Investors Have Feelings Too

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Abstract

We experimentally investigate the relationship between an investor and a project manager. Project managers choose from a pool of projects, the success probabilities of which are uncertain. Information about the future success probability of a project is gained by observing its outcome. Investors can change projects, but also have to change project managers if they want to do so. An additional joint project or a voluntary transfer precedes their interaction. We hypothesize that investors favor projects that are managed by project managers with whom they have shared positive experiences in the past, even though these past experiences do not provide any information about a project's success probability. The role of this social element is isolated using a control treatment in which the role of the project manager does not exist. Interaction through a voluntary transfer plays a clear and significant role in the investors' decision making, whereas the influence of merely sharing a positive or negative experience proves more complex.

Keywords: Relationship banking, experiment, relationships, social ties, group formation, investment

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1 Introduction

Relationships matter. This statement is true not only for everyday human interaction, but also when it comes to business. The experiment presented here was designed to shed further light on the role of relationships in one specific context: the interaction between an investor and a project manager, who can either be retained or replaced by a new project manager, following different experiences shared with that manager. In particular, we are focusing on the role that affect can have in this context. How affect is directed and how it develops are questions that have long been at the center of social psychology research, but have entered the field of experimental economics only fairly recently. We try to shed some more light on this issue in the specific context of an investment game, excluding the trust element that is often at the center of such games in an experimental context and focusing purely on social preferences.

Relationship banking is an important topic in microeconomics and finance (Boot, 2000) and has attracted attention in experimental economics (Brown and Zehnder, 2007; Cochard et al., 2004; Cornée et al., 2012). In both fields the focus has been on the strategic motives that come into play once an investor-borrower relationship extends through time. While it is of great relevance to answer questions such as the role of trust (Houser et al., 2010), regulation (Cornée et al., 2013; Brown and Serra-Garcia, 2014; Fehr and Zehnder, 2009; Lunawat, 2013), and reciprocity (Cochard et al., 2004), we believe that there is one more aspect that is part of such relationships. Any repeated interaction with another person may trigger emotional reactions in at least a subset of subjects. Inspired by experimental studies on affective relationships (van Dijk et al., 2002; Hoyer et al., 2014), our goal here is to contribute to the understanding of such non-strategic factors in the asymmetric context of the investor-borrower relationship with its inherent power imbalance. In further contrast to much of the research in the area of trust games, our focus is on the behavior of the investor, rather than the recipient of an investment. We are interested in the way that a personal relationship affects the decision making of the investor, and how it affects the way

that the investor interprets information about the value of the investment opportunities as presented by the borrower, relative to the value of investing with another borrower. The individuals who make investment decisions within organizations are only human, hence it should be of relevance for any organization to understand what drives the behavior of those who make decisions in its name.

The specific setting studied in this experiment is a repeated investment decision, wherein subjects decide whether to proceed investing with project managers that they have been in contact with previously or to let them go and invest with a new project manager instead. There is anecdotal and empirical evidence that personal relationships play an instrumental role in banking. The following quote, taken from Uzzi (1999), who collected field data from lending officers, illustrates this notion: "After he [the entrepreneur] becomes a friend, you want to see your friend succeed and that goes along many lines. If I can be a part of helping them do that, it's a real good feeling and I'm providing a service not only to them but their employees.... So there's a lot of things that you kind of from a moral standpoint take into effect.... That is kind of a side effect of your relationship." However, there are also pitfalls in relationship banking, such as the hold-up problem and soft budget constraints, which can distort borrowers' incentives ex post if the lender finds herself forced to grant more credit just to preserve an earlier investment (Boot, 2000). We can point to more practical examples of situations in which an excessive focus on the relationship aspect of banking can have detrimental effects. A practical and dramatic example concerns the infamous Anglo Irish bank, the downfall of which contributed gravely to the struggles of the whole Irish economy during the financial crisis of the late 2000s, partly driven by the excessive interweaving of its fortunes with those of its lenders (Carswell, 2012). More to the point of this paper, such relationships also have the potential to decrease the effectiveness of an investor to identify the most valuable projects to invest in. Relationship banking can hence have both positive and negative sides, which we will observe in our experiment.

When the early stage of a project provides negative signals about its value, investing more

money into a partner can become a question of loyalty[1]. Stopping to provide financing to a borrower, firing an employee, and other self-interested acts that come at somebody else's cost are difficult, especially if we share a history with that person. It is human nature to feel responsibility for others and abandoning others goes against human nature. This is even more so as it is often difficult to evaluate which choice is going to be most profitable. In fact, when it comes to investment opportunities, especially in the context of venture capital, even repeated signals of bad performance do not necessarily imply that a company is not going to be successful. This is exemplified in the phenomenon of "pivoting"businesses, which change their business model before finding success[2]. At the same time, taking money away from an investment to put it into a completely new project means transferring it somewhere where there is even less available information. Because of the important role that uncertainty plays in the way that people make economic decisions, especially in a social context (Bosman and van Winden, 2010; Brock et al., 2013; Cappelen et al., 2013), the risk structure outlined in this paragraph is mimicked in the design of our experiment.

We contribute to the analysis of the nature of investment relationships by isolating the role of shared experiences with a project manager from the predictive power that such experiences might have for the future profitability of a project. Specifically, we investigate two different mechanisms: one in which project managers are given the opportunity to send or withhold a monetary transfer at the beginning of an interaction, and another in which investors merely experience success or failure of an independent additional project that is chosen by project managers at the beginning of the interaction. In both cases investors share an experience with a manager when they have to decide whether to stay with this manager or not: one that is positively charged (transfer or successful previous project) and one that is negatively charged (no transfer or failed previous project).

[1]It should be noted that our design restricts the action space of the investor to exiting the relationship, as opposed to voicing dissent with the quality of the project when loyal, using Hirschmann's terminology (Hirschman, 1970).

[2]Examples are Twitter (Carlson, 2011), Paypal (Penenberg, 2011), GroupOn (Penenberg, 2011) and Buzzfeed (Kafka, 2015)

In the experiment the best response of an investor is not affected by the type of experience. What we are investigating is the question if they react to the different histories nonetheless, and if so, why. In the treatment that uses transfers, reciprocity provides a motivation for deviations from the pure best response. Moreover, this treatment shows some similarity to an experiment of Malmendier and Schmidt (2012), which finds such effects[3]. The impact of a shared history is not as obvious. However, there is a number of concepts that drive our hypothesis of a potential effect of such experiences. For example, investors could be driven to more positive reactions towards managers with whom they have shared positive experiences in the past on the basis of simply attaching a positive emotion to that interaction. Negative experiences could trigger the opposite reaction. Evidence suggests that even simple subliminal stimuli can cause liking or disliking, as demonstrated by mere exposure experiments (Zajonc, 2001)[4]. As we will see in the Design section, our managers' decisions essentially lead to random results, but investors might nevertheless attribute the success of the project to the manager's capability of selecting profitable projects. There is also evidence in experimental economics of an effect of unjust blame. Gurdal et al. (2013) show that principals routinely punish managers for events they had no influence on. Further arguments in support of our hypothesis of a "mere experience" effect will be discussed in section 2.

We find a strong difference in the investors' decisions after either having received a transfer or not and this reaction is significantly different from their behavior in a non-social control treatment without a project manager. We are not able to detect an increase in reaction to an experience shared with a manager relative to the non-social control treatment, despite the fact that post-experiment questionnaires indicate that a subset of investors reacted to the experience emotionally. Furthermore, decision times are similarly and significantly affected by the presence of a manager.

[3]There are some notable differences between their experiment and ours; see section 2.

[4]There is also evidence that neurological processes related to preference ordering are activated when cues are not consciously recognizable (Pessiglione et al., 2008), and that subjects may unconsciously learn how to perform a task (Lebreton et al., 2009).

We begin with a literature survey in section 2. Section 3 presents the experimental design, together with an analysis of the investors' best responses and our hypotheses ins section 3. Results are presented in section 4, followed by a brief discussion in section 5. Section 6 concludes.

2 Literature

The relevance of relationships in the context of lending and borrowing has been recognized for a long time. In line with that, the term "relationship banking" has become a staple of the literature (Boot, 2000). Seeing how the act of providing credit to somebody else implies some expression of trust, this is hardly surprising: relationships can help facilitate trust-based interactions on a multitude of levels.

One element of relationship banking has only recently become actively researched: the creditors' preferences about whom they actually want to grant credit to, everything else being equal. Research on social distance (Goette et al., 2012) suggests that, if given a choice, people are much more cooperative towards people that they share social ties with. We are building on this idea.

Theoretically there is a strong connection between this paper and social distance theory (Tajfel and Turner, 1979), insofar as one could look at the initial partner allocation in our experiment as related to the minimal group paradigm[5]. In a laboratory setting such minimal groups can lead to significantly more cooperative behavior in different environments, including investment situations. Examples within experimental economics can be found in Charness et al. (2007) and Chen and Li (2009). Akerlof and Kranton (2000, 2005) provide important arguments as to the role of identity in situations such as the one analyzed here, but differ to some extent in that the implicit focus lies on relatively low level members of an organization, such as employees, or, in our case, project managers. We explicitly focus on the role that identity plays for subjects that are better positioned in the hierarchy compared to those they interact with, as expressed through framing, their decision power, and outside options. We therefore look at a positive history as a source of a group identity that makes our investors look at their relationship with the relatively powerless managers as a team relationship.

[5] As we will see later, our investors actively choose partners, but do so in complete ignorance of who it is that they are choosing.

There are a number of field studies that present evidence for the role of social relationships in the context of lending, especially using data from developing countries. Khwaja and Mian (2005) for example find that in a Pakistani sample firms with good political connections receive 45% bigger loans, even though they show 50% higher default rates. In a study that uses data from a highly developed country, Haselmann et al. (2014) find significantly positive effects for the influence of social proximity on lending in a German dataset of local banks. One of the measures used for social proximity is the shared service club membership of local bank board members and firm CEOs[6].

The German example demonstrates that also in countries in which outright corruption is thought to be limited, social connections matter. We are not implying that such effects are necessarily indicative of corruption, or even merely inefficient: it is possible that the social connections that are being studied give banks better access to information that is vital in making an informed decision about which type of credit to grant. Even rather simple forms of relationships between a bank and a borrower can help reduce the default risk (Puri et al., 2013), implying that there is an objective value in these relationships[7]. Separating valuable information from favoritism is difficult to do in the field, opening up the potential for additional insight to be gained from a laboratory experiment.

Since we are interested in the relationship between a financier and a borrower, there is a connection to trust or investment games, which are widely used in the experimental economics literature, typically in some variation of the design of Berg et al. (1995). In these games an investor transfers a certain amount of money to a borrower, who has access to technology that can potentially increase the value of the investment (see Johnson and Mislin

[6]The relevance of social ties is by no means limited to the traditional banking sector. Duchin and Sosyura (2013) for example show that relationships are also highly relevant in the internal capital markets of companies. (Kuhnen, 2009) finds evidence of favoritism in manager choice in the mutual fund industry.

[7]On the borrower side we see that even simple text message reminders can improve repayment if they include the name of the responsible loan officer, further showing that personal relationships can influence behavior on that end of the interaction (Karlan et al., 2015).

(2011) for an overview). This similarity is mostly superficial: the typical trust game largely focuses on the question of how the behavior of the borrower can be controlled or predicted by the lender. Investment games therefore have a second stage, in which the borrower can decide to keep or return the proceeds of the investment. Our focus in this project is on the affective reactions that drive an investor's decision making. In a repeated trust game it is difficult to disentangle the investor's desire to benefit a borrower with whom they have made positive experiences from their largely self-interested desire to invest in a manager who is more likely to repay the returns of a project. Our design in this experiment therefore shares much of the framing with trust games, but not the actual game design.

An example of an experiment that provides some separation between informative and emotional aspects of the investor/borrower relationship is presented by Brown and Zehnder (2007). The authors design a credit rating mechanism that provides information about a borrower's repayment history that is equivalent to information acquired in a treatment in which investors have previously been in a business relationship with the borrower. The main focus of their experiment is, however, on how that affects the borrowers' repayment discipline, rather than on potential effects on lenders, with the exception of hold-up strategies[8]. Their market mechanism is too complex to compare the investors' reaction to the relationship between treatments with and without the credit reporting facility. Similarly, Cornée et al. (2012) have treatments without repeated interaction, with repeated interaction, and with repeated interaction and additional information about the precise behavior of the borrower. However, also their design focuses on honest and dishonest behavior of the borrower, rather than the disentanglement of different motives of lenders. It can therefore not identify what role the relationship plays for them.

Clearly, the isolation of the emotional influence of bonding over shared success or failure is difficult if the behavior of the borrower has predictive power for the future income of the

[8] A lender can extract rents from a borrower by asking relatively high prices for the renewal of short term debt after a relationship has been established and provided the lender with exclusive positive information, because other lenders are not privy to the same information.

lender. For this reason we simplify the situation by creating a design that still has identifiable features of a repeated investment setting, but which eliminates this confounding factor. As we will see in section 3, our borrowers still make meaningful choices, but they are not predictive of future returns. Project managers select a project, which can either be more or less valuable, but they do not actually know which project it is. The project allocation is therefore essentially random, while still being a direct result of their decision.

Our experiment is framed as a investment game, but since it completely lacks an element of trust, it's design differs notably from most investment games. A comparison with a blame game (Gurdal et al., 2013) is more appropriate. In this game somebody is blamed for the outcome of a decision, even though there is no meaningful way in which it could have been known what the result of the decision was going to be. The results show that such behavior is a robust phenomenon not only in psychology, but also in a situation more in line with the methodology of experimental economics. Gurdal et al. (2013) mention two different potential drivers of such an effect: outcome bias (Baron and Hershey, 1988) and salient perturbations (Myerson, 1997). The former describes the effect that people rate the quality of someone else's decision making differently based on an uncertain outcome, even if the decision maker took all relevant information into account. Despite the fact that the result of a uncertain draw says nothing about the competency of the decision maker, it is rated higher after a positive outcome. In our so called History treatment project managers decide which project to implement without being able to distinguish more or less valuable projects, but the investors' evaluation of their decision quality might be affected by the outcome nonetheless. Salient perturbations, on the other hand, should not be of importance in this context. The concept describes the idea that agents interpret an unfamiliar situation in a manner that is more familiar to them. In our context this could imply that they assume the possibility of additional insight into the project quality as a function of some type of effort to be exuded by the manager. We think that our way of presentation prevents such mis-attributions, although it should be noted that this effect does not necessarily rely on a conscious misunderstanding of the situation, but merely on the situation being difficult to

analyze and similar to a more familiar situation.

Yet another comparison can be drawn to experiments where different groups are constructed in the lab in order to analyze how the subjects' decisions in social games react to that. Cason et al. (2015), for example, find noticeably higher rates of cooperation in an inter-group prisoner's dilemma game if the two groups played a successful minimum effort game together before engaging in the prisoners' dilemma game, as compared to a treatment where the minimum effort game was not present. This effect is reinforced by inter-group communication possibilities during the minimum effort game. Morita and Servátka (2013) find higher investment rates and lower rejection rates in a holdup problem if the first and second movers are from the same group than if they are from different groups. In their experiment subjects are assigned to groups using different shirts and group members perform a trivia task together prior to the holdup (trust) game.

We are further guided by a a number of experiments on "affective ties", which are based on a model that treats agents' social preferences vis-à-vis other agents, with whom they interact repeatedly, as endogenous (van Dijk and van Winden, 1997). The utility specification in our predictions allows for this feature, albeit in a simpler way, since we merely have to track a investor's reaction to a binary state rather than a more complex space that is repeated over multiple periods. Nonetheless, results such as van Dijk et al. (2002) provide a foundation for our predictions, which are further supported by more recent experimental studies (Hoyer et al., 2014).

One distinction that should further be made is that our concept of a relationship based on a shared history clearly differs from the concept of intention based social preferences as it in Rabin (1993). Our project managers do not have any insight into the effect that their choice is going to have. Therefore, kind or unkind intentions cannot play any role in the investors' perception of the situation. This changes in the treatment in which project managers make an active choice to send a transfer or not. In this case the investor's response becomes much

more similar to a response in a reciprocity setting (Fehr and Gächter, 2000). This treatment shares some features with an experiment by Malmendier and Schmidt (2012), who in their "gift" treatment give a manager ("producer" in their terminology) the option to send a gift prior to the investor ("decision maker") having to choose between investing with that manager or another manager. In this decision the projects associated with both managers are lotteries with different returns in case of a success or failure[9]. One way of looking at our experiment is to see it as a combination of ideas that can be found in Malmendier and Schmidt (2012) and in Gurdal et al. (2013). In both papers the authors use a mechanism in which the choice that is disadvantageous to one partner benefits a previously unknown third party. In the first experiment the authors focus on favoritism and reciprocity as drivers for such behavior, while in the second the attribution of blame is investigated. Our design attempts to compare these mechanism[10].

[9]Major differences between our experiment and theirs are as follows. In their experiment investors ("decision makers") are assigned two potential managers ("producers"), whereas in our design managers are chosen endogenously from a pool. Furthermore, managers in our experiment are involved in two projects that affect the investor, and the projects are more complex to analyze due to the need to apply Bayesian updating to precisely calculate a best response. This difference in design also leads to a noticeably longer amount of time to pass between a transfer and the decision being made in our experiment. Finally, our investors decide between staying with a manager they interacted with before and switching to a new one, as opposed to deciding between two equally unknown managers. To explain their result, Malmendier and Schmidt model their decision makers behavior using a dynamic social preferences approach similar to what we do in this paper.

[10]Notable differences between our experiment and Gurdal et al. (2013) are the fact that their experiment clearly juxtapositions the result of an agent's choice with the hypothetical result had she chosen otherwise and again the more complex risk structure in our experiment.

3 Design and Hypotheses

The experiment consisted of three different treatments. Our main motivation was to isolate the role of different social experiences on the investors' project and manager choice in a stochastic environment. The three treatments are: a **History** treatment, in which the investor and the manager have experienced a success or failure together in a previous project; a **Transfer** treatment, in which a project's manager either sends a monetary transfer to the investor or not; and a **Control** treatment, which is similar to the History treatment, but does not include a manager, eliminating the social aspect completely.

3.1 Treatments

History

The History treatment has twice as many managers as investors. At the beginning of each round an investor chooses a manager from a pool of managers, who are presented in the form of identical icons on a screen. The position of the icons is randomized in each round, so that the identities of the managers can not be tracked across rounds. The order in which investors make this choice is randomized anew for each round. Investors who have not yet made a choice and managers who have not yet been chosen see the screen with all icons until they have made a choice or have been chosen, respectively. The icons that represent managers who have already been chosen by an investor disappear from the screen one after another. Managers are also informed which icon they are represented by. Managers who are not chosen by any investor are redirected to a waiting screen[11].

Managers who have been chosen by an investor choose one out of eight potential projects. Each project either has a success probability of $\frac{1}{4}$ or $\frac{3}{4}$. Both types of projects are equally likely and neither investors nor managers can identify the projects at the time of choosing

[11]To ensure attention inactive managers were given the possibility to watch a neutral video while they were inactive. We did not test possible behavioral effects of the video, but they would be irrelevant, since we do not analyze the managers' behavior.

(i.e. their positions on the screen are randomized anew for every decision). The decisions are made in the same order as the choices of the investors, that is a manager who was chosen third is also the third to choose a project. Since all managers chose from the same set of projects, a manager who has been chosen by the final of 8 investors has only one project to choose from. The project choice screen works in the same way as the investor screen: randomly positioned projects disappear one after another once they are chosen and are no longer available to other managers. After a project has been chosen a manager is asked to "implement" it by clicking on a box that symbolizes the project. Both investor and manager see a 5 second long animation similar to the "processing" animation typically found on computers[12], after which the success or failure of the project is announced.

After investor and manager have observed the result, the manager chooses a *second* project, which has no relation to the first project in any way. This implies that the success or failure of the first project provides no information at all about the success probability of any later project. The understanding of the last point was tested before the beginning of the experiment.

After observing the outcome of this second project, investors are now given the choice to either stay with this project and project manager or to choose an alternative project manager and project. If the investor chooses the first option the manager is redirected to the implementation screen once more. After the implementation of the second project both parties are informed about the success or failure of the second implementation of the project. If s/he chooses to change managers the investor first has to wait until all investors have made their decision. Once that is the case all investors who opted to replace their managers are assigned a new random order and choose a new manager from the pool of managers who were not chosen to be managers at the beginning of the round. Newly chosen managers then choose a new project with the same blind procedure as before, which they subsequently implement. After all results have been observed the round ends. There is a

[12] see the online demo for an example.

total of eight rounds, which only differ in the payoffs of the alternative projects, as explained later in this section.

Transfer

The Transfer treatment follows the same general structure as the History treatment, with one difference. Whereas every round of the History treatment starts with a project that is completely unrelated to future projects, this part is now replaced. Instead managers who have been chosen by an investor are now given the option to transfer money to the investor or not. They are endowed with an extra 10 experimental currency (ECU) for this transfer. If a manager decides to make that transfer these 10 units are doubled and investor's earnings grow by 20 units[13].

After deciding whether to transfer money or not, the manager chooses a project from a pool of 8 different projects using the same procedure as in the History treatment. It is then implemented in exactly the same way. After this project has been chosen and implemented, investors face the same decision as in the History treatment: to stay with the same project manager and project as before or to choose a new manager, who then chooses a new project.

Control

The Control treatment eliminates the social element that is present in the two other treatments. Investors now choose and implement their own projects instead of choosing a manager who then chooses and implements a project. Managers are not part of this treatment. Apart from that difference this treatment is exactly identical to the History treatment. Projects are chosen by the investors from a pool of eight projects in the same manner as in the other treatments.

Figure 1 illustrates the design of a single round in all three treatments.

[13]The size of the transfer was chosen based on the observation that a transfer that was similarly sized relative to a project's expected earnings lead to reasonably evenly distributed decisions to transfer and not to transfer in Malmendier and Schmidt (2012).

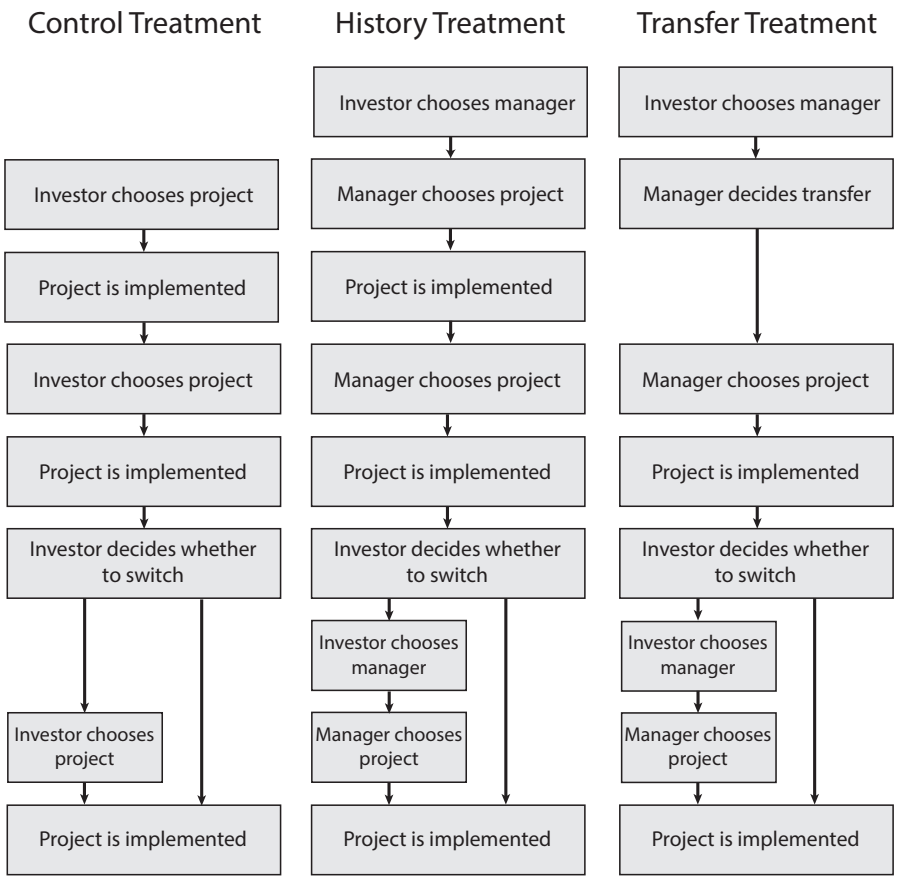


Figure 1: Design of Treatments

3.2 Projects

The following explains the earnings of investors and managers and the investor's best response.

A manager who is actively managing a project at a given time receives 200 experimental currency units irrespective of the project's success or failure. Managers who are inactive during the first project (Transfer treatment) or the first and second project (History treatment) also receive the same 200 units[14]. During the final project inactive managers receive nothing.

[14]We made this choice to eliminate inequity aversion as much as possible from the experiment.

Ignoring all social aspects of this experiment for the moment, a profit maximizing investor must use past observations as a signal for the underlying success probability of the project in order to determine the best response.

In every round an investor can only choose one project. All projects either have a high (p = $\frac{3}{4}$) or a low (p = $\frac{1}{4}$) success probability. The ex-ante probability of both types of projects is 50%. With the exception of the alternative project that an investor can switch to at the end of a round, all projects generate earnings of 300 in case of a success and 100 in case of a failure. In order to precisely calculate the expected value of a project with unknown success probability and the investor's best response we therefore have to calculate the expected value of both types of projects and then combine them to get to the overall expected value:

$$E(\pi_H) = \frac{3}{4}300 + \frac{1}{4}100 = 250 \tag{1a}$$

$$E(\pi_L) = \frac{1}{4}300 + \frac{3}{4}100 = 150 \tag{1b}$$

where we use π_H and π_L for projects with known high or low success probabilities, respectively.

If the project in question is a completely new project (π) this implies an expected value of

$$E(\pi) = \frac{1}{2}E(\pi_H) + \frac{1}{2}E(\pi_L) = 200 \tag{2}$$

The probability of observing the good outcome with payoff 300 is therefore $\frac{1}{2}$.

If however a project has been implemented in the previous period its success or failure provides information about its underlying success probability. Using Bayesian updating we can calculate the probability of the project being of the good type after having observed a successful draw:

$$P(\pi = \pi_H | success) = \frac{P(success|\pi_H)P(\pi_H)}{P(success)} = \frac{\frac{3}{4}\frac{1}{2}}{\frac{1}{2}} = \frac{3}{4} \tag{3}$$

Using the same procedure we get $P(\pi = \pi_L|success) = \frac{1}{4}$, $P(\pi = \pi_H|failure) = \frac{1}{4}$, and $P(\pi = \pi_L|success) = \frac{3}{4}$. Combining equations (3) and (1) we can calculate the expected value of a project that was observed to succeed:

$$\begin{aligned} E(\pi|success) &= P(\pi = \pi_H|success)E(\pi_H) + P(\pi = \pi_L|success)E(\pi_L) \\ &= \frac{3}{4}\left(\frac{3}{4}300 + \frac{1}{4}100\right) + \frac{1}{4}\left(\frac{1}{4}300 + \frac{3}{4}100\right) \\ &= \frac{5}{8}300 + \frac{3}{8}100 = 225 \end{aligned} \tag{4}$$

Similarly we can calculate the expected value after observing a project to fail to be

$$E(\pi|failure) = \frac{3}{8}300 + \frac{5}{8}100 = 175 \tag{5}$$

Facing the decision whether to implement an old project again or choose a new one, a risk neutral selfish investor would therefore stay with a project that has been successful before (to earn $E(\pi|success) = 225$ in expectation) and choose a new manager with an unknown project if the first project implementation was a failure (to earn $E(\pi) = 200$ in expectation).

However, investors face a more complex situation. During the first (Transfer treatment) or first and second project (History and Control treatments) they earn 300 units in case of a success and 100 units in case of a failure. The alternative project has different returns, of which they are informed when they have to decide whether to stay with the original manager and project or have a new manager choose a new project. For this reason the most convenient way of expressing the expected value of a original project is the more general

$$E(\pi^O|h) = P(\pi = \pi_H|h)E(\pi_H^O) + P(\pi = \pi_L|h)E(\pi_L^O) \tag{6a}$$

$$E(\pi^A) = \frac{1}{2}E(\pi_H^A) + \frac{1}{2}E(\pi_L^A) \tag{6b}$$

where π_H^O and π_L^O stand for the high and low success probability type of the original project

and π_H^A and π_L^A for the high and low success probability type of the alternative project. h is a particular history of experiences.

The original and alternative project's returns are chosen such that they are either equal in their variance[15] or their expected earnings or both. As illustrated in table A.1 in the appendix, we offer three combinations of returns in which the alternative project has higher expected earnings, one with lower expected earnings, two with a lower variance and one with a higher variance. in five of the cases the alternative project either has a higher expected value or a lower standard deviation, therefore we take the alternative projects as the benchmark both in the appendix and the results section when describing differences in expected value and standard deviation. In order to get the most efficient experimental design possible we condition the alternative project returns that investors are offered on the success or failure of the previous project.

Calculating the optimal decision in the way outlined above is a task that is challenging and we do not expect participants to be very good at this part of the task[16]. In fact, there are reasons to think of it as even beneficial. One is the greater degree of realism that subject face if they are not able to perfectly determine the value of the different options they are facing. Another reason is that situations which present a subject with a high cognitive load are understood to be more likely to trigger impulsive behavior from subjects (Duffy and Smith, 2014), in particular in situations that call for other-regarding behavior (Cornelissen et al., 2011; Schulz et al., 2014).

Every investor faced each combination of returns exactly once and the order of the different combinations was randomized so as to ensure that the distribution of experienced orders was as flat as possible.

[15] That is, up to a negligible difference.

[16] In the instructions to the Control and History treatments subjects were told a second time that information from earlier draws could be used to estimate the success probability of a project, on top of merely outlining the design of the experiment. This was not the case in the Transfer treatment.

3.3 Presentation and Organization

Much of the experimental design was driven by the aim to provide an engaging experience for subjects, as the blind matching and project choice procedures are fairly impersonal. This was the main reason to implement the experiment with the computerized equivalent of a choice method in which subjects blindly choose cards that indicate their assigned managers and projects in turn. The act of choosing a partner should trigger a stronger engagement than if a partner had been assigned in purely random manner. A similar logic applies to the active project choice by the manager. We reinforced these effects by showing subjects that the pools of available managers and projects were constantly depleting and by using a design language that promotes the notion that projects are actually implemented, similar to the animations used in computer games to illustrate the execution of projects or tasks. The mechanic of choosing whether to stay with the project (and manager) or to choose anew was designed using a deliberately slow animation to reinforce the notion that this decision, which is our main outcome variable, is of relevance[17].

The original instructions as they were presented to participants and an interactive example round of each treatment of the experiment can be accessed on `http://www.mhoyer.com/dev/inv_feelings`.

The participants' understanding was checked using a quiz that covered the most important features of the experiment, including the concept that Bayesian updating can be performed in this setting. After the experiment subjects answered a short questionnaire covering demographic variables and some short questions about their emotional state during different situations in the experiment (see appendix B.2).

The experiment was run in 12 sessions at the CREED laboratory of the University of

[17]The animation in question took 3 seconds.

Amsterdam in March and April of 2015. A total of 222 participants participated. Both the Transfer and the History treatment had 87 participants, of which a third (29) were investors. 48 participants were in the Control treatment, all of which were investors. At the end of each session a random round was chosen for payout. In the History and Control treatments no show-up fee was paid. The replacement of the first project with a relatively low-value transfer in the Transfer treatment required us to pay a show-up fee of 7 euros in the Transfer treatment to ensure satisfactory minimum earnings for participants. Sessions took approximately 70 minutes on average including instructions and payout and participants earned an average of 16.55 euros.

3.4 Hypotheses

For the reasons outlined in sections 1 and 2 we hypothesize that an investor's preference for the earnings of a project manager is stronger a) if the first project was a success relative to the situation in which it was a failure (History treatment), and b) if the manager sent a transfer relative to the situation in which it was withheld (Transfer treatment). From now on we will speak of a positive experience or a negative experience whenever we summarize the two different cases across treatments.

A simple formalization of this idea is to incorporate the manager's earnings into the investor's utility function and multiply it with a weight α which adjusts based on experience. We can then compare the expected utility of switching to the alternative project project (π^A, see (6a)) with the expected utility of staying with the original (π^O, see (6b)). Using a simple linear function and reformulating (6a) to the situation in the experiment, we get the following expected utility from choosing either of the two possible options:

$$E\big(U(\pi^O|h)\big) = E(\pi^O|h) + \alpha 200 \tag{7a}$$

$$E\big(U(\pi^A)\big) = E(\pi^A) \tag{7b}$$

Under the assumption that α is larger in case of a positive history compared to the opposite

situation there are therefore more combinations of project payoffs for which $E\big(U(\pi^O|h)\big)$ is bigger than $E\big(U(\pi^A)\big)$ after a positive than after a negative experience. Therefore we expect a higher proportion of investors to stay with their original project and manager after positive than after negative experiences.

It should be noted that the second project in the History treatment - which finds it's equivalent in the first project of the Transfer treatment - can be expected to have similar effects as those we ascribe to the first project of the History treatment. They are however much more difficult to analyze due to being confounded with the calculation of the expected value of proceeding with the original project. Moreover it does not lend itself well to inter-treatment comparison in our design since we do not know how precisely the effect from the potential transfer interacts with an additional experience effect of a different type.

In this simple analysis we have ignored both models of inequality aversion and considerations concerning the income earned in the stages of the the experiment that precede the investor's decision. Canonical models of inequity aversion do not play a role in the investor's behavior, since at the time the decision is made all project managers have earned exactly the same amount. Whatever decision the investor makes, the number of managers who earned more or less than she and the size of the difference in earnings will not be affected[18]. Similarly, considerations such of efficiency do not affect our argument, since they coincide with the self-interested best response. Note that we do not model general social preferences regarding anybody but the first project manager (i.e. they have an α of zero). We think that this is natural due to the relatively big group of alternative managers. It would however not make a meaningful difference if we would take their income into account, because prior to the investor's decision all managers have had exactly identical earnings.

The direction of the assumed mechanism is the same in the History and in the Transfer

[18]More details and an extension covering the role of inequity aversion in a design without fixed manager earnings can be found in appendix C.

treatment, the only difference is in the motivation. Whereas in History we assume that the investor is more concerned about the earnings of the original manager if they experienced success in the very first project, in Transfer the trigger is whether the manager chose to send the transfer or not, analogous Malmendier and Schmidt (2012)[19].

Hypothesis 1. *The probability of switching to the alternative project is lower in case of a positive experience than after a negative experience.*

As outlined in section 3, there are two potential problems in our design because subjects could be confused by the fact that one project - the first project in the control and history treatments - is not predictive of the success probability of future projects, whereas another project in fact is predictive. In addition, a positive experience could generally affect the subjects' emotional state regarding any familiar project, making them feel more positive about the original project, as opposed to the person who chose it. Behavior born from more general types of misunderstanding probabilities, such as the gambler's fallacy, add further potential problems. Without a method to control for these effects we would not be able to attribute the supposed result in hypothesis 1 to the assumed social effect of sharing a positive or negative history (or receiving a transfer or not). Therefore, in addition to the first hypothesis, we also require that the effect size of the different experiences is larger in History and Transfer than in Control.

Additionally, due to existing literature (such as Malmendier and Schmidt (2012)) we have a stronger prior for the existence of such an effect after a transfer than after a positive history. Based on the idea that a direct transfer, which may trigger reciprocity on the side of the investor, is a stronger intervention than merely sharing a common history we expect the overall effect to be strongest in the Transfer treatment.

Hypothesis 2. *The switching probability effect of different experiences follows the order Control < History < Transfer.*

[19]It should be noted that inactive managers were not compensated for the transfer stage. Therefore active managers that had not sent the transfer had a slightly higher income level than inactive managers.

4 Results

Table 1 presents demographic data about the participants in the experiment and specifies the histories that the investors in the different treatments experienced prior to making their decision about staying with the same project (and manager) or not. We define a "positive" experience to describe either the experience of a successful project or a transfer, whereas a "negative" experience describes a failed project or not receiving a transfer. The distribution of positive and negative experiences in the Control treatment is perfectly balanced at 192 each by design, while in the History treatment the balance is not perfect because some sessions were run with only 18 or 21 instead of 24 participants due to low show-up, leading to a success rate of 48.7%. Experienced histories in the Transfer treatment are a function of the participants' decision making: Managers sent the voluntary transfer in 166 out 232 possible cases, a grand total of 71.6%. This is close enough to our optimal distribution of 50% to allow us to make statements about the reaction of investors to either receiving the transfer or not[20].

					Experienced Histories			
	N	Age	Female	Economics Students	−/−	−/+	+/−	+/+
Control Treatment	48	22.65	24 (50%)	31 (64.6%)	83 (21.6%)	109 (28.4%)	109 (28.4%)	83 (21.6%)
History Treatment	87	22.07	60 (69%)	57 (65.5%)				
History, Investors only	29	22.1	21 (72.4%)	14 (48.3%)	61 (26.3%)	58 (25%)	56 (24.1%)	57 (24.6%)
Transfer Treatment	87	22.26	48 (55.2%)	70 (80.5%)				
Transfer, Investors only	29	22.76	16 (55.2%)	24 (82.8%)	34 (14.7%)	32 (13.8%)	80 (34.5%)	86 (37.1%)
Total	222	22.27	132 (59.46%)	158 (71.17%)	178 (20.99%)	199 (23.47%)	245 (26.65%)	226 (26.65%)

A "+" indicates either a successful project or a transfer, a "−" indicates a failed project or the absence of a transfer. One manager's age was ignored due to obvious misreporting.

Table 1: Demographic Data and Experienced Histories

There is no notable change in the investors' decision across the 8 rounds of the experiment(figure 2)[21]. In the Transfer treatment there appears to be a slight increase in the

[20]The hypothesis of equal transfer ratios in all rounds is rejected with 0.04% significance due to one outlier in round 3, where 90% of all transfer where sent. Excluding that round the hypothesis cannot be rejected (Chi-square). Regressing the transfer decision on a trend in a random effects model produces a significantly negative coefficient at the 5%-level (see figure A.1 in the appendix).

[21]The null hypothesis of equal project switching rates in different rounds cannot be rejected (p=0.66) and there is no discernible trend.

second half of the experiment[22].

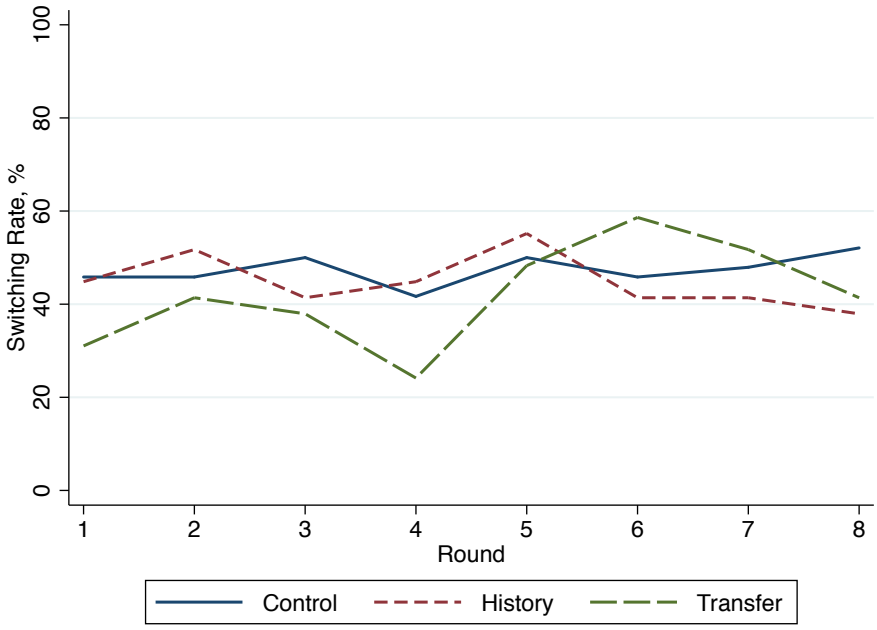


Figure 2: Switching Rate across Rounds

We begin our investigation into the investor behavior with a simple question: Does the experience made at the very beginning of a round matter? Figure 3a shows the proportions of investors that decided to choose a new project (and manager) after a positive experience in the different treatments. Recall that a perfectly selfish investor with a perfect ability to perform Bayesian updating would switch in 62.5% to 75% of all cases, irrespective of the experience or treatment. We observe a switching rate of 53.6% in case of a negative experience and 38.4% in case of a positive experience and the difference is highly significant with a chi-square test statistic of 16.1[23]. This first result confirms hypothesis 1:

Result 1. *A positive experience leads to a significant drop in switching rates relative to a negative experience.*

[22] p= 0.069 in a regression of only the trend and a constant in a random effects model.

[23] We use a clustered chi-square procedure (Stata package clchi2). Here and later we cluster at the subject level. While the subjects interact indirectly, we argue that there is no possible channel for behavioral spillover within a group of investors, allowing us to treat different investors as independent. We also ran a test on only the first round as a robustness check, but results are only reported if they differ qualitatively using common significance criteria.

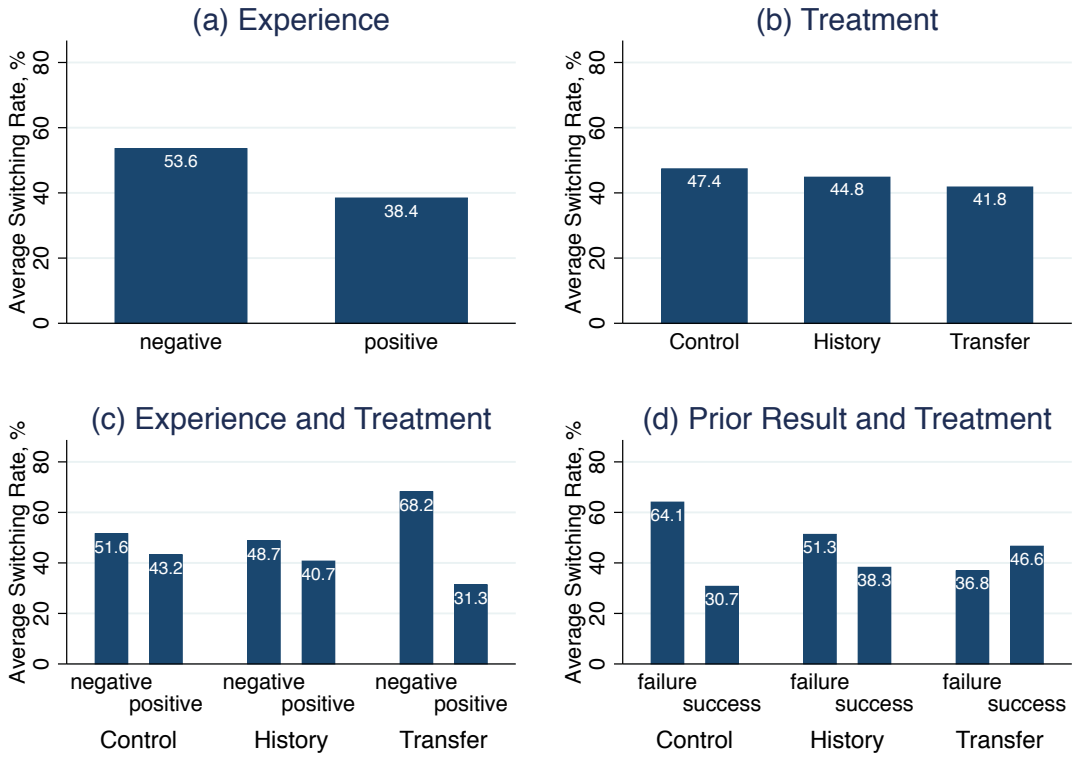


Figure 3: Switching Rates

Next we look at differences in the switching rate between the different treatments. Figure 3b shows overall switching rates in the three treatments. We observe a constant decrease going from the Control to the History and the Transfer treatment. The differences are not significant, however[24].

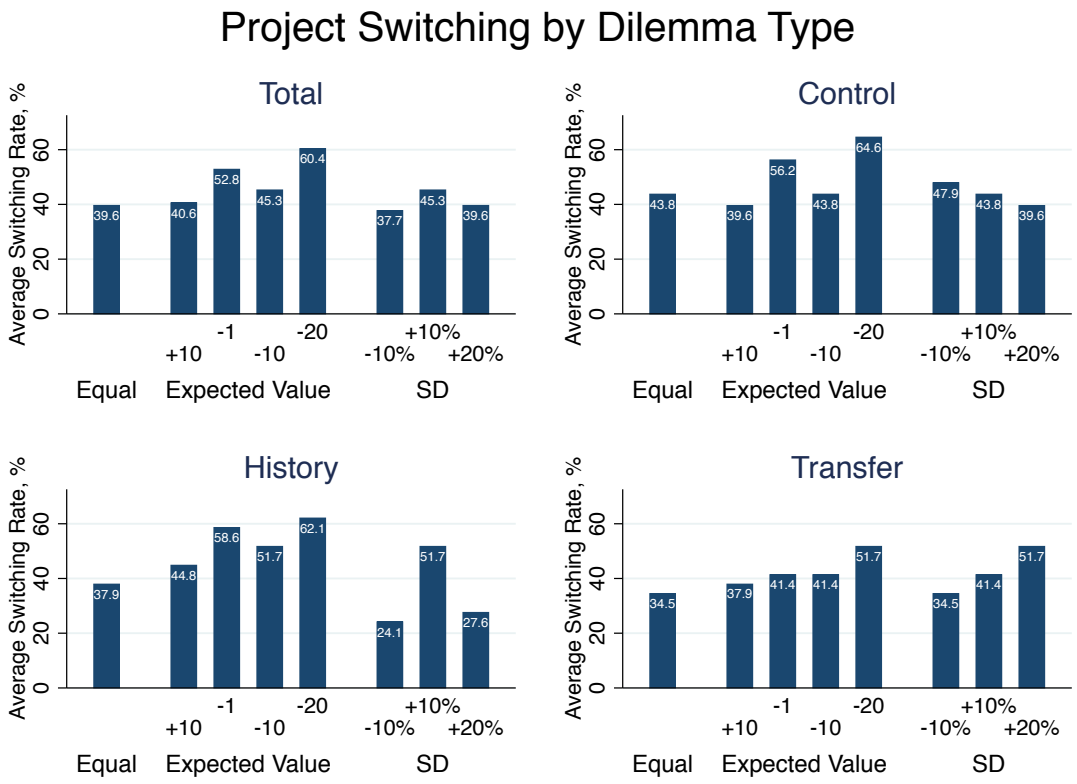
The natural next step is to focus on the difference in switching ratios relative to the different types of experience in the separate treatments, see figure 3c. While the difference in switching rates is substantial in the Transfer treatment (36.9%), the difference in the History treatment (8%) is not only nigh-identical to the Control treatment (8.3%), but even slightly smaller. The only treatment in which the investors' behavior differs significantly between experiences is found in the Transfer treatment.

[24]The lowest p-value is found comparing the Control and Transfer treatments at $p = 0.207$.

Another dimension along which we can separate the investors' decisions is the result of the project that was implemented just prior to the decision, which we refer to as the "prior project" from now on. While the value of the alternative project was adjusted to the expected value of the original project as it could be calculated using Bayesian updating, we might still expect a positive experience effect relative to the result of this project. This is however not what we find, as the difference decreases between Control and History and even reverses in Transfer (Fig 3d).

The ability of the subjects to correctly perform Bayesian updating is not at the core of this analysis and not necessary for the interpretation of the other results. However, investors have a monetary incentive to switch projects more often if it is relatively beneficial to do so. Figure 4 distinguishes the different alternatives that investors faced in the experiment. Generally speaking, there seems to be a discernible effect when comparing the most extreme cases of positive or negative differences in expected value (19.8%, $p < 0.01$)[25]. However, we do not see the monotonic increase in switching rates with increasing differences in expected value that one would expect. The same is true for the projects with different variances, where we would expect increasing switching rates the lower the variance of the alternative project becomes.

[25]In this case the data were insufficient to run a meaningful test using only the first round.



Original and alternative projects either have the same expected value and variance or differ in one of the two dimensions. The labels used here refer to the situation in which the respective values for the original project differ from the benchmark alternative project as indicated in the label, the other dimension is always identical between projects. For example, in the case of expected value -20 the original project has an expected value that is 20 units lower than the alternative project, implying that switching is the best response for a purely self-interested investor. Expected value differences are in absolute values and differences in standard deviation are in relative values, rounded to full percentage points.

Figure 4: Project Switching by Dilemma Type

So far we have only compared the investors' behavior relative to their different experiences within the three treatments. In order to answer hypothesis 2 we need to go one step further. We hypothesized that the *difference* between the investors' switching rates after a positive experience and after a negative experience should be smallest in the Control treatment, and largest in the Transfer treatment. A visual inspection of figure 3c suggests that the Transfer treatment indeed has the greatest difference, but it seems unlikely that the first part of the hypothesis, which concerns the difference between Control and History, holds. To come to a more conclusive statement we construct a number of different panel regressions, in which we interact the treatments with a dummy variable for the experience

(Table 2). Irrespective of the specification, the results fall in line with the first impression from figure 3c. The coefficient of the interaction term between the Transfer treatment and the experience dummy is always significant at the 1%-level, while the coefficient of the interaction term between the History treatment and the experience dummy is positive but not significant. This impression is confirmed by running chi-square tests over the differences in ratios predicted by the logit coefficients in the different treatments[26]. We also predicted the switching probability using only positive experiences, to prevent potential overlap with inequality aversion motives in the Transfer treatment. The difference in predicted switching rates between Control and Transfer was negative and marginally significant ($-9.4\%, p = 0.08$, specification (4)). Running a regression similar to specification (4), but using History as the baseline confirms that there is no difference in the differences in Control and History, but the Transfer dummy shows a significant interaction with the experience dummy. In conclusion, we can only partly confirm hypothesis 2:

Result 2. *Switching rates after different experiences are not significantly different between the Control and History treatments. In the Transfer treatment the difference is significantly larger than in the Control treatment and the History treatment.*

Using the whole sample we also see that the result of the prior project affects switching rates negatively (a positive outcome leads to a lower switching rate). The difference between the expected values of the original and the alternative projects affects switching in the expected direction (if the original project has a relatively high expected value we predict less switching), whereas standard deviation differences do not show a significant effect, although also pointing in the expected direction (original projects with a relatively high future variance lead to more switching)[27].

[26] Predicted between treatment change in the difference of switching ratios relative to experience, keeping all other variables at their mean and using specification (4) from table 2: Control vs History $3.9\%, p = 0.64$; Control vs Transfer: $23.5\%, p < 0.01$.

[27] See table A.2 in the appendix for the same regression using a probit model. Results are qualitatively comparable.

	Investor switches project			
	(1)	(2)	(3)	(4)
History	-0.119		-0.190	-0.263
	(-0.48)		(-0.74)	(-1.00)
Transfer	0.699*		0.630	0.623
	(2.22)		(1.95)	(1.91)
Positive Experience	-0.348	-0.707***	-0.497*	-0.499*
	(-1.66)	(-4.73)	(-4.62)	(-2.30)
History × Positive Experience	0.0160		0.139	0.163
	(0.05)		(0.40)	(0.46)
Transfer × Positive Experience	-1.214**		-1.075**	-1.047**
	(-3.18)		(-2.75)	(-2.67)
Prior Result Positive		-0.708***	-0.699***	-0.723***
		(-4.81)	(-4.71)	(-4.83)
Expected Value Difference		-0.0282**	-0.0269**	-0.0258**
		(-3.18)	(-3.01)	(-2.87)
SD Difference		0.00437	0.00329	0.00294
		(0.30)	(0.37)	(0.33)
Round				0.0196
				(0.61)
Female				0.0662
				(0.40)
Age				0.00825
				(0.29)
Economics Student				-0.206
				(-1.16)
Choice number				-0.0526
				(-1.56)
Constant	0.0664	0.452**	0.408*	0.486
	(0.43)	(3.21)	(2.26)	(0.65)
Individuals	106	106	106	106
N	848	848	848	848

Random effects model with z-statistics in parentheses, using robust standard errors
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2: Investor decision regressions, logit

Next we investigate the behavior of the investors as expressed in their decision times. We observe that decision times in the two social treatments are significantly longer than in the

Control treatment (Figure 5)[28]. At the same time the difference in decision times between History and Transfer is negligible at 0.5 seconds. This result seem to be driven by a smaller proportion of investors who made their decision very quickly (see density estimate in figure A.2 in the appendix) and is stable throughout all rounds (see figure A.3 in the appendix). Again we repeat this analysis by using a panel regression, which confirms our findings (Table A.3 in the appendix). Interestingly, we find no indication that decision speeds are significantly correlated with either the decision made by the investor or the absolute difference in expected value or variance between the two projects[29].

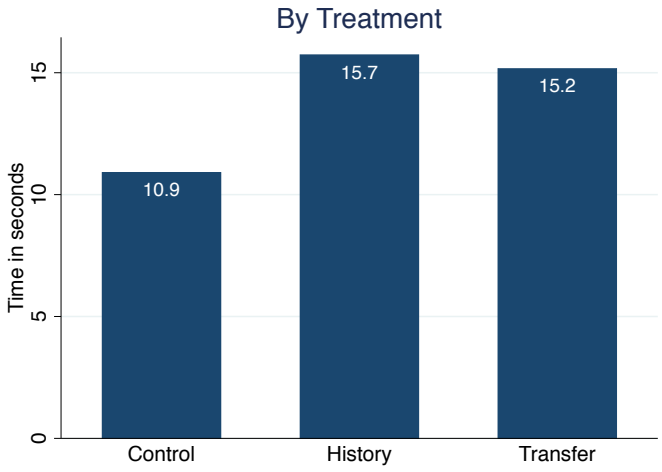


Figure 5: Investor decision times, in seconds

[28]Using clustered t-test, both Control vs History and Control vs Transfer have p-values below 0.001. As was done with investor decisions, decision time comparisons were also ran on only the first round as a robustness check (here using a simple logit instead of a random effects panel model), but results are only reported if they differ qualitatively using common significance criteria. Note that all decision times include the 3 seconds that a investor has to wait as part of the confirmation screen, plus additional waiting time if they decided to change their decision before confirming. In History and Transfer subjects saw an additional reminder of the effect a decision has on the managers, but that was identical in all 8 rounds and hence unlikely to be relevant for this comparison.

[29]In the most complete specification (4) the coefficients for the absolute difference in expected value and variance both point in the direction that implies that decisions in situations in which the difference is larger are made quicker, but fail to achieve significance. Note, though, that in both social treatments with a positive experience decisions to stay with the current manager are made slower than decisions to switch, a relationship that completely reverses with a negative experience, but only in the social treatments (Control treatment: -0.2 to -0.2 seconds; History treatment: from -2.6 to +2.1 seconds; Transfer treatment: from -2 to +4.8 seconds, see figure A.4. The differences are weakly significant in a regression using an experience/investor decision dummy when we pool the social treatments, but not when analyzed in any treatment in isolation.

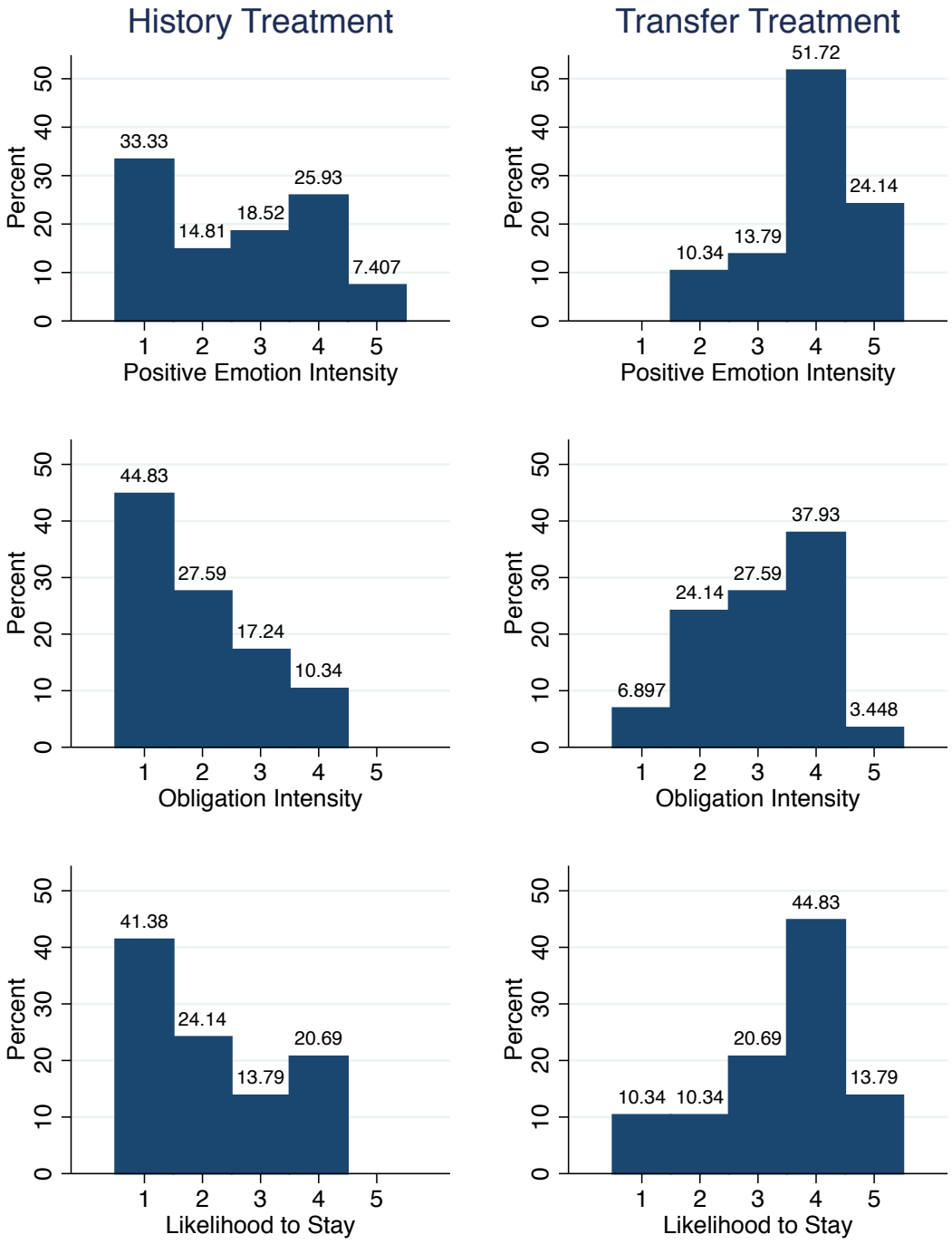
As part of our post-experiment questionnaire we asked investors a set of questions regarding their reaction to either having received a transfer or the first project having been a success, dependent on the treatment[30]. Figure 6 shows the distribution of their answers in the two different treatments. Asked if they felt a positive emotion towards a manager with whom they had shared a positive experience, the distribution of answers in the History treatment is bimodal with 33% choosing 1, the lowest possible choice on our 5-point scale. In the Transfer treatment 0% chose 1 and 75% chose a value of 4 or higher. This picture is confirmed by the questions asking for a sense of obligation towards that manager and a direct question concerning their likelihood to stay with such a manager. There is always a lot more mass on the upper part of distribution in the Transfer treatment compared to the History treatment[31]. The investors' answers to the emotion and "likely to stay" questions are predictive of their reaction to their experiences, with the obligation question also being marginally significant (see table A.5 in the appendix[32]). Correlations between the questions regarding the feeling of positive emotion and feeling of obligation are between 0.35 and 0.58 and always at least weakly significant, but among the other possible combinations only the Transfer treatment emotion and "likelihood to stay" correlation is significant without pooling the treatments[33].

[30] See section B.2 in the appendix for the exact questions.

[31] In all questions average answers are significantly higher in the Transfer treatment with p-values below 0.001.

[32] The interaction for the emotion and the likelihood to stay variables with experience are significant at the 5%-level, while the obligation variable just misses that mark. Running the same regressions separately in the History and Transfer treatment mostly results in results to weak to make any conclusive statements about the directionality of the effect, with the exception of the "likelihood to stay" variable, the coefficient of which is negative and significant with a p-value of 2.9% when interacted with experience in the Transfer treatment.

[33] see table A.4 in the appendix.



Two subjects answered they had not experienced a success in the first project when asked for their emotion rating, leaving 27 observations. In all other cases we have answers from all 29 investors in both treatments.

Figure 6: Questionnaire: investor scores on emotion, obligation, and likelihood to stay with a project manager after a positive experience

5 Discussion

Based on the results presented in the previous section, one result is clearly established: We observe a much lower rate of switching to alternative projects and managers after a transfer has been sent than after the absence of a transfer. The predicted size of the effect reaches almost 37%, despite the fact that investors only face the decision whether they want to stay with the same manager or not after an intermediate project. Not only does this project come with the cognitively strenuous task of having to evaluate the relative value of the two options, it also introduces a non-negligible amount of time that passes between the transfer and the decision. This makes our study a much more demanding test of the impact of direct transfers than the one that was implemented in Malmendier and Schmidt (2012). Moreover, our design further differs from that experiment by having an investor decide between staying with a manager or switch to a new one, as opposed to choosing between two completely unknown managers ("decision makers"). In our case the decision is one about an ongoing relationship, rather than a simple binary choice between two otherwise identical partners. The comparison with a non-social, yet otherwise comparable treatment makes for another difference.

Moving to the History treatment we observe more mixed results. A majority of investors (14/26) reports to have felt a relatively positive emotion towards a manager after a successful first project[34]. Reported scores for a sense of obligation and a likelihood to stay with the manager were lower, but still 8 and 10 investors reported relatively high scores, respectively. Moreover, the decision times in this treatment are almost identical to the Transfer treatment, while decision times in the Control treatment are significantly shorter. Despite the self-reported emotional reaction to the experience shared with the original manager and the apparently more time demanding complexity of the decision compared to the non-social setting we cannot detect a noticeable effect on the investors' incentivized decision. We are therefore forced to conclude that the hypothesized effect of being more willing to

[34]Defined as reporting three or higher on the emotion intensity scale.

forgo future earnings in exchange for the ability to benefit the earnings of a manager after a positive history than after a negative history was not observable in this experiment. There are a number of potential reasons for this.

First, it is possible that the situation experienced in the laboratory was too artificial to trigger the kind of social reaction that we were interested in. This explanation can go in two different directions: Either investors genuinely did not care about the fate of their managers, or they cared only to the extent that it did not hurt themselves. We feel rather confident that the first explanation can be rejected on the basis of the significantly higher decision times observed in the History treatment as compared to the Control treatment. The fact that decision times were nigh-identical in the History and Transfer treatments would seem to imply that the decision presented investors with a similar degree of complexity. Combined with the observation that the Transfer treatment did show a significantly larger effect of the transfer decision, this leads us to believe that subjects may have perceived some kind of conflict between their relationship induced preferences and their own self-interest.

A second explanation could be that investors were not fully aware of the fact that both active and inactive managers earned the same in the first two rounds of the History treatment, and also forgot that the managers' earnings are independent of the projects' success or failure. If some investors thought that inactive managers earned nothing or less than active managers that could have given them an additional motive to switch projects after a shared success, an effect that would be weaker after a shared failure. A standard inequity aversion utility function (Fehr and Schmidt, 1999), where we define all participants rather than only the matched manager as the relevant group, predicts investors would switch away from losing projects more often in order to equalize earnings (see appendix C.2.2). This effect might cancel out some of the hypothesized decrease in switching away after a shared success. However, for this to be relevant a subject would have to misunderstand or forget two carefully explained features of the experiment, one of which - the fact that manager

earnings are independent of success or failure - was tested in a post-instruction quiz. Note, furthermore, that if the subjects had only forgotten that inactive managers also receive income during the first two projects that would have affected switching rates after both histories equally.

In fact, the fixed earnings of managers might also have counteracted the detectability of the hypothesized effect. It is conceivable that the investors' perception of a bond or group identity with the manager that they interacted with might have been stronger if the managers had also been exposed to payoff uncertainty, as opposed to merely choosing a project and observing the result. There is suggestive evidence that the experience of a common threat can drive attachment (Carter, 1998), whereas our design explicitly exposes only the investor to the threat of losing.

To put the impact of different types of experiences into perspective we note that their effect on the investor's earnings was much less dramatic in the Transfer treatment, compared to the other two treatments. A success of the first game implied a profit of 300 ECU, 200 ECU more than a failure of that same project. In comparison to this the size of the transfer, 20 ECU, was negligible. This was a deliberate design choice, intended to make the transfer a primarily symbolic act. Nonetheless, it makes the clear result found for the Transfer treatment all the more notable.

The subjects' understanding of the relative values of the different projects presented to them was at best tenuous, as is demonstrated in figure 4. At least in the Control treatment one would expect a dramatic difference in switching rates between the situation in which switching is advantageous compared to when it is disadvantageous. While we had expected a somewhat better performance on the aggregate level, this is not in and of itself a problem for the investigation of our group level results, as the dilemmas that subjects are presented with are identical across treatments. In fact, as indicated in the design section, we intentionally chose a rather difficult stochastic environment under the assumption that

that would give us a better chance of finding the hypothesized effects. There is little reason to believe that the intensity of the investor's loyalty towards a manager is weakened by the complexity of the situation that she is facing. If that is true it should be of relatively greater relevance in the decision making process that leads to the ultimate behavior if the monetary value of the different alternatives is relatively difficult to determine. This argument is rooted in the idea of cognitive efficiency: The analysis of a relatively complex situation is cognitively demanding, therefore other decision factors can become more important with greater complexity.

6 Conclusion

We have shown conclusively that a project manager's decision to send a voluntary transfer to an investor at the beginning of a relationship changes the investor's decision whether to stay with that manager or switch to another manager at a later stage. This is true despite the fact that our design introduces additional steps in between the transfer and the investor's decision. However, we do not detect a similar effect for a merely shared positive history related to the outcome of a project. The relative difference in the likelihood to stay with or switch is not noticeably different from a control treatment in which the investor also takes on the role of the manager. Our hypothesis that positive experiences would facilitate bonding in a stochastic setting is therefore not substantiated without qualification by our participants' behavior.

Other measures such as decision time and the post-experiment questionnaires suggest that subjects did take the fate of the project manager into account in their decision making even when transfers were not possible. This factor is apparently not sufficiently relevant to sway their actual behavior significantly. It seems safe to assume that if such an effect exists its impact may be moderated by the saliency of the experience shared between investor and manager. We cannot exclude, therefore, that our design just failed to generate a sufficiently strong bond or group identity. The experience of a random draw from a relatively small lottery is clearly not as involving an experience as the type of long term business relationships we were inspired by. In any case, the effect appears to be a lot smaller than the effect that is triggered by the decision whether to send a transfer or not.

Appendix A Additional Figures and Tables

	Original Project				Alternative Project				Comparison Alternative Project and Original Project	
	EV ex ante	EV after failure	SD ex ante	SD ex post	Earnings if success	Earnings if failure	Expected value	Standard Deviation	Difference EV*	Difference SD**
Same Return and Variance	200	175	100	96.82	272	78	175	97	0	0%
Different Expected Return	200	175	100	96.82	262	68	165	97	10	0%
Different Expected Return	200	175	100	96.82	273	79	176	97	-1	0%
Different Expected Return	200	175	100	96.82	282	88	185	97	-10	0%
Different Expected Return	200	175	100	96.82	292	98	195	97	-20	0%
Different Spread of Returns	200	175	100	96.82	283	67	175	108	0	-10%
Different Spread of Returns	200	175	100	96.82	263	87	175	88	0	10%
Different Spread of Returns	200	175	100	96.82	256	94	175	81	0	20%

* The difference in expected value is expressed as the absolute difference in ECU by which the original project differs from the benchmark alternative project.

** The difference in Standard deviation is the relative difference in variance of the original project compared to the alternative project, rounded to full percentage points.

(a) Possible situations after an experienced failure

	Original Project				Alternative Project				Comparison Alternative Project and Original Project	
	EV ex ante	EV after success	SD ex ante	SD ex post	Earnings if success	Earnings if failure	Expected value	Standard Deviation	Difference EV*	Difference SD**
Same Return and Variance	200	225	100	96.82	322	128	225	97	0	0%
Different Expected Return	200	225	100	96.82	312	118	215	97	10	0%
Different Expected Return	200	225	100	96.82	323	129	226	97	-1	0%
Different Expected Return	200	225	100	96.82	332	138	235	97	-10	0%
Different Expected Return	200	225	100	96.82	342	148	245	97	-20	0%
Different Spread of Returns	200	225	100	96.82	333	117	225	108	0	-10%
Different Spread of Returns	200	225	100	96.82	313	137	225	88	0	10%
Different Spread of Returns	200	225	100	96.82	306	144	225	81	0	20%

* The difference in expected value is expressed as the absolute difference in ECU by which the original project differs from the benchmark alternative project.

** The difference in Standard deviation is the relative difference in variance of the original project compared to the alternative project, rounded to full percentage points.

(b) Possible situations after an experienced success

Table A.1: Project Combinations

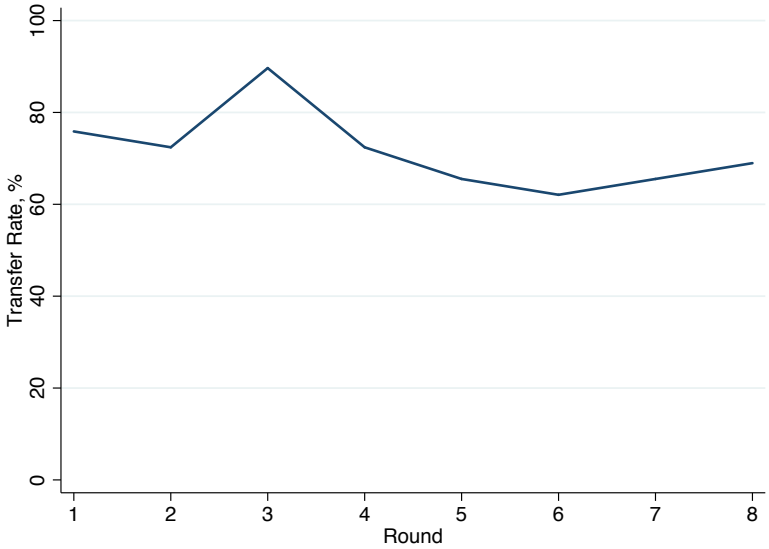


Figure A.1: Transfer Decisions over Different Rounds

	Investor switches project			
	(1)	(2)	(3)	(4)
History	-0.119		-0.190	-0.263
	(-0.48)		(-0.74)	(-1.00)
Transfer	0.699*		0.630	0.623
	(2.22)		(1.95)	(1.91)
Positive Experience	-0.348	-0.707***	-0.497*	-0.499*
	(-1.66)	(-4.73)	(-2.29)	(-2.30)
History × Positive Experience	0.0160		0.139	0.163
	(0.05)		(0.40)	(0.46)
Transfer × Positive Experience	-1.214**		-1.075**	-1.047**
	(-3.18)		(-2.75)	(-2.67)
Prior Result Positive		-0.708***	-0.699***	-0.723***
		(-4.80)	(-4.71)	(-4.83)
Expected Value Difference		-0.0282**	-0.0269**	-0.0258**
		(-3.18)	(-3.01)	(-2.87)
SD difference		0.00437	0.00329	0.00294
		(0.50)	(0.37)	(0.33)
Round				0.0196
				(0.61)
Female				0.0662
				(0.40)
Age				0.00825
				(0.29)
Economics Student				-0.206
				(-1.16)
Choice number				-0.0526
				(-1.56)
Constant	0.0664	0.452**	0.408*	0.486
	(0.43)	(3.21)	(2.26)	(0.65)
Individuals	106	106	106	106
N	848	848	848	848

Random effects model with z-statistics in parentheses, using robust standard errors

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.2: Investor decision regressions, probit

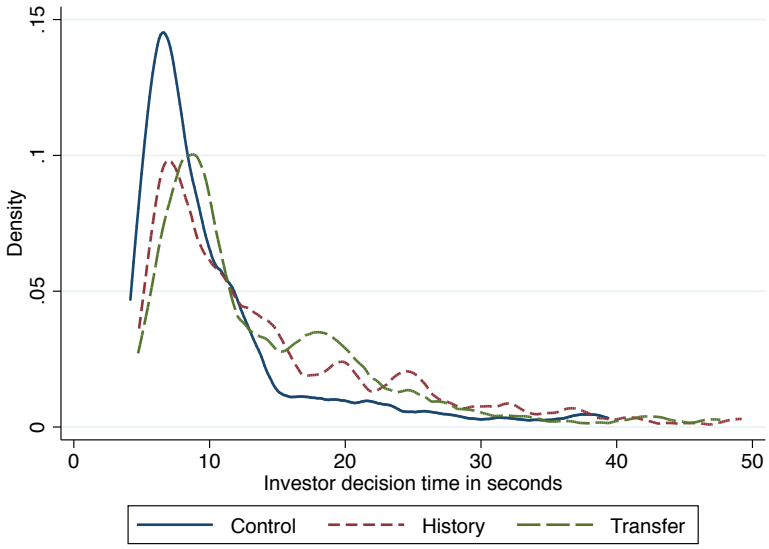


Figure A.2: Decision time density estimate, ignoring outliers above 50 seconds (Epanechnikov kernel with bandwidth of 1 second)

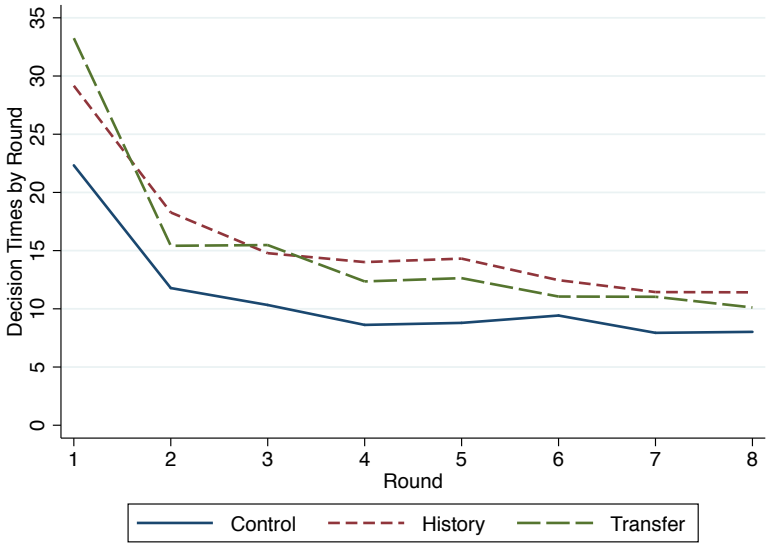


Figure A.3: Decision time over different rounds

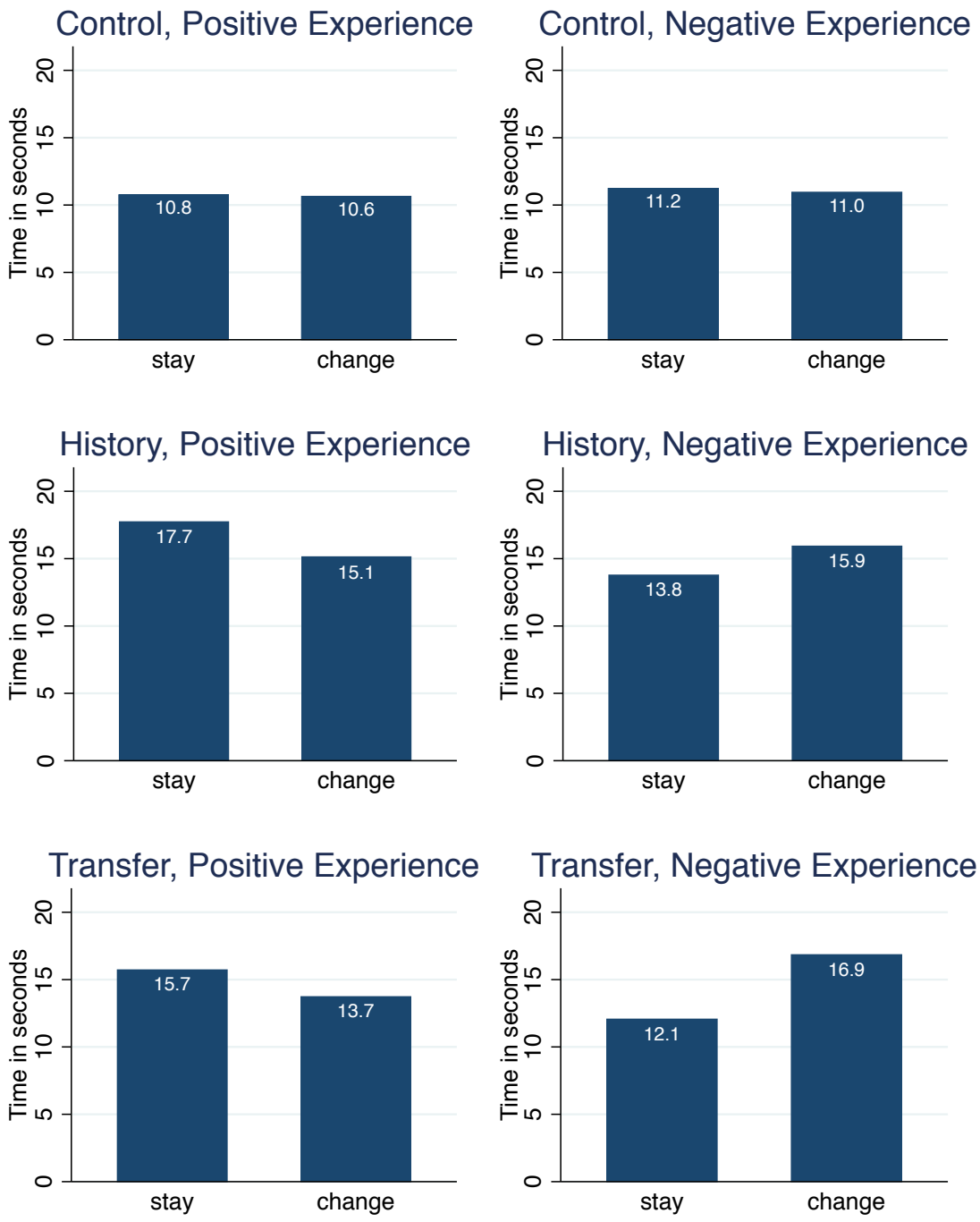


Figure A.4: Decision time by Experience

	Decision Time			
	(1)	(2)	(3)	(4)
History	3.595*		3.440*	3.686**
	(2.54)		(2.43)	(2.62)
Transfer	4.267*		4.180*	4.734**
	(2.55)		(2.49)	(2.94)
Positive Experience	-0.389	0.485	-0.622	-0.606
	(-0.35)	(0.63)	(-0.56)	(-0.60)
History × Positive Experience	2.523		2.801	2.619
	(1.39)		(1.55)	(1.58)
Transfer × Positive Experience	0.110		0.292	-0.831
	(0.06)		(0.15)	(-0.47)
Investor Switches Project		-0.360	-0.376	-0.106
		(-0.46)	(-0.48)	(-0.15)
Prior Result Positive		-1.490*	-1.587*	-1.599*
		(-1.97)	(-2.10)	(-2.32)
Absolute Expected Value Difference		0.00198	0.000921	-0.00277
		(0.03)	(0.02)	(-0.05)
Absolute SD Difference		-0.0586	-0.0586	-0.0614
		(-0.95)	(-0.95)	(-1.10)
Round				-1.843***
				(-12.80)
Female				-0.0618
				(-0.07)
Age				-0.208
				(-1.29)
Economics Student				1.558
				(1.53)
Choice Number				-0.0132
				(-0.08)
Constant	11.10***	14.32***	12.48***	24.48***
	(12.72)	(13.66)	(10.42)	(5.81)
Individuals	106	106	106	106
N	848	848	848	848
R2	0.0434	0.0071	0.0499	0.2009

Random effects model with z-statistics in parentheses, using robust standard errors

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.3: Investor decision time regressions

	History	Transfer	Total
Emotion and Obligation	0.54 (0.0038)***	0.35 (0.0598)*	0.58(0.0000)***
Obligation and Likelihood to Stay	0.15 (0.4260)	0.06 (0.7430)	0.32 (0.0149)**
Emotion and Likelihood to Stay	0.11 (0.5742)	0.41 (0.0266)**	0.42(0.0012)***

Table A.4: Correlations between questionnaire answers

	Investor Switches Project		
	(1)	(2)	(3)
Transfer	-0.0152	-0.203	0.396
	(-0.05)	(-0.73)	(1.47)
Positive Experience	0.134	-0.0514	0.167
	(0.25)	(-0.10)	(0.35)
Result previous project	-0.00932	-0.0217	-0.0346
	(-0.05)	(-0.11)	(-0.17)
Expected Value Difference	-0.0296*	-0.0288*	-0.0276*
	(-2.39)	(-2.36)	(-2.26)
SD difference	0.0112	0.0137	0.0135
	(0.91)	(1.13)	(1.12)
Emotion	0.210		
	(1.55)		
Positive Experience × Emotion	-0.333*		
	(-2.11)		
Obligation		0.424**	
		(2.71)	
Positive Experience × Obligation		-0.337	
		(-1.88)	
Likelihood to Stay			0.0304
			(0.22)
Positive Experience × Likelihood to Stay			-0.409*
			(-2.53)
Constant	-0.438	-0.800*	-0.0670
	(-0.97)	(-1.99)	(-0.17)
Individuals	56	58	58
N	448	464	464

Random effects model with z-statistics in parentheses, using robust standard errors
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5: Investor decision regressions

Appendix B Supplementary Material

B.1 Instructions

An interactive version of the instructions is available on `http://www.mhoyer.com/dev/inv_feelings`, together with an interactive demo of one round of the experiment.

B.2 Questionnaires

B.2.1 Transfer, manager

1. What is your age (in numbers)?

2. What is your gender?
   - female
   - male
3. What is your (primary) study program (if not a student please choose that)?

4. How would you describe your decision making process when deciding whether to send a transfer or not?

5. Did you send any transfers to your investor after being chosen?
   - Yes
   - No
6. If yes, which was the most important reason to do so?
   - Transferring doubled the income for the group as a whole
   - I hoped making the transfer would make the investor stay with my project
   - I just tried to be nice to the investor
7. Were you disappointed by an investor who switched to another project?
   - Yes, every time.
   - Yes, but only if my project was better than the alternative.
   - Yes, but only if I had sent the transfer.
   - No, the investor can choose what they want.
   - Not applicable, every investor I met stayed with my project.

B.2.2 History, Manager

1. What is your age (in numbers)?

2. What is your gender?
   - female
   - male
3. What is your (primary) study program (if not a student please choose that)?

B.2.3 Transfer, investor

1. What is your age (in numbers)?

2. What is your gender?

- female
- male

3. What is your (primary) study program (if not a student please choose that)?

4. How would you describe your decision making process when choosing whether to stay with a project manager or not in general?

5. Did you calculate the success probability of a project?

- Yes
- No
- I tried to, but failed

6. Did you try to calculate the expected value of the different projects? (expected value is probability times earnings)

- Yes
- No

7. Did you feel a positive emotion towards a manager who sent you a transfer?

- 1 - Not at all
- 2
- 3
- 4
- 5 - Very strongly
- The first project never succeeded.

Next page:

8. Did you feel a sense of obligation towards a manager who sent you a transfer?

- 1 - Not at all
- 2
- 3
- 4
- 5 - Very strongly
- Never received a transfer

9. Were you more likely to stay with a project and manager if the manager sent you a transfer earlier?

- 1 - Not at all

- 2
- 3
- 4
- 5 - A lot
- Never received a transfer

B.2.4 History, investor

1. What is your age (in numbers)?

2. What is your gender?

   - female
   - male

3. What is your (primary) study program (if not a student please choose that)?

4. How would you describe your decision making process when choosing whether to stay with a project manager or not in general?

5. Did you calculate the success probability of a project?

   - Yes
   - No
   - I tried to, but failed

6. Did you try to calculate the expected value of the different projects? (expected value is probability times earnings)

   - Yes
   - No

7. Did you feel a positive emotion towards a manager if the first project succeeded?

   - 1 - Not at all
   - 2
   - 3
   - 4
   - 5 - Very strongly
   - The first project never succeeded

Next page:

8. Did you feel a sense of obligation towards a manager who's first project was a success?

   - 1 - Not at all
   - 2
   - 3
   - 4

- 5 - Very strongly
- Never received a transfer

9. Were you more likely to stay with a project and manager if the manager's first project was a success and, if so, how much more?

- 1 - Not at all
- 2
- 3
- 4
- 5 - A lot
- Never received a transfer

B.2.5 Control, investor

1. What is your age (in numbers)?

2. What is your gender?

- female
- male

3. What is your (primary) study program (if not a student please choose that)?

4. How would you describe your decision making process when choosing whether to stay with a project or not in general?

5. Did you calculate the success probability of a project?

- Yes
- No
- I tried to, but failed

6. Did you try to calculate the expected value of the different projects? (expected value is probability times earnings)

- Yes
- No

Appendix C Alternative Theories

C.1 The Role of Other Social Preference Models in our Design

Looking at the History treatment, in the design that is implemented in this experiment many social decision theories popular within economics do not play any role whatsoever. This is a direct result of two design features:

First, project managers do not know what project they are choosing and do not have any influence on the income of the investor beyond the act of choosing a project that is more or less likely to be successful. This precludes any influence of reciprocity of any kind.

Secondly, the payment scheme chosen for the project manager effectively nullifies many concerns that investors might have about the effect that their choice might have on other subjects. Since by the time the investors' decisions are made every project manager has earned the exact same amount - 200 ECU for each of the first two projects - motives such as inequity aversion or envy are meaningless. Since investors cannot affect the type of distribution of the others' earnings in any way, this is true irrespective of the exact theory applied, such as for example Fehr and Schmidt (1999) or Bolton and Ockenfels (2000). In fact, social welfare concerns are also irrelevant, therefore excluding approaches such as simple max-min preferences or the model in Charness and Rabin (2002).

In the Transfer treatment the situation is slightly different. Since the transfer decision is intentional, intention-based theories predict an effect. Still, inequality-oriented motives play a negligible role: They are only relevant if the project manager withholds the transfer. In that case she earns 10 ECU more from the exchange than managers who did sent the transfer or, more importantly, were inactive. This however is only a one-twentieth of the fixed income that managers earn if they are recruited for the final project, giving us confidence that we can ignore it as a confounding factor.

C.2 The Case of Flexible Project Manager Payment

In section 5 we mentioned that one potential explanation for not being able to pick up a result is that subjects might have thought of the design in terms of a outcome dependent payment systems for managers. We also mentioned that such a system could be argued to potentially have lead to stronger results, creating a stronger emotional connection between investors and managers. To shed further light on how these two claims can coexist we will now investigate the theoretical implications of a flexible manager payment system in our design.

C.2.1 Game Design and Best Response

We start by changing the payment of the project manager from a fixed fee (200 ECU in the experiment) to a share of the return of the project, assigning s to the investor and $1 - s$ to the manager ($0 < s < 1$). In a world in which earnings are outcome dependent it makes sense to normalize the income of inactive managers at 0. We assume that the income from a failed project is positive and the income from a successful project is only restricted by the condition that it be strictly larger than the income from a failed project. This is a more general formulation than the fixed earnings of 300 and 100 used in equation (1), but still ensures that nobody can make losses from a project, which will be convenient later on. Together with the assumption that both project types, whether they have a high or low success probability, generate each outcome with a probability that is strictly larger than zero, implies that even a project with a low success probability has positive expected value. The only further restriction on projects is that, while different projects can have different returns, we impose that returns after successes and failures are such that for any projects of the high or low success probability type (π_H, π_L) or an unknown history (π) the ordering $0 < E(\pi_L) < E(\pi) < E(\pi_H)$ holds. Compared to the situation in section 3, equations (1) through (6) stay fundamentally the same, but are scaled by the factor s and generalized,

leading to the following:

$$E(\pi^O|h) = s\Big(P(\pi = \pi_H|h)E(\pi_{HO}) + P(\pi = \pi_L|h)E(\pi_{LO})\Big) \tag{8a}$$

$$E(\pi^A) = s\Big(\frac{1}{2}E(\pi_{HA}) + \frac{1}{2}E(\pi_{LA})\Big) \tag{8b}$$

The situation differs more when we start constructing the equivalent of equation (7). In particular we are looking at an investor with a positive or negative regard for the manager with whom she has some kind of previous history, henceforth manager M_1. We weigh the influence of this manager's earnings by factor α_{M_1}. With a utility function that is linear in the investor's and the manager's payoffs the expected utility of staying with the current project is:

$$\begin{aligned} E\Big(U(\pi_o|h)\Big) =& s\Big(P(\pi = \pi_H|h)E(\pi_H) + P(\pi = \pi_L|h)E(\pi_L)\Big) + \\ & \alpha_{M_1}(1-s)\Big(P(\pi = \pi_H|h)E(\pi_H) + P(\pi = \pi_L|h)E(\pi_L)\Big) \\ =& \Big(s + (1-s)\alpha_{M_1}\Big)\Big(P(\pi = \pi_H|h)E(\pi_H) + P(\pi = \pi_L|h)E(\pi_L)\Big) \end{aligned} \tag{9}$$

where we ignore income from the first project (History treatment) or the transfer stage (Transfer treatment) for the moment.

The expected utility of choosing a new project is unaffected by any preferential other-regarding factor and free of any informative history about the project success. As a result it is identical to the expected value of a project of unknown type that is managed by a manager who has not been encountered previously.

$$E\Big(U(\pi)\Big) = s\Big(\frac{1}{2}E(\pi_H) + \frac{1}{2}E(\pi_L)\Big) \tag{10}$$

This implies that, even if the expected value of an alternative project is higher than the expected value of a previously implemented project, an investor might decide to stay with the current project and project manager, if the specific other-regarding preference factor

α_M is big enough. Vice versa, a negative α_M could make the investor switch projects, even if the expected value of the alternative project is lower than that of the original project.

The arguments for why we should observe the effect hypothesized about in the main experiment is therefore fairly similar to what we described in section 3. What differs however are the implications of other theories of social preferences, which we investigate in the rest of the appendix.

C.2.2 General inequity aversion

Since we cannot directly observe if the investor develops a specific regard for the original manager's earnings, we have to rely on their decision to indirectly analyze their behavior for the presence of the presumed effect. We will now look at other potential motives for staying with the original manager. In particular, could different types of inequity aversion explain this behavior?

A complicating factor would be if investors do not only develop a specific factor α_M for the well-being of their immediate peer, but also exhibit general inequity aversion that incorporates the whole pool of potential managers – in this case adding all possible managers in a experiment session. We argue that, since the investor can shift income to at most one additional manager, we can restrict ourselves to the effect that such a decision would have on the manager that is ultimately chosen, even though the particular manager has not yet been selected when the investor decides whether to change projects or not. To analyze this situation formally we need to describe all histories that can arise out of the up to two (History treatment) different projects that can be experienced prior to the decision whether to stay with the current project and project manager or not. From this point onwards we speak only of the History treatment. Equivalent arguments apply to the Transfer treatment.

Assume one investor I and two managers M_1 and M_2 and different histories of the implementations of the first two projects, h_{ff} (all project draws were failures) and h_{fs}, h_{sf}, h_{ss}

(the first and/or the second project were successful). We summarize these three cases by writing $\overline{h}$. Assuming a completely linear payment scheme, the relative wealth position of the investor and the first manager is only determined by factor s. In all histories the incomes of the investor and M_1 are at least as high as the income of M_2. After any history $\overline{h}$ we know for certain that their income prior to the decision being made is larger than that of M_2. Staying with the original partner is equivalent to choosing to repeat the previous project and choosing a new partner is equivalent to letting them choose a new, random project, therefore we will express the investor's choices in terms of choosing projects rather than partners. We denote cumulative earnings of agent x at time t by Π_t^x, the utility of the investor at time t as a function of history h and project choice c as $U_t(h,c)$, and use E_2 to describe an expectation that is formulated at time $t=2$, just after the second (History treatment) or first (Transfer treatment) project has been implemented and before the decision to proceed or switch has been made.

We start by assuming max-min preferences for the investor. If that is the true model for her preferences we can express her expectation of the utility at $t=3$ that results from choosing the alternative project at $t=2$ as

$$\begin{aligned} E_2\Big(U_3(h,\pi_A)\Big) =& \min\Big(E_2(\Pi_3^I(h,\pi_A)), E_2(\Pi_3^{M_1}(h,\pi_A)), E_2(\Pi_3^{M_2}(h,\pi_A))\Big) \\ =& \min\Big(\Pi_2^I(h) + s\Big(\frac{1}{2}E(\pi_H)+\frac{1}{2}E(\pi_L)\Big), \Pi_2^{M_1}(h), \\ & \Pi_2^{M_2}(h) + (1-s)\Big(\frac{1}{2}E(\pi_H)+\frac{1}{2}E(\pi_L)\Big)\Big) \end{aligned} \tag{11}$$

The expected utility from choosing a new draw of the original project is

$$\begin{aligned} E_2\Big(U_3(h,\pi_O)\Big) =& \min\Big(\Pi_2^I(h) + s\Big(P(\pi_H|h)E(\pi_H)+P(\pi_L|h)E(\pi_L)\Big), \\ & \Pi_2^{M_1}(h) + (1-s)\Big(P(\pi_H|h)E(\pi_H)+P(\pi_L|h)E(\pi_L)\Big), \\ & \Pi_2^{M_2}(h)\Big) \end{aligned} \tag{12}$$

We first look at the three different histories subsumed under $\overline{h}$: Since $\Pi_2^{M_2}(\overline{h}) \leq \Pi_2^{M_1}(\overline{h})$ (strict inequality unless the return of a failed project is zero) the expected utility of switch-

ing is greater or equal than the alternative that would result from sticking with the current manager, irrespective of the expected values of π_L, π, or π_H. Following the fourth history, h_{ff}, the result is fundamentally the same, with the sole exception being the situation in which the income from a failed project is exactly zero. In that case everybody's earned income is zero and max-min preferences lead to indifference between the two options, because any choice gives at least one manager an income of zero. Thus we can state conclusively that the max-min preference can only ever drive an investor to be more prone to switching to an alternative project and manager, but not the opposite. What we are really interested in however is how much more or less of a factor this becomes after different histories. The active manager's income scales with the amount earned in the projects directly leads to the answer: The effect is smallest after h_{ff}, equally big after h_{fs} and h_{sf}, and biggest after h_{ss}. This is a direct result of the fact that $(1-s)\Pi^{M_1}(h_{ff}) < (1-s)\Pi^{M_1}(h_{fs}) = (1-s)\Pi^{M_1}(h_{sf}) < (1-s)\Pi^{M_1}(h_{ss})$.

Note that the situation is different if inactive managers earn the same as employed managers during the first and second period of the game, as was the case in section 3. In that case $\Pi_2^{M_1}(h) = \Pi_2^{M_2}(h)$ and the only difference between the expected utility following the two choices is in the relative size of the factors $\frac{1}{2}E(\pi_H) + \frac{1}{2}E(\pi_L)$ and $P(\pi_H|h)E(\pi_H) + P(\pi_L|h)E(\pi_L)$. Max-min preferences can therefore only ever play a role if the investor has earned less than the managers and in that case their effect points in the opposite direction as our main hypothesis implies.

Inequity preferences of the Fehr and Schmidt (1999) type have slightly different implications. We use the standard formulation $U_i(x) = x_i - \alpha_i \frac{1}{n-1} \sum_{j \neq i} \max(x_j - x_i, 0) - \beta_i \frac{1}{n-1} \sum_{j \neq i} \max(x_i - x_j, 0)$ with $\beta_i \leq \alpha_i$ and $0 \leq \beta_i < 1$. Assuming $s \geq \frac{1}{2}$, the only possible difference is advantageous to the investor, i.e. only the investor's β is of relevance. Since dis-utility from earning differences only affects utility linearly in the model, it does not matter if the welfare difference between the investor and the original manager or the welfare difference between the investor and the alternative manager becomes grows more extreme. The investor only

cares about the sum of inequality, but not its distribution among the two managers. If $s < \frac{1}{2}$, the active manager earns more from a project than the investor. This triggers the "envy" parameter α of the utility function, which is multiplied with the difference between their respective earnings. Since the inactive manager does not have any accrued income and the model assumes $\beta_i \leq \alpha_i$, the envy aspect of the utility function increases the investor's motive to stay with the current manager. Similarly to the mechanism applied in the max-min case, this motive is stronger the higher previous earnings are, making it more relevant after histories h_{fs} and h_{sf}, and biggest after h_{ss}.

ERC (Bolton and Ockenfels, 2000) preferences do not consider the total distribution of incomes, but merely the agent's own income and the relative income of the agent compared to the average income of the group. As such, even without using any of ERC's other assumptions, we know that the distribution of income between the two managers is not of relevance to the investor's utility. Since ERC also imposes strictly increasing utility in own income it therefore implies that the project with the highest expected value is preferred.

As in the design used in the experiment, the equivalent argument in the Transfer treatment is simultaneously simpler - we only have to consider the outcomes of one project as opposed to two - and more complicated - managers who withhold the transfer have earned more than those who send it or are inactive[35]. The former difference does not change the direction of any of the described effects. The latter difference can be shown to motivate an investor to switch away from an original manager who withheld a transfer more often than from one who sent it, using any of the theories presented here.

C.2.3 Concluding Remarks on Flexible Manager Payment

In conclusion, we have shown that in the History treatment the only effect that the presented theories of inequity aversion could explain would be a propensity to switch more

[35] In case of a loss a withholding manager now also has higher earnings than the investor.

often to a different project and manager after successful than after failed projects. Therefore they are unequivocally pointing in the opposite direction of the hypothesis that forms the basis of this experiment, namely that investors place a positive weight on the future income of managers with whom they experienced a positive history. Using a result-dependent payment system for the managers would therefore be a valid approach from the perspective of hypothesis testing. As we have seen, it would however create many situations in which at least some conventionally found forms of social preferences (max-min, ERC) describe effects that work in the opposite direction as the suspected effect, making it very hard to detect. This is why we decided to choose a fixed fee for the managers instead.

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