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Measuring Aversion to Debt: An Experiment Among Student Loan Candidates

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Abstract
Debt aversion, an unwillingness to enter into a financial contract framed or labeled as debt, distorts household investment and financing decisions. We test through an experiment for the presence of debt aversion among a relevant population. The tests allow us to identify two sources of debt aversion: one due to framing (as debt or as an income-contingent contract) and another due to labeling (as a loan or as a human capital contract). Most of the debt aversion we identified was due to labeling. Labeling a contract as a loan decreased its probability of being chosen over a financially equivalent contract and increased its perceived cost.

Keywords Debt aversion · Human capital contracts · Income contingent loans · Income share agreements

Household finance analysis attempts to understand the motives underlying household financial decisions. The starting point is often a rational utility-maximizing agent trying to make the best of available opportunities. Yet a large body of empirical evidence, some related to household finance, challenges the rational-agent paradigm as an accurate description of behavior. Central questions for this research are when do the deviations from the paradigm take place and how much do they matter?

Deviations from the paradigm matter when decisions have a major impact on future wealth. Perhaps the largest such decision is to invest in education. Although ascertaining the value of education is fraught with econometric issues, a simple comparison of the wages of full-time employed college and high-school graduates in the United States (based on data on 2010 earnings from the 2011 annual Current Population Survey) reveals that college graduates earned 107% more than high-school graduates in 2010. The difference elsewhere in the world is often larger. Assuming that the difference is due to education and that the growth

and riskiness of the earnings for both groups are similar, the value of human capital is 107% greater for a 25-year-old college graduate than for a 25-year-old high-school graduate. The wage difference in 2010 was $38,522, or roughly one-fifth of the average home value for households that own a house, townhouse, or apartment in the United States. It was also about one-fifth of the average financial and business assets for household heads under age 40.[1] Based on these data, human capital could be the largest asset for individuals under age 40, and therefore investment in education has first-order long-term consequences on their well-being. Furthermore, most students finance their education, so it is important to look at deviations from the rational-agent paradigm in education investment decisions.

Work in behavioral economics dating back to Kahneman and Tversky (1979) and the pervasive marketing campaigns for financial products highlighting features unrelated to cash flows suggest that investment decisions are not simply a function of a product's future flows. Related work has studied biases in investment decisions (e.g., Choi et al. 2011). But rather than focus on savings, we asked what happens when agents are choosing a financing mechanism. Do similar biases affect their decision? Our research design, focused on student financial aid, addressed this question directly. If

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[1] Based on data from the 2007 Survey of Consumer Finances, the weighted average of a homeowner's property, excluding farms, ranches, and mobile homes, in 2007 was $204,344, and the weighted average of all financial and business assets was $182,298.

Table 1 Loan parameters used in the questions

Variable	Chile	Colombia	Mexico
Panel A: amounts in local currency			
Financed amount—1 year	$3,500,000.00	$6,000,000.00	$52,000.00
Cap—1 year	$700,000.00	$1,200,000.00	$10,400.00
Monthly payment with cap—1 year	$70,000.00	$120,000.00	$1040.00
Financed amount in local currency—2 years	$7,000,000.00	$12,000,000.00	$104,000.00
Cap—2 years	$1,050,000.00	$1,800,000.00	$15,600.00
Monthly payment with cap—2 years	$157,500.00	$270,000.00	$2340.00
Panel B: amounts in U.S. dollars			
Financed amount—1 year	$6693.18	$3024.94	$4146.73
Cap—1 year	$1338.64	$604.99	$829.35
Monthly payment with cap—1 year	$133.86	$60.50	$82.93
Financed amount in local currency—2 years	$13,386.36	$6049.88	$8293.46
Cap—2 years	$2007.95	$907.48	$1244.02
Monthly payment with cap—2 years	$301.19	$136.12	$186.60

biases exist, their consequences could reach much further than those of education investments because if households consider features unrelated to cash flows in deciding one of their most important investment decisions—in education—they may consider the same features in many other settings. Biases result in welfare costs for households and could affect asset prices.

One prominent potential bias is debt aversion.[2] A standard definition does not exist, but the term is loosely defined as a borrower suffering, for some reason, a subjective cost when taking on debt. We explored debt aversion in two new ways. First, we narrowed the definition to focus on aspects of the financing decision unrelated to cash flows. We did so by varying the features of a financial instrument while keeping the instrument's cash flows constant. Second, recognizing that debt aversion could stem from the framing of the contracts or from a negative association with the word "debt" or "loan," we separated debt aversion into two concepts: one stemming from framing effects, the other from labeling effects. Our definition of debt aversion, including its two sources, allowed us to design an experiment that tests for its presence and measures the importance of each source.

Investment in education is a natural place to look for debt aversion. Labor economists have long studied the value and return of investing in education (at least since Mincer 1974). A robust result in this literature is that the internal rate of return of the investment is high (on the order of 10% in the United States) when compared with that of other available financial investments (e.g., Card 1999; Palacios-Huerta 2003; Psacharopoulos and Patrinos 2004). That raises the question of why people do not invest more in their education. One possible answer is that the instruments available for

financing education appear expensive relative to their true cost—expensive enough to make the investment not worthwhile.[3] Concretely, this argument points out that potential borrowers display debt aversion, leading them to pass on a good investment. In this setting, debt aversion acts as a self-imposed borrowing constraint.

Our experiment consisted of a survey with two parts, one aimed at measuring a preference for contracts not framed or labeled as debt and the second at measuring the cost of debt aversion, if present. The survey was conducted in three Latin American countries: Chile, Colombia, and Mexico. Of the 1422 people who took the survey and were randomly assigned to the treatment or control group, 767 responded to the questions most relevant to this study.[4] Two strengths of our design are the diversity of survey respondents and the fact that respondents were all recent student loan applicants, so the survey questions reflected a situation that they had recently experienced.

The financing contracts in our survey were income-contingent contracts for education, which have been gaining traction around the world since Friedman (1955) proposed them. They include income-contingent loans—issued by Yale University in the early 1970s and later as part of student funding policy in multiple countries, starting with Australia's Higher Education Contribution Scheme in 1989—and

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[2] The term is used in the economics of education literature. See Eckel et al. (2007) and Rasmussen (2006).

[3] Other possible answers not related to this article are that after risk is adjusted for, the returns to education are not that large; that after appropriately accounting for selection biases, the returns are smaller; that young high-school graduates are misinformed about the returns to additional education; and that market imperfections lead providers of capital to ration it, constraining students as a result.

[4] Table 1 shows that the sample of respondents and, more important, the sample of respondents to the relevant questions are, on average, very similar across treatment status.

more recently human capital contracts (HCCs), issued by multiple firms starting in the early 2000s and now known as income share agreements (ISAs). We base our study on the observation that income-contingent loans and HCCs with a cap have financially equivalent outcomes.

In the first part of our experiment subjects were asked to choose between contracts that were framed as debt or as an income-contingent contract.[5] Following Palacios (2004), who showed that paying a percentage of income up to a cap (a capped HCC) is financially equivalent to an income-contingent loan, we framed financially equivalent contracts as loans or as HCCs and asked respondents which one they would choose. Debt aversion would reveal a preference for HCCs over loans. For the treatment group each contract was clearly labeled as a loan or as an HCC, whereas for the control group no label was attached to either contract. Except for the label, the wording in the explanations of the HCC and loan contract were exactly the same for both groups. This design allowed us to control for framing effects and to identify debt aversion due to labeling as the excess of preference for the HCC in the treatment group with respect to the control group, analogous to a difference-in-differences design.

In the second part of our experiment we measured the monetary value of debt aversion due to labeling by asking what fixed monthly payment would make respondents indifferent between the loan contract and a capped HCC. For the treatment group the question labeled the fixed payment contract as "debt," whereas for the control group the question labeled the fixed payment contract as "a different contract." We then investigated whether students in the treatment group were willing to pay the same monthly amount as students in the control group. Students in the treatment group being willing to pay less on average than students in the control group would be evidence of debt aversion due to labeling.

We found evidence consistent with debt aversion and debt aversion due to labeling, even with the small sample size of the study, suggesting that it affects the choices students make when contemplating an investment on education. When confronted with contracts with identical financial payoffs, a larger percentage of respondents chose the HCC contract (51%) over the debt contract (37%; some were indifferent). Furthermore, most of the difference seems to be because

of debt aversion due to labeling. Labeling a contract a loan decreased the probability that a respondent would choose it by more than 8%, ceteris paribus.

Moreover, based on the results from the second part of the survey, we estimate that participants placed a statistically significant premium of about 4% of the financed value on avoiding contracts labeled as debt. Our results suggest that students disfavor contracts labeled as debt, so the way financing alternatives are presented may have a major impact on their choices.

The results in this study imply, at least for human capital investment, that debt aversion can distort investments. The results have implications for policymakers promoting access to higher education and for providers of student financing: The label of the financial mechanism matters. More broadly, debt aversion may act as a self-imposed borrowing constraint affecting portfolio decisions and, indirectly, asset prices.

The remainder of the article is organized as follows. The next section revisits previous literature on the topic. The Survey Description section explains in more detail the survey methodology and the identification strategy, Data and Results presents and analyzes the results, and Discussion of Results provides additional discussion focusing on potential forthcomings of the study. In the Conclusion we revisit the results and highlight their relevance for the design of student financing products and policies.

Literature Review

This article fits into the literature that attempts to understand household decisions on investment, savings, and portfolio allocation. Several authors have pointed out that agents forgo what is effectively "free cash" (Choi et al. 2011) and that asset allocation and trading decisions are not driven by rational decisions or better information (e.g., Beshears et al. 2008; Odean 1998). We complement this literature by studying preferences for various financing options given a large investment opportunity.

Several authors have also studied credit frictions in different settings to understand how they affect optimal choices and to explain asset pricing anomalies (e.g., Constantinides et al. 2002; Guiso et al. 1996). These frictions are typically associated with asymmetric information or a compatibility constraint given by bankruptcy laws (Zhang 1997). In this article we explore debt aversion as a potential source of a credit friction, particularly in education financing.

Ever since Becker (1964) and Mincer (1974) started measuring the returns to education, the estimates have been surprisingly high around the world (see Psacharopoulos and Patrinos 2004 for a survey). The ratio between the wages of college graduates and high-school graduates varies across time and countries but is typically 150% or higher, implying

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[5] The population consisted of people who had applied for financial aid until 2011 through Lumni Inc., a for-profit student-financing company, operating in Chile, Colombia, Mexico. Lumni does not offer loans to students, but instead offers variations of HCCs in which students agree to pay a percentage of their income during a fixed period of time. By February 2012, Lumni had provided financing for approximately 2500 students and had received twice as many applications in the four countries. The vast majority of survey respondents had applied to an education institution in their own country. One of the authors of this study is a co-founder and the second largest shareholder in Lumni.

a 50% premium or greater for college graduates.[6] Further, although returns to education are generally higher in developing countries than in developed countries, developing countries also have lower levels of education on average (Psacharopoulos and Patrinos 2004). This suggests that, particularly in developing countries, people may be under-investing in education.[7] One possible reason for this under-investment is the frictions that prevent an efficient match between capital and education investments.

Barr (2001) has suggested that information asymmetry is an important source of such friction. Students lack good information about the value of education, and potential investors cannot get a tangible asset as collateral or force graduates to work to obtain a return on their investment. So valuable investment opportunities go unfunded.

Another source of friction is aversion to risk. Education is a risky investment, even though it is worthwhile on average. Debt, by far the most widespread credit instrument to finance education, does not transfer much risk away from the student, so students with loans take on substantial risk. As a result, risk-averse individuals are less likely to take loans. Friedman (1955) proposed an alternative to debt, analogous to the equity investments by capitalists in risky projects. Palacios (2004) analyzed this alternative, which he referred to as HCCs. Starting with the introduction of Australia's income-contingent loan program in 1989, an increasing amount of income-contingent loans, equivalent to an HCC with a cap on the value of payments made by students, have been made available by governments and in a few instances by the market.[8]

These instruments transfer risk away from students and thus partially alleviate the problem that students face in financing their education with loans.

Debt aversion may be another source of friction in the higher education financing market. Students who dislike debt might simply not invest as much in their education as they would if the contract were framed differently. This underinvestment in education would imply lower wages over the rest of their career. Evidence supporting the existence of debt aversion stems from surveys suggesting that individuals belonging to particular demographic groups are unwilling

to finance their education using debt (see Rasmussen 2006 and references therein). Experimental data offer contradictory evidence: Eckel et al. (2007) found no evidence of debt aversion among Canadian citizens, while Field (2009) has provided empirical evidence that a contract framed as a loan is less attractive for students than a convertible grant (a grant that converts into a loan) even though the financial payoffs are identical. The study of borrowing constraints from the perspective of debt aversion is, to our knowledge, present only in the labor economics literature.

Our work is similar in spirit to that of Eckel et al. (2007) and Field (2009). In the case of Field (2009), the choices were made in a real transaction, while in Eckel et al. (2007) the choices were made in a survey under which respondents enter a lottery whose payoffs are related to their answers. We offered equivalent options to students and observed their choices in a survey, contributing to the literature in two ways. First, our design allowed us to disentangle framing effects and labeling effects, to shed new light on the nature of debt aversion. Second, our study sample comprised individuals who typically apply for financial aid to continue their studies in developing countries, where underinvestment in education is particularly important.

The labeling effects that we document are consistent with previous research findings that labels for programs or contracts affect economic decisions. Kooreman (2000) found evidence consistent with expenditures on children's clothing increasing because a benefit is labeled as a child benefit, implying that income from different sources is not seen as fungible by some households. Our work complements that literature by showing that the perception of the value of future payments is affected by how the debt is labeled.

Survey Description

This section describes the survey and sample in detail, emphasizing the research design used to identify debt aversion due to labeling and framing.

The main null hypothesis tested through the survey is whether individuals were rational in the current sense of the literature: Would they consider the financial features of the contracts to be the sole relevant features of the contracts? More precisely, the null hypothesis is that financially equivalent contracts would be equally preferred, on average. The key assumption here (under both the null and the alternative hypotheses) is that we indeed have an experiment: The average propensity to choose a given contract in the treatment and the control group is the same for any given contract.

We designed the survey to refine our understanding of the sources of debt aversion for students. In particular, the design allows disentangling debt aversion due to the description of the contract (the framing effect) from that due to

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[6] Ability bias may affect the estimates. Nevertheless, the literature on returns to education that uses quasi-experimental designs has also found high estimates (e.g., Angrist and Krueger 1991; Ashenfelter and Krueger 1994). See also Card (1999, 2001).

[7] There is also ample evidence of underinvestment in education because of credit constraints in developed countries (e.g., Carneiro and Heckman 2002).

[8] Following the introduction of Australia's Higher Education Contribution Scheme, several countries implemented similar programs, including Chile, New Zealand, South Africa, the United Kingdom, and the United States, among others.

the labeling of the contract (the labeling effect). Previous work by others has not disentangled these two effects, so their results reflect only the combined effect of framing and labeling.

We conducted the survey by emailing more than 3000 people in Chile, Colombia, and Mexico who had applied for education financing (about 1800 of whom had received it) between 2007 and 2010.[9] Of these people, 1422 responded to the survey. Respondents were automatically assigned to either the treatment or the control group through a random algorithm.

After obtaining demographic and current status information, the survey followed with 15 questions in the same order for all participants. The key questions for this survey were Questions 1–8.[10] Of the 1422 respondents, only 767 responded to the eight relevant questions, and throughout the article we focus on the subsample that answered each question.[11]

The survey was conducted in Spanish, and the appendix contains the translated text of the survey's most relevant questions (the original text is available on request). The surveys differed slightly across countries to reflect the relative cost of education in each country. Whereas students in Colombia typically need US$3000 to finance a year of education, students in Chile need US$6700 and students in Mexico US$4150. The amounts were converted into local currencies, and the size of hypothetical loans and payments were adjusted to reflect realistic contracts in each country. (Panel A of Table 1 shows the amounts in local currency, and Panel B shows the amounts in US dollars).

Survey Questions and Identification Strategy

Questions 1–3: Choices Between Contracts

To explore debt aversion due to framing, Question 1 presented respondents with financially equivalent contracts framed differently. The contract framed as a loan stated that a fixed payment had to be made every month, except in months when income was below a certain threshold and only a percentage of income had to be paid. The contract framed

as an HCC stated that a percentage of income had to be paid every month except in months when income was above a certain threshold, in which case a fixed payment had to be made. The contract parameters were specified so that the monthly payment, regardless of income, was the same across contracts. In particular, the income threshold at which payments switched from variable to fixed was the same in both cases. (The threshold levels for each contract are reported in Table 1.) Debt aversion due to framing appears here as respondents avoiding the contract framed as debt.

To explore the presence of debt aversion due to labeling, we randomly assigned respondents to one of two groups, the treatment group or the control group. People in the treatment and control groups received exactly the same descriptions of the debt and the HCC contract, with one exception: The descriptions for the treatment group included the labels "loan" and "HCC," whereas the descriptions for the control group did not. Debt aversion due to framing and labeling appears here as respondents in the treatment group avoiding the contract framed and labeled as debt, and debt aversion due to labeling appears here as respondents in the treatment group avoiding the loan contract even more frequently than respondents in the control group. This identification strategy is analogous to the difference-in-differences design widely used in empirical work.

In Question 2 we changed the parameters so that the loan contract was better than the HCC, and in Question 3 we changed the parameters so that the loan contract was worse than the HCC.[12] We expected that more people would accept the better contract in each question. Because this was expected to happen for both the treatment and control groups, we could identify debt aversion due to labeling as respondents in the treatment group avoiding the loan contract more than respondents in the control group did.

Question 4: A Choice After Being Told Contracts are Financially Equivalent in Question 1

As a follow up to Questions 1–3, we performed a second type of test about the presence of debt aversion. In Question 4 participants were told that the two contracts in Question 1 were equivalent and asked to state their preference again.[13] In this question we tested whether debt aversion exists in an environment where subjects are aware of the financial equivalence of the two contracts they are choosing between. This setting provides even stronger evidence of debt aversion

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[9] The vast majority of people contacted had applied to an education institution in their own country.

[10] The order of the questions in the survey did not follow the order in which we present them here. The survey asked the questions that we presumed required higher effort at the beginning.

[11] We provide evidence in the next section that the subsample that answered all the relevant questions is reasonably balanced with respect to observables across treatment status. This is expected, since the difference in the questions for treatment and control groups was not likely to imply any difference in the costs of responding to the survey.

[12] Contract A is better than Contract B in the sense that the payments are lower in Contract A than in Contract B for some future income levels and never higher, regardless of future income level. More formally, Contract A first-order stochastically dominates Contract B.

[13] Survey participants could not go back to Question 1 to see what they had originally answered.

than before because we controlled for the possibility that respondents did not understand the payoffs of each contract when expressing a preference.

Questions 5–8: Value of Debt Aversion

The previous questions allowed us to test for the presence of debt aversion and debt aversion due to labeling among respondents. We further tested for debt aversion due to labeling by quantifying the premium that respondents would be willing to pay to avoid a contract labeled as debt.

If debt aversion due to labeling exists, a borrower would be willing to pay less in a contract labeled as debt than in a financially equivalent contract not labeled as debt. Thus, in Questions 5–8 we offered respondents two contracts: a capped contract, with monthly payments that could not exceed $200, depending on future income,[14] and a non-capped contract, with fixed monthly payments of $X. The respondent was asked what value of X would make her or him indifferent between the two contracts. The numbers reported in the next section have been normalized to be a proportion of the maximum value of the capped contract ($200). Because a contract with a fixed payment of $X = \$200$ could never be better than a contract with maximum payment of $200, the reported values are less than or equal to 1. We asked four questions, depending on when the person would enter the workforce (1 or 2 years) and depending on the framing of the first contract (HCC or flexible debt). In all four questions the only difference between the question for the treatment group and that for the control group was that for the treatment group the second contract was labeled "debt," whereas for the control group the second contract was labeled "a different contract." Again, the difference-in-differences design allowed identifying the premium to avoid a contract labeled as debt even though the contracts were not financially equivalent. A premium to avoid a contract labeled as debt would appear as respondents in the treatment group reporting a lower X than respondents in the control group did.

Econometric Analysis

This section explains the analysis of survey answers taking more than one question at a time, to obtain more-precise estimates. The identification strategy still relies on random assignment to the treatment and control groups, as previously stated.

Questions 1–4

We tested for the preference between a loan and an HCC using Questions 1, 2, and 3 together, according to the following panel regression:

$$Loan_{i,j} = \alpha_1 + T_i\phi + LoanBetter_j\alpha_2 + HCCBetter_j\alpha_3 + \epsilon_{i,j},$$
(1)

where $Loan_{i,j}$ equals 1 if respondent i chose debt in question j, 0 if respondent i chose indifferent in question j, and -1 if respondent i chose the HCC in question j, $j = 1, 2, 3$; $LoanBetter_j$ is an indicator variable for whether $j = 2$; $HCCBetter_j$ is an indicator variable for whether $j = 3$; and T_i equals 1 if respondent i was assigned to the treatment group and 0 if respondent i was assigned to the control group. Since we included multiple answers for each individual in our regressions, we clustered at the individual level.

In Eq. (1), α_1 identifies debt aversion due to framing, and ϕ identifies debt aversion due to labeling. The measure of debt aversion commonly used in the literature is $\alpha_1 + \phi$. α_2 is naturally expected to be positive and α_3 to be negative.

We also report two additional specifications by augmenting Eq. (1) by observed demographic variables to improve the precision of the estimates. In addition, we report estimates using a multinomial logit specification, to relax the assumption of linearity in Eq. (1).

To end this section, we repeat the analysis only for respondents of Question 4 to study the results when respondents have been told that the two contracts have identical payoffs.

Questions 5–8

We also estimated the premium to avoid a contract labeled as debt using Questions 4, 5, 6, and 7 together, according to the following panel regression:

$$X_{i,j} = \alpha_4 + T_i\beta + Q5_j\alpha_5 + Q6_j\alpha_6 + Q7_j\alpha_7 + \eta_{i,j}$$
(2)

where Ql_j is an indicator variable equal to 1 for $j = l$, $j, l = 5, 6, 7$; T_i equals 1 if respondent i was assigned to the treatment group and 0 if respondent i was assigned to the control group; and $X_{i,j}$ is the answer to question j by respondent i. The parameter β is the premium to avoid a contract labeled as debt. We also report two additional specifications by augmenting Eq. (2) with observed demographic variables to improve the precision of the estimates.

Data and Results

Table 2 shows demographic characteristics of respondents based on treatment status. Their average age was 23, and less than 10% had children or were married. About two-thirds

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[14] The number $200 changes across countries, as discussed before.

Table 2 Summary statistics

	Treatment		Control		Difference	
	N = 378		N = 389		N = 767	
	Mean	Std. error	Mean	Std. error	Mean	Std. error
Age	23.4418	0.2625	23.5039	0.2705	− 0.0621	0.3772
Male	0.5423	0.0257	0.5476	0.0253	− 0.0052	0.0360
Student	0.6931	0.0238	0.7172	0.0229	− 0.0241	0.0330
Colombia	0.6058	0.0252	0.5861	0.0250	0.0197	0.0355
Mexico	0.2884	0.0233	0.2828	0.0229	0.0056	0.0327
Chile	0.1058	0.0158	0.1311	0.0171	− 0.0253	0.0234
Children	0.0794	0.0139	0.0951	0.0149	− 0.0158	0.0204
Married	0.0688	0.0130	0.0643	0.0124	0.0045	0.0180
Mother education	0.7037	0.0235	0.7018	0.0232	0.0338	0.0252
Father education	0.6667	0.0242	0.6427	0.0243	0.024	0.0343

were still students, and about half were male. Most were in Colombia or Mexico. Respondents were also very similar, on average, in observed demographic characteristics across treatment status. As discussed in the previous section, this is expected because respondents were randomly assigned to the two groups and because the difference in the questions across groups is unlikely to have generated further selection problems.

Testing for Debt Aversion

We begin with an analysis of respondents' choices between financially equivalent contracts framed or labeled differently. Table 3 shows the raw data from respondents' answers to Questions 1–3. When confronted with financially equivalent contracts (Question 1), one framed and labeled as debt and the other framed and labeled as an HCC, 37% of respondents in the treatment group chose debt, and 50% chose the HCC. This difference (13 percentage points), also seen in Panel A of Table 4, is our first evidence of debt aversion. The difference encompasses both framing and labeling effects and is thus similar in nature to the coefficients reported in the previous literature (e.g., Field 2009). The difference of 2 percentage points in the control group suggests that debt aversion due to framing is small. The difference-in-differences estimate, 11 percentage points (13-2) suggests that most debt aversion is due to labeling.

Table 3 and Panel A of Table 4 show also the results for Questions 2 and 3. When the loan contract is better than the HCC, some respondents in both the treatment and control groups switched from being indifferent to choosing the loan contract, and vice-versa, which suggests some internal consistency in the results. Debt aversion due to labeling is still high and statistically significant for these questions, though the estimates are relatively imprecise, ranging from 7% for Question 3 to 18% for Question 2.

To exploit the information in the first three questions together and to obtain more precise estimates, we simultaneously estimated Eq. (1) for Questions 1–3. Panel B of Table 4 summarizes the results for this model.

The first column of Table 4 shows the results of Eq. (1) without control variables. Two coefficients are of interest. First, "Constant" identifies debt aversion due to framing. Although not statistically significant, the estimate (− 0.0202) is consistent with the value in Panel A. Second, "Treatment" identifies debt aversion due to labeling. The estimate (− 0.118) is significant at the 5% level. This coefficient means that a respondent for whom the contracts are labeled as a loan and an HCC is more likely to choose the HCC than a respondent for whom the contracts have exactly the same wording without any labels. Thus, Table 4 provides further evidence of debt aversion due to labeling. Our estimate of debt aversion under this specification (− 0.138) is the sum of the coefficients for "Treatment" and "Constant" and implies that respondents were more likely to choose the HCC over an income-contingent loan, even though the two contracts were financially equivalent.

Table 3 Questions 1–3: responses

Question	Alternative	Treatment	Control	N
1 Equivalent alternatives	Loan	0.3708	0.4060	790
	HCC	0.5064	0.4286	
	Indifferent	0.1228	0.1654	
Loan better	Loan	0.4212	0.4962	778
	HCC	0.4884	0.3836	
	Indifferent	0.0904	0.1202	
HCC better	Loan	0.3532	0.3795	775
	HCC	0.5636	0.5128	
	Indifferent	0.0831	0.1077	

HCC is human capital contract

Table 4 Questions 1–3: cross-sectional and panel regressions

	Equivalent alternatives		Loan better		HCC better	
	N = 790		N = 778		N = 775	
	Mean	Std. error	Mean	Std. error	Mean	Std. error
Panel A: cross-sectional regression						
Treatment	− 0.1355**	0.0469	− 0.0672	0.0484	− 0.2104***	0.0477
Control	− 0.0226	0.0458	0.1125**	0.0472	− 0.1333*	0.0474
Difference	− 0.1130†	0.0655	− 0.1797**	0.0676	− 0.0771	0.0672
	Specification 1		Specification 2		Specification 3	
	N = 2343					
	Coeff.	Std. error	Coeff.	Std. error	Coeff.	Std. error
Panel B: panel regression						
Treatment	− 0.118*	0.0545	− 0.115*	0.0544	− 0.114*	0.0544
Constant	− 0.0202	0.0422	− 0.0217	0.0472	− 0.128	0.1780
Loan better	0.101**	0.0333	0.101**	0.0333	0.101**	0.0333
HCC better	− 0.0954**	0.0329	− 0.0954**	0.0329	− 0.0956**	0.0330
Mexico			− 0.0441	0.0632	− 0.0233	0.0873
Chile			0.108	0.0850	0.128	0.0943
Age					0.00586	0.0072
Male					− 0.0441	0.0549
Student					0.0818	0.0651
Children					− 0.142	0.1110
Married					0.0585	0.1280
Mother education					0.00463	0.0744
Father education					− 0.105	0.0732

HCC is human capital contract.

The dependent variable in both panels is $Loan_{i,j}$, which equals 1 if respondent i chose debt in question j, -1 if respondent i chose the HCC, and 0 if respondent i chose indifferent

† < .10, *p < .05, **p < .01, ***p < .001

The coefficient for "Loan Better" (0.101) has the expected sign (because more people should choose the loan when it is cheaper) and is significant at the 1% level. The coefficient for "HCC Better" (− 0.0954) also has the expected sign and is significant at the 1% level.

The second and third specifications of Table 4 estimate the same model with control variables. Specification 2 includes indicator variables for the country, and Specification 3 includes indicator variables for the country as well as age, gender, whether the respondent is still a student, parent education level, and presence of children. The estimated coefficient for "Treatment" does not change, providing further evidence of the random assignment.

One issue with the results in Table 4 is that the three possible outcomes make the coefficients difficult to interpret, whereas in a binary choice the coefficient can be interpreted as the marginal increase in the probability of choosing a particular contract. To deal with this issue, we ran regressions on Eq. (1) with outcomes of debt or not debt, where not debt included choosing the HCC or being indifferent, and with outcomes of HCC or not HCC, where not HCC included choosing debt or being indifferent. The results are in Tables 5 and 6.

Table 5 does not provide enough information about framing effects, since the constant includes indifference as well as choosing debt. However, the results do allow us to comment on labeling effects. In this case they are consistent with those in Table 4, providing evidence of debt aversion due to labeling, though the magnitude (− 0.0421) is not statistically significant. The probability of choosing the debt contract increased by about 7 percentage points (significant at the 1% level) when the loan contract was better and decreased by about 2 percentage points (not statistically significant) when the HCC was better. These results are consistent for the three specifications outlined in Table 4.

Table 6 reveals similar but opposite effects when the choice is between taking the HCC and not taking the HCC. In this case the coefficient for choosing the HCC is 0.0755

Table 5 Choosing between two contracts (Questions 1–3): binary panel regression

	Specification 1		Specification 2		Specification 3	
	Coeff.	Std. error	Coeff.	Std. error	Coeff.	Std. error
Treatment	− 0.0421	0.0285	− 0.0408	0.0285	− 0.0401	0.0284
Loan better	0.0699***	0.0181	0.0699***	0.0181	0.0697***	0.0181
HCC better	− 0.0232	0.0171	− 0.0232	0.0171	− 0.0233	0.0172
Mexico			− 0.0402	0.0328	− 0.0215	0.0450
Chile			0.0634	0.0453	0.0782	0.0499
Age					0.00226	0.00371
Male					− 0.0333	0.0287
Student					0.0498	0.0337
Children					− 0.0750	0.0581
Married					0.0426	0.0660
Mother education					− 0.0185	0.0388
Father education					− 0.0423	0.0387
Constant	0.409***	0.0227	0.413***	0.0254	0.380***	0.0914
Observations	2343		2343		2343	
Number of id	790		790		790	

HCC is human capital contract.

Panel regression—$Loan_{i,j}$ equals 1 if respondent i chose *Debt* in question j and 0 otherwise

***$p < .001$

Table 6 Choosing between two contracts (Questions 1–3): panel regression

	Specification 1		Specification 2		Specification 3	
	Coeff.	Std. error	Coeff.	Std. error	Coeff.	Std. error
Treatment	0.0755***	0.0292	0.0746*	0.0291	0.0735*	0.0291
Loan better	− 0.0312†	0.0169	− 0.0311†	0.0169	− 0.0310†	0.0169
HCC better	0.0722***	0.0179	0.0722***	0.0179	0.0723***	0.0179
Mexico			0.00389	0.0341	0.00180	0.0474
Chile			− 0.0444	0.0445	− 0.0500	0.0498
Age					− 0.00360	0.00401
Male					0.0108	0.0293
Student					− 0.0320	0.0350
Children					0.0665	0.0601
Married					− 0.0160	0.0689
Mother education					− 0.0231	0.0395
Father education					0.0624	0.0386
Constant	0.430***	0.0227	0.434***	0.0254	0.508***	0.0981
Observations	2343		2343		2343	
Number of id	790		790		790	

HCC is human capital contract.

Panel regression - $Loan_{i,j}$ equals 1 if respondent i chose *HCC* in question j and 0 otherwise

† $< .10$, *$p < .05$, ***$p < .001$

for the treatment group (significant at the 1% level). Under this specification, labeling contracts increased the probability of choosing the HCC by about 7.5 percentage points across specifications. Together, the results in Tables 5 and 6 are consistent with the presence of debt aversion due to labeling.

We repeated the previous analysis relaxing the linearity assumption of Eq. (1) by running a multinomial logit regression, where the choices were a preference for the HCC, indifference, or a preference for the loan. Table 7 reports the results for the marginal effect on the probability of choosing a given option, where the options are given in each column.

Table 7 Choosing between two contracts (Questions 1–3): multinomial logit regression

	HCC		Indifferent		Loan	
	Coeff.	Std. error	Coeff.	Std. error	Coeff.	Std. error
Treatment	0.0762***	0.0208	− 0.0314*	0.0129	− 0.0448*	0.0205
Loan better	− 0.0349	0.0255	− 0.0337*	0.0139	0.0686**	0.0252
HCC better	0.0689**	0.0255	− 0.0436**	0.0138	− 0.0253	0.0251
Mexico	0.00303	0.0325	0.0184	0.0206	− 0.0214	0.0318
Chile	− 0.0512	0.0360	− 0.0268	0.0202	0.0780*	0.0362
Age	− 0.00391	0.00278	0.00149	0.00168	0.00242	0.00272
Male	0.0122	0.0211	0.0221†	0.0129	− 0.0342†	0.0208
Student	− 0.0330	0.0246	− 0.0169	0.0154	0.0499*	0.0239
Children	0.0690	0.0429	0.00315	0.0273	− 0.0722†	0.0403
Married	− 0.0176	0.0475	− 0.0229	0.0243	0.0404	0.0477
Mother education	− 0.0229	0.0280	0.0444**	0.0154	− 0.0215	0.0275
Father education	0.0643*	0.0274	− 0.0248	0.0181	− 0.0396	0.0272
Observations	2343		2343		2343	

HCC is human capital contract.

The dependent variable is $Loan_{i,j}$, which equals 1 if respondent i chose debt in question j, -1 if respondent i chose the HCC, and 0 if respondent i chose indifferent. The coefficients should be interpreted as the marginal effect on the probability of choosing the alternative labeled in the column

† $< .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

Table 8 Measuring the cost of Debt Aversion (Questions 1–3): ordered probit regression

	HCC		Indifferent		Loan	
	Coeff.	Std. error	Coeff.	Std. error	Coeff.	Std. error
Treatment	0.0608**	0.0197	− 0.00172*	0.000720	− 0.0591***	0.0191
Loan better	− 0.0508*	0.0239	0.00112*	0.000528	0.0497*	0.0235
HCC better	0.0495*	0.0241	− 0.00169	0.00104	− 0.0478*	0.0232
Mexico	0.00910	0.0307	− 0.000270	0.000957	− 0.00883	0.0297
Chile	− 0.0654†	0.0339	0.000656	0.000557	0.0647†	0.0342
Age	− 0.00312	0.00261	8.81e-05	7.73e-05	0.00303	0.00254
Male	0.0228	0.0200	− 0.000626	0.000558	− 0.0222	0.0194
Student	− 0.0410†	0.0233	0.00141	0.000990	0.0395†	0.0224
Student	0.0726†	0.0408	− 0.00359	0.00292	− 0.0691†	0.0380
Married	− 0.0271	0.0448	0.000535	0.000534	0.0266	0.0443
Mother education	− 0.000272	0.0264	7.68e-06	0.000749	0.000264	0.0257
Father education	0.0525*	0.0260	− 0.00117*	0.000576	− 0.0513*	0.0255
Observations	2343		2343		2343	

HCC is human capital contract.

The dependent variable is $Loan_{i,j}$, which equals 1 if respondent i chose debt in question j, -1 if respondent i chose the HCC, and 0 if respondent i chose indifferent. The coefficients should be interpreted as the marginal effect on the probability of choosing the alternative labeled in the column

† $< .10$, * $p < .05$, ** $p < .01$, *** $p < .001$

The evidence of debt aversion due to labeling is still high and statistically significant, but under this specification it is much more precise: 7.6% of respondents chose the HCC because of debt aversion due to labeling. If the options had not been labeled debt, 3.14% of respondents (about 40% of the 7.6% who chose the HCC) would have been indifferent, and 4.48% (the other 60% of the 7.6%) would have chosen the loan. Moreover, the sign of the coefficients for "Loan Better" and "HCC Better" were consistent with the results found in Table 6, though not always statistically significant.

We ran a similar analysis using an ordered probit regression, given the clear ordering of loan preferred, indifference, and HCC preferred. Table 8 reports the results for the marginal effect on the probability of choosing a given option.

Table 9 Results after knowledge of financial equivalence of contracts (Question 4): cross-sectional regression

	Specification 1		Specification 2		Specification 3	
	N=767					
	Coeff.	Std. error	Coeff.	Std. error	Coeff.	Std. error
Treatment	− 0.189**	0.0667	− 0.185**	0.0667	− 0.183**	0.0669
Constant	− 0.0231	0.0464	− 0.0100	0.0548	0.2600	0.2190
Mexico			− 0.0941	0.0743	0.0423	0.0995
Chile			0.1020	0.1070	0.1770	0.1150
Age					− 0.0101	0.0088
Male					− 0.132†	0.0673
Student					0.115	0.0773
Children					− 0.0290	0.1440
Married					− 0.1340	0.1510
Mother education					− 0.1340	0.0912
Father education					0.0018	0.0897

HCC is human capital contract.

The dependent variable is $Loan_{i,4}$, which equals 1 if respondent i chose debt in question 4, -1 if respondent i chose the HCC, and 0 if respondent i chose indifferent

**$p< .01$

Table 10 Results after knowledge of financial equivalence of contracts (Question 4): multinomial logit regression

	HCC		Indifferent		Loan	
	N=767					
	Coeff.	Std. error	Coeff.	Std. error	Coeff.	Std. error
Treatment	0.1299***	0.0361	− 0.0742***	0.0233	− 0.0557	0.0354
Mexico	− 0.0412	0.0571	0.0449	0.0404	− 0.0037	0.0562
Chile	− 0.0945	0.0632	0.0080	0.0425	0.0865	0.0640
Age	0.0048	0.0049	0.0005	0.0033	− 0.0053	0.0048
Male	0.0502	0.0370	0.0348	0.0229	− 0.0850**	0.0360
Student	− 0.0620	0.0428	0.0035	0.0260	0.0585	0.0413
Children	0.0494	0.0762	− 0.0695*	0.0333	0.0200	0.0752
Married	− 0.0545	0.0826	0.0558	0.0627	− 0.0012	0.0835
Mother education	0.0474	0.0490	0.0416	0.0286	− 0.0890†	0.0482
Father education	0.0057	0.0484	− 0.0112	0.0319	0.0055	0.0465

HCC is human capital contract.

The dependent variable is $Loan_{i,4}$, which equals 1 if respondent i chose debt in Question 4, -1 if respondent i chose the HCC, and 0 if respondent i chose indifferent. The coefficients should be interpreted as the marginal effect on the probability of choosing the alternative labeled in the column

† $< .10$, *$p< .05$, **$p< .01$, ***$p < .001$

The evidence of debt aversion due to labeling is consistent with the evidence from the multinomial logit regression (Table 7): 6.1% of the sample chose the HCC because of debt aversion due to labeling. If the options had not been labeled, 5.9% of respondents (97% of the 6.1% who chose the HCC) would have chosen the loan. The ordered probit highlights that most of the labeling effect comes from switching between contracts rather than from being indifferent between them.

Table 9 presents the average treatment effect for Question 4, in which respondents were told that the contracts were financially equivalent. The results are consistent with the findings discussed so far. The constant is negative but not statistically significant in Specifications 1 (− 0.02) and 2 (− 0.01), suggesting a small but nonsignificant framing effect. Specification 3 yields a large but statistically nonsignificant result, reflecting large differences across demographic groups. We do not attempt to explain these differences because none of them, except gender, is statistically significant.

A multinomial logit analysis confirms the result that respondents in the treatment group are more likely to avoid

Table 11 Measuring the cost of Debt Aversion (Questions 5–8): responses

Question	Treatment			Control			Difference	
	N	Mean	Std. error	N	Mean	Std. error	Mean	Std. error
Panel A: respondents of Questions 5–8								
Question 5	378	24.237	22.043	389	2.412**	0.801	21.816	21.74
Question 6	378	1.988***	0.377	389	1.729***	0.339	0.258	0.508
Question 7	378	1.863***	0.361	389	1.546***	0.244	0.316	0.434
Question 8	378	1.627***	0.324	389	2.025**	0.7196	− 0.397	0.797
Panel B: only respondents with $X \leq 1$								
Question 5	268	0.742***	0.018	285	0.776***	0.016	− 0.034	0.024
Question 6	298	0.656***	0.020	323	0.665***	0.020	− 0.008	0.028
Question 7	292	0.678***	0.017	300	0.729***	0.016	− 0.0502*	0.023
Question 8	310	0.674***	0.016	328	0.735***	0.015	− 0.060**	0.022

HCC is human capital contract

*$p < .05$, **$p < .01$, ***$p < .001$

the debt contract. The treatment coefficient in Table 9 (− 0.18) does not change across different specifications and is significant at the 1% level. It provides evidence that respondents for whom the contracts were not labeled were more likely to choose the HCC. Table 10 corroborates this result: Respondents in the treatment group were 5.5 percentage points less likely to choose the loan (not statistically significant), 7.4 percentage points less likely to be indifferent (significant at the 1% level), and 13 percentage points more likely to choose the HCC (significant at the 1% level).

Given the above analysis, we conclude that there is evidence of debt aversion, particularly due to labeling, in our sample, which is similar to the population that routinely applies for education financial aid. The results are particularly strong in the light of answers to Question 4, since respondents were told that the contracts are financially equivalent. Lack of awareness about the payoffs does not seem to drive the results.

Measuring the Value of Debt Aversion

The evidence of debt aversion due to labeling from Questions 1–4 led us to measure the monetary value of debt aversion. This section reports the results of asking survey respondents to choose between a fixed payment and an income-contingent contract. In this case, the only difference between the treatment and control groups was the label of the contract: The treatment group saw the fixed payment contract labeled as "debt" and the control group as a "different contract." We tested whether the average fixed payment differed between groups.[15]

The contingent contract stated a maximum that the respondent would have to pay monthly. We report the results as a proportion of the monthly maximum. Panel A of Table 11 shows the average payment for the total sample and for the treatment and control groups for the four related questions (Questions 5–8). Some respondents did not understand the question, as evidenced by the fact that the reported average monthly payment is over 100% of the maximum in the contingent contract. That the average reported values in Question 5 are substantially larger than those for Questions 6–8 provides further evidence.[16]

Panel B of Table 11 shows the average payment when we restrict the sample to respondents who answered with values less than or equal to 100% of the maximum. The restricted sample excludes, depending on the question, 16–29% of the full sample.[17] The difference between the treatment and control groups show that respondents place some premium on avoiding contracts labeled as debt, though the estimated values are statistically significant only for Questions 7 and 8.

Table 12 pools all the data from the four questions to estimate the premium to avoid a contract labeled as debt, as in Eq. (2). Three specifications of the model are used, with different controls added in the regression. Specification 1 summarizes the findings for the model stated in Eq. (2). Respondents in the treatment group were willing to pay, on average, 3.97% less per month than respondents in the control group (significant at the 5% level). That coefficient is precise, stable, and statistically significant across specifications. The result for the best specification (Specification 3) implies that respondents were willing to pay 4.5% less per month on a contract labeled as debt.

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[15] In our context the maximum amount the person is willing to hand over to avoid a specific contract.

[16] Respondents were not allowed to revise their answers after completing a question.

[17] These results are available on request.

Table 12 Measuring the cost of Debt Aversion (Questions 5–8): panel regression

	Specification 1 N=2404		Specification 2		Specification 3	
	Coeff.	Std. error	Coeff.	Std. error	Coeff.	Std. error
Treatment	− 0.0397*	0.0189	− 0.0431*	0.0184	− 0.0448**	0.0183
Question 5	0.782***	0.0147	0.786***	0.0163	0.621***	0.0649
Question 6	− 0.0976***	0.0139	− 0.0959***	0.0139	− 0.0960***	0.0139
Question 7	− 0.0554***	0.0111	− 0.0553***	0.0111	− 0.0555***	0.0112
Question 8	− 0.0578***	0.0111	− 0.0577***	0.0111	− 0.0576***	0.0111
Mexico			0.0607**	0.0231	0.0124	0.0290
Chile			− 0.153***	0.0210	− 0.195***	0.0251
Age					0.00602*	0.0026
Male					0.0014	0.0182
Student					0.0173	0.0224
Children					− 0.0642	0.0465
Married					− 0.0851	0.0569
Mother education					0.0251	0.0253
Father education					0.0361	0.0244

HCC is human capital contract. This table shows the panel regression results of the fixed payment that would make respondents indifferent between the fixed payment and a given income-contingent contract (Questions 5–8), following Eq. (2). The dependent variable is X as a proportion of the monthly fixed payment that will make the respondent indifferent in each question

*$p < .05$, **$p < .01$, ***$p < .001$

The estimates of the premium to avoid a contract labeled as debt are positive and statistically significant. Taking the results together, there is evidence of debt aversion, due mainly to labeling effects. Given these results, further attempts to estimate a monetary value of the impact of debt aversion become relevant to draw welfare and policy implications.

Discussion of Results

The results in this article support the hypothesis that debt aversion affects financing decisions for large investments. When financing higher education, respondents shy away from a debt contract, particularly because of the label. This section discusses potential drawbacks of our experimental approach and suggests future research related to our findings.

A potential problem with our specification is the different wording used to describe debt. In English they correspond to "debt" and "loan." Informally, when one talks about types of financing, one talks about debt, whereas the actual contract is a loan. Our survey reflected these differences. On the choice between financially equivalent contracts, the actual label was "loan," whereas the questions that referred to the fixed payment included the word "debt." An alternative hypothesis is that loan aversion is different from debt aversion. Further study allowing for this difference would shed light on this question.

One practical constraint in this study was its small sample. The experiment was designed with this restriction in mind. For instance, our goal of identifying differences in the preference for one type of contract over another led us to ask questions in the same order across respondents. However, if the order of the questions is a determinant in respondents' answers, our estimates are biased. With a larger sample, randomizing the order of the questions would have allowed us to control for this issue.

Moreover, before asking students for their preference, we could have asked whether they understood that the contracts they were asked to choose between were financially equivalent. Including such a question would have shed more light on why respondents are averse to debt. But including the question would have been artificial in the sense that in the real world students make decisions without the financial implications being spelled out for them.

A related concern with the experimental design is the robustness of results when considering a wider range of settings. Although we conducted our survey with students in different countries, the question remains: How universal are the results? The answer requires future research in a wide variety of geographical, cultural, and institutional settings.

As a consequence of the previous observation, our results should be interpreted cautiously in settings that differ significantly from the higher education systems in Chile, Colombia, and Mexico. One potentially major difference is the perception and actual proceedings around bankruptcy.

Whereas consumers in the United States use bankruptcy with relative frequency, it is much less understood in Latin America. Thus, the consequences of defaulting on debt contracts may be perceived very differently by US consumers (and students) relative to Chilean, Colombian, and Mexican consumers. Similarly, familiarity with debt—particularly student debt—is much greater in the United States, where for decades a large federal program has financed students in the trillions of dollars. In contrast, government higher education financing has traditionally been marginal in Chile, Colombia, and Mexico. Recent developments have greatly increased state funding of higher education, particularly in Chile, but had not been implemented at the time of our study. These two issues point to a lower impact of labeling and framing in the United States. However, research in the United States that replicates our work has found results similar to ours (Boatman et al. 2017).

More generally, our experimental design provides evidence on one link in the education investment decision—the instrument—but does not test other links, such as the impact of the instrument choice on future welfare or on the decision to study. In our main analysis the choices are financially equivalent, implying no welfare effect if a respondent chose one or the other. The question we ultimately want to address is whether the bias away from debt translates into lower investment in education with correspondingly lower welfare afterward. Answering this question requires a more general setup, one left for future research. Another avenue for future research is to measure the consequences of the debt aversion found in this article. Students may not only decide to not invest in education because of debt aversion, but, conditional on accepting a loan, may also change other behaviors, which may lead to unanticipated long-term consequences.

An important shortcoming of this experimental design is that it is a survey, so respondents are not actually choosing a financial instrument in a real transaction. Research in this direction could provide evidence on the extent to which the findings of this article hold in a real financial transaction.

Finally, and more broadly, it would be useful to see whether the findings of this article prevail in other investment decisions.

Conclusion

Standard utility theory predicts that investment and financing decisions depend only on the characteristics of the payoffs of the investment. In particular, the label or framing of a particular financing vehicle should not affect its value. This article's main contributions are to provide a sharper definition of debt aversion than the one previously used in the literature and to test for its presence in an experiment using a relevant sample. Concretely, we tested whether the labeling and framing of debt among a population for whom the financing decision was recent and important affect its perceived value. This question is important because biases in student decisions can lead to the choice of less desirable financial mechanisms or worse, to avoiding a valuable investment. Either consequence can lead to reduced welfare and wasted opportunities. We found that both labeling and framing impact the attractiveness of a financial contract. Labeling a contract as a loan decreased its probability of being chosen over a financially equivalent contract by more than 8 percentage points. We also provide evidence that respondents were willing to pay a premium of about 4% of the financed value to avoid a contract labeled as debt.

Our analysis sheds new light on different aspects of debt perceptions by disentangling two types of debt aversion: one that has been studied before in the literature, which encompasses both framing and labeling effects combined, and another that controls for framing effects and identifies only what we call debt aversion due to labeling. The results suggest that participants in the experiment exhibited debt aversion and that most debt aversion was due to labeling.

These perceptions can prevent individuals from choosing an optimal portfolio or from undertaking attractive investment opportunities, such as in education. More generally, these perceptions act as self-imposed borrowing constraints, reducing welfare and potentially affecting asset prices. They can explain why apparently profitable investments such as higher education are not pursued more widely and why most investors do not actively participate in markets for risky assets. Ultimately, a better understanding of the perceptions of financial assets should lead to better understanding of the drivers of household investment, borrowing, and (indirectly) asset prices.

Our findings deliver suggestions to practitioners and policymakers interested in student financing. The most important point is that in the design of student financing products and policies, students react to both the label of an instrument and the framing of the instrument. In particular, the findings imply that simply naming an instrument debt will lower the student's interest in it. Similarly, framing an instrument like debt by emphasizing the requirement to make fixed payments lowers the student's interest in it. Either way, if lower interest translates into less investment in education—something we do not document, and a clear remaining question for future research—then features in the design of student financial programs can affect the welfare of students and those who would benefit from their acquired skills.

Compliance with Ethical Standards

Conflict of interest Caetano and Palacios received funding from The World Bank. Palacios is co-founder, shareholder, and board member in Lumni Inc.

Ethical Approval All procedures performed in studies involving human participants were in accordance with the ethical standards of the institutional and/or national research committee and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards.

Informed Consent Informed consent was obtained from all individual participants included in the study.

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