

To Deceive or Not To Deceive: The Effect of Deception on Behavior in Future Laboratory Experiments*

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Abstract

Experimental economists believe (and enforce the idea) that researchers should not employ deception in the design of experiments. This rule exists in order to protect a public good: the ability of other researchers to conduct experiments and to have participants trust their instructions to be an accurate representation of the game being played. Yet other social sciences, particularly psychology, do not maintain such a rule. We examine whether such a public goods problem exists by purposefully deceiving some participants in one study, informing them of this fact, and then examining whether the deceived participants behave differently in a subsequent study. We find significant differences in the selection of individuals who return to play after being deceived as well as (to a lesser extent) the behavior in the subsequent games, thus providing qualified support for the proscription of deception. We discuss policy implications for the maintenance of separate participant pools.

Keywords: laboratory experimental methods; experimental economics; deception; psychology and economics; laboratory selection effects

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1 Introduction

In two of the original experimental economics textbooks, Davis and Holt (1993) and Friedman and Sunder (1994), among others, proscribe the use of deception in experiments. The primary concern with deception is that many experimental laboratories use a common pool of participants. Thus, a public goods problem exists in which experiencing deception in one experiment may cause participants to react differently (and uncertainly) in future games with other researchers. Clearly, maintaining this “public good” involves trade-offs between benefits to the individual (ability to conduct experiments that require deception) and the group (maintaining a subject pool that is trained to believe that experiment instructions are truthful). As some research questions may be better answered by using deception, should we forgo the knowledge which could be acquired through such experiments in order to maintain a common pool of deception-free participants? This concern warrants testing, and in this paper we determine the presence and extent of such sample contamination in a particular setting.

Although we focus here on the particular issue of deception, the overall methodological question of understanding what we mean by “control” in the laboratory is important more generally. Levitt and List (2007) discuss the tradeoffs between the laboratory and the field, and specifically focus on how the manner in which individuals are selected and watched may alter their behavior. Deception is just one example of how a subject pool may be altered which may influence interpretation of later experiments. If a subject pool is entirely confident that the environment is deception-free, this may heighten “control” at the expense of reality. On the other hand, in the field one may have participants who are suspicious of deception in unknown ways (thus exhibiting less control, but more reality). This paper does not compare the field to the laboratory with respect to behavior in economic experiments. Instead we examine an important selection effect: does a singular experience in the laboratory affect the likelihood that different individuals return in a future month for another experiment run by a different researcher?

Bonetti (1998) writes the first, and perhaps only, article arguing that deception should be allowed in experimental economics. He reviews the evidence from experiments in psychology and concludes that deception has a minimal effect on behavior. Two immediate, mainly philosophical, replies to Bonetti's argument are Hey (1998) and McDaniel and Starmer (1998). Later, Ortmann and Hertwig (2002) conduct a thorough review of the evidence from experiments in psychology. They conclude that the experience of deception does affect participants' expectations, suspicions, and future behavior.

Here we give a brief overview of the different strands of the psychology literature looking either at the effects of suspicion of deception or the effects of past experiences with deception itself on different outcomes. A reader interested in more details should consult the review by Ortmann and Hertwig (2002). There is a rather large strand of literature in psychology looking at differences in behavior based on whether a participant admits to being suspicious of deception. Stricker, Messick, and Jackson (1967)—one of the seminal papers on this topic—find that subjects who admitted to suspecting deception in the experiment in which they participated conformed less (i.e. were less likely to agree to unreasonable propositions simply because other subjects agreed) than those who claimed not to suspect anything. The converse of this line of study is that which looks at whether subjects who have been deceived claim they will be suspicious of information given to them by experimenters in the future. Both Epley and Huff (1998) and Krupat and Garonzik (1994) find that deceived subjects anticipate being more suspicious in the future. There is a third line of literature looking at the effects on behavior of being warned before an experiment by the experimenter that the experiment “might” include misinformation, or of being informed by a confederate before the game that it actually does involve deception. This type of forewarning tends not to lead to changes in behavior (see Wiener and Erker (1986) for the first type of warning and Golding and Lichtenstein (1970) for the second) although there is some mixed evidence.

The three areas of literature discussed above do not look at the direct effects of experiencing deception in one experiment on behavior in future experiments. These

consequences are the principal motivation for the prohibition of deception in experimental economics and are the focus of this paper. There do exist a few experiments in psychology which attempt exactly this type of test, albeit with non-economic experiments such as personality tests and memory games.¹ Some of these papers find that experiencing deception leads to changes in behavior (Silverman, Shulman, and Wiesenthal 1970; Christensen 1977) while others do not (Fillenbaum 1966). Some find that past experiences with deception lead to changes in behavior when the first and second set of experiments are noticeably similar, but not when the experiments are dissimilar (Brock and Becker 1966; Cook *et al* 1970).

There are many differences between the above psychological literature measuring the effects of experienced deception on play in future experiments and the setup in this paper. Some of the psychological experiments did not give the control group (those who were not deceived) any prior experimental experience (Silverman *et al* 1970). Other experiments subjected the control group to a very different experience than that experienced by the deceived (Christensen 1977). In contrast, in this paper, the initial treatment was almost identical for those who were deceived and those who were not.

Additionally, in some of the psychological experiments, the first and second treatments were conducted immediately sequentially. After the participants took part in the first experiment, a different researcher walked into the room and claimed that he really needed more subjects and wondered if they might have time to participate in one more experiment being run next door (Brock & Becker 1966; Cook *et al* 1970; Christensen 1977). This is not the conventional method of subject recruitment. In the current paper, the initial and secondary experiments were separated by approximately three weeks, and the recruitment mechanism for both experiments used e-mail, a conventional approach.

In almost all of these psychological experiments, the deception involved lying to the subjects about the purpose of the experiment. For example, subjects were given two poems by Robert Frost, were told that one was by Frost and one by a high school English

¹ Ortmann and Hertwig (2002) discuss in detail nine papers which look at the effects of past deception on behavior in future experiments in section 3.2.2 of their paper. Here we focus on five of those studies.

teacher, and were then asked to rate the merits of the two poems. After the experiment they were debriefed and told that both poems were actually by Frost and that the experiment was looking at how beliefs regarding authorship affected the rating of the poems. In another experiment participants were told to try to write down as many phone numbers as they could as quickly as possible. After doing so, they were told that excellence in writing phone numbers quickly was a sign of obsessive compulsive disorder. Then they were told to try again. After the experiment, they were told that excellence in writing phone numbers was not actually a sign of obsessive compulsion; rather the experimenter wanted to see if the participant would write fewer phone numbers after hearing that misinformation.

Experimental economists tend not to implement this particular form of deception. One of the more common forms of deception used in economic experiments is deceiving the player with regards to the partner with whom he is matched. Weimann (1994), Blount (1995), Scharlemann *et al* (2002), Sanfey *et al* (2003), and Winter and Zamir (2005) all told players that their partners are humans, when in fact they are computers playing either predetermined or random strategies. Kim and Walker (1984) told subjects that they were playing a public goods game with many more individuals than was in fact the case, using a stochastic variable to approximate the contributions from the nonexistent players. Ball *et al* (2001) and Gibbons and Van Boven (2001) tell players that their partner is of a certain intelligence level or personality type. Although their partner is a human, not a computer, the type is generated artificially. This allows the researcher to test how subjects react to specific situations which may not arise naturally with high probability.

Different types of deceit may lead to different contamination of subjects. In this paper, we focus on one specific type of deception, and the results may not be generalizable to all types of deception. Specifically, we deceived subjects as to the identity of their partner. We chose this type of deception because, from both conversations with experimental economists and the small literature in experimental economics using deception (mentioned in the previous paragraph), this seems to be the type of deception that economists are primarily interested in. We randomize subjects into

a treatment group which experiences deception regarding the identity of their partner and a control group which participates in a similar experiment but with no deception. Then we measure the effects of deception on the sample selection of those who return to participate in further experiments, as well as on subjects' behavior in those future experiments.

2 Experimental Procedures

2A Overall Setup

The Xlab (experimental social science laboratory) of the University of California at Berkeley maintains two participant lists, consisting primarily of undergraduates but also a few graduate students as well as some staff members. One participant list does not maintain the no-deception rule; it is less active but is sometimes used by behavioralists in the business school.² The second participant list is used primarily by economists and maintains the no-deception rule. Occasionally, as in the present experiment, researchers are allowed to deceive subjects from the latter pool, who are then moved to the former.

In all cases, potential subjects are recruited using a variety of methods, including mass emails (to class lists or student organizations), flyers posted around campus, and booths at student and staff activity fairs. Subjects must actively choose to join the list, which then makes them eligible to receive announcements regarding (and to subsequently sign up for) specific experimental sessions. There are approximately six to ten unique experiments and concomitant e-mail announcements in a given month.

Our first round of experiments involved 261 subjects across ten sessions, each of which lasted for approximately one hour. Five of these sessions involved deception (treatment) while five did not (control). The three initial sessions were necessarily control, so that we could use that data to program the computer players, but after that sessions were randomized as to treatment or control. Subjects signed a consent form and

² The psychology department maintains a separate pool, although it is entirely possible for a subject to be on both the Xlab list and the psychology list.

were given a written instruction sheet (see Appendix A), which was then read aloud by the researcher. The experiment itself was conducted on laptop computers implementing a z-Tree (Zurich Toolbox for Readymade Economic Experiments) program. When all subjects had completed the experiment, payoffs were determined and subjects waited approximately 15 minutes for individual checks to be filled out and distributed.

In the non-deception treatment (five sessions, 132 subjects), subjects were randomly assigned to roles and matched with one another, and payoffs were determined according to actual play. (The actual games are described in detail below.) In the deception treatment (five sessions, 129 subjects), subjects were randomly assigned to roles, but in all cases their opponents' actions were determined by a computer program that simulated the human play.^{3,4} During the check-processing wait time, these subjects were informed that they had been deceived and that they had actually been partnered with a computer rather than with another human. They were told that this was necessary for the research (but no more specific details) and were asked to sign a second consent form allowing their data to be used. All subjects signed this consent form.

Two to three weeks later, all subjects from the first round were sent a recruitment email for new experimental sessions using the name of a researcher different from that in the first round. This email was identical to the standard Xlab recruitment email, except that it promised \$10 above and beyond the normal earnings in order to facilitate sufficient return rates. Subjects did not know that only they had received this particular email, although it is possible that some of them checked with friends and noticed that it had not gone out to the entire pool. However, that in itself is not an uncommon occurrence given various screening criteria used by other researchers at the lab.

In all, 142 people returned for one of the eight sessions that took place three to four weeks after the sessions in round one. These lasted slightly under an hour, and each

³ We explain this in more detail in the next section.

⁴ If we were only interested in the results from the first round of experiments, it may have been possible to use a technique suggested by Bardsley (2000) which avoids deception while at the same time maintains a setup in which players who think their partner is a human are actually partnered with a computer. Using this technique, though, would not allow us to measure the effects of being deceived on return rates and play in subsequent experiments.

consisted of a mixture of both deceived and non-deceived subjects. A different researcher than in the first round was physically present in the room for these sessions. Subjects signed a standard consent form, were given the instructions, and then completed the experiment as three separate interactions on the VeconLab website.

Afterward, as they waited to be paid, they were informed that these sessions were in fact a continuation of the previous experiment, and they were asked to sign another consent form allowing use of their data. For those from the deception treatment, this was thus the fourth informed consent form that they signed in the course of the full protocol. None of the participants refused to give their consent at any point throughout the experiments.

2B. Specific games

In our first set of sessions, we ran a trust game (Berg, Dickhaut, and McCabe 1995) with a \$20 endowment to the trustor.⁵ The trustor chose an amount (the ‘investment’) to send to the trustee, and this amount was then tripled. The trustee then returned as much or as little as desired. There were two practice rounds and four actual rounds. Anonymous partners were randomly chosen and maintained for all six rounds (and, like everything else above, subjects were told that this would be the case), but individuals were not told the identity of their partner at any point. No communication in the room was allowed.

One of the four actual rounds was randomly chosen to determine payoffs. All subjects were given an additional \$5 to make sure that no one received a final payout of \$0, so the potential individual range (for both roles) was \$5-\$65. In fact, players in both roles sometimes received \$5 (i.e. \$0 in the game itself), and some trustees received \$65.

We ran three non-deception sessions first and used those to program the computer play in the deception sessions in order for it to match the human play as closely as

⁵ Despite evidence suggesting that the first player in the Trust game is not merely acting due to “trust” but also due to a desire to take risks (Karlan 2005, Schechter 2006), we use the canonical terminology here of “trustor” and “trustee.”

possible. Since these were repeated games, we needed to try to mimic the entire strategy, not simply the observed play-paths. Obviously this was imperfect, but in the end we categorized trustors into five types, and trustees into three types. A small percentage of trustors never invested more than \$5 of their endowment in any round, while a significant fraction invested \$20 every time. We also included a “trigger strategy” for all types of trustors (as observed in the real subjects): if the trustee ever sent back less than what was invested, the trustor never again invested anything. The results section below outlines the summary statistics showing that the actual game play by participants in the deception and non-deception treatments is not significantly different.

In our second set of sessions, which occurred three to four weeks after the first session, we ran three different games with each subject. All games were run with subjects simultaneously connected to the VeconLab website at the University of Virginia. The three decision problems were run as independent sequential online sessions, since the type of game was different in each case. All instructions were provided on the website under the game selection categories of Bargaining and Fairness / Bargaining Games; Individual Decision Problems / Lottery Choice Menu; and Game Theory Experiments / 2x2 Matrix Games, respectively.

The first game was a dictator game with an endowment of \$20 (i.e. subjects chose how much of this amount to give away to an anonymous partner). All subjects played the role of sender (and therefore, in effect, also the role of potential receiver), but for payoffs only half of the matchings were consummated, so that each subject ended up as either sender or receiver but not both.

The second “game” was a series of gambles (Holt and Laury 2002), with ten ordered binary choices between two lotteries. The exact payoffs are shown in Appendix Table A. The first choice was between a (safe) lottery that paid \$11 with 10% chance and \$8.80 with 90% chance and a (risky) lottery that paid \$21.20 with 10% chance and \$0.55 with 90% chance. Here the first lottery was risk-dominant and also had a significantly higher expected value. As the choices progressed, the probability of the higher payoff in

each lottery increased by 10%, until the final choice was between \$11 (with certainty) versus \$21.20 (with certainty), so that the second lottery dominated in all respects.

In the third game, subjects played a prisoner's dilemma with payoffs $(C,C) = (10,10)$; $(D,D) = (6,6)$; and $(D,C) = (15,1)$. They were randomly assigned to be the row or column player and were randomly matched with one another. No deception was involved for any of the games in these sessions. Finally, one of the three games was randomly chosen to determine payoffs. (In the risk game, which involved multiple decisions, one decision was randomly chosen.) We also paid each player \$10 in addition to their earnings from the games (as promised in the recruitment email).

These three games were chosen in part because they are so common in the field and laboratory, and in part because previous research has suggested that subjects behave differently in the dictator game and in the prisoners' dilemma when they think their partner is a human versus when they think their partner is a computer. Our hypothesis was that players who had been deceived in the past as to the identity of their partner would be suspicious in subsequent experiments that their partner was actually a computer, even when we told them they were playing with a human.

There is evidence that, in the prisoners' dilemma, players cooperate more often when they are playing with another human than they do when playing with a computer (Abic and Kahan 1972, Kiesler *et al* 1996, Rilling *et al* 2004). Thus, we hypothesized that those players who had been deceived in the past would suspect they might be playing against a computer and so would cooperate less often in the prisoners' dilemma. Frohlich, Oppenheimer, and Moore (2001) find that players in a dictator game who suspect that they were not paired with real people give less. Thus, we also hypothesized that players who had been deceived would keep more as dictator in the dictator game. We included the Holt-Laury sequence of risky lotteries as a control, our hypothesis being that those who were deceived would not play any differently than those who were not deceived, as it did not involve a partner (unless they believed that part of the deception was our lying about the results of random draws).

3 Results

As there were 261 participants who participated in the first round and 142 in the second round, our sample size is somewhat small when compared with some studies of selection effects. This may make it more difficult to find statistically significant results. On the other hand, adding more participants would have meant deceiving even more subjects. Before conducting this research we did not know how large an effect deception might have on the participants and so were conservative in our recruiting. A second potential drawback of the data is that in the Xlab no demographic information was universally collected other than sex and the number of experiments in which the participant had previously participated.

Before looking to see if being deceived in a previous experiment affects play in a subsequent experiment, we would like to make sure that both the randomization and the deception were successful. Remember, the first round of our experiment involved six rounds of a trust game. The senders who were deceived (not deceived) sent on average \$14.11 (\$12.88) in the first round, \$11.68 (\$9.95) in the last round, and an average of \$14.59 (\$13.33) in all six rounds. Using a *t*-test with equal variance, the *t*-values are 1.041, 1.160, and 1.281 respectively. The receivers who were deceived (not deceived) returned 0.435 (0.438) of what they had received in the first round, 0.186 (0.174) in the last round, and on average 0.413 (0.407) in all rounds. The *t*-values for these tests are 0.095, 0.261, and 0.318 respectively. Males make up 37% of the deceived population and 42% of the non-deceived population, yielding a *t*-value of -0.858. Since none of these differences is significant the randomization appears to have been successful.⁶

3A Effect of Deception on Selection into Subsequent Experiment

⁶ Psychologists believe there may be "faithful subjects" who attempt to take the experiment at face value, even if he suspects deception. Hence, the fact that the deceived and non-deceived play no differently in the initial experiment could be a sign that the deceived do not suspect the deception or that they are merely being "faithful."

The next step in our analysis involves looking at the effect of deception on return rates. As described above, all subjects were invited to participate in another round of experiments approximately three weeks after the first round. Subjects were not led to believe that this subsequent experiment was in any way related to the previous experiments. Only 55% of the original participants returned for the second round of experiments.

Table 1 shows that 51 percent of those who were deceived returned while 58 percent of those who were not deceived returned and that this difference is not statistically significant. While the subjects were not living in a vacuum during the three weeks between the first and second set of experiments, because of the randomization we only need to assume that these two groups had, on average, similar experiences in the time between the two experiments.

Interestingly, gender differences played a significant role in our analysis: females who have been deceived are significantly less likely to return than females in the control group. On the other hand, males who were deceived are significantly *more* likely to return than males in the control group. In results not shown here, we tested for return status based on the number of previous experiments at the Xlab in which the player participated. Although participants who have participated in other experiments in the past are significantly more likely to return in general, the deception effect does not differ for the more or less experienced.

The next few rows of Table 1 examine further selection effects. We look at return rates dividing the sample into four categories: “High Passers” and “Low Passers,” and Player As (trustors) and Player Bs (trustees).⁷ We find no significant differences within three of these four groups. Only “Low Passer, Player B” types seemed to be influenced by the deception in their decision to return. One possible explanation for this is that after the untrustworthy (i.e. the “Low Passers”) found out that they were deceived, they felt more self-conscious about their behavior.

⁷ A player is a “High Passer” if he passed at least half of what he could pass, and a “Low Passer” otherwise. When categorizing players as high or low passers we ignore play in the last round of the trust game. Since many Player As passed no money in the last round, making it impossible to calculate the share returned by Player B in that round, this also allows us to include more observations.

One could worry that, in making multiple comparisons, a few of them are bound to appear significant when in fact there are no true differences. We employ a Bonferonni correction for multiple significance tests in order to correct the significance of independent comparisons and derive conservative estimates for these non-independent comparisons. Given our correlated outcomes, the Bonferonni correction may hold each test to an unreasonably high standard and increase the probability of a Type II error. Still, it is interesting to see which, if any, of our comparisons will stand up to the Bonferonni correction. In Table 1, only one comparison remains significant (at the 5% significance level) after making the Bonferonni correction, and that is the fact that deceived females are less likely to return to participate in the second set of experiments.

In Table 2, we look at selection effects not based on how the player himself played, but rather on how his partner treated him, or on the round of the trust game that was randomly chosen to count for payoffs. When we compare return rates conditional on the play of the partner we find that, for the sub-sample of Player Bs who were sent very little by Player A, being deceived made them less likely to return. Thus, it seems that deception most influenced the individual's decision to return to the laboratory when he also was not lucky in the game. (Recall that Player B had no endowment. If Player A sent him very little, then he went home with very little.) Clearly, then, it was not merely the experience of deception that altered the future decision, but rather deception coupled with low winnings.

This is further reinforced by the final two rows of Table 2. Here we examine the effect of the deception treatment on those whose payoffs were determined by the final round of the Trust game. We had randomly chosen which round of the Trust game determined payoffs. Earnings in the final round, presumably due to backwards induction, were significantly lower than in the previous rounds. There is no difference in overall return rates for those players for whom the last round determined their earnings as opposed to those for whom an earlier round determined earnings. We find that players who were deceived, and for whom the last round of the trust game counted, were much

less likely to return. (We should note that none of the comparisons in Table 2 stand up to the Bonferonni correction.)

Summarizing Table 2: being deceived made individuals less likely to return only when they were unlucky insofar as having received a low payout. Perhaps these players felt that not only did we deceive them in terms of the identity of their partner, but that we may have also deceived them either in terms of purposely choosing to pay them for the round with the lowest payoffs or by programming the “trustor” computer to send them very little. (This, of course, was not the case.) One could argue that it might not be the experience of deception *per se* that encouraged these ‘unlucky’ players not to return; they may have felt that the procedures in the experiment were unjust and not have returned for that reason. For more on this, refer to Thibaut and Walker (1975), whose seminal work on procedural justice finds that, holding the outcome constant, satisfaction depends upon the process by which outcomes are reached.

3B Effect of Deception on Play in a Subsequent Experiment

Next, we analyze whether deception has an effect on play in future experiments. We preface these results with the caveat that we cannot differentiate in practice between any indirect selection effects and direct deception effects. If all participants came back to play in the second round of experiments, we could claim that any differences in play were due to the direct effect of having experienced deception. Since only 55% of the participants came back for the second round, any differences in play in the second set of experiments may be due to a change in behavior at the individual level after experiencing deception: a player who was deceived once begins to second-guess the experimenter and uses a different strategy than he would otherwise. However, differences in behavior may also be due to the selection effect: a player who was deceived once may play no differently in future experiments, but only certain types of players may decide to return after having been deceived. Although we cannot separate the indirect and direct effects,

the combination of the two makes up the true effect on subsequent experimental outcomes, which is the practical outcome of interest.

Table 3 presents these results. We measure four outcomes: (1) the amount kept in the dictator game; (2) risk aversion from the Holt-Laury gambles (if consistent); (3) a binary variable if responses to the risk aversion questions were inconsistent; and (4) defection in the prisoners' dilemma. We analyze the results for the sample as a whole, as well as for (a) inexperienced versus experienced participants; (b) males versus females; and (c) those who took on the role of Player A versus Player B in the first round.

Lastly, we make an attempt at controlling for selection. We saw previously that those players who were deceived and unlucky (either because their payoffs were decided by the last round of the trust game, or because they were a trustee who received very little from their trustor) were less likely to return than those who were not deceived and unlucky. By limiting our analysis to only 'lucky' players, we can look specifically at the group of players exhibiting less selection.

The top left quadrant of Table 3 shows the primary results for the full sample. We show via a *t*-test that mean behavior of the deceived is different from the non-deceived. We also carry out Levene's test comparing the variance of behavior of the deceived to that of the non-deceived. For the results on variance we look only at the amount kept in the dictator game and at the number of safe choices in the Holt-Laury risk game. We do not compare the variances of the two binary outcome variables. One might imagine there to be more noise in the actions of the deceived because they may believe that their actions are less likely to map into payoffs.

As can be seen, we find that the deceived individuals were more likely to behave inconsistently in the risky gambles, and we find no significant differences in the other three outcomes. For the Holt-Laury risk lotteries, we find no differences in risk aversion either in the full sample or in any sub-sample analysis (except for those who were 'lucky'). However, eight percent of the players did not make consistent choices and were categorized as "switchers," and we do find a difference for these individuals. Remember, the Holt-Laury experiment involves a series of choices between a risky and safe option.

As one moves along in the series, the risky option becomes more and more appealing. A rational player should not switch back and forth between the risky and safe options, but rather switch from the safe option to the risky one at one and only one point.

If we compare the risk aversion of the players who played consistently, there is no difference, as we expected, between those who were deceived and those who were not. The measure of risk-aversion in Table 3 is the number of safe choices, which could hypothetically range from 0 to 10, with higher values representing more risk aversion. Using Levene's test for equality of variance, in the full sample we find that the variance for the deceived is significantly higher than for the non-deceived at the 5% level. The variances are also significantly different for all player Bs at the 10% level, for all players and player Bs who had participated in at least 10 previous experiments (at the 5% level), and for all females (at the 10% level). The variances of the amount kept in the dictator game are not significantly different in any of the comparisons.

We also find that significantly more of the deceived players are "switchers"; it is possible that they no longer take the research seriously, and thus switch back and forth between the risky and safe choices. This result is quite significant among the general population of players, as well as among females and the more inexperienced participants.⁸ Using the Bonferonni correction within panels of Table 3, the only finding which remains significant at the 10% level is that deceived females are more likely to play inconsistently in the Holt-Laury gamble.⁹

In the dictator game, we find no significant effect of previous deception on the amount kept in the pooled sample. We do find, however, that those who played as trustor in the first round and were deceived kept significantly more as dictator than did Player As who were not deceived. Further analysis shows that this result holds for both the inexperienced Player As (i.e. who had participated in fewer than 10 previous experiments

⁸ There is also one player who chose all 10 safe lotteries. As the tenth lottery gives 100% probability to the higher of the two payoffs, and the high payoff is higher for the risky choice, no rational player should choose all 10 safe lotteries. The player who did this had also been deceived (although we do not consider this player to be a switcher).

⁹ Note that this effect, albeit significant statistically, is small in terms of the proportion of individuals who answer inconsistently in either the deceived or undeceived pool.

in the Xlab) and female Player As. For the prisoners' dilemma, we find no difference in behavior among those who were deceived versus those who were not (except that females were almost significantly more likely to defect if deceived).

In panel F we look only at the 'lucky' players, focusing attention on the group of players that exhibits less selection. Within this group of players, we still find that players who experienced deception are more likely to play inconsistently in the risk game. On the other hand, there are no differences in play in the dictator game or the prisoners' dilemma within this group of people. This gives suggestive evidence that the difference in behavior in the risk game, which does not involve a partner, is not due to selection, but rather to the effect of experiencing deception. On the other hand, any differences in behavior in the dictator game and prisoners' dilemma, games which do involve partners, may be due to selection.

4 Conclusion

We find that deception influences both the selection of experiment participants as well as their behavior. Specifically, we find that females are less likely to return after being deceived, and that those who fare badly due to luck *and* are deceived are less likely to return than those who fare badly due to luck and are not deceived. Regarding behavior for those who return, we find an increase in the likelihood of answering risk aversion questions inconsistently and an increase in the variance of the number of safe gambles chosen for those who do answer consistently. We suggest that this is evidence of not taking the games seriously.¹⁰ We also find that Player As, especially those who are either female or inexperienced, kept more of their money in the dictator game in the second round.

We have discussed these results with both psychologists and economists and are struck by their reactions: both see the data as supporting their priors! Perhaps this should

¹⁰ An alternative explanation is selection: those who did not fully understand the deception and why it mattered are more likely to return, and such individuals are also less likely to understand the risk aversion questions.

come as no surprise, given what we know about confirmation bias (Lord, Ross, and Lepper 1979). We fully understand that, although we do find clear differences in behavior, they are subject to interpretation as to their economic (or psychological) importance, as well as to further refinement regarding their magnitude and generalizability. The irony is that further study of how deception influences behavior, both in the laboratory and in the real world, requires relaxing the no-deception rule.

In one sense, all we have shown is that prior experiences in life influence the way individuals play games in a laboratory, and that experience in the laboratory is a part of real life (and hence influences later behavior). An opponent of the no-deception rule might argue that deception is no different than other such (uncontrollable) experiences, which must simply be assumed to be orthogonal to the treatments of interest. However, experimental deception is in fact controllable and, as we have demonstrated, has some non-random effects on behavior in the types of treatments that interest experimental economists. At the very least, since prior exposure to deception is potentially knowable information to researchers, it should be accounted for when possible.

Another possibility is that it may be necessary to deceive subjects in order to convince them that they have not been deceived.¹¹ This is perhaps most evident when dealing with probabilities, where people tend to have strong and incorrect preconceived notions. For instance, it is well known that individuals underestimate the number of long strings of either heads or tails in a sequence of random coin-flips, so when presented with a truly random sequence they may well become suspicious upon observing an extended sequence of one outcome. Slightly more subtly, if an experiment involves a very small probability of some extreme event, subjects may doubt the researcher if it ever does occur (especially if this entails a negative outcome for the subject). This provides an incentive to ensure that such extreme events never happen.

Lastly, one further point should be made: all we have shown is that those who were deceived behave differently than those who were not, primarily in terms of return rates. If deception is deemed rampant in the real world, are we better off testing behavior

¹¹ Thanks go to Matthew Rabin for suggesting this line of reasoning.

in a sterile, deception-free environment? Or would we get more widely relevant results if individuals were suspicious of the administrators, just as they may be in the real world with respect to economic transactions? To the extent that we want to use laboratory experiments to infer real world behavior, merely finding that deceived individuals behave differently than non-deceived individuals does not tell us which pool behaves more similarly in a lab to how they would in their natural environment. These issues are drawn out in more detail in Levitt and List (2007), which discusses the link between the laboratory and the real world, and how issues such as those discussed here influence the interpretability of laboratory experiments.

The debate in the deception literature thus far has been pursued at a philosophical level (something to which economists are not often accustomed), rather than as a cost-benefit analysis (something to which economists are accustomed).¹² This paper identifies some of the costs, but does not address the benefits. The benefits clearly accrue from the research questions that would remain unanswered because of a proscription on deception. Maintaining separate subject pools, conceivably with a higher rate paid for access to unadulterated subjects, might be one way to address this issue.

Ortmann and Hertwig (2001) discuss four characteristics of experimental economics which are not used by experimental psychologists, one of which is the proscription of deception. They argue that psychologists can learn from economists' practices. Roth (2001) replies that the public costs to deception may be lower for psychologists than they are for economists. Even if psychologists were to stop using deception as an experimental tool today, students would continue to be taught about research that used deception. Thus, participants in psychological experiments may remain suspicious for many years. Since economists have maintained the reputation of eschewing deception, the costs of deception carried out by a few researchers in economics may be higher for the profession as a whole. The effect of deception on non-deceived participants is even harder to measure. Those who have not yet been deceived

¹² For instance, some academics pose ethical considerations as reason for this rule. We suggest this is the role for the human subjects review boards at universities, not for one subfield of one discipline to decide independently of the rest of academe. See Friedman and Sunder (1994) for more discussion on this.

may talk to deceived participants or hear about experiments using deception in their economics classes, thus tainting their perceptions of experimentation.¹³ If these spillover effects are truly large, it may suggest that the maintenance of a “deception-free” participant list is not enough to limit the effects of deception on experimental outcomes.

¹³ Henrich (2001) makes the argument that this reputational spillover will be of lesser importance if experimentalists use subjects who are not students.

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Appendix A

Experimental Instructions, Round One:

Welcome to this experiment on decision making and thank you for being here. You will be compensated for your participation in the experiment, though the exact amount you will receive will depend on the choices you and others make, and on random chance (as explained below). Please pay careful attention to these instructions, as a significant amount of money is at stake.

Information about the choices that you make during the experiment will be kept strictly confidential. Your name will appear only on the payment-receipt form and will not be linked to any specific choices you make. You will not be asked to reveal your identity or the content of your decisions to anyone else (either the experimenter or other participants) at any time during or after the experiment. In order to maintain privacy and confidentiality, please do not speak to anyone during the experiment and please do not discuss your choices with anyone even after the conclusion of the experiment.

In this experiment each of you will be paired with somebody else who is in this room. You will not be told who that person is either during or after the experiment. Half of you will be assigned to the role of Player A and half of you will be assigned to the role of Player B. Each person playing the role of A will be assigned \$20 to begin the experiment. Those assigned the role of A (henceforth Player A) will have the opportunity to send some, all, or none of their \$20 endowment to a player playing the role of B (henceforth Player B). Each dollar sent to Player B will be tripled. For example, if Player A sends \$4, Player B will receive \$12. If Player A sends \$18, Player B will receive \$54. Player B will then decide how much money (if any) to send back to Player A and how much money (if any) to keep.

You will play this game in its entirety six times (or rounds). You will remain with the same partner and role for all six rounds. The first two rounds are just for practice so that

you learn the rules of the game. The next four rounds will be used to determine the payouts. In the end, one of these last four rounds will be chosen at random to determine you and your partner's payouts according to your payoff in that round.

Figure 1, below, illustrates the sequence.

PLAYER A => SEND between \$0 and \$20 to B.

PLAYER B => RECEIVE triple what A sent.

PLAYER B => SEND back some, none or all of new total to Player A.

Figure 1. One complete Round.

The earnings of A and B are given by:

A's earnings = 20 - [what (s)he gives to B] + [what (s)he receives from B]

B's earnings = [3 times what (s)he has been given by A] - [what (s)he returns to A]

Your role (A or B) will be told to you before the experiment starts. You will play this role in all subsequent rounds.

After all participants have made their choices in the first round, you will receive information about your earnings. Then you will play again and you will receive information about your earnings again. After the two practice rounds, you will play four more times. One of these last four rounds will be chosen at random to decide your actual payoffs. The dollar amount in that round, plus a \$5 show-up bonus, will be the amount you are paid for your participation in the experiment.

Once the experiment begins please remain silent and in your seat until it is over.

Computer Program Description

To make your decisions you will use the computer in front of you. Right now, you can see an initial waiting screen. The program will be activated when the instructions are finished.

Once the program is activated, you will go through two practice rounds to make certain that you correctly understand how the experiment will work. These practice rounds will be numbered “-1” and “0.” When we start, a new window will pop up and replace the initial waiting window. If you are Player A, the new window will resemble the one shown below. In this case, the upper-left of the screen informs you that you are in the first of 4 Rounds. The upper-right of the screen shows the time in which you are encouraged to make your decision. After you have completed the practice rounds, the experiment will begin. The rounds that determine your payoffs will be numbered “1” through “4.” The center of the screen will inform you that you are in round 1 and, if you are Player A, ask you for your decision. If you are Player B you will be asked to wait for the other player to make their choice. You may choose any discrete amount between \$0.00 and your full endowment (\$20.00 in the case of player A.) Your choice will be rounded to the nearest penny.

Round 1 of 4 Time Remaining 21

You have been assigned the role of Player A
You have an endowment of \$20.00
How much of that (if any) do you want to send to player B?

OK

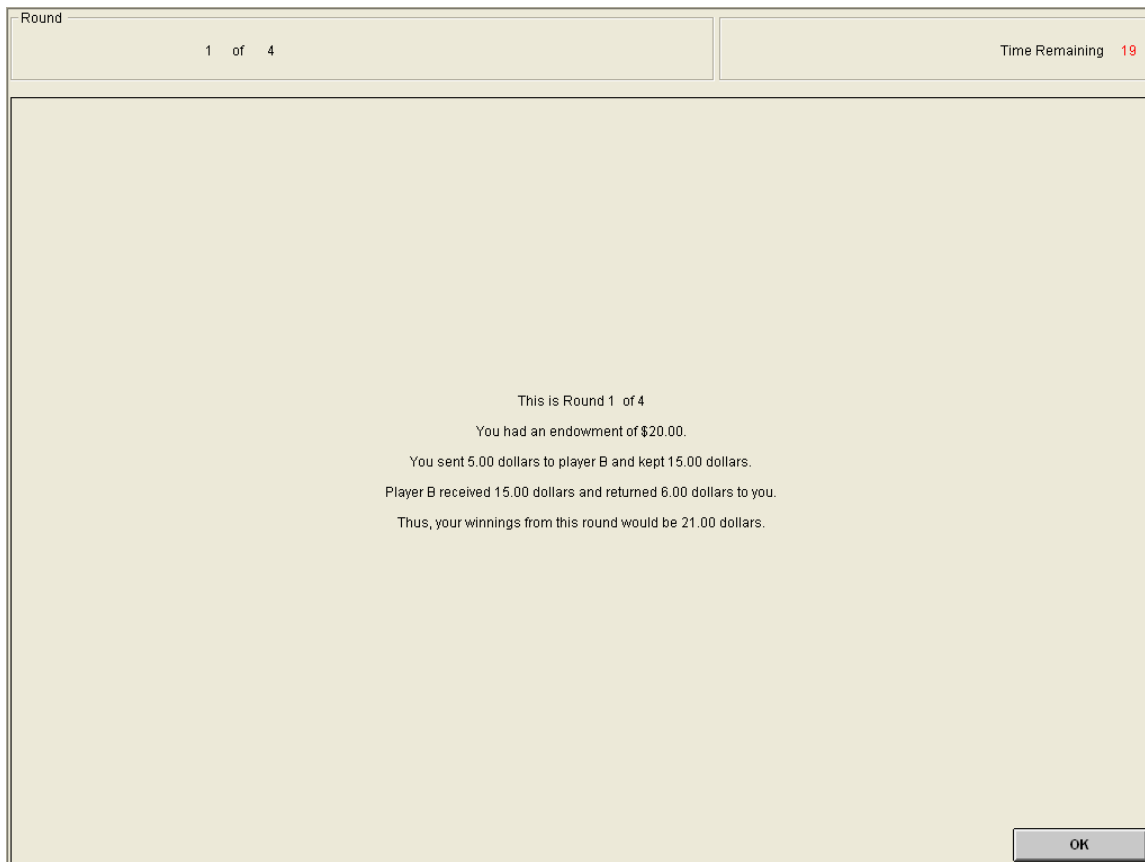
After Player A makes his (or her) decision, Player B will be given the option of sending some, all or none of his (or her) money back to Player A. The computer will display the screen below:

Round 1 of 4 Time Remaining 18

Player A received an endowment of \$20.00.
Player A sent you 5.00 dollars. This was tripled so you have received 15.00 dollars.
How much of that (if any) do you want to send back to player A?

OK

In this case, Player B would be able to send back any amount (s)he wants to Player A. Player B can choose to send “\$0.00,” “\$15.00” or any number in between. Player A will view a waiting screen while Player B makes their decision.



Finally, you will be shown the results of the round, including your payout and a summary of the choices you and the other player made. Player A's screen is shown above. Player B will view a similar screen. You can click OK, or wait until the "Time Remaining" expires, at which point you will automatically progress to the next screen.

Then, the next round will start. You will continue to play the role of Player A or B, and your partner will not change. Your final payout for your time here will be chosen randomly from the four non-practice rounds, so you will want to pay attention throughout the experiment.

After the last round is finished, please sit quietly while we process the results and issue your payment. You may go to the bathroom, work on homework or read during this period, which should last between 10 and 20 minutes.

At no point in the experiment will you be told who your partner is, nor will we identify you to your partner. There are no right or wrong choices in this experiment.

Example

Player A starts with \$20. She sends \$15.

Player B receives \$45, and sends back \$30.

HOW MUCH does each player have at the end of the round?

Rules

Please do not talk with anyone during the experiment. We ask everyone to remain silent through the end of the last round.

Your participation in the experiment and any information about your earnings will be used solely for research purposes. Your name and association to your decisions will be kept strictly confidential. If you have any remaining questions about the confidentiality agreement you will be given a chance to ask in just a minute.

We ask that you assist us in maintaining the integrity of the experiment. If at all possible, please do not discuss the experiment, your choices in it, or the outcomes with anyone, either during the experiment, or after you leave the Xlab.

Questions

Any clarification questions should be asked at this time. Please raise your hand and wait for a researcher to come to your desk. If you need to use the bathroom, please do so before the start of the experiment.

Table 1: Selection Effects in Round Two (Based on Player Characteristics)
Mean, Standard Errors and Number of Observations

	Total	Not Deceived	Deceived	t-stat: diff≠0
Proportion Returned to the Lab in Round Two	0.547 (0.031) 261	0.583 (0.043) 132	0.512 (0.044) 129	1.162
Proportion of Males who Returned to the Lab in Round Two	0.558 (0.049) 104	0.482 (0.067) 56	0.646 (0.071) 48	-1.682*
Proportion of Females who Returned to the Lab in Round Two	0.541 (0.040) 157	0.658 (0.055) 76	0.432 (0.055) 81	2.895***
Proportion of "High Passers" (Player A) who Returned to the Lab in Round Two	0.490 (0.050) 102	0.471 (0.071) 51	0.510 (0.071) 51	-0.393
Proportion of "Low Passers" (Player A) who Returned to the Lab in Round Two	0.724 (0.084)	0.800 (0.107) 34	0.643 (0.133) 47	0.927
Proportion of "High Passers" (Player B) who Returned to the Lab in Round Two	0.533 (0.075) 45	0.500 (0.096) 28	0.588 (0.123) 17	-0.564
Proportion of "Low Passers" (Player B) who Returned to the Lab in Round Two	0.538 (0.062) 65	0.655 (0.090) 29	0.444 (0.084) 36	1.706*

Standard errors are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. A player is a "High Passer" if they passed at least half of what they could pass in the Trust game, and a "Low Passer" otherwise.

Table 2: Selection Effects in Round Two (Based on Player Experience)
Mean, Standard Errors and Number of Observations

	Total	Not Deceived	Deceived	t-stat: diff≠0
Proportion of "High Receivers" (Player A) who Returned to the Lab in Round Two	0.500 (0.063) 64	0.464 (0.096) 28	0.528 (0.084) 36	-0.497
Proportion of "Low Receivers" (Player A) who Returned to the Lab in Round Two	0.556 (0.068) 54	0.586 (0.093) 29	0.520 (0.102) 25	0.480
Proportion of "High Receivers" (Player B) who Returned to the Lab in Round Two	0.505 (0.052) 93	0.549 (0.070) 51	0.452 (0.078) 42	0.922
Proportion of "Low Receivers" (Player B) who Returned to the Lab in Round Two	0.676 (0.078) 37	0.867 (0.091) 15	0.545 (0.109) 22	2.117**
Proportion of Players "Whose Actual Payout Was Not Determined by Last Round" who Returned to the Lab in Round Two	0.565 (0.037) 184	0.568 (0.051) 95	0.562 (0.053) 89	0.090
Proportion of Players "Whose Actual Payout Determined by Last Round" who Returned to the Lab in Round Two	0.506 (0.057) 77	0.622 (0.081) 37	0.400 (0.078) 40	1.967*

Standard errors are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Player A is referred to as a "High Receiver" if Player B sent back at least as half as much as he received in the Trust game. Player B is referred to as a "High Receiver" if their Player A passed at least half of what they could pass. The final two rows divide the sample into those whose payout was determined by the last round, and those whose payout was determined by any other round. Many passed zero back in the final round, since the final round was pre-announced. Thus, those whose last round was chosen as the actual round earned on average less money.

Table 3: Effect of Deception in Round One on Behavior in Round Two Games
Mean and Standard Errors

	Full Sample				Player A in Round One				Player B in Round One			
	Not Deceived	Deceived	t-stat: mean diff≠0	Levene test f-stat: sd ratio≠1	Not Deceived	Deceived	t-stat: diff≠0	Levene test f-stat: sd ratio≠1	Not Deceived	Deceived	t-stat: diff≠0	Levene test f-stat: sd ratio≠1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Full Sample												
Amount Kept in Dictator Game	14.584 (0.484)	14.923 (0.518)	-0.477	1.032	13.472 (0.676)	15.441 (0.697)	-2.029**	0.996	15.561 (0.658)	14.355 (0.771)	1.193	0.963
Risk Aversion (Holt-Laury Gambles if Consistent)	6.067 (0.156)	5.804 (0.236)	0.968	0.586**	6.171 (0.254)	5.867 (0.338)	0.732	0.660	5.975 (0.191)	5.731 (0.331)	0.685	0.514*
Risk Aversion (Holt-Laury Gambles Switchers)	0.026 (0.018)	0.138 (0.043)	-2.537**		0.028 (0.028)	0.118 (0.056)	-1.461		0.024 (0.024)	0.161 (0.067)	-2.117**	
Defected in Prisoner's Dilemma	0.653 (0.055)	0.677 (0.058)	-0.293		0.611 (0.082)	0.686 (0.080)	-0.651		0.692 (0.075)	0.667 (0.088)	0.223	
Number of Observations	77	65			36	34			41	31		
Panel B: Participants Played At Least 10 Prior Games in Laboratory												
Amount Kept in Dictator Game	15.179 (0.687)	15.032 (0.734)	0.146	1.103	13.941 (0.979)	14.882 (0.996)	-0.674	0.967	16.136 (0.922)	15.214 (1.125)	0.630	1.055
Risk Aversion (Holt-Laury Gambles if Consistent)	6.211 (0.233)	5.923 (0.404)	0.659	0.487**	6.411 (0.429)	6.143 (0.592)	0.376	0.639	6.048 (0.244)	5.667 (0.555)	0.724	0.338**
Risk Aversion (Holt-Laury Gambles Switchers)	0.026 (0.026)	0.133 (0.063)	-1.723*		0.000 (0.000)	0.125 (0.085)	-1.510		0.045 (0.045)	0.143 (0.097)	-1.017	
Defected in Prisoner's Dilemma	0.684 (0.076)	0.700 (0.085)	-0.138		0.824 (0.095)	0.706 (0.114)	0.792		0.571 (0.111)	0.692 (0.133)	-0.689	
Number of Observations	39	31			17	17			22	14		
Panel C: Participants Played Fewer Than 10 Prior Games in Laboratory												
Amount Kept in Dictator Game	13.974 (0.675)	14.824 (0.740)	-0.850	0.930	13.053 (0.947)	16.000 (0.985)	-2.154**	1.034	14.895 (0.939)	13.647 (1.057)	0.886	0.883
Risk Aversion (Holt-Laury Gambles if Consistent)	5.919 (0.206)	5.700 (0.272)	0.653	0.711	5.944 (0.286)	5.625 (0.375)	0.687	0.652	5.895 (0.305)	5.786 (0.408)	0.219	0.756
Risk Aversion (Holt-Laury Gambles Switchers)	0.026 (0.026)	0.143 (0.060)	-1.828*		0.053 (0.053)	0.111 (0.076)	-0.637		0.000 (0.000)	0.176 (0.095)	-1.961*	
Defected in Prisoner's Dilemma	0.622 (0.081)	0.657 (0.081)	-0.310		0.421 (0.116)	0.667 (0.114)	-1.504		0.833 (0.090)	0.647 (0.119)	1.252	
Number of Observations	38	35			19	18			19	17		
Panel D: Males												
Amount Kept in Dictator Game	14.111 (0.902)	14.516 (0.746)	-0.349	1.272	13.818 (1.394)	14.214 (1.065)	-0.230	1.346	14.313 (1.217)	14.765 (1.066)	-0.280	1.227
Risk Aversion (Holt-Laury Gambles if Consistent)	5.703 (0.291)	5.393 (0.339)	0.694	0.714	5.545 (0.511)	5.545 (0.593)	0.000	0.742	5.813 (0.356)	5.294 (0.418)	0.938	0.683
Risk Aversion (Holt-Laury Gambles Switchers)	0.000 (0.000)	0.097 (0.054)	-1.671		0.000 (0.000)	0.214 (0.122)	-1.661		0.000 (0.000)	0.000 (0.000)	NA	
Defected in Prisoner's Dilemma	0.769 (0.084)	0.581 (0.090)	1.508		0.818 (0.122)	0.643 (0.133)	0.948		0.733 (0.118)	0.529 (0.125)	1.178	
Number of Observations	27	31			11	14			16	17		
Panel E: Females												
Amount Kept in Dictator Game	14.840 (0.567)	15.294 (0.725)	-0.498	0.902	13.320 (0.774)	16.300 (0.936)	-2.532**	0.942	16.360 (0.723)	13.857 (1.143)	1.942*	0.715
Risk Aversion (Holt-Laury Gambles if Consistent)	6.271 (0.175)	6.214 (0.314)	0.170	0.534*	6.458 (0.276)	6.053 (0.415)	0.842	0.557	6.083 (0.216)	6.556 (0.444)	-1.063	0.632
Risk Aversion (Holt-Laury Gambles Switchers)	0.040 (0.028)	0.176 (0.066)	-2.122**		0.040 (0.040)	0.050 (0.050)	-0.158		0.040 (0.040)	0.357 (0.133)	-2.829***	
Defected in Prisoner's Dilemma	0.592 (0.071)	0.765 (0.074)	-1.645		0.520 (0.102)	0.714 (0.101)	-1.342		0.667 (0.098)	0.846 (0.104)	-1.163	
Number of Observations	50	34			25	20			25	14		
Panel F: Non-'unlucky'												
Amount Kept in Dictator Game	13.867 (0.604)	14.675 (0.649)	-0.912	0.976	13.593 (0.779)	15.043 (0.870)	-1.245	0.942	14.278 (0.976)	14.176 (0.990)	0.073	1.03
Risk Aversion (Holt-Laury Gambles if Consistent)	6.091 (0.213)	5.480 (0.265)	1.803*	0.858	6.192 (0.304)	5.667 (0.388)	1.079	0.888	5.944 (0.286)	5.267 (0.358)	1.499	0.763
Risk Aversion (Holt-Laury Gambles Switchers)	0.022 (0.022)	0.154 (0.059)	-2.214**		0.037 (0.037)	0.182 (0.084)	-1.679*		0.000 (0.000)	0.118 (0.081)	-1.504	
Defected in Prisoner's Dilemma	0.659 (0.072)	0.700 (0.073)	-0.397		0.630 (0.095)	0.652 (0.102)	-0.162		0.706 (0.114)	0.765 (0.106)	-0.378	
Number of Observations	45	40			27	23			18	17		

Standard deviations are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Some observation numbers are different than those reported. For the dependent variable 'Proportion Kept in Dictator Game', the observation numbers change to the following: For Panel C, there are 34 for 'Not Deceived' in the Full Sample; 17 for 'Not Deceived' in Player A. For the dependent variable 'Risk Aversion if Consistent', the observation numbers change to the following: For Panel A there are 75 'Not Deceived' and 56 'Deceived' in the Full Sample; 35 for 'Not Deceived' and 30 for 'Deceived' in Player A and 40 for 'Not Deceived' and 26 for 'Deceived' in Player B. For Panel B, there are 38 'Not Deceived' and 26 'Deceived' in the Full Sample; 14 'Deceived' in Player A; 21 'Not Deceived' and 12 'Deceived' in Player B. For Panel C, there are 37 'Not Deceived' and 30 'Deceived' in the Full Sample; 18 for 'Not Deceived' and 16 for 'Deceived' in Player A; 14 for 'Deceived' in Player B. For Panel D there are 28 'Deceived' in the Full Sample; 11 for 'Deceived' in Player A. For Panel E there are 48 'Not Deceived' and 28 'Deceived' in the Full Sample; 24 for 'Not Deceived' and 19 for 'Deceived' in Player A; 24 for 'Not Deceived' and 9 for 'Deceived' in Player B. For Panel F there are 44 'Not Deceived' and 33 'Deceived' in the Full Sample; 26 for 'Not Deceived' and 18 for 'Deceived' in Player A; 15 for 'Deceived' in Player B. For the dependent variable 'Risk Aversion, Switchers', the observation numbers change to the following: For Panel B there are 30 'Deceived' in the Full Sample, 16 'Deceived' in Player A, and 21 'Not Deceived' and 12 'Deceived' in Player B. For Panel C there are 17 'Deceived' in Player B. For Panel D there are 39 'Deceived' in the Full Sample; 22 'Deceived' in Player A. For the dependent variable 'Defected in Prisoner's Dilemma', the observation numbers change to the following: For Panel A, there are 75 for 'Not Deceived' in the Full Sample; 35 for 'Deceived' in Player A; 39 for 'Deceived' and 30 for 'Deceived' in Player B. For Panel B, there are 38 for 'Not Deceived' and 30 for 'Deceived' in the Full Sample; 21 for 'Not Deceived' and 13 for 'Deceived' in Player B. For Panel C, there are 37 for 'Deceived' in the Full Sample; 18 for 'Not Deceived' in Player B. For Panel D there are 26 for 'Not Deceived' in the Full Sample; 15 for 'Not Deceived' in Player B. For Panel E there are 49 'Not Deceived' in the Full Sample; 22 for 'Not Deceived' in Player A; 24 for 'Not Deceived' and 13 for 'Deceived' in Player B. For Panel F there are 44 'Not Deceived' in the Full Sample; 17 for 'Not Deceived' in Player B.

Appendix Table A: Holt-Laury Risky Lottery

Screen	Option A		Option B	
1	9/10 of \$8.80	1/10 of \$11.00	9/10 of \$0.55	1/10 of \$21.20
2	8/10 of \$8.80	2/10 of \$11.00	8/10 of \$0.55	2/10 of \$21.20
3	7/10 of \$8.80	3/10 of \$11.00	7/10 of \$0.55	3/10 of \$21.20
4	6/10 of \$8.80	4/10 of \$11.00	6/10 of \$0.55	4/10 of \$21.20
5	5/10 of \$8.80	5/10 of \$11.00	5/10 of \$0.55	5/10 of \$21.20
6	4/10 of \$8.80	6/10 of \$11.00	4/10 of \$0.55	6/10 of \$21.20
7	3/10 of \$8.80	7/10 of \$11.00	3/10 of \$0.55	7/10 of \$21.20
8	2/10 of \$8.80	8/10 of \$11.00	2/10 of \$0.55	8/10 of \$21.20
9	1/10 of \$8.80	9/10 of \$11.00	1/10 of \$0.55	9/10 of \$21.20
10	0/10 of \$8.80	10/10 of \$11.00	0/10 of \$0.55	10/10 of \$21.20

The respondents were presented with the above choices in sequence, starting with the first choice shown in Screen 1, and ending with the choice in Screen 2. We record in our analysis whether individuals (a) were consistent in their answers, and (b) if they were consistent, at what point they switch from Option A to Option B.