Does Risk Shifting Really Happen? Results from an Experiment

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Do firms increase the riskiness of their cash flows in the presence of high levels of debt? So far, the evidence is mixed and difficult to interpret, because risk taking is hard to measure and because some assumptions from the theory do not map well into broad empirical settings. We overcome these problems by using a controlled experiment, with high-level managers as subjects. Our findings shed light on the environments in which risk-shifting is likely to happen (and could be detected using observational data): when managers' reputation concerns are relatively unimportant, and when firms' going concern values are low.

Keywords: Experimental Corporate Finance, Risk Shifting, Asset Substitution

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1. Introduction

Risk shifting (asset substitution) is caused by a conflict of interest between a firm's creditors and its owners: Firms with high levels of debt outstanding benefit from making decisions that increase the risk of future cash flows, even if they wouldn't do so with low levels of debt (i.e., the NPV of such decisions would be zero or negative). It is easy to construct examples or simple models that generate this behavior,[1] and the concept is so well-known that it features in standard corporate finance textbooks. Risk shifting often shapes the interpretation of corporate policies in the empirical literature, e.g. firms' price setting behavior (Pichler et al., 2008), production technology choices (MacKay, 2003), or the terms of debt financing (Ortiz-Molina, 2006). This wide acceptance of risk shifting as an agency problem is striking, given how hard it has proved to find evidence that supports the idea (see, Andrade and Kaplan, 1998, and Gilje, 2015).

Do firms really increase the riskiness of their cash flows in the presence of high levels of debt? That question is hard to answer for many reasons, both theoretical and empirical. The standard risk shifting model is a one-period model in which the firm is liquidated at the end. However, with more periods, a firm's going-concern value (or continuation value) may keep it from taking on excessive risk, because that value is lost in bankruptcy. Similarly, the decision makers at a firm may worry about adverse reputation effects after a default, giving them an incentive to avoid a default. Alternatively, the shareholders may themselves face agency problems, caused by the separation of ownership and control, and a CEO may prefer a less risky strategy than the shareholders, to protect her own position. Empirical concerns are important, too. Risk taking is hard to measure, and researchers commonly use measures of ex-post risk, instead. These measures (the volatility of ROA is the most common) may not be reliable measures of *operating decisions* that increase risk. Furthermore, unobservable to researchers, the attractiveness of risk taking may be limited by debt covenants or hedging programs. Compounding these concerns, firms make many decisions, some of which affect the attractiveness of risk taking, so endogeneity can make it hard to measure how much risk-shifting is caused by debt.

[1] For an early example, see Fama and Miller (1972, ch. 4). Applications to incremental debt issuance or dividends are discussed in 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).

In this paper, we overcome these issues by using data on managers' decisions made in a controlled environment: an experiment. The participants in our experiment are high-level managers and business owners, enrolled in the Executive MBA program at Universidad de Los Andes (Chile). They have many years of experience in business, and they are 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, so the participants had strong incentives to maximize their payoffs.

Our experimental design allows us to observe decisions and to vary the level of debt outstanding. By looking directly at the decisions made by the executives, we can measure their risk-taking behavior without having to rely on empirical proxies that may not capture the decisions accurately. By varying the debt levels, we have an exogenous measure of indebtedness that we can use to study the causal effect of debt on risk choices. Our data is also free from concerns about agency conflicts between shareholders and managers that may taint the study of the risk shifting problem. Similarly, covenants or hedging are not mentioned in the experiment, so they cannot affect the decision-making.

We also manipulate the context in which decisions are made, to study what strengthens or weakens risk shifting behavior. The baseline situation involves a choice between a safe cash flow and a risky cash flow, with a given debt level. The expected payoff to the firm (before the debt is repaid) is higher if the safe cash flow is chosen, so risk shifting happens if the risky cash flow is chosen. All participants faced this baseline situation (with randomized debt levels). All participants also faced two extended situations, in which an additional payoff could be earned if the firm did not default. This was either presented as the NPV of future investment opportunities (with a fixed value), or as an opportunity to repeat the decision (with the same debt level) and earn a second payoff.[2]

Reputation concerns may have an effect on risk shifting behavior, too. We modified some of the experimental sessions, in order to test whether *reputation effects* reduce the participants'

[2] These extensions capture realistic scenarios firms face. When firms go bankrupt, the value of their operations disintegrates quickly due to management distraction, delays, lack of trust from stakeholders, lack of cash, etc. Investment opportunities are even more likely to be lost, in particular if a firm is liquidated.

incentive to take risk. A participant may care about her reputation with other participants, because that may affect their future interactions (as executives at their firms, or as MBA students).[3] 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 *reputation* should reduce the 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). To test for the presence of a reputation effect, 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 it ended. Subjects were assigned either to a session with this type of revelation at the end, or to a session without it, but never both.

We find strong and significant evidence of risk shifting in the baseline setting: A high debt level makes risk taking more likely, consistent with the predictions of a simple one-period model. This finding lends support to the basic idea of risk shifting, support that has been hard to find. We also find support for the theoretical predictions coming from extensions of the basic model. The introduction of a continuation value if the firm survives (either a fixed continuation payoff or the possibility of making the same decision again) reduces risk shifting. The reputation effect has a negative impact on risk shifting as well, suggesting that negative connotations of bankruptcy have an important effect on the participants' decisions. The reductions in risk shifting from these extensions are significant. Importantly, when the continuation value and the reputation effect are combined, the effect of debt on risk taking is weak and statistically insignificant.[4]

We make several contributions to the literature. Our paper is the first to use data produced in a controlled experiment to test for the presence of risk shifting behavior. Our experimental design allows us to analyze the role of some key assumptions and the validity of the theoretical

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[3] See e.g. Holmström (1999). In our case, these reputation effects may also be labeled as *audience effects*, which may arise if a participant cares about how she is perceived by others even if there are no real consequences of being viewed in a particular way. Both effects have similar empirical implications. Distinguishing them is not the purpose of the experiment. For more on audience effects, see, e.g., Bénabou and Tirole (2006) and Andreoni and Bernheim (2009).

[4] We ran similar experiments with undergraduate business students registered at the same university. The results were very similar, providing further support for our findings. Results are shown in Appendix A. Neither undergraduate nor MBA students at this university were exposed to the risk shifting (asset substitution) problem in their coursework.

predictions. It also allows us to overcome some of the difficulties faced by existing studies that use observational data.

For many reasons, it is hard to find evidence of risk shifting using observational data. Not surprisingly, the findings in the (small) empirical literature are ambiguous. Eisdorfer (2008) and Becker and Strömberg (2010) find indirect support; Andrade and Kaplan (2001) do not; and Gilje (2015) finds evidence inconsistent with risk shifting.

One reason for the difficulties is that the attractiveness of risk taking may be reduced by factors that are difficult to observe: Debt covenants (Smith and Warner 1979), hedging programs (Campbell and Kracaw 1990), convertible securities (Green 1984), reputation concerns (Diamond 1989; Hirshleifer and Thakor 1992), future financing constraints (Almeida et al. 2011), or the possible loss of a firm's continuation value and growth opportunities (Herring and Vankudre 1987).[5] By using a controlled experiment, we can avoid these issues.

Endogeneity is also an issue when using observational data. Capital structure decisions affect the attractiveness of risk taking — since the cost of risk shifting is borne ex ante by shareholders, managers may prefer to keep leverage low if risk shifting is possible (Parrino and Weisbach 1999; Leland 1998; Ericsson 2000). Similarly, managers make decisions that mitigate risk shifting incentives, about debt covenants, hedging, or the use of convertible securities. Again, by using a controlled experiment, we can identify causal effects and avoid these issues.

Compounding these issues, it is hard to identify the risk-taking decisions of a firm. Some earlier studies have therefore resorted to measuring the effects of risk taking, using the volatility of ROA or stock prices as indirect evidence of risk taking. 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, in a recent study, 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.

[5] The continuation value is particularly important in banking. Keeley (1990) argues that the introduction of competition reduced "franchise values," leading 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 et al. 2011).

Our findings also shed light on what factors should be controlled for when testing for the presence of risk shifting using observational data. Where earlier studies did not find evidence of risk shifting, the reason may be (besides measurement and endogeneity issues) that the firms had valuable growth opportunities that would be lost in bankruptcy. This should be of particular importance in industries that rely on intangible assets like human capital or good relations with stakeholders, and less so in industries that use homogenous fixed assets. On the other hand, firms that face unexpected opportunities to add significant risk may do so if continuation values are low or decreasing (the S&L crisis and the recent financial crisis fit this description). Measures of an industry's risk-taking culture could be useful, too: If managers at financial institutions think that successes help their careers much more than failures damage them, then reputational concerns may increase risk taking; on the other hand, if managers at auditing firms, say, feel threatened by possible failure, then reputational concerns should reduce risk taking. Alternatively, managers' characteristics may contain useful information regarding their reputational concerns. For example, managers that specialize in dealing with financially distressed firms may assign a high value to their reputation, due to repeated interactions with lenders; while managers near retirement age may be much less concerned about future interactions, or about what others think of them.[6]

Our paper is also one of very few experimental corporate finance papers. Two prior papers deal with equilibrium selection and coordination issues (see Cadsby, Frank and Maksimovic (1990) on the Myers and Majluf (1984) underinvestment problem; and Kale and Noe (1997) on the Grossman and Hart (1980) hold-out problem in hostile tender offers). 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. Our paper is the first to evaluate how assumptions coming from a corporate finance theory of individual decision making affect the nature of its empirical results.

2. 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

[6] The structure of executive compensation contracts is also likely to be important (Tufano 1996; Coles et al. 2006).

to be made, which affects the riskiness of the cash flows that the firm will generate. After the cash flows have been 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 (she benefits from limited liability in that 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, i.e., $R_c > ½·R_h$. 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 assuming that there is no effort decision, and 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 cares only about her 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 $½·(R_h – D) > R_c – D$, i.e., if $D > 2·(R_c – ½ R_h)$. 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 owner-manager may care about what others think of her, when her performance is revealed. For example, an agent may like to be perceived as reliable, trustworthy, etc. Thus, she may suffer utility losses if it is revealed she is unable to repay the debt the firm owes and must go bankrupt. As discussed previously, we refer to these as reputation effects. In order 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 $½·(R_h – D) – ½·v_B > R_c – D$, i.e., if $D > 2·(R_c – ½ R_h) + v_B$. 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.

[7] The cost v_B in our set up 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. Holmstrom 1999).

Concerns about reputation may also have the opposite effect. An agent may benefit from having earned the highest possible final payoff, either from being proud, or because of admiration by 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 $\frac{1}{2}\cdot(R_h - D + v_P) > R_c - D$, i.e., 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 she cannot repay the debt. The firm may have other positive-NPV investment opportunities available in a future period, which 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 $\frac{1}{2}\cdot(R_h - D + v_F) > R_c - D + v_F$, i.e., 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 gets to earn 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 her fear of losing her expected continuation payoff (either $R_c - D$ or $\frac{1}{2}\cdot(R_h - D)$) is not too large. With low debt levels, $D < 2\cdot(R_c - \frac{1}{2} R_h)$, the owner-manager prefers the certain cash flows in both periods. If $D > 2\cdot(R_c - \frac{1}{2} R_h)$, she prefers the risky choice in both periods if $\frac{1}{2}(R_h - D + \frac{1}{2} (R_h - D) > R_c - D + \frac{1}{2} (R_h - D)$, i.e., if $D > 2\cdot(R_c - \frac{1}{2} R_h) + \frac{2}{3}\cdot(R_h - R_c)$. 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 in others they are amplified. We thus have the following hypotheses:

H1: "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).*

H2: "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.*

H3: "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; and (b) risk shifting is more likely in the second period than in the first period for high debt levels.*

H4: "Reputation Effect". *(a) If agents care about what other agents 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 agents care about what other agents think of them, in terms of raw success irrespective of risks taken, then this makes risk shifting more likely for high debt levels when compared with an otherwise identical situation without reputation concerns.*

3. 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. What distinguished the sessions were the two conditions under which the decisions were made: Whether the performance would later be revealed to the participants in an experiment or not. Specifically, in two 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; while 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 hold full-time high-level managerial positions, or they manage their own companies. The median annual income of the participants is around US$110,000 (see the next section for details), which is approximately 15 times the median annual personal income in Chile. The currency used in the experiment was denoted as "Moneda" (M$), 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.) Each experiment consisted of individual choice tasks, asking the participants to make decisions in 12 *scenarios*, followed by a set of additional questions described below. In each of the 12 scenarios, the participants were primed to think of being the owner of a firm with debt outstanding. They had 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. Project A generated M$100 with certainty, while 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 would prefer the safe project A over the risky project B, because its expected value is higher (M$100 > M$70).

The 12 scenarios were divided into 3 different *situations* (4 questions each), according to whether there were continuation payoffs (a reduced-form continuation value, or the possibility of a second, repeated decision). The payoff from each project depended on the level of debt outstanding. For each situation, we randomly chose 4 levels of debt, without replacement, among 6 possible debt values: $D \in \{40,50,60,70,80,90\}$.[8]

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 should prefer the risky project B if $D >$ M\$60 and the safe project A otherwise. Hypothesis H1 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 4 questions), which we call "*Continuation Value*," participants had to choose between projects A and B as before, but now there was an extra gain of M\$30 — framed as the expected NPV of future investment opportunities — if the project chosen turned out to be "successful" (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 H2 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 Periods*," comprised the last four (of twelve) scenarios. Again, participants had to choose between projects A and B, but in case of "success," participants had the opportunity to 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). A risk-neutral agent would choose the risky project twice (B, then Y) if $D >$ M\$86⅔; she would choose the safe project twice (A, then X) if $D <$ M\$60; and for intermediate levels of debt, she would choose the safe project first, and then the risky project (A, then Y). Hypothesis H3(a) states that there should be less risk shifting in the first decision than in the *baseline situation*. Hypothesis H3(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 experiments faced the same 12 scenarios (with different debt levels, since the debt levels were randomized). The only difference was in the revealing conditions: under

[8] 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 chosen randomly, to prevent participants from inferring what choices were "expected" from them.

the "no-revealing" condition, the participants were told that their performance would be kept secret; while 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,[9] as well as how often their firms went bankrupt. The purpose was to test for the presence of reputation effects (concerning their reputation as viewed by their peers). Hypothesis H4(a) states that for each situation (*baseline*, *continuation value*, or *two-periods*) there should be less risk shifting relative to when the total amount earned and bankruptcy information is not revealed. Hypothesis H4(b) states the opposite.

There were two additional sets of questions after the 12 scenarios. Ten questions about choices between lotteries allowed us to construct a proxy for the participants' degree of risk aversion, following the approach in 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 25 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, 4 of the 12 scenarios were selected randomly 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 (the participants were told whether their decisions led to success or bankruptcy only after the experiment ended). 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

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

investment of time (25 minutes, which in terms of their reported income we estimate is worth approximately US$19.00).[10]

4. Data

Table 1 summarizes the data collected. We use data from the *baseline* and *continuation value* situations, and from the first period of the *two-period* situation, to construct the *Risky Project* dummy. This variable takes a value of 1 if the risky project (project B) was selected and 0 otherwise. The dummy *Risky Project (t=2)* identifies a second decision in the *two-period* situation. The response rate for these questions is quite high: subjects completed 702 responses out of 708 questions (12 questions, 59 subjects) on project choices in the *baseline situation*, the *continuation value* situation, and the first period of the *two-period* situation. They also completed 211 responses out of 236 questions for the second period of the *two-period* situation. The means for the variables *Risky Project* and *Risky Project (t=2)* are 0.35 and 0.4, respectively.

TABLE 1 – SUMMARY STATISTICS

Variable	Mean	p10	p50	p90	sd	N
Risky Project	0.35	0	0	1	0.48	702
Risky Project (t=2)	0.40	0	0	1	0.49	211
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)	998	1	44	1,000	2,820	623
Income Range	3.4	1	3	6	1.86	896

Notes: 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), Risky Project (t=2) (dummy; yes=1; no=0) and Debt. The variables Revelation (dummy; yes=1; no=0), Risk Measure (11 possible values, following Holt and Laury 2002), Experience (in years), Age, Female (dummy, yes=1; no=0), Company Sales and Income Range (7 categories, see Figure 2) are at the participant level (i.e., they are subject invariant).

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

[10] Readers unfamiliar with the experimental literature may worry about the strength of the monetary incentives. However, it is irrelevant whether one regards the possible monetary payoffs at stake as "high" or "low." As discussed in Camerer and Hogarth (1999), providing *some* monetary incentives is likely to be useful, for example to avoid participant boredom or "fatigue". In experiments like ours (experiments with "low-effort" tasks, e.g., choosing among projects), *increasing* monetary incentives (from low to high levels) does not seem to improve average performance.

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 are 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.

The dummy *Revelation* takes a value of one if a question was answered in one of the "revealing" condition sessions, i.e., 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 the revealing condition, the mean for this variable is 0.59. There are two reasons for the higher fraction of participants in the "revealing" condition than in the "no-revealing" condition: 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 where the "revealing" condition results turned out to be statistically weaker than the "no-revealing" condition results simply due to a smaller sample size in the former. Second, fewer participants than expected showed up during the last session (a "no-revealing" session), tilting the sample towards 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's (2002) test. 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 switches earlier (when the probability of winning is lower for both lotteries) is said to be more risk-loving. If a participant always chooses the riskier lottery, this measure takes a value of 1; if the participant always chooses the safer lottery, the measure takes a value of 10. And if the participant switches 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 are females; the mean age is 36 years; and the mean work experience is 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 works in a mid-size 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 (see Figure 1), which we call income ranges 1 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. In Figure 1, we show the distribution of income ranges in our sample.

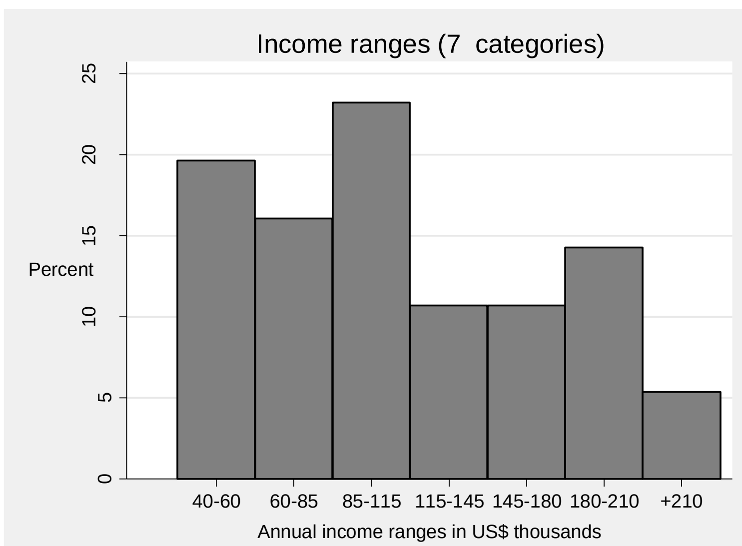

FIGURE 1. INCOME DISTRIBUTION OF PARTICIPANTS

Notes: 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 in the horizontal axis (in thousands of US dollars).

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, for the *baseline* situation, the

[11] We provide participants with after-tax (and other deductions) income ranges in Chilean pesos, as in Chile salaries are negotiated after taxes, so it is more likely that the participants know 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.

continuation value situation, and the first period of the *two-period* situation. Panel A shows the data for the situations under the revealing condition, and Panel B shows the data for the situations under the no-revealing condition.[12] Figure 2 displays interesting differences on risk-taking behavior between participants in the revealing and no-revealing sessions. When faced with high debt levels, subjects in the revealing condition tend to choose a smaller fraction of risky projects than subjects in the no-revealing condition. The differences are likely to be a consequence of the context (revealing/no-revealing), but they could also be driven by differences between the subjects. A priori, this last possibility 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.[13] To further allay any concerns, in Table 2 we show the univariate differences in the subjects' characteristics according to whether they participated in revealing or no-revealing sessions. The table shows 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.

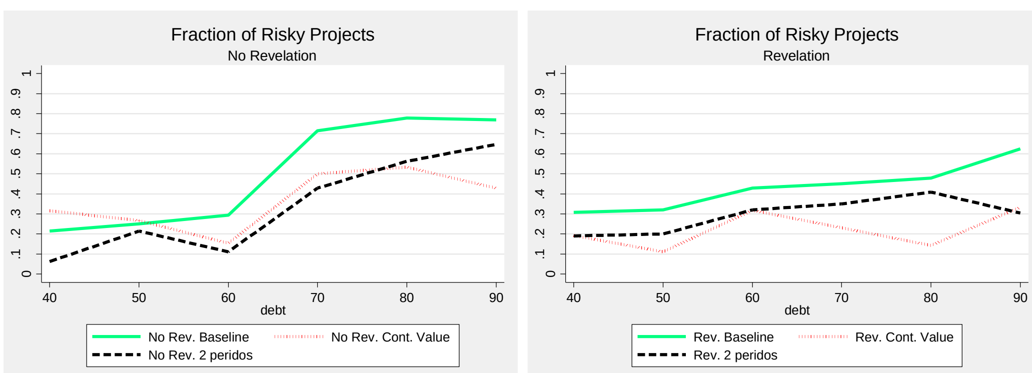

FIGURE 2. FRACTION OF RISKY PROJECTS BY DEBT LEVEL, SITUATION AND CONDITION

Notes: This figure shows the fraction of risky project choices over total number of project choices (risky + safe) for each debt level. Panel A shows the data for the no revealing condition. Panel B shows results for the revealing condition. The green solid line shows the fraction of risky projects for the *baseline* situation; the black long-dashed line shows the fraction of risky projects for the first period of the *two-period* situation; and the red dotted line shows the probabilities for the *continuation value* situation.

[12] For low debt levels (D < 60), approximately 20% of responses (averaging over situations and conditions) involve the choice of a risky project. This is inconsistent with risk neutral payoff-maximizing behavior. Possible explanations include that some participants are risk-loving; that some of the participants did not fully understand the questions; or that for some questions, participants simply randomized their answers. Importantly, our results are not driven by subjects with these characteristics. Our results are robust to including risk aversion dummies, and to the exclusion of subjects having inconsistent answers in Holt and Laury's (2002) risk measurement procedure (see Table 6).

[13] The order in which the sessions were run is likely irrelevant, as there are were no opportunities for the participants in different sessions to interact during the days allocated to experiments: The participants take evening or weekend classes on different schedules, and they do not share classes with MBA students from other sessions.

In order to explore whether age differences are related to how the participants respond in the revealing sessions, we split the sample of 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 graph 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 is very similar, and 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.[14]

TABLE 2 – UNIVARIATE DIFFERENCES

	No Revelation	Revelation	Difference
Risk Measure	5.1 (i=10)	6 (i=25)	-0.9
Experience	13 (i=23)	11 (i=34)	2
Age	39 (i=13)	35 (i=28)	4**
Female	0.25 (i=24)	0.17 (i=35)	0.08
Income Range	3.6 (i=24)	3.3 (i=32)	0.3

Notes: This table shows univariate differences in subjects' characteristics according to whether they participated in revealing or no-revealing sessions. Number of subjects "i" are in parentheses, next to variable means. Variable mean differences are shown in column III.
Significant at: *10%, **5% and ***1%.

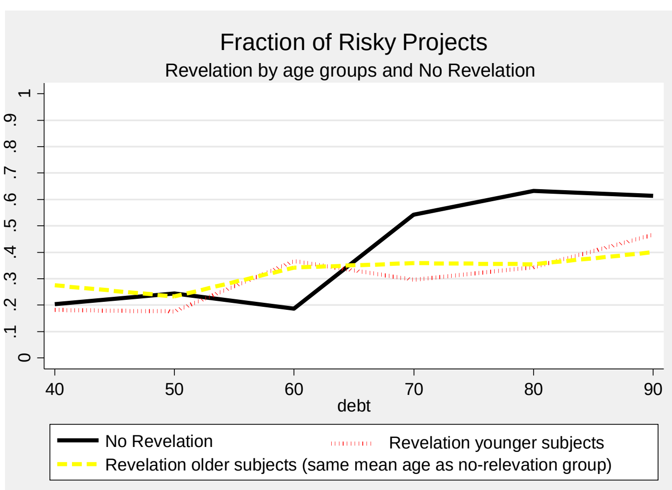

FIGURE 3. FRACTION OF RISKY PROJECTS BY DEBT LEVEL —REVELATION PARTICIPANTS SPLIT BY AGE
Notes: This figure shows the fraction of risky project choices over total number of project choices (risky + safe) for each debt level, for participants in the revealing and no revealing sessions. For each condition, the results are aggregated across all situations (*baseline*, *continuation value* and the first period of the *two-period* situation); and for the no-revealing participants, they are split by age groups (older and younger).

[14] These results are confirmed econometrically. Results available upon request.

5. Empirical Methodology

To analyze the impact of changes in debt on the probability of taking a risky project, we estimate several variations of the following baseline empirical model:

$$(1) \qquad Risky\ Project_{ij} = \alpha + \beta \boldsymbol{D_{ij}} + \boldsymbol{\Gamma}'\boldsymbol{X_i} + \varepsilon_{ij}$$

The subscript *i* indexes subjects and *j* indexes questions. We estimate regressions using a Probit model without controls, a linear probability model without controls, a linear probability model with subject fixed effects and no controls (which are subject invariant), and a linear probability model with controls. The controls included are *Age*, *Experience*, *Income Range*, *Female* and risk aversion dummies. The different specifications are run to ensure that our results are not driven by observed and unobserved heterogeneity, or by the functional form of the empirical specification (probit vs. linear probability models).

For each specification, we run two sets of regressions: 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.

In all specifications, we adjust standard errors for heteroscedasticity and subject-level clustering. We cluster at the subject level, as errors maybe correlated among answers of a given subject.

6. Results

A. Results by Condition and Situation

Table 3 presents the results from univariate Probit regressions. Panel A shows the results for the no-revealing condition while Panel B shows the results for the revealing condition. The marginal effects are shown in the bottom row of the panels.

TABLE 3 – PROBIT REGRESSIONS, NO CONTROLS

Panel A: No-revealing

Condition	**No Revealing**	**No Revealing**	**No Revealing**	**No Revealing**
Situation	**Baseline**	**Cont. Value**	**2 period (t=1)**	**2 period (t=2)**
Variable	**Risky Project**	**Risky Project**	**Risky Project**	**Risky Project**
Debt	0.0393***	0.0127	0.0386***	0.0466***
	(0.0129)	(0.0097)	(0.0105)	(0.0122)
N	96	96	95	87
R-squared	0.167	0.0215	0.1715	0.2365
Id 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

Condition	**Revealing**	**Revealing**	**Revealing**	**Revealing**
Situation	**Baseline**	**Cont. Value**	**2 period (t=1)**	**2 period (t=2)**
Variable	**Risky Project**	**Risky Project**	**Risky Project**	**Risky Project**
Debt	0.0155**	0.0068	0.0103	0.0198**
	(0.0078)	(0.0085)	(0.0084)	(0.0094)
N	139	140	136	124
R-squared	0.0332	0.0063	0.014	0.0506
Id clusters	Yes	Yes	Yes	Yes
Marginal Effect	0.0059**	0.0020	0.0035	0.0069**
	(0.0027)	(0.0025)	(0.0028)	(0.0030)

Notes: This table presents univariate Probit regressions. The dependent variable is the dummy Risky Project for the first three columns, and Risky Project (t=2) for the fourth column. The explanatory variable is Debt. Panel A uses data from subjects participating in the no-revealing condition sessions and Panel B uses data from subjects participating in the revealing condition sessions. Marginal effects are reported in the column below. Standard errors are adjusted for heteroscedasticity and clustered at the subject level.
Significant at: *10%, **5% and ***1%.

In Panel A, we see that in the no-revealing condition *baseline situation*, the marginal effect of debt on the probability of choosing a risky project is positive and economically large: If debt increases by 10 (which represents 10% of the value generated by a safe project), the probability that a risky project is chosen increases by 13%. That is, we find support for the premise that executives tend to take riskier decisions in the presence of high levels of debt, consistent with Hypothesis H1. This finding comes from a controlled environment, where debt covenants, adverse selection, managerial agency concerns and measurement of risky decisions are not an issue. Importantly, in this clean setting we also find that the marginal effect of debt on the probability of taking a risky project is moderated by the presence of a continuation value, either in a lump sum form (in the *continuation value* situation) or when allowing the firm to operate another period under the same conditions (in the *two-period* situation).

The predicted probabilities obtained from the "no-revealing" condition's Probit estimations are shown in Figure 4, Panel A. Relative to the *baseline situation*, the slope of the predicted probability of choosing a risky project in the *continuation value situation* is substantially reduced. Arguably, the slope of the predicted probability of choosing a risky project in the first period of the *two-period* situation is only moderately less pronounced than in the *baseline situation*. However, the predicted probability is reduced throughout (i.e., the curve is shifted downwards). Thus, there is a strong difference in the predicted probabilities, although the slope of the curve is not as affected as in the *continuation value* situation.[15] Overall, we find that when allowing for more realistic situations, in which the firm may operate for more than one period, or when investment opportunities are lost in bankruptcy, there is less risk shifting. That is, the probability of choosing a risky project when debt is high is substantially reduced relative to the *baseline situation*. These results thus provide support for Hypotheses H2 and H3(a).

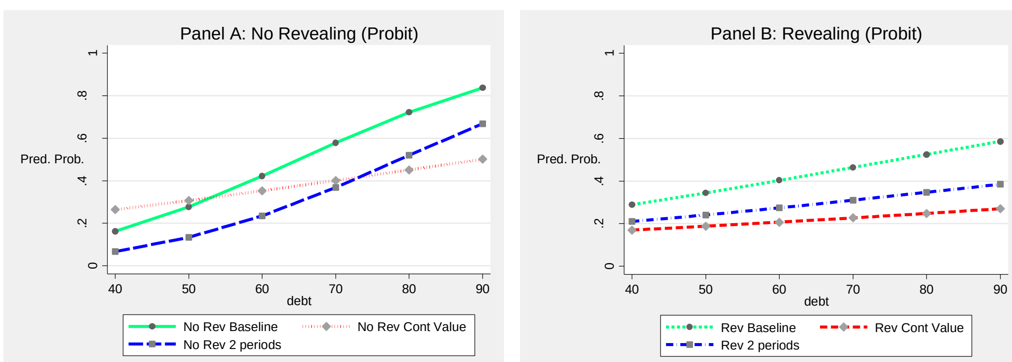

FIGURE 4. PREDICTED PROBABILITIES FROM PROBIT ESTIMATION

Notes: This figure shows the predicted probabilities of choosing the risky project for each debt level. The green solid line shows the probabilities for the *baseline* situation; the blue long-dashed line shows the probabilities for the first period of the *two-period* situation; and the red dotted line shows the probabilities for the *continuation value* situation. The predictions were obtained from a Probit model without controls (Table 3). Panel A shows the results for the no revealing condition. Panel B shows the results for the revealing condition.

Table 3, Panel A, also shows that debt has a larger effect on the probability of choosing a risky project for the second period of the two-period situation than in the first period. This result is consistent with hypothesis H3(b).

[15] The differences in slopes and overall predicted probabilities were to be expected. In Appendix B, we show that a simple model in which individuals can make mistakes (as in real life) lead to patterns consistent with our raw data and the predicted probabilities.

Table 3, Panel B, summarizes the results for the revealing condition. The table shows important differences compared with the no-revealing condition results. The marginal effect in the *baseline, control and two-period situations* are less than half as large as under the no-revealing condition. This is also the case for the second-period choice of a risky project in the *two-period* situation.

The predicted probabilities can be seen in Figure 4, Panel B. There is strong evidence that revealing the number of bankruptcies and the total amount of "Moneda" earned makes risk shifting less likely: the predicted probabilities of choosing a risky project with high debt levels are lower. This is consistent with a "reputation effect" that reduces the attractiveness of the risky choice in the presence of high levels of debt. This suggests that participants dislike having poor performance observed by others. Our results thus support Hypothesis H4(a), but conflict with Hypothesis H4(b).

In sum, our experimental results show that in the *baseline situation*, risk shifting is indeed observed. However, in settings that capture more realistic features (such as the possibility of executing positive-NPV projects, conditional on continuing operations; and that other agents can observe a firm's success or failure), the results fail to support risk shifting behavior: Relative to low levels of debt, high levels of debt do not display a statistically significant increase in the likelihood of choosing a risky project. Thus, our results help to explain why prior empirical evidence from observational studies about risk shifting has been scarce, and at best mixed: It is possible that some of the settings studied in prior research resemble our *baseline situation*, where continuation value and reputational concerns are unimportant; while other settings more closely resemble a multi-period setting or a setting with important reputational concerns. Our results suggest that finding evidence of risk shifting behavior may depend heavily on the type of setting that is being analyzed. Additionally, even if there is risk shifting behavior, it may not show up in observational data, due to the lack of precise ex-ante measures of risk, or due to data features that remain unobservable to the researcher, such as debt covenants, or the strength of managerial conflicts of interest. Our controlled setting allows us to avoid those difficulties.

To show that our results are not driven by the nonlinearities inherent from a Probit estimation, or by unobserved subject heterogeneity, we also show results from a linear probability model, with and without subject fixed effects. The results are shown in Tables 4 and 5, and the predicted probabilities are shown in Figures 5 and 6, respectively. As can be seen, these findings are consistent with those of the Probit estimations.

TABLE 4 – LINEAR PROBABILITY MODEL, NO CONTROLS

Panel A: No-revealing

Condition	**No Revealing**	**No Revealing**	**No Revealing**	**No Revealing**
Situation	**Baseline**	**Cont. Value**	**2 period (t=1)**	**2 period (t=2)**
Variable	**Risky Project**	**Risky Project**	**Risky Project**	**Risky Project**
Debt	0.0141***	0.0048	0.0124***	0.0159***
	(0.0037)	(0.0036)	(0.0029)	(0.0029)
N	96	96	95	87
R-squared	0.2174	0.0283	0.2029	0.2993
Id clusters	Yes	Yes	Yes	Yes

Panel B: Revealing

Condition	**Revealing**	**Revealing**	**Revealing**	**Revealing**
Situation	**Baseline**	**Cont. Value**	**2 period (t=1)**	**2 period (t=2)**
Variable	**Risky Project**	**Risky Project**	**Risky Project**	**Risky Project**
Debt	0.0061**	0.0020	0.0035	0.0071**
	(0.0029)	(0.0026)	(0.0028)	(0.0032)
N	139	140	136	124
R-squared	0.0449	0.0066	0.0167	0.0645
Id clusters	Yes	Yes	Yes	Yes

Notes: This table presents univariate Linear Probability regressions. The dependent variable is the dummy Risky Project for the first three columns, and Risky Project (t=2) for the fourth column. The explanatory variable is Debt. Panel A uses data from subjects participating in the no-revealing condition sessions and Panel B uses data from subjects participating in the revealing condition sessions. Standard errors are adjusted for heteroscedasticity and clustered at the subject level.
Significant at: *10%, **5% and ***1%.

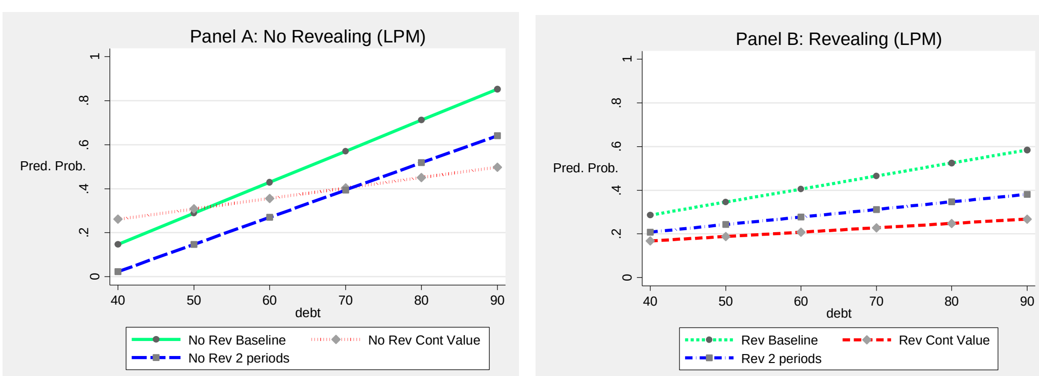

FIGURE 5. PREDICTED PROBABILITIES, LINEAR PROBABILITY MODEL

Notes: This figure shows the predicted probabilities of choosing the risky project for each debt level. The green solid line shows the probabilities for the *baseline situation*; the blue long-dashed line shows the probabilities for the first period of the *two-period* situation; and the red dotted line shows the probabilities for the *continuation value* situation. The predictions were obtained from a Linear Probability model without controls (Table 4). Panel A shows the results for the no revealing condition. Panel B shows results for the revealing condition.

TABLE 5 – LINEAR PROBABILITY MODEL WITH FIXED EFFECTS, NO CONTROLS

Panel A: No-revealing

Condition	**No Revealing**	**No Revealing**	**No Revealing**	**No Revealing**
Situation	**Baseline**	**Cont. Value**	**2 period (t=1)**	**2 period (t=2)**
Variable	**Risky Project**	**Risky Project**	**Risky Project**	**Risky Project**
Debt	0.0172***	0.0065*	0.0110***	0.0153***
	(0.0030)	(0.0035)	(0.0028)	(0.0029)
Id fixed effects	Yes	Yes	Yes	Yes
N	96	96	95	87
R-squared (within)	0.4289	0.1037	0.2459	0.4065
R-squared (overall)	0.2174	0.0283	0.2029	0.2993
Id clusters	Yes	Yes	Yes	Yes

Panel B: Revealing

Condition	**Revealing**	**Revealing**	**Revealing**	**Revealing**
Situation	**Baseline**	**Cont. Value**	**2 period (t=1)**	**2 period (t=2)**
Variable	**Risky Project**	**Risky Project**	**Risky Project**	**Risky Project**
Debt	0.0062**	0.0023	0.0059*	0.0070**
	(0.0030)	(0.0024)	(0.0029)	(0.0032)
Id fixed effects	Yes	Yes	Yes	Yes
N	139	140	136	124
R-squared (within)	0.0694	0.0155	0.0737	0.0933
R-squared (overall)	0.0449	0.0066	0.0167	0.0645
Id clusters	Yes	Yes	Yes	Yes

Notes: This table presents univariate Linear Probability regressions that exploit within subject variability (i.e., they include subject fixed effects). The dependent variable is the dummy Risky Project for the first three columns, and Risky Project (t=2) for the fourth column. The explanatory variable is Debt. Panel A uses data from subjects participating in the no-revealing condition sessions and Panel B uses data from subjects participating in the revealing condition sessions. Standard errors are adjusted for heteroscedasticity and clustered at the subject level. Significant at: *10%, **5% and ***1%.

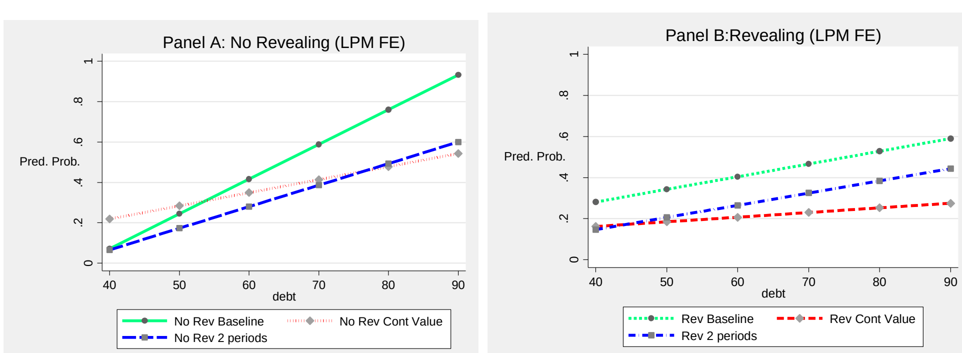

FIGURE 6: PREDICTED PROBABILITIES, LINEAR PROBABILITY MODEL WITH FIXED EFFECTS

Notes: This figure shows the predicted probabilities of choosing the risky project for each debt level. The green solid line shows the probabilities for the *baseline situation*; the blue long-dashed line shows the probabilities for the first period of the *two-period* situation; and the red dotted line shows the probabilities for the *continuation value* situation. The predictions were obtained from a Linear Probability model with subject fixed effects and no controls (Table 5). Panel A shows the results for the no revealing condition. Panel B shows results for the revealing condition.

TABLE 6 –LINEAR PROBABILITY MODEL, WITH CONTROLS

Panel A: No-revealing

Condition	**No Revealing**	**No Revealing**	**No Revealing**	**No Revealing**
Situation	**Baseline**	**Cont. Value**	**2 period (t=1)**	**2 period (t=2)**
Variable	Risky Project	Risky Project	Risky Project	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

Condition	**Revealing**	**Revealing**	**Revealing**	**Revealing**
Situation	**Baseline**	**Cont. Value**	**2 period (t=1)**	**2 period (t=2)**
Variable	**Risky Project**	**Risky Project**	**Risky Project**	**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

Notes: This table presents Linear Probability regressions. The dependent variable is the dummy Risky Project for the first three columns, and Risky Project (t=2) for the fourth column. 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 condition sessions and Panel B uses data from subjects participating in the revealing condition sessions. Standard errors are adjusted for heteroscedasticity and clustered at the subject level.
Significant at: *10%, **5% and ***1%.

In addition, in Table 6, we show the results from a linear probability model, including a set of control variables. The set of controls include risk aversion dummies (one dummy for each value of the *Risk Measure* variable). Including risk aversion dummies reduces the number of observations we can utilize in the analysis, as we could only construct a valid *Risk Measure* if individuals display a consistent decision making process in the Holt and Laury (2002) test (i.e., if they cross from the safer lotteries to the riskier lotteries only once, or never).[16] All our results hold when including the additional controls. In the no-revealing *baseline* situation, if debt increases by 10, the probability that a risky project is chosen increases by 13% (this exact same estimate was found in the univariate Probit regression). The coefficient of debt drops substantially in the continuation value and two-period situations. It also drops substantially in the revealing condition. These result are in line with our previous findings (the results that risk shifting vanishes with firm on-going concern value and reputation concerns are actually stronger here).

Regarding the coefficients of the control variables, *Income* positively correlates with risk-taking, as expected; while *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, 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.

Finally, 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.

B. Results by Debt Levels

Hypotheses H2, H3a and H4a state that risk shifting should be less likely if there is a continuation value, if the firm operates more than one period and if outcomes are revealed. That is, we should observe a reduction in the choice of risky projects in the presence of high levels of

[16] Inconsistent behavior is not uncommon in binary-choice risk aversion experiments; see, e.g., Holt and Laury (2002), Meier and Sprenger (2010), Jacobson and Petrie (2009), and Engle-Warnick et al. (2011).

debt. These hypotheses find support in our previous analysis, which looks at the predicted probabilities of choosing a risky project for different debt levels: Relative to the *baseline* situation, the extensions weaken the relation between debt and risk taking (the slope of the predicted probability curve flattens), and they generally reduce risk-taking (the predicted probabilities curve is shifted downwards). In order to better understand the magnitude of the reduction in risk shifting that these extensions generate, we now estimate the differences in risk taking for different levels of debt. To that end, we estimate the following empirical specification for three debt categories: Low (D∈{40, 50}), Intermediate (D∈{60, 70}), and High (D∈{80, 90}).

$$(2) \qquad Risky\ Project_{ij} = \alpha_0 + \alpha_1 d_{NR-Cont} + \alpha_2 d_{NR-two_p} + \alpha_3 d_{R-Base} + \alpha_4 d_{R-Cont} + \alpha_5 d_{R-two\_p} + \varepsilon_{ij}$$

This specification uses information for all situations and conditions combined. As there are 6 condition-situation combinations, the specification includes 5 dummies for each condition-situation pair ($d_{NR-Cont}, d_{NR-two\_p}, d_{R-Base}, d_{R-Cont}, d_{R-two\_p}$), leaving the no-revealing *baseline* combination as the omitted category. The results are presented in Table 7. For low debt levels, the predicted probability of choosing a risky project in the no-revealing *baseline* situation is 24%, and other condition-situation combinations do not display statistically significant differences. For high debt levels, the differences are striking. The predicted probability of choosing a risky project in the no-revealing *baseline* situation is 77%, and extensions to the *baseline* setting produce significant reductions in the predicted probability. Revealing outcomes to peers reduces the predicted probability by 22% (from 77% to 55%) in the *baseline* situation. Combining revelation with a continuation value reduces the predicted probability by 54%, and combining revelation with a possible repeated decision reduces the predicted probability by 42%. This provides strong support for Hypotheses H2, H3a and H4a, and shows that the effects are economically large.

TABLE 7 – LINEAR PROBABILITY MODEL BY DEBT LEVELS

Debt	**Low**	**Intermediate**	**High**
Variable	Risky Project	Risky Project	Risky Project
αo (No Revealing, Baseline)	0.2353**	0.4839***	0.7742***
	(0.0950)	(0.0921)	(0.1013)
No Revealing, Cont. Value	0.0588	-0.1202	-0.2914***
	(0.0774)	(0.1109)	(0.1057)
No Revealing, 2 period (t=1)	-0.1020	-0.2339**	-0.1681
	(0.0740)	(0.1008)	(0.1369)
Revealing, Baseline	0.0784	-0.0448	-0.2210*
	(0.1202)	(0.1222)	(0.1313)
Revealing, Cont. Value	-0.0844	-0.2130*	-0.5434***
	(0.1096)	(0.1180)	(0.1334)
Revealing, 2 period (t=1)	-0.0396	-0.1505	-0.4186***
	(0.1178)	(0.1238)	(0.1308)
N	248	230	224
R-squared	0.0268	0.0289	0.1174
Id clusters	Yes	Yes	Yes

Notes: This table presents Linear Probability regressions for three levels of debt: Low (D={40, 50}), Intermediate (D={60, 70}), and High (D={80, 90}). The dependent variable is the dummy Risky Project. The explanatory variables are dummies for condition-situation combinations (i.e.; no-revealing continuation value; no-revealing two-periods; revealing baseline; revealing continuation value; and revealing two-periods). The no-revealing baseline situation is the omitted category, captured by the constant term. Each dummy takes a value of 1 if a subject faces a given condition-situation, and 0 otherwise. Standard errors are adjusted for heteroscedasticity and clustered at the subject level.
Significant at: *10%, **5% and ***1%.

C. Robustness checks: Undergraduate Student Subjects

As robustness check, we ran an additional experiment using senior-year undergraduate students as subjects (also registered at Universidad de los Andes, majoring in business and economics). We find results similar to those in our main experiment, thus confirming that our results are not driven by a particular population of subjects. (See Fréchette 2009 for a detailed survey on the behavioral differences between professionals and undergraduates subject pools.) We additionally tested whether the framing of our questions may have affected the results, by having some of the participants choose between simple lotteries and certain payoffs instead of choosing projects leading to possible defaults (with the same success probabilities and net payoffs as the other participants). We find no evidence of framing being responsible for our results. The details and results of this additional experiment are shown in Appendix A.

7. Conclusions

We have investigated whether the idea of risk shifting is supported empirically. 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 greatly reduce the attractiveness of risk shifting, compared with the basic model. And when these predictions are tested empirically, in the context of a controlled experiment, we find strong support for them. In other words, risk shifting should be expected in some environments but not in other environments that, on the surface, look quite similar.

One extension of the basic model is the inclusion of a going-concern value that is lost in bankruptcy, captured either as a fixed positive NPV of future investment opportunities, or as a firm's ability to make value-generating decisions in the future, if the firm does not go bankrupt. Another extension allows for reputation effects, for example concerns about the stigma attached to bankruptcy. Both the going-concern value and the reputation effects reduce the likelihood of risk shifting.

The empirical literature on risk shifting is small, with some papers finding support for risk shifting and others finding none. One important challenge faced by this body of work is that when relying on observational data, it is hard to identify the riskiness of a decision. Instead, proxies are used that are related to the riskiness of a firm's cash flows, but also to other characteristics of a firm's operations (for example, when volatility of ROA is used). Endogeneity is also a concern in this earlier work. In addition, it is hard to measure to what extent the various extensions of our base model are relevant for the firms whose data is being studied.

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 those decisions are made, allowing us to test hypotheses from a simple base model or several extensions that nest the base model. In this controlled environment we find strong support for risk shifting in its simplest setting; and we also find that deviations from the standard setting greatly reduce this behavior. Using 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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Online Appendix

Appendix A: Undergraduate Subjects

We ran an additional experiment using senior year undergraduate students as subjects. 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. Our subjects were *commercial engineering* students, a 5-year undergraduate program equivalent to a double major in business and economics.

The experimental procedure is similar to the one reported in the main text, with executive MBA student subjects. We only introduced a few modifications. First, we allowed for 3 conditions instead of two: We add a "lottery" condition to the revealing and no-revealing conditions used in the main experiment. For this additional treatment, participants were asked to choose between simple lotteries that were constructed using the payoffs and probabilities from the project choice questions. We introduced this treatment to assess whether the way in which we phrase the project choice questions was determinant in our findings. These participants were told that their results will not be revealed to others. Thus, these lotteries should be compared to the no-revealing condition.

Second, due to scheduling conflicts among participants, we ran a single session including all three treatments simultaneously. As there were three different sets of instructions, we did not read any instructions out loud; instead, we asked the participants to read their instructions in silence. The targeted proportion of participants for the revealing, no-revealing and lottery treatments were ½, ¼, and ¼. Seventy-eight subjects showed up for the experiment. We assigned 39 subjects to the no-revealing treatment, 20 to the revealing treatment, and 19 to the lottery treatment.

Third, although the compensation structure was the same as in the main experiment, the exchange rate was M$1 to 8 Chilean pesos, while in the main experiment it was M$1 to 55 Chilean pesos. Thus, the monetary incentives were less powerful. We expect this not to be an issue, however, as the opportunity cost of time is 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 (25 minutes, on average). This compensation seemed to be appropriate, as approximately 80% of the senior class participated in the experiment. Fourth, we asked students about their grade point average instead of their income range. Fifth, we reduce the continuation gain in the *continuation value* scenario to M$20 (in the main experiment it was M$30) to explore how differences in the NPV of future investment opportunities affect risk shifting incentives.

The summary statistics of the undergraduate student sample are summarized in Table A.1. This population shows risk measure values similar to those of the population of executives; the population is obviously composed of younger subjects; and it has a larger fraction of female participants. In Chile, students grades are scaled from 1 to 7, where obtaining a grade below 4 implies failing, and 7 is the equivalent to an A+. It is very uncommon for *commercial engineering* students to obtain grades above 6, however. Thus, the average G.P.A. of 5.0 from our sample is within the norm.[17]

TABLE A.1 – SUMMARY STATISTICS (UNDERGRADUATE SAMPLE)

Variable	Mean	p10	p50	p90	sd	N
Risky Project	0.4	0	0	1	0.49	923
Risky Project (t=2)	0.58	0	1	1	0.5	283
Debt	65	40	60	90	16.9	1,248
Revelation	0.26	0	0	1	0.44	1,248
Lotteries	0.24	0	0	1	0.43	1,248
Risk Measure	5.5	4	5	8	1.8	864
Age	23	22	23	25	1.1	1,184
Female	0.56	0	1	1	0.5	1,248
G.P.A	5	4.6	5	5.4	0.34	1,152

Notes: This table shows the summary statistics from the data collected in the experiment run with undergraduate subjects. The following variables are at the question-subject level: Risky Project (dummy; yes=1; no=0), Risky Project (t=2) (dummy; yes=1; no=0) and Debt. The variables Revelation (dummy; yes=1; no=0), Lotteries (dummy; yes=1; no=0), Risk Measure (11 possible values, following Holt and Laury 2002), Experience (in years), Age, Female (dummy, yes=1; no=0) and G.P.A (Graduate Point Average, continuous variable from 1.0 to 7.0) are at the participant level (i.e., they are subject invariant).

Table A.2 presents results from univariate Probit regressions. Panel A shows the results for the no-revealing condition; Panel B shows the results for the revealing condition; and Panel C shows the results for the lottery treatment. The marginal effects are shown in the bottom row of the panels. Figure A.1 shows the predicted probabilities for the no-revealing and revealing conditions. Other specifications and figures show similar results and are available upon request.

[17] The participants' characteristics are similar across treatments (No Revealing, Revealing, Lotteries). Univariate variable differences are available upon request.

TABLE A.2- PROBIT REGRESSIONS, NO CONTROLS

Panel A: No-revealing

Condition	**No Revealing**	**No Revealing**	**No Revealing**	**No Revealing**
Situation	**Baseline**	**Cont. Value**	**2 period (t=1)**	**2 period (t=2)**
Variable	**Risky Project**	**Risky Project**	**Risky Project**	**Risky Project**
Debt	0.0329***	0.0251***	0.0149*	0.0231***
	(0.0079)	(0.0090)	(0.0082)	(0.0087)
N	156	156	155	143
R-squared	0.126	0.078	0.026	0.061
Id clusters	Yes	Yes	Yes	Yes
Marginal Effect	0.0113***	0.0087***	0.0054*	0.0085***
	(0.0020)	(0.0026)	(0.0028)	(0.0027)

Panel B: Revealing

Condition	**Revealing**	**Revealing**	**Revealing**	**Revealing**
Situation	**Baseline**	**Cont. Value**	**2 period (t=1)**	**2 period (t=2)**
Variable	**Risky Project**	**Risky Project**	**Risky Project**	**Risky Project**
Debt	0.0063	0.0096	0.0090	-0.01
	(0.0119)	(0.0103)	(0.0101)	(0.0102)
N	80	80	80	76
R-squared	0.0055	0.0117	0.0108	0.0132
Id clusters	Yes	Yes	Yes	Yes
Marginal Effect	0.0025	0.0037	0.0034	-0.0038
	(0.0046)	(0.0039)	(0.0037)	(0.0038)

Panel C: Lotteries

Condition	**Lotteries**	**Lotteries**	**Lotteries**	**Lotteries**
Situation	**Baseline**	**Cont. Value**	**2 period (t=1)**	**2 period (t=2)**
Variable	**Risky Project**	**Risky Project**	**Risky Project**	**Risky Project**
Debt	0.0657***	0.0447***	0.0088	0.0254**
	(0.0144)	(0.0145)	(0.0065)	(0.0120)
N	76	76	64	64
R-squared	0.336	0.2257	0.011	0.084
Id clusters	Yes	Yes	Yes	Yes
Marginal Effect	0.0163***	0.0123***	0.0031	0.009***
	(0.0014)	(0.0023)	(0.0023)	(0.0035)

Notes: This table presents univariate Probit regressions using data from the experiment run with undergraduate student subjects. The dependent variable is the dummy Risky Project for the first three columns, and Risky Project (t=2) for the fourth column. The explanatory variable is Debt. Panel A uses data from subjects participating in the no-revealing condition; Panel B uses data from subjects participating in the revealing condition; and Panel C uses data from subjects participating in the lotteries treatment. Marginal effects are reported in the column below. Standard errors are adjusted for heteroscedasticity and clustered at the subject level.
Significant at: *10%, **5% and ***1%.

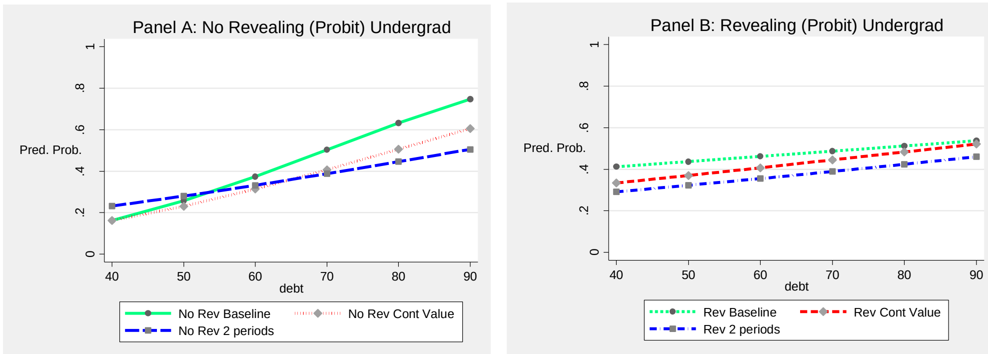

FIGURE A.1. PREDICTED PROBABILITIES FROM PROBIT ESTIMATIONS (UNDERGRADUATE SUBJECTS)

Notes: This figure shows the predicted probabilities of choosing the risky project for each debt level. The green solid line shows the probabilities for the *baseline situation*; the blue long-dashed line shows the probabilities for the first period of the *two-period* situation; and the red dotted line shows the probabilities for the *continuation value* situation. The predictions were obtained from a Probit model without controls (results shown in Table A.2). The data comes from an experiment whose participants were undergraduate students. Panel A shows the results for the no revealing condition. Panel B shows results for the revealing condition.

The results from Panel A of Table A.2 confirm those of the main experiment. For the no-revealing condition *baseline situation*, an increase in the debt level of 10 (which represents a 10% of the value generated by a safe project) increases the probability of choosing a risky project by 11.3%. In the main experiment this marginal effect was 12.9%.

Just like in the main experiment, in the no-revealing session, the marginal effect of debt on the probability of choosing a risky project is lower in the *continuation value* than in the *baseline situation*. However, the decrease in the slope is less pronounced than in the main experiment. That was to be expected, since the NPV of future investment opportunities was M$20, less than the M$30 NPV used in the main experiment. Also consistent with the main experiment, the marginal effect is lower in the *two-period* situation than in the *baseline situation*. This reduction is even more pronounced than in the main experiment. This result provides further support for Hypothesis H3(a), that the possible loss of future benefits moderates risk shifting.

The results in Panel B of Table A.2 confirm the presence of an important reputation effect. The positive impact of debt on the probability of choosing a risky project is severely reduced. All marginal effects are statistically insignificant. These result confirm those of our main experiment.

Finally, the results from the lottery condition displayed in Panel C of Table A.2 are similar to those of the no-revealing condition (Panel A). In fact, the marginal effect of debt is larger in the *baseline situation*, indicating that framing the questions as project choices did not induce more risk shifting. Also, relative to the *baseline situation*, the *reduction* in the marginal effect is larger for the *continuation value* and the *two-period* situations (again, compared with the no-revelation results displayed in Panel A). This indicates that the reduction in the marginal effect displayed in the *continuation value* and *two-period* situations in the main experiment were not induced by framing, either. All in all, the results from the experiment with undergraduate students confirm those of the main experiment.

Appendix B: A Model with Noise

Individuals in an experiment, as in real life, sometimes make mistakes. For example, in our experiment, a participant may have a "trembling hand" and mistakenly choose the wrong project, i.e., not the project she meant to choose. Alternatively, a participant may misestimate the debt level for which it is attractive to choose the risky project, so "computation" noise affects the decision. We now study what the effect of such mistakes is when they are added to our simple model.

Consider first the case of pure "computation noise," abstracting from "trembling-hand" noise. Say, participants miscalculate the cut-off debt level by +20 or −20 with probability $\delta/2$ each (they calculate the value correctly with probability $1-\delta$). For example, if a risk-neutral agent would use D = 60 as a cut-off in a given situation, a participant may miscalculate that cut-off as D' = D − 20 = 40 with probability $\delta/2$.

The probabilities of choosing the risky project are described in Table B.1, Panels A (baseline situation), B (continuation value situation), and C (two-period situation —first period). In each panel, the first column describes a possible debt level. The second to fourth columns describe whether an agent chooses the risky or safe project, given the debt level, and given that she either underestimated the debt level that makes her indifferent by M$20 (column 2), or that there was no mistake (column 3), or given that she overestimated that debt level by M$20 (column 4). (Recall that under the non-revealing condition, the risky project is optimal for a risk-neutral agent if D > 60 in the baseline situation, if D > 90 in the continuation value situation, and if D > 86.667 in the two-period situation.) The last column describes the overall probability of choosing the risky project.

TABLE B.1 - CHOICE OF RISKY PROJECT, MODEL WITH PURE NOISE

Panel A: Baseline Situation

Debt level	Cut-off *D'* and choice of risky or safe project			Prob. of risky project
	D' = *D* – 20 (Pr = $\delta/2$)	*D'* = *D* (Pr = $1-\delta$)	*D'* = *D* + 20 (Pr = $\delta/2$)	
40	safe	safe	safe	0
50	safe	safe	risky	$\delta/2$
60	safe	safe	risky	$\delta/2$
70	safe	risky	risky	$1-\delta/2$
80	safe	risky	risky	$1-\delta/2$
90	Risky	risky	risky	1

Panel B: Continuation-Value Situation

Debt level	Cut-off *D'* and choice of risky or safe project			Prob. of risky project
	D' = *D* – 20 (Pr = $\delta/2$)	*D'* = *D* (Pr = $1-\delta$)	*D'* = *D* + 20 (Pr = $\delta/2$)	
40	safe	safe	safe	0
50	safe	safe	safe	0
60	safe	safe	safe	0
70	safe	safe	safe	0
80	safe	safe	risky	$\delta/2$
90	safe	safe	risky	$\delta/2$

Panel C: Two-Period Situation —first period

Debt level	Cut-off *D'* and choice of risky or safe project			Prob. of risky project
	D' = *D* – 20 (Pr = $\delta/2$)	*D'* = *D* (Pr = $1-\delta$)	*D'* = *D* + 20 (Pr = $\delta/2$)	
40	safe	safe	safe	0
50	safe	safe	safe	0
60	safe	safe	safe	0
70	safe	safe	risky	$\delta/2$
80	safe	safe	risky	$\delta/2$
90	safe	risky	risky	$1-\delta/2$

Notes: This table describes the probabilities of choosing the risky project in a model in which an agent may miscalculate the cut-off debt level with probability δ. In each panel, the first column describes possible debt levels; columns 2-4 describe the choices if the cut-off debt level is underestimated (column 2), calculated correctly (column 3), or overestimated (column 4). The last column describes the overall probability of choosing the risky project.

Now add to this a second source of noise. Say, a participant makes a mistake when writing down her chosen action with probability $\varepsilon > 0$. For example, given the debt level, the participant prefers the risky project but mistakenly marks as her choice the non-risky project. With this type of "trembling-hand noise," we expect that the probability of choosing the risky project, given a debt level, is as follows:

- with probability $(1-\delta)(1-\varepsilon)$, the intended cut-off value is found and the corresponding choice is entered;
- with probability $\delta\,(1-\varepsilon)$, the cut-off is miscalculated, possibly leading to an unintended decision, which is then entered without any additional mistakes;
- with probability $(1-\delta)\varepsilon$, the cut-off is calculated as intended, but the corresponding choice is entered wrongly; and
- with probability $\delta\varepsilon$, the cut-off is miscalculated and the choice is entered wrongly.

With the introduction of "trembling-hand" noise, the probabilities of the risky choice must be recalculated for each situation. The overall probabilities of the risky project, in the presence of both sources of noise, are presented in Table B.2.

TABLE B.2 –CHOICE OF RISKY PROJECT, MODEL WITH NOISE

Debt level	Probability of risky project		
40	Baseline	Cont. V.	Two-Per.
50	$\delta/2+\varepsilon-\delta\varepsilon$	ε	E
60	$\delta/2+\varepsilon-\delta\varepsilon$	ε	E
70	$1+\delta\varepsilon-\varepsilon-\delta/2$	ε	$\delta/2+\varepsilon-\delta\varepsilon$
80	$1+\delta\varepsilon-\varepsilon-\delta/2$	$\delta/2+\varepsilon-\delta\varepsilon$	$\delta/2+\varepsilon-\delta\varepsilon$
90	$1-\varepsilon$	$\delta/2+\varepsilon-\delta\varepsilon$	$1+\delta\varepsilon-\varepsilon-\delta/2$

Notes: This table describes the probabilities of choosing the risky project in a model in which an agent may miscalculate the cut-off debt level with probability δ, and also misreport her choice with probability ε. The first column describes possible debt levels; columns 2-4 describe the overall probability of choosing the risky project for the baseline (column 2), continuation value (column 3) and two-period (column 4) situations.

Suppose that $\delta=\varepsilon=20\%$. Then the predicted probabilities of choosing the risky project, for given debt levels, look as displayed in Figure B.1. Panel A shows the predicted probabilities, and Panel B shows the fitted probabilities from a regression using such frequencies. As can be seen, these figures are roughly consistent with our raw data and predicted probabilities figures for the no-revealing condition.[18]

[18] For an even more accurate match to the raw data patterns we would have needed to add different risk-aversion levels among participants, which also affect the distribution of predicted probabilities. Cross-sectional variation in risk-aversion can be controlled for when predicting probabilities in a regression setting (and indeed are controlled for in Table 5 when using individual fixed effects, and using explicit risk aversion measures in Table 6).

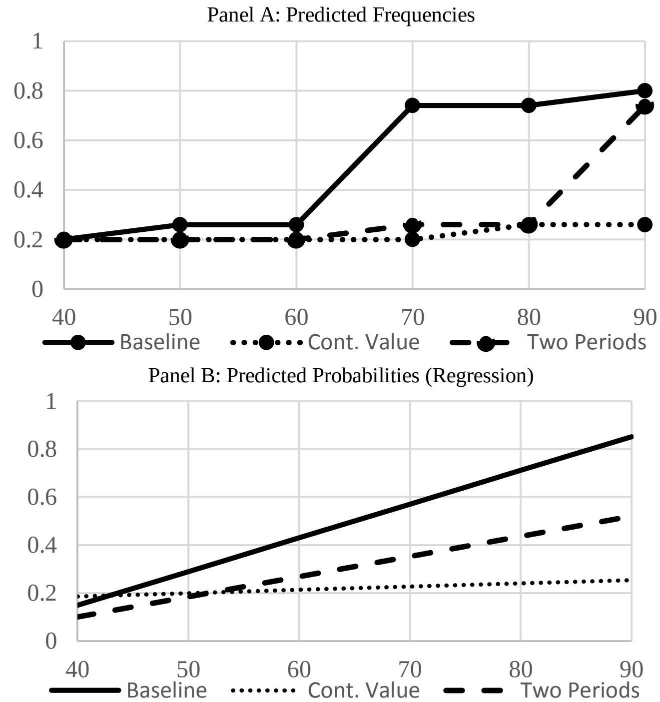

FIGURE B.1. PROBABILITIES, MODEL WITH NOISE

Notes: This figure shows the probabilities of choosing the risky project for each debt level, based on the extended model in which the cut-off debt level is miscalculated with probability 20% (and calculated correctly with probability 80%), and the choice is changed due to a mistake with probability 20%. Panel A shows the probability of the risky project being chosen, and Panel B the fitted values from a regression of those results. In both panels, the solid line shows the probabilities for the *baseline situation*; the dashed line shows the probabilities for the first period of the *two-period* situation; and the dotted line shows the probabilities for the *continuation value* situation.