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ORIGINAL ARTICLE

Third Parties and Specific Investments

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Abstract Competitive advantage is typically based on a unique nexus of firm-specific investments that creates inimitable quasi-rents. Because it is impossible to write complete contracts on how to distribute the quasi-rents, stakeholders tend to underinvest in firm-specific assets to avoid the hold-up risk. This paper empirically tests the effect of third-party ownership on specific investments. Third-party ownership assigns the rights of residual control to independent fiduciaries. We conduct variations of the trust game, in which a third party, rather than the receiver, distributes the returns on investments. A randomly chosen third party with a fixed payment induces larger investments over time although the experimental design rules out reputation building. If receivers select the third parties, this benefit vanishes. If the third party receives a reward for the appointment, investments actually decrease. Investors (unwarrantedly) fear lower back transfers in such cases.

Keywords Third Parties · Specific Investments · Residual Control · Experiment

JEL Classification D23 · D33 · D72

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1 Introduction

Firms can achieve a competitive advantage through valuable and rare resources that competitors can neither imitate nor substitute (e. g., Barney 1991; Wang and Barney 2006; Wang et al. 2009). A network of mutually specialized inputs is such a resource that creates economic rents that the market cannot replicate. Firm-specific investments create quasi-rents, denoting the differences between the value of the investments in the firm and the value of their next-best use in another firm (Klein et al. 1978). As writing complete, contingent contracts on how to distribute the quasi-rents is impossible, the co-specialization makes each specific investor vulnerable to opportunistic and inefficient behavior by the other. To prevent any hold-up risk, investors tend to underinvest in firm-specific assets. As a result, the quasi-rents and the firm's competitive advantage decrease (Milgrom and Roberts 1992). Many scholars (e. g., Rajan and Zingales 1998; Blair and Stout 1999; Lan and Heracleous 2010; Franck 2011) therefore suggest transferring the rights of residual control to independent third-party trustees to facilitate firm-specific investments. By mediating between the conflicting interests of firm-specific investors, third-party trustees act as an institutional safeguard for the returns to firm-specific investments.[1]

While the theoretical argument is obvious, it is difficult to test the effect of third-party ownership on specific investments. First, the empirical identification of the effect of third-party ownership on specific investments is complicated by endogeneity concerns. As ownership models and investments are jointly determined, correlations are likely to be confounded by omitted variables. Second, the amount of specific investments and truly independent third parties are difficult to identify in the field.

To deal with these problems, we document results from an experiment in which investors are randomly assigned to different treatments. The standard investment or trust game established by Berg et al. (1995), in which an investor transfers money to a receiver who receives the tripled transfer and decides how much of this money to return to the investor, serves as a baseline treatment. As with specific investments, the returns to investments in the trust game are uncertain and not hedged by an outside option. In addition to the baseline treatment, we conducted three third-party treatments in which a third party, instead of the receiver, decides on the back transfer to the investor. In the *random third-party (RTP)* treatment, the third party is randomly assigned and receives a fixed payment. In the *RTP* treatment, the third party is truly independent. In the *selected third-party (STP)* treatment, the receiver selects a third party who receives a fixed payment based on non-binding promises about the back transfer. In the *STP* treatment the third party is materially independent, but the selection procedure can still bias the back-transfer decision. In the *competitive third-party (CTP)* treatment, the selection procedure is the same as in the *STP* treat-

[1] Another approach to protect firm-specific investments is to increase the decision rights and bargaining power of those who contribute to the quasi-rents. Such a shared ownership model recommends that firm-specific investors participate in all matters not specifically regulated via contracts or the law (Furubotn (1988); Blair (1995); Franck (2002); Osterloh and Frey (2006)). However, the involvement of many firm-specific investors with heterogeneous interests is likely to result in high costs of collective decision-making. For a detailed comparison of the shared ownership and third-party ownership model, we refer to Nüesch and Franck (2010).

ment, but the third party's payment increases with the number of receivers selecting that third party. In the *CTP* treatment both the selection mechanisms and the payment may compromise the third party's independence. Using a repeated stranger-matching procedure between investors and receivers, we compare investments, back transfers, and the net payoffs of both the investor and the receiver across treatments.

We find significantly higher investments, on average, if the back transfer decision is assigned to a third party that is randomly assigned and that receives a fixed payment. This increase develops over time even though our repeated stranger-matching protocol excludes reputation building between investor and receiver. Although the investment levels initially do not differ from the investment levels in the baseline treatment, they are significantly higher in later rounds. Thus, independent third parties are not a sufficient condition for increased investments. Investors also have to experience positive returns before increasing their investments.

We also find that selection and payment procedures can easily impair perceived independence and destroy trust. If the receiver is able to select a third party based on non-binding promises about the relative rent allocation and previous back transfer decisions, and if the third party is paid its fixed payment only when it is selected, investments significantly decrease. Investors fear lower back transfers, even though these feared outcomes do not occur. Third parties who materially benefit from appointments do not return less money than the receivers would return if they made the return decision themselves. Last, while the introduction of truly external third parties increases aggregate welfare, receivers still have incentives to oppose such a policy as their payoffs are lower in the third party treatments than in the baseline treatment.

The remainder of this paper is structured as follows: The next section explains our experimental design. Sect. 3 presents the behavioral predictions, and Sect. 4 the results. Sect. 5 concludes.

2 Experimental Design

This paper tests the effects of third parties on investments by playing four variants of the "investment game" (or trust game) of Berg et al. (1995). At the beginning of the experiment, subjects learned whether they were investors or receivers. They kept this role throughout the entire experiment. In the standard investment game, our baseline treatment, *BASE*, one investor and one receiver were anonymously paired in each of 10 rounds according to a stranger matching protocol. At the beginning of the round both players received 10 Euros. The investor was asked to transfer a portion I of the endowment ($0 \leq I \leq 10$) to the receiver. This transfer measures the investor's investment. The experimenter tripled the transferred money so that $3I$ was passed to the receiver. Then the receiver could pass any portion T of the money received ($0 \leq T \leq 3I$) back to the investor.

We implemented three variants of the investment game in which the back transfer decision was assigned to a third party. As in the standard investment game, the investor transferred between 0 and 10 Euros to the receiver, and the receiver obtained three times the transferred money. Then the third party decided what portion of the tripled investment was returned to the investor.

We exogenously manipulated the third party's selection and payment procedures across the different treatments. In the *random third-party* treatment, *RTP,* the computer randomly assigned a third party in each round. All third parties received a fixed payment of 10 Euros regardless of the amount of money they transferred back to the investor. The *RTP* treatment describes a situation in which the third party is completely independent from the receiver.

In the *selected third-party* treatment, *STP,* each receiver could choose a third party out of the potential third parties. At the beginning of each round, the potential third parties made non-binding promises about the relative allocation of the tripled investment. Each third party had his or her own constant numeric identifier (1–9) across all rounds. As in the previous treatment, a third party received 10 Euros independent of any decision. A third party with multiple appointments made separate decisions for each of the receivers. While the third parties are materially independent in this treatment, the selection procedure can induce a bias towards the receiver.

In the *competitive third-party* treatment, *CTP,* the third-party selection mechanism was the same as in the *STP* treatment. However, the payment to a third party increased with the number of receivers selecting that third party: A third party received 5 Euros plus an additional 5 Euros for each appointment as third party. For example, a third party chosen by three receivers was paid 20 Euros (5 Euros fixed payment + 3 × 5 Euros per appointment). This payment scheme ensured the same average payment for third parties across all treatments. In this treatment both the payment and the selection procedure may compromise the third party's independence.

The third party's payment is paid by the experimenter in all three third-party treatments to allow for a simple comparison between the different treatments (see also Fershtman and Gneezy 2001). Otherwise the introduction of a third party would reduce the pie to be divided between the investor and the receiver independent of the investment.

As previously mentioned, we implemented a repeated stranger matching protocol for investors and receivers over 10 rounds in all treatments. The computer randomly (re-)matched investors and receivers in each round.[2] Investors invested without knowing which receiver and/or third party was selected or assigned in that round. Within groups, full feedback about investments and back transfers was given at the end of each round. All details of the game, such as the matching protocol, the payment schemes, and the feedback rules, were common knowledge. Table 1 summarizes the experimental design.

We conducted 14 sessions with a total of 373 subjects. All sessions took place in November and December 2012 at the *Lakelab* at the University of Konstanz. Subjects were University of Konstanz students recruited through the software "ORSEE"

[2] Because we played 10 rounds but had only nine investors, receivers and third parties per session in *RTP, STP* and *CTP*, a perfect stranger matching protocol was not feasible. However, due to the investor's lack of knowledge of the assigned receiver's or third party's identity, the large number of subjects, and the random matching protocol, repeated game effects should not play a role.

Table 1 The Experimental Design (1 out of 10 Rounds)

	Baseline (BASE)	Random third party (RTP)	Selected third party (STP)	Competitive third party (CTP)
Stage 1	Random anonymous matching of investor and receiver			
Stage 2	Investor makes investment I, receiver obtains 3 * 1			
	No third party	Random assignment of third party	Potential third partys announce relative allocation (non-binding), receiver selects a third party	
Stage 3	Receiver decides on the back transfer (T) to the investor	Third party decides on the back transfer (T) to the investor		
Stage 4	Full information about decisions and payoffs within groups Payoff investor 10 – I + T Payoff receiver: 10 + 3 * I – T			
	No third party	Payoff third party: 10	Payoff third party: 5 + #selections*5	

(Greiner 2004). The experiments were computerized with the software "z-Tree" (Fischbacher 2007). Each subject participated in only one of the treatments. Upon arrival at the laboratory, subjects were randomly assigned the role of investor, receiver, or third party, and kept that role during the entire experiment (i. e., no role reversal). They received written instructions and comprehension questions that they had answer correctly before the experiment could start. An English translation of the instructions is included in Appendix A.[3] The sessions lasted approximately 50 minutes, and subjects earned 13.65 Euros, on average.[4] To avoid wealth effects, one round was randomly selected to count for payment at the end of the experiment. All subjects were paid privately so that no one observed how much the others received.

3 Behavioral Predictions

In this section we describe the predictions of the behavior of the subjects in our experiment. While our main interest lies in the investment decisions, these decisions depend on the investors' beliefs about the back transfers in the different treatments. Thus, we initially focus on these back transfers.

Our analysis relies on two assumptions about preferences. First, all participants benefit from increasing their own monetary payoff. Second, they also care about the payoff distribution between investors and receivers. We consider those preferences as separable. The second assumption takes into account that inequity aversion, reciprocity, or social norms induce some receivers to return money even at their own cost to implement the distribution they consider as appropriate. This effect

[3] The experiments were conducted in German. The instructions in the Appendix A constitute a translation of the original instructions.

[4] At the time of the experiment, 1 Euro equaled about 1.3 USD.

is well documented in the literature.[5] We therefore expect some receivers to return money to the investors in case of a positive investment. In the *RTP* treatment, the third party does not gain any material benefit from favoring the receiver over the investor (or vice versa). Thus, we expect higher back transfers in the *RTP* treatment than in the *BASE* treatment.

Hypothesis 1 The expected back transfers T for a given investment level I are higher in the *RTP* treatment than in the *BASE* treatment.

In both the *STP* and *CTP* treatments, third parties make non-binding promises on the relative rent allocation, but the number of selections affects the third party's payment only in the *CTP* treatment. In the *CTP* treatment, third parties have an incentive to make a promise that reflects the presumed preferences of receivers.[6] In the first round, these promises constitute the only meaningful criterion for distinguishing the third parties. In rounds 2 to 10, receivers can select a third party based on actual decisions made in previous rounds. By keeping their promises, third parties can build up a good reputation, which increases the likelihood of future selections by the same receiver. Therefore, the back transfers of the selected third parties in the *CTP* treatment reflect the preferences of the receivers.

Hypothesis 2 The back transfers T for a given investment level I are equal in the *BASE* and *CTP* treatments.

Unlike in the *CTP* treatment, the third parties in the *STP* treatment have no financial incentives for matching the preferences of the receivers in their promises, for keeping that promise or for favoring the receiver over the investor when deciding on the back transfers. Thus we predict higher back transfers in the *STP* treatment than in the *CTP* treatment. This prediction holds even in the case of heterogeneous promises of third parties in the *STP* treatment. Ideally a receiver would select a promise that reflects most closely his or her own preferences. However the probability of obtaining such a promise is much lower in the *STP* treatment than in the *CTP* treatment because third parties in the *STP* treatment have no incentives to make such a promise. Hence, the *expected* back transfer in the *STP* treatment will be more egalitarian than in the *CTP* treatment.

Hypothesis 3 The back transfers T for a given investment level I are higher in the *STP* treatment than in the *CTP* treatment.

The combination of hypotheses 2 and 3 implies larger back transfers in the *STP* than in the *BASE* treatment and lead to the following hypothesis.

[5] Fehr (2009) provides a summary of the relevant results in this context; Johnson and Mislin (2011) provide a meta-analysis.

[6] Due to the heterogeneity of preferences among the different receivers we actually obtain multiple equilibria regarding the optimal proposal of a third party in this case. We assume that the proposing third parties can overcome the resulting coordination problem.

Hypothesis 4 The back transfers T for a given investment level I are higher in the *STP* treatment than in the *BASE* treatment.

In monetary terms the incentives of third parties in the *STP* and *RTP* treatments do not differ. Yet some non-monetary motives govern why the rewards in the *STP* treatment will be smaller than in the treatment with randomly selected third parties (*RTP*). Several studies have shown that many individuals perceive lying as a violation of social norms, a violation that induces psychological costs to themselves and others (Brandts and Charness 2003; Croson et al. 2003; Sánchez-Pagés and Vorsatz 2007, 2009; Erat and Gneezy 2012). Thus third parties in the *STP* treatment are expected to adhere to their promised distributions. If third parties have heterogeneous fairness preferences and make different promises, the honesty motive allows receivers to select third parties according to their promises. As a result the selected third parties will be biased towards the receivers. Moreover, some third parties might receive ego rents merely from being selected (Rogoff 1990; Garrì 2010; Fehr, Herz and Wilkening 2013). These people then also benefit from building up a reputation by implementing their promises.

Hypothesis 5 The expected back transfers T for a given investment level I are higher in the *RTP* treatment than in the *STP* treatment.

Overall hypotheses 1 to 5 lead to the following relationship between expected back transfers T for a given investment level I across all treatments:

$$0 < E\left(T_{CTP} I\right) = E\left(T_{BASE} I\right) < E\left(T_{STP} I\right) < E\left(T_{RTP} I\right),$$

A risk-neutral investor will transfer all 10 points as long as the expected return proportion is at least one third. Otherwise the investment is 0 points. We assume that the proportion of individuals who have beliefs above that threshold is increasing monotonically with the true distribution of back transfers. Thus we obtain the following hypothesis for *expected* investments:

Hypothesis 6 The average investment levels I are highest in the *RTP* treatment, followed by the *STP* treatment, and then followed by the *CTP* and *BASE* treatments.

4 Experimental Results

In line with the order of the behavioral predictions, we first analyze back transfers in Sect. 4.1 and then investments in Sect. 4.2. In Sect. 4.3 we examine the evolution of investments and back transfer decisions over time. In Sect. 4.4 we analyze the investors' and receivers' payoffs.

Table 2 displays a summary of the average proportion returned by the receiver or the third party, the average investment by the investor, and the average investor's and receiver's payoffs. The average payoff of the third party was 10 in all third-party treatments.

Table 2 Summary of Behavior

	BASE		*RTP*		*STP*		*CTP*	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Investments	5.10	3.64	6.24	3.42	4.81	3.22	3.52	3.35
Proportion returned[a]	0.32	0.19	0.58	0.21	0.39	0.24	0.29	0.25
Investor's payoff	10.10	3.52	14.80	5.34	10.80	3.19	9.45	4.18
Receiver's payoff	20.11	7.84	17.68	6.05	18.83	7.77	17.59	7.46
Sessions	3		4		3		4	
Investor/receiver/third party	41/41/n.a.		35/35/35		27/27/27		35/35/35	
Rounds	10		10		10		10	
Observations	410/410/n.a.		350/350/350		270/270/270		350/350/350	

[a] To calculate the means and standard deviations of the proportions returned, we used only observations with an initial investment above 0. If the investor invested 0, the receiver/third party was not able to return anything. Number of observations are 325 in *BASE*, 314 in *RTP*, 216 in *STP* and 247 in *CTP*, respectively

4.1 Back Transfers

Table 3 presents OLS estimates of the treatment effects on proportions returned and investments. The main explanatory variables are dummies for each third-party treatment (the baseline treatment serves as reference category). As individuals were randomly selected into treatments, treatment dummies are exogenous and OLS provides unbiased estimates. To allow for any arbitrary correlation of the error terms within a session, we use robust standard errors clustered at the session level.[7] As the receiver or the third party could only decide to return something if the investor had made a positive investment, we restrict the sample to observations with investments above 0 when analyzing back transfer decisions.[8] In Model 2 we additionally control for investments because we expect them to differ across treatments (see next subsection).

Models 1 and 2 in Table 3 show that the return proportion is around 0.25 higher in *RTP* and around 0.07 higher in *STP* than in *BASE* for a given investment level. Whereas the first difference is statistically significant at the 0.1 % level, the second difference is marginally significant at the 10 % level. As predicted in Hypothesis 2, the return proportion in *CTP* is not significantly different from the return proportion in *BASE*. The size of the investments does not influence the return proportion. Further estimations documented in Appendix B also reveal that neither gender nor the education background of the participants drive the effects.

[7] The results remain virtually the same whether we cluster the standard errors on the subject level or whether we estimate random effects models.

[8] The exclusion of observations with 0 investments reduces the sample by 278 observations and by one subject who experienced 0 investments in all ten rounds.

Table 3 Third-Party Influence on Proportions Returned and Investments

Dependent variable Sample	Proportions returned Obs. with investment > 0		Investments All obs.
	(1)	(2)	(3)
BASE	Ref. group	Ref. group	Ref. group
RTP	0.255*** (0.026)	0.253*** (0.025)	1.135** (0.519)
STP	0.066* (0.036)	0.068* (0.036)	–0.288 (0.545)
CTP	–0.038 (0.046)	–0.031 (0.045)	–1.585*** (0.505)
Investments		0.005 (0.003)	
Constant	0.325*** (0.022)	0.294*** (0.034)	5.102*** (0.476)
Number of observations	1102	1102	1380
Number of subjects	137	137	138
R^2	0.219	0.222	0.080

RTP, STP and *CTP* are treatment dummies. Table displays OLS coefficients with White robust standard errors clustered at the session level in parentheses
Significance levels are denoted by *** 1 percent, ** 5 percent, * 10 percent (two-tailed tests)

T-tests and non-parametric Wilcoxon rank-sum tests (all two-tailed) of differences in mean proportions returned across all rounds, using each receiver or third party as one observation, support the OLS results and additionally show that average return proportions in *CTP* are significantly lower than in *STP* ($t = -2.21$, $p = 0.03$; $z = -2.24$, $p = 0.02$), and that the average return proportions in *RTP* are significantly higher than in *STP* ($t = 3.62$, $p < 0.001$; $z = 3.69$, $p < 0.001$). Overall, the results confirm Hypotheses 1 to 5 regarding the back transfer decisions.

Result 1: The average back transfers for a given investment level are highest in the *RTP* treatment, followed by the *STP* treatments, and then followed by the *BASE* and *CTP* treatments.

4.2 Investments

The OLS estimates in Model 3 of Table 3 show that investments are significantly higher in in *RTP* and significantly lower in *CTP* than in *BASE*, whereas investments in *STP* are not significantly different from investments in *BASE*. T-tests and non-parametric Wilcoxon rank-sum tests (all two-tailed) of differences in mean investments across all rounds, using each investor as one observation, confirm these results and additionally show that investments in *STP* are significantly lower than investments in *RTP* ($t = -2.06$, $p = 0.04$; $z = -2.01$, $p = 0.04$). Again, neither gender nor the education background of the participants shape the effects (see Appendix B).

Result 2: The average investment levels are highest in the *RTP* treatment, followed by the *STP* and *BASE* treatments, and then followed by the *CTP* treatment.

Result 2 rejects Hypothesis 6. The lower investments in *CTP* than in *BASE* indicate that investors feared receiving lower back transfers from an incentivized third party than from the receiver. The fact that investments in *STP* and *BASE* do not significantly differ shows that investors perceived the receivers as equally trustworthy as their appointed third parties without financial incentives.

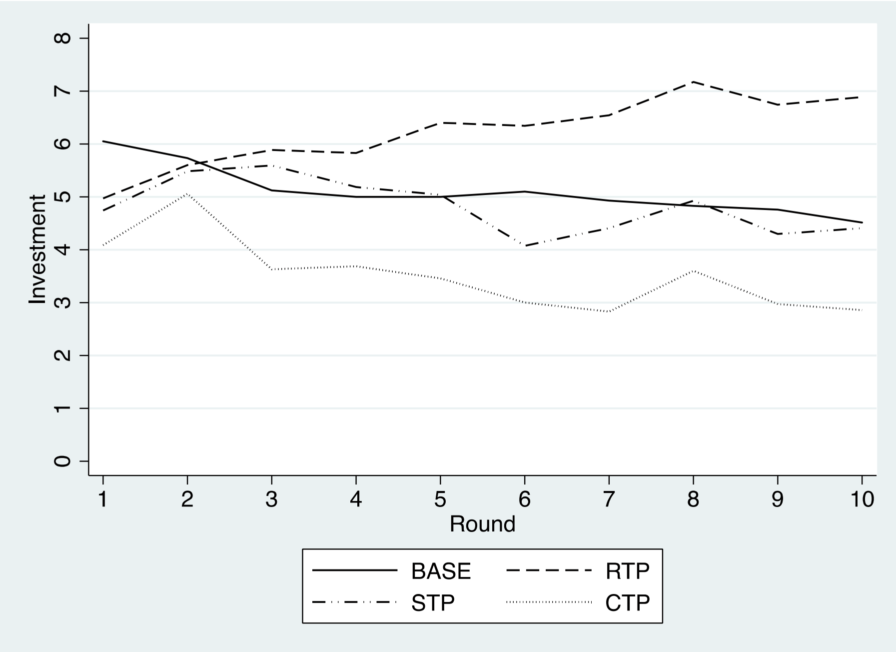

Fig. 1 Mean Investments by Treatment and Round

4.3 Evolution of Trust and Investments

Thus far we have pooled all 10 rounds and analyzed mean investments and return proportions. This section looks at temporal developments. Fig. 1 illustrates the evolution of investments across rounds and treatments. Whereas investments generally decreased over time in the *BASE* treatment, investments tended to increase in the *RTP* treatment. In the treatments in which the third party was selected (*STP, CTP*) investments initially increased but then generally decreased in later rounds.

Table 4 shows the results of t-tests of differences in mean investments between treatments and baseline for each round.[9] In rounds 1–4, the mean investment in the *RTP* treatment was not significantly higher than in the *BASE* treatment. Comparing the mean investments in rounds 5 to 10, we observe much larger and statistically significant differences $\left(BASE: 4.85; RTP: 6.68; t = 2.54, p < 0.05\right)$. In the *STP* treatment, investments were very similar to the investments in the *BASE* treatment. Except in round 1, the difference was never statistically significant. When we compare investments between the *BASE* and *CTP* treatments, investments are always lower in the *CTP* treatment, and the difference is statistically significant in every round except in rounds 2 and 8.

Because investors and receivers were randomly (re-)matched in each round, and because investors did not get any information about the identities of the matched receiver and/or the (selected) third party, reputation effects are ruled out. Neverthe-

[9] The statistical tests are robust to the use of alternative tests like non-parametric Wilcoxon rank-sum tests.

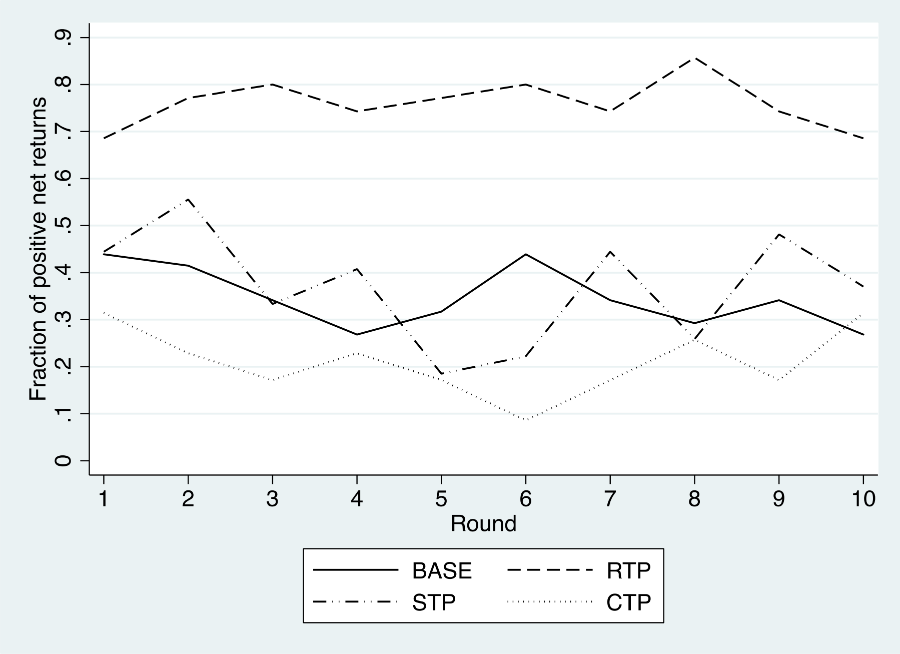

Fig. 2 Fractions of Positive Net Returns per Round and Treatment

less, with a feedback about the investment and the back transfer at the end of each round, investors could still learn about aggregate behavior over time. Learning includes both an improved understanding of the game and updating priors concerning the expected behaviors of the others (Muller et al. 2008). The following section tests whether learning could explain the treatment effects in later rounds.

Fig. 2 shows the fraction of positive net returns per treatment and round. If more than one third of the tripled investment is returned, the investor obtains a positive net return on investment. Whereas the fraction of investors experiencing a positive net return was below 50 percent in the *BASE* treatment in each round, it was always substantially above 50 percent in the *RTP* treatment. In the *STP* and the *CTP* treatments, most investors experienced negative net returns because they had invested more than was returned to them. The fraction of positive net returns remained relatively stable over time.

Table 5 provides an econometric test of learning processes at the individual level. More specifically, we include the fraction of positive net returns an investor experienced in previous rounds as a control variable in our model. The variable has a mean of 0.44 and varies between 0 and 1. A value of 1 indicates that back transfers exceeded investments in all previous rounds. A value of 0 indicates that investments exceeded back transfers or that the investor invested nothing in all previous rounds. As learning is possible only from round 2 onwards, Table 5 excludes first round observations.

Table 4 Mean Investments by Treatment and Round

Round	*BASE* ($n = 41$)	*RTP* ($n = 35$)	t-statistics of difference with *BASE*	*STP* ($n = 27$)	t-statistics of difference with *BASE*	*CTP* ($n = 35$)	t-statistics of difference with *BASE*
1	6.05	4.97	1.43	4.74	1.67*	4.09	3.03***
2	5.73	5.60	0.40	5.48	0.22	5.06	0.83
3	5.12	5.89	0.93	5.59	–0.46	3.63	1.81*
4	5.00	5.83	0.97	5.19	–0.17	3.69	1.66*
5	5.00	6.40	1.61	5.04	0.04	3.46	1.86*
6	5.10	6.34	1.37	4.07	1.15	3.00	2.30**
7	4.93	6.54	1.83*	4.41	0.78	2.83	2.21**
8	4.83	7.17	2.58***	4.93	0.02	3.60	1.39
9	4.76	6.74	2.22**	4.30	0.49	2.97	2.15**
10	4.51	6.89	2.62***	4.41	0.07	2.86	1.88*

Non-parametric Wilcoxon-ranksum tests lead to virtually the same results
Significance levels (two-tailed) are denoted by *** 1 percent, ** 5 percent, * 10 percent

Model 1 in Table 5 is a replication of Model 3 in Table 3, except that this time the first round is not considered. When we control for the *fraction of positive net returns* in previous rounds in Model 2, the positive *RTP* treatment effect becomes small and statistically insignificant. While the size of the negative *CTP* treatment effect also decreases, the effect remains statistically significant at the 10 percent level. The new control variable *fraction of positive net returns* has a large and significantly positive influence on investments. Investors who had experienced only positive net returns in previous rounds invested 4.2 Euros more than investors who had experienced only negative net returns in previous rounds. Model 3 additionally includes interaction terms of the treatment dummies with the control variable *fraction of positive net returns.* Table 5 shows that the interaction effect of the fraction of positive net returns and *CTP* is significantly negative, implying that investors in the *CTP* treatment react more cautiously to positive net returns from previous rounds.

Result 3: Investors increase investments only if they obtained positive net returns on their investments in previous rounds.

Result 3 implies that third-party independence is a not a sufficient condition for encouraging investments. An increase in investments also requires repeated interactions under the same institution (but not necessarily the same person) and the experience of positive net returns on investment. Even with the stranger-matching protocol, investors update their back transfer beliefs according to the net returns in previous rounds. As the return proportions and thus the fractions of investors experiencing a positive net return remained relatively stable over time within a treatment (see Fig. 2), investments increased over time in the *RTP* treatment, in which back transfers were higher than investments on average. In the other treatments, in which back transfers were lower than investments on average, investments decreased over time.

Table 5 Evolution of Trust and Investments

Dependent variable Sample	Investments Rounds 2 to 10		
	(1)	(2)	(3)
BASE	Ref. group	Ref. group	Ref. group
RTP	1.380**	–0.164	–0.211
	(0.579)	(0.343)	(0.775)
STP	–0.174	–0.282	–0.127
	(0.566)	(0.510)	(0.733)
CTP	–1.543**	–0.910*	–0.420
	(0.552)	(0.456)	(0.348)
Fraction of positive net returns (up to t-1)		4.214***	4.766***
		(0.509)	(0.131)
Fraction of positive net returns * *RTP*			–0.207
			(1.254)
Fraction of positive net returns * *STP*			–0.416
			(1.083)
Fraction of positive net returns * *CTP*			–1.762**
			(0.669)
Constant	4.997***	3.390***	3.179***
	(0.530)	(0.353)	(0.567)
# Observations	1,242	1,242	1,242
# Subjects	138	138	138
R^2	0.082	0.207	0.211

RTP, STP and *CTP* are treatment dummies, *Fraction of positive net returns* is the share of previous periods in which the back transfer was higher than the investment. Table displays OLS coefficients with White robust standard errors clustered at the session level in parentheses

Significance levels are denoted by *** 1 percent, ** 5 percent, * 10 percent (two-tailed tests)

4.4 Payoffs

Table 6 reports the results with the investor's and receiver's payoff as dependent variables and treatment dummies as independent variables. For each dependent variable we also estimate a specification with initial investments as a control variable, because investments determine the created value that can be divided between the investor and the receiver.

The investor's payoff was significantly higher when the return decision was delegated to a randomly assigned third party with a fixed payment (*RTP*). While the payoff was slightly higher if the return decision was delegated to a selected third party with a fixed payment (*STP*) and slightly lower if the return decision was delegated to a third party whose payment increased with the number of selections (*CTP*), the latter two effects are not significantly different from 0. Model 2 shows that an additional point of investment increased the investor's payoff by 0.216.

Models 3 and 4 in Table 6 compare the receiver's payoff across treatments. Model 3 shows that the receiver's payoff was considerably lower in all three third-party treatments. The payoff reduction in comparison to the *BASE* treatment was

Table 6 Third-Party Influence on Payoffs

Dependent variable	Investor's payoff		Receiver's payoff	
	(1)	(2)	(3)	(4)
BASE	Ref. group	Ref. group	Ref. group	Ref. group
RTP	4.700***	4.455***	–2.430***	–4.455***
	(0.532)	(0.475)	(0.741)	(0.475)
STP	0.699	0.761	–1.274	–0.761
	(0.490)	(0.493)	(1.134)	(0.493)
CTP	–0.649	–0.307	–2.522***	0.307
	(0.447)	(0.433)	(0.790)	(0.433)
Investments		0.216*		1.784***
		(0.111)		(0.111)
Constant	10.098***	8.997***	20.107***	11.003***
	(0.284)	(0.556)	(0.673)	(0.556)
Number of observations	1,380	1,380	1,380	1,380
Number of subjects	138	138	138	138
R^2	0.209	0.235	0.021	0.702

RTP, STP and *CTP* are treatment dummies. Table displays OLS coefficients with White robust standard errors clustered at the session level in parentheses

Significance levels are denoted by *** 1 percent, ** 5 percent, * 10 percent (two-tailed tests)

large and statistically significant in the *RTP* and *CTP* treatments and smaller and statistically insignificant in the *STP* treatment. However, the explanations for the lower payoffs differ. In the *RTP* and *STP* treatments, the payoff was lower because third parties returned more money, whereas in the *CTP* treatment the payoff was lower due to lower investments. The *CTP* coefficient becomes very small and statistically insignificant when we control for investments in Model 4.

Result 4: The investor obtains a significantly higher payoff if the back transfer decision is delegated to a randomly assigned third party with a fixed payment.

Result 5: The receiver obtains a lower payoff if the return decision is delegated to a third party, either due to higher relative back transfers or due to lower initial investments.

Result 5 suggests that receivers would not voluntarily delegate ownership rights to a third party. Delegating reward decisions is not profitable for the receiver.

5 Discussion

The transfer of residual ownership rights to an independent third party has been a prominent recommendation for promoting firm-specific investments (Rajan and Zingales 1998; Blair and Stout 1999; Lan and Heracleous 2010; Franck 2011). However, empirical evidence on the effect of third-party ownership on specific investments has been missing so far. The experimental evidence presented in this paper reveals that the selection and payment procedure of the third party strongly moderates the effect on investments. Compared to a standard investment game without third parties (Berg et al. 1995), average investments were significantly higher if a randomly assigned third party with a fixed payment, rather than the receiver, decided on the back transfer. A more detailed analysis of investments revealed that the independent third parties induced higher investments only after a few rounds. Even though reputation building was ruled out due to the stranger-matching protocol, investors had to experience positive returns on investments before they increased their investments. Controlling for the fraction of positive net returns in previous rounds largely eliminates the treatment effects.

Transferring the back transfer decision to a third party who benefits from being selected by the receiver significantly decreased investments even though such delegation did not decrease actual return proportions. Fear about lower back transfers due to a diffusion of responsibility may explain our finding. Fershtman and Gneezy (2001) and Hamman et al. (2010) find that delegation increases selfishness in ultimatum and dictator games if the agent is selected by the principal and incentivized to act in favor of the principal. Hamman et al. (2010, 1826) conclude that "[t]hrough the use of agents, [...] accountability for morally questionable behavior can become vertically diffused." By selecting a specific third party, principals hire the most appropriate agent to act on their behalf while the agent possibly feels that he or she is merely carrying out orders. Fershtman and Gneezy (2001) and Hamman et al. (2010) show that the agent is thus more likely to make selfish decisions on the principal's behalf than the principal himself or herself would do. While we did not observe such diffusion of responsibility between the receiver and the third party, we still found that investors feared to receive less in the treatment with an incentivized third party than in the baseline treatment.

This paper shows that randomly assigned third parties with no financial stakes in the distribution decision induced the highest investments. In business, random selection has been less accepted thus far. However, random selection procedures was an important element of demarchy, a form of political governance used in ancient Athens and in the medieval republics of Northern Italy. Zeitoun et al. (2014) therefore suggest implementing random selection procedures also in modern corporate governance. In particular, Zeitoun et al. (2014) propose to structure the board of directors in two chambers. The first chamber is composed of shareholder representatives who are elected by the stockholder meeting as it is the case today. The second chamber, however, is composed of representatives of other specific investors such as employees with firm-specific knowledge or suppliers providing specific inputs, drawn by lot. While the results of our paper confirm the benefit of random selection procedures, they also show that the randomly selected persons have to be financially independent to credibly act as third-party trustees who mediate between the conflicting interests of the specific investors. Thus, unlike suggested by Zeitoun et al. (2014), we recommend drawing third-party trustees by lot among qualified fiduciaries who have no financial stakes in the firm themselves.

The results of this paper also indicate that investments decrease when third parties are selected and not drawn by lot. Third-party selection creates inefficiencies because first, any appointment committee could select people who are ready to return a favor, and second, even if this bias was not involved, outside investors might consider the selected third party to be biased against them. If random third-party assignment is not feasible, reputation-building activities such as fostering repeated interactions between the same partners and/or informing investors about past behavior (Berg et al. 1995; Bohnet and Huck 2004) could serve as alternative strategies for increasing specific investments.

Appendix A

General Instructions for the Participants in the Treatment BASE

This is an English translation of the German instructions of the baseline treatment BASE. We integrated control questions about the experiment into the z-tree file.

We would like to welcome you to this economic experiment.

Your decisions and if applicable the decisions of the other participants in this experiment can influence your payment. It is important that you carefully read these instructions. If you have any questions, please ask **before** the experiment starts. All participants receive the same instructions.

During the experiment it is not allowed to talk with other participants. Disregard of this rule will lead to exclusion from the experiment and the payment.

During the experiment we do not talk about Euros. We talk about points instead. Your payment will be first calculated in points. The total number of points you will achieve in this experiment will be converted into Euros at the end with a conversion rate of:

$$1\,\text{point} = 1\,\text{Euro}$$

We will pay out the payment in cash at the end of today's experiment. On the following pages we explain the detailed procedure of this experiment.

Structure of the Experiment

In this experiment you are always a group of two. In this pairing there is always a *participant A* and a *participant B*. At the beginning of the experiment the computer randomly determines if you are a participant A or B. You will keep the same role during the whole experiment.

The experiment lasts for ten rounds. *In each round a new pairing will be formed at random***.** We explain the procedure of one round. All ten rounds have the same procedure. You will be paid according to the points achieved in a randomly chosen round.

Participant A and participant B are endowed with 10 points. Participant A can send between 0 and 10 points to participant B. The amount sent is tripled by the experimenter and given to B. Participant B can now decide how many of the received points to return back to participant A. This back transfer is not tripled.

The participants will receive the following payment, if the computer determines this round for the payment:

- Participant A: 10 points – amount sent by participant A + back transfer,
- Participant B: 10 points + 3 × (amount sent by participant A) – back transfer.

At the end of a round the participants will be informed about their points earned in that round.

Sequence of Decisions

A round proceeds on the screen as follows. Firstly, participant A decides on the transfer to B by entering a number between 0 and 10 and reports his belief about the expected back transfer. Parallel participant B reports his belief concerning the amount sent by participant A.

Participant B then learns how many points A has sent and how many points B accordingly has received. Then participant B decides on the back transfer by entering the corresponding amount.

General Instructions for the Participants in the Treatment RTP

This is an English translation of the German instructions of the treatment RTP. We integrated control questions about the experiment into the z-tree file.

Structure of the Experiment

In this experiment you are always a group of three. In this triad there is always a *participant A*, a *participant B* and a *participant C*. At the beginning of the experiment the computer randomly determines if you are a participant A, B or C. You will keep the same role during the whole experiment.

The experiment lasts for ten rounds. *In each round the triad will be newly formed at random*. We explain the procedure of one round. All ten rounds have the same procedure. You will be paid according to the points achieved in a randomly chosen round.

Participant A and participant B are endowed with 10 points. Participant A can send between 0 and 10 points to participant B. The amount sent is tripled by the experimenter and given to B. Participant C can now decide how many of the received points to return back to participant A. This back transfer is not tripled. C cannot return more than B received from A. The 10 points that B received from the experimenter remain with B in any case. *Participant C receives 10 points from the experimenter independent of her decision.*

The participants will receive the following payment, if the computer determines this round for the payment:

- Participant A: 10 points – amount sent by participant A + back transfer,
- Participant B: 10 points + 3 × (amount sent by participant A) – back transfer,
- Participant C: 10 points.

At the end of a round the participants will be informed about their points earned in that round.

Sequence of Decisions

A round proceeds on the screen as follows. Firstly, participant A decides on the transfer to B by entering a number between 0 and 10 and reports his belief about the expected back transfer. Parallel participant B reports his belief concerning the amount sent by participant A.

Participant C then learns how many points A has sent and how many points B accordingly has received. Then C decides on the back transfer by entering the corresponding amount. Parallel participant B reports his belief concerning the amount sent by participant C.

General Instructions for the Participants in the Treatment STP

This is an English translation of the German instructions of treatment STP. We integrated control questions about the experiment into the z-tree file.

Structure of the Experiment

In this experiment you are always a group of three. In this triad there is always a participant A, a participant B and a participant C. At the beginning of the experiment the computer randomly determines if you are a participant A, B or C. You will keep the same role during the whole experiment.

The experiment lasts for ten rounds. We explain the procedure of one round. All ten rounds have the same procedure. You will be paid according to the points achieved in a randomly chosen round.

Participant A and participant B are endowed with 10 points. Participant A can send between 0 and 10 points to participant B. The amount sent is tripled by the experimenter and given to B. A *from B selected participant C* can now decide how many of the received points to return back to participant A. This back transfer is not tripled. C cannot return more than B received from A. The 10 points that B received from the experimenter remain with B in any case.

Participants A and B are randomly matched in each round. Participant B selects a participant C in each round. The selection procedure runs as follows: All participants C tell the participants B, what *percentage* of the transferred money they want to remain with B. At this stage each participant C gets a number, which clearly identifies her over all rounds, without compromising her anonymity. Afterwards each participant B selects a participant C. Note that several participants B can simultaneously select a player C. The announcement of participant C concerning the back transfer is not binding. Thus, a selected participant C can reconsider her decision regarding the back transfer. *Participant C receives 10 points from the experimenter independent of her decision.*

The participants will receive the following payment, if the computer determines this round for the payment:

- Participant A: 10 points – amount sent by participant A + back transfer,
- Participant B: 10 points + 3 × (amount sent by participant A) – back transfer,
- Participant C: 10 points.

At the end of a round the participants will be informed about their points earned in that round.

Sequence of Decisions

A round proceeds on the screen as follows. Firstly, all participants C inform the participants B about the percentage of the transferred money that should remain with B. Secondly, each participant B selects a participant C. Simultaneously participant A decides on the transfer to B by entering a number between 0 and 10 and reports his belief concerning the expected back transfer.

The selected participant C then learns how many points A has sent and how many points B accordingly has received. Then the selected participant C decides on the back transfer by entering the corresponding amount. If a participant C has to make several decisions, they appear simultaneously on the screen in an arbitrary order.

General Instructions for the Participants in the Treatment CTP

This is an English translation of the German instructions of the treatment CTP. We integrated control questions about the experiment into the z-tree file.

Structure of the Experiment

In this experiment you are always a group of three. In this triad there is always a *participant A*, a *participant B* and a *participant C*. At the beginning of the experiment the computer randomly determines if you are a participant A, B or C. You will keep the same role during the whole experiment.

The experiment lasts for ten rounds. We explain the procedure of one round. All ten rounds have the same procedure. You will be paid according to the points achieved in a randomly chosen round.

Participant A and participant B are endowed with 10 points. Participant A can send between 0 and 10 points to participant B. The amount sent is tripled by the experimenter and given to B. A *participant C* who has been selected by player B can now decide how many of the received points to return back to participant A. This back transfer is not tripled. C cannot return more than B received from A. The 10 points that B received from the experimenter remain with B in any case.

Participants A and B are randomly matched in each round. Participant B selects a participant C in each round. The selection procedure runs as follows: All participants C tell the participants B, what *percentage* of the transferred money they want to remain with B. At this stage each participant C gets a number, which clearly identifies her over all rounds without compromising her anonymity. Afterwards each participant B selects a participant C. Note that several participants B can simultaneously select a player C. The announcement of participant C concerning the back transfer is not binding. Thus, a selected participant C can reconsider her decision regarding the back transfer.

C's salary, which the experimenter pays, is independent from her own decision regarding the back transfers and depends only on the number of participants B selecting that participant C. If no participant B selects a specific participant C, this participant C receives 5 points. For every selection by a player B she gets additional 5 points. That means, if three participants B select a participant C in a round, she receives 20 points. The participants will receive the following payment, if the computer determines this round for the payment:

- Participant A: 10 points – amount sent by participant A + back transfer,
- Participant B: 10 points + 3 × (amount sent by participant A) – back transfer,
- Participant C: 5 points + 5 points * number of B's selecting that C.

At the end of a round the participants will be informed about their points earned in that round.

Sequence of Decisions

A round proceeds on the screen as follows. Firstly, all participants C inform the participants B about the percentage of the transferred money that should remain with B. Secondly, each participant B selects a participant C. Simultaneously participant A decides on the transfer to B by entering a number between 0 and 10 and reports his belief concerning the expected back transfer.

The selected participant C then learns how many points A has sent and how many points B accordingly has received. Then the selected participant C decides on the back transfer by entering the corresponding amount. If a participant C has to make several decisions, they appear simultaneously on the screen in an arbitrary order.

Appendix B

Table B.1 Third-Party Influence on Proportions Returned and Investments When Controlling for Gender and Education Background

Dependent variable	Proportions returned		Investments	
Sample	Obs. with investment > 0		All obs.	
	(1a)	(1b)	(2a)	(2b)
BASE	Ref. group	Ref. group	Ref. group	Ref. group
RTP	0.255***	0.262***	1.127*	1.050
	(0.024)	(0.021)	(0.529)	(0.535)
STP	0.069**	0.073**	–0.281	–0.318
	(0.036)	(0.031)	(0.553)	(0.656)
CTP	–0.029	–0.020	–1.585***	–1.667***
	(0.044)	(0.044)	(0.509)	(0.511)
Investments	0.005	0.005		
	(0.003)	(0.003)		
Male	–0.009	–0.017	0.145	0.187
	(0.022)	(0.020)	(–0.435)	(0.379)
Humanities		Ref. group		Ref. group
Science/Engineering		0.024		–0.334
		(0.034)		(0.651)
Law		–0.008		–0.861
		(0.033)		(1.221)
Social Sciences		0.065**		–0.632
		(0.041)		(0.562)
Economics		0.011		–0.388
		(0.019)		(0.562)
Others		0.064		–0.843
		(0.076)		(0.910)
Constant	0.297***	0.275***	5.032***	5.426***
	(0.032)	(0.030)	(0.474)	(0.607)
Number of observations	1,102	1,102	1,380	1,380
Number of subjects	138	138	138	138
R^2	0.222	0.233	0.076	0.082

RTP, STP and *CTP* are treatment dummies. Table displays OLS coefficients with White robust standard errors clustered at the session level in parentheses

Significance levels are denoted by *** 1 percent, ** 5 percent, * 10 percent (two-tailed tests)

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