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Being Watched in an Investment Game Setting: Behavioral Changes when Making Risky Decisions*

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Abstract

We design a laboratory experiment to test for behavioral differences due to observation within a novel arena: investment games. We find that fund managers are more risk-averse when investors can observe their investment allocations. This effect is more pronounced when investors, in addition to observing the allocations, can also observe the investment outcomes. Interestingly, allowing investors to observe how their investment is allocated does not impact how much they invest. Last, when the outcome of the risky investment is public knowledge, disclosing managers' allocations leads them to return more tokens to investors and to expropriate fewer tokens for themselves at the end of the game, ceteris paribus. We discuss potential causes of these effects.

Keywords: Investment Game; Risk; Reciprocity; Trustworthiness; Experiment
JEL Classifications: C71; D9; D81; D82; C91

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1 Introduction

There is a large literature showing that individuals make choices differently while being observed (e.g., Weigold and Schlenker, 1991; Trautmann and Vieider, 2012; Nettle, Nott, and Bateson, 2012). Spanning both economics and psychology, this effect has been documented across many different contexts, from increasing prosocial behavior in standard cooperation (Rosenbaum, Billinger, and Stieglitz, 2014) and donation laboratory games (Ekström, 2012; Powell, Roberts, and Nettle, 2012; Nettle et al., 2013) to decreasing littering (Ernest-Jones, Nettle, and Bateson, 2011; Bateson et al., 2013) and improving honesty among police officers in the field (Ariel, Farrar, and Sutherland, 2015; Ariel, 2016).

However, there is a gap in this observation effect literature regarding investment behavior. We examine how allowing for observation impacts investment, profit-sharing, and decisions involving risk. For example, observation may induce individuals to choose assets as a means to be perceived as fair, or perhaps they allocate monies away from risky investments to signal their "type" as socially responsible. We explore these issues and discuss potential underlying causes to fill this gap in the literature.

In the laboratory, we embed a risky decision within a modified multi-stage trust/investment game to look for the existence of an observation effect. An investor first chooses how much of her endowment to invest in a "firm" (phrased neutrally to subjects). A fund manager then decides how to split that investment between a "safe fund" that has a guaranteed interest rate and a "risky fund" that has a stochastic interest rate with a known distribution.[1] Following this allocation, the risky fund's interest rate is determined and the two funds earn interest. The investor then has a second chance to invest in the firm. Trivially, this investment is placed in the safe fund, after which the two funds earn interest for the second time, though both now earn the same guaranteed rate. The manager then concludes the game by dividing the final firm value – i.e., the joint total of the two funds – between himself and the investor.

We use two treatment dimensions to examine the effects of observation. Our main treatment dimension varies whether the investor can see how the manager splits the primary investment between the safe fund and the risky fund. Comparing behaviors across this dimension identifies the observation effects in our game. The "direct" observation effect describes how the manager alters his allocation decision when that same decision is made visible. The "indirect" observation effects describe how the two players alter their *other* decisions – the investor's two investment opportunities and the manager's division of the final firm value – when the allocation decision is made visible. Our secondary treatment dimension varies whether the investor can observe the risky fund's realized interest rate, allowing us to test for all four observation effects under both regimes.

[1]The investor and manager in an investment game correspond to the trustor and trustee in a trust game, respectively.

When the risky fund's realized interest rate is public knowledge, we find strong evidence that the manager chooses a safer allocation when being observed. However, we find no evidence that this observability impacts either primary investment or how the final firm value is split. We find mild evidence of a decrease in secondary investments, though this is likely explained by a mechanical change in the decision-maker's information set. When the realized interest rate is revealed only to the manager, there is mild evidence that observing the investment allocation leads to a safer allocation, and there is no evidence that it causes the investor to alter either her primary or secondary investments. However, at the end of the game, the manager tends to return more tokens to the investor – and expropriate fewer tokens for himself – when the investor can see how her primary investment is allocated, ceteris paribus.

The remainder of the paper is organized as follows: Section 2 reviews the related literature, focusing on behavioral changes due to observation and decisions in the face of risk. Section 3 describes the design of the laboratory experiment, details the game's treatments and hypotheses, and describes the process of the experiment, while Section 4 provides the results of testing those hypotheses. Section 5 discusses these results in a broader context and concludes.

2 Review of Related Literature

The behavioral effects of being observed have been explored in many contexts across both the economics and psychology literatures. We add to this by examining how introducing an observer impacts behavior within an investment context. Our discussion of this observation effect literature includes underlying causes, which we relate to our experimental results in Section 5.

Risk preferences are perhaps the most basic driving factor behind an individual's risky investment decision. There is mixed evidence regarding how individuals' risk tolerances change when their risky decisions are observed (for reviews, see Trautmann and Vieider, 2012; Albert, Chein, and Steinberg, 2013). In a hypothetical setup, Weigold and Schlenker (1991) find that self-identified low risk-taking individuals act more risk-averse when being observed, though high risk-taking individuals do not alter behavior when observed. Gardner and Steinberg (2005); Smith, Chein, and Steinberg (2014); and Silva, Chein, and Steinberg (2016) find that peer observation increases risky behavior in teenagers, young adults, and older adults, though adult observation erases this effect for teenagers. On the other hand, Baltussen, van den Assem, and van Dolder (2016) find that observation by a large audience leads to less risk-averse choices. At the very least, the results surrounding risk preferences in the face of observation are inconclusive, and the effects are likely context-dependent. We add to this facet of the observation effect literature by studying how risky behavior that jointly affects both the observer and the subject of observation is impacted by that observation, as well as by adding the new (to this specific literature) context of a principal-agent investment game.

Beyond risky decisions, much research has also found that being observed – or even the notion of being observed – increases individuals' prosocial behavior in a variety of ways.[2] For example, there is ample evidence that the presence of an observer increases honest behavior (for a review of the literature, see Rosenbaum, Billinger, and Stieglitz, 2014). Mol, van der Heijden, and Potters (2018) find that this extends to the presence of a "virtual observer" during a game played inside a simulated "pub" in a virtual reality environment. Interestingly, when the more prosocial outcome is to lie rather than to be honest, observation leads to an increase in lying (Oda, Kato, and Hiraishi, 2015).

One prosocial behavior for which the effect of observation has been heavily studied is charitable donation. Generally speaking, even including a generic image of eyes to imply oversight leads to increased charitable donations (e.g., Powell, Roberts, and Nettle, 2012; Ekström, 2012; Fathi, Bateson, and Nettle, 2014; Oda and Ichihashi, 2016) – known as the "watching-eye effect" – especially along the extensive margin (Nettle et al., 2013). However, Sparks and Barclay (2013) find that the increase in donations diminishes over time when initially caused by a simple image of watching eyes.

There is also ample evidence that prosocial cooperation increases in the presence of an "observer," whether that observer is a real human, an image of "watching" eyes, or (primed) god notions (e.g., Nettle, Nott, and Bateson, 2012; Pfattheicher and Keller, 2015; Shariff and Norenzayan, 2007). This effect extends to various social settings, from littering (Ernest-Jones, Nettle, and Bateson, 2011; Bateson et al., 2013) to police body cameras (Ariel, Farrar, and Sutherland, 2015; Ariel, 2016).

Matching, as in a signaling framework, is a particularly relevant type of cooperation for investment games, and the ability to signal, sort, and/or match often arises from the principal's ability to observe some action by the agent. For example, Farrington (2019) finds that managers are more likely to engage in costly corporate social responsibility (CSR) acts when this choice to engage is subsequently observable by potential business collaboration partners (for a second-stage prisoners dilemma game).[3] Albert et al. (2007) find similar results when a player in an asymmetric prisoners' dilemma game can observe individual information (charity donations) regarding her partner, but not when she can observe similar

[2]Bateson et al. (2013) and Fathi, Bateson, and Nettle (2014) find that this effect is the result of increased prosocial behavior rather than increased conformance to social norms.

[3]She also finds that, if and only if CSR is disclosed, CSR managers are more likely to cooperate than non-CSR managers, and CSR managers are more likely to cooperate with other CSR managers than with non-CSR managers.

social information (the partner's membership to either a high- or low-average donation group).

There are many potential driving forces for the sundry prosocial behaviors that observation impacts, such as social esteem, which describes one's concern for their social image (e.g., Ellingsen and Johannesson, 2008; Gächter, Nosenzo, and Sefton, 2013; Bénabou and Tirole, 2006). In investment games, managers may act to project traits such as fairness, trustworthiness, intelligence, or even luck. Broadly speaking, there is a large literature examining how individuals often engage in costly behavior in the name of fairness (Forsythe et al., 1994; Eckel and Grossman, 1996; Hoffman, McCabe, and Smith, 1996; List, 2007; Engel, 2011), and there is also additional evidence that many individuals do so specifically as a means to *appear* fair (Andreoni and Bernheim, 2009; Ariely, Bracha, and Meier, 2009; Ubeda, 2014). Considering a specific context of fair behavior, Bénabou and Tirole (2010) discuss the role of social image in personal and corporate socially responsible behavior at length. Beyond the lab, concerns for a social image of fairness apply in many other settings, such as blood donation (Lacetera and Macis, 2010) and ethical purchasing (Friedrichsen and Engelmann, 2018).

In the right situation, even an individual who is largely indifferent to their social image may be induced to act differently if sufficiently pressured. This social pressure has been found to impact many prosocial behaviors, including mitigating tax evasion (Bosco and Mittone, 1997; Battiston and Gamba, 2016), promoting charitable giving (DellaVigna, List, and Malmendier, 2012), and increasing voter turnout (Casal and Mittone, 2016). Furthermore, Panagopoulos (2014) finds that even subtle, implicit social pressure can increase the voting rate. However, there is evidence that social pressure for reciprocal situations is stronger in the real world than in laboratory trust games (Baran, Sapienza, and Zingales, 2010).

Beyond social esteem, there is also a monetary incentive for a trustee to appear trustworthy in trust/investment game contexts. Consider the classic trust game: since the trustee divides the final value of the investment between the two players, a trustor should invest more when paired with a more trustworthy partner, ceteris paribus. Given that there is ample evidence showing a strong correlation between trustworthiness and generosity (e.g., Ashraf, Bohnet, and Piankov, 2006; Albert et al., 2007; Chaudhuri and Gangadharan, 2007; Blanco, Engelmann, and Normann, 2011; Elfenbein, Fisman, and McManus, 2012; Fehrler and Przepiorka, 2013; Przepiorka and Liebe, 2016), it follows that trustees also have a monetary incentive to appear generous. Interestingly, however, Gambetta and Przepiorka (2014) find that trustors discount the potential information (regarding a trustee's trustworthiness) contained in actions of generosity when that generosity could be a strategic signal relative to when it occurs naturally.[4]

[4]Gambetta and Székely (2014); Fehrler and Przepiorka (2016); and Bird, Ready, and Power (2018) find similar results.

Specifically within an investment context, observation effects may also stem from managers hoping to convince investors that they can "beat the market." In a traditional sense, fund managers often try to signal to investors that they are able to consistently earn higher returns than the market's average due to intelligence or skill.[5] This signaling can take place through the structuring of manager fees (Das and Sundaram, 2002; Gil-Bazo and Ruiz-Verdu, 2008) or managers' portfolio choices (Huddart, 1999). When it is salient that returns are orthogonal to intelligence and skill, however, and chance instead determines the result, the game reduces to no more than a standard gamble. To that point, there is extensive evidence that many people believe in and act in accordance with both the gambler's fallacy and the hot hand fallacy across a large variety of such settings, from the laboratory to casinos to when selecting shots in professional basketball games – and even when managing financial investments (e.g., Camerer, 1989; Croson and Sundali, 2005; Rao, 2009; Huber, Kirchler, and Stöckl, 2010; Rabin and Vayanos, 2010; Powdthavee and Riyanto, 2015). Thus, given that many individuals seem to believe in continued personal "luck" in many similar situations, it seems plausible that fund managers in purely chance-based markets may try to signal that they are lucky enough to consistently beat the market, though this remains an open empirical question.

Last, we consider that asymmetric information can provide a path to exploit moral "wiggle room" (Dana, Weber, and Kuang, 2007). In such situations, a player with private information generally has plausible deniability over related outcomes, which affords that player an opportunity to mislead the other player(s) regarding the true state of the world for personal financial gain. There is some evidence that this occurs in similar trust and investment games (Regner, Matthey et al., 2015; Gillies and Rigdon, 2017), despite the mixed and context-dependent evidence surrounding moral wiggle room more broadly (Grossman, 2014; van der Weele et al., 2014).

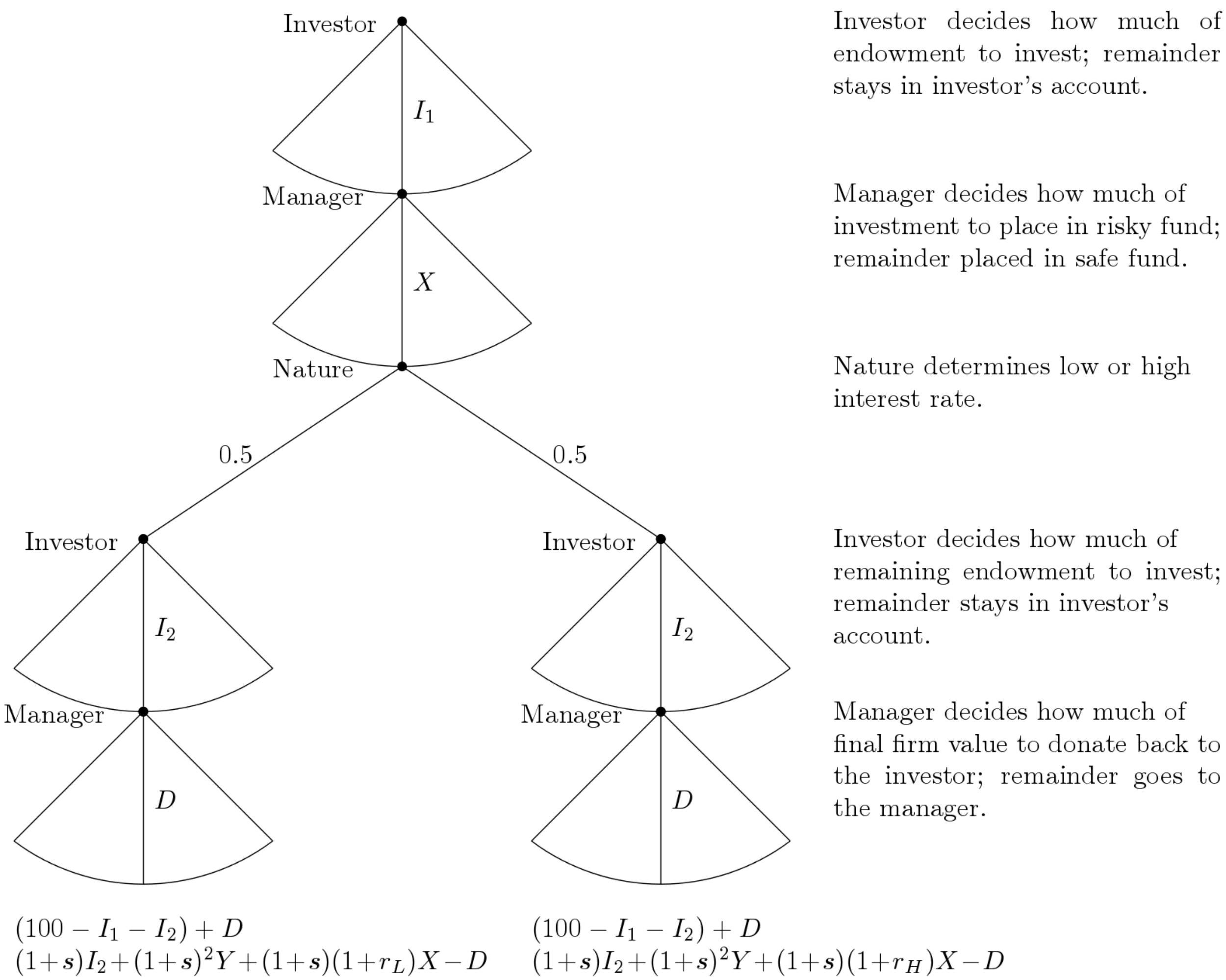
3 Experimental Design

3.1 The Game

We design a multi-stage trust/investment game with two players – an investor and a manager – as depicted in Figure 1. The investor is endowed with 100 tokens, of which she chooses $I_1 \in [0, 100]$ to invest in a "firm" run by the manager. The manager then splits this primary investment, placing $X \in [0, I_1]$ tokens into a risky fund and $Y = I_1 - X$ tokens into a safe

[5]There is also ample evidence of costly intelligence signaling solely in the name of social esteem across a variety of settings (Murphy, 2007; Montano-Campos and Perez-Truglia, 2019; Friedrichsen, König, and Schmacker, 2018; Ewers and Zimmermann, 2015; Bursztyn, Egorov, and Jensen, 2019). However, McManus and Rao (2015) find evidence that in at least one context, individuals who privately value their own intelligence seek to hide it from others.

Figure 1: Extensive Form Game Diagram



fund. Next, the risky fund's interest rate is realized from a publicly known distribution: $r \in \{r_L, r_H\} = \{0.6, 0.8\}$ where each outcome occurs with probability 0.5. The safe fund's interest rate is known to be $s = 0.7$. Both funds then earn interest at their respective rates, leaving the risky fund with $(1 + r) \cdot X$ tokens and the safe fund with $(1 + s) \cdot Y$ tokens.

Next, the investor chooses her secondary investment $I_2 \in [0, 100 - I_1]$ from what remains of her original endowment. Trivially, these tokens are placed in the safe fund. The two funds again earn interest, though both now earn the guaranteed rate of $s = 0.7$. Subsequently, the risky and safe funds contain $(1 + r) \cdot (1 + s) \cdot X$ and $(1 + s)^2 \cdot Y + (1 + s) \cdot I_2$ tokens, respectively. The sum of these two values is the "final firm value," denoted Z.

Last, the manager divides the final firm value, donating $D \in [0, Z]$ back to the investor and expropriating $E = Z - D$ for himself. Thus, the investor's account contains both the tokens she never invested and those she received from the final division, totaling $100 - I_1 - I_2 + D$

Table 1: Experimental Treatment-Outcome Combinations

Low Interest Rate (L)

Can the investor see the...		Interest Rate? No (IN)	Interest Rate? Yes (IY)
Split?	No (SN)	**SN-IN-L**	**SN-IY-L**
	Yes (SY)	**SY-IN-L**	**SY-IY-L**

High Interest Rate (H)

Can the investor see the...		Interest Rate? No (IN)	Interest Rate? Yes (IY)
Split?	No (SN)	**SN-IN-H**	**SN-IY-H**
	Yes (SY)	**SY-IN-H**	**SY-IY-H**

tokens. The E tokens expropriated by the manager are his total earnings in the game.[6]

The social welfare-maximizing outcome occurs when the investor invests her entire endowment at the beginning of the game. The manager's fund allocation does not affect the resulting expected final firm value of 289 tokens.[7] Under standard neoclassical assumptions, the Nash equilibrium occurs when the investor invests 0 tokens in both investment cycles. The resulting payoff is 100 (0) tokens for the investor (manager). In reality, however, we predict subjects' behavior will largely fall between these two ends of the spectrum, as is frequently found with trust and investment games.

3.2 Treatments

Across our first treatment dimension, we vary whether the manager's fund allocation is observable by the investor. In **SN** treatments, this **s**plit can **n**ot be seen by the investor, while in **SY** treatments, it can. Behavioral differences across this dimension produce the game's observation effects.

We test for these observation effects under two separate scenarios, defined along our secondary treatment dimension. Though the manager can always see the risky fund's realized interest rate, the realized **i**nterest rate can **n**ot be seen by the investor in **IN** treatments, while in **IY** treatments, it can.

Last, we distinguish between the risky fund's two realized interest rates: **L** denotes the **l**ow interest rate outcome, while **H** denotes the **h**igh outcome. Table 1 summarizes this 2×2×2 design by showing all four treatment combinations for the low (high) outcome on the left (right). The abbreviations for all 8 resulting treatment-outcome combinations are in bold.

[6] We phrased everything neutrally to subjects. For example, the game's two players were respectively referred to as "Player A" and "Player B" rather than "investor" and "manager," and the risky and safe funds were similarly referred to as "Fund X" and "Fund Y," respectively.

[7] Ex post, social welfare is maximized when the manager allocates the entire 100-token primary investment to the safe (risky) fund when the low (high) interest rate is realized, resulting in a final firm value of 289 (306) tokens.

3.3 Hypotheses

Our four (sets of) testable hypotheses correspond to the four decisions made in the game, ordered chronologically by the game's timing. Given the lack of existing experimental evidence, we are agnostic with respect to the direction of the observation effect for primary investment.

Hypothesis 1. The investor's primary investment when she cannot observe the manager's fund allocation is equal to that when she can. $\left(I_1\Big|_{SN} = I_1\Big|_{SY}\right)$.

Turning to the manager's allocation decision, signaling and social esteem literatures predict that the manager will make a safer allocation when he is being observed. However, there are mixed results regarding how observation impacts decisions involving risk more generally. Thus, in the face of these conflicting predictions, we formulate two competing hypotheses.

Hypothesis 2A. The manager allocates a smaller portion of the primary investment to the risky fund when his decision is not being observed than when it is $\left(\frac{100 \cdot X}{I_1}\Big|_{SN} < \frac{100 \cdot X}{I_1}\Big|_{SY}\right)$.

Hypothesis 2B. The manager allocates a larger portion of the primary investment to the risky fund when his decision is not being observed than when it is $\left(\frac{100 \cdot X}{I_1}\Big|_{SN} > \frac{100 \cdot X}{I_1}\Big|_{SY}\right)$.

As with Hypothesis 1, there is no existing experimental evidence regarding how secondary investment varies due to observation. However, if making safer fund allocations occurs as an effective signaling tool, then the investor should increase her secondary investment when she can observe the manager's fund allocation.

Hypothesis 3. The investor chooses a smaller portion of her remaining tokens as the secondary investment when she cannot observe the fund allocation $\left(\frac{100 \cdot I_2}{100 - I_1}\Big|_{SN} < \frac{100 \cdot I_2}{100 - I_1}\Big|_{SY}\right)$.

Last, existing literature provides no guidance on how the manager's division of the final firm value might be impacted by whether the investor can observe the fund allocation.

Hypothesis 4. Neither the amount that the manager donates back to the investor nor the amount he expropriates for himself are affected by whether the investor can observe the fund allocation $\left(D\Big|_{SN} = D\Big|_{SY} \text{ and } E\Big|_{SN} = E\Big|_{SY}\right)$.

3.4 Experimental Procedures

Students at the University of Arkansas at Little Rock were recruited via ORSEE (Greiner, 2015) to participate in this experiment, run at the campus's TREE Lab (TRojan Experimental Economics/finance Laboratory). As subjects entered the lab, they were randomly assigned to computer stations using lettered notecards. Subjects were able to see one another, but their screens and identities were kept private throughout. We ran the experiment using z-Tree (Fischbacher, 2007), ensuring full privacy, anonymity, and randomness.

Within each session, subjects played multiple repetitions of various treatments of the game, allowing us to exploit within-subject variation. Each experimental subject was randomly assigned a role – either investor or manager – which remained unchanged throughout the session.

At the beginning of the first repetition, the instructions were shown on-screen, as was our standard double-blind payment methodology (Hoffman, McCabe, and Smith, 1996), and investors and managers were randomly, anonymously paired. Pairs were redrawn with replacement before each repetition. For repetitions where gameplay changed to a different treatment, the new instructions were similarly shown. At the end of each session, one game repetition was selected at random, and the number of tokens subjects earned solely in that selected game repetition determined their actual monetary payouts. This prevents the need to control for individual wealth effects over the course of the experiment during analysis.

After subjects learned their payment amounts, they completed a questionnaire while the payment envelopes were being stuffed. As a consequence, subjects could neither receive their payments nor leave the lab sooner by rushing through the questionnaire. The questionnaire consisted of standard demographic questions and a Holt-Laury risk ladder (Holt and Laury, 2002).[8] The full instructions and questionnaire are in the online appendix.

We ran nine total sessions, which lasted roughly 90 to 120 minutes each, resulting in a total of 110 subjects and 381 relevant pairwise observations. Table 2 details which treatments occurred during which game repetitions within each session, as well as the per-session counts of subjects and of resulting useable pairwise observations.[9]

[8]In addition to the Holt-Laury risk ladder, we also build two subject-level measures of risk preferences specifically for managers based on their allocation decisions across all treatment SN-IN game repetitions that they play. The first measure is the subject's mean percentage of primary investment placed in the risky fund, and the second is the subject's total risky fund allocation as a percent of the total primary investment they receive. All robustness checks featuring these measures are in the online appendix.

[9]Note that in addition to the four treatments described in this paper, some sessions featured additional treatments that are not relevant to this study (denoted in Table 2 as "Other"). As part of a separate project, these treatments allowed the manager to pay "dividends" to the investor from the risky fund preceding the secondary investment. Throughout the paper, we include subject-specific random effects and cluster errors at the session level in all analysis (unless otherwise noted) to at least partially account for any potential impact of these sessions. We also run additional robustness checks specifically controlling for whether a subject has seen an "Other" treatment at any prior point. These regressions nearly always corroborate all our main

Table 2: Summary of Treatments by Session

Session	Subjects	Game Repetitions 1-3	4-6	7-9	10-12	13-15	Useable Observations
1	16	SY-IY	SY-IY	Other			48
2	10	Other	Other	SY-IY	SY-IY	Other	30
3	10	SY-IY	SY-IY	Other	Other		30
4	12	SY-IY	Other	SY-IY	SN-IN		54
5	16	Other	SY-IY	Other	Other		24
6	10	SN-IY	SN-IN	SY-IN	SN-IY		60
7	10	SY-IN	SY-IY	SN-IY	SY-IN		60
8	12	SN-IN	Other	SN-IN	SY-IY		54
9	14	Other	SN-IN	Other	Other		21
Total	110						381

Notes. "Other" denotes treatments that are not relevant to this study (see footnote 9). Observations from those treatments are not counted as useable observations.

Of those 110 participants, 86.4% were undergraduates and 52.3% were female.[10] The median age was 23 years, with a mean of 26.3 (SD: 9.5), which is not atypical of the university's population (no participants were under 18). There were black, white, Native American, and Asian/Pacific Islander participants, with 64.8% identifying as non-white or biracial. There were 3 white Hispanic and 2 non-white Hispanic participants. English was the first language of 73.6% of participants. Economics majors and minors made up 24.5% and 14.5% of participants, respectively, with 80.9% of all subjects reporting to be College of Business students. Subjects' median GPA was 3.265, with a mean of 3.168 (SD: 0.771). A full 53% of subjects reported having at least one parent with a bachelor's degree, while 8% reported having neither parent finish high school (or equivalent). At an exchange rate of 10 tokens to 1 US Dollar, participants earned an average of $19.88, including a $10 participation fee.

4 Results

Table 3 provides means for the variables of interest for each of the four treatments, as well as when pooled together across all treatments. The variable notation follows directly from Section 3, while "N" represents the number of observations.[11]

results. These robustness checks are in the online appendix.

[10] The subjects in session 7 were able to skip some demographic questions due to a technical issue. Numbers reported only reflect those who responded to each given question.

[11] For two variables, the value of N is smaller than reported in Table 3 for some treatments due to observations where the denominator equals zero: for $\left(\frac{100 \cdot X}{I_1}\right)$, N=79 in treatment SN-IN and N=176 in

Table 3: Summary Statistics – Means

	Treatment				
Variable	SN-IN	SY-IN	SN-IY	SY-IY	Pooled
Primary Investment (I_1)	42.678	47.556	46.933	52.413	48.892
Risky Fund Allocation (X)	18.678	22.289	21.644	25.189	22.890
Safe Fund Allocation (Y)	24.000	25.267	25.289	27.224	26.003
Risky Fund Allocation % $\left(\frac{100 \cdot X}{I_1}\right)$	44.192	43.854	50.507	47.934	46.880
Realized Interest Rate (r)	0.702	0.702	0.702	0.704	0.703
Secondary Investment (I_2)	15.767	19.533	23.711	11.502	14.900
Secondary Investment % $\left(\frac{100 \cdot I_2}{100 - I_1}\right)$	32.024	44.261	50.923	30.636	35.627
Final Firm Value (Z)	150.400	171.444	176.667	171.403	167.068
Returns to Investor (D)	47.422	53.111	57.133	64.308	58.150
Expropriations by Manager (E)	87.611	105.400	101.889	91.751	93.583
N	90	45	45	201	381

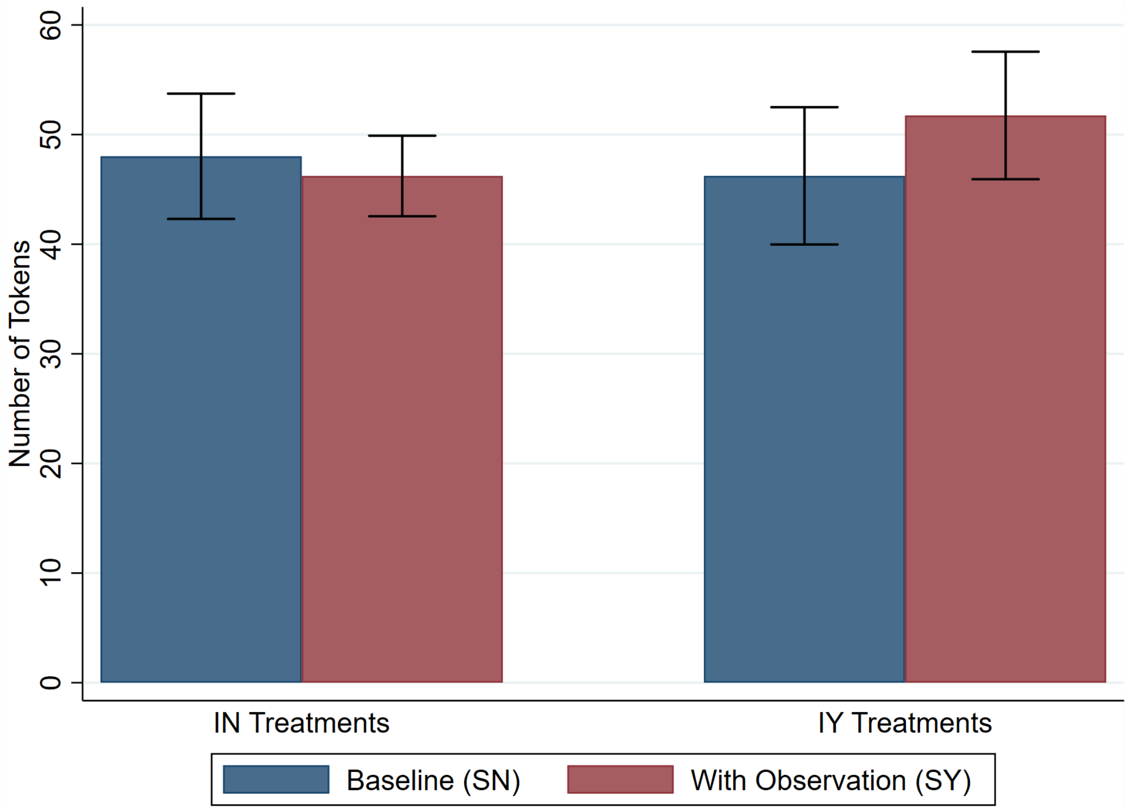
Notes. In SN (SY) treatments, the investor cannot (can) see the manager's risky/safe fund allocation. In IN (IY) treatments, the investor cannot (can) see the risky fund's realized interest rate. For the two percentage variables, the value of N sometimes differs from that of other variables within the same treatment due to the denominator being zero for some observations (see footnote 11).

At first glance, Table 3 contains a few noticeable examples of observation impacting behavior. Perhaps the most intuitive is row four: the mean percentage of the primary investment that managers allocate to the risky fund decreases when this allocation decision is visible, especially within the IY regime (50.5% to 47.9%). We use these means comparisons as motivation suggesting that further exploration is warranted.

There are four total behavioral choices in our experiment: the investor's primary investment, the manager's fund allocation, the investor's secondary investment, and the manager's end-game division of the final firm value. In a sense, a change in the manager's allocation behavior is the most "direct" observation effect because it shows how a behavior by an individual changes when that specific behavior is being observed. However, the anticipation of and/or reaction to the investor's observability of the allocation decision may also drive differences in *other* related behaviors. We consider these to be "indirect" observation effects. We examine the observation effect for each choice variable in chronological order through the game.

treatment SY-IY; for $\left(\frac{100 \cdot I_2}{100 - I_1}\right)$, N=158 for treatment SY-IY.

Figure 2: Observation Effect for Primary Investment (I_1)



Notes. GLS estimated with subject-level random effects and session-level error clusters. Error bars indicate 1 standard error.

4.1 Primary Investment

We first compare the primary investment I_1 when the investor can observe the manager's subsequent allocation of that investment between the risky and safe funds (SY treatments) to that when she cannot (SN).[12] We test this both when the risky fund's realized interest rate is known only to the manager (IN) and when it is public knowledge (IY). We include treatment-specific indicators as covariates and subject-level random effects in our GLS estimation, and we cluster the error term at the session level.[13] Figure 2 details the results of this estimation.

The primary investment is slightly greater in baseline treatment SN-IN (48.019) than in treatment SY-IN (46.223), all else equal, though the difference is not statistically significant ($p > 0.1$). Within the IY regime, the primary investment when investors cannot subsequently view managers' allocations (46.234) is smaller than when they can (51.741), though the difference again lacks statistical significance.[14]

[12]Note that because the investor's initial endowment is 100 tokens, the number of tokens invested is equivalent to the percentage of tokens invested.

[13]We use this same error-term structure for all subsequent analysis unless otherwise noted.

[14]We additionally run several robustness tests for this and all other results. For instance, we add controls

Result 1. There is no evidence that primary investment differs due to an observation effect, regardless of whether the risky fund's realized interest rate is public knowledge.

4.2 Managers' Allocations: Risky Fund vs. Safe Fund

After the primary investment is made, the manager must decide how to split that investment between a risky fund and a safe fund. We examine how the percent of the primary investment placed into the risky fund differs when this allocation is observable by the investor as our "direct" observation effect. In addition to treatment-specific intercepts, our main specification also includes treatment-specific effects for income, income squared, and income cubed as control variables.[15]

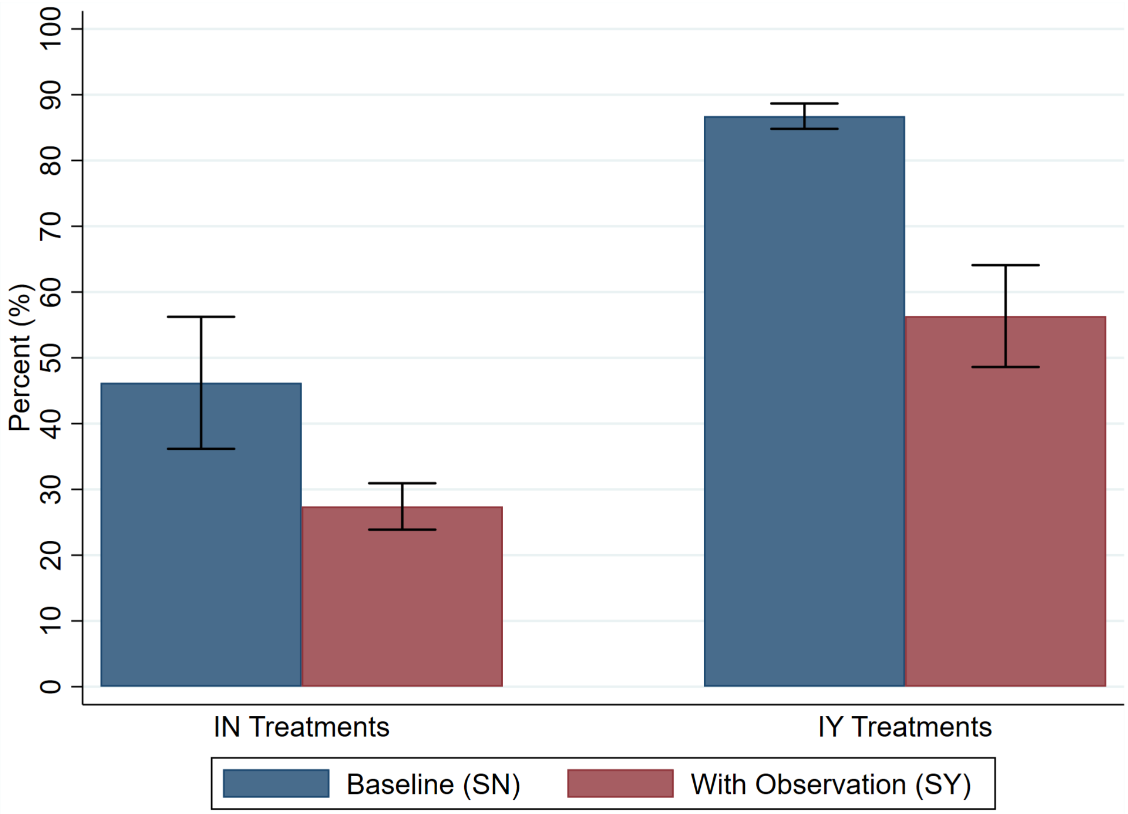
Figure 3 shows the main results of this GLS estimation. Within the IN regime, managers place a smaller percentage of the primary investment into the risky fund when they can be seen doing so (27.403%) than when they cannot (46.198%). While this drop is statistically significant ($p = 0.096$), it loses statistical significance in the vast majority of our robustness checks. Thus, given that investors cannot see the risky fund's realized interest rate, there is weak evidence that the direct observation effect is negative, supporting Hypothesis 2A over competing Hypothesis 2B.

The third and fourth bars of Figure 3 show the analogous estimates within the IY treatments. We find that disclosing managers' fund allocation decisions decreases the percent of the primary investment that managers place in the risky fund by 30 percentage points ($p < 0.0005$), from 86.737% to 56.346%. This effect is negative and statistically significant across nearly all robustness checks. This is strong evidence that if investors can see the risky fund's realized interest rate, the direct observation effect is negative, again supporting

for demographic information (sex, race using indicators by each race, race using an indicator for nonwhite, Hispanic, and whether English was their first language), whether they study economics (majors and minors separately, as well as pooled), and risk preferences (Holt-Laury risk ladder score, dummies for risk-seeking and risk-averse based on Holt-Laury score, and the two measures discussed in footnote 8 – though these two are only useable when the manager is the decision-maker). We also trim off all observations that represent an individual's first exposure to a new treatment to test for learning/adjustment effects (LaRiviere, McMahon, and Neilson, 2018), include indicator variables for whether a subject has previously seen each separate treatment, and run tobit models for each of our main specifications. All robustness check results are available in the online appendix. These tests largely corroborate our results.

[15]Both previous literature, including work by Ostrom and Walker (2003), D'Exelle and Verschoor (2015), and Delis and Mylonidis (2015), and intuition regarding the setup of our experiment suggest the possibility of a nonlinear relationship between the number of tokens invested in the primary investment and the percentage of that investment that is placed in the risky fund. We chose a cubic specification for our main estimation using a non-parametric quantile-based approach. Because income is a control variable rather than a variable of interest, we omit the intuition discussion and quantile-based regression results from the text. The online appendix contains the full results – including the estimated effects for the control variables – of the quantile-based regressions, the main specification, and a robustness check dropping all cubic and quadratic income effects.

Figure 3: Observation Effect for Percent Allocation to the Risky Fund $\left(\frac{100 \cdot X}{I_1}\right)$



Notes. GLS estimated with subject-level random effects and session-level error clusters. Controls include treatment-specific cubic, quadratic, and linear income effects. Error bars indicate 1 standard error.

Hypothesis 2A.[16]

Result 2. Allowing the investor to see the manager's fund allocation leads to a more risk-averse allocation. This effect is both more pronounced and more robust when the risky fund's realized interest rate is public knowledge.

4.3 Secondary Investment

After the manager's allocation decision, the risky fund's interest rate is realized, the two funds earn interest at their respective rates, and the investor then has a second chance to invest. We

[16] We also examine time spent on the decision screen as the dependent variable in all our main result specifications (with an added control variable indicating the first time a subject faces a new treatment) to look for how observation impacts decision speed. Interestingly, the only result we find is that, within the IN (IY) regime, managers spend 20.6 (14.6) fewer seconds making their allocation decision in the SY treatment than in the SN treatment. We posit that in the SY treatments, subjects can deduce the social expectations and react accordingly relatively quickly. When the decision is not being observed, however, there is no social pressure, and thus managers spend more time analyzing which option best aligns with their own preferences. Future research should explore this idea more fully.

Table 4: Observation Effect for Secondary Investment Percentage $\left(\frac{100 \cdot I_2}{100 - I_1}\right)$

Dep Var:	$\frac{100 \cdot I_2}{100 - I_1}$
Treatments	
Treat IN: Baseline	37.614***
	(5.939)
Treat IN: Observation Effect	-13.267*
	(6.867)
Treat IY-L: Baseline	35.248***
	(4.222)
Treat IY-L: Observation Effect	-0.183
	(8.460)
Treat IY-H: Baseline	41.419***
	(7.493)
Treat IY-H: Observation Effect	-15.379
	(10.691)
Controls	
Treat SY-IN $\times \frac{100 \cdot X}{I_1}$	0.178***
	(0.068)
Treat SY-IY-L $\times \frac{100 \cdot X}{I_1}$	-0.013
	(0.138)
Treat SY-IY-H $\times \frac{100 \cdot X}{I_1}$	0.184
	(0.114)
N	292
N_i	47

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes. GLS estimated with subject-level random effects and session-level error clusters.

examine how this secondary investment I_2 as a percentage of possible secondary investment $(100 - I_1)$ varies due to the observation effect. Table 4 shows these GLS estimation results.

In the IN treatments, because the investor cannot see the risky fund's realized interest rate, she cannot use it as an input when making her secondary investment decision. Hence, we aggregate our independent variables to the treatment level for both IN treatments, as shown in Rows 1, 2, and 7 of Table 4, rather than separating them by outcome. Row 1 shows the baseline: 37.614% of investors' remaining endowment makes up the secondary investment in treatment SN-IN. When investors can observe allocation decisions, secondary investment drops by a statistically significant 13.267 percentage points ($p = 0.053$), as seen in Row 2. However, we caution that this drop may surface simply due to a difference in how investors choose their secondary investment between the SN-IN and SY-IN treatments. When the manager's fund allocation is hidden, the investor has no information to use when choosing her secondary investment, while when the manager's allocation is observable, the investor can use that allocation as an input when choosing her secondary investment. Indeed, Row 7 shows significant evidence that the latter occurs ($p = 0.009$). A 6-percentage-point increase in the percent of investment allocated to the risky fund leads to a 1-percentage-point increase in the secondary investment. Thus, we take this "observation effect" for comparing differences in treatment intercepts for secondary investment with a grain of salt.

Turning to where investors can see the risky fund's realized interest rate, we separately examine the IY treatments based on which rate is realized. Rows 3 and 5 show baseline secondary investments of 35.248% and 41.419% when the low and high interest rates are realized, respectively. However, as Rows 4 and 6 illustrate, there is no significant evidence of an observation effect in either direction here, regardless of the risky fund's realized rate.

Result 3. Allowing the investor to observe the manager's fund allocation does not impact the investor's subsequent secondary investment decision.

Interestingly, unlike with treatment SY-IN, we do not find evidence that investors base their secondary investment decisions on managers' fund allocation within either SY-IY-L or SY-IY-H. We previously posited that the decrease in the treatment-specific intercept for secondary investment within the IN regime is actually a reflection of the additional information (managers' allocations) that investors can and do use when choosing secondary investment. We conversely posit the analogous point for both SY-IY outcomes: since there is no evidence that investors use managers' allocations when choosing secondary investments, as shown in Rows 8 and 9, it then follows that managers have roughly the same investment percentages in SY-IY as in SN-IY, as Rows 4 and 6 illustrate. Thus, while this evidence is far from conclusive, it does corroborate our evidence regarding secondary investment choices within the IN regime.

4.4 Division of Final Firm Value

Last, we examine observation effects with regards to the final decision of the game: the manager's division of the final firm value between himself and the investor. We measure this by examining both the number of tokens the manager donates to the investor D and the number of tokens the manager expropriates for himself E. For each dependent variable, we consider the same two model specifications. Our main specification includes the treatment-specific indicators of interest and controls for the four potential sources of the final firm value: primary investment, how primary investment is split between the two funds, the risky fund's realized interest rate (via an indicator variable), and secondary investment. Our alternate specification splits the treatment indicators into treatment-outcome indicators and correspondingly drops the separate indicator variable for outcome H from the controls. The left two columns of Table 5 show the main specification estimates for dependent variables D and E, respectively, while the right two columns show the corresponding estimates using the alternate specification. All four estimations use GLS.

When the investor cannot see the risky fund's realized interest rate (IN treatments), Row 2 of Columns 1 and 2 indicates that allowing the investor to see the manager's split of the primary investment (SY) has no significant marginal impact on either the number of tokens returned to the investor or the number expropriated by the manager. Similarly, when we separate each treatment by outcome, Rows 6 and 8 in Columns 3 and 4 show the same lack of impact on both donations and expropriations, regardless of which outcome is realized. We posit that there is no effect because in both treatments SN-IN and SY-IN, the manager has (ex post) plausible deniability about how lucky he was regarding the risky investment.[17]

Result 4A. When only the manager can see the risky fund's realized interest rate, allowing the investor to observe the manager's fund allocation impacts neither the amount the manager donates back to the investor nor the amount he expropriates for himself.

When the investor can see the risky fund's realized interest rate (IY), however, Row 4 of Columns 1 and 2 shows that disclosing the manager's fund allocation leads him to donate 13.216 more tokens to the investor ($p = 0.046$) and expropriate 14.837 fewer tokens for himself ($p = 0.030$), ceteris paribus. When we disaggregate this effect by realized interest rate, the results are largely the same. Row 12 shows that when the high rate is realized, the results are nearly identical to the aggregated results from Columns 1 and 2. When the low rate is realized (Row 10), the effect on donations is still positive, though no longer statistically significant, and the impact on expropriations is again negative and statistically significant.[18]

Result 4B. When the risky fund's realized interest rate is public knowledge, allowing the investor to observe the manager's fund allocation increases the amount the manager pays back to the investor and decreases the amount he expropriates for himself.

While the overall result here differs from the null found for the IN treatments in Result 4A, Result 4B is again consistent with the same information asymmetry story. In treatment

[17] Consider treatment SY-IN. If the manager allocates heavily toward the risky fund, he can plausibly "claim" the low interest rate was realized. If he allocates heavily toward the safe fund, he can plausibly claim the high rate was realized. Either is (ex post) the "unlucky" outcome given his allocation. In this way, the manager can act as if he were unlucky regardless of the actual outcome in both IN treatments.

[18] We also run both ex post power analysis calculations and an ex post power test, which uses 10,000 runs of a Monte Carlo simulation based on our observed effect sizes and standard deviations (see the online appendix). The main conclusion from these is that only Row 6 of Table 4 is likely to become statistically significant and counter our main findings if we increased statistical power by increasing our sample size. Power analysis suggests this would require 256 pairwise observations in treatment-outcome SY-IY-H. On the other hand, however, increased sample size/statistical power would also likely turn the coefficient estimate for Column 3, Row 10 of Table 5 statistically significant, which would strengthen Result 4B.

Table 5: Observation Effect for Investor Backpayment (D) and Manager Expropriation (E)

Dep Var:	(1) D	(2) E	(3) D	(4) E
Treatments				
Treat IN: Baseline	-22.894**	18.491*		
	(10.087)	(9.863)		
Treat IN: Observation Effect	-9.077	9.998		
	(10.201)	(9.711)		
Treat IY: Baseline	-30.970**	27.602**		
	(12.119)	(12.273)		
Treat IY: Observation Effect	13.216**	-14.837**		
	(6.614)	(6.824)		
Treat IN-L: Baseline			-27.056***	23.147**
			(9.233)	(9.307)
Treat IN-L: Observation Effect			-3.670	4.653
			(3.953)	(3.528)
Treat IN-H: Baseline			-14.139	18.134*
			(10.489)	(9.655)
Treat IN-H: Observation Effect			-12.950	13.562
			(17.334)	(17.011)
Treat IY-L: Baseline			-29.443***	26.582**
			(10.715)	(10.680)
Treat IY-L: Observation Effect			10.734	-13.493*
			(6.927)	(7.119)
Treat IY-H: Baseline			-26.670**	31.405***
			(11.566)	(11.428)
Treat IY-H: Observation Effect			15.493**	-15.727**
			(7.051)	(7.230)
Controls				
I_1	✓	✓	✓	✓
$\frac{100 \cdot X}{I_1}$	✓	✓	✓	✓
Treat H	✓	✓		
I_2	✓	✓	✓	✓
N	345	345	345	345
N_i	55	55	55	55

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes. GLS estimated with subject-level random effects and session-level error clusters.

SN-IY, the manager has ex post plausible deniability regarding his luck because the investor cannot observe how the manager splits the primary investment.[19] In treatment SY-IY, however, the manager no longer has plausible deniability regarding his investment choice: the investor can see both the manager's fund allocation and the risky fund's realized interest rate, and so she knows the exact number of tokens in each fund throughout the game. Thus, without the veil of plausible deniability to hide behind, the manager faces increased social pressure to divide the final firm value more fairly at the end of the game in treatment SY-IY than in treatment SN-IY. This stands in contrast to Result 4A, where the manager has plausible deniability in both treatments SN-IN and SY-IN.

5 Discussion and Conclusion

We examine the effects of observation within an investment game framework, both when only the managers know the risky fund's realized returns and when the returns are public knowledge. We find no observation effect on either primary or secondary investment. However, we find that managers' fund allocations are more risk-averse when investors can observe these allocations, and this effect is both more pronounced and more robust when the risky fund's realized rate is public knowledge. We consider this a "direct" observation effect because an investor observing a manager's decision causes a difference in the manager's behavior for that same decision.

The literature provides a handful of possible explanations. Within an investment game framework similar to ours, results of matching based on types found by Albert et al. (2007) and Farrington (2019) would translate to managers allocating tokens to match according to investors' risk preferences. While our experimental design does not allow for investors to directly reveal their risk preferences, managers may act based upon their own prior beliefs regarding the risk preference distribution of the general (or sample) population. Managers may also adjust their allocations as they learn more about other subjects' risk preferences through game repetitions.[20] Future work should elicit managers' beliefs about the distribution of other players' risk preferences to examine this.

Managers may also allocate primary investments as a means to project an image of themselves, either for direct monetary gain or for non-monetary psychic benefits. To the former point, managers may attempt to reveal their personal characteristics to induce investment, such as their ability to "beat the market." Salient to all parties, market returns

[19] If the low rate is realized, he can plausibly claim to have allocated heavily toward the risky fund. If the high rate is realized, he can plausibly claim to have allocated heavily toward the safe fund. Either is the "unlucky" allocation given the observable realized rate.

[20] Even with anonymous matches and random rematching for each game repetition, managers can potentially still glean some information on the distribution of risk preferences in the room, especially in small sessions.

in our game are entirely random. Consequently, neither intelligence nor skill can be used to "beat the market" in any way. However, to the extent that players believe in continued luck, such as with the hot hand fallacy, fund managers may allocate monies strategically to signal their personal "luckiness" to investors.

Separately, managers may also financially benefit by projecting an image of themselves as trustworthy or fair.[21] In our game, managers have no means to signal these traits before the primary investment, but in some treatments they can use their fund allocations to send such signals before the secondary investment. This signaling story yields three testable hypotheses for a separating equilibrium: signals are a reasonably credible reflection of manager type, managers send signals when possible, and investors respond to the signals as managers intend. Our direct observation effect supports the second of these hypotheses, but we find no evidence supporting the other two. Echoing the existing literature, we find no link between risk preferences and trustworthiness: managers' relative allocations of primary investments have no discernible relationship to the amount they repay investors.[22] Furthermore, we find no evidence that these signals work as intended. Investors' secondary investments are not affected by managers' allocations (when visible). This lack of response may be due to the cost of sending the signals being too low, which mirrors findings by Gambetta and Przepiorka (2014) regarding generosity signaling. Future work should look to increase the cost of trustworthiness signaling in an investment context to see at what point a separating equilibrium can be established, if at all.

Alternatively, managers' social esteem may drive them to project a particular social image of themselves for non-monetary, psychic benefits. The literature on social esteem and social pressure shows that many individuals are willing to bear explicit costs in exchange for appearing trustworthy, for example, which could explain the direct observation effect.[23] Interestingly, we could find no existing literature on individual preferences for projecting a social image as a "lucky" (or "unlucky") person, which could be a promising avenue for future work.

Last, we find extensive evidence of an observation effect on the managers' division of the final firm value. When the risky fund's realized rate is public knowledge, managers conclude the game with larger back-payments to investors and smaller expropriations for themselves

[21] Because more trustworthy managers pay higher returns, they should attract more tokens from investors. Thus all managers should want to appear as trustworthy.

[22] We also find this lack of statistical significance in 59 of 60 relevant robustness checks, as seen in the online appendix.

[23] Of course, a manager's only ex ante "cost" that drives this difference in allocations is that it pits his social image concerns against his personal risk preferences. This cost is essentially a psychic cost rather than a monetary one, but it could still lead an image-concerned manager to allocate investments differently than his risk preferences alone would dictate (depending on the relative magnitudes of his social image and risk preferences).

when investors can see managers' fund allocations. This effect is not present when only managers know the risky fund's realized rate, however.

While this result may also be due to social pressure, it could alternatively stem from the opportunity for the manager to exploit moral "wiggle room" (Dana, Weber, and Kuang, 2007) that arises due to information asymmetry in three of our four treatments. There is no moral wiggle room to exploit if and only if the investor knows both the manager's fund allocation and the risky fund's realized interest rate. In this full information treatment, the investor can fully trace each token throughout the game. When the investor cannot see the manager's fund allocation, however, the manager can ex post plausibly "claim" to have made what ultimately turned out to be an unlucky allocation decision.[24] On the other hand, when the investor can see the manager's fund allocation but cannot see the realized interest rate, the manager can plausibly claim to have been unlucky regarding which rate was realized.[25] Of course, the manager has even more wiggle room when the investor can see neither aspect. To see whether players truly value this information, additional treatments could allow for investors to pay to reveal it – or for managers to pay to hide it – in addition to our mandatory disclosure treatments.

Of course, these possible explanations are not an exhaustive list. Our results provide guidance in that they suggest which causes should be explored in more detail. Future work should disentangle these possible explanations to explicitly determine causality. For example, adding a treatment where a third party observes the manager's allocation rather than or in addition to the investor observing it would separate the "pure" observation effect from information asymmetry-induced moral wiggle room.

[24] That is, if the investor sees that the risky fund earned the high (low) rate, then the manager can claim he allocated more heavily toward the safe (risky) fund.

[25] If the manager visibly invests heavily in the risky (safe) fund, then he can claim that the risky fund earned the low (high) interest rate.

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