can begin. The same mechanism operates within organizations: When teams pair senior and junior members with different expertise, the senior member's reluctance to initiate knowledge sharing is likely to drag down internal knowledge flows.

The stylized laboratory setting allows us to separate initiation from continuation; a separation that field data on alliances cannot deliver, because failed initiations are unobserved and endowment gaps are endogenous. Several limitations nonetheless merit discussion. Our experiment's use of known payoff parameters abstracts from the uncertainty and complexity of real-world R&D collaborations. The model assumes that the less-endowed player (player $B$ in the experiment) can productively use any idea the better-endowed player (player $A$) shares, regardless of player $B$'s starting stock. As noted above, limits to *absorptive capacity* could further weaken the collaborative gains between partners with extreme asymmetries. The expectations channel we identify may also interact with other factors (such as trust, reputation, and repeated interactions across partnerships) that are not included in our design. Our experiment uses a stranger-matching protocol in which subjects are rematched each period, and we do not separately test for the effect of past experience on current sharing behavior. Ganglmair et al. (2020) study these dynamics in the baseline treatment and find that negative past experience (i.e., having had a rival or oneself terminate a prior match by defecting) reduces a player's willingness to share and lowers beliefs about rivals' intentions. We expect similar experience effects here, but leave formal tests to future work. Future work could also extend our analysis to settings with richer information structures, endogenous partner choice, or field data on R&D alliances.

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# A Appendix

## A.1 Proof of Proposition 1

*Proof.* We treat $\psi$ as a function of a continuous argument $k \in [1, \infty)$ and write the fraction in the initiation condition (7) as $N(k)/D(k)$, where

$$N(k) = (1-\beta)\, p\, \beta^k \qquad \text{and} \qquad D(k) = (1-\beta p) - (1-p)\, \beta^k,$$

so that $\psi(k) = N(k)/D(k) - \theta$. Recall that $\beta \in (0,1)$ and $p \in (0,1)$, and note that the continuation condition (4) implies $\theta \leq p\beta < 1$. We further assume $\theta > 0$; for $\theta = 0$, we have $\psi(k) > 0$ for all $k$, and player $A$ initiates the exchange regardless of its initial endowment.

*Step 1: $D(k)$ is positive.* Because $\beta^k$ is strictly decreasing in $k$ and $1 - p > 0$, the denominator $D(k)$ is strictly increasing in $k$. At $k = 1$, $D(1) = 1 - \beta p - (1-p)\,\beta = 1 - \beta > 0$. Hence, $D(k) \geq 1 - \beta > 0$ for all $k \geq 1$.

*Step 2: $\psi(k)$ is strictly decreasing.* The numerator $N(k) > 0$ is proportional to $\beta^k$ and therefore strictly decreasing in $k$. By Step 1, the denominator is positive and strictly increasing. The ratio $N(k)/D(k)$ is therefore strictly decreasing in $k$, and so is $\psi(k) = N(k)/D(k) - \theta$.

*Step 3: Boundary values.* At $k = 1$,

$$\psi(1) = \frac{(1-\beta)\, p\, \beta}{1-\beta} - \theta = p\beta - \theta = \phi,$$

so the initiation condition at $k = 1$ coincides with the continuation condition (4), and $\psi(1) = \phi \geq 0$ by assumption. As $k \to \infty$, $\beta^k \to 0$, so $N(k) \to 0$ and $D(k) \to 1 - \beta p > 0$, and thus $\psi(k) \to -\theta < 0$.

*Step 4: Existence and uniqueness of $k^*$.* The function $\psi$ is continuous and strictly decreasing on $[1, \infty)$, with $\psi(1) \geq 0$ and $\lim_{k\to\infty} \psi(k) < 0$. There exists, therefore, a unique $k^* \geq 1$ such that $\psi(k^*) = 0$, and strict monotonicity implies $\psi(k) \geq 0$ if and only if $k \leq k^*$. Player $A$ thus initiates the exchange for $k \leq k^*$ (because endowments take integer values, for all integer $k \leq \lfloor k^* \rfloor$) and does not initiate for $k > k^*$.

*Step 5: Closed form.* Setting $\psi(k^*) = 0$ is equivalent to $N(k^*) = \theta D(k^*)$:

$$(1-\beta)\, p\, \beta^{k^*} \quad = \quad \theta \left[(1-\beta p) - (1-p)\, \beta^{k^*}\right].$$

Collecting the $\beta^{k^*}$ terms on the left yields

$$\beta^{k^*} \left[(1-\beta)\, p + \theta\, (1-p)\right] \quad = \quad \theta\, (1-\beta p)\,,$$

and taking logarithms yields expression (8). This expression is well defined: The argument of the logarithm in the numerator is positive because $\theta > 0$, and it is less than one because

$$(1-\beta)\, p + \theta\, (1-p) - \theta\, (1-\beta p) = p\, (1-\beta)\, (1-\theta) > 0,$$

so that both the numerator and the denominator of (8) are negative and $k^* > 0$. That $k^* \geq 1$ follows from Steps 2 and 3. Q.E.D.

## A.2 Initiation with a Positive Initial Stock for Player $B$

The initiation condition (7) is derived for $k_B = 0$. Here we show that it extends to $k_B > 0$: The condition depends on the two initial stocks only through the gap $g \equiv k_A - k_B$, so Proposition 1 applies with the gap in place of $k$. As in the experimental implementation, player $B$'s initial ideas are contained in player $A$'s initial stock, so that player $A$ sharing its initial stock equalizes the two stocks at $k_A$.

If player $A$ conceals its initial stock, the game ends with stocks $k_A$ and $k_B$, and player $A$'s payoffs are

$$conceal_{A1} = (1-\theta)\left(1-\beta^{k_A}\right) + \theta\left(\beta^{k_B} - \beta^{k_A}\right),$$

which reduces to the expression in the main text for $k_B = 0$. If player $A$ shares (and expects player $B$ to share in all subsequent rounds), its payoffs are the same as in the main text,

$$share_{A1} = (1-\theta)\left[1 - \frac{(1-p)\,\beta^{k_A}}{1-p\beta}\right].$$

The difference is

$$share_{A1} - conceal_{A1} = (1-\theta)\,\frac{p\,(1-\beta)}{1-p\beta}\,\beta^{k_A} - \theta\,\beta^{k_B}\,(1-\beta^{g})\,.$$

Dividing by $\beta^{k_B} > 0$, the sharing requirement $share_{A1} \geq conceal_{A1}$ becomes

$$(1-\theta)\,p\,(1-\beta)\,\beta^{g} \geq \theta\,(1-\beta^{g})\,(1-p\beta)\,,$$

which no longer involves $k_A$ or $k_B$ separately. Adding $\theta p\,(1-\beta)\,\beta^{g}$ to both sides and collecting terms as in the proof of Proposition 1 yields

$$(1-\beta)\,p\beta^{g} \geq \theta\,[(1-p\beta) - (1-p)\,\beta^{g}]\,,$$

which is the initiation condition (7) with the gap $g$ in place of $k$. For $k_B = 0$, the two statements coincide. The threshold $k^*$ in Proposition 1 therefore applies to the endowment gap: Player $A$ initiates whenever $k_A - k_B \leq k^*$.

## A.3 Additional Tables

**Table A.1:** Covariate Balance Across Treatments

| | Treatment [$k_A$-$k_B$] | | | | | |
|---|---|---|---|---|---|---|
| | 1-0 | 2-0 | 2-1 | 3-0 | 3-2 | $p$-value |
| Risk aversion (Holt-Laury) | 6.25 | 5.77 | 6.08 | 7.08 | 6.29 | 0.348 |
| Fairness | 5.79 | 5.91 | 6.12 | 5.62 | 6.04 | 0.958 |
| Trustworthiness | 5.21 | 4.82 | 5.04 | 5.58 | 5.25 | 0.819 |
| Gender (survey code) | 1.08 | 1.32 | 1.42 | 1.29 | 1.42 | 0.077 |
| Subjects | 24 | 22 | 24 | 24 | 24 | |

*Notes:* We report subject-level means of pre-treatment characteristics by treatment. Risk aversion is the Holt and Laury (2002) measure (1 to 10, higher values indicating greater risk aversion); fairness and trustworthiness are the 10-point exit-survey scales described in Section 3; gender is the exit-survey code. The last column reports $p$-values from one-way ANOVA ($F$-tests) for the survey measures and from a $\chi^2$ test for gender. A joint MANOVA of risk aversion, fairness, and trustworthiness across treatments yields Pillai's trace = 0.08 ($p = 0.68$).

**Table A.2:** Average Treatment Effects on Initiation (Player $A$, Round 1)

| | Sharing in Round 1 (player $A$) | | |
|---|---|---|---|
| Treatment [$k_A$-$k_B$] | Mean (s.e.) | $N$ | |
| [1-0] ($k_A = 1$, $k_B = 0$) | 0.8939 (0.0269) | 132 | |
| [2-0] ($k_A = 2$, $k_B = 0$) | 0.7928 (0.0386) | 111 | |
| [2-1] ($k_A = 2$, $k_B = 1$) | 0.7955 (0.0352) | 132 | |
| [3-0] ($k_A = 3$, $k_B = 0$) | 0.6439 (0.0418) | 132 | |
| [3-2] ($k_A = 3$, $k_B = 2$) | 0.9470 (0.0196) | 132 | |
| **Treatment comparison** | **Difference** | **(s.e.)** | **90% CI** |
| [1-0] − [2-0] | 0.1097 | (0.0759) | [−0.0180, 0.2374] |
| [1-0] − [2-1] | 0.0959 | (0.0779) | [−0.0350, 0.2268] |
| [1-0] − [3-0] | 0.2511$^{***}$ | (0.0848) | [0.1083, 0.3939] |
| [1-0] − [3-2] | −0.0735 | (0.0570) | [−0.1695, 0.0225] |
| [2-0] − [2-1] | −0.0138 | (0.0861) | [−0.1584, 0.1308] |
| [2-0] − [3-0] | 0.1414 | (0.0924) | [−0.0140, 0.2967] |
| [2-0] − [3-2] | −0.1832$^{**}$ | (0.0679) | [−0.2980, −0.0683] |
| [2-1] − [3-0] | 0.1552 | (0.0941) | [−0.0028, 0.3131] |
| [2-1] − [3-2] | −0.1694$^{**}$ | (0.0701) | [−0.2878, −0.0510] |
| [3-0] − [3-2] | −0.3246$^{***}$ | (0.0777) | [−0.4562, −0.1930] |

*Notes:* In the top portion of the table, we report the average level of sharing in Round 1 by player $A$ for each treatment. In the bottom portion, we report unpaired two-sample Welch $t$-tests on subject-level means (one observation per subject: the subject's average across matches), so that repeated decisions by the same subject do not enter the tests as independent observations. We report differences in subject-level means with standard errors in parentheses. The last column reports the 90% confidence interval of the difference; by the logic of two one-sided tests, the difference is statistically equivalent to zero within any bound that contains this interval (Lakens, 2017). $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

**Table A.3:** Average Treatment Effects on Continuation (Player $B$, Round 2)

| | Sharing in Round 2 (player $B$) | | |
|---|---|---|---|
| Treatment [$k_A$-$k_B$] | Mean (s.e.) | $N$ | |
| [1-0] ($k_A = 1$, $k_B = 0$) | 0.8396 (0.0358) | 106 | |
| [2-0] ($k_A = 2$, $k_B = 0$) | 0.8750 (0.0372) | 80 | |
| [2-1] ($k_A = 2$, $k_B = 1$) | 0.8211 (0.0395) | 95 | |
| [3-0] ($k_A = 3$, $k_B = 0$) | 0.9740 (0.0182) | 77 | |
| [3-2] ($k_A = 3$, $k_B = 2$) | 0.9828 (0.0121) | 116 | |
| **Treatment comparison** | **Difference** | **(s.e.)** | **90% CI** |
| [1-0] − [2-0] | −0.0365 | (0.0748) | [−0.1624, 0.0895] |
| [1-0] − [2-1] | 0.0444 | (0.0858) | [−0.0997, 0.1885] |
| [1-0] − [3-0] | −0.1113 | (0.0664) | [−0.2241, 0.0015] |
| [1-0] − [3-2] | −0.1273 * | (0.0631) | [−0.2353, −0.0193] |
| [2-0] − [2-1] | 0.0809 | (0.0719) | [−0.0400, 0.2019] |
| [2-0] − [3-0] | −0.0748 | (0.0469) | [−0.1542, 0.0046] |
| [2-0] − [3-2] | −0.0908 ** | (0.0422) | [−0.1631, −0.0186] |
| [2-1] − [3-0] | −0.1557 ** | (0.0631) | [−0.2629, −0.0486] |
| [2-1] − [3-2] | −0.1718 *** | (0.0597) | [−0.2738, −0.0697] |
| [3-0] − [3-2] | −0.0160 | (0.0245) | [−0.0576, 0.0255] |

*Notes:* In the top portion of the table, we report the average level of sharing in Round 2 by player $B$ for each treatment. In the bottom portion, we report unpaired two-sample Welch $t$-tests on subject-level means (one observation per subject: the subject's average across matches), so that repeated decisions by the same subject do not enter the tests as independent observations. We report differences in subject-level means with standard errors in parentheses. The last column reports the 90% confidence interval of the difference; by the logic of two one-sided tests, the difference is statistically equivalent to zero within any bound that contains this interval (Lakens, 2017). $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

**Table A.4:** Average Treatment Effects on Continuation (Player $A$, Round 3)

| | Sharing in Round 3 (player $A$) | | |
|---|---|---|---|
| Treatment $[k_A$-$k_B]$ | Mean (s.e.) | $N$ | |
| [1-0] $(k_A = 1,\ k_B = 0)$ | 0.9634 (0.0209) | 82 | |
| [2-0] $(k_A = 2,\ k_B = 0)$ | 0.9375 (0.0305) | 64 | |
| [2-1] $(k_A = 2,\ k_B = 1)$ | 0.9041 (0.0347) | 73 | |
| [3-0] $(k_A = 3,\ k_B = 0)$ | 0.9701 (0.0209) | 67 | |
| [3-2] $(k_A = 3,\ k_B = 2)$ | 0.9615 (0.0189) | 104 | |
| **Treatment comparison** | **Difference** | **(s.e.)** | **90% CI** |
| [1-0] − [2-0] | 0.0521 | (0.0851) | [−0.0919, 0.1961] |
| [1-0] − [2-1] | 0.0604 | (0.0767) | [−0.0687, 0.1895] |
| [1-0] − [3-0] | −0.0047 | (0.0681) | [−0.1191, 0.1096] |
| [1-0] − [3-2] | −0.0193 | (0.0555) | [−0.1127, 0.0740] |
| [2-0] − [2-1] | 0.0083 | (0.0940) | [−0.1501, 0.1668] |
| [2-0] − [3-0] | −0.0568 | (0.0871) | [−0.2040, 0.0904] |
| [2-0] − [3-2] | −0.0714 | (0.0776) | [−0.2039, 0.0610] |
| [2-1] − [3-0] | −0.0652 | (0.0789) | [−0.1979, 0.0676] |
| [2-1] − [3-2] | −0.0798 | (0.0683) | [−0.1955, 0.0360] |
| [3-0] − [3-2] | −0.0146 | (0.0584) | [−0.1133, 0.0841] |

*Notes:* In the top portion of the table, we report the average level of sharing in Round 3 by player $A$ for each treatment. In the bottom portion, we report unpaired two-sample Welch $t$-tests on subject-level means (one observation per subject: the subject's average across matches), so that repeated decisions by the same subject do not enter the tests as independent observations. We report differences in subject-level means with standard errors in parentheses. The last column reports the 90% confidence interval of the difference; by the logic of two one-sided tests, the difference is statistically equivalent to zero within any bound that contains this interval (Lakens, 2017). $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

**Table A.5:** Average Treatment Effects on Round 1 Expectations of Player $A$

| Treatment $[k_A\text{-}k_B]$ | Expectations in Round 1 (player $A$) Mean (s.e.) | $N$ | |
|---|---|---|---|
| [1-0] $(k_A = 1,\ k_B = 0)$ | 77.30 (2.15) | 132 | |
| [2-0] $(k_A = 2,\ k_B = 0)$ | 79.56 (2.90) | 111 | |
| [2-1] $(k_A = 2,\ k_B = 1)$ | 65.77 (2.85) | 132 | |
| [3-0] $(k_A = 3,\ k_B = 0)$ | 70.17 (2.99) | 132 | |
| [3-2] $(k_A = 3,\ k_B = 2)$ | 77.92 (2.20) | 132 | |
| **Treatment comparison** | **Difference** | **(s.e.)** | **90% CI** |
| [1-0] − [2-0] | 0.70 | (6.85) | [−10.85, 12.24] |
| [1-0] − [2-1] | 10.01 | (6.19) | [−0.39, 20.40] |
| [1-0] − [3-0] | 7.31 | (6.39) | [−3.44, 18.06] |
| [1-0] − [3-2] | −1.49 | (5.87) | [−11.34, 8.36] |
| [2-0] − [2-1] | 9.31 | (7.44) | [−3.20, 21.83] |
| [2-0] − [3-0] | 6.61 | (7.62) | [−6.19, 19.42] |
| [2-0] − [3-2] | −2.18 | (7.18) | [−14.27, 9.90] |
| [2-1] − [3-0] | −2.70 | (7.03) | [−14.50, 9.10] |
| [2-1] − [3-2] | −11.50 * | (6.55) | [−22.50, −0.50] |
| [3-0] − [3-2] | −8.80 | (6.75) | [−20.13, 2.54] |

*Notes:* In the top portion of the table, we report the average Round 1 expectations of player $A$ about player $B$ sharing in Round 2, for each treatment. In the bottom portion, we report unpaired two-sample Welch $t$-tests on subject-level means (one observation per subject: the subject's average across matches), so that repeated decisions by the same subject do not enter the tests as independent observations. We report differences in subject-level means with standard errors in parentheses. The last column reports the 90% confidence interval of the difference; by the logic of two one-sided tests, the difference is statistically equivalent to zero within any bound that contains this interval (Lakens, 2017). $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

**Table A.6:** Average Treatment Effects on Round 2 Expectations of Player $B$

| Treatment $[k_A\text{-}k_B]$ | Expectations in Round 2 (player $B$) Mean (s.e.) | $N$ | |
|---|---|---|---|
| [1-0] ($k_A = 1$, $k_B = 0$) | 73.00 (2.57) | 106 | |
| [2-0] ($k_A = 2$, $k_B = 0$) | 76.14 (3.45) | 80 | |
| [2-1] ($k_A = 2$, $k_B = 1$) | 69.11 (3.07) | 95 | |
| [3-0] ($k_A = 3$, $k_B = 0$) | 79.18 (2.62) | 77 | |
| [3-2] ($k_A = 3$, $k_B = 2$) | 81.82 (1.88) | 116 | |
| **Treatment comparison** | **Difference** | **(s.e.)** | **90% CI** |
| [1-0] − [2-0] | −1.81 | (7.14) | [−13.84, 10.22] |
| [1-0] − [2-1] | 5.94 | (6.72) | [−5.35, 17.23] |
| [1-0] − [3-0] | −8.11 | (5.64) | [−17.59, 1.37] |
| [1-0] − [3-2] | −6.30 | (5.78) | [−16.01, 3.41] |
| [2-0] − [2-1] | 7.75 | (7.84) | [−5.43, 20.93] |
| [2-0] − [3-0] | −6.30 | (6.94) | [−18.02, 5.42] |
| [2-0] − [3-2] | −4.49 | (7.06) | [−16.39, 7.41] |
| [2-1] − [3-0] | −14.05 ** | (6.51) | [−25.00, −3.10] |
| [2-1] − [3-2] | −12.24 * | (6.63) | [−23.38, −1.09] |
| [3-0] − [3-2] | 1.81 | (5.54) | [−7.49, 11.11] |

*Notes:* In the top portion of the table, we report the average Round 2 expectations of player $B$ about player $A$ sharing in Round 3, for each treatment. In the bottom portion, we report unpaired two-sample Welch $t$-tests on subject-level means (one observation per subject: the subject's average across matches), so that repeated decisions by the same subject do not enter the tests as independent observations. We report differences in subject-level means with standard errors in parentheses. The last column reports the 90% confidence interval of the difference; by the logic of two one-sided tests, the difference is statistically equivalent to zero within any bound that contains this interval (Lakens, 2017). $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

**Table A.7:** Discrete-Time Hazard Model with Player $A$ Type

| | Dependent variable: Defection (0/1) | | |
|---|---|---|---|
| | (I) | (II) | (III) |
| Endowment Gap | 0.0518 | −0.0138 | −0.0954 |
| | (0.1957) | (0.2000) | (0.1961) |
| Initial Ideas | −0.4481*** | −0.4213** | −0.4240** |
| | (0.1640) | (0.1649) | (0.1686) |
| Round | −0.0057 | −0.0074 | 0.0276* |
| | (0.0170) | (0.0166) | (0.0159) |
| Expectations $\alpha_j$ | | | −0.0493*** |
| | | | (0.0040) |
| Player $A$ Type (High) | | −0.4219** | −0.4019** |
| | | (0.2027) | (0.1665) |
| Subject controls | ✓ | ✓ | ✓ |
| Observations | 3118 | 3118 | 3118 |
| Log-likelihood | −666.99 | −664.52 | −493.84 |

*Notes:* We report estimates from the discrete-time proportional hazards model of Table 4, augmented with player $A$'s BLUP-defined type. Column (I) replicates the baseline specification. Column (II) adds a binary indicator for player $A$'s type (High = 1 if $\hat{u}_i \geq 0$; Low = 0), classified using the subject-level random intercept from the RE probit in Table 5 (Column III). Column (III) adds the deciding player's current-round expectation of their partner's sharing alongside the type indicator. The sample conditions on initiation and begins at Round 2. All models use the complementary log-log link. All specifications include subject-level controls (Holt-Laury risk preference, gender, fairness attitudes, trust attitudes), not reported. Standard errors clustered at the subject level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

# Online Appendix

When Goliath Shares with David:
The Effects of Asymmetry on Collaboration

Bernhard Ganglmair Alex Holcomb

July 13, 2026

# Information Sharing Experiment
# Instructions

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## Experiment Overview

You are about to participate in an experiment on the economics of decision-making. If you listen carefully and make good decisions, you can earn a considerable amount of money. You will be paid in cash at the end of the experiment.

Please do not communicate with the other participants. If you have questions, please raise your hand. The experimenter will come to you to answer them.

It will take you about 90 minutes to complete this session. After the experiment, you will be given a short survey to complete.

You will be working with a fictitious currency called *Francs*.

**Exchange rate:** 100 *Francs* = 1 USD

Today's experiment consists of two tasks. In Task 1, you will be asked to choose from a pair of options. Each option involves two payments. Each payment has a specified probability (i.e., choose one of two lotteries). For Task 2, you and another player in this room will be matched to perform a computer experiment.

## Detailed Instructions

## Task 1: Choose a Lottery

Your decision sheet shows ten decisions listed on the right. Each decision is a paired choice between "Choice A" and "Choice B." You will make ten decisions and record these in the first column. You may choose A for some decision rows and B for other rows. You may change your decisions and make them in any order. Only one of these decisions will be used to determine your earnings upon completion of Task 2.

A ten-sided die is used to determine your earnings. The faces are numbered from 1 to 10 (the "0" face will serve as 10.) After you have made all of your Task 1 decisions and completed the computer experiment (Task 2) you will be asked to come to the front desk. The experimenter will throw the die twice: The first throw will determine which of your ten decisions is to be used. Given your choice for this decision (A or B), the second throw will determine your earnings (in *Francs*). The earnings for this choice will be added to your earnings from Task 2, and, when finished, you will be paid all earnings in cash.

Even though you will make ten decisions, only one of these will affect your earnings. You will not know in advance which decision will be used. Obviously, each decision has an equal chance of being used in the end.

Look at **Decision 1** and **Decision 2:**

| **Your Choice:** | **Choice A** | **Choice B** |
|---|---|---|
| *Write A or B* | Die face 1 pays 200 (chance of 1/10)<br>Die face 2-10 pays 160 (chance of 9/10) | Die face 1 pays 385 (chance of 1/10)<br>Die face 2-10 pays 10 (chance of 9/10) |
| *Write A or B* | Die face 1-2 pays 200 (chance of 2/10)<br>Die face 3-10 pays 160 (chance of 8/10) | Die face 1-2 pays 385 (chance of 2/10)<br>Die face 3-10 pays 10 (chance of 8/10) |

For Decision 1, Choice A pays 200 *Francs* if the throw of the ten-sided die is 1 (i.e., with a chance of 1/10), and it pays 160 *Francs* if the throw is 2 through 10 (i.e., with a chance of 9/10). Choice B yields 385 *Francs* if the throw of the die is 1 (chance of 1/10), and it pays 10 *Francs* if the throw is 2 through 10 (chance of 9/10).

For Decision 2, Choice A pays 200 *Francs* if the throw of the ten-sided die is either 1 or 2 (i.e., with a chance of 2/10), and it pays 160 *Francs* if the throw is 3 through 10 (i.e., with a chance of 8/10). Choice B yields 385 *Francs* if the throw of the die is either 1 or 2 (chance of 2/10), and it pays 10 *Francs* if the throw is 3 through 10 (chance of 8/10).

Decisions 3 through 10 are similar except that as you move further down the table, the chance of the higher payoff for each choice increases. Since either option in Decision 10 pays the highest with certainty (200 or 385 *Francs*), the die will not be needed.

Are there any questions?

You may now begin making your choices. Look at the empty boxes on the left side of the record sheet. For each decision row, decide between Choice A and B and write your decisions in these boxes until all ten decisions are complete.

Please do not talk with anyone during the experiment. If you have any questions, raise your hand. After you have completed this task, please stay in your seat. Once all participants have finished, the computer experiment (Task 2) will begin.

## Task 2: Computer Experiment

Below is an explanation about the decisions you will be making in the computer experiment, the players you will be playing against, and the information you will receive and have available during this experiment.

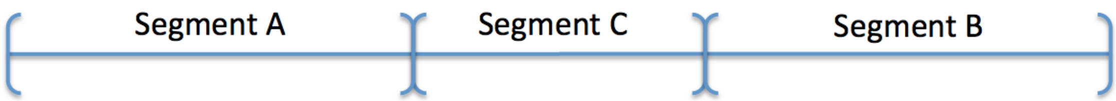
**Players:** You are a fund manager. Your goal is to earn as much money as possible. Your earnings can increase in two ways: a) increase the returns from your investments and b) obtain more investors.

24 people in this room are participating in this experiment. That splits into two groups of 12 each. To begin the game, you will be randomly matched with another player from your group. Then there will be a series of matches. For the first match, you and this player will be randomly assigned roles (either Fund Manager A or Fund Manager B). There will be several matches, all with different players from your group. You will be matched with the same person only once. During each match, you will play the game for an undetermined number of rounds. From one match to another, your assignment as either Fund Manager A or Fund Manager B is determined randomly. You may be assigned as Fund Manager A for some matches and as Fund Manager B for other matches. This is determined randomly. This means that you will not be matched with a person with whom you have previously been matched, regardless of whether you were Fund Manager A or Fund Manager B.

Your identity is kept anonymous for the entire experiment. You are only displayed as “Fund Manager A” or “Fund Manager B.”

Your decision affects only you and the person with whom you are matched. Your decision does not affect the other people participating in this experiment.

**Setup:** The investors you are trying to attract are divided into three segments.




Fund Manager A has completely captured the investors in Segment A. These investors have already invested with Fund Manager A and are currently in a lock-up period. This means, these investors have agreed not to move their investments for a period of several years. Therefore, they are locked-up with Fund Manager A. As Fund Manager A, you charge each Segment A investor a fee. As you generate greater returns for investors in Segment A, the fee increases. Therefore, even though these investors are locked-up, Fund Manager A is better off by generating higher returns for these investors.

For the same reason, Fund Manager B has completely captured the investors in Segment B. As Fund Manager B, you charge a fee to the investors in Segment B. Even though the Segment B investors are locked-up, the Fund Manager B receives higher fees by generating higher returns for these investors.

Segment C consists of new investors. They are not locked-up by either of the managers. Therefore, Fund Manager A and Fund Manager B must compete for the investors in Segment C. Investors in Segment C will invest with the fund manager who provides the highest expected returns.

**Note** that none of the participants in this experiment are assigned the role of "investor." Decisions made by investors are done automatically. This means that investors will automatically choose the fund manager who offers the highest expected return.

Many of the computations are done for you and the payments will be clearly shown to you in a table format. You do not have to figure out the fees you want to charge nor the expected return of the investment. The computer will automatically compute these for you and show you your actual earnings. The only decision you, as the fund manager, will have to make is explained below.

**Decision:** Fund managers increase their returns as they gain more knowledge or information about potential investments. This information is referred to as "ideas." Having more ideas will give you an advantage over the other fund manager and you will be able to generate higher returns. Furthermore, if you have more ideas than the other fund manager, you will capture all the investors in Segment C. Conversely, if you have fewer ideas than the other fund manager, you will not capture any of the investors in Segment C. Essentially, the manager with the most ideas will capture all the investors in Segment C. Finally, if you and the other fund manager have the same number of ideas, then you will split the investors in Segment C evenly, but you will both have zero earnings from this segment.

**Note** that because Segments A and B's investments are locked-up, the competition between Fund Manager A and Fund Manager B does not affect those investments; however, having more ideas will increase the earnings the fund manager receives from Segment A or Segment B.

**Your decision, as the fund manager, is to decide whether or not to share your ideas with the competing fund manager.**

Fund Manager A initially starts out with one idea. Fund Manager B starts out with no ideas. Look at the diagram on the following page.

- In round 1, Fund Manager A must decide whether or not to share his one idea with Fund Manager B (starts with no ideas). If Fund Manager A chooses not to share, the match terminates and the earnings are realized. In that case, Fund Manager A has one idea and Fund Manager B has no ideas. If Fund Manager A chooses to share, then the experiment moves on to round 2.
- At the beginning of round 2, both fund managers start with one idea. Here, there is a 90% chance that Fund Manager B will generate a new idea and a 10% chance that Fund Manager B will not be able to generate a new idea (denoted as "chance" in the following diagram). If Fund Manager B does not generate a new idea, then the match terminates with each manager having one idea and the earnings are realized. If Fund Manager B generates a new idea, then Fund Manager B has a total of two ideas while Fund Manager A has only one idea. At this time, Fund Manager B must decide whether or not to share this new idea with Fund Manager A. If Fund Manager B does not share, then the match terminates and the earnings are realized. If Fund Manager B chooses to share, then the experiment moves on to round 3.
- Similar to the previous round, at the beginning of round 3 both fund managers begin with two ideas. This time, there is a 90% chance that Fund Manager A generates a new idea and a 10% chance that Fund Manager A will not be able to generate a new idea. If Fund Manager A does not generate a new idea, then the match terminates with each manager having two ideas and the earnings are realized. If Fund Manager A generates a new idea, then Fund Manager A has a total of three ideas while Fund Manager B has only two ideas. At this time, Fund Manager A must decide whether or not to share this new idea with Fund Manager B. If Fund Manager A does not share, then the match terminates and the earnings are realized. If Fund Manager A chooses to share, then the experiment moves on to round 4.
- This process will continue until the match is terminated. Termination occurs either by one of the managers not sharing a new idea or when a new idea fails to be generated. As you may have noticed, the decisions are made in alternating sequence between Fund Manager A and Fund Manager B. Furthermore, the only way for a fund manager to generate a new idea is to have one shared with he or she by the other manager in the previous round.

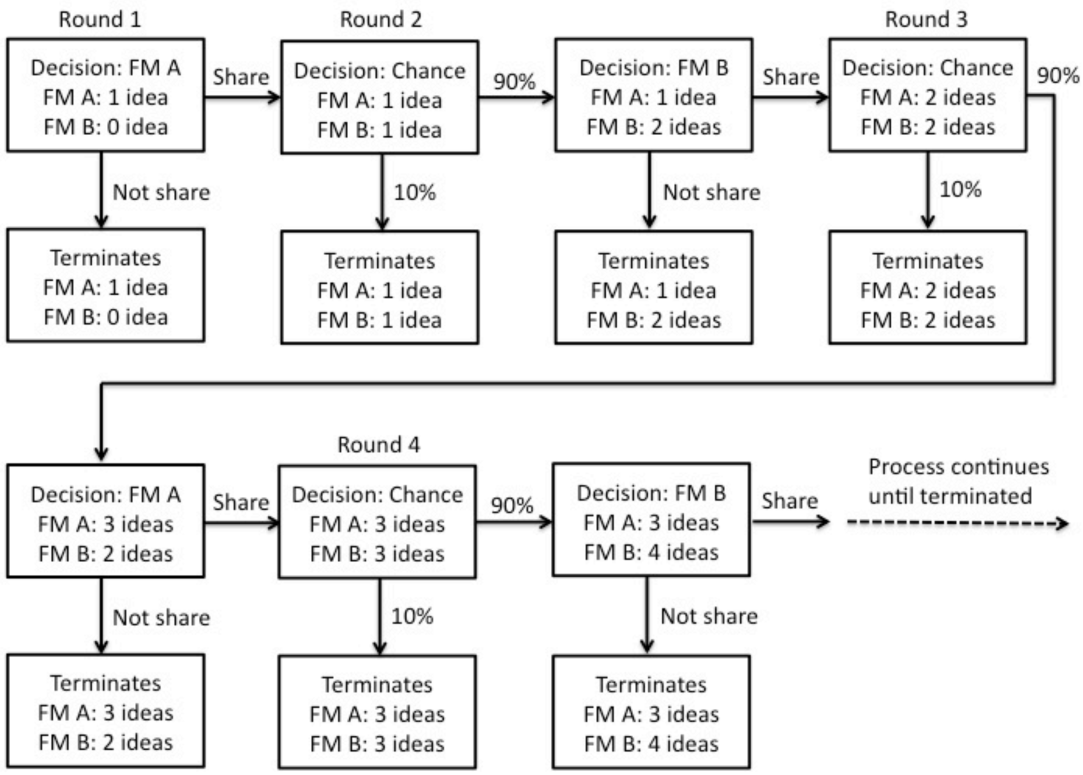
**Termination of a match:** There are two ways your current match can terminate:

- <u>Chance</u>: At the beginning of each round (after the first round), there is a 10% chance that the match terminates. This is because the fund manager (whose turn it is in this round) is not able to generate a new idea.
  <u>Think of a 10% chance as in the following analogy</u>: There are 10 balls in a jar: 9 blue balls and 1 red ball. One ball is drawn from the jar and, if it is a red ball, the match terminates. The match continues if any one of the blue balls is

drawn. In the actual experiment, the experiment's program is used to mimic this process.

- <u>A fund manager decides not to share an idea</u>: The current match terminates if the fund manager decides not to share a newly generated idea.

The figure below summarizes the above statements:




**Note:** "Chance" makes the move before the fund manager is able to decide to share or terminate.

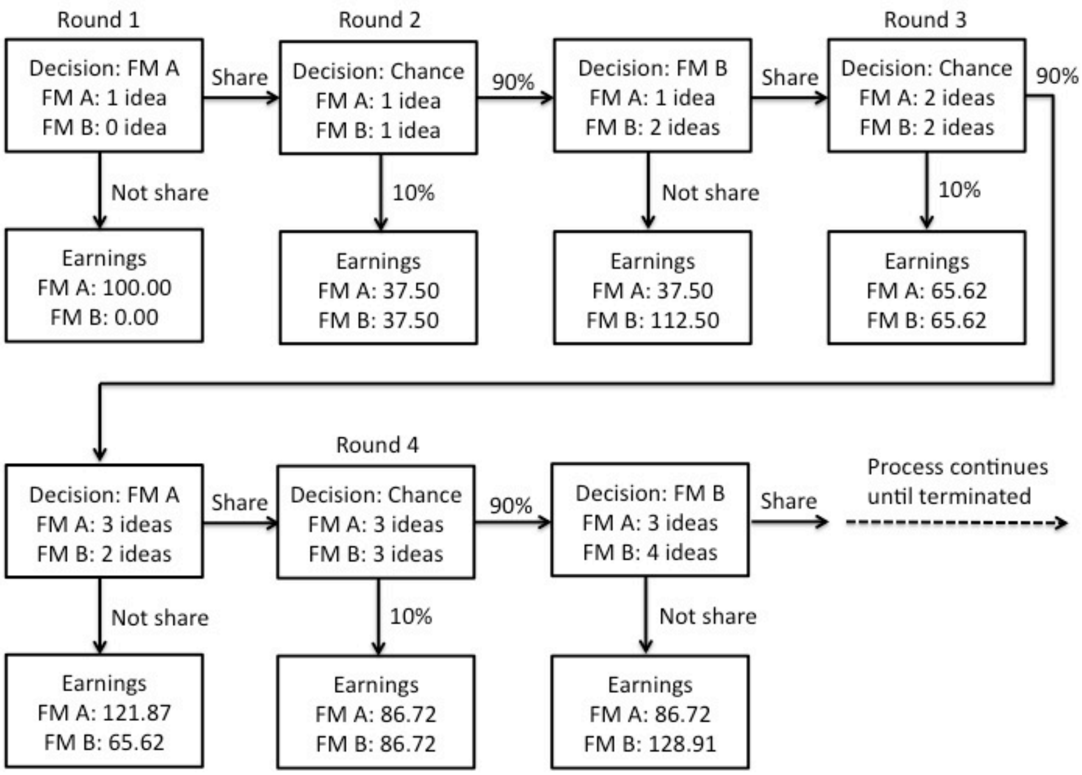
**Note:** The specific round of termination of a match is not set by the experimenter. The match continues (potentially indefinitely) as long as neither the fund managers nor "chance" terminates.

**Note:** While the other fund manager makes his or her decision you will see a screen asking you to wait until it is again your turn. Please always click the "Continue" button when you see it on the screen for the experiment to continue.

**Information:** You and the person with whom you are matched will both know whether the termination is due to "chance" or because the other fund manager decided not to share a newly generated idea. When you and the other fund manager have the same realized earnings, then the match is terminated by "chance." If it is not terminated by "chance," then the match is terminated by the other fund manager.

**New Match:** When the current match terminates, please wait until everyone else's match terminates as well. When all matches are terminated, you will be randomly matched with a new player from your group and begin again. This procedure will be repeated until you have been matched exactly once with all other players in your group.

**Earnings:** Earnings for each match are determined in the following manner. First, the figure below shows you the total earnings for each fund manager, conditional on how the match was terminated.




For a better understanding of earnings when the match is terminated, the following table shows each fund manager's earnings from Segment A, Segment B and Segment C. This is the information you will be provided on your computer screen.

**Note:** Due to rounding errors (a possible difference of 0.01), the sum of segment earnings (Segment A and Segment C for Fund Manager A and Segment B and

Segment C for Fund Manager B) may not be exactly the same as the Total Earnings. Your definitive earnings are your Total Earnings.

| Round | Terminated by | Earnings Fund Manager A | Earnings Fund Manager B |
|---|---|---|---|
| 1 | Fund Manager A | Segment A: 37.50<br>Segment C: 62.50<br>Total: 100.00 | Segment B: 0.00<br>Segment C: 0.00<br>Total: 0.00 |
| 2 | Chance | Segment A: 37.50<br>Segment C: 0.00<br>Total: 37.50 | Segment B: 37.50<br>Segment C: 0.00<br>Total: 37.50 |
| | Fund Manager B | Segment A: 37.50<br>Segment C: 0.00<br>Total: 37.50 | Segment B: 65.62<br>Segment C: 46.88<br>Total: 112.50 |
| 3 | Chance | Segment A: 65.62<br>Segment C: 0.00<br>Total: 65.62 | Segment B: 65.62<br>Segment C: 0.00<br>Total: 65.62 |
| | Fund Manager A | Segment A: 86.72<br>Segment C: 35.16<br>Total: 121.87 | Segment B: 65.62<br>Segment C: 0.00<br>Total: 65.62 |
| 4 | Chance | Segment A: 86.72<br>Segment C: 0.00<br>Total: 86.72 | Segment B: 86.72<br>Segment C: 0.00<br>Total: 86.72 |
| | Fund Manager B | Segment A: 86.72<br>Segment C: 0.00<br>Total: 86.72 | Segment B: 102.54<br>Segment C: 26.37<br>Total: 128.91 |
| 5 | Chance | Segment A: 102.54<br>Segment C: 0.00<br>Total: 102.54 | Segment B: 102.54<br>Segment C: 0.00<br>Total: 102.54 |
| | Fund Manager A | Segment A: 114.40<br>Segment C: 19.78<br>Total: 134.18 | Segment B: 102.54<br>Segment C: 0.00<br>Total: 102.54 |
| 6 | Chance | Segment A: 114.40<br>Segment C: 0.00<br>Total: 114.40 | Segment B: 114.40<br>Segment C: 0.00<br>Total: 114.40 |
| | Fund Manager B | Segment A: 114.40<br>Segment C: 0.00<br>Total: 114.40 | Segment B: 123.30<br>Segment C: 14.83<br>Total: 138.13 |
| 7 | Chance | Segment A: 123.30<br>Segment C: 0.00<br>Total: 123.30 | Segment B: 123.30<br>Segment C: 0.00<br>Total: 123.30 |
| | Fund Manager A | Segment A: 129.98<br>Segment C: 11.12<br>Total: 141.10 | Segment B: 123.30<br>Segment C: 0.00<br>Total: 123.30 |

| | | | |
|---|---|---|---|
| 8 | Chance | Segment A: 129.98<br>Segment C: 0.00<br>Total: 129.98 | Segment B: 129.98<br>Segment C: 0.00<br>Total: 129.98 |
| | Fund Manager B | Segment A: 129.98<br>Segment C: 0.00<br>Total: 129.98 | Segment B: 134.98<br>Segment C: 8.34<br>Total: 143.33 |
| 9 | Chance | Segment A: 134.98<br>Segment C: 0.00<br>Total: 134.98 | Segment B: 134.98<br>Segment C: 0.00<br>Total: 134.98 |
| | Fund Manager A | Segment A: 138.74<br>Segment C: 6.26<br>Total: 144.99 | Segment B: 134.98<br>Segment C: 0.00<br>Total: 134.98 |
| 10 | Chance | Segment A: 138.74<br>Segment C: 0.00<br>Total: 138.74 | Segment B: 138.74<br>Segment C: 0.00<br>Total: 138.74 |
| | Fund Manager B | Segment A: 138.74<br>Segment C: 0.00<br>Total: 138.74 | Segment B: 141.55<br>Segment C: 4.69<br>Total: 146.25 |
| 11 | Chance | Segment A: 141.55<br>Segment C: 0.00<br>Total: 141.55 | Segment B: 141.55<br>Segment C: 0.00<br>Total: 141.55 |
| | Fund Manager A | Segment A: 143.66<br>Segment C: 3.52<br>Total: 147.18 | Segment B: 141.55<br>Segment C: 0.00<br>Total: 141.55 |
| 12 | Chance | Segment A: 143.66<br>Segment C: 0.00<br>Total: 143.66 | Segment B: 143.66<br>Segment C: 0.00<br>Total: 143.66 |
| | Fund Manager B | Segment A: 143.66<br>Segment C: 0.00<br>Total: 143.66 | Segment B: 145.25<br>Segment C: 2.64<br>Total: 147.89 |
| 13 | Chance | Segment A: 145.25<br>Segment C: 0.00<br>Total: 145.25 | Segment B: 145.25<br>Segment C: 0.00<br>Total: 145.25 |
| | Fund Manager A | Segment A: 146.44<br>Segment C: 1.98<br>Total: 148.42 | Segment B: 145.25<br>Segment C: 0.00<br>Total: 145.25 |
| 14 | Chance | Segment A: 146.44<br>Segment C: 0.00<br>Total: 146.44 | Segment B: 146.44<br>Segment C: 0.00<br>Total: 146.44 |
| | Fund Manager B | Segment A: 146.44<br>Segment C: 0.00<br>Total: 146.44 | Segment B: 147.33<br>Segment C: 1.48<br>Total: 148.81 |

Although the table shows the first 14 rounds, the game continues (potentially indefinitely) until the match terminates. For every round during the experiment, you will be provided on your computer screen with the current round's earnings and the next round's potential earnings for both Fund Manager A and Fund Manager B. You may reference the above chart as well.

**Note:** While each additional idea increases your earnings, the next additional idea adds less of an improvement than the previous idea. This means that the first idea results in the greatest earnings increase; then the second idea results in a slightly smaller earnings increase; then the third idea results in less, and so on.

**In Summary**, your earnings as a fund manager are determined in two parts:

The **first part** of your earnings is determined by how many ideas you have. The more ideas you collect, the greater the return (and earnings) will be for this part. In order to collect more ideas, you and the other fund manager must generate and share your respective ideas with each other. In this way, having more ideas always increases Fund Manager A earnings from Segment A or for Fund Manager B from Segment B.

The **second part** of your earnings is determined by whether you have more ideas than the fund manager you are currently matched with. Finishing the game with more ideas than the other fund manager means that you will capture all the investors in Segment C. Thus, you will capture all the earnings from Segment C as well. Conversely, finishing the game with the same number of ideas as the other fund manager, or less ideas, will result in you having no earnings from Segment C.

**Expectations:** During the round in which you are deciding whether or not to share a newly generated idea, you will be asked the following question:

*If, in the next round, the other fund manager successfully generates a new idea (i.e., "chance" does not terminate the match), how likely do you think the other fund manager will share this newly generated idea with you?*

In the field provided, fill in these expectations. Enter a number between 0% and 100% (You do not have to add the %-sign).

**Note:** The probability that the other fund manager generates a new idea in the next round is 90%. You are asked to enter your expectations of the other fund manager sharing this idea given that it has been generated.

**Quiz:**

**Note:** The quiz does not affect your earnings.

1) Assume that you are Fund Manager A. During Round 6, do you make a decision?

*Answer:*

2) Assume that you are Fund Manager B. During Round 3, Fund Manager A decided not to share the new idea with you. What are your earnings?

*Answer:*

3) Assume that you are Fund Manager B. During Round 5, Fund Manager A has decided to share his or her idea with you. At the beginning of Round 6, however, you fail to generate a new idea ("chance" terminates the match). What are your earnings? What are Fund Manager A's earnings?

*Answer:*

4) In question 3, do you, as Fund Manager B, get to make a decision after "chance" terminates the match in Round 5 (i.e., when Fund Manager A fails to generate a new idea)?

*Answer:*

5) Assume that you are Fund Manager B. During Round 5, Fund Manager A has decided to share the new idea with you. At the beginning of Round 6, "chance" does not terminate your match (i.e., you generate a new idea). What are your earnings if you do not share the new idea? What are Fund Manager A's earnings?

*Answer:*

6) In question 5, if you, as Fund Manager B, decide to share the new idea, then "chance" will determine whether the game terminates or not. If "chance" does not terminate (i.e., Fund Manager A generates a new idea), does Fund Manager A get to make his or her decision on whether to share the new idea with you?

*Answer:*

**Procedural Summary:**

Here is what will happen after the instructions:

1. The first match will begin and you will be told what role you are assigned (Fund Manager A or Fund Manager B).
2. When it is your turn to make a decision (i.e., you have generated a new idea), you will be shown the earnings for this and the next several rounds; asked whether you wish to share your idea or not; and also asked to estimate the probability that the matched fund manager will choose to share his or her idea in next round. This continues until the match is terminated, but there is no predetermined end point for any given match: the match can be terminated only by "chance" (i.e., a fund manager fails to generate a new idea) or by the fund manager (he or she decides not to share). You will then be shown your earnings and the other fund manager's earnings. Because there are other matches simultaneously participating in this experiment, you must wait until everyone else's matches are also terminated.
3. When all matches are terminated, you will be randomly matched with another person and randomly assigned a new role (A or B). Then you and the new match will play the game again.
4. This continues until there are no more possible matches with the people in your group. You will know that the experiment has ended when you see a final survey showing up on your screen.
5. The experimenter will then ask you individually to come to the front. You will be paid in cash. Your total cash will be based upon the outcome of your decision in Task 1; and how much you earned in your matches combined during today's computer experiment (Task 2). In other words, you will be paid the total of Task 1 and Task 2 earnings.

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## IMPRINT

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