An Experimental Analysis of Risk-Shifting Behavior

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We study risk-shifting behavior in a laboratory experiment, a setup that overcomes methodological hurdles faced by empiricists in the past. The participants are high-level managers. We observe risk shifting in a simple setup, but less so in a setup with a continuation value. Reputation effects also reduce risk shifting. When combined, a continuation value and reputation effects eliminate risk shifting. Our findings shed light on environments in which risk shifting is unlikely to happen, and why earlier studies produced conflicting results. In particular, our findings show that managers' concerns with their own reputations are an important factor that mitigates risk shifting. (*JEL* G31, G32, G33)

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Risk shifting (asset substitution) is the result of a conflict of interest between a firm's creditors and its owners: Firms with high levels of debt benefit from increasing the risk of future cash flows, but firms with low levels of debt do not. It is easy to construct simple models that generate this behavior, and the concept is so well known that it is featured in standard corporate finance textbooks.[1] Empiricists refer to

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[1] See our model in Section 1. For an early example, see Fama and Miller (1972, chap. 4). Applications to incremental debt issuance or dividends are discussed by Black and Scholes (1973). Jensen and Meckling (1976) discuss various incentive problems caused by the presence of outside debt or equity, including risk shifting. See also Gavish and Kalay (1983), Green (1984), and Green and Talmor (1986).

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risk shifting when interpreting corporate choices on, for example, prices (Pichler, Stomper, and Zulehner 2008), technology (MacKay 2003), or the terms of debt financing (Ortiz-Molina 2006). But for both theoretical and empirical reasons, it has proven hard to directly test whether and when risk shifting happens (Andrade and Kaplan 1998; Gilje 2015).

We analyze the risk-shifting decisions made by managers in a laboratory experiment, a setup that allows us to overcome the hurdles that empiricists faced in the past when using observational data. Some of these hurdles are theoretical in nature: Simple and realistic extensions of the standard one-period model reduce or eliminate risk shifting. In a multiperiod setting, future profits are lost if a firm defaults, which reduces risk-shifting incentives, and a firm's managers may worry about adverse effects on their own reputation, should their firm fail to honor its obligations. These extensions are both realistic and important, but they (or the ambiguity in predictions that they cause) largely have been ignored in the existing empirical work on risk shifting.

Other important hurdles are empirical in nature. Risk taking is difficult to observe, and researchers have instead resorted to measuring ex post risk, usually by looking at the volatility of ROA. But such ex post risk measures are not reliable measures of *operating decisions* that increase risk. Compounding this issue, managers make many financial and operating decisions that are interdependent, so endogeneity can make it hard to measure whether risk taking is caused by debt.

By using a controlled laboratory experiment we avoid all of these concerns. The participants in our experiment were high-level managers and business owners enrolled in the Executive MBA program at Universidad de Los Andes (Chile), with many years of experience in business and used to making important business decisions. We asked them to picture themselves as the owners of a business that has debt outstanding and to choose between safe and risky future cash flows. The compensation that the participants could earn was significant, and it depended on the outcome of their decisions, thus giving them a strong incentive to maximize their pecuniary payoffs.

By directly looking at the decisions made by the executives, we can measure their risk-taking behavior without relying on empirical proxies that may not capture decisions accurately. Since we used a controlled experiment, we can examine how risk shifting is affected by continuation values and reputation concerns. Our data are not affected by hedging, covenants, or other issues that may have obscured the findings of earlier studies (see below for details). And, by varying debt levels, we can study the causal effects of exogenous changes in debt on risk choices.

To analyze the role of continuation values, we varied the context in which the participants made risk choices. We asked them to make multiple risk choices in three "situations": a "*Baseline* situation" that resembles the standard single-period risk-shifting model, and two situations with a continuation value, described either as a fixed value of future investment opportunities or as the possibility to make a second decision and earn an additional payoff. In all of these situations, the choices were either a fixed cash flow or a risky cash flow, and the firm's debt level was randomized. So, sometimes, the safe cash flow maximized the expected payoff, but at other times, the risky cash flow maximized it.

We further tested whether *reputation* concerns affected risk taking, by modifying the conditions under which some of the participants operated. Specifically, the participants in some of the sessions were informed that their performance in the experiment (total realized payoff of their firms; total number of bankruptcies suffered) would be revealed to all participants in the corresponding session (after the session's end). Subjects were assigned either to a session with this type of revelation at the end or to a session without it, but never both. Participants may care about their reputation with other participants, because they may expect it to affect future interactions (as EMBA students and as executives at their firms).[2] If participants felt that letting one's firm go bankrupt has negative connotations, and that doing so may be regarded as a sign of failure or of a lack of reliability, then this should reduce their willingness to take risk. Conceivably, the reputation effect may also enhance the willingness to take risk. Through repeated risk taking, matched with luck, a participant may accumulate a substantial total payoff, possibly creating a positive reputation effect (the participant may be proud of the achievement or may enjoy being admired by other participants).

The results from the *Baseline* situation confirm the predictions of the standard single-period risk-shifting model. There was significant risk shifting, confirming that managers act in a rational way (maximizing monetary expected utility) when facing a setting in which risk shifting is advantageous.[3] It also suggests that it should be *possible* to find evidence of risk shifting using observational data, despite the difficulties the literature has had so far in testing whether risk shifting happens.

In the situations with a continuation value or a repeated decision, the risky cash flow was chosen less frequently, consistent with the predictions of extended models. This illustrates conditions under which risk shifting

[2] See, for example, Holmström (1999). Similar effects may arise even without real consequences of being viewed in a particular way; these are denoted as *audience effects* (Bénabou and Tirole 2006; Andreoni and Bernheim 2009). Both effects have similar empirical implications. Distinguishing them is not the purpose of the experiment.

[3] This finding may seem unsurprising at first, but managers' decisions under uncertainty have been found to be inconsistent with expected utility maximization (see, e.g., March and Shapira 1987). Features, such as framing or reference points (see, e.g., DellaVigna 2009, 316), may affect risk-taking behavior.

should be harder to detect, and it also confirms that the participants in our experiment responded to incentives in a rational way.

The most striking results are found when considering the role of reputational concerns. There was less risk shifting in the sessions with "revelation", that is, when participants knew that their performance would be revealed to the other participants in that session. This suggests that the negative connotations of bankruptcy (or failure) have significant reputational effects that reduce risk taking. As with the results for the role of continuation values, this result sheds light on circumstances under which risk shifting is less likely to be found in observational data.

The reduction in risk-shifting incentives was strongest when the continuation value was *combined* with revelation: The effect of debt on risk taking was then weak and statistically insignificant. For many firms and managers, continuation values and reputation concerns are significant, and this finding suggests that it may be hard to find evidence of risk shifting when using observational data.

We make several contributions to the literature. First, we overcome methodological problems faced by earlier empirical work on risk shifting. This work largely has ignored the possibility that reputational concerns or continuation values mitigate risk shifting. This is not due to a lack of theoretical analysis of the effects. For example, the effect of continuation values has been studied in the banking literature.[4] Herring and Vankudre (1987) study the effects of future growth opportunities (that can be lost); Almeida, Campello, and Weisbach (2011) study the effects of possible future financing constraints; and Hirshleifer and Thakor (1992) argue that reputational considerations affect managerial risk taking. We test these effects in our experimental framework, and we obtain results consistent with the theoretical predictions. Our methodology is also unaffected by other factors that mitigate risk shifting but are usually ignored (partly due to observability issues) in the empirical work, including unobserved debt covenants (Smith and Warner 1979), hedging programs (Campbell and Kracaw 1990), or convertible securities (Green 1984).[5]

An additional hurdle we overcome is the difficulty of identifying *operating decisions* that increase risk. Instead, earlier studies resorted to measuring risk taking indirectly by using the volatility of ROA or stock

[4] Keeley (1990) argues that the introduction of competition reduced "franchise values" and led to excessive risk taking. However, it is unclear how competition affects risk taking (Boyd and De Nicoló 2005; Martinez-Miera and Repullo 2010) and the stability of a banking system (Allen, Carletti, and Marquez 2011).

[5] Additional factors could make it hard to find evidence or risk shifting. For some firms or managers, projects that change the risk-return structure of cash flows in a drastic way may be unavailable; a risk-avoiding conservative culture may moderate risk taking; agency problems may prevent managers from acting in the shareholders' interest.

prices. It is unclear, however, whether such increases in volatility are evidence of risk shifting (short-term volatility may have no cumulative effect, and increased cash flow risk may not cause short-term volatility). In fact, Gilje (2015) uses a more direct measure of risk taking and finds that ROA volatility fares badly in comparison. These issues are avoided in our experiment, in which the risk-taking decision is clearly identified.

We also overcome endogeneity concerns that plague research that uses observational data. Capital structure affects the attractiveness of risk shifting, and since the shareholders bear its cost ex ante, they may prefer to keep leverage low if risk shifting is possible (Leland 1998; Parrino and Weisbach 1999; Ericsson 2000). In our controlled experiment, we are able to identify causal effects and avoid endogeneity issues.

A second (and key) contribution is that we shed light on the conditions under which risk shifting is less likely to be expected. Reputational concerns play a surprisingly large role in mitigating risk shifting, and continuation values lost after a default also mitigate risk taking. Finding these results using high-level managers in a laboratory experiment is a first step in predicting whether and when evidence of risk shifting *could* be found by using observational data.[6] Our results suggest that it may be possible to find such evidence in settings that closely resemble the standard single-period setup. However, it is likely difficult to actually find examples of such settings (where reputational concerns and continuation values are negligible), so we interpret our results as predicting that it should be *hard* to find evidence of risk shifting by using observational data. It is thus not surprising that the small existing literature has produced conflicting results (Eisdorfer 2008 and Becker and Strömberg 2010 find indirect support; Andrade and Kaplan 1998 do not; Gilje 2015 finds evidence inconsistent with risk shifting).

A third contribution of our paper is that, to our knowledge, it is the first experimental paper to focus on pure corporate finance questions. Two prior papers deal with equilibrium selection and coordination issues (Cadsby, Frank, and Maksimovic 1990; Kale and Noe 1997). Pikulina et al. (2013) use experiments to study the impact of overconfidence on effort provision and investment, which has implications for the analysis of capital expenditure and M&A activity. Laboratory experiments as an empirical strategy have been successfully applied in other areas of finance (see, e.g., Biais, Bisière, and Pouget 2014; Gneezy, Kapteyn, and Potters 2003; Haigh and List 2005; Kirchler, Huber, and Stöckl 2012; Smith, Suchanek, and Williams 1998), and our paper shows that they have benefits in the area of corporate finance too. We show the benefits of our method by tackling questions that, so far, have proven

[6] Managers have a significant impact on their firms' decisions and performance; see Bertrand and Schoar (2003) or Adams, Almeida, and Ferreira (2005).

hard to answer convincingly. We use methods that are standard in areas in which experiments are more common, and we mitigate potential external validity issues (Levitt and List 2007a, 2007b; Camerer 2011) by employing high-level executives in this experiment. As a test run, we also performed an experiment with undergraduate students. The results are similar to those in the main experiment, consistent with findings in the literature (Fréchette 2014).

1. Hypotheses

The idea of "asset substitution" or "risk shifting" is simple and well understood, so we restrict our attention to a very simple setup that captures the relevant effects. Consider an owner-managed firm that has zero-coupon debt outstanding. Before the debt matures, an operating decision needs to be made that affects the riskiness of the cash flows the firm generates. After the cash flows are realized, the firm is liquidated, and the debt may or may not be repaid. If the debt is not repaid, the owner-manager's payoff is zero (the owner-manager benefits from limited liability in this case).

There are two possible choices for the operating decision: a safe choice that generates a certain cash flow of R_c and a risky choice that generates (with equal probability) either a high cash flow $R_h > R_c$ or a low cash flow $R_\ell < R_c$. To simplify, we assume that $R_\ell = 0$, and to make the problem interesting, we assume that the certain choice is superior in terms of efficiency, that is, $R_c > ½ \cdot R_h$. The firm owes debt in the amount of D, which can be repaid in full, unless the firm made the risky choice and was unlucky: $0 < D < R_c < R_h$. We assume that the owner-manager's choice cannot be specified as part of the debt contract. We abstract from other possible agency problems by not including an effort decision and by assuming that the owner-manager cannot hide or steal any of the realized cash flow. We normalize the owner-manager's reservation payoff to zero.

We initially assume that the owner-manager only cares about the payoff from the current operations—the firm's realized cash flow, less the repayment made on the debt. In this setup, the owner-manager prefers the risky choice if $½ \cdot (R_h - D) > R_c - D$, that is, if $D > 2 \cdot (R_c - ½\ R_h)$. The presence of sufficiently high debt induces the owner-manager to prefer the risky choice, instead of the more efficient certain choice, so there is "risk shifting" or "asset substitution".

We extend this simple setup in several ways. First, the participants in the experiment may care about what others think of them when their performance is revealed to all participants in a session. For example, they may like to be perceived as reliable, trustworthy, etc. Thus, they may suffer utility losses if it is revealed that they are unable to repay the debt and their firm went bankrupt. As previously discussed, we refer to these as reputation effects. To assess the reputation effects, we incorporate a utility loss v_B in the event of disclosed bankruptcy: if the owner-manager made the risky choice and the low payoff (zero) was realized, then the owner-manager's payoff is reduced by $v_B > 0$.[7] In this setup, the owner-manager prefers the risky choice if $\frac{1}{2}\cdot(R_h - D) - \frac{1}{2}\cdot v_B > R_c - D$, that is, if $D > 2\cdot(R_c - \frac{1}{2} R_h) + v_B$. Thus, all else equal, such a reputation effect makes risk shifting less attractive to the owner-manager, and the larger the utility loss, the less attractive risk shifting becomes.

Reputation concerns also may have the opposite effect. A participant may benefit from having earned the highest possible final payoff, either from being proud or because of the admiration of others. Suppose that the owner-manager's payoff is increased by $v_P > 0$ if the highest possible net payoff ($R_h - D$) is realized. In this setup, the owner-manager prefers the risky choice if $\frac{1}{2}\cdot(R_h - D + v_P) > R_c - D$, that is, if $D > 2\cdot(R_c - \frac{1}{2} R_h) - v_P$. All else equal, this type of reputation effect makes risk shifting *more* attractive to the owner-manager.

Second, the owner-manager may fear other losses if the debt cannot be repaid. The firm may have other positive-NPV investment opportunities available in a future period that are lost in the case of bankruptcy. (The investment opportunities may not be transferable, or they vanish if the firm is in bankruptcy.) Suppose the present value of this NPV is $v_F > 0$. In this setup, the owner-manager prefers the risky choice if $\frac{1}{2}\cdot(R_h - D + v_F) > R_c - D + v_F$, that is, if $D > 2\cdot(R_c - \frac{1}{2} R_h) + v_F$. All else equal, a fear of losing future investment opportunities makes risk shifting less attractive to the owner-manager, and the higher the value of those future investment opportunities, the less attractive risk shifting becomes.

Third, we include a more specific type of investment opportunity: the owner-manager may have the opportunity to make a similar choice again if the firm does not go bankrupt. This captures the idea of a firm's going-concern value: If there is no default, the firm earns future payoffs. For simplicity, we add a second decision with an identical project choice, debt level, and payoff structure. If the firm's debt is fully repaid in the first period, the incentives in the second period are identical to those in the one-period game: the owner-manager prefers the risky choice if $D > 2\cdot(R_c - \frac{1}{2} R_h)$. In the first period, the owner-manager prefers the risky choice if the threat of losing the expected continuation payoff (either $R_c - D$ or $\frac{1}{2}\cdot(R_h - D)$) is not too large. If $D < 2\cdot(R_c - \frac{1}{2} R_h)$, the owner-manager prefers the certain cash flows in both periods. If $D > 2\cdot(R_c - \frac{1}{2} R_h)$, the owner-manager prefers the risky choice in both periods if $\frac{1}{2}(R_h - D + \frac{1}{2}(R_h - D)) > R_c - D + \frac{1}{2}(R_h - D)$, that is, if $D > 2\cdot(R_c - \frac{1}{2} R_h) + \frac{2}{3}\cdot(R_h - R_c)$.

[7] The cost v_B in our setup is a reduced-form representation of a more elaborated "audience effect" model (see, e.g., Bénabou and Tirole 2006) or a reputation concern (see, e.g., Holmström 1999).

For intermediate debt levels, $D \in [2\cdot(R_c - \frac{1}{2} R_h)\,,\, 2\cdot(R_c - \frac{1}{2} R_h) + \frac{2}{3}\cdot(R_h - R_c)]$, the owner-manager makes the safe choice in the first period and the risky choice in the second period. So, as before, risk shifting happens if the debt level is sufficiently high, but a larger going-concern value makes it less likely.

In sum, high levels of debt outstanding create risk-shifting incentives, but these incentives are mitigated or eliminated in some settings with less restrictive assumptions, while they are amplified in others. We thus have the following hypotheses:

Hypothesis 1. (Baseline Situation)
If there are no concerns about reputation effects (positive or negative) and no concerns about possible losses in terms of going-concern value, then the presence of a sufficiently large debt makes risk shifting more likely (i.e., we should observe more choices of the risky cash flow).

Hypothesis 2. (Continuation Value Situation)
If bankruptcy causes the loss of NPVs from subsequent investment opportunities, then this makes risk shifting less likely than in the "Baseline Situation" for high debt levels.

Hypothesis 3. (Two-period Situation)
(*a*) If bankruptcy eliminates the possibility of repeating the decision problem a second time, then this makes risk shifting less likely than in the "Baseline Situation" for high debt levels. (*b*) Risk shifting is more likely in the second period than in the first period for high debt levels.

Hypothesis 4. (Reputation Effect)
(*a*) If the participants care about what others think of them in terms of reliability, trustworthiness, and their ability to avoid failure, then this makes risk shifting less likely for high debt levels when compared with an otherwise identical situation without reputation concerns. (*b*) If the participants care about what other agents think of them in terms of raw success, regardless of risks taken, then this makes risk shifting more likely for high debt levels when compared with an otherwise identical situation without reputation concerns.

2. Experimental Procedure

Our experiment consisted of five sessions, and it was designed to test the hypotheses described in the previous section. In those sessions, the participants were asked to make a series of choices between safe and risky projects. Each subject participated in only one session. In each session, the participants faced the same sequence of choices and payoff structures (i.e., *Baseline*, with a continuation value, and with a second period), so the only source of variation was the random level of debt outstanding. The distinguishing feature of the sessions was the condition under which the decisions were made; that is, whether or not each participant's performance would be revealed to all participants. Specifically, in two of the five sessions, the participants knew that their performance would be revealed to all participants in their session (after its conclusion); in the other three sessions, the participants knew that their performance would remain secret. A total of thirty-five individuals participated in the two sessions under the "*Revealing*" condition, and a total of twenty-four individuals participated in the three sessions under the "*No revealing*" condition.

The participants were students in the Executive MBA program at Universidad de los Andes in Chile. Students in this Executive MBA program held full-time high-level managerial positions or they managed their own companies. The median annual income of the participants was around US$110,000 (see the next section for details), which was approximately 15 times the median annual personal income in Chile. The currency used in the experiment was denoted as "Moneda" (M$), with an exchange rate of M$1 to 55 Chilean pesos (around US$0.10).

Each session started with the participants gathered in a classroom. We read the instructions aloud at the beginning of the experiment. (The instructions and the questionnaire are available as a supplement to this paper.) The experiment consisted of individual choice tasks asking the participants to make decisions in twelve scenarios, followed by a set of additional questions described below. In each of the twelve scenarios, the participants were primed to think of being the owner of a firm with debt outstanding. They were framed to choose between two projects available to the firm, project A and project B, where project A promised a safe payoff and project B a risky payoff.[8] Project A generated M$100 with certainty, and project B generated (with equal probability) either M$140 (if "successful") or M$0 (if "not successful"). The payoff to the firm's "owner" was the amount left after the firm's debt D was repaid. If the firm's payoff was insufficient to repay the debt D, then the firm went "bankrupt" and the "owner's" payoff was zero. The payoff values were chosen such that in the absence of debt, a risk-neutral agent prefers the safe project A over the risky project B, because its expected value is higher (M$100 > M$70).

The twelve scenarios were divided into three different *situations* (four questions each), according to whether there were continuation payoffs (a reduced-form continuation value, or the possibility of a second, repeated

[8] Framing has been widely used in economics experiments (see, e.g., Andreoni 1995).

decision). The payoff from each project depended on the level of debt outstanding. For each situation, we randomly chose four levels of debt, without replacement, from among six possible debt values: $D \in \{40,50,60,70,80,90\}$.[9] The participants could observe the outcome of their decisions only after their session ended, so the outcomes of early decisions could not influence later decisions.

In the *Baseline* situation, the participants faced a choice between Project A and Project B, without any continuation value. This describes the *Baseline* situation from the previous section. The six possible debt levels varied the attractiveness of the risky project. A risk-neutral agent prefers the risky project B if $D >$ M\$60 and the safe project A otherwise. Hypothesis 1 states that risk shifting happens in the presence of a sufficiently high level of debt (or is at least more likely in the case of a risk-averse agent).

In the second situation (the next four questions), which we call "*continuation value*," participants chose between projects A and B, like before, but now there was an extra gain of M\$30—framed as the expected NPV of future investment opportunities—if the project chosen was "successful" (i.e., if the firm did not go bankrupt). Under these conditions, a risk-neutral agent would strictly prefer the safe project A if $D <$ M\$90. Given the possible debt levels, from Hypothesis 2 we should expect less risk shifting than in the *Baseline* situation (i.e., project B is chosen less frequently for high debt levels).

The third situation, which we call "*two-period*", comprised the last four (of twelve) scenarios. Again, participants chose between projects A and B, but in case of "success", participants could choose a second time, under the same conditions (same debt level D). To avoid confusion, the second choice was between "project X" (safe, identical to project A) and "project Y" (risky, identical to project B). Risk-neutral agents would choose the risky project twice (B, then Y) if $D >$ M\$86⅔; they would choose the safe project twice (A, then X) if $D <$ M\$60; and they would choose the safe project first and then the risky project (A, then Y) for intermediate levels of debt. Hypothesis 3(a) states that there should be less risk shifting in the first decision than in the *Baseline* situation. Hypothesis 3(b) states that there should be more risk shifting in the second decision (Y rather than X) than in the first decision (A rather than B).

Each participant in the experiment faced the same twelve scenarios (with different debt levels, since the debt levels were randomized). The

[9] We used four debt levels (out of six) to limit the duration of the experiment, thus both enhancing participation among the EMBA students and avoiding participant boredom or "fatigue" during the experiment. The debt levels were randomly chosen to prevent participants from inferring which choices were "expected" from them.

only difference was in the revealing conditions: Under the *No revealing* condition, the participants were told that their performance would be kept secret; under the *Revealing* condition, the participants knew that the outcome would be revealed to the other participants in their session, after it ended. Specifically, the total amount of "Moneda" earned by each participant in a session was to be disclosed, as well as how often their firms went bankrupt.[10] The purpose was to test for the presence of reputation effects (concerning their reputation as viewed by their peers). Hypothesis 4(a) states that for each situation (*Baseline, Continuation value*, or *Two-period*) there should be less risk shifting relative to when the total amount earned and bankruptcy information is not revealed. Hypothesis 4(b) states the opposite.

There were two additional sets of questions after the twelve scenarios. Ten questions about choices between lotteries allowed us to construct a proxy for the participants' degree of risk aversion, following the approach of Holt and Laury (2002). After that, the participants were asked about their age, gender, work experience, current position, annual sales of their employer or the company they owned, and annual income.

The experiment took around twenty-five minutes to complete. Each subject participated in only one session and hence in only one condition (either *Revealing* or *No revealing*). Subjects were told in the instructions that they would receive a payment after the experiment, calculated as follows. First, they received a show-up fee of approx. US$6.00. Second, four of the twelve scenarios were randomly selected and the realized payoffs from those four questions were added (this is a standard procedure, used to prevent income effects in experimental settings). Third, one of the ten questions to elicit the participants' degree of risk aversion was selected randomly, and the realized payoff was added. To determine both types of payoff, we created random numbers. We used random numbers to determine whether a project would fail or succeed (with probability ½), allowing us to calculate the total realized payoff, as well as the total number of bankruptcies suffered by each participant. We also used random numbers to compute the payoff from the risk-aversion measurement question and to select questions to determine the compensation paid to the participants. This compensation was significant, with an average of around US$30.00 for a short investment of time (25 minutes, which, in terms of their reported income, we estimate was worth approximately US$19.00).

The participants had not been introduced to the concept of risk shifting before they participated in the experiment. They were also not students of

[10] These amounts are not equal to the participants' experiment compensation; details are explained below.

any of the experimenters, so experimenter effects, if any, are likely to be negligible. We performed an almost identical experiment with undergraduate students registered at the same university. We discuss this experiment and its results in Section 6.

3. Data

Table 1 summarizes the data collected. The *Risky project* dummy takes a value of 1 if the risky project was selected and 0 otherwise. The response rate for these questions is quite high: Subjects completed responses to 913 out of 944 questions (59 subjects $\times$ 16 project choices = 944; there were twelve questions, but the four questions regarding the *two-period* situation involved two project choices). The mean for the variable *Risky project* is 0.36.

The variable *Debt* takes values between 40 and 90, in increments of 10 (for simplicity of exposition we omit the "Moneda" symbol M$). We chose this range of possible debt levels to allow for interesting variation, since risk shifting is attractive to a risk-neutral agent with high debt levels from this range, but not with low debt levels. Thus, we designed the experiment to focus the variation of debt on the range of interest. As debt observations were part of the questions, we have complete data on this variable. The mean value of *Debt* is 65, as expected, given that debt values were randomly generated, with $D \in \{40,50,60,70,80,90\}$ and all levels equally likely.

Table 1
Summary statistics

Variable	Mean	p10	p50	p90	SD	N
Risky project	0.36	0	0	1	0.48	913
Debt	65	40	60	90	17.1	944
Revelation	0.59	0	1	1	0.49	944
Risk measure	5.7	4	5	8	1.86	560
Experience	12	6	10	20	5.5	912
Age	36	30	34	45	5.6	656
Female	0.2	0	0	1	0.4	944
Company sales (US$ million)	996	1	44	1,000	2,820	624
Income range	3.4	1	3	6	1.86	896

This table shows the summary statistics from the data collected in the experiment. The following variables are at the question-subject level: *Risky project* (dummy; yes=1; no = 0; N = total questions completed) and *Debt* (*Debt* $\in$ {40,50,60,70,80,90}; N = total number of observations, 59 subjects $\times$ (12 questions first period + 4 questions second period) = 944). The following variables are at the participant level (i.e., subject invariant): *Revelation* (dummy; yes=1; no = 0; N = total number of questions in both conditions combined, 59 subjects $\times$ 16 questions = 944), *Risk measure* (11 possible values, following Holt and Laury (2002); N = total number of observations with nonmissing values: 35 subjects gave valid answers, 35 $\times$ 16 questions = 560), *Experience* (in years; N = total number of observations with nonmissing values = 912), Age (N = total number of observations with nonmissing values = 656), *Female* (dummy, yes=1; no = 0; N = total number of observations with nonmissing values = 944), *Company sales* (N = total number of observations with nonmissing values = 624) and *Income range* (seven categories, see Figure 1; N = total number of observations with nonmissing values = 896).

The dummy *Revelation* takes a value of one if a question was answered in one of the *Revealing* sessions, that is, if the participant knew that the total amount of "Moneda" earned in the 12 scenarios and the total number of bankruptcies suffered would be revealed after the experiment. The dummy *Revelation* is zero otherwise. As 35 out of 59 subjects participated in a *Revealing* session, the mean for this variable is 0.59. There are two reasons for the higher fraction of participants in the *Revealing* sessions than in the *No revealing* sessions: one planned and one unexpected. Given estimates of how many people would participate in each session, our intention was to have at least as many *Revealing* subjects as *No revealing* subjects, to be conservative in our analysis. That is, we did not want to find ourselves in a situation in which the *Revealing* condition results are statistically weaker than the *No revealing* condition results simply due to a smaller sample size in the former. Second, fewer participants than were expected showed up during the last session (a *No revealing* session), tilting the sample toward having more participants for the *Revealing* condition.

At the end of the questions related to the choice of risky projects, we asked the participants to choose between two plain lotteries ten times, with increasing probabilities of winning (the payoffs in one of the lotteries were more dispersed than in the other). The goal was to measure each participant's risk aversion, following Holt and Laury (2002). The variable *Risk Measure* summarizes this information. This variable takes the value of the question number in which participants switched from choosing the lottery with less dispersed payoffs (the "safer" lottery) to choosing the lottery with more dispersed payoffs (the "riskier" lottery). A participant that switched earlier (with a lower probability of winning for both lotteries) is said to be more risk loving. If a participant always chose the riskier lottery, this measure takes a value of one; if the participant always chose the safer lottery, the measure takes a value of ten. And if the participant switched when answering question X, where $2 \leq X \leq 9$, the measure takes the value of X. The mean value for this variable is 5.7. Similar values were found by Holt and Laury (2002).

The participants were also asked to provide demographic information after answering the questions. In the experiments, we emphasized that this information was not going to be shared with other participants, even in the *Revealing* condition. Twenty percent of the participants were female; the mean age was 36 years; and the mean work experience was 12 years—most undergraduate programs in Chile last 5–6 years, thus graduates enter the labor market when they are 23–24 years old. The median participant in our experiment worked in a midsize company (annual sales of US$ 44 million).

We also asked the participants to report their annual income. We gave them a choice of seven income brackets, which we call income ranges 1

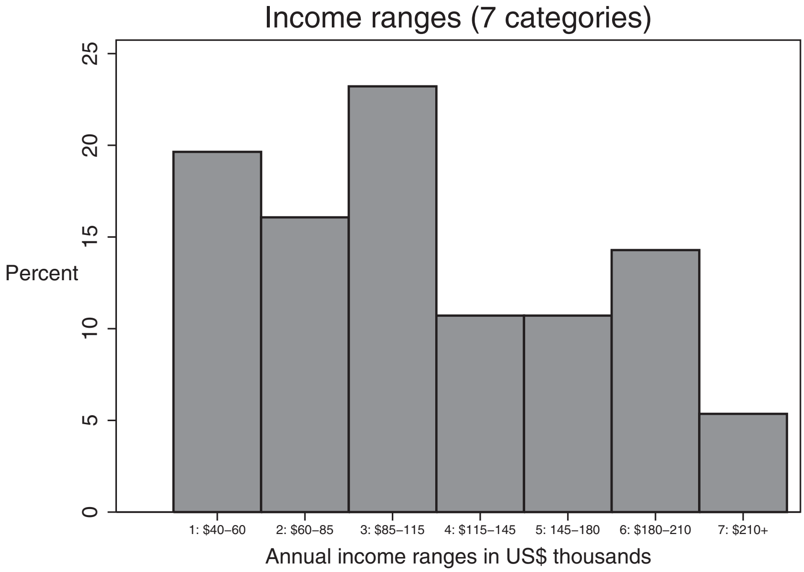

Figure 1
Income distribution of participants.
This figure shows the distribution of self-reported annual income ranges for the sample of executives that participated in the experiment. The seven income ranges are summarized on the horizontal axis (in thousands of US dollars).

to 7.[11] The average income range is 3.4. Given that the midpoints of income ranges 3 and 4 are US$100,000 and US$130,000, respectively, we estimate the average annual income to be US$112,000. This number is consistent with information provided to us by the MBA program office. Figure 1 shows the distribution of income ranges in our sample.

In Figure 2 we show the fraction of risky projects chosen (over the total number of projects chosen, either risky or safe) for all participants, by debt levels. The five panels show the results for the overall sample (top) and for the three situations: *Baseline* (center left), *Continuation value* (center right), the first period of the *Two-period* situation (bottom left), and the second period of the *Two-period* situation (bottom right). In each of the five panels, we separately display the results from the *Revealing* sessions (dashed) and from the *No revealing* sessions (solid). The differences between the risk choices in the *Revealing* and *No revealing* sessions are apparent to the eye. When faced with high debt levels, subjects in the *Revealing* condition tended to choose risky projects less often than subjects in the *No revealing* condition.

[11] We provided participants with after-tax (and other deductions) income ranges in Chilean pesos, as salaries in Chile are negotiated after taxes, so it is more likely that the participants knew their after-tax salary. We translate these ranges into annual gross income in U.S. dollars. We use the conversion rate at the time we ran the experiment: $500 Chilean pesos = US$1.

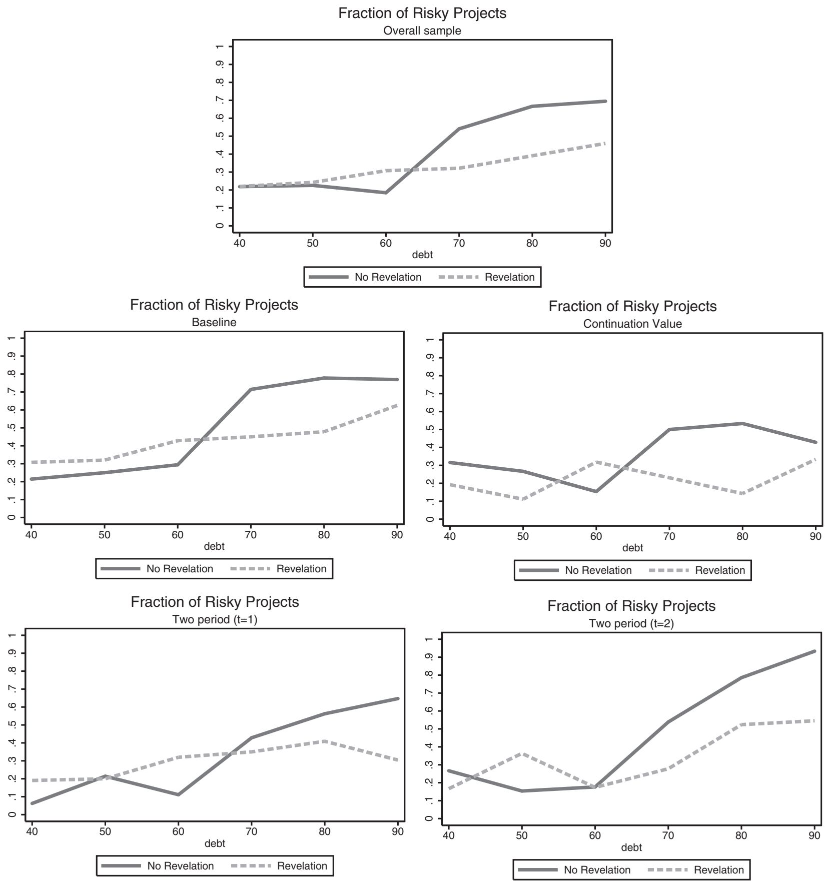

Figure 2
Fraction of risky projects by debt level, situation, and condition.
The five panels in this figure show the fraction of risky project choices over the total number of project choices (risky + safe) for each debt level. Each panel separately shows the results from the *No revealing* (solid) and *Revealing* (dashed) sessions for the entire sample (top) and the situations (*Baseline*, *Continuation value*, *Two-period* first decision, and *Two-period* second decision).

One could worry that these differences between the *Revealing* and *No revealing* results could be partially driven by heterogeneity across the groups of participants in the *Revealing* and *No revealing* sessions. A priori, this is unlikely, as we assigned sessions based on the participants' class schedules, alternating *Revealing* and *No revealing* sessions in an attempt to obtain random assignments.[12] This randomization was

[12] The order in which the sessions were run is irrelevant, as participants in different sessions were not given the opportunity to interact during the days allocated to experiments: The participants took evening or

Table 2
Univariate differences

	No revealing	Revealing	Difference
Risk measure	5.1 ($n=10$)	6 ($n=25$)	-0.9
Experience	13 ($n=23$)	11 ($n=34$)	2
Age	39 ($n=13$)	35 ($n=28$)	4**
Female	0.25 ($n=24$)	0.17 ($n=35$)	0.08
Income range	3.6 ($n=24$)	3.3 ($n=32$)	0.3

This table shows univariate differences in subjects' characteristics according to whether they participated in *Revealing* or *No revealing* sessions. Number of subjects: *n* in parentheses, next to variable means. Variable mean differences are shown in the third column. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

effective, as shown in Table 2, which presents the univariate differences in the subjects' characteristics according to whether they participated in *Revealing* or *No revealing* sessions. There are no significant differences in the subjects' characteristics, except for their average age: 39 years in the *No revealing* sessions vs. 35 years in the *Revealing* sessions.

To analyze whether the age differences had an effect, we split the participants in the *Revealing* sessions into two groups: an older group, with the same mean age as *No revealing* participants (39 years) and a younger group (mean age of 31). In Figure 3, we display the fraction of risky projects by debt levels (aggregated across all situations) for the groups of younger and older participants in the *Revealing* sessions and for all the participants in the *No revealing* session. Clearly, the behavior of older and younger subjects in the *Revealing* sessions was very similar, but very different from that of the participants in the *No revealing* sessions. This confirms that the differences in the average age cannot explain the differences in risk taking that we observe across the two conditions.

Another possible concern is that the participants' risk choices were not fully consistent with the theoretical predictions. Specifically, the participants, on average, chose the risky project when it is not optimal (with low levels of D) in approximately one in five questions. This type of "mistakes" is common in laboratory experiments, and the incidence of "mistakes" in our sessions is comparable to that of other experiments. We discuss this in more detail in Section 6, where we show that, while aesthetically not pleasing, the "mistakes" are not driving our results.

4. Empirical Methodology

We conduct two types of analyses. First, we pool the conditions (*Revealing* and *No revealing*) and situations (*Baseline*, *Continuation*

weekend classes on different schedules, and they did not share classes with EMBA students from other sessions.

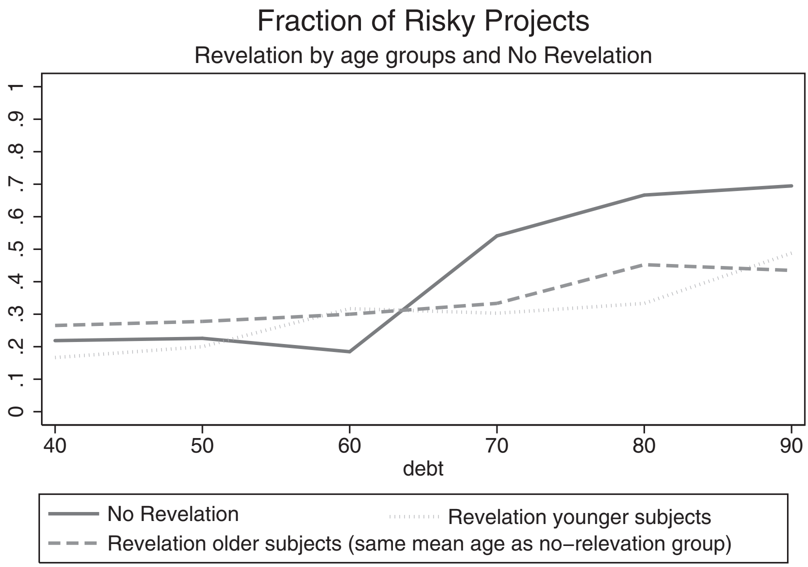

Figure 3
Fraction of risky projects by debt level: Revelation participants split by age.
This figure shows the fraction of risky project choices over the total number of project choices (risky + safe) for each debt level. It distinguishes participants in *No revealing* sessions from participants in *Revealing* sessions; the latter is split into two age groups (older and younger). For each condition, the results are aggregated across all situations (*Baseline*, *Continuation value*, and the *Two-period* situation).

value, and the *Two-period* situation) and study how debt and belonging to the *Revealing* condition affect risk taking. Our goal is to assess whether high debt levels increased the likelihood of participants choosing risky projects and whether that effect is weakened in the *Revealing* sessions, that is, if the outcomes are revealed to other participants. We split the debt levels into two groups, high ($D > 60$) and low ($D \leq 60$), and we estimate variations of the following model:

$$RiskyProject_{ij} = \alpha + \beta HighDebt_{ij} + \gamma Revelation_i + \delta HighDebt_{ij} * Revelation_i + \epsilon_{ij.} \tag{1}$$

The subscript i indexes subjects and j indexes questions. We estimate regressions using a linear probability model, since the main coefficient of interest, δ, captures the effect of an interaction term. We estimate specifications both with and without subject fixed effects. Given that the demographic variables we collected are subject-invariant, estimating a model with subject fixed effects controls for demographics, as well as for potential unobserved heterogeneity among participants.

In our second set of regressions, we estimate how debt affects the probability of taking a risky project, separately for conditions and situations. This allows us to measure how debt affects risk taking, under different circumstances (*Baseline* vs. *Two-period* vs. *Continuation value*, and *Revealing* vs. *No revealing*). In this specification we utilize debt levels as the main explanatory variable. Using a dummy variable for debt no longer eases the interpretation, as the key explanatory variable is not an interaction term (conditions are studied separately). We estimate variations of the following model:

$$RiskyProject_{ij} = \alpha + \beta D_{ij} + \epsilon_{ij.} \tag{2}$$

We run two sets of regressions for Equation (2): one set for the *Revealing* and one set for the *No revealing* condition. Each set comprises four separate regressions to study the choice of projects in the three different situations—*Baseline*, *Continuation value*, and *Two-period*—and for the second period in the *Two-period* situation. The decisions made in this later situation should be similar to those made in the *Baseline* situation, as there is no future period or continuation value after it. As before, we estimate specifications both with and without subject fixed effects.

In all our regressions, we adjust standard errors for heteroscedasticity and subject-level clustering. We cluster at the subject level since the errors may be correlated among the answers of a given participant.

5. Results

5.1 Pooled analysis

The results from estimating Equation (1) for the pooled sample are presented in Table 3. The first four columns show coefficients estimated without subject fixed effects; the last column shows coefficients estimated with subject fixed effects. The results show that the risky project was more frequently chosen with high debt levels; the coefficients are significant throughout, both statistically and economically. *Revelation* (the dummy indicating whether a question was answered by a participant in a *Revealing* session) has a small but insignificant effect; this coefficient is dropped in the fixed-effects estimation, as it is subject-invariant. More importantly, the coefficient of the interaction between Revelation and *High debt* (>*60*) is strongly negative and statistically significant, in both specifications.

Even at this high level of aggregation, these results suggest that the participants tended to make expected utility-maximizing decisions (they were more likely to choose the risky project when the debt level was high), and that reputational concerns reduced the participants' willingness to choose the risky project.

Table 3
Linear probability model without and with subject fixed effects

Variable	Risky project	Risky project	Risky project	Risky project	Risky project
High debt (>60)	0.2548***		0.2538***	0.4245***	0.4258***
	(0.0599)		(0.0599)	(0.0866)	(0.0836)
Revelation		-0.0961	-0.0934	0.0468	
		(0.0615)	(0.0620)	(0.0739)	
High debt (>60)*Revelation				-0.2892**	-0.2710**
				(0.1144)	(0.1121)
Subject fixed effects	No	No	No	No	Yes
N	913	913	913	913	913
R-squared	0.0703	0.0097	0.0795	0.1014	0.1215
Subject clusters	Yes	Yes	Yes	Yes	Yes

This table presents the results from linear probability model regressions of Equation (1), without subject fixed effects (first four columns) and with subject fixed effects (last column). The dependent variable is the dummy *Risky project*. The explanatory variables are the dummy variables *High debt* (>*60*) and *Revelation*, as well as their interaction term. Standard errors are adjusted for heteroscedasticity and clustered at the subject level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

5.2 Results by condition and situation

We now separately analyze how debt affects risk taking for the situation and condition under which a participant answered questions. More precisely, we estimate Equation (2) for all possible condition-situation pairs (*Revealing* vs. *No revealing*; *Baseline* vs. *Continuation value* vs. *Two-period* first decision vs. *Two-period* second decision). We first present the estimates for these eight regressions without subject fixed effects (in Table 4) and then with subject fixed effects (in Table 5).

The results in panel A of Table 4 (*No revealing* condition) show that participants were more likely to choose the risky project for high debt levels exactly when the model predicts that they should: in the *Baseline* situation (Hypothesis 1) and in the second decision of the *two-period* situation (Hypothesis 3(b)). In both situations, the setup is that of the standard single-period risk-shifting model. Supporting the interpretation that participants responded to incentives in an expected utility-maximizing way is the lower incidence of the risky project choice exactly when the model predicts it should be lower: when there was a continuation value (Hypothesis 2; the coefficient is statistically insignificant) and in the first decision of the *two-period* situation (Hypothesis 3(a)). In the latter case, the coefficient remains large, but the overall likelihood of the risky project is reduced. That is evident from an inspection of predicted probabilities based on these regressions, which we present in the Appendix (Figure A1). The predicted probabilities are considerably lower in the first period of the *two-period* situation, relative to the *Baseline* situation.

Panel B of Table 4 presents the results for the *No revealing* condition. The coefficients are smaller throughout, and their statistical significance

Table 4
Linear probability model

Panel A: No revealing

Situation Variable	Baseline Risky project	Cont. value Risky project	2 period (t = 1) Risky project	2 period (t = 2) Risky project
Debt	0.0141***	0.0048	0.0124***	0.0159***
	(0.0037)	(0.0036)	(0.0029)	(0.0029)
Subject fixed effects	No	No	No	No
N	96	96	95	87
R-squared	0.2174	0.0283	0.2029	0.2993
Subject clusters	Yes	Yes	Yes	Yes

Panel B: Revealing

Situation Variable	Baseline Risky project	Cont. Value Risky project	2 period (t = 1) Risky project	2 period (t = 2) Risky project
Debt	0.0061**	0.0020	0.0035	0.0071**
	(0.0029)	(0.0026)	(0.0028)	(0.0032)
Subject fixed effects	No	No	No	No
N	139	140	136	124
R-squared	0.0449	0.0066	0.0167	0.0645
Subject clusters	Yes	Yes	Yes	Yes

This table presents the results from linear probability model regressions of Equation (2), without subject fixed effects. The dependent variable is the dummy *Risky project*. The explanatory variable is *Debt*. Panel A shows the results for participants in the *No revealing* sessions, and panel B shows those for participants in the *Revealing* sessions. In both panels, the first column shows the results for the *Baseline* situation; the second column the results for the *Continuation value* situation; the third column the results for the *Two-period* situation, first decision; and the fourth column the results for the *Two-period* situation, second decision. Standard errors are adjusted for heteroscedasticity and clustered at the subject level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

is reduced or lost. The incidence of the risky project remains highest where the model predicts it should: in the *Baseline* situation and in the second decision of the *two-period* situation. But compared with the results for the *No revealing* condition (panel A), the coefficients are less than half as large. Thus, in all situations, reputation reduced a participant's willingness to choose the risky project, consistent with Hypothesis 4.

Reputation considerations and continuation values or repeated decisions have similar effects: Both made risk taking less attractive to the participants. When combined, they eliminated the willingness to choose the risky project. In the *Revealing* sessions, the coefficients are small and statistically insignificant, both with a continuation value and in the first decision of the *two-period* situation. There was no risk shifting in those situations. That has important implications for empirical research on risk shifting that relies on observational data: Continuation values, multiple-period settings, and reputational concerns are realistic extensions of the basic single-period setup. Thus, in long-lived firms in which managers care about maintaining their reputations, it may be hard to impossible

Table 5
Linear probability model with fixed effects

Panel A: No revealing

Situation Variable	Baseline Risky project	Cont. value Risky project	2 period (t = 1) Risky project	2 period (t = 2) Risky project
Debt	0.0172***	0.0065*	0.0110***	0.0153***
	(0.0030)	(0.0035)	(0.0028)	(0.0029)
Subject fixed effects	Yes	Yes	Yes	Yes
N	96	96	95	87
R-squared (within)	0.4289	0.1037	0.2459	0.4065
Subject clusters	Yes	Yes	Yes	Yes

Panel B: Revealing

Situation Variable	Baseline Risky project	Cont. value Risky project	2 period (t = 1) Risky project	2 period (t = 2) Risky project
Debt	0.0062**	0.0023	0.0059*	0.0070**
	(0.0030)	(0.0024)	(0.0029)	(0.0032)
Subject fixed effects	Yes	Yes	Yes	Yes
N	139	140	136	124
R-squared (within)	0.0694	0.0155	0.0737	0.0933
Subject clusters	Yes	Yes	Yes	Yes

This table presents the results from linear probability model regressions of Equation (2), with subject fixed effects. The dependent variable is the dummy *Risky project*. The explanatory variable is *Debt*. Panel A shows the results for participants in the *No revealing* sessions, and panel B shows those for participants in the *Revealing* sessions. In both panels, the first column shows the results for the *Baseline* situation; the second column the results for the *Continuation value* situation; the third column the results for the *Two-period* situation, first decision; and the fourth column the results for the *Two-period* situation, second decision. Standard errors are adjusted for heteroscedasticity and clustered at the subject level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

to find any trace of risk shifting, because very likely there is no risk shifting.

Table 5 presents results corresponding to those in Table 4, but here the regressions include subject fixed effects. Fixed effects change the magnitudes somewhat, but qualitatively the results are unchanged. Thus, it is unlikely that observed or unobserved subject heterogeneity is driving the results.

One may wonder whether a linear probability model is best suited to estimating how debt affects risk taking. As a robustness test, we repeated the regressions using a probit framework. The results are consistent with the results reported here; they can be found in the Appendix.

In sum, the evidence is consistent with debt having a positive effect on risk taking at high debt levels, but this effect is reduced or eliminated if there are continuation values or repeated decisions, or if there are possible reputational concerns. Specifically, the evidence supports Hypotheses 1, 2, 3(a), 3(b), and 4(a). It is inconsistent with Hypothesis 4(b), that reputational considerations enhance risk taking. Thus, in connection with risk taking, managers are more concerned with possibly damaging their reputation (losing face) than with having a reputation for aggressively taking risks.

6. Robustness

The results in Tables 3, 4, and 5 support the predictions of our model, and the risk choice patterns are consistent with the choices a rational expected utility-maximizing agent would make. However, a few empirical concerns could be raised, and we address them in this section.

First, as discussed in Section 3, for a nontrivial number of project choices, participants chose the risky project even if it was not optimal to do so according to the model. Our model predicts that a risk-neutral or risk-averse expected utility maximizer should choose the safe project in all situations if the debt level D is below 60. However, Figures 2 and 3 show that the fraction of risky project choices for low debt levels is well above zero. We thus need to analyze whether there are clear patterns in the incidence of "mistakes", and whether these could explain the results.

Second, our participants were high-level managers or owners of businesses, enrolled in an Executive MBA program. One may worry whether the results extend to other populations. We performed a similar experiment with undergraduate students as subjects, and we discuss the results below.

6.1 "Mistakes" and "inconsistent" responses

Behavior seemingly inconsistent with models of rational decision making is common in experiments such as ours, where participants make many choices (see Charness et al. 2013 for a review). Possible explanations for such "mistakes" include a participant's poor understanding of the tasks, fatigue, lack of attention, impatience, fondness for risk, or the fact that humans often make mistakes for no apparent reason.

We now examine whether such "mistakes" bias our results in a systematic way. We define a new dummy variable, *Inconsistent*, which takes the value of one if a subject chooses a risky project in a situation where a rational risk-neutral or risk-averse agent would unambiguously prefer the safe project; and it takes a value of zero otherwise. Specifically, *Inconsistent* has a value of one if a participant chose the risky project with a debt level of 40 or 50 in the *Baseline* situation or in the second decision ($t = 2$) of the *Two-period* situation. It also takes a value of one if a participant chose the risky project with a debt level of 80 or less in the *Continuation value* situation or in the first decision ($t = 1$) of the *Two-period* situation. Of all the answers given by the participants, 16% had the dummy *Inconsistent* set to one.

We also examine whether there were inconsistencies in the answers used to construct the Holt and Laury (2002) risk measure. We define a dummy *Invalid risk measure*, which takes a value of one if we cannot construct a valid risk measure because a participant crossed the selection of lotteries (from riskier to safer) more than once; the dummy takes a value of zero otherwise.

Interestingly, although the fraction of "inconsistent" risky project choices is not large, almost all the participants (93%) made at least one type of "inconsistent" choice during the experiment (i.e., including both project choices and the Holt and Laury (2002) risk-aversion questions). However, the incidence of "mistakes" or "inconsistent" choices in our experiment is not unusual (see Charness et al. 2013). The question we need to address is whether the mistakes and inconsistencies bias the results, or whether they are pure noise and can thus be ignored when analyzing the overall patterns in the data.

There is no apparent difference in the incidence of mistakes across the three situations, so the mistakes are not entirely caused by fatigue. Similarly, the mistakes cannot be entirely caused by initial misunderstandings about the task at hand that the participants resolved after answering the first few questions (partly because the participants could revisit their earlier choices). The results are not driven by participants consistently making mistakes, either. The regressions with subject fixed effects uses only within-subject variation and produce results very similar to those from regressions without subject fixed effects. Furthermore, our results are robust to including risk aversion dummies and to excluding subjects having inconsistent answers in Holt and Laury's (2002) risk measurement procedure (see Table A2 in the Appendix).

A different test consists of measuring whether some participant characteristics (age, income, gender, etc.) drive the incidence of inconsistent choices, or whether inconsistent choices in the risk-aversion measure questions are positively correlated with inconsistent project choices.

Table 6 presents the results for these tests. The first column shows that the relation between inconsistent choices in the Holt and Laury test and inconsistent project choices is negative and statistically insignificant. This suggests that inconsistent project choices were not systematically caused by some participants having difficulties when comparing risk choices (otherwise, the coefficient would have to be positive). The second column shows that inconsistent project choice was not systematically related to any of the participants' characteristics. The third column shows similar results for subject characteristics after including the dummy *Invalid risk measure*. As in the first column, in this specification the *Invalid risk measure* also has a negative coefficient, but now it is statistically significant. Again, this refutes the proposition that some subjects made inconsistent choices in a systematic way. The fourth column in

Table 6
Determinants of potential inconsistent behavior (linear probability model)

Variable	Inconsistent	Inconsistent	Inconsistent	Inconsistent
Invalid risk measure	-0.0587		-0.1296***	
	(0.0358)		(0.0477)	
Age		-0.0044	-0.0027	-0.0007
		(0.0110)	(0.0102)	(0.0128)
Experience		0.0065	0.0058	0.0077
		(0.0108)	(0.0103)	(0.0121)
Income range		0.0202	0.0158	0.0152
		(0.0136)	(0.0125)	(0.0151)
Female		-0.0347	-0.0861	-0.0822
		(0.0592)	(0.0652)	(0.0640)
Risk measure				-0.0054
				(0.0170)
N	913	603	603	429
R-squared	0.0061	0.0200	0.0415	0.0345
Id clusters	Yes	Yes	Yes	Yes

This table presents the results from regressions of inconsistent project choices on various explanatory variables. The dependent variable is the dummy *Inconsistent*. The explanatory variable in the first column is the dummy *Invalid risk measure*. The explanatory variables in the second column include various characteristics of the participants. The third column combines the explanatory variables from the first and second columns. The explanatory variables in the fourth column include the participants' characteristics and their risk measure. Standard errors are adjusted for heteroscedasticity and clustered at the subject level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 6 shows results from a regression using only data from participants with a valid risk measure. Again, there is no evidence that inconsistent project choices were driven by participant characteristics.

Overall, the evidence suggests that inconsistent choices were unrelated to subject characteristics, and that it was not the case that some subjects consistently made "mistakes." Thus, it is likely that inconsistent choices were simply pure noise and inconsequential for our main findings.

6.2 Sessions with undergraduate students as participants

A second robustness check concerns the type of participant in our experiment. The standard approach in the literature is to rely on undergraduate subjects as participants, because they are readily available and require smaller financial incentives, thus making experiments less costly to run. We are interested in the risk-shifting problem, a decision problem faced by senior managers, so it seems appropriate to require the participants to have business experience (like the Executive MBA students who participated).

According to the literature, the findings of an experiment should not depend on whether undergraduate students are used as participants (see Fréchette 2015 for a detailed survey of the behavioral differences between professionals and undergraduates in experiments). We can directly test this, since we ran a separate experiment using undergraduate students as subjects.

The participants in this additional experiment were senior-year undergraduate students registered at Universidad de los Andes, majoring in business and economics. Specifically, the subjects were commercial engineering students, a five-year undergraduate program equivalent to a double major in business and economics. In Chile, students choose a specific program of study when they enter college. Thus, their majors are determined in the first year of their studies. As senior-year students, the participants thus were comfortable with problems and decisions businesses and managers face, but without having any senior management experience.

To make participation easier, the *Revealing* and *No revealing* participants took part in one session, unlike the main experiment, which had five separate sessions. So the performance of the *Revealing* participants was to be shared with all participants, including the *No revealing* participants. To avoid confusion, we did not read any instructions out loud and instead asked the participants to read their instructions in silence. (The instructions for *Revealing* participants were different, since they were told that their performance would be announced to all participants after the session ends.) We assigned 39 subjects to the *No revealing* treatment and 20 to the *Revealing* treatment.

A second change in the experiment, compared with the main experiment, was that the level of compensation expected by the participants was lower. The compensation *structure* was the same as in the main experiment, but the exchange rate was M$1 to 8 Chilean pesos instead of 55 Chilean pesos. This cost-saving change seemed reasonable, since the opportunity cost of time was lower for undergraduate participants than for the executives that participated in the main experiment. Students earned an average US$4.50 for a short investment of time (twenty-five minutes, on average).

A third change concerns the demographics questions. We asked students about their grade point average, and we did not ask them about business experience, income, or company sales. A fourth change is that the continuation value in the *Continuation value* situation was lower, M$20 (in the main experiment it was M$30); the experiment with undergraduate students preceded that using Executive MBA students, and we decided that a more significant continuation value would be appropriate in the main experiment.

The summary statistics of the undergraduate student sample are summarized in Table 7. The risk measure values are similar to those of the Executive MBA students; the undergraduate students were much younger; and the fraction of female participants was higher. In Chile, student grades are scaled from 1 to 7, where obtaining a grade below 4

Table 7
Summary statistics, sessions with undergraduate students

Variable	Mean	p10	p50	p90	SD	N
Risky project	0.45	0	0	1	0.5	926
Debt	65	40	60	90	16.7	944
Revelation	0.34	0	0	1	0.5	944
Risk measure	5.5	4	5	7	1.5	656
Age	23	22	23	25	1.1	880
Female	0.49	0	0	1	0.5	944
GPA	5	4.5	5	5.4	0.4	848

This table shows the summary statistics from the data collected in the additional sessions using 59 undergraduate students. The following variables are at the question-subject level: *Risky project* (dummy; yes=1; no = 0; N = total questions completed) and *Debt* (*Debt* $\in$ {40,50,60,70,80,90}; N = total number of observations, 59 subjects $\times$ (12 questions first period + 4 questions second period) = 944). The following variables are at the participant level (i.e., subject invariant): *Revelation* (dummy; yes=1; no = 0; N = total number of questions in both conditions combined, 59 subjects $\times$ 16 questions = 944); *Risk Measure* (11 possible values, following Holt and Laury 2002; N = total number of observations with nonmissing values: 41 subjects gave valid answers, 41 $\times$ 16 questions = 656); *Age* (continuous variable; N = total number of observations with nonmissing values = 880); *Female* (dummy, yes=1; no = 0; N = total number of observations with nonmissing values = 944); and *GPA* (grade point average, continuous variable from 1.0 to 7.0; N = total number of observations with nonmissing values = 848).

implies failing, and 7 is the equivalent to an A+. It is very uncommon for commercial engineering students to obtain grades above 6, and the average GPA of 5.0 in our sample is within the norm. As before, there are no significant differences between the characteristics of the participants in the *Revealing* and *No revealing* conditions (not reported).

Table 8 presents results corresponding to those in Table 3 above. The results are qualitatively the same: There is risk shifting when it is predicted (with high debt levels), but this effect is reduced if there is revelation. Using subject fixed effects does not affect these results.

Table 9 presents results equivalent to those in Table 4. Again, the results are qualitatively the same. There are two main differences. First, the undergraduate *Revealing* participants were even less willing to choose the risky projects than the Executive MBAs that participated in the main experiment: The coefficients are insignificant throughout, while in the main experiment the coefficients for the *Baseline* situation and the second decision of the *Two-period* situation are significant. A possible explanation is that reputational concerns are even more important for undergraduate students. Second, the undergraduates chose the risky project more often in the *Continuation value* situation than in the main experiment (where the coefficient was insignificant in the *No revealing* condition), which is likely due to the lower amount of continuation value in this experiment (M$20 instead of M$30 in the main experiment).

Table 10 presents results corresponding to those in Table 9, except that subject fixed effects are included in the regressions. Like before (see Tables 4 and 5), this does not change the results qualitatively.

Table 8
Linear probability model without and with subject fixed effects, undergraduate participants

Variable	Risky project	Risky project	Risky project	Risky project	Risky project
High debt (>60)	0.2043***		0.2043***	0.2976***	0.3102***
	(0.0644)		(0.0643)	(0.0791)	(0.0789)
Revelation		0.0243	0.0249	0.1601*	
		(0.0484)	(0.0479)	(0.0844)	
High debt (>60)*Revelation				-0.2732**	-0.2980**
				(0.1263)	(0.1277)
Subject fixed effects	No	No	No	No	Yes
N	926	926	926	926	926
R-squared	0.0422	0.0005	0.0428	0.0598	0.0689
Subject clusters	Yes	Yes	Yes	Yes	Yes

This table presents the results from linear probability model regressions of Equation (1), without subject fixed effects (first four columns) and with subject fixed effects (last column). The dependent variable is the dummy *Risky project*. The explanatory variables are the dummy variables *High debt* (>*60*) and *Revelation*, as well as their interaction term. Standard errors are adjusted for heteroscedasticity and clustered at the subject level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 9
Linear probability model, undergraduate participants

Panel A. No revealing

Situation Variable	Baseline Risky project	Cont. value Risky project	2 period ($t=1$) Risky project	2 period ($t=2$) Risky project
Debt	0.0120***	0.0090***	0.0055*	0.0087***
	(0.0025)	(0.0029)	(0.0029)	(0.0030)
Subject fixed effects	No	No	No	No
N	156	156	155	143
R-squared	0.1651	0.0995	0.0342	0.0810
Subject clusters	Yes	Yes	Yes	Yes

Panel B. Revealing

Situation Variable	Baseline Risky project	Cont. value Risky project	2 period ($t=1$) Risky project	2 period ($t=2$) Risky project
Debt	0.0025	0.0038	0.0034	-0.0039
	(0.0047)	(0.0040)	(0.0038)	(0.0039)
Subject fixed effects	No	No	No	No
N	80	80	80	76
R-squared	0.0076	0.0159	0.0143	0.0180
Subject clusters	Yes	Yes	Yes	Yes

This table presents the results from linear probability model regressions of Equation (2), without subject fixed effects. The dependent variable is the dummy *Risky project*. The explanatory variable is *Debt*. Panel A shows the results for participants in the *No revealing* sessions, and panel B shows those for participants in the *Revealing* sessions. In both panels, the first column shows the results for the *Baseline* situation; the second column the results for the *Continuation value* situation; the third column the results for the *Two-period* situation, first decision; and the fourth column the results for the *Two-period* situation, second decision. Standard errors are adjusted for heteroscedasticity and clustered at the subject level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Table 10
Linear probability model with fixed effects, undergraduate participants

Panel A. No revealing

Situation **Variable**	**Baseline** **Risky project**	**Cont. value** **Risky project**	**2 period ($t=1$)** **Risky project**	**2 period ($t=2$)** **Risky project**
Debt	0.0111***	0.0098***	0.0063**	0.0091***
	(0.0027)	(0.0030)	(0.0029)	(0.0031)
Subject fixed effects	Yes	Yes	Yes	Yes
N	156	156	155	143
R-squared (within)	0.1759	0.1466	0.0639	0.1067
Subject clusters	Yes	Yes	Yes	Yes

Panel B. Revealing

Situation **Variable**	**Baseline** **Risky project**	**Cont. value** **Risky project**	**2 period ($t=1$)** **Risky project**	**2 period ($t=2$)** **Risky project**
Debt	0.0016	0.0051	0.0005	-0.0015
	(0.0050)	(0.0038)	(0.0039)	(0.0038)
Subject fixed effects	Yes	Yes	Yes	Yes
N	80	80	80	76
R-squared (within)	0.0033	0.0409	0.0004	0.0053
Subject clusters	Yes	Yes	Yes	Yes

This table presents the results from linear probability model regressions of Equation (2), with subject fixed effects. The dependent variable is the dummy *Risky project*. The explanatory variable is *Debt*. Panel A shows the results for participants in the *No revealing* sessions, and panel B shows those for participants in the *Revealing* sessions. In both panels, the first column shows the results for the *Baseline* situation; the second column the results for the *Continuation value* situation; the third column the results for the *Two-period* situation, first decision; and the fourth column the results for the *Two-period* situation, second decision. Standard errors are adjusted for heteroscedasticity and clustered at the subject level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Overall, the experiment with undergraduate students as participants produced results similar to those in the main experiment. This is consistent with the literature and confirms that our results are not driven by using a particular subject population.

7. Conclusions

The theoretical argument for risk shifting is simple. In fact, its simplicity may be a concern. As we have shown, several natural generalizations or extensions of a basic model reduce the attractiveness of risk shifting, compared with the basic model. And when we tested these predictions empirically, in the context of a controlled experiment, we found strong support for them.

Reputational considerations have a strong moderating effect on risk-taking: When agents worry about the possible downside of risky choices for their own reputation, they find risky choices less attractive. Continuation values also have a moderating effect. Both are realistic features of situations in which managers make decisions that affect the riskiness of their firms' cash flows, but empiricists have so far largely ignored these possibilities. In light of these findings, it is likely that risk shifting is not a significant concern for many firms. Thus, there is little hope of finding evidence of risk shifting, and care must be taken when arguing that risk shifting may be the cause of some other effects.

The empirical literature on risk shifting is small and the results are inconsistent. As we have explained, empiricists have struggled to overcome the methodological problems that arise when observational data are used. Our experimental setup allows us to avoid these issues. We have a clean measure of the riskiness of our participants' decisions, and we can control the environment in which decisions are made, allowing us to test hypotheses from a simple base model or several extensions that nest the base model. A controlled experiment thus offers great advantages, and given the challenges faced in many areas of empirical corporate finance, experiments should be useful to test theories in those areas.

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Appendix

A.1 Predicted Probabilities

Based on the regression results reported in Table 4, we compute the predicted probabilities of choosing the risky project for different situations (*Baseline*, *Continuation value*, and *Two-period*, first decision) and for the two conditions (*Revealing*, *No revealing*). Figure A1 shows these predicted probabilities.

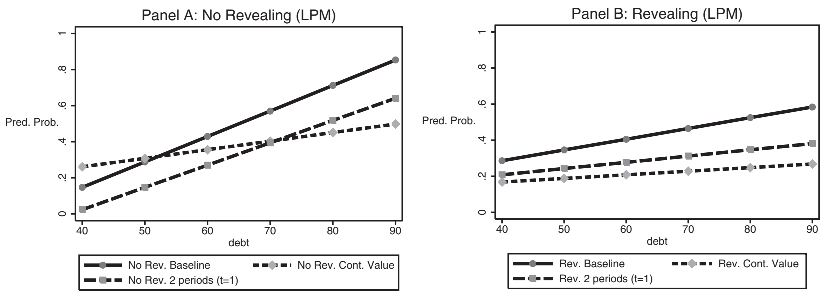

Figure A1
Predicted probabilities, linear probability model.
This figure shows the predicted probabilities of choosing the risky project for each debt level. The solid line shows the probabilities for the *baseline* situation; the long-dashed line shows the probabilities for the first period of the *two-period* situation; and the short-dashed line shows the probabilities for the *continuation value* situation. The predictions were obtained from the regressions reported in Table 4. The left panel shows the results for the *No revealing* condition, and the right panel shows the results for the *Revealing* condition.

Table A1
Probit regressions

Panel A. No revealing

Situation Variable	Baseline Risky project	Cont. value Risky project	2 period ($t = 1$) Risky project	2 period ($t = 2$) Risky project
Debt	0.0393***	0.0127	0.0386***	0.0466***
	(0.0129)	(0.0097)	(0.0105)	(0.0122)
Subject fixed effects	No	No	No	No
N	96	96	95	87
R-squared	0.167	0.0215	0.1715	0.2365
Subject clusters	Yes	Yes	Yes	Yes
Marginal effect	0.0129***	0.0047	0.0116***	0.014***
	(0.0027)	(0.0035)	(0.0023)	(0.0018)

Panel B. Revealing

Situation Variable	Baseline Risky project	Cont. value Risky project	2 period ($t = 1$) Risky project	2 period ($t = 2$) Risky project
Debt	0.0155**	0.0068	0.0103	0.0198**
	(0.0078)	(0.0085)	(0.0084)	(0.0094)
Subject fixed effects	No	No	No	No
N	139	140	136	124
R-squared	0.0332	0.0063	0.014	0.0506
Subject clusters	Yes	Yes	Yes	Yes
Marginal effect	0.0059**	0.0020	0.0035	0.0069**
	(0.0027)	(0.0025)	(0.0028)	(0.0030)

This table presents univariate probit regressions. The dependent variable is the dummy *Risky project*. The explanatory variable is *Debt*. Panel A uses data from subjects participating in the *No revealing* sessions, and panel B uses data from subjects participating in the *Revealing* sessions. Marginal effects are reported in the column below. Standard errors are adjusted for heteroscedasticity and clustered at the subject level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

A.2 Probit Regressions

To capture nonlinearities, we repeat the regressions in Table 4 using estimations instead of linear estimations. Table A1 presents the results from the univariate probit regressions corresponding to those in Table 4. The patterns are similar to those found by using a linear probability model (Table 4). The marginal effects, shown in the bottom row of the panels, are also very similar.

A.3 Regressions with Controls

In Table A2, we repeat the regressions reported in Table 4, adding control variables that include various participant characteristics, as well as risk-aversion dummies (one dummy for each value of the *Risk Measure* variable; this reduces the sample size since the measure could not be constructed for all participants).

All our results hold when including the additional controls. Regarding the coefficients of the control variables with demographic information, *Income* positively correlates with risk taking, as expected; and *Age* and *Experience* have opposite effects on risk taking, due to the high collinearity between these variables. The coefficient of the dummy *Female* is positive,

Table A2
Linear probability model, with controls

Panel A. No revealing

Situation Variable	Baseline Risky project	Cont. value Risky project	2 period (t = 1) Risky project	2 period (t = 2) Risky project
Debt	0.0134*	0.0021	0.0092	0.0184**
	(0.0057)	(0.0068)	(0.0083)	(0.0041)
Age	0.4562***	0.2981***	0.2025***	-0.1399***
	(0.0318)	(0.0016)	(0.0533)	(0.0116)
Experience	-0.6343***	-0.4008***	-0.2740***	-0.0757***
	(0.0493)	(0.0260)	(0.0666)	(0.0128)
Income range	1.0738***	0.8111***	0.5082***	0.0974***
	(0.0816)	(0.0440)	(0.1154)	(0.0195)
Female	0.6345***	0.7604***	0.2038***	0.1582***
	(0.0569)	(0.0339)	(0.0414)	(0.0207)
Risk dummies	Yes	Yes	Yes	Yes
N	28	28	28	20
R-squared	0.7258	0.2036	0.5110	0.6957
Id clusters	Yes	Yes	Yes	Yes

Panel B. Revealing

Situation Variable	Baseline Risky project	Cont. value Risky project	2 period (t = 1) Risky project	2 period (t = 2) Risky project
Debt	0.0039	-0.0014	0.0022	0.0031
	(0.0038)	(0.0031)	(0.0036)	(0.0037)
Age	0.0904*	0.0111	0.0202	0.0211
	(0.0470)	(0.0291)	(0.0392)	(0.0672)
Experience	-0.0790*	-0.0065	-0.0228	0.0116
	(0.0384)	(0.0285)	(0.0320)	(0.0552)
Income range	0.0053	0.0215	0.0168	0.0581
	(0.0351)	(0.0175)	(0.0315)	(0.0402)
Female	0.4187**	0.0351	0.1158	0.0065
	(0.1784)	(0.1008)	(0.2277)	(0.2198)
Risk dummies	Yes	Yes	Yes	Yes
N	83	84	80	78
R-squared	0.2782	0.3418	0.2346	0.281
Id clusters	Yes	Yes	Yes	Yes

This table presents Linear Probability regressions. The dependent variable is the dummy *Risky project*. The explanatory variables are *Debt*, a set of subject-invariant controls (*Age*, *Experience*, *Income range*, *Female*) and a set of dummy variables that capture subjects' risk aversion measure (10 dummies in total, as the risk measure variable can take 11 possible values). Panel A uses data from subjects participating in the *No revealing* sessions and Panel B uses data from subjects participating in the *Revealing* sessions. Standard errors are adjusted for heteroscedasticity and clustered at the subject level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

which, at first glance, seems inconsistent with prior findings that women are more likely to be risk averse than men. However, in our regressions, women are more likely to take a risky project *conditional* on other observables, including their risk-aversion measure. The *unconditional* correlation between the dummy *Female* and the risk aversion measure is negative in our sample, consistent with prior studies.

In unreported results, we also looked at whether the participants' characteristics affect their choices, by interacting the variable *Debt* with the participants' demographics. None of these interaction coefficients are significant. Age and experience do not differentially affect the likelihood of choosing the risky project for different debt levels. This is not surprising, given that the differences in the participants' age and experience are not substantial. The interaction coefficients for income and gender are not significant either.