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Trust and Reciprocity, Empowerment and Transparency

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Abstract

In a laboratory-controlled environment characterized by uncertainty and incomplete information we provide experimental evidence on the effects of transparency and empowerment on trust (investment by a principal) and trustworthiness (reciprocal behavior of an agent) in a simple two-person investment game. We find that when principals are empowered by being able to punish agents who may not act in a way the principal believes is in the principal's best interest, trust and investment increases over that which is realized in the absence of empowerment. We also find that when asymmetric or incomplete information characterizes the investment game the levels of trust (investment) are lower than when information is complete (the environment is transparent). In transparent environments the effect of empowerment is about the same regardless of whether empowerment is introduced or removed. However, in opaque environments, the loss of empowerment has a substantially greater negative effect on trust that the positive effect associated with the introduction of empowerment.

While this environment is substantially abstracted from the naturally occurring environment, these results suggest that practical public policies designed to increase transparency in financial transactions are likely to have positive effects on investment. Furthermore, public policies designed to empower principals, such as the Say on Pay practices, are likely to increase investment while the limitation of the empowerment of principals with respect to their agents (consistent with deregulation) will have a much more dramatic negative impact on trust (and investment).

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1. Introduction

During the last two decades, the phenomenon of arguably unwarranted exorbitant compensation packages of corporate chief executive officers (CEOs) relative to the firm performance, coupled with the issue of ever recurring corporate scandals (e.g., Enron, WorldCom, Global Crossing, Countrywide Financial, etc.) have resulted in a significant loss of the shareholders confidence and trust in the integrity of corporate managers. This has created public perception that CEOs may use their power to exploit their firms' resources to maximize their own self-interest at the expense of their shareholders. For example, in 2007, the CEO of Countrywide was paid $103 million, while shareholders suffered an 80% decline in share value (Morgenson, 2010).

Arrow (1974) shows that trust plays an essential role in advancing economic growth and promoting business activities. Trust involves economic interaction between at least two parties who need to collaborate (cooperate) to make the most of business opportunities, where one party (the trustor) must put some economic resources at risk with a person (the trustee) deemed trustworthy who uses his expertise to take actions with the objective of maximizing the joint wealth of both parties.[1] When a trustor enters into such a relationship, she must have formed certain *expectations* about the potential actions of the trustee and the likely related

[1] Zand (1972) defines trust as the willingness of one person to increase her vulnerability to the actions of another person whose behavior cannot be controlled.

outcomes. These expectations reflect the degree of confidence in the integrity of the trustee and the likelihood that these expectations will be fulfilled.

Recent changes in regulations are aimed at addressing the excessive CEO compensation and restoring trust in investors through empowering investors with the ability to veto excessive CEO compensation and automatic compensation claw backs for firms that misreport earnings. Recent regulations such as the Dodd-Frank Financial Reform Act of 2010 aims at rectifying the balance of power between shareholders and CEOs by introducing stronger provisions for shareholders' to have input on CEO compensation (Say-on-Pay proposals) and permit automatic claw backs in CEO compensation for poorly performing firms or firms that misreport.

This paper presents the results of a laboratory-controlled experiment designed to study the effects of the introduction of an empowerment mechanism comparable to a binding Say-on-Pay practice into a simple investment environment. Treatments include the presence or absence of an empowerment mechanism and whether or not information is transparent. The design permits us to study both the role of learning on the effect of the costless veto and the effects of removing empowerment on the behavior of investors and their agents. Our results suggest that the loss of empowerment (similar to the removal of a Say-on-Pay practice) may lead to a precipitous drop in trust (and investment). While this environment is highly abstracted from the naturally occurring environment, it provides evidence that the loss of trust in the actions of CEO's may lead to substantial reductions in investment in their enterprises. This may have significant ramifications for

depressed economies suffering from lost trust in the leaders of their productive enterprises.

2. The Literature on Say on Pay and Claw Backs

Although CEOs' compensation packages are determined by their Boards of Directors, previous studies provide strong evidence suggesting that CEOs have significant power to influence their own remuneration and extract economic rent by managing their Board members and their compensation decisions (Elhagrasey et al., 1999). Research on the process of determining the CEOs' compensation packages contends that the periodic renegotiation over the compensation packages with the Boards of Directors is not usually done at arm's-length (e.g., Bebchuk and Fried, 2004; Mack, 2008).

The above arguments suggest that CEOs often fail to fulfill their fiduciary and moral responsibilities, which oblige them to place the interest of the shareholders before their own interests. The natural consequence of this phenomenon is that the general public's *trust* in corporate managers and their professional ethics has suffered significant erosion. During the past ten years a significant number of investors withdrew their investments from stock markets and directed them into opportunities not reliant on an agent who must be trusted. This, in turn, may have significantly and adversely affected both the rate of economic growth and the speed of recovery from the recent financial crises and the economic downturn.

The goal of regulations such as the Dodd-Frank Financial Reform Act of 2010 is to empower shareholders so as to defend themselves against excessive

compensation by powerful CEOs and to build upon the Sarbanes-Oxley Act of 2002. In fact, shareholders have already started signaling a growing willingness to use their new powers as evidenced by Say-on-Pay proposals at recent Annual General Meetings of Citigroup, Best Buy (www.say-on-pay.com) and J. P. Morgan. At recent J. P. Morgan meeting, shareholders rejected management's pay structure proposal in a non-binding vote. This vote has resulted in J. P. Morgan's Board working to bring its executives' pay structure in line with the shareholder proposals.

While the early claw-back provisions are adopted voluntarily, they are based in, and potentially have implications for, regulatory initiatives of the U.S. government (Denis, 2012). Section 304 of the Sarbanes–Oxley act (SOX), adopted in 2002, authorizes the Securities and Exchange Commission to recover bonuses paid to CEOs or CFOs whose financial statements are restated for reasons of material noncompliance with any financial reporting requirements. Moreover, Section 954 of the 2010 Dodd–Frank Wall Street Reform and Consumer Protection Act, signed in 2010 and scheduled to take effect in 2012, also provides for the recovery of awarded compensation from executives deemed erroneous ex post.

Chan et al. (2012) find that voluntary adoption of compensation claw-back provisions under SOX regulations is followed by fewer financial restatements and fewer auditor reports of material internal control weaknesses, higher earnings response coefficients, and reduced auditing fees and lags. They conclude that voluntary adoption of claw-back provisions leads to increased financial integrity. Based on these findings Chan et al. (2012) suggest that U.S. government mandated

claw-back provisions would be effective in reducing material financial misstatements.

In contrast to compensation claw backs, the Say-on-Pay provisions have been more controversial. Mangen and Magnan (2012) debate whether Say on Pay can solve executive excessive pay. They argue that Say on Pay curtails executive pay when shareholders' concerns offset CEO power and mitigates directors' information deficiencies. They also caution that Say on Pay may raise novel problems. The pay resulting from Say on Pay can harm stakeholders whose interests differ from those of shareholders influential in pay setting. Moreover, boards may resist shareholders' intervention in pay setting and, accordingly, manage compensation disclosures to ensure a passing shareholder vote. Consequently, Say on Pay may not only fail to remedy excessive CEO pay but also legitimize it (Mangen and Magnan, 2012).

Early evidence on the benefits of Say on Pay regulations is mixed. Cai and Walkling (2011) document that when the U.S. Congress passed the Say on Pay Bill, the market reaction was significantly positive for firms with high abnormal CEO compensation, with low pay-for-performance sensitivity, and responsive to shareholder pressure. However, Cai and Walkling (2011) point out that activist sponsored Say on Pay proposals target large firms, not those with excessive CEO pay, poor governance, or poor performance. The market reacts negatively to labor sponsored proposal announcements and positively when these proposals are defeated. Their findings suggest that Say on Pay creates value for companies with inefficient compensation, but can destroy value for others.

The international evidence from United Kingdom (UK) is more supportive of Say on Pay regulations. Ferri and Maber (2012) examine the effect of Say on Pay regulation in the UK. Consistent with the view that shareholders regard Say on Pay as a value-creating mechanism, Ferri and Maber (2012) find that the regulation's announcement triggered a positive stock price reaction at firms with weak penalties for poor performance. UK firms responded to negative Say on Pay voting outcomes by removing controversial CEO pay practices criticized as rewards for failure (e.g., generous severance contracts) and increasing the sensitivity of pay to poor realizations of performance.

In summary, the evidence on the benefits on Say on Pay practices appears inconclusive. In particular, while Say on Pay has the potential to curtail excessive compensation, it can also target well-governed large firms thus open to abuse. Consequently, a careful examination of an instrument comparable to a Say-on-Pay practice in a controlled laboratory setting may provide insights about Say-on-Pay practices in the industry and possibly lead to a refinement of existing policy and regulations.

3. An Experiment to Address Say-on-Pay Practices: Theoretical Development, Hypotheses and Related Literature

Kanagaretnam et al. (2010, 2012) design and implement controlled laboratory settings to test the effects of reputation building and empowerment on participants in an investment game comparable to that in Berg et al. (1995). The laboratory environments that permit investors (comparable to the trustors described earlier)

to veto decisions made by investees (comparable to the trustees described earlier) utilize tools closely related to empowering investors with tools such as the Say-on-Pay practices to control their agents.[2] They provide empirical evidence suggesting that building *reputation* through repeated period interaction with the same partner and empowering the investor to *punish* her agent (the investee) for betraying trust are two key ingredients in building trust. The objective of this study is to examine the effects on trust and reciprocity in an abstract investment environment in the presence of asymmetric information, repeated interactions, and the presence of a mechanism that allows the investor to punish the investee if her trust is abused or betrayed. This work extends the earlier work of Kanagaretnam et al. (2010, 2012) by permitting interactions between empowerment and transparency. The following sections provide the foundations for this experiment.

3.1. Information[3]

Under certain information in the investment game, both the investor (sender) and the investee (receiver, responder) know each other's initial endowments and the investment multiplier (technology). There is no opportunity for the investor to hide under a *veil of uncertainty*. The amount invested signals her trust and the investee gets an unambiguous signal. When information is uncertain the participants' initial endowments are randomly picked from a uniform distribution known by both the investor and the investee, where the expected value of the endowment equals the

[2] Kanagaretnam et al. (2010, 2012) use an instrument that is binding on the trustee. Say-on-Pay votes are non-binding in nature. However, two majority-vote rejections may constitute grounds for breach of fiduciary duties on part of senior management in a litigation context.

[3] Much of the material in this section draws on Kanagaretnam et al. (2010).

certain endowment in the full-information condition. This modification allows us to use the unobservability of each participant's endowment by the person with whom she is paired to characterize the uncertain-information environment.

Anderhub et al. (2002), Brandts and Figueras (2003), Bohnet and Huck (2003), Coricelli et al. (2006), Cox and Deck (2006) all introduce uncertain information into an investment game or similar environment and demonstrate that the information treatment is important. However, none directly compares the differences in trust and reciprocity between environments in which the certain and uncertain information is related to the endowments of the participants (particularly of the investor). Kanagaretnam et al. (2010) show that in one-shot games, uncertain information (the lack of transparency) will lead to lower levels of trust and lower levels of reciprocity than will be realized if information is certain.

Most investment activities and business transactions are conducted on an ongoing basis rather than as one time encounters. In a repeated interaction environment, one's reputation may be an effective a priori control on ex-ante opportunism. We conjecture that in a repeated multi-period investment game, subjects may attempt to create incentives that induce the other party to cooperate (which is how we have defined *building reputation*). Sending credible signals to their counterparts is likely to influence them to adopt strategies that enhance cooperation and lead to Pareto-superior outcomes (see Kreps et al., 1982; Fudenberg and Maskin, 1986; Fudenberg and Levine, 1992; Eckel and Wilson, 2003). However, even if the repeated game is capable of inducing cooperation, it is

uncertain that it will be sufficient to offset any effects on trust or reciprocity that may be realized because of the lack of transparency.

Kanagaretnam et al. (2010) show that although repeated play results in increased trust with certain and uncertain information, differences in trust that arise in a one-shot investment game because of the lack of transparency are not offset with the introduction of four rounds of repeated play in an investment game. Repeated play also results in an increase in reciprocity with both certain and uncertain information, however, the differences observed in reciprocity in one-shot investment games disappear with the introduction of repeated play.

3.2. Empowerment[4]

In the two-person investment game, trust is constrained by the uncertainty involved in investing a positive amount that may or may not be reciprocated by the trustee. In particular, trust is an action taken by a party to an economic transaction with the anticipation that the other party of the transaction will not behave opportunistically, i.e., will not exploit the vulnerability that the party has created for himself or herself by taking the action with an uncertain outcome (James, 2002; Bohnet et al., 2008). This is especially so in a one-shot investment relationship where there is no opportunity for the investor to retaliate or for the entrepreneur to build reputational capital. In this environment, the individual investors place trust in the entrepreneurs to manage the new ventures in the collective interest of all parties, and expect that managers will behave in a cooperative and non-exploitative ways (Cook and Cooper, 2003). However, in the absence of an effective monitoring

[4] Much of the material in this section draws on Kanagaretnam et al. (2012).

mechanism in these incomplete contractual relationships, investors may hesitate to fully invest in these projects due to the fear of being betrayed by the entrepreneur. Kanagaretnam et al. (2012) conjecture that in these environments empowering investors with the right to punish opportunistic entrepreneurs is likely to reduce the betrayal aversion behavior and hence increase the levels of trust (investment) and trustworthiness (return to investment).

One way of introducing empowerment into the investment game environment is to move from a one-shot game to a repeated game. In a repeated interaction environment, investors can retaliate by reducing their future investments in response to a low level of reciprocity. In this case, one's reputation may be an effective a priori control on ex-ante opportunism. In a repeated multi-period investment game, subjects may attempt to create incentives that induce the other party to cooperate. Sending credible signals to their counterparts is likely to influence them to adopt strategies that enhance cooperation and lead to Pareto-superior outcomes (see Kreps et al., 1982; Fudenberg and Levine, 1992; Eckel and Wilson, 2003). This reputation building mechanism (the repeated interaction), is expected to encourage the investor to trust more (Engel-Warnick and Slonim, 2004) in order to influence her agent (the investee) to acknowledge the increased trust with greater returns (increased reciprocity).

A more direct and more effective way of empowering investors is to permit them the opportunity to exhibit their objection to what is returned to them in the investment game by vetoing the response and voiding the contract. This veto could be costly to only the investee (costless veto) or to both the investee and the investor

(costly veto).[5] Kanagaretnam et al. (2012) implement these mechanisms in a laboratory investment game environment with certain information and find that both costly and costless vetoes significantly increase trust. They argue that fear of retaliation by investors may increase the propensity to reciprocate by the investees, i.e., with the investor acquiring an ability to punish, investees may be more likely to return a greater portion of the grossed-up investment. Therefore, the level of reciprocity under a costly veto may be greater than the level of reciprocity under repeated games. As the cost of punishment falls, the investee may expect the investor will be even more likely to veto an unacceptable return. Therefore, the level of reciprocity under a costless veto may be greater than the level of reciprocity under a costly veto. Generally, the laboratory sessions reported by Kanagaretnam et al. (2012) support this result.

3.3. The Interaction of Empowerment and Information

Introducing uncertain information into a repeated investment game environment results in significantly lower levels of trust than in a comparable environment with transparency (Kanagaretnam et al., 2010). This leads to a smaller social surplus. Introducing costly or costless vetoes into a repeated investment game environment with certain information leads to increased trust (Kanagaretnam et al., 2012). The same forces that lead to increased trust with empowerment with complete

[5] Costly vetoes are possible in the presence of inequity aversion. According to Fehr and Schmidt (1999) inequity aversion means that people resist inequitable outcomes; i.e., they are willing to give up some material payoff to move in the direction of more equitable outcomes. Fehr et al. (1997), Fehr and Gächter (2000) Andreoni (2005) and Rigdon (2009) have introduced punishment mechanisms in public good, gift exchange and investment game environments.

information should lead to increased trust when investors are empowered in an environment with incomplete information.

Of particular interest is whether empowerment has a sufficiently large effect on trust that the difference between trust in the repeated game with and without complete information is reduced. Because the naturally occurring environment is generally characterized by incomplete information, this would provide added support for Say-on-Pay regulations than is provided by example using complete information environments. On the other hand, a widening of the gap might reduce the support for Say-on-Pay legislation.

4. Experimental Design

A total of 182 subjects were recruited from undergraduate classes at a medium-sized university. During the recruitment phase, the students were told that the sessions in which they would participate would involve simple decision-making, and that the details would be given to them during the session. They were also informed that during the course of the session they would earn money that would be paid to them in cash at the conclusion of the session.

4.1. The Investment Game

Each individual participated in a session consisting of a series of twenty or twenty-two periods of a computer-mediated investment game. In this paper we are reporting the results from the last eighteen periods (last three of four phases) of

these sessions.[6] There were two information conditions (certain and uncertain) and two sequences (forward and backwards). Participants were included in only one of four combinations of information treatment and sequence.

In the first eight periods of the forward sequence (both with certain and uncertain information), the second phase of the session, the participants played a repeated investment game with anonymous partners. In the next six periods, phase three of the session, participants played a repeated investment game and investors had the opportunity to exercise a costly veto of the investee's response. In the last phase, the final four periods, participants played a repeated investment game and investors had the opportunity to exercise a costless veto of the investee's response. Participants knew that there would be different phases in the session and that the rules of the game in each phase may be different from the other phases. Participants did not know how the rules would be adjusted from phase to phase and they did not know the number of rounds in each phase.

In the backwards sequence (both with certain and uncertain information), the repeated game with the costless veto, the repeated game with the costly veto and the repeated game with no veto were played in the first four, next six and final eight periods (the second, third and fourth phases of the session).

The following describes the way a forward-sequence certain-information session was conducted, beginning with the first period of the first phase. In the first period, half of the participants played the role of the investor (sender). The

[6] In the first phase of the sessions the 182 participants played a one-shot investment game with anonymous partners. Information was certain. The results of this phase are reported in Kanagaretnam et al. (2009).

participants were reassigned to different partners for the second period, during which the people who were investors in the first period took the role of investee (responder). At the start of the third period, the beginning of the second phase, participants were reassigned to new partners, their roles are reversed, and they are told that they will be playing the same game repeatedly for an unknown number of periods. After four periods, a new assignment of partners was made and the roles were reversed. Again, the participants were told that the game would be repeated for an unknown number of periods. The phase was ended after four periods and a new assignment of partners was made and the costly-veto treatment was introduced. The process followed for the no-veto treatment was repeated and after six rounds participants were reassigned and the costless-veto treatment began. The backwards sequence sessions followed a similar pattern, only they began with a one-shot game and proceeded to the costless-veto treatment for four rounds, the costly-veto treatment for six rounds and finished with the no-veto treatment for eight rounds.

At the beginning of the session each subject was assigned an ID (i.e., their experimental identities) by drawing an index card from a set of shuffled cards numbered from 1 to N, where N is the total number of the students attending the session. Students were told that the ID number is private information and that they should not show it to or share it with any one. Individuals were then told to select a computer workstation at which they would remain for the duration of the session. After the participants were seated, the instructions were shown on the screens of

the participants' monitors and read aloud to them by the experimenters. Subjects were given an opportunity to ask questions for clarification.

In addition to the veto characteristic of the game, information was also a treatment variable. The endowment that senders and receivers had each round was either fixed at 100 laboratory euros (L€) or it was randomly assigned from the set e = [L€0, L€50, L€100, L€150, L€200]. Individuals knew if they were in a certain-information or uncertain-information environment. Participants in the certain-information sessions knew that the person with whom they were matched had an endowment of L€100. Participants in the uncertain-information sessions knew the distribution from which endowments were selected, but did not know the endowment of the individual with whom they were matched.

At the beginning of each decision-period, subjects were endowed with laboratory euros and were told whether they would assume the role of an investor or an investee for the period. Investors were asked to make a decision about how much of their endowment they wished to invest with their paired anonymous person. They were told that they have the choice of investing some, all, or none of their endowment. They were instructed to enter this number on the appropriate place on their computer screen. This investment was multiplied by three and reported to the person with whom the investor was paired. After the investees received this information, they decided how much of the resources they controlled (the sum of their own endowment *plus* three times any investment made by the investor) they would return to the investor. Investees entered this amount onto their computer screen through their keyboards. These values were reported to the

investors and the total payoffs to the investors and investees appeared on their screens. Each participant could calculate what the other person received, but was not explicitly told this value.

At the end of the session, subjects completed a short questionnaire. The purpose of this questionnaire was to collect background information as well as information concerning the subjects' perceptions of the session in which they participated. Subjects were then paid privately, where laboratory euros were converted into dollars at the rate L€100 = 1.00 dollar. The average earnings per subject for the session were $34. The average earnings for the eighteen rounds reported in this paper were about $31.[7]

4.2. Trust and Reciprocity

Investors' decisions in the investment game provide a measure of trust. The Nash equilibrium of the one-shot game played by income-maximizing risk-neutral individuals has nobody investing any endowment resources because investors anticipate that income-maximizing risk-neutral investees will keep everything they receive, leaving the investors with less than their initial endowments. Sending anything implies some measure of trust by the investor that the investee will not keep everything. Trust is measured as the proportion of the endowment that is invested.

Typically, reciprocity is measured as the proportion of the grossed-up (in this case, tripled) investment returned by the investee to the investor. This measure lies

[7] This is estimated by noting that mean trust is 0.57 in the one-shot game (see Kanagaretnam et al., 2009). This makes the mean social surplus $3.14 per period. Two people share this surplus so the mean earnings per person is $3.14 in the two one-shot rounds. $34 - $3.14 ≈ $31.

between unity and zero. However, in our environment the responder also has an endowment, and so the potential amount returned to the sender could be as much as the grossed-up investment plus the receiver's endowment. Thus, it is possible for the ratio of what is returned to the sender divided by the grossed-up investment to exceed unity. If this occurs, reciprocity is reported as unity. It also is possible for this measure to be undefined if the sender invests nothing. In these cases, the observation is dropped from the sample of reciprocity measures.

5. Results

5.1. Trust

Table 1 and Figure 1 summarize the average trust indices for the three veto treatments by information condition and sequence. Analyses of these data are conducted following an OLS regression analysis of a fully saturated model with the three *veto* treatments, the two *information* conditions and the design variables *sequence* (with two levels) and *order* (with two levels). The data are clustered on subject identification numbers (ID) to account for the repeated observations that characterize the within-subject design given the information condition and sequence characterizing each session.

The significance of the *order* variable is tested using the fully saturated model and the null that there is no significant *order* effect is maintained ($F(10, 181) = 0.72$, $p = 0.702$). The data are pooled across *order* and an OLS regression, clustered on ID, is estimated for the fully saturated restricted model using all of the variables identified above other than *order*. The restricted regression results are presented in Table 2.

Visual inspection of Figure 1 suggests that moving from no veto to a costly veto to a costless veto increases the investor's trust in the trustee. This pattern is reflected in the certain and uncertain information conditions and in the backward and forward sequences. There appears to be an information effect, with lower trust associated with the less transparent environment. This is the case across *veto* treatments. There may be a sequence effect and an interaction effect between *sequence* and the *information* condition.

5.1.1. Information and Sequence Effects

The coefficients in Table 2 indicate that although the marginal effect of *sequence* is not significant there are significant interaction terms between *sequence* and the *veto* treatments. On the other hand, the marginal effect of *information* is significant and two of the second-order and third-order interaction terms are also significant. F-tests run to identify the significance of the main effects of *sequence* and *information* permit us to reject the null hypotheses that there are no significant main effects (for *sequence*, $F(6, 181) = 4.67$, $p = 0.000$; for *information*, $F(6, 181) = 6.72$, $p = 0.000$).

5.1.2. Veto Treatment Effects

The summary average trust indices presented in Figure 1 and the summary statistics in Table 1 do not reflect their dependence across veto treatments. The results of the OLS regression reported in Table 2 allows us to test for differences across veto treatments accounting for the repeated observations of the within-subject design that has been used. The results of pairwise comparisons of average trust indices for veto treatments by information condition and sequence are presented in Table 3.

The statistics in Tables 1 and 3 show that the steady increase in the average trust indices with the introduction of investor empowerment through the costly and then costless veto is statistically significant at each stage in the forward sequence with certain information. This pattern is followed with uncertain information, although the introduction of the costly veto does not result in a statistically significant increase in the average trust indices. However, the move to the costless veto leads to a significant increase in the average trust indices.

Referring to the backwards sequence, the average trust indices with the costless veto are at comparable levels as they are at the end of the forward sequence. These are 0.95 versus 0.92 for the certain information condition and 0.81 versus 0.77 for the uncertain information condition. The values in each of these pairs are not statistically different from one another ($F = 0.51$, $p = 0.476$ for the certain information condition and $F = 0.24$, $p = 0.627$ for the uncertain information condition).

With the backward sequence, the move from the costless to costly veto does not lead to a significant change in the average trust indices. However, the move from the costly veto to the no veto option results in a significant reduction in the average trust indices.

Trust is compromised by the absence of transparency. Empowering investors with the ability to veto an investee's distribution decision increases trust even in the absence of transparency, but the empowerment mechanism presented in this laboratory environment does not fully restore the trust lost to opacity. This result holds for both the backwards and forward sequences in the laboratory.

In summary, the trust data do not support a significant order effect but they do support significant information and sequence effects. The data support the prediction that empowering investors will increase trust but this only partially offsets the trust lost to the loss of transparency.

5.2. Reciprocity

Table 4 and Figure 2 summarize the average reciprocity indices for the three veto treatments by information condition and sequence. Analyses of these data are conducted following an OLS regression analysis of a fully saturated model with the three *veto* treatments, the two *information* conditions and the design variables *sequence* (with two levels) and *order* (with two levels). The data are clustered on subject identification numbers (ID) to account for the repeated observations that characterize the within-subject design given the information condition and sequence characterizing each session.

The significance of the *order* variable is tested using the fully saturated model and the null that there is no significant *order* effect is maintained ($F(10, 181) = 0.37$, $p = 0.958$). The data are pooled across *order* and an OLS regression, clustered on ID, is estimated for the fully saturated restricted model using all of the variables identified above other than *order*. The restricted regression results are presented in Table 5.

Visual inspection of Figure 4 suggests that moving from no veto to a costly veto to a costless veto has a small effect on the trustee's reciprocity. This pattern is reflected in the certain and uncertain information conditions and in the backward and forward sequences. There appears to be an information effect, with lower

reciprocity associated with the less transparent environment. This is the case across veto treatments. There may be a sequence effect and an interaction effect between sequence and the information condition.

5.2.1. Information and Sequence Effects

The coefficients in Table 5 indicate that the marginal effect of *sequence* is significant and there are significant interaction terms between *sequence* and the *veto* treatments. On the other hand, the marginal effect of *information* is not significant and none of the second-order and third-order interaction terms are significant. However, F-tests run to identify the significance of the main effects of *sequence* and *information* permit us to reject the null hypotheses that there are no significant main effects (for *sequence*, $F(6, 181) = 2.71$, $p = 0.015$; for *information*, $F(6, 181) = 4.69$, $p = 0.000$).

5.2.2. Veto Treatment Effects

The summary average reciprocity indices presented in Figure 2 and the summary statistics in Table 4 do not reflect their dependence across veto treatments. The results of the OLS regression reported in Table 5 allows us to test for differences across veto treatments accounting for the repeated observations of the within-subject design that has been used. The results of pairwise comparisons of average reciprocity indices for veto treatments by information condition and sequence are presented in Table 6.

The statistics in Tables 4 and 6 show that very small increase in the average reciprocity indices with the introduction of investor empowerment with the costly veto (from 0.59 to 0.65) is significant in the forward sequence with certain

information ($F = 5.40$, $p = 0.021$). The introduction of the costless veto does not change the level of reciprocity significantly (reciprocity falls from 0.65 to 0.64, $F = 0.64$, $p = 0.425$). This pattern of quantitative changes is followed with uncertain information (0.56 to 0.58 to 0.57), although the introduction of the costly veto does not result in a statistically significant increase in the average reciprocity indices ($F = 0.08$, $p = 0.782$). The small decrease associated with the move to the costless veto is also not significant ($F = 0.06$, $p = 0.802$).

Referring to the backwards sequence, the average reciprocity indices with the costless veto are at comparable levels as they are at the end of the forward sequence. These are 0.64 versus 0.70 for the certain information condition and 0.57 versus 0.55 for the uncertain information condition. The values in each of these pairs are not statistically different from one another ($F = 2.98$, $p = 0.086$ for the certain information condition and $F = 0.13$, $p = 0.724$ for the uncertain information condition).

With the backward sequence, the move from the costless to costly veto does not lead to a significant change in the average reciprocity indices. However, the move from the costly veto to the no veto option results in a significant reduction in the average reciprocity indices.

Although the main effect of the information condition is significant, the data presented in Figure 2 suggests that there is an interaction effect between the *information* condition and *sequenc*e. There is no significant difference between the average reciprocity indices for the no veto treatment in the forward sequence under the different information conditions (0.59 versus 0.56, $F= 0.61$, $p = 0.435$) but this

difference between information conditions widens slightly with the costless veto treatment (0.64 versus 0.57, $F = 3.63$, $p = 0.058$) and is marginally significant. However, for the backward sequence, the difference between the reciprocity indices for the no veto treatments is large and highly significant (0.56 versus 0.35, $F = 8.54$, $p = 0.004$), as is the difference between the reciprocity indices for the costless veto (0.70 versus 0.55, $F = 7.57$, $p = 0.007$). In the forward condition the gap never exceeds 0.07 while in the backward condition the gap always exceeds 0.09.

Reciprocity is compromised by the absence of transparency, but not significantly so in the forward sequence. In the backwards sequence, there is a greater difference between reciprocity displayed by trustees under the different information conditions. Transparency fosters greater reciprocity. In the absence of reciprocity, moving from a no veto environment to an environment in which it is possible to exercise a veto has no significant affect on reciprocity. However, if investors and investees begin in an environment in which there is a costless or costly veto, the move to a no veto environment brings a significant reduction in reciprocity.

In summary, the reciprocity data do not support a significant order effect but they do support significant information and sequence effects as well as a significant interaction between sequence and information. In the absence of transparency, being empowered does not lead to an increase in reciprocity but losing empowerment leads to a reduction in reciprocity.

5.3. Rejection Rates

Table 7 and Figure 3 summarize the rejection rates for the costly and costless veto treatments by information condition and sequence. Analyses of these data are conducted using χ^2 tests of the differences between the proportions of total opportunities to cast a veto that were exercised across treatments and conditions.

Visual inspection of Figure 3 suggests that there is a sequence effect and an interaction effect between sequence and the information condition. The proportions of vetoes exercised in the forward sequence appear to be different for the two veto treatments but if there is an information effect it is for the costless veto. However, while the proportions of vetoes exercised in the backwards sequence appear to be different for the two veto treatments, there does appear to be an information effect.

In addition, there appears to be an interaction between sequence and information because the differences in veto rates across treatments by information condition appear to be different for the backwards sequence than for the forward sequence.

The first set of tests that we report compare the rejection rates across information conditions for each veto treatment in each sequence. There are four pairs of proportions: costly vetoes in the forward sequence (pair I), costless vetoes in the forward sequence (pair II), costless vetoes in the backwards sequence (pair III) and costly vetoes in the backwards sequence (pair IV). These four pairs appear as vertical pairs in Figure 3 from left to right across the figure.

None of the elements of these four pairs of rejection rates are significantly different from one another. For each pair, the p-value associated with the χ^2 statistic exceeds 0.366.[8] These results lead us to conclude that there is no significant information effect on the rejection rates. Increasing transparency in this investment environment does not lead to a significant change in the rate at which investors accept the returns provided by investees.

The next two tests compare the pooled data in pair II with the pooled data in pair III (the proportions associated with the costless veto) and the pooled data in pair I with the pooled data in pair IV (the proportions associated with the costly veto). These tests address the existence of a sequence effect.

The differences between the rejection rates for the costless and costly vetoes in the forward and backward sequences are not statistically significant at the conventional 5 percent significance level. For the costless veto $\chi^2 = 0.167$, $p = 0.683$ and for the costly veto $\chi^2 = 3.520$, $p = 0.061$. This addresses a design issue. The sequence in which the empowerment treatments are presented to the participants in the experiment does not have a significant impact on the outcomes. This result may be tempered because the difference between the frequencies with which the veto is exercised when it is costly may be judged to be marginally significant. It is possible that increasing the cost of the veto will have a smaller impact on lowering rejection rates than lowering the cost of the veto increases rejection rates.

[8] The χ^2 statistics and p-values for the four cases are $\chi^2 = 0.152$, $p = 0.696$ for I, $\chi^2 = 0.210$, $p = 0.647$ for II, $\chi^2 = 0.502$, $p = 0.479$ for III and $\chi^2 = 0.818$, $p = 0.366$ for IV.

The final test compares the rejection rates for the costly and costless vetoes pooled over information conditions and sequence. This pools the pairs of data I and IV (with 55 rejections from 546 opportunities) and the pairs II and III (with 45 rejections from 268 opportunities).[9] The null hypothesis that these proportions are the same cannot be maintained ($\chi^2 = 7.529$, $p = 0.006$). Rejections increase when the cost of rejection falls.

In summary, the rejection rate data display neither significant information nor sequence effects. However, the data support the conclusion that costless vetoes lead to more rejections of investees decisions than do costly vetoes.

6. Conclusions

Our laboratory-controlled environment characterized by uncertainty and incomplete information permits us to provide experimental evidence on the effects of transparency and empowerment on trust (investment by a principal) and trustworthiness (reciprocal behavior of an agent) in a simple two-person investment game. We find that when principals are empowered by being able to punish agents who may not act in a way the principal believes is in the principal's best interest, trust and investment increases over that which is realized in the

[9] Some of the observations in the rejection set for the costly veto are provided by the same people who enter a veto in the costless environment because of the within-subject design used for the forward and backward sequences. This test provides a rough measure of the difference between the rejection rates from the two veto treatments. A test that does not have this drawback uses the data from the first veto treatment seen by participants in each sequence. This test compares the data from pair I (the costly veto in the forward sequence) and from pair III (the costless veto in the backward sequence). All of the observations are from independent samples and none of the participants have seen a veto treatment of any kind before participating in the treatments reported here. The rejection rate from pair I is 8.6% and the rejection rate from pair III is 18%. The null hypothesis that these rejection rates are equal cannot be maintained ($\chi^2 = 9.340$, $p = 0.002$). This uses a between subjects test to conclude that the rejection rate increases as the cost of rejection falls.

absence of empowerment. We also find that when asymmetric or incomplete information characterizes the investment game the levels of trust (investment) are lower than when information is complete (the environment is transparent).

We also find that these relationships are maintained regardless of whether the empowerment instrument most favorable to the principal is implemented first or whether it is introduced after the participants experience less favorable environments. In transparent environments the effect of empowerment is about the same regardless of whether empowerment is introduced or removed. However, in opaque environments, the loss of empowerment has a substantially greater negative effect on trust that the positive effect associated with the introduction of empowerment.

While this environment is substantially abstracted from the naturally occurring environment, these results suggest that practical public policies designed to foster increased transparency in financial transactions are likely to have positive effects on investment. Furthermore, public policies designed to empower principals, such as the Say on Pay practices, are likely to increase investment. A particularly notable result is related to the removal of empowerment in the laboratory environment. If this is related to the field, it suggests that policies directed towards deregulation and the limitation of the empowerment of principals with respect to their agents will have a much more dramatic negative impact on trust (and investment). In the context of a sluggish economy, these may be policies for practical policy makers to avoid.

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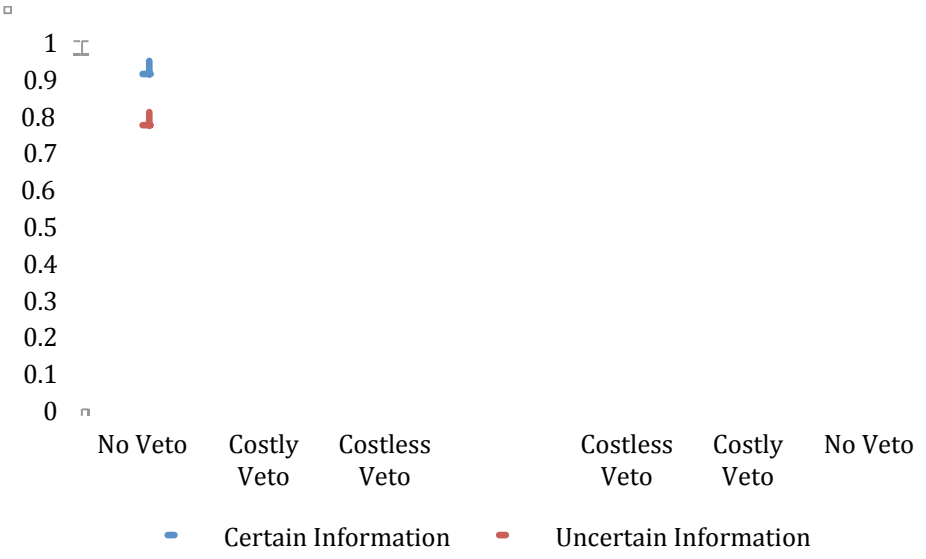

Figure 1. Average Trust by Sequence, Information Condition and Veto Treatment

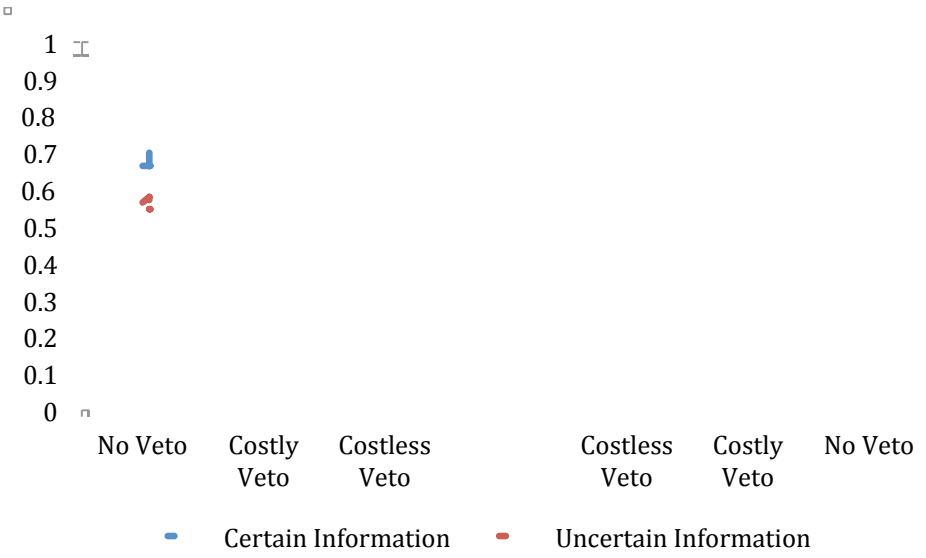

Figure 2. Average Reciprocity by Sequence, Information Condition and Veto Treatment

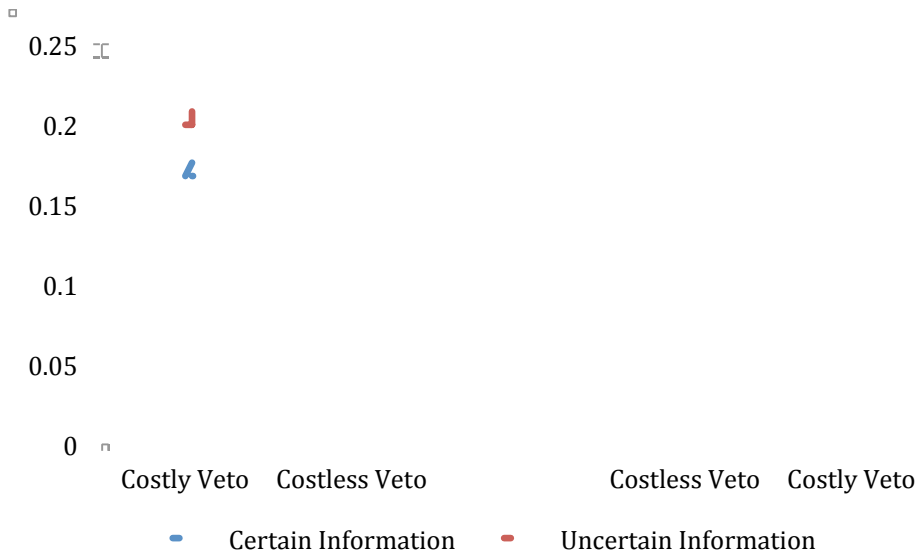

Figure 3. Rejection Rates in Empowerment Treatments by Sequence and Information Condition

Table 1. Average Trust

	Forward Sequence				Backward Sequence		
	No Veto	Costly Veto	Costless Veto		Costless Veto	Costly Veto	No Veto
Certain	0.7115 (0.2465) [60]	0.8427 (0.1907) [60]	0.9471 (0.1285) [34]		0.9162 (0.1908) [26]	0.8669 (0.2516) [26]	0.6875 (0.3116) [26]
Uncertain	0.6157 (0.3080) [72]	0.6430 (0.3112) [70]	0.8082 (0.2825) [48]		0.7717 (0.3091) [24]	0.7735 (0.2834) [23]	0.3895 (0.2770) [24]

Table 2. Restricted Regression for Average Trust Index

Variables	Coefficient	Robust Standard Error	p-value
Repeated Game with No Veto (rg)	-0.192	0.047	0.000
Costly Veto (v1)	-0.165	0.049	0.001
Costless Veto (v2)	dropped		
Sequence (S)	-0.036	0.075	0.627
Information (I)	0.200	0.045	0.000
rgS	-0.190	0.090	0.037
v1S	0.167	0.084	0.049
v2S	dropped		
rgI	-0.104	0.039	0.009
v1I	dropped		
v2I	-0.061	0.056	0.277
SI	0.006	0.086	0.949
rgSI	0.197	0.112	0.082
v1SI	-0.112	0.091	0.223
v2SI	dropped		
Constant	0.808	0.041	0.000

Number of clusters (ID) = 182
Number of observations = 493
F(11, 181) = 17.73
Prob > F = 0.0000
R-squared = 0.1981
Root mean square error = 0.2663

Note: rgS, v1S and v2S are the interactions between rg, v1, v2 and Sequence (S = 1 is the Backward sequence), rgI, v1I and v2I are the interactions between rg, v1, v2 and Information (I = 1 is certain information), SI is the interaction between Sequence and Information, rgSI, v1SI and v2SI are the third order interactions of treatments, Sequence and Information and Constant is the estimated mean for the no veto treatment with uncertain information in the forward sequence.

Table 3. F-Statistics and p-Values (in parentheses) for Differences between Average Trust Indices

	Forward Sequence				Backward Sequence		
	No Veto versus Costly Veto	Costly Veto versus Costless Veto	Costless Veto versus No Veto		Costless Veto versus Costly Veto	Costly Veto versus No Veto	No Veto versus Costless Veto
Certain	37.50 (0.000)	15.54 (0.000)	47.89 (0.000)		0.04 (0.848)	14.50 (0.000)	22.96 (0.000)
Uncertain	0.70 (0.405)	11.33 (0.001)	16.85 (0.000)		0.00 (0.979)	33.26 (0.000)	24.49 (0.000)

Table 4. Average Reciprocity

	Forward Sequence				Backward Sequence		
	No Veto	Costly Veto	Costless Veto		Costless Veto	Costly Veto	No Veto
Certain	0.5925 (0.1799) [52]	0.6490 (0.0917) [60]	0.6350 (0.0940) [34]		0.7001 (0.1749) [26]	0.6327 (0.1388) [26]	0.5622 (0.2314) [26]
Uncertain	0.5633 (0.2325) [72]	0.5795 (0.1576) [69]	0.5716 (0.1966) [46]		0.5539 (0.2012) [24]	0.5279 (0.1453) [23]	0.3537 (0.2557) [21]

Table 5. Restricted Regression for Average Reciprocity Index

Variables	Coefficient	Robust Standard Error	p-value
Repeated Game with No Veto (rg)	dropped		
Costly Veto (v1)	0.016	0.026	0.538
Costless Veto (v2)	0.008	0.033	0.802
Sequence (S)	-0.210	0.062	0.001
Information (I)	0.029	0.037	0.435
rgS	dropped		
v1S	0.158	0.063	0.013
v2S	0.192	0.080	0.018
rgI	dropped		
v1I	0.040	0.036	0.260
v2I	0.034	0.045	0.452
SI	0.083	0.063	0.188
rgSI	0.096	0.099	0.330
v1SI	-0.048	0.070	0.498
v2SI	dropped		
Constant	0.563	0.028	0.000

Number of clusters (ID) = 182
Number of observations = 497
F(11, 181) = 5.28
Prob > F = 0.0000
R-squared = 0.1186
Root mean square error = 0.1792

Note: rgS, v1S and v2S are the interactions between rg, v1, v2 and Sequence (S = 1 is the Backward sequence), rgI, v1I and v2I are the interactions between rg, v1, v2 and Information (I = 1 is certain information), SI is the interaction between Sequence and Information, rgSI, v1SI and v2SI are the third order interactions of treatments, Sequence and Information and Constant is the estimated mean for the no veto treatment with uncertain information in the forward sequence.

Table 6. F-Statistics and p-Values (in parentheses) for Differences between Average Reciprocity Indices

	Forward Sequence				Backward Sequence		
	No Veto versus Costly Veto	Costly Veto versus Costless Veto	Costless Veto versus No Veto		Costless Veto versus Costly Veto	Costly Veto versus No Veto	No Veto versus Costless Veto
Certain	5.40 (0.021)	0.64 (0.425)	1.86 (0.174)		1.44 (0.232)	4.16 (0.043)	8.86 (0.003)
Uncertain	0.38 (0.538)	0.08 (0.782)	0.06 (0.802)		0.29 (0.591)	9.33 (0.003)	7.46 (0.007)

Table 7. Rejection Rates in Empowerment Treatments

	Costly Veto		Costless Veto	
	Percent Rejected	Rejected/Total	Percent Rejected	Rejected/Total
Forward				
Certain	0.083	15/180	0.176	12/68
Uncertain	0.088	19/216	0.150	15/100
Total	0.086	34/396	0.161	27/168
Backward				
Certain	0.115	9/78	0.154	8/52
Uncertain	0.167	12/72	0.208	10/48
Total	0.140	21/150	0.180	18/100
Grand Total	0.101	37/182	0.168	45/268