Producer Attitudes toward Output Price Risk: Experimental Evidence from the Lab and from the Field*

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Abstract

A number of agricultural and food policy instruments are predicated on the idea that producers dislike output price risk. We test experimentally some theoretical predictions about the behavior of producers in the face of output price risk, namely Sandmo's (1971) prediction that price risk at the extensive margin causes risk-averse producers to decrease how much they produce and Batra and Ullah's (1974) prediction that price risk at the intensive margin causes producers whose preferences exhibit decreasing absolute risk aversion to further decrease how much they produce. We do so in labs with US college students as well as in the field with Peruvian farmers. First, as regards price risk at the extensive margin, we find no support for Sandmo's prediction. Second, consistent with Batra and Ullah's prediction, we find that output decreases in response to price risk at the intensive margin when imposing a linear relationship between output and price risk. When relaxing the assumption of linearity between output and price risk, however, we find a nonmonotonic relationship between output and price risk. Taken together, these results are broadly inconsistent with expected utility theory. Alternative models of behavior in the face of risk, such as prospect theory, may explain the behavior we observe.

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1 Introduction

A well-known empirical regularity in the agricultural and applied economics literature is that agricultural producers tend not to want to purchase crop insurance, and that empirical regularity holds true even when said insurance is heavily subsidized. The persistently low uptake of agricultural insurance has been documented both in developed countries (see Miranda 1991, Knight and Coble 1997, Miranda and Glauber 1997, and Glauber 2004 for the US case) as well as in developing countries (see Platteau, de Bock, and Gelade 2017 for a recent review).

Why are opportunities for risk reduction often not taken up in agricultural settings? In order to begin answering that question, it is useful to start with how producers make production decisions in risky environments. Because agricultural producers tend to produce commodities whose prices are characterized by a higher degree of volatility than either manufactured goods or services, one obvious area of investigation is how producers respond to output price risk. In a seminal article, Sandmo (1971) showed that, starting from a situation of price certainty, a risk-averse producer would hedge against price risk by producing less than she would were she faced with a certain output price equal to the mean of the risky output price distribution. Consistent with this, many agricultural and food policy instruments (e.g., administrative pricing, buffer stocks, marketing boards, and variable tariffs) are predicated on the idea that agricultural producers dislike price risk, and that those same producers respond to price risk by underproducing relative to situations of price certainty.

Sandmo's prediction was entirely theoretical, and it has hitherto not been tested empirically with a high degree of internal validity. To remedy this, we apply experimental methods to Sandmo's prediction. Specifically, we study how experimental subjects, whom we put in the role of producers for whom storage and insurance are not available, behave in the face of output price risk.[1] To do so, we use a two-stage randomized design to determine (i) whether our subjects face a certain or a risky output price, and (ii) conditional on facing a risky price, how much risk they face by taking a randomly chosen mean-preserving spread of the (degenerate) certain price distribution from among four potential such mean-preserving spreads. This allows disentangling the effects of output price risk on production at both the extensive and intensive margins, i.e., it allows disentangling the effect of output price risk relative to output price certainty as well as the effect of more output price risk relative to less of it.

In doing so, we develop an experimental protocol that mimics the theoretical framework developed by Sandmo (1971) to study the behavior of individual producers in the face of price risk. To increase the external validity of our findings, we then use this protocol to test the aforementioned theoretical prediction in three distinct contexts. In the first two contexts, we run our experiments with college students in labs at two US universities. In the third context, we run our experiments in the field, with farmers in Peru's

Lima province.

Our work is in the same vein as a number of empirical contributions on the effects of risk and uncertainty on producer behavior in agricultural and applied economics.[2] Chavas and Holt (1990) looked at the acreage decisions of corn and soybean producers, finding that risk plays an important role in the acreage decision. Barrett (1996) estimated the price risk preferences of rice producers in Madagascar and found that those preferences may drive the inverse farm size–productivity relationship. Myers (2006) extends conventional welfare measures of the costs of food price volatility in low-income countries to allow for both economic growth and food security effects. On the basis of simulations, he finds that the latter two effects might exceed the welfare costs of commodity price volatility.

More recently, Mason and Myers (2013) estimate the effects of the Zambian Food Reserve Agency–whose goal is to stabilize staple prices–on the level and variability of maize prices, then draw on other studies to speculate about the likely welfare effects. Bellemare, Barrett, and Just (2013) estimate the welfare impacts of price risk among seven staple commodities for rural Ethiopian households, finding that price risk is associated with a welfare loss that is on average increasing in income among rural Ethiopian households, which often consume and produce the same commodities.[3] Bellemare (2015) looks at the effects of rising food price and food price volatility on social unrest, finding that although the former cause social unrest, increases in the latter do not. Mattos and Zinn (2016) conduct lab experiments aimed at studying how grain producers form their reference points, and how those reference points adapt over time, finding among other things that those producers' highest experienced price drives their reference point. Similarly, Tonsor (2018) uses a split-sample choice experiment to look at how US cattle producers make decisions in the face of uncertainty, also finding that those producers tend to treat their best experienced outcomes as reference points. In a recent working paper, Lee (2018) studies the relationship between price risk and migration, finding that the presence of price risk appears to be one of the drivers of rural out-migration in rural Ethiopia. Lastly, in another recent working paper, Maples et al. (2018) introduce both input and output price uncertainty in a decision-theoretic framework, which they then test using data on US cattle producers, finding that the presence of input price uncertainty leads to an increased use of inputs as well as a decreased output.

Our contribution is threefold. First, ours is the first experimental test of the theory of producer behavior in the face of output price risk.[4] Second, we bring internally valid empirical evidence to a question of considerable importance for policy in that our experiments reproduce the production conditions faced by many producers in rural areas of developing countries, who lack access to storage technology and face missing insurance markets while nevertheless having to sink resources into production well ahead of the resolution of price risk. Third, and most importantly, our findings shed light on whether and how well expected utility can help researchers and policy makers think through how producers make their production decisions when faced with price risk. This may ultimately help those same researchers and policy makers design better agricultural policy instruments to insure producers against risk and uncertainty.

Our results are as follows. First, we find that producers do not significantly change how much they produce in response to price risk at the extensive margin. Second, we find that output decreases in response to price risk at the intensive margin when imposing a linear relationship between the two. Finally, when relaxing the assumption of linearity between output and price risk, we find that there is a nonmonotonic relationship between output and price risk.

Specifically, we find no support for Sandmo's prediction. Across all settings, when looking at the effect price risk at the extensive margin on production (i.e., what happens to output when price risk is turned on), we find that going from a situation of price certainty to a situation of price risk does not cause our subjects to decrease their production. This finding is consistent with Sandmo's prediction—and thus with expected utility (EU) theory—only if our subjects are risk-neutral. Yet on the basis of a Holt and Laury (2002) risk preference elicitation experiment, we find that the vast majority of our subjects are risk-averse.

When looking at the effect of price risk at the intensive margin on production (i.e., what happens to output in response to more price risk, conditional on there being price risk to begin with), we find that our subjects decrease their production in response to price risk at the intensive margin. This finding is consistent with Batra and Ullah (1974), who posited that with preferences exhibiting decreasing absolute risk aversion (DARA), increases in risk at the intensive margin would lead to decreases in output. This finding, however, holds only in the US; in Peru, we find that neither changes at the extensive or intensive margin cause our subjects to change their behavior.

When looking at the effects of price risk on production by using dummy variables to capture each of our four levels of price risk, we find an intriguing pattern of behavior—one that appears inconsistent with the behavior of expected utility maximizers. According to that pattern, the behaviors we have just described at (i) the extensive margin of price risk (i.e., testing Sandmo's prediction), and then (ii) at the intensive margins (i.e., testing Batra and Ullah's prediction) are driven by the least risky of our risky price distributions. In other words, our subjects' behavior is nonmonotonic: Though they increase their production relative to situations of price certainty in response to low levels of price risk, at higher levels of price risk, their production falls back to a level that is statistically indistinguishable from their production in situations of price certainty.

The remainder of this paper is organized as follows. In section 2, we lay out our core theoretical hypotheses and describe our experimental design, paying particular attention to how we ensured that we ran the same experiment in the lab as well as in the field. Section 3 presents our data and discusses descriptive statistics. In section 4 we present and discuss the empirical framework we rely on to test the predictions of the theory. Section

5 presents our results. We summarize and conclude with directions for future research in section 6.

2 Experimental Design

We design an experimental protocol to study producer behavior in the face of price risk following the theoretical framework of Sandmo (1971).

Sandmo's theoretical framework is as follows. Assume that a firm manager's preferences over profit π are represented by a utility function $u(\pi)$, where $u'(\pi) > 0$ and $u''(\pi) < 0$. Profit is such that $\pi = px - c(x) - F$, where output price $p > 0$ is risky, with $E(p) = \mu$ and $Var(p) = \sigma_p^2 > 0$, and where $x > 0$ is the firm manager's output choice, $c(x)$ is an increasing and convex function representing the firm's variable costs, i.e., $c'(x) > 0$ and $c''(x) > 0$, and F is a constant representing the firm's fixed costs.

The firm manager's objective is to maximize the utility she derives from her firm's profit by choosing how much to produce *ex ante* of the realization of the risky output price p. That is, the firm manager chooses her quantity produced x so as to

$$\max_x E\left\{u(px - c(x) - F)\right\}, \tag{1}$$

Given the foregoing, the following results can be established.

Proposition 1 (Sandmo, 1971) *Under the assumptions made so far, the* *presence of output price risk causes an income risk-averse firm manager to underproduce relative to the case where output price is certain and equal to the mean of the price distribution.*

Proof. See Sandmo (1971). ■

Proposition 2 (Batra and Ullah, 1974) *Under the assumptions made so far, an increase in output price risk causes an income risk-averse firm manager to decrease her output choice if her preferences exhibit decreasing absolute risk aversion.*

Proof. See Batra and Ullah (1974). ■

In the remainder of this section, we first describe the design of the experimental game we use to elicit behavior in the face of price risk. Then, because Sandmo's prediction only holds for risk-averse individuals, we also briefly discuss the design of the experimental game we use to elicit risk preferences, which was developed by Holt and Laury (2002), and which allows conditioning our core estimation results on the sub-sample of risk-averse subjects. Finally, we describe the differences among the three contexts wherein we conducted our experiments, where applicable.[5]

2.1 Price Risk Game

In the output price risk game, each subject assumes the role of a producer (or of the manager of a firm) producing a single commodity. To focus on the effect of output price risk, we assume away all other forms of risk and uncertainty (e.g., production risk). We also abstract from strategic interactions in that a subject's behavior has no effect on another subject's payoff. Lastly, we abstract from general equilibrium effects in that every subject is a price taker, i.e., her production decisions have no effect on the price drawn.

To mimic the theoretical framework in Sandmo (1971), we choose simple cost and profit functions. We assume that the fixed cost F is such that $F = 15$ and the variable cost function $c(x)$ is such that $c(x) = 2x^{1.4}$, which is an increasing and convex function of output. We picked this specific functional form in accordance with both the theoretical framework in section 2 and the payoff structure we use, which we describe below. Accordingly, the profit function $\pi = px - 2x^{1.4} - 15$ is a concave function of output. Our experimental subjects' monetary reward from this price risk game is directly tied to their profits.

The level of output x that a subject can choose ranges from 0 to 20. It must be determined *ex ante* of the realization of output price. Once experimental subjects have made their production decisions, the price per unit is drawn and is one of five values in the set $\{5, 6, 7, 8, 9\}$. We made sure that subjects could not cheat by changing their production level in response to the price drawn by closely monitoring them in our US lab experiments and by having production decisions recorded by the enumerators before prices were drawn in our Peruvian framed experiments.

To facilitate decision making, subjects are given charts that describe the relationship between output level, price, cost, and profit, along with graphs of the profit function under each of the five different possible prices in the set $\{5, 6, 7, 8, 9\}$. A combined chart summarizing the relationship between output and profit under all five price scenarios is also provided to facilitate comparisons and, ultimately, decision-making. Exhibit B1 in Appendix B shows those charts for the US experiments, and exhibit C1 in Appendix C shows those charts for the Peruvian experiments. The charts in exhibits B1 and C1 only differ in the language in which they are written; their payoff structure is identical.

To determine the price of the output in each round, we follow a two-stage randomization strategy, as follows:

1. In the first stage, we determine whether there is any price risk (i.e., the extensive margin of the price risk). In expectation, in one third of the cases, subjects are presented with a certain price of \$7 per unit, which is the mean of the five possible prices. We refer to this as experimental setting 1 in the US and scenario 0 in Peru, wherein the standard deviation σ of the price distribution is such that $\sigma = 0$. Appendix Figure A1a shows the chart shown to our subjects in cases of price certainty.

2. The foregoing means that in expectation subjects are presented with a risky price in two thirds of the cases.[6] Conditional on facing a risky price in stage 1, in stage 2, the level of price risk is determined by randomly selecting one of four price distributions which are all mean-

preserving spreads of one another. We refer to these as experimental settings 2 to 5, wherein the standard deviation σ of the price distribution respectively takes on the values $\sigma = 0.795$, $\sigma = 1.17$, $\sigma = 1.451$, and $\sigma = 1.58$. Because we are taking mean-preserving spreads, the mean of the price distribution is always equal to 7, so in terms of coefficients of variation, our subjects face price risk ranging from $\frac{\sigma}{\mu} = 0$ in setting 1 to $\frac{\sigma}{\mu} = 0.23$ in setting 5. Appendix Figures A1b to A1e show the charts shown to our subjects in cases of price risk. Exhibit C6 shows the Spanish version of those charts. The difference between the US and Peruvian contexts is that setting 1 in the US becomes scenario 0 in Peru, setting 2 in the US becomes scenario 1 in Peru, and so on until setting 5 in the US becomes scenario 4 in Peru.[7]

Randomization is done publicly. In the US, where sessions involved approximately 24 participants, we showed subjects the Excel spreadsheet used to randomize the presence of price risk (i.e., the extensive margin of price risk). Conditional on there being price risk, the price was drawn from a bag filled with balls marked with prices (i.e., the intensive margin of price risk). In Peru, where we conducted experiments individually (a difference between the US and Peruvian contexts we discuss below), randomization is done subject-by-subject by the successive throw of a six-sided die to determine whether the price is certain or risky (to reflect the same one-third–two-thirds odds as in the US, rolling a 1 or a 2 means facing a certain price, and rolling a 3, 4, 5, or 6 means facing a risky price) and, in cases of a risky price, that of a four-sided die to determine which risky distribution the price is drawn from, as shown in exhibit C5. Subjects are told explicitly that there are no strategic interactions or general equilibrium effects.[8]

In each round, once we determined the setting from the two-stage randomization process, subjects are shown the shape of the price distribution of the corresponding setting or scenario. Again, those are shown in exhibit C6.

Subjects choose how much to produce in each round *ex ante* of the resolution of price risk by looking at the picture of the randomly drawn price distribution and the profit charts they are given. Once all subjects have recorded their output choice, we draw a ball from the bag with the corresponding setting to determine the *ex post* market price and to give each subject a chance to figure out how she has done in terms of profit before moving on to the next round.

Subjects were told they could—and were encouraged to—inspect those bags once the experiment was concluded in order to see that the actual price distributions we drew from reflected what was on the charts we showed them. Ideally, the subjects would have been able to inspect the bags prior to the experiment. The fact that no subject anywhere took us up on the offer to inspect the bags leads us to believe that our subjects trusted us when we told them there was no deception.

We were especially careful to ensure that our subjects could not change their answers. In the US, in each round, we had them fill out a note card indicating their output choice for that round, which we then collected before prices were drawn and profits realized to prevent our subjects from cheating. In Peru, a subject's output choice in a given round was recorded by the enumerator in charge of recording that subject's answers before the price was drawn, and it was not possible to change one's output choice *ex post* of the resolution of price risk.

In each context, subjects played 10 practice rounds and 20 actual rounds of this price game. During the practice rounds, subjects were explicitly encouraged to ask questions to ensure that they properly understood the structure of the game. Profits from the actual rounds were mapped into a monetary reward function. To make our setup incentive compatible, at the end of each experimental session, we randomly chose one of the 20 actual rounds for each subject using a 20-sided die thrown by the subject herself in order to determine which round we would base that subject's experimental payout on.[9] In the US, the experimental payoff from the production game was determined by adding a $25-base payoff plus a half of the subject's profit or loss in the randomly selected price game round, her proceeds from the Holt-Laury list experiment, which we describe below, and a $45 payment for showing up to the experiment. The average payoff for our US subjects was thus on the order of $75, which we felt was high enough to make our subjects to take the experiment seriously.

In Peru, subjects received a payment of 45 Peruvian soles (PEN) for showing up to the experiment,[10] a 25 PEN base payoff for the price risk game plus half of a subject's profit or loss in the randomly selected price game round, and her proceeds from the Holt-Laury list experiment (ranging from PEN 0.30 to 11.55), as presented in exhibit C3. The average payoff for our Peruvian subjects was about $20, which represents two to three times the daily wage of a farmer or low-skilled worker in Peru, i.e., a substantial amount of money for our Peruvian subjects.

Because our subjects do not know *ex ante* which of the 20 actual rounds will be chosen for their payoff, they are induced to truthfully reveal their preference in every round, and this allowed us to economize on the transactions costs involved with compiling individual earnings over 20 rounds, given the pencil-and-paper nature of the experiment.

One concern with our results might be that our subjects might feel like they are playing with "house money," and thus be more willing to gamble that money and take risks with it. To mitigate the possibility that subjects feel like they are playing with house money in economic experiments, subjects are usually given money to perform a simple task in exchange for the money they can expect to go home with at the end of the experiment. We wanted subjects to incorporate the expected payout from the experiment in what they thought of as their own money. Thus, we told them that they were getting a fixed payment for showing up and that they were receiving an endowment to play with which was theirs. We also told them they could win more money or lose some of their endowment, depending on both their decisions and the prices drawn.

2.2 Holt-Laury List Experiment

Recall that Sandmo's prediction is that risk-averse producers respond to price risk by decreasing how much they produce, which means that we need a means of determining which of our subjects are risk-averse. Along with the price risk game, we thus had our subjects play the list game developed by Holt and Laury (2002), which we use here to elicit our subjects' risk preferences.[11] We do this to make sure that we can control for risk preferences and to explore different behaviors for different degrees of risk aversion. The list of choices used in the Holt-Laury game is shown in exhibit B4 in the appendix, under Set II, for the US context. For the Peruvian context, what was shown to our subjects is in exhibit C4, and the sheet where their answers were recorded is shown in exhibit C3, under the sixth heading.[12]

In the Holt-Laury game, subjects are shown a list of ten rows. Each row contains two options, A and B, which are both different specifications of a lottery. Option A is always less risky than option B. The expected value of the payoff starts off higher for option A than for option B in the top row, but the difference between the two decreases as the row number increases.

Subjects choose which option to take starting from the top row. The game is designed so that most subjects will eventually switch from A to B. Switching to B at later rows means that a subject is more risk-averse. Once a subject switches to B, the game ends. In other words, in order to make sure that our setup satisfies the axioms of EU theory, we enforce monotonic switching, as is often done in the literature (see, for instance, Magnan et al., 2018).[13]

To determine the payoff in the Holt-Laury lottery game, each subject rolled a 10-sided die twice: first to randomly select the row number on which we based that subject's payout, and then to actually play the lottery in that row to determine her payoff for this part of the experiment. The monetary payoff in this case was identical to the dollar amount shown in the table.

2.3 Differences across Contexts

The preceding section already highlights some of the differences between the US and Peruvian contexts. This section describes the remainder of those differences.

Recall that we conducted our experiments in three contexts. In the first two contexts, we conducted our experiments in lab settings at Cornell University and at the University of Minnesota. In the third context, we conducted our experiments in lab-in-the-field settings with farmers in rural areas of Peru's Lima province.

The Cornell lab experiments were conducted in December 2014 and March 2015. The Minnesota lab experiments were conducted in October 2015. The Peruvian lab-in-the-field experiments were conducted in August and September 2016. We conducted the lab experiments at Cornell and Minnesota ourselves; the lab-in-the-field experiments in Peru were contracted out to Innovations for Poverty Action (IPA), a New Haven, CT-based organization with offices throughout the developing world whose expertise lies in implementing field experiments and surveys, usually for economists. Prior to IPA conducting the lab-in-the-field experiments in Peru, we spent three full days training the enumerators on the right implementation of our experimental design.

At Cornell and Minnesota, experimental subjects were undergraduate students. At Cornell, subjects were recruited from the general undergraduate population via the lab listserv. At Minnesota, subjects were recruited via listserv from the population of undergraduates enrolled in one of two majors offered by the Department of Applied Economics. In Peru, subjects were recruited by IPA staff from among a population of farmers in rural Lima province who were numerate and literate enough to take part in the experimental games discussed above.

An important difference between the lab experiments conducted at Cornell and Minnesota and the lab-in-the-field experiments conducted in Peru lies in the variation off of which we identify parameters of interest. At both Cornell and Minnesota, where the per-subject costs of conducting experiments were relatively high, prices vary within each subject across rounds, but not across subjects within a round, i.e., we draw a single price for all subjects in each round, because subjects participated group sessions. In Peru, where the per-subject cost of conducting the experiments was much lower, we had the luxury of being able to conduct the experiments one-on-one with each subject, and so each subject faces her own price in each round, i.e., prices vary across subjects within each round and within subject across all rounds. Doing so also has the added benefit of giving us more statistical power to detect the effect of price risk on production in Peru given the added variation that comes from having distinct prices for each subject in each round relative to the US cases.

Another important difference between contexts is the order in which the price risk and the Holt-Laury games were played. At Cornell in December 2014, the price risk game was played first and the Holt-Laury game was played second. At Cornell in March 2015 and at Minnesota in October 2015, the order of games was inverted, with the Holt-Laury game played first and the price risk game played second. In Peru, the order of games was randomly determined for each respondent by the throw of a die. Across contexts, this provides us with enough variation to determine whether the order in which the games are played matters. Within contexts, this is only possible at Cornell and in Peru.

The notional commodities subjects are told to imagine they are producing differ as well. In the US, we talk of bushels of wheat. Because wheat is uncommon in Peru, and because we wanted to run a framed field experiment, we asked our Peruvian subjects to imagine they are producing potatoes. This also has the added advantage of making the prices we drew overlap with the price of potatoes Peruvian farmers actually receive for similar quantities of potatoes.[14]

As regards deception, Cornell's Lab for Experimental Economics and Decision Research (LEEDR), where we conducted our experiments, does not allow deception. Since the Minnesota subjects were recruited from a departmental listserv, and not as part of a lab's pool of subjects, it is impossible to know whether they had been exposed to lab experiments with deception before participating in our experiment, but it seems unlikely. As for the Peruvian subjects, they had never been part of a lab-in-the-field experiment before our experiment. Therefore, we are confident that the vast majority of our subjects had never been exposed to deception in the context of an experiment before. Finally, although we conducted two group sessions at Cornell, subjects were not allowed to participate in our experiment twice, and so there was no overlap between the two Cornell groups.

3 Data and Descriptive Statistics

Our sample consists of 24 subjects at Cornell in December 2014, 24 subjects at Cornell in March 2015, 23 subjects at Minnesota in October 2015, and 48 subjects in Peru in August and September 2016, for a total of 119 subjects across all three contexts. Because each subject played 20 actual rounds (after playing 10 practice rounds) of the price game, our pooled sample size consists of 2,380 subject-round observations for the price risk game. In some cases, we lose a subject-round or all of a subject's rounds due to missing data, so our pooled estimation sample consists of $N = 2,339$ observations encompassing data on 119 subjects for 19 or 20 rounds each. The 41 missing observations were due to two subjects (i.e., 40 subject-round observations) whose Holt-Laury switch point was mistakenly entered as zero—an impossible value—

when the data were entered and one subject-round observation for which the production level was missing.

Table 1 presents summary statistics for our pooled sample of experimental subjects (column 1) as well as for our Cornell 1 (i.e., December 2014; column 2), Cornell 2 (i.e., March 2015; column 3), Minnesota (column 4), and Peru (column 5) sub-samples.

In the interest of brevity, we do not describe those data except to discuss our subjects' risk preferences. Following Holt and Laury's recommendations (2002), we assume constant relative risk aversion (CRRA), and thus DARA, by ascribing values of the Arrow-Pratt coefficient of relative risk aversion, denoted as R, to each subject. We assigned $R = -0.95$ to subjects who switch in the first line of the Holt-Laury list experiment; $R = -0.49$ to subjects who switch in the second line; $R = -0.15$ to subjects who switch in the third; $R = 0.15$ to subjects who switch in the fourth line; $R = 0.41$ to subjects who switch in the fifth line; $R = 0.68$ to subjects who switch in the sixth line; $R = 0.97$ to subjects who switch in the seventh line; $R = 1.37$ to subjects who switch in the eighth line; and $R = 1.50$ to subjects who switch in the ninth or tenth line.

On the basis of that functional form assumption, the average US subject is risk-averse (i.e., has an Arrow-Pratt coefficient of relative risk aversion that is positive and statistically significant at less than the 5 percent level) but that the average Peruvian subject is risk-neutral (i.e., has an Arrow-Pratt coefficient of relative risk-aversion that is not significantly different from zero at any of the conventional levels). This finding might seem striking, but it is in line with the findings of Vieider et al. (2018), who report that levels of risk tolerance in their rural Ethiopian sample is substantially higher than the same levels for students in Western countries.

Figure 1 shows a histogram of the output choices made by our Peruvian subjects in certainty rounds. All US subjects always choose to produce 10 units–the profit-maximizing choice–when the price is certain, and so we do not show histograms for cases of price certainty at Cornell or Minnesota. It is interesting, however, to see in Figure 1 that some Peruvian subjects still make mistakes in cases where the optimal choice is *a priori* unambiguous.[15] Figure 2 show histograms of the output choices made by our subjects in price risk rounds by location, with both Cornell in Figure 2a and Minnesota and Peru in Figure 2b. Figure 2 shows that even when we consider only the rounds in which the price is risky, there is still a tendency to choose to produce $y = 10$.

4 Empirical Framework

To test Sandmo's prediction, we first estimate the following equation

$$y_{it} = \alpha_0 + \beta_0 I(\sigma_{it} > 0) + \delta_0 R_i + \tau_0 t + \nu_{0i} + \epsilon_{0it}, \quad (2)$$

where y_{it} denotes the subject i's output choice in round $t \in \{1, ..., 20\}$, $I(\sigma_{it} > 0)$ is an indicator variable equal to one if subject i has to make her output choice in the face of price risk and equal to zero otherwise,[16] R_i denotes subject i's Arrow-Pratt coefficient of relative risk aversion obtained from the Holt-Laury lottery game, ν_{0i} is a fixed or random effect specific to subject i, and ϵ_{0it} is an error term with mean zero.

To test Sandmo's hypothesis that output price risk at the extensive margin causes producers to decrease their level of output, we test the null hypothesis $H_0 : \beta_0 = 0$. Rejecting the null in favor of the alternative hypothesis that $\beta_0 < 0$ would constitute evidence in favor of Sandmo's hypothesis. Strictly speaking, however, this would only really need to hold in cases where our subjects are risk-averse, i.e., for subjects for whom $R > 0$. We thus also estimate equation 2 for the restricted sample of subjects classified as risk-averse on the basis of the Holt-Laury experiment.

In addition to equation 2, we estimate two additional specifications of our core equation. The first is such that

$$y_{it} = \alpha_1 + \beta_1 I(\sigma_{it} > 0) + \gamma_1 \sigma_{it} + \delta_1 R_i + \tau_1 t + \nu_{1i} + \epsilon_{1it}, \tag{3}$$

where σ_{it} denotes the standard deviation of the price distribution used in round t and all other variables are defined as before. Equation 3 allows studying the effects of price risk at the extensive and intensive margins, or the effect of going from a situation of price certainty to a situation of price risk, and then effect of increases in price risk conditional on there being some price risk, respectively. This latter phenomenon was explored theoretically by Batra and Ullah (1974), who showed that producers decrease the quantity they produce in response to further mean-preserving spreads of the output price distribution if their preferences exhibit DARA.[17]

The last specification is

$$y_{it} = \alpha_2 + \sum_{j=2}^{5} \gamma_{2j}\sigma_{ijt} + \delta_2 R_i + \tau_2 t + \nu_{2i} + \epsilon_{2it}, \tag{4}$$

where σ_{ijt} is a dummy variable equal to one if setting $j \in \{2, ..., 5\}$ was randomly drawn for subject i in round t and equal to zero otherwise. Because it allows for the possibility that price risk affects behavior in a nonlinear fashion, equation 4 allows studying the effect of various levels of price risk separately from the others.

We present ordinary least squares (OLS) regressions throughout,[18] and we cluster standard errors everywhere at the subject level due to the longitudinal nature of our data (Abadie et al., 2017).

We estimate random effects versions of equations 2 to 4. We do so for two reasons: First, because random effects are in principle superior to fixed effects when dealing with experimental data given that the variables of interest are clearly orthogonal to the error term, and because the fixed effects estimator is inefficient. Second, we do so based on the results of Hausman tests which fail to reject the null that the coefficients from random and fixed effects specifications are statistically equal. That said, in preliminary analyses, we estimated fixed effects specifications, and our core results are robust to estimating fixed instead of random effects specifications.

5 Experimental Results

This section proceeds in two parts. In the first part, we discuss our core results (i.e., the estimation results for equations 2 to 4), which aim first and foremost at testing Sandmo's (1971) prediction that price risk at the extensive margin causes producers to decrease how much they produce. In the second part, we discuss the significance of our findings.

5.1 Core Results

Tables 2a and 2b presents estimation results for equation 2 in the pooled sample for all subjects (column 1) and across settings for all subjects (columns 2 to 5), both with (Table 2a) and without control variables (Table 2b). Then, because Sandmo's prediction applies only to risk-averse subjects, we condition our analysis on subjects classified as risk-averse on the basis of their Holt-Laury switch point in the pooled sample (column 6) and in the Peruvian context (column 7). The results in Tables 2a and 2b constitute our main test of Sandmo's prediction that the presence of price risk causes (risk-averse) subjects to decrease how much they produce. In Tables 2a and 2b, we find that the presence of price risk causes no detectable change in how much our subjects produce. As such, these results offer little to no support for Sandmo's prediction.

In Tables 3a and 3b, we differentiate between price risk at the extensive and intensive margins by presenting estimation results for equation 3, again in the pooled sample for all subjects (column 1), across settings for all subjects (columns 2 to 5), and for risk-averse subjects in the pooled sample (column 6) and in the Peruvian context (column 7), both with (Table 3a) and without control variables (Table 3b). Tables 3a and 3b show a striking pattern for US subjects: price risk at the extensive margin causes US subjects to produce more—not less—than situations of price certainty, but price risk at the intensive margin causes them to produce less.[19] This latter finding is consistent with Batra and Ullah's (1974) theoretical prediction that with preferences exhibiting DARA, producers respond to price risk at the intensive margin by decreasing the quantity they produce. Peruvian subjects, however, do not significantly change their behavior in the face of changes in price risk at either the extensive or the intensive margin.[20]

Tables 4a and 4b let the data speak for themselves by including a dummy variable for each price-risk setting (i.e., settings 2 to 5), once again in the pooled sample for all subjects (column 1), across settings for all subjects (columns 2 to 5), and for risk-averse subjects in the pooled sample (column 6) and in the Peruvian context (column 7), both with (Table 4a) and without control variables (Table 4b). Because we let the data speak for themselves, unlike the specifications in Tables 2a, 2b, 3a, and 3b, the specifications in

Tables 4a and 4b do not impose any restrictions on the relationship between output and price risk. Consequently, Tables 4a and 4b shows results that are more nuanced than what the results in Tables 2a, 2b, 3a, and 3b would suggest. In the pooled sample (i.e., columns 1 and 6), the patterns in Tables 2 and 3 appear entirely driven by the fact that setting 2 causes subjects to produce significantly more than in either the price certainty case (i.e., setting 1) or other price-risk settings (i.e., settings 3 to 5). When looking at results by location, it looks as though this is driven by US subjects in the full sample, but by all subjects in the sample of risk-averse subjects. Moreover, in the full sample, Peruvian respondents only change their behavior in response to setting 5, increasing their output level in response to the riskiest setting.[21]

So much for the statistical significance of our findings. In terms of economic significance, our results paint the following picture. In situations of price certainty, subjects tend to stick close to the profit-maximizing amount of 10 units of output. Though Sandmo's result states that (risk-averse) subjects should decrease their production in response to price risk, our average subject tends to increase her production by 0.2 units of output in response to price risk.

When disentangling the extensive and intensive margins of price risk, we find that in the full sample, subjects increase their production by almost exactly one unit in response to price risk at the extensive margin, but they decrease their production by 0.5 units of production in response to an increase in price risk at the intensive margin of one standard deviation. In the sample of risk-averse respondents, these effects are doubled. This suggests that there is a significant discontinuity in preferences at the boundary of certainty and risk.

Finally, when we let the data speak for themselves, we find that our subjects generally increase their production in response to various price risk treatments. Relative to a baseline of about 10 units of production in price certainty cases, as we take mean-preserving spreads of the price distribution our average subject increases her production by 0.6 units in response to setting 2 (the least risky of our price risk settings), by 0.4 units in response to setting 3 (a slightly riskier setting), but she then leaves her production unchanged in response to a uniform price distribution (setting 4), and she increases her production by 0.3 units in response to the riskiest of our price risk settings (setting 5). In other words, our average subject's behavior in the face of price risk not only appears not to be continuous per the results in Tables 3a and 3b, it also appears nonmonotonic per the results in Tables 4a and 4b.

5.2 Discussion

Our findings reveal interesting patterns of behavior. First, when testing Sandmo's (1971) prediction that risk-averse subjects respond to the presence of price risk by decreasing how much they choose to produce, we find that subjects do not change their production in response to price risk—a finding that also holds true when looking at all subjects. This is consistent with expected utility theory only if our average subject is risk-neutral. Our elicitation of risk preferences, however, suggests that our average subject is risk-averse rather than risk-neutral.

Second, when it comes to Batra and Ullah's (1974) theoretical prediction, we find that our subjects decrease how much they produce in response to price risk at the intensive margin. This is consistent with our subjects having preferences that exhibit DARA.

Lastly, when we let the data speak for themselves and control for each of the four possible levels of price risk in our research design with a dummy variable controlling for that level, we find that the above findings as they relate to Sandmo (1971) and Batra and Ullah (1974) are explained as follows. There are four possible levels of price risk: $\sigma = 0.795$, $\sigma = 1.17$, $\sigma = 1.451$, and $\sigma = 1.58$. Relative to situations of price certainty, the average subject in our data only changes her behavior in response to $\sigma = 0.795$. Once risk exceeds that level, however, behavior reverts back to what it is in situations of price certainty. This is true both when looking at the full sample and when looking only at the sub-sample of risk-averse subjects.

Our findings when we let the data speak for themselves by controlling for each level of price risk with a dummy variable for that level of risk suggest two things. First, they suggest that the risk preferences of our subjects are not monotonic. This is especially true given that the point estimates for those dummies, though they are not statistically significant, do not suggest a monotonic response to price risk. This means that any analysis or policy instrument that starts from the assumption that producers respond monotonically to risk would be on shaky ground in these data.

Second, and more tentatively, our findings suggest that there might be a discontinuity in our subjects' preferences immediately to the right of $\sigma = 0$. In other words, our findings do not rule out the possibility that there is a discontinuity in production when going from $\sigma = 0$ to $\sigma > 0$. This further suggests that any analysis or policy instrument that starts from the assumption that producer preferences over price risk are continuous would likewise be on shaky ground.

Ultimately, this leaves a few possibilities. First, it is possible that our subjects approached our experiments as mere gambling. Against this potential criticism, we note that our experimental protocol was incentive compatible, and that we paid our subjects a significant amount of money, viz. $75 on average to US students, and two to three days worth of unskilled labor wages to Peruvian farmers, both for a few hours of each subject's time. Second, it is possible that the Holt and Laury (2002) experiments we used to elicit risk preferences simply fail to accurately elicit risk preferences, and that instead of being risk-averse, our average subject is actually risk-neutral. Against this criticism, we note that if our average subject were actually risk-neutral, we would observe no change in her behavior in Tables 3 and 4. Finally, it is possible that expected utility theory is simply not the right way to model how our subjects behave. Looking at possible alternative explanations for our subjects' behavior, we find evidence that that behavior is consistent with prospect theory in Appendix D.

More broadly, our results might seem a priori inconsistent with the existence of risk management instruments for producers. This is especially true for agricultural markets, which most closely resemble the stylized markets we study in our experiments. For instance, agricultural producers in the United States and other rich countries are usually insured against price risk, and the development and testing of insurance schemes in developing countries has become an active area of research in development economics over the last ten years.

But as Coble and Knight (2002) note, for a long time, US agricultural producers refused to buy insurance (even when that insurance was offered at better-than-actuarially-fair rates). In fact, it was not until they were forced to purchase insurance as a condition for receiving other payments that they started doing so (Glauber, 2004). Second, as we note in the introduction, a recent review of the literature on insurance in developing countries by Platteau, de Bock, and Gelade (2017) documents the widespread lack of uptake of micro-insurance, even when faced with financial incentives aimed at improving uptake. The fact that we find in Table 4 that producers respond to price risk by increasing how much they produce is consistent with the findings in Coble and Knight (2002), Glauber (2004), and Platteau, de Bock, and Gelade (2017).

6 Summary and Concluding Remarks

We have conducted the first experimental test of the theory of producer behavior in the face of output price risk. To do so, we have developed an experimental protocol that mimics the theoretical framework in Sandmo (1971) to test his prediction that risk-averse producers respond to the presence of price risk by decreasing how much they choose to produce.

Our core results offer little to no support for Sandmo's prediction. Because that prediction is rooted in EU theory, we then explore the ramifications thereof by looking at what happens to production at both the extensive and intensive margins of price risk, in line with Batra and Ullah's (1974) theoretical exploration of the same. In that case, we found that the average subject in our data increases her output level in response to price risk at the extensive margin, but decreases it in response to price risk at the intensive margin. When we let the data speak for themselves by regressing output choice on dummies representing the various price risk settings, it looks as though there is no systematic pattern except for the fact that our results contrasting the extensive and intensive margins of price risk appear to arise from how our subjects react to low levels of price risk.

Because we conducted the same experiments in the lab with US college students and in the field with Peruvian farmers, our results have more external validity than the usual experiment, which tends to focus on a relatively homogeneous group of subjects (often students at a single university) in a single country. If one were to take our results at face value as having full external validity—admittedly, a big "if"—one would need to rethink a number of policies aimed at stabilizing prices. Indeed, a number of such programs are based on the belief that producers respond to the presence of price risk by decreasing how much they produce. But a recent review by Platteau, de Bock, and Gelade (2017) documents the low uptake of insurance schemes in developing countries, and in the US, where insurance markets tend to be more complete than in developing countries, the demand for agricultural insurance tends to be low. As Glauber (2004) writes: "Participation in the [US crop insurance] program grew slowly in the 1980s, reaching only 55.6 million acres in 1988, about 25% of eligible acreage. Participation reached 40% in 1989 and 1990, largely because of disaster legislation that required recipients of disaster payment in 1988 and 1989 to buy crop insurance in the subsequent crop year. By 1993, participation had fallen to 32% of eligible area." Our work suggests that the theory of producer behavior in the face of output price risk—as well as the policies used to protect producers from price risk—might need to be rethought along the lines of more complex behavioral rules. Likewise, our experiments only considered producers facing output price risk in situations without storage or insurance. This suggests that running similar experiments aimed at studying producer behavior in the face of price risk with either access to storage, insurance, or both might be a fruitful avenue for future research in this area.

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Notes

[1]Price risk is generally distinct from income risk. This is best seen in the case of a consumer, whose indirect utility function $V(p, w)$ is defined over both income w and prices p. Though the problem we study analytically can be viewed equivalently as income risk, we will talk of "price risk" throughout this article, since output price is the source of risk in our experimental setup.

[2]There exists an entire theoretical literature on the welfare effects of price risk (or the lack thereof via price stabilization schemes) for consumers as well as for producers. On the consumer side, notable contributions include Waugh (1944, 1966), Samuelson (1972), and Turnovsky, Shalit, and Schmitz (1980). On the producer side, in addition to Sandmo (1971), notable contributions include Oi (1961), Baron (1970) and, within agricultural economics, Quiggin and Anderson (1979), Schmitz, Shalit, and Turnovsky (1980), and Chambers and Quiggin (2003), who look at the behavior of the risk-averse firm and generalize prior results. Finkelshtain and Chalfant (1991) unite the production and consumption sides by considering agricultural households that face price risk for a commodity that they both consume and produce. We focus on testing Sandmo (1971) because for better or for worse, his contribution is the most well-known of all theoretical articles on producer behavior in the face of price risk.

[3]Replicating the findings in Bellemare, Barrett, and Just (2013), McBride (2016) notes that the welfare loss can be instead decreasing in income if one makes a different imputation assumption regarding missing income values.

[4]Because price risk is analytically equivalent to income risk arising from production risk in this context, a reviewer asked whether experiments similar to our had been conducted to look at production risk. We know of no lab-experimental study focusing on the effects of production risk on output choice. There are certainly randomized controlled trials looking at the impacts of index insurance on various behaviors on the part of producers (e.g., crop or technique choice, or investment decisions, with the best examples being Cole et al. 2013

and Karlan et al. 2014). The closest study to ours would be by Brick and Visser (2015), who use lab experiments to look at the effect of index insurance on the choice of activities, but not on quantity produced.

[5]The field component of our study was the subject of a pre-analysis plan filed with the American Economics Association's trial registry at `https://www.socialscienceregistry.org/trials/1497`. Part of what we label our core analysis—specifically, equation 3—is in the pre-analysis plan. The specifications in equations 2 and 4 and our exploration of alternatives to EU theory were not in the pre-analysis plan.

[6]We settled upon a one third—two thirds breakdown of situations of price certainty versus price risk to guarantee that we would have enough variation to study the effect of price risk at the extensive margin.

[7]Exhibit C5 shows the Spanish version of our randomization strategy, which was shown to our Peruvian subjects to make it easier to understand the structure of the game.

[8]A possible objection to our design might be that the mean of the price distributions we use should also have been randomized instead of being held constant at 7. Though it would certainly be interesting to also randomize the mean of the price distribution, we chose to keep the mean price the same across all settings to study the effect of mean-preserving spreads, which is what Sandmo (1971) focused on. How producers behave when both the mean and the variance of the price distribution they face vary should be looked at in future research.

[9]Some readers might worry that paying subjects for just one round chosen at random might distort our subjects behavior. Charness, Gneezy, and Halladay (2016), however, note that in most experimental contexts, paying for just one round is just as effective (and in some cases more effective) than paying for all rounds. In our experiment, we believe that paying for all rounds would be counterproductive. Indeed, it is easy to imagine that a subject's cumulative earnings might induce her to behave a certain way if she knows she is going to get paid for all rounds. For instance, a subject who knows she is losing money overall might decide to expose herself to as much risk as possible toward the end of the

game in an effort to recoup her losses.

[10]As of writing, USD 1 $\approx$ PEN 3.29.

[11]Although Drichoutis and Lusk (2017) show that the Holt and Laury (2002) method is better suited to elicit the shape of the probability weighting function than it is to measure the curvature of the utility function, the findings in Drichoutis and Lusk were not yet published when we initially developed our experimental protocol in 2014.

[12]Some subjects played the price risk game first, and others played it second. We discuss the order of games in the next sub-section.

[13]Readers worried about our enforcing monotonic switching should note that our empirical results are invariant to our use of subject random or fixed effects, and that subject fixed effects hold constant the number of times a subject switches in the Holt-Laury game. In other words, enforcing monotonic switching in this context only affects the interpretation of the coefficient on a subject's risk preferences, and not our core results about the effect of price risk on production.

[14]One reviewer noted that, given the prominence of potatoes in the Peruvian diet, the behavior of our Peruvian subjects might differ depending on whether they are a net buyer or a net seller of potatoes. Though this could be a concern in principle, we note that our results are qualitatively robust to whether we rely on subject random effects or subject fixed effects. Since subject fixed effects control for whether subjects are net buyers or net sellers of potatoes in their day-to-day life, we conclude that our Peruvian respondents' position vis-à-vis the potato market does not impact their choices in the price risk game. Still, if buyers and sellers of potatoes adjust their production differently in response to price risk, then their buyer or seller status would interact with price risk, which the use of fixed effects does not account for. This is unfortunately something we cannot do anything about given data limitations.

[15]Omitting those subjects who made an "irrational" choice at any time in the price risk game from the estimation sample does not qualitatively change the results below.

[16]Strictly speaking, this only varies at the subject level in Peru. Recall that in the US,

we ran group experiments and drew a single price for all subjects in a given round. We maintain this (somewhat abusive) notation for the remainder of this article.

[17]A reviewer suggested that β_1 did not have a meaningful interpretation because it is never the case that the uncertainty dummy is equal to one and the standard deviation is equal to zero. While this is theoretically right, the inclusion of both the uncertainty dummy and the standard deviation of the price risk distribution allows studying what happens as the standard deviation approaches zero from the right, i.e., $\lim_{\sigma \to 0^+} y$, and compare that value with cases where $\sigma = 0$. This the proper way to test for Batra and Ullah's (1974) core proposition. This is also how we can test whether there is a discontinuity in preferences at zero. Getting rid of the uncertainty dummy to only regress on the standard deviation would essentially force continuity at the boundary between price certainty and price risk on our subjects behavior, which is something we do not want to do, especially in light of the evidence surrounding the existence of a certainty effect. See Just (2014, pp. 231-234) for a discussion of the certainty effect.

[18]Although the dependent variable in equations 2 to 4 is a nonnegative integer and thus lends itself to the estimation of count data models such as Poisson or negative binomial regressions, we present throughout the results of ordinary least squares (OLS) regressions for three reasons. First, with likelihood-based procedures like Poisson or negative binomial regressions, there is always a possibility that a coefficient might be identified off of the specific functional form imposed on the error term. Second, the coefficients from an OLS regression are directly interpretable as marginal effects. Finally, and most importantly, when we tried to estimate the right count-data model during preliminary work, a test of overdispersion showed that we should estimate a negative binomial model, but we could not get such a model to converge with any of the conventional algorithms (i.e., Newton-Raphson, Davidson-Fletcher-Powell, Berndt-Hall-Hall-Hausman, or Broyden-Fletcher-Goldfarb-Shanno). Since this left us only with the possibility of estimating the wrong count-data model (i.e., Poisson, which does not allow for overdispersion), we chose to go with OLS results instead.

[19]Because the results for price risk at the extensive margin only look at what happens to output as the standard distribution of the price distribution goes from zero to $\epsilon > 0$, where ϵ is a very small number, this is not a true test of Sandmo's prediction in this context.

[20]Because the results in Batra and Ullah (1974) are for those cases where there is already price risk, at the request of an anonymous reviewer, we show regression results for observations with price risk only (i.e., results omitting price certainty rounds) in Appendix Tables A2a and A2b. As one would expect, those results are highly consistent with those in Tables 3a and 3b.

[21]The difference in results between the US and Peru may stem from the fact that US subjects were told to imagine they were producing bushels of wheat, but Peruvian subjects were told to imagine they were producing kilograms of potatoes. In reality, potatoes are storable for a longer period than wheat is storable, something which our subjects might have incorporated in their decisions even though the experimental protocol made it clear that storage was not possible in the context of our experiment.

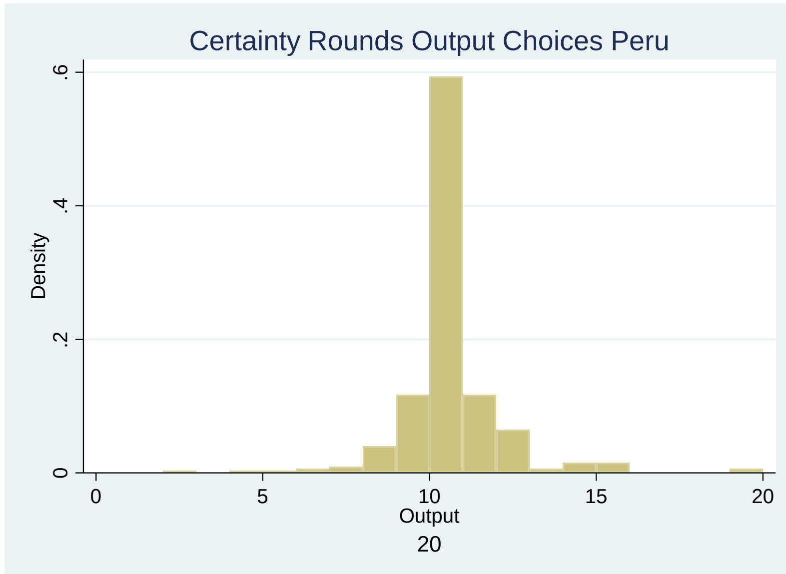

Figure 1. Frequency of Output Choices for Price Certainty Rounds in Peru.

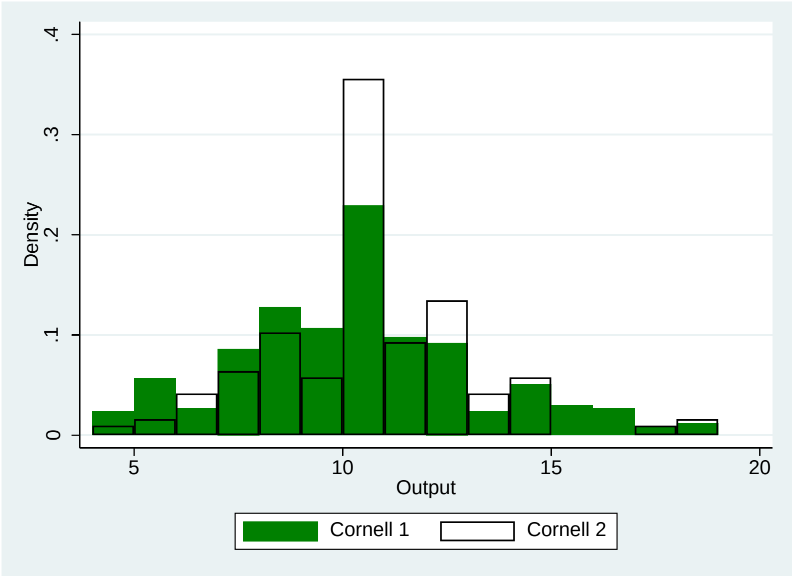

Figure 2a. Frequency of Output Choices for Price Uncertainty Rounds at Cornell.

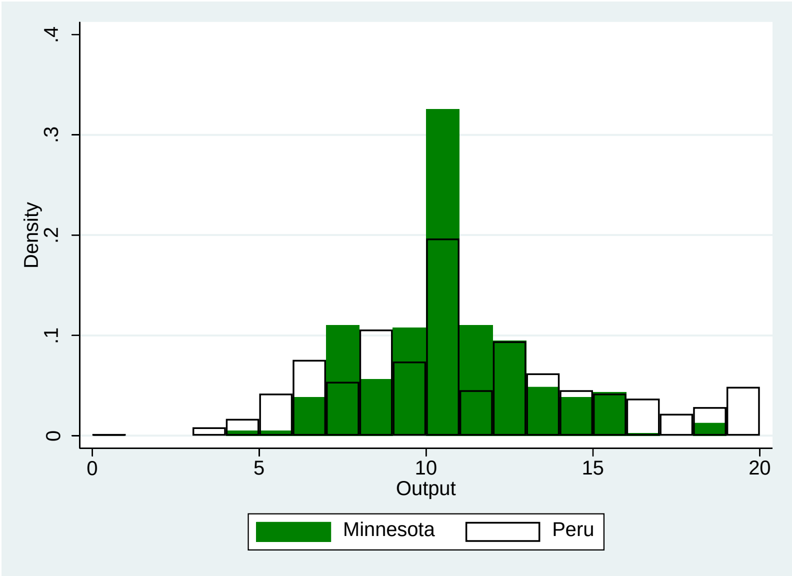

Figure 2b. Frequency of Output Choices for Price Uncertainty Rounds at Minnesota and in Peru.

Table 1. Descriptive Statistics.

Variables	Pooled	Cornell 1	Cornell 2	Minnesota	Peru
Output (Units)	10.247	9.915	10.110	10.159	10.535
	(0.056)	(0.112)	(0.091)	(0.105)	(0.109)
Price (Currency Units)	6.944	6.850	6.900	6.902	7.037
	(0.024)	(0.051)	(0.048)	(0.069)	(0.033)
Price Risk Dummy	0.698	0.700	0.650	0.850	0.646
	(0.009)	(0.021)	(0.022)	(0.017)	(0.016)
Standard Deviation	0.909	0.967	0.857	1.127	0.797
	(0.013)	(0.031)	(0.031)	(0.025)	(0.021)
$I(\sigma = 0.795)$, or Setting 2 Dummy	0.151	0.100	0.150	0.148	0.180
	(0.007)	(0.014)	(0.016)	(0.017)	(0.013)
$I(\sigma = 1.17)$, or Setting 3 Dummy	0.110	0.100	0.050	0.100	0.151
	(0.006)	(0.014)	(0.010)	(0.014)	(0.012)
$I(\sigma = 1.451)$, or Setting 4 Dummy	0.235	0.150	0.250	0.451	0.163
	(0.009)	(0.016)	(0.020)	(0.023)	(0.012)
$I(\sigma = 1.58)$, or Setting 5 Dummy	0.202	0.350	0.200	0.150	0.152
	(0.008)	(0.022)	(0.018)	(0.017)	(0.012)
Coefficient of Relative Risk-Aversion	0.596	0.889	1.076	1.050	-0.033
	(0.016)	(0.019)	(0.015)	(0.018)	(0.025)
Holt-Laury First Dummy†	0.555	-	-	-	0.391
	(0.010)				(0.016)
Observations	2,338	480	480	459	919

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
† The Holt-Laury game was played last at Cornell 1, but first at Cornell 2 and at Minnesota.

Table 2a. Random Effects Regression Results for Sandmo's Hypothesis with Control variables

	Full Sample					Risk-Averse Only	
Variables	**Pooled**	**Cornell 1**	**Cornell 2**	**Minnesota**	**Peru**	**Pooled**	**Peru**
			Dependent Variable: Quantity Produced				
Uncertainty	0.262	-0.122	0.440	0.361	0.473	0.242	0.747
	(0.199)	(0.336)	(0.309)	(0.340)	(0.387)	(0.220)	(0.674)
Relative Risk Aversion	-0.129	-1.065**	0.544	-0.628	0.047	-0.235	0.302
	(0.232)	(0.525)	(0.558)	(0.909)	(0.267)	(0.349)	(0.505)
Holt-Laury First	0.481*				0.770	0.675**	1.876***
	(0.289)				(0.518)	(0.307)	(0.728)
Round	-0.020**	0.012	-0.038**	0.022	-0.051***	-0.015	-0.080***
	(0.010)	(0.019)	(0.016)	(0.020)	(0.019)	(0.011)	(0.029)
Cornell	-0.455					-0.747**	
	(0.360)					(0.375)	
Minnesota	-0.580					-0.959*	
	(0.518)					(0.535)	
Constant	10.593***	10.692***	10.015***	10.076***	10.989***	10.804***	10.982***
	(0.315)	(0.672)	(0.671)	(1.359)	(0.482)	(0.492)	(0.807)
Observations	2,339	480	480	459	920	1,759	340

Standard errors clustered at the subject level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2b. Random Effects Regression Results for Sandmo's (1971) Hypothesis without Control Variables.

Variables	Pooled	Cornell 1	Pooled Cornell 2	Minnesota	Peru	Risk-Averse Pooled	Peru
			Dependent Variable: Quantity Produced				
Uncertainty	0.244	-0.122	0.170	0.195	0.499	0.213	0.763
	(0.198)	(0.336)	(0.290)	(0.370)	(0.393)	(0.215)	(0.716)
Holt-Laury First	0.276				0.766	0.386	1.936***
	(0.273)				(0.512)	(0.263)	(0.666)
Constant	9.926***	10.000	10.000***	10.000	9.917***	9.799***	9.545***
	(0.158)	(.)	(0.000)	(.)	(0.253)	(0.155)	(0.375)
Observations	2,339	480	480	459	920	1,759	340

Standard errors clustered at the subject level in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 3a. Random Effects Regression Results for Price Risk at the Extensive and Intensive Margins with Control Variables.

	Full Sample					Risk-Averse Only	
Variables	**Pooled**	**Cornell 1**	**Cornell 2**	**Minnesota**	**Peru**	**Pooled**	**Peru**
				Dependent Variable: Quantity Produced			
Uncertainty	0.993***	2.164***	1.928***	1.809***	-0.612	1.825***	0.937
	(0.350)	(0.670)	(0.539)	(0.551)	(0.636)	(0.335)	(1.057)
Standard Deviation	-0.564*	-1.655***	-1.242***	-1.184**	0.879	-1.199***	-0.154
	(0.289)	(0.536)	(0.473)	(0.498)	(0.561)	(0.283)	(1.053)
Relative Risk Aversion	-0.128	-1.065**	0.544	-0.633	0.043	-0.236	0.302
	(0.232)	(0.526)	(0.559)	(0.913)	(0.268)	(0.352)	(0.507)
Holt-Laury First	0.469				0.765	0.648**	1.883***
	(0.288)				(0.519)	(0.309)	(0.712)
Round	-0.020**	0.003	-0.017	0.006	-0.053***	-0.018	-0.081***
	(0.010)	(0.018)	(0.018)	(0.019)	(0.018)	(0.011)	(0.029)
Cornell	-0.411					-0.655*	
	(0.361)					(0.377)	
Minnesota	-0.537					-0.871	
	(0.518)					(0.538)	
Constant	10.575***	10.888***	9.686***	10.508***	11.029***	10.799***	10.990***
	(0.317)	(0.669)	(0.712)	(1.343)	(0.480)	(0.493)	(0.806)
Observations	2,339	480	480	459	920	1,759	340

Standard errors clustered at the subject level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3b. Random Effects Regression Results for Price Risk at the Extensive and Intensive Margins without Control Variables.

VARIABLES	Pooled	Cornell 1	Pooled Cornell 2	Minnesota	Peru	Risk-Averse Pooled	Peru
			Dependent Variable: Quantity Produced				
Uncertainty	1.005***	2.178***	1.941***	1.807***	-0.509	1.796***	0.752
	(0.347)	(0.655)	(0.536)	(0.549)	(0.609)	(0.338)	(1.014)
Standard Deviation	-0.587**	-1.665***	-1.344***	-1.215**	0.816	-1.200***	0.010
	(0.285)	(0.536)	(0.437)	(0.504)	(0.548)	(0.283)	(1.017)
Holt-Laury First	0.275				0.762	0.375	1.936***
	(0.273)				(0.513)	(0.262)	(0.654)
Constant	9.928***	10.000***	10.000***	10.000***	9.919***	9.809***	9.546***
	(0.158)	(0.000)	(0.000)	(0.000)	(0.253)	(0.155)	(0.368)
Observations	2,339	480	480	459	920	1,759	340

Standard errors clustered at the subject level in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 4a. Random Effects Regression Results for Nonlinear Effects of Price Risk with Control Variables

	Full Sample					Risk-Averse Only	
Variables	Pooled	Cornell 1	Cornell 2	Minnesota	Peru	Pooled	Peru
	Dependent Variable: Quantity Produced						
$I(\sigma = 0.795)$	0.566***	0.732**	1.273***	0.824***	0.120	0.944***	1.040**
	(0.194)	(0.332)	(0.338)	(0.313)	(0.359)	(0.192)	(0.506)
$I(\sigma = 1.17)$	0.387	0.074	-0.050	0.934**	0.417	0.346	0.332
	(0.266)	(0.383)	(0.294)	(0.426)	(0.475)	(0.260)	(0.681)
$I(\sigma = 1.451)$	-0.027	0.483	0.107	-0.448	0.461	-0.084	0.820
	(0.253)	(0.556)	(0.423)	(0.406)	(0.542)	(0.272)	(1.032)
$I(\sigma = 1.58)$	0.270	-0.681	0.624	0.743	0.957*	0.104	0.823
	(0.278)	(0.440)	(0.554)	(0.576)	(0.552)	(0.302)	(1.008)
Relative Risk Aversion	-0.126	-1.065**	0.544	-0.633	0.044	-0.238	0.299
	(0.231)	(0.527)	(0.560)	(0.915)	(0.268)	(0.352)	(0.498)
Holt-Laury First	0.497*				0.773	0.675**	1.878***
	(0.287)				(0.518)	(0.306)	(0.701)
Round	-0.023**	0.011	-0.049**	-0.006	-0.053***	-0.022*	-0.081***
	(0.011)	(0.019)	(0.024)	(0.021)	(0.018)	(0.012)	(0.029)
Cornell	-0.424					-0.678*	
	(0.360)					(0.377)	
Minnesota	-0.494					-0.843	
	(0.516)					(0.538)	
Constant	10.606***	10.717***	10.198***	10.834***	11.014***	10.882***	10.983***
	(0.321)	(0.677)	(0.772)	(1.347)	(0.480)	(0.498)	(0.814)
Observations	2,339	480	480	459	920	1,759	340

Standard errors clustered at the subject level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4b. Random Effects Regression Results for Nonlinear Effects of Price Risk without Control Variables

VARIABLES	Pooled	Cornell 1	Pooled Cornell 2	Minnesota	Peru	Risk-Averse Pooled	Risk-Averse Peru
			Dependent Variable: Quantity Produced				
$I(\sigma = 0.795)$	0.536***	0.771**	0.986***	0.842***	0.173	0.877***	0.991*
	(0.191)	(0.323)	(0.303)	(0.311)	(0.352)	(0.189)	(0.532)
$I(\sigma = 1.17)$	0.424	0.062	0.042	1.000**	0.456	0.382	0.322
	(0.270)	(0.390)	(0.293)	(0.392)	(0.491)	(0.264)	(0.741)
$I(\sigma = 1.451)$	-0.030	0.458	-0.217	-0.391	0.455	-0.090	0.863
	(0.250)	(0.555)	(0.358)	(0.415)	(0.549)	(0.269)	(1.063)
$I(\sigma = 1.58)$	0.216	-0.679	0.073	0.783	0.976*	0.033	0.918
	(0.273)	(0.438)	(0.395)	(0.604)	(0.556)	(0.291)	(1.020)
Holt-Laury First	0.317				0.770	0.414	1.929***
	(0.268)				(0.513)	(0.258)	(0.647)
Constant	9.909***	10.000***	10.000***	10.000	9.915***	9.790***	9.546***
	(0.156)	(0.000)	(0.000)	(.)	(0.252)	(0.155)	(0.367)
Observations	2,339	480	480	459	920	1,759	340

Standard errors clustered at the subject level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

AJAE Appendix:
Producer Attitudes toward Output Price Risk: Experimental Evidence from the Lab and from the Field

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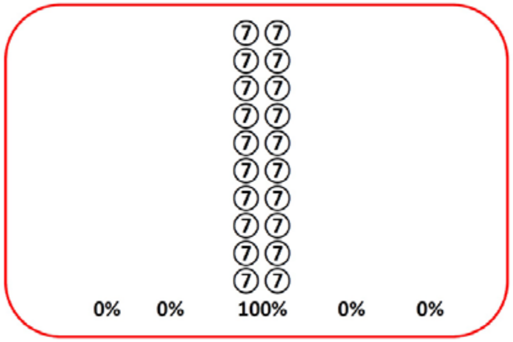
Appendix A: Additional Results

Setting 1

- There are 20 balls in the bag marked with prices \$5, \$6, \$7, \$8, and \$9. The number of balls marked with each price are shown in the following picture.

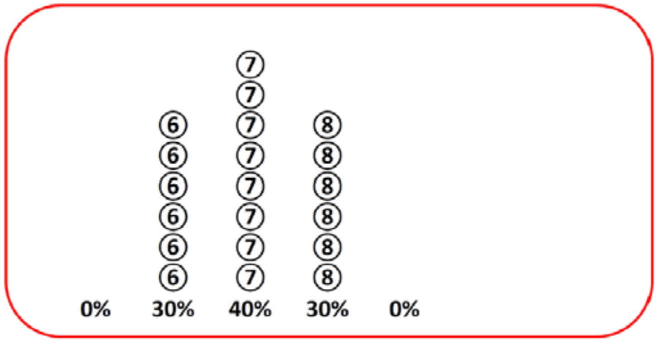


- Write down your choice of input (0-20) on the answer sheet.

Figure A1a. Setting 1, or Price Certainty Case.

Setting 2

- There are 20 balls in the bag marked with prices $5, $6, $7, $8, and $9. The number of balls marked with each price are shown in the following picture.

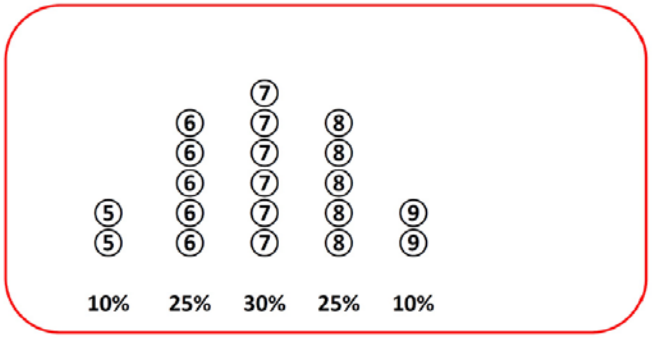


- Write down your choice of input (0-20) on the answer sheet.

Figure A1b. Setting 2.

Setting 3

- There are 20 balls in the bag marked with prices $5, $6, $7, $8, and $9. The number of balls marked with each price are shown in the following picture.



- Write down your choice of input (0-20) on the answer sheet.

Figure A1c. Setting 3.

Setting 4

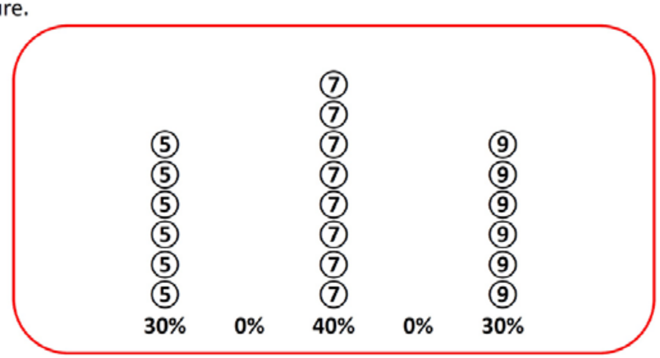
- There are 20 balls in the bag marked with prices \$5, \$6, \$7, \$8, and \$9. The number of balls marked with each price are shown in the following picture.

- Write down your choice of input (0-20) on the answer sheet.

Figure A1d. Setting 4.

Setting 5

- There are 20 balls in the bag marked with prices \$5, \$6, \$7, \$8, and \$9. The number of balls marked with each price are shown in the following picture.



- Write down your choice of input (0-20) on the answer sheet.

Figure A1e. Setting 5.

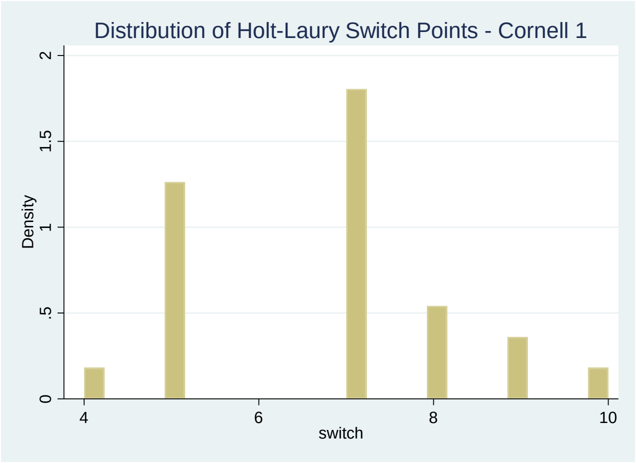


Figure A2. Distribution of Holt-Laury Switch Points for Respondents in the Cornell 1 Sub-Sample.

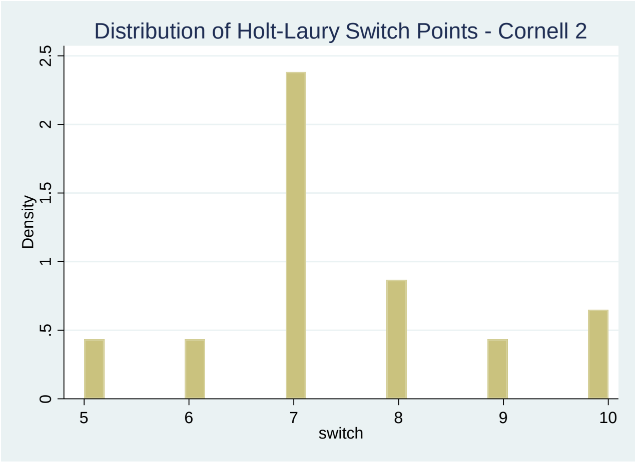


Figure A3. Distribution of Holt-Laury Switch Points for Respondents in the Cornell 2 Sub-Sample.

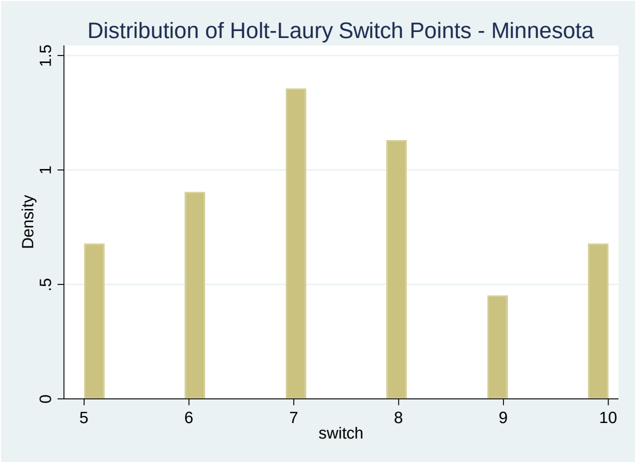


Figure A4. Distribution of Holt-Laury Switch Points for Subjects in the Cornell 2 Sub-Sample.

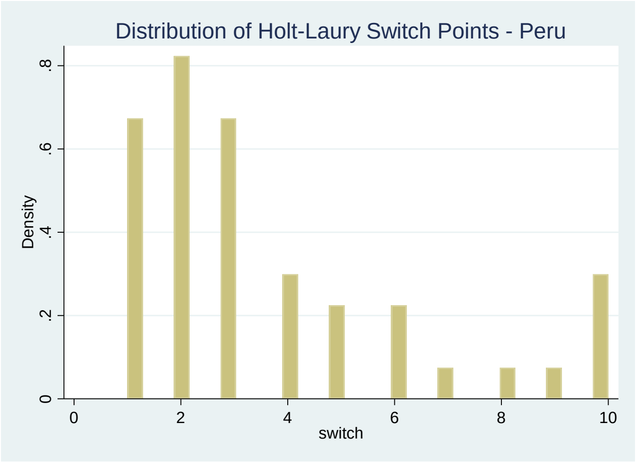


Figure A5. Distribution of Holt-Laury Switch Points for Subjects in the Peruvian Sub-Sample.

Table A1. Pooled Results with Holt-Laury Switch Points and Round Dummies

Variables	(1)	(2)	(3)
Dependent Variable: Output Level			
Uncertainty	0.242	0.813**	
	(0.210)	(0.362)	
Standard Deviation		-0.440	
		(0.301)	
$I(\sigma = 0.795)$, or Setting 2			0.484**
			(0.199)
$I(\sigma = 1.17)$, or Setting 3			0.310
			(0.289)
$I(\sigma = 1.451)$, or Setting 4			-0.090
			(0.269)
$I(\sigma = 1.58)$, or Setting 5			0.315
			(0.278)
Holt-Laury First	0.497*	0.489*	0.522*
	(0.291)	(0.291)	(0.290)
Cornell	-0.609*	-0.574	-0.588
	(0.367)	(0.368)	(0.367)
Minnesota	-0.702	-0.667	-0.609
	(0.499)	(0.498)	(0.499)
Constant	10.235***	10.726***	9.790***
	(0.385)	(0.684)	(0.380)
Holt-Laury Switch Point Dummies	Yes	Yes	Yes
Round Dummies	Yes	Yes	Yes
Observations	2,339	2,339	2,339

Standard errors clustered at the subject level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2a. Random Effects Regression Results for Price Risk at the Extensive and Intensive Margins with Control Variables (Price Risk Rounds Only)

	Full Sample					Risk-Averse Only	
Variables	**Pooled**	**Cornell 1**	**Cornell 2**	**Minnesota**	**Peru**	**Pooled**	**Peru**
	Dependent Variable: Quantity Produced						
Standard Deviation	-0.540*	-1.651***	-1.203**	-1.193**	0.849	-1.129***	-0.040
	(0.285)	(0.537)	(0.493)	(0.497)	(0.524)	(0.283)	(0.946)
Relative Risk Aversion	-0.088	-1.522**	0.837	-0.752	0.167	-0.141	0.922
	(0.347)	(0.751)	(0.860)	(1.079)	(0.413)	(0.528)	(0.938)
Holt-Laury First	0.739*				1.219	0.966**	2.824**
	(0.427)				(0.767)	(0.483)	(1.253)
Round	-0.011	0.004	-0.024	0.006	-0.032	-0.010	-0.046
	(0.013)	(0.024)	(0.025)	(0.020)	(0.025)	(0.013)	(0.042)
Cornell	-0.584					-0.908	
	(0.563)					(0.623)	
Minnesota	-0.815					-1.262	
	(0.721)					(0.787)	
Constant	11.294***	13.435***	11.397***	12.449***	9.839***	12.340***	10.227***
	(0.521)	(1.171)	(1.059)	(1.617)	(0.819)	(0.785)	(1.441)
Observations	1,633	336	312	390	595	1,240	202

Standard errors clustered at the subject level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2b. Random Effects Regression Results for Price Risk at the Extensive and Intensive Margins without Control Variables (Price Risk Rounds Only)

VARIABLES	Pooled	Cornell 1	Pooled Cornell 2	Minnesota	Peru	Risk-Averse Pooled	Risk-Averse Peru
			Dependent Variable: Quantity Produced				
Standard Deviation	-0.565**	-1.665***	-1.344***	-1.228**	0.810	-1.142***	0.176
	(0.281)	(0.535)	(0.437)	(0.500)	(0.517)	(0.280)	(0.895)
Holt-Laury First	0.470				1.222	0.597	3.054***
	(0.384)				(0.765)	(0.385)	(1.180)
Constant	10.793***	12.178***	11.941***	11.825***	9.218***	11.395***	9.668***
	(0.395)	(0.655)	(0.535)	(0.549)	(0.636)	(0.409)	(1.098)
Observations	1,633	336	312	390	595	1,240	202

Standard errors clustered at the subject level in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

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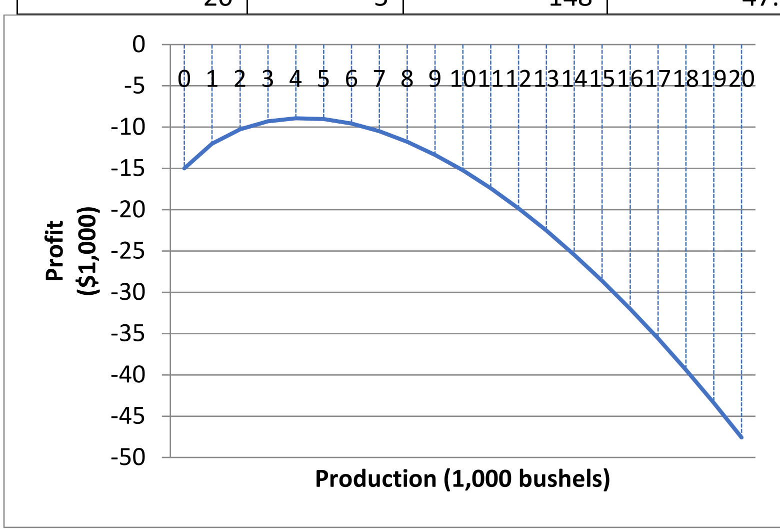
Appendix B: Experimental Protocol and Answer Sheets for Experiments in the US

Exhibit B1. Production, Price, Cost, and Profit Charts

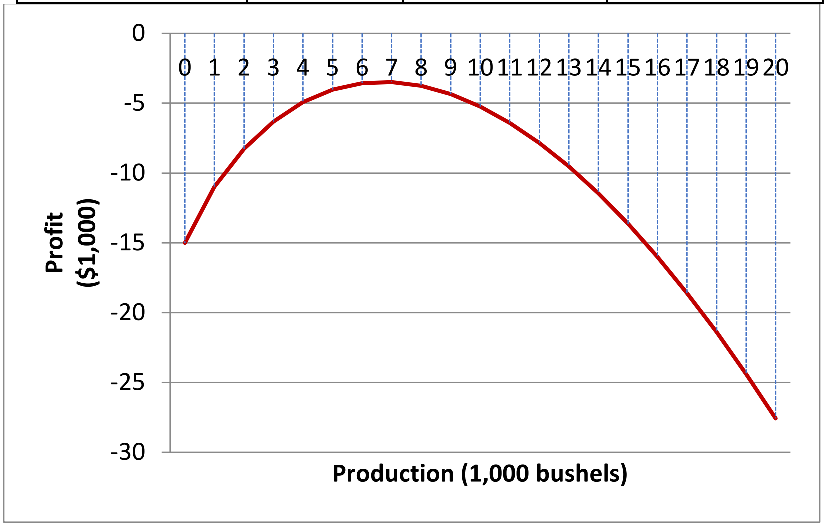
1. Wheat production, cost, and profit when price of wheat is **$5/bushel**.

(1) Wheat Production (1,000 bushels)	(2) Price ($/bushel)	(3) Cost $= 2 \times (1)^{1.4}+15$ ($ 1,000)	(4) Profit = (1)X(2)-(3) ($1,000)
0	5	15	-15.00
1	5	17	-12.00
2	5	20	-10.28
3	5	24	-9.31
4	5	29	-8.93
5	5	34	-9.04
6	5	40	-9.57
7	5	45	-10.49
8	5	52	-11.76
9	5	58	-13.35
10	5	65	-15.24
11	5	72	-17.41
12	5	80	-19.85
13	5	88	-22.54
14	5	95	-25.47
15	5	104	-28.63
16	5	112	-32.01
17	5	121	-35.60
18	5	129	-39.40
19	5	138	-43.39
20	5	148	-47.58



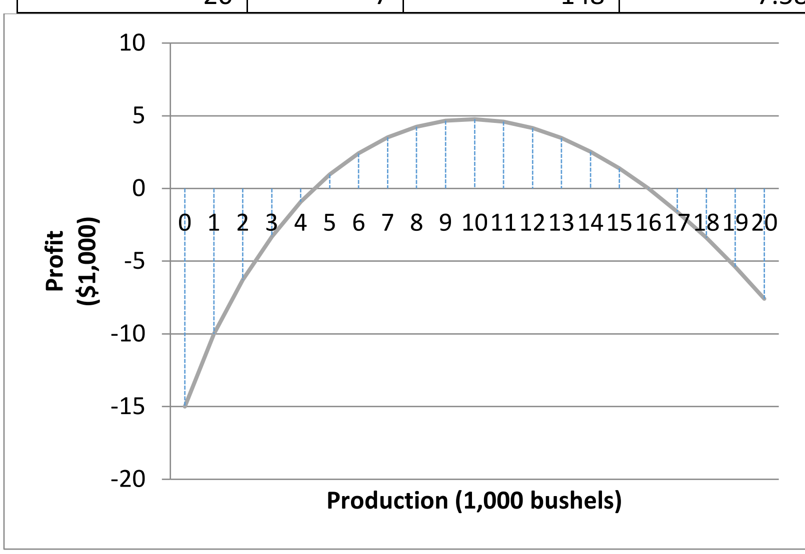
2. Wheat production, cost, and profit when price of wheat is <u>$6/bushel</u>.

(1) Wheat Production (1,000 bushels)	(2) Price ($/bushel)	(3) Cost = 2 X $(1)^{1.4}$+15 ($ 1,000)	(4) Profit = (1)X(2)-(3) ($1,000)
0	6	15	-15.00
1	6	17	-11.00
2	6	20	-8.28
3	6	24	-6.31
4	6	29	-4.93
5	6	34	-4.04
6	6	40	-3.57
7	6	45	-3.49
8	6	52	-3.76
9	6	58	-4.35
10	6	65	-5.24
11	6	72	-6.41
12	6	80	-7.85
13	6	88	-9.54
14	6	95	-11.47
15	6	104	-13.63
16	6	112	-16.01
17	6	121	-18.60
18	6	129	-21.40
19	6	138	-24.39
20	6	148	-27.58



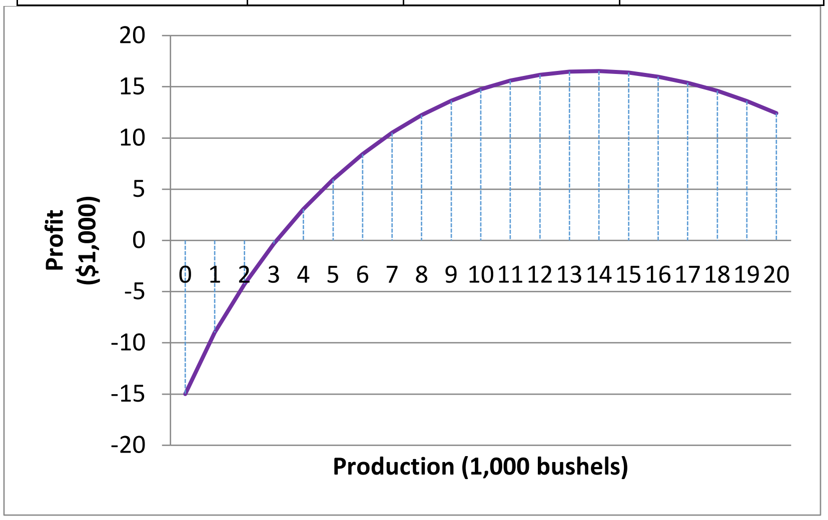
3. Wheat production, cost, and profit when price of wheat is **<u>$7/bushel</u>**.

(1) Wheat Production (1,000 bushels)	(2) Price ($/bushel)	(3) Cost = 2 X $(1)^{1.4}$+15 ($ 1,000)	(4) Profit = (1)X(2)-(3) ($1,000)
0	7	15	-15.00
1	7	17	-10.00
2	7	20	-6.28
3	7	24	-3.31
4	7	29	-0.93
5	7	34	0.96
6	7	40	2.43
7	7	45	3.51
8	7	52	4.24
9	7	58	4.65
10	7	65	4.76
11	7	72	4.59
12	7	80	4.15
13	7	88	3.46
14	7	95	2.53
15	7	104	1.37
16	7	112	-0.01
17	7	121	-1.60
18	7	129	-3.40
19	7	138	-5.39
20	7	148	-7.58



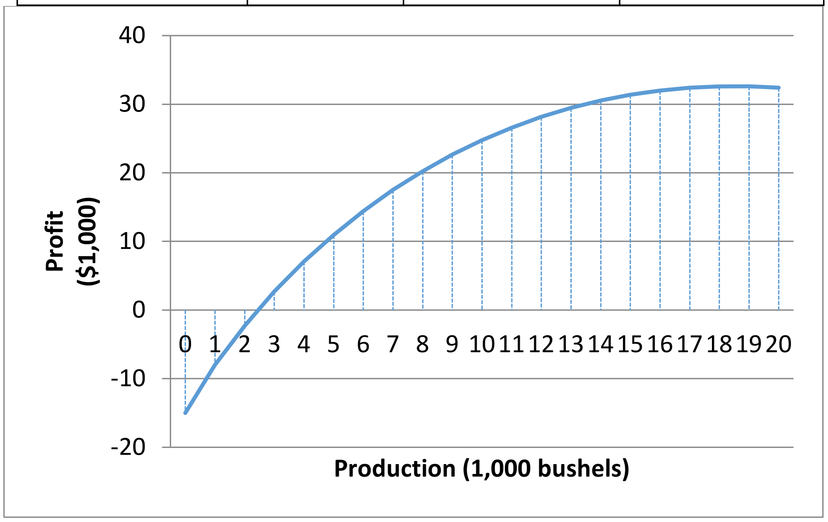
4. Wheat production, cost, and profit when price of wheat is **<u>$8/bushel</u>**.

(1) Wheat Production (1,000 bushels)	(2) Price ($/bushel)	(3) Cost $= 2 \times (1)^{1.4}+15$ ($ 1,000)	(4) Profit = (1)X(2)-(3) ($1,000)
0	8	15	-15.00
1	8	17	-9.00
2	8	20	-4.28
3	8	24	-0.31
4	8	29	3.07
5	8	34	5.96
6	8	40	8.43
7	8	45	10.51
8	8	52	12.24
9	8	58	13.65
10	8	65	14.76
11	8	72	15.59
12	8	80	16.15
13	8	88	16.46
14	8	95	16.53
15	8	104	16.37
16	8	112	15.99
17	8	121	15.40
18	8	129	14.60
19	8	138	13.61
20	8	148	12.42



5. Wheat production, cost, and profit when price of wheat is **<u>$9/bushel</u>**.

(1) Wheat Production (1,000 bushels)	(2) Price ($/bushel)	(3) Cost = 2 X $(1)^{1.4}$+15 ($ 1,000)	(4) Profit = (1)X(2)-(3) ($1,000)
0	9	15.00	-15.00
1	9	17.00	-8.00
2	9	20.28	-2.28
3	9	24.31	2.69
4	9	28.93	7.07
5	9	34.04	10.96
6	9	39.57	14.43
7	9	45.49	17.51
8	9	51.76	20.24
9	9	58.35	22.65
10	9	65.24	24.76
11	9	72.41	26.59
12	9	79.85	28.15
13	9	87.54	29.46
14	9	95.47	30.53
15	9	103.63	31.37
16	9	112.01	31.99
17	9	120.60	32.40
18	9	129.40	32.60
19	9	138.39	32.61
20	9	147.58	32.42



6. Profits when price of wheat is **<u>$5/bushel-$9/bushel</u>**.

Wheat Production	Profit				
	P = $5	P = $6	P = $7	P = $8	P = $9
0	-15.00	-15.00	-15.00	-15.00	-15.00
1	-12.00	-11.00	-10.00	-9.00	-8.00
2	-10.28	-8.28	-6.28	-4.28	-2.28
3	-9.31	-6.31	-3.31	-0.31	2.69
4	-8.93	-4.93	-0.93	3.07	7.07
5	-9.04	-4.04	0.96	5.96	10.96
6	-9.57	-3.57	2.43	8.43	14.43
7	-10.49	-3.49	3.51	10.51	17.51
8	-11.76	-3.76	4.24	12.24	20.24
9	-13.35	-4.35	4.65	13.65	22.65
10	-15.24	-5.24	4.76	14.76	24.76
11	-17.41	-6.41	4.59	15.59	26.59
12	-19.85	-7.85	4.15	16.15	28.15
13	-22.54	-9.54	3.46	16.46	29.46
14	-25.47	-11.47	2.53	16.53	30.53
15	-28.63	-13.63	1.37	16.37	31.37
16	-32.01	-16.01	-0.01	15.99	31.99
17	-35.60	-18.60	-1.60	15.40	32.40
18	-39.40	-21.40	-3.40	14.60	32.60
19	-43.39	-24.39	-5.39	13.61	32.61
20	-47.58	-27.58	-7.58	12.42	32.42

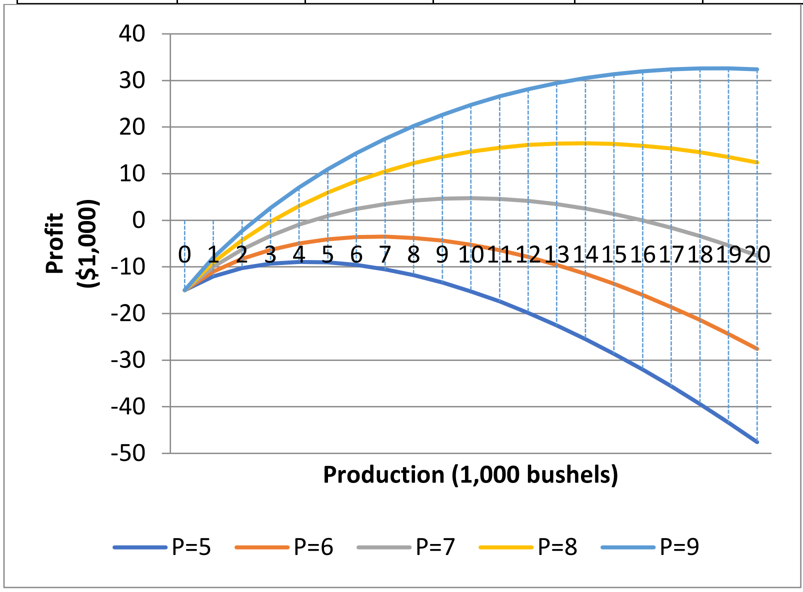


Exhibit B2. Consent Form

This study involves research in economics and it is being conducted by researchers from the University of Minnesota and from Cornell University. The purpose of this study is to test some of the predictions of economic theory. You will spend 60 to 90 minutes in the laboratory playing economic games for money. We will "endow" you with an amount of money ($45 plus $25) which you will use in playing the economic games you will be playing as part of this experiment. Depending on your performance as part of those experimental games, you may win additional money or lose some of the $25 portion of your endowment. You will, however, go home with at least $45 to compensate you for your time. Note, however, that you might go home with an additional $1.31 to $45.16 depending on your performance. Beyond that, there is no direct benefit to participation.

You will face minimal risk by participating in this experiment: the risks include loss of confidentiality and potential embarrassment associated with losing money. You will feel no discomfort at all.

The benefits of this research will be to validate an important and hitherto neglected aspect of economic theory as well as to inform policy makers about an important area of economic policy.

In the interest of confidentiality, we will not be recording your name, but we will be recording some demographic information about you (e.g., age, gender, level of education), and we will obviously be recording your actions in the experimental economic games we will have you play. It will not be possible from the data to tell who you are, and so all the data provided here today is confidential in its strictest sense.

For more information, please contact the Principal Investigator, Professor Marc F. Bellemare, Department of Applied Economics, University of Minnesota, by writing to him at mbellema@umn.edu or calling him at 612-624-1692. You may also contact Ms. Yu Na Lee, PhD student, Department of Economics, University of Minnesota, by writing to her at leex5244@umn.edu or calling her at 213-393-6712, or Professor David R. Just, Dyson School of Applied Economics and Management, Cornell University, by writing to him at drj3@cornell.edu or calling him at 607-255-2086.

Your participation is voluntary, and your refusal to participate will involve no penalty or decrease in benefits to which you are otherwise entitled outside of this experiment. Moreover, you may discontinue participation at any time without penalty or loss of benefits to which you are entitled outside of this experiment.

Name (please print): ____

Date: ____

Signature: ____

Exhibit B3. Instructions Given and Read to the Subjects

General Instructions

- This is an experiment in the economics of individual decision making. We are trying to understand how people make production decisions when they are unsure of the price they will receive. We have designed simple decision-making games in which we will ask you to make choices in a series of situations.
- There are two sets of games. In the first set of games, you will be making decisions assuming that you are a farmer producing a single commodity, wheat. In the second set of games, you will be given a series of lotteries to choose from. More detailed explanations will follow in each set.
- You will spend 90 to 120 minutes in this study playing economic games. You will automatically receive $45 for participation and in addition may earn between $1.31 and $45.16 depending on your performance and also on the luck on the experiment.

- You should make your own decision and should not discuss your decisions or the decision scenarios with other participants. Also, please turn off your cell phones.
- You need to have a good understanding on how your decisions affect your payoff. Please raise your hand at any time during the session if you have any question.

Set I: Single-Commodity Production Game

- You are a farmer who produces and sells only one commodity, <u>wheat</u>.
- The selling price of wheat in dollars per bushel will be one of the five possible values: $5, $6, $7, $8, and $9, and it will be realized *after* you make your production decision to reflect the real-world output price uncertainty.
- You will be given charts 1 through 5 which document the amount of cost to be incurred according to production levels 0 through 20 (in 1,000 bushels), and the corresponding profit (in $1,000) that will occur *under the five different price scenarios*. These charts contain all the information about how your production decision, cost of production, and your profit relate to one another. Chart 6 is a summary of charts 1 through 5 and shows only the relationship between the production level and the profit.
- Prices will be determined in the following way: In each round, you will be presented a picture of a bag with 20 balls. Each of the 20 balls have different prices (⑤, ⑥, ⑦, ⑧, and ⑨) marked. There are five bags with different composition of balls. In each round, a bag will be selected randomly. The average of the prices marked on the 20 balls will always be $7, but the composition of balls marked with different prices will change each round.
- Given a picture of a bag, you decide how much wheat to produce, knowing *only the composition of prices marked in the bag, not the actual price that will be drawn*.
- Here is an example:

⑦⑦
⑦⑦
⑦⑦
⑦⑦
⑦⑦
⑦⑦

		⑦⑦		
		⑦⑦		
		⑦⑦		
		⑦⑦		
0%	0%	100%	0%	0%

In the bag depicted above, all the balls are marked with price $7. Therefore, you are 100% sure that the wheat price will be $7 per bushel.

		⑦		
	⑥	⑦	⑧	
	⑥	⑦	⑧	
	⑥	⑦	⑧	
⑤	⑥	⑦	⑧	⑨
⑤	⑥	⑦	⑧	⑨
10%	25%	30%	25%	10%

In the bag depicted above, among the 20 balls, there are 2 balls each with $5 and $9 marked, 5 balls each with $6 and $8 marked, and 6 balls with $7 marked. There is a 10% probability that a random draw from this bag will be the price of $5, and a 25% probability for the price of $6, etc. Here is another example.

⑤	⑥	⑦	⑧	⑨
⑤	⑥	⑦	⑧	⑨
⑤	⑥	⑦	⑧	⑨
⑤	⑥	⑦	⑧	⑨
20%	20%	20%	20%	20%

In this bag, we can see that all possible wheat prices are equally likely to occur. In comparison to the last given situation, it is more likely to have more extreme prices than the first round.

- Given such information, you will be asked to choose any integer between 0 and 20 as your production level. You may refer to the charts 1-6 to facilitate your decision.
- Your goal is to maximize the profit (price times quantity produced minus cost of production), since maximizing profit is identical to maximizing your payoff.
- Note that there is no subsistence constraint, meaning that there is no minimum required level of production for your survival. Nor is there a requirement to make a positive profit in order for you to survive. Negative profits mean that you lose some of the money that you are endowed with.
- *After* you have chosen how much to produce, a ball will be drawn randomly from the bag which will determine your selling price. You will sell your wheat at that price, which will determine your profit.
- You will first play 10 rounds of practice games. After the practice games, you will play 20 rounds of the real games. In the real games, your profits will affect your actual payoffs from the games.
- In this set of the games, you start from base payoff of $25. In a given round, your profit will be between -47.58 and 32.61. After the 20 actual rounds, we will randomly select a round. Your payoff from these games will be determined in the following way: $25 base payoff + a half of your profit in the randomly selected round. For example, if you have made a loss of 30 in the

selected round, your final payoff will be $25 + (-$30 X 0.5) = $10. If you have made a profit of 28, your final payoff will be $25 + ($28 X 0.5) = $39.

- Your final payoff in this set of the games will range between $1.21 and $41.31.

Set II: Lottery Choice Game

- In this set of games, you will be presented a table of ten paired lotteries, A and B, from which you are asked to choose one that you prefer.
- Below is an example of the options that you will be given:

Option A	Option B
1/10 of $2.00, 9/10 of $1.60	1/10 of $3.85, 9/10 of $0.10

If you choose option A, there is a probability of 0.1 that you will be receiving $2.00, and a probability of 0.9 that you will be receiving $1.60. If you choose option B, there is a probability of 0.1 that you will be receiving $3.85 which is much bigger than $2.00 in option A. However, there is also a 0.9 probability that you will be receiving only $0.10.

- Stop once you have chosen the option B.
- Your payoff from this round of game will be determined in the following way: A random number will be drawn to determine the row number of one of your choices. Then, according to the probability that the row of the choice dictates, either option A or B will be drawn, which will determine your payoff.
- Your payoff from this round will range between $0.1 and $3.85.

Exhibit B4. Answer Recording Sheet

Set I: Single-Commodity Production Game

- **Practice Rounds**

Round	Choice of Production Level (1,000 bushels)
1	
2	
3	
4	
5	
6	
7	
8	
9	
10	

- **Actual Rounds**

Round	Choice of Production Level (1,000 bushels)
1	
2	
3	
4	
5	
6	
7	
8	
9	
10	
11	
12	
13	
14	
15	
16	
17	
18	
19	
20	

Set II: Lottery Choice Game

	Option A	Option B	Your Choice (circle one)
1	1/10 of $2.00, 9/10 of $1.60	1/10 of $3.85, 9/10 of $0.10	A , B
2	2/10 of $2.00, 8/10 of $1.60	2/10 of $3.85, 8/10 of $0.10	A , B
3	3/10 of $2.00, 7/10 of $1.60	3/10 of $3.85, 7/10 of $0.10	A , B
4	4/10 of $2.00, 6/10 of $1.60	4/10 of $3.85, 6/10 of $0.10	A , B
5	5/10 of $2.00, 5/10 of $1.60	5/10 of $3.85, 5/10 of $0.10	A , B
6	6/10 of $2.00, 4/10 of $1.60	6/10 of $3.85, 4/10 of $0.10	A , B
7	7/10 of $2.00, 3/10 of $1.60	7/10 of $3.85, 3/10 of $0.10	A , B
8	8/10 of $2.00, 2/10 of $1.60	8/10 of $3.85, 2/10 of $0.10	A , B
9	9/10 of $2.00, 1/10 of $1.60	9/10 of $3.85, 1/10 of $0.10	A , B
10	10/10 of $2.00, 0/10 of $1.60	10/10 of $3.85, 0/10 of $0.10	A , B

Please stop once you have chosen the option B.

❖ **Demographics**

- Age:__________
- Sex: M / F
- Ethnicity/Race:
  (1) Hispanic or Latino
  (2) American Indian or Alaska Native
  (3) Asian
  (4) Black or African American
  (5) Native Hawaiian or Other Pacific Islander
  (6) White
- Nationality: ____________

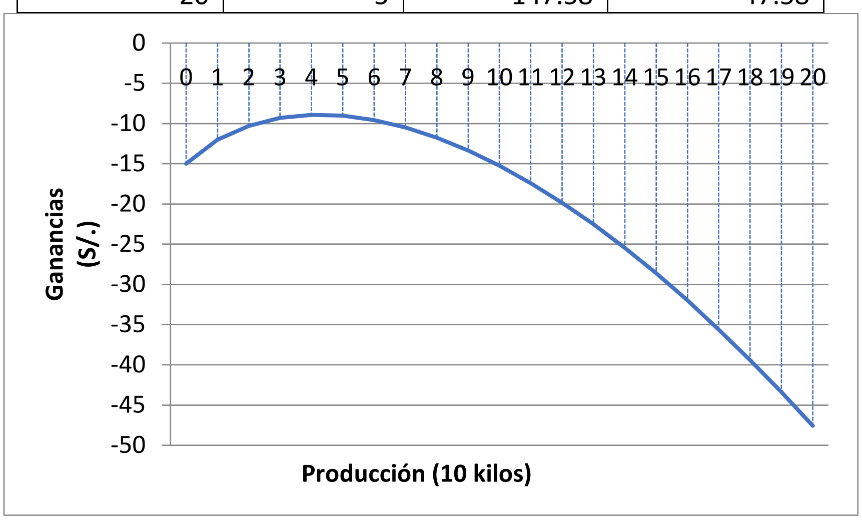
FOR ONLINE PUBLICATION

Appendix C: Experimental Protocol and Answer Sheets for Experiments in Peru

Exhibit C1. Production, Price, Cost, and Profit Charts

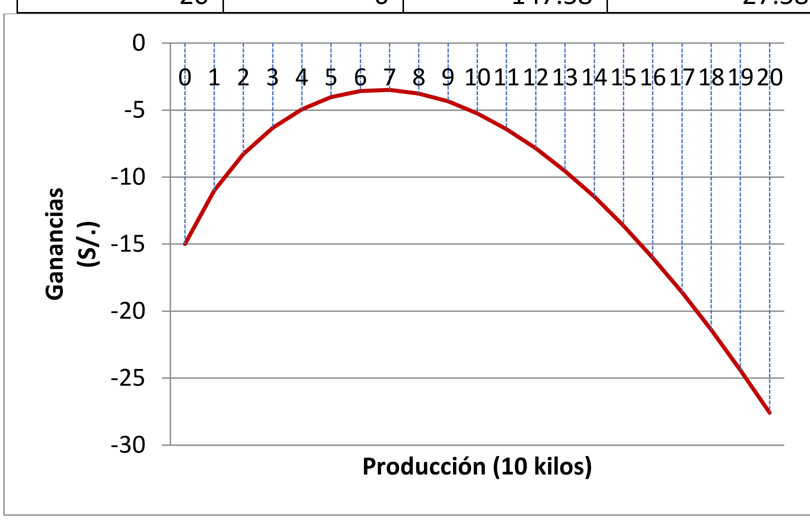
1. La producción de papas, el costo y las ganancias cuando el precio del papas es de S/. 5 / 10 kilos.

(1) Producción de papas (10 kilos)	(2) Precio (S/./10 kilo)	(3) Costo = 2 X $(1)^{1.4}$+15 (S/.)	(4) Ganancias = (1)X(2)-(3) (S/.))
0	5	15.00	-15.00
1	5	17.00	-12.00
2	5	20.28	-10.28
3	5	24.31	-9.31
4	5	28.93	-8.93
5	5	34.04	-9.04
6	5	39.57	-9.57
7	5	45.49	-10.49
8	5	51.76	-11.76
9	5	58.35	-13.35
10	5	65.24	-15.24
11	5	72.41	-17.41
12	5	79.85	-19.85
13	5	87.54	-22.54
14	5	95.47	-25.47
15	5	103.63	-28.63
16	5	112.01	-32.01
17	5	120.60	-35.60
18	5	129.40	-39.40
19	5	138.39	-43.39
20	5	147.58	-47.58



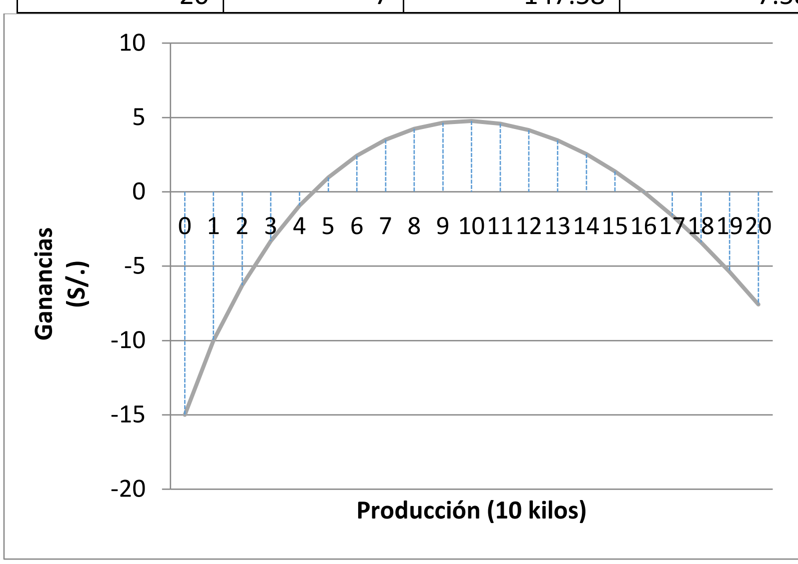
2. **La producción de papas, el costo y las ganancias cuando el precio del papas es de S/. 6 / 10 kilos.**

(1) Producción de papas (10 kilos)	(2) Precio (S/./10 kilo)	(3) Costo = 2 X $(1)^{1.4}$+15 (S/.)	(4) Ganancias = (1)X(2)-(3) (S/.))
0	6	15.00	-15.00
1	6	17.00	-11.00
2	6	20.28	-8.28
3	6	24.31	-6.31
4	6	28.93	-4.93
5	6	34.04	-4.04
6	6	39.57	-3.57
7	6	45.49	-3.49
8	6	51.76	-3.76
9	6	58.35	-4.35
10	6	65.24	-5.24
11	6	72.41	-6.41
12	6	79.85	-7.85
13	6	87.54	-9.54
14	6	95.47	-11.47
15	6	103.63	-13.63
16	6	112.01	-16.01
17	6	120.60	-18.60
18	6	129.40	-21.40
19	6	138.39	-24.39
20	6	147.58	-27.58



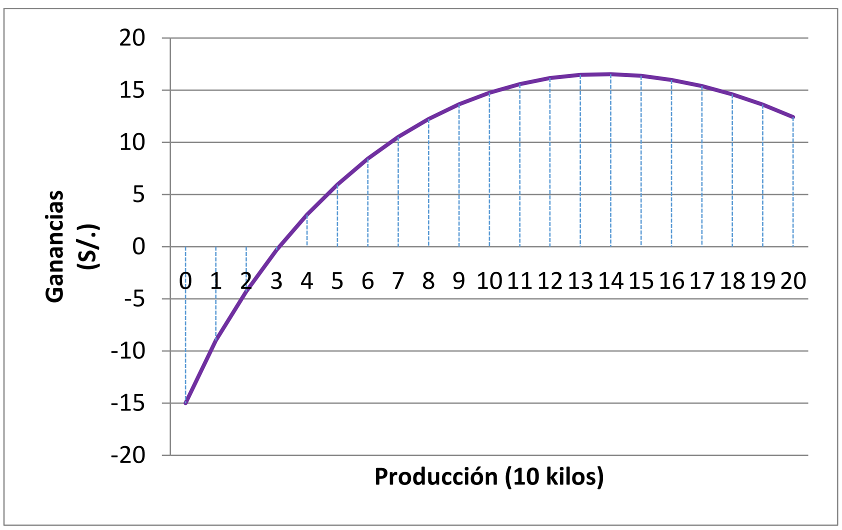
3. La producción de papas, el costo y las ganancias cuando el precio del papas es de S/. 7 / 10 kilos.

(1) Producción de papas (10 kilos)	(2) Precio (S/./10 kilo)	(3) Costo $= 2 X (1)^{1.4}+15$ (S/.)	(4) Ganancias = (1)X(2)-(3) (S/.))
0	7	15.00	-15.00
1	7	17.00	-10.00
2	7	20.28	-6.28
3	7	24.31	-3.31
4	7	28.93	-0.93
5	7	34.04	0.96
6	7	39.57	2.43
7	7	45.49	3.51
8	7	51.76	4.24
9	7	58.35	4.65
10	7	65.24	4.76
11	7	72.41	4.59
12	7	79.85	4.15
13	7	87.54	3.46
14	7	95.47	2.53
15	7	103.63	1.37
16	7	112.01	-0.01
17	7	120.60	-1.60
18	7	129.40	-3.40
19	7	138.39	-5.39
20	7	147.58	-7.58



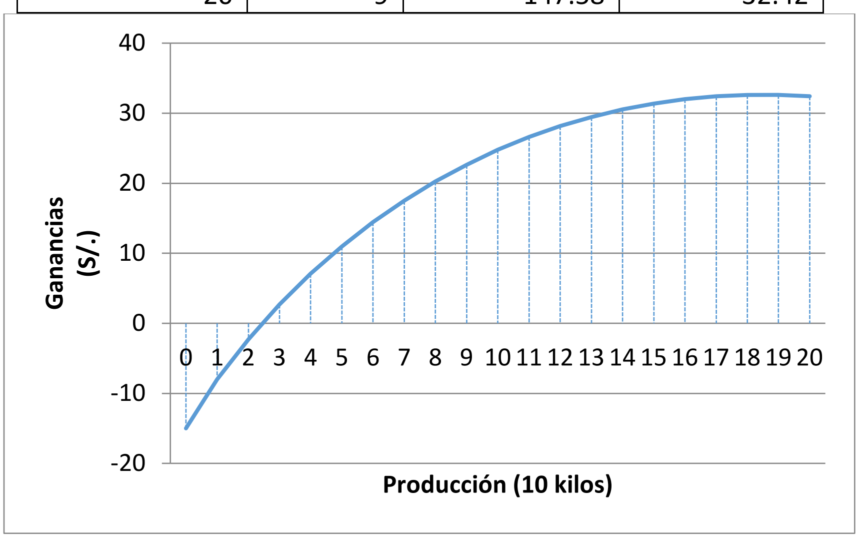
4. **La producción de papas, el costo y las ganancias cuando el precio del papas es de S/. 8 / 10 kilos..**

(1) Producción de papas (10 kilos)	(2) Precio (S/./10 kilo)	(3) Costo $= 2 X (1)^{1.4}+15$ (S/.)	(4) Ganancias = (1)X(2)-(3) (S/.))
0	8	15.00	-15.00
1	8	17.00	-9.00
2	8	20.28	-4.28
3	8	24.31	-0.31
4	8	28.93	3.07
5	8	34.04	5.96
6	8	39.57	8.43
7	8	45.49	10.51
8	8	51.76	12.24
9	8	58.35	13.65
10	8	65.24	14.76
11	8	72.41	15.59
12	8	79.85	16.15
13	8	87.54	16.46
14	8	95.47	16.53
15	8	103.63	16.37
16	8	112.01	15.99
17	8	120.60	15.40
18	8	129.40	14.60
19	8	138.39	13.61
20	8	147.58	12.42



5. La producción de papas, el costo y las ganancias cuando el precio del papas es de S/. 9 / 10 kilos.

(1) Producción de papas (10 kilos)	(2) Precio (S/./10 kilo)	(3) Costo = 2 X $(1)^{1.4}$+15 (S/.)	(4) Ganancias = (1)X(2)-(3) (S/.))
0	9	15.00	-15.00
1	9	17.00	-8.00
2	9	20.28	-2.28
3	9	24.31	2.69
4	9	28.93	7.07
5	9	34.04	10.96
6	9	39.57	14.43
7	9	45.49	17.51
8	9	51.76	20.24
9	9	58.35	22.65
10	9	65.24	24.76
11	9	72.41	26.59
12	9	79.85	28.15
13	9	87.54	29.46
14	9	95.47	30.53
15	9	103.63	31.37
16	9	112.01	31.99
17	9	120.60	32.40
18	9	129.40	32.60
19	9	138.39	32.61
20	9	147.58	32.42



6. Las ganancias cuando los precios van desde S/. 5 a S/. 9 por 10 kg

Producción de papas (en 10 kilos)	Ganancias				
	P = S/.5	P = S/.6	P = S/.7	P = S/.8	P = S/.9
0	-15.00	-15.00	-15.00	-15.00	-15.00
1	-12.00	-11.00	-10.00	-9.00	-8.00
2	-10.28	-8.28	-6.28	-4.28	-2.28
3	-9.31	-6.31	-3.31	-0.31	2.69
4	-8.93	-4.93	-0.93	3.07	7.07
5	-9.04	-4.04	0.96	5.96	10.96
6	-9.57	-3.57	2.43	8.43	14.43
7	-10.49	-3.49	3.51	10.51	17.51
8	-11.76	-3.76	4.24	12.24	20.24
9	-13.35	-4.35	4.65	13.65	22.65
10	-15.24	-5.24	4.76	14.76	24.76
11	-17.41	-6.41	4.59	15.59	26.59
12	-19.85	-7.85	4.15	16.15	28.15
13	-22.54	-9.54	3.46	16.46	29.46
14	-25.47	-11.47	2.53	16.53	30.53
15	-28.63	-13.63	1.37	16.37	31.37
16	-32.01	-16.01	-0.01	15.99	31.99
17	-35.60	-18.60	-1.60	15.40	32.40
18	-39.40	-21.40	-3.40	14.60	32.60
19	-43.39	-24.39	-5.39	13.61	32.61
20	-47.58	-27.58	-7.58	12.42	32.42

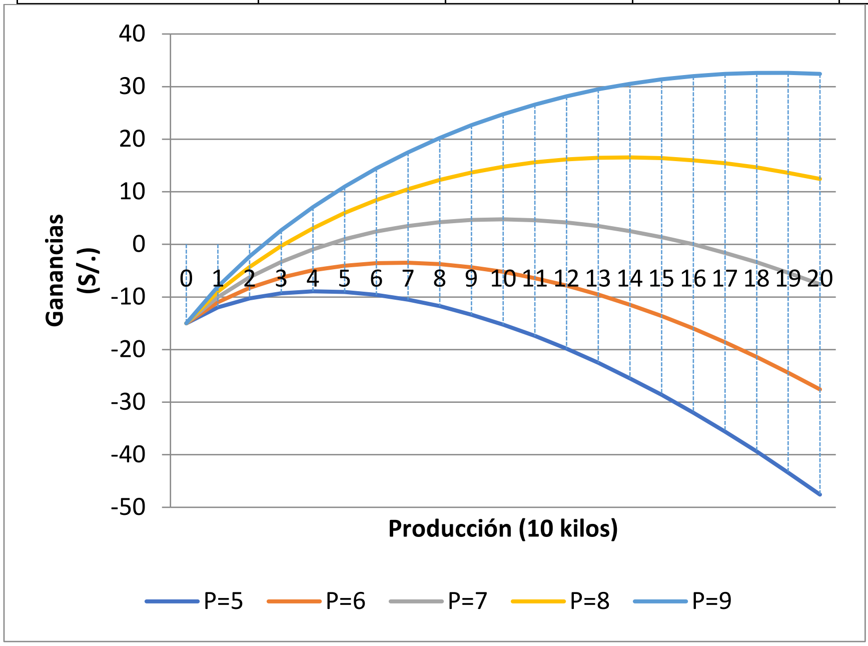


Exhibit C2. Consent Form

Formulario de consentimiento

Usted está siendo invitado a ser parte de un estudio de investigación en el área económica que está conducido por investigadores de la University of Minnesota y Cornell University. El objetivo del estudio es probar algunas teorías económicas que esperaríamos encontrar. Su tiempo de compromiso esperado para este estudio es de 60 a 90 minutos en el cual usted estaría practicando algunos juegos experimentales con el uso del dinero. Les corresponderíamos un monto de S/. 45 + un promedio de S/. 25 que usted utilizará para participar a los juegos de economía y que son parte del estudio. Dependiendo de su rendimiento en estos juegos experimentales, usted podrá ganar o perder una parte de los S/. 25 que se le va a corresponder para el juego. De toda forma, usted recibirá un mínimo de S/. 45 como compensación de su tiempo con nosotros. Además, usted puede recibir un monto adicional entre S/. 1.51 y S/. 52.86 dependiendo de su rendimiento en los juegos. A parte esto, no hay beneficios directos asociados a este estudio.

Usted enfrentará un riesgo mínimo en participar en esta investigación: falta de confidencia y potencial vergüenza asociada en perder algo de dinero. No habrá ningún tipo de incomodidad. Los beneficios de esta investigación serán particularmente relevante para validar algunos aspectos que hasta ahora no han sido debidamente explorados en la teoría económica e informar los hacedores de política en poner más atención a temas de políticas económicas.

Todos los datos registrados en este estudio serán confidenciales y sus respuestas serán privadas: no estaremos registrando su nombre, pero si alguna información demográfica (edad, sexo, nivel de educación), además de sus acciones en este juego económico experimental. No incluiremos ninguna información que pueda hacer posible identificarlo/a.

Para mayores informaciones, por favor contactarse con el Investigador Principal, el Profesor Marc F. Bellemare, Departamento de Economía Aplicada, University of Minnesota, a su correo mbellema@umn.edu o llamarle a su número +1-612-624-1692. También puedes contactarte con Ms. Yu Na Lee, estudiante de Doctorado, en el Departmento de Economía Aplicada, University of Minnesota, a su correo leex5244@umn.edu o llamandola al +1-213-393-6712, o el Professor David R. Just, Dyson School of Applied Economics and Management, Cornell University, a su correo drj3@cornell.edu o a su teléfono +1-607-255-2086.

Su participación es totalmente voluntaria: usted puede decidir si participar o no en la encuesta y no hay problemas si es que decide no participar. También, puede detenerla en cualquier momento o no responder preguntas que no te gusten. No estará recibiendo ninguna penalidad o pérdida de beneficios en todo lo que no concierne este estudio.

Nombre (en mayúscula): ____

Fecha: ____

Firma: ____

Exhibit C3. Instructions for Enumerators

Hoja de respuesta individual

1. Código del encuestador: _____ (1 = Marcela; 2 = Nadesca).
2. Código del encuestado: _____ (1-24 = Juego Risk encuestado por Marcela;

   25-48 = Juego Risk encuestado por Nadesca;

   49-72 = Juego Ambiguity encuestado por Marcela;

   73-96 = Juego Ambiguity encuestado por Nadesca).

Instrucciones: Empieza leyendo el consentimiento informado al encuestado. Asegúrate que escribió bien su nombre, su firma y puso la fecha.

Preguntas de revisión

Instrucciones: Hágale al encuestado las siguientes 2 preguntas. Si se equivoca en más de una respuesta, agradécele de su disponibilidad y sigues con otro encuestado.

2. 1. ¿Cuál es el 40% de S/. 100? S/. _____.

2. 2. Si existe el 25% de posibilidad que llueva, ¿cuál es el porcentaje de posibilidad que no llueva? _____.

2. 3. Imagina sortear una pelota en una bolsa que contiene 3 pelotas azules y 7 pelotas rojas. ¿Cuál es la probabilidad de que puedas escoger una pelota de color azul? _____.

Instrucciones: Lees el texto siguiente.

"Este cuestionario presenta unas pocas preguntas sobre usted y les pedirá jugar en dos juegos económicos con dinero real con el objetivo de testear teoría económica. No hay respuestas correctas o incorrectas. Porque su desempeño en los dos juegos dictara cuánto dinero podrá ganar, tú tienes que hacer su mejor en ambos juegos."

Preguntas de información demográfica:

3.1 ¿Cuál es su edad (en años)? ______

3.2 ¿Es hombre o mujer? Por favor seleccione una opción M F

3.3 ¿Cuál es el nivel educativo más alto alcanzado?

3.3.1. Educación inicial
3.3.2. Educación primaria
3.3.3. Educación secundaría
3.3.4. Universidad

3.3.5. Instituto técnico

3.3.6. Otro

3.4. ¿Usted cultiva papas? _____ (1= Sí; 2= No.)

Instrucciones: Por favor avise al encuestado que, en virtud de haber contestado las preguntas de arriba, ha ganado un pago por su participación de S/. 45, pero que está por jugar dos sets de juegos donde podrá ganar dinero adicional entre los rangos de S/. 1.51 to S/. 52.86 dependiendo en su desempeño y en su suerte.

Instrucciones: Tira un dado de 6 lados (D6)

4.1 Registra el número aquí: _____

Instrucciones: Para 1-2-3, juega el juego de producción primero, después el juego de la lotería; para 4-5-6 juega el juego de la lotería primero, después finaliza con el juego de producción.

Si juega el juego de producción primero, ve a la siguiente página 4.

Si tú juegas el juego de la lotería primero, ve a la página 8.

5. Juego de producción

Instrucciones: lees el texto siguiente:

"Este es un estudio de economía de toma de decisiones individuales. Nosotros tratamos de entender como las personas toman decisiones de producción cuando no tienen conocimiento del precio que recibirán por su producto. También, hemos diseñado simples juegos de decisión donde le preguntaremos que tome decisiones en una serie de situaciones diferentes.

Para este juego, usted comienza con un capital de S/. 25 Soles, donde puede perder casi todo y terminar con S/. 1.21 soles o ganar más dinero y terminar con S/. 41.31. Ambas son la ganancia mínima o máxima, sin embargo en la mayoría de los casos, usted podrá ganar un valor entre este rango por ser parte del juego.

Usted necesita tener una buena comprensión en como su decisión puede afectar su paga. Por favor avísame en cualquier momento durante la sesión si tiene alguna pregunta.

Imagine que usted es un agricultor que produce y vende únicamente un bien/producto, papas.

El precio de venta de un saco de 10 kg de papa en soles (S/.) será uno de 5 valores posibles: 5, 6, 7, 8, o 9, y este será dado después que usted tome su decisión de producción para reflejar la incertidumbre del precio de venta del mundo real.

Recibirá unas tablas/imágenes del 1 hasta el 5 donde se documenta la cantidad de costo que se debe incurrir acorde al nivel de producción que va del 0 al 20 (en 10 kilos), y la correspondiente ganancia (en S/.) que ocurriría bajo las 5 diferentes posibilidades de precios. Estas tablas contienen toda la información acerca de cómo tu decisión de producción, costos de producción, y la ganancia se relacionan entre ellos. La tabla 6 es un resumen de las tablas del 1 al 5 y muestra únicamente la relación entre el nivel de producción y la ganancia.

Instrucciones: Oriente al participante en como leer las tablas. Dígales que ellos son libres de inspeccionar los sacos al finalizar del juego si quieren estar seguros que efectivamente representan a los números presentes en las tablas.

Instrucciones: Ahora, es necesario que el participante tenga conocimiento de cómo el precio será determinado. Lea lo siguiente al encuestado:

"Los precios se determinaran de la siguiente manera: en cada ronda, se te presentará una imagen de un saco con 20 bolas. Cada una de las 20 bolas tienen distintos precios (5, 6, 7, 8, y 9) asignados. Hay 5 sacos con distintas composiciones de 20 bolas. En cada ronda, una bolsa será seleccionada aleatoriamente. El promedio de precios asignado en las 20 bolas será siempre S/. 7, pero la composición de bolas asignadas por diferentes precios cambiara en cada ronda."

Dada una imagen de un saco, tú decides cuantos sacos de papas producir, conociendo solo la composición de precios asignadas en el saco, no el precio actual que será sorteado.

Este es un ejemplo (muestra la imagen del escenario 0). En el saco mostrado aquí, en todas las bolas se les asignó el precio de S/.7. Por lo tanto, tú estás 100% seguro que el precio de la papa será 7 Soles por saco de papas.

Aquí hay otro ejemplo (muestra la imagen del escenario 2). En el saco mostrado aquí, entre las 20 bolas hay dos bolas cada una con S/. 5 y S/. 9 asignados, 5 bolas con 6 S/. y 8 s/. , y 6 bolas con S/.7 marcados. Hay un 10 % de probabilidad de que haciendo un sorteo aleatorio de este saco el precio será S/. 5, un 25% de probabilidad que el precio sea S/. 6, etc.

Aquí hay otro ejemplo (muestra la imagen del escenario 3). En esta bolsa, podemos ver que todos los posibles precios de la papa puedan darse por igual. En comparación con las últimas situaciones mostradas, es probable de tener más precios extremos que los casos anteriores.

Dada esta información, se te preguntara que escojas un nivel de producción entre 0 a 20. Puedes referirte a las tablas 1-6 para facilitar tu decisión.

Nota que no hay una restricción de subsistencia, lo que significa que no hay un nivel mínimo requerido de producción para tu supervivencia. Tampoco hay un requerimiento para obtener una ganancia positiva para que sobrevivas. Ganancias negativas significa que pierdes algo del capital (S/.25) que se te ha entregado.

Después de que hayas escogido cuanto producir, una bola será sorteada aleatoriamente del saco la cual determinará el precio de venta. Tú podrás vender tus papas a este precio, y con esto se determinará tu ganancia.

Usted primero jugara 10 rondas de práctica. Después de los juegos de práctica, jugarás 20 rondas reales de juego. En los juegos verdaderos, tus ganancias van a afectar tus actuales pagos en los juegos.

En este set de juegos, tú comienzas con un capital de S/.25. En cada ronda, tus ganancias estarán entre "- 47.58 y 32.61". Después de 20 actuales rondas, nosotros al azar seleccionaremos una ronda. Tu paga de este juego se determinara de la siguiente forma: S/. 25 de capital base + la mitad de la ganancia en la ronda seleccionada al azar. Por ejemplo, si tú has tenido una pérdida de 30 Soles en la ronda seleccionada, tu pago final será S/. 25 + (S/. -30 X 0.5) = S/. 10. Si, en cambio, has tenido una ganancia positiva de 28, tu pago final será S/. 25 + (S/. 28 X 0.5) = S/. 39.

Tu pago final en este set de juegos será entre el rango de S/. 1.21 y S/. 41.31."

Instrucciones:

Muestre las imágenes en el dado para ayudar a los encuestados a entender el proceso de aleatorización. Recuerda usar un dado de 6 lados (D6) y un dado de cuatro lados (D4) para este juego. El tiro del D6 determinará a qué precio se enfrentará el encuestado (escenario 0) de S/. 7 o un precio incierto, como sigue:

1-2 = Precio certero (escenario 0, o precio de S/. 7);

3-4-5-6 = Precio incierto. Para estos precios inciertos tire el D4.

El tiro del D4 determinará en cuál escenario el participante se encuentra:

1 = escenario 1,

2 = escenario 2,

3 = escenario 3,

4 = escenario 4.

Cada escenario corresponde al número en su bolsa de bolas de ping pong.

Muestre al sujeto la tabla/imagen para el escenario relevante. Anote el escenario en la parte de abajo de la hoja de respuestas. Dígale que usted al azar extraerá el precio de la bolsa por ese escenario, y que el realice lo que cree sea su mejor decisión de producción. Una vez que haya decido cuanto producir, anote su decisión de producción para esta ronda en la hoja de respuesta. Sucesivamente, el encuestado puede escoger su un precio de la bolsa. Registra el precio y deja que el encuestado mire cuanto es lo que ha ganado o ha perdido en esta ronda, de acuerdo a su decisión de producción y bola extraída.

5.1. Rondas de Práctica

Instrucciones: Dígale al encuestado que el ahora estará jugando 10 ronda de práctica que lo ayudaran a entender mejor el juego. Avísele que las ganancias que tenga en estas rondas no afectaran su pago actual.

5.1.1. Ronda	**5.1.2.Escenario (0 a 4)**	**5.1.3. Producción de papas (0 a 20)**	**5.1.4. Precio (5 a 9)**	**5.1.5. Ganancias (-47.58 a 32.61)**
1				
2				
3				

4				
5				
6				
7				
8				
9				
10				

Instrucciones: Después de terminar las rondas de práctica, pregunta al encuestado si tiene alguna pregunta. Contesta su pregunta lo mejor que puedas. Una vez que su pregunta haya sido contestada, muévete a las rondas reales.

5.2. Rondas Reales

Instrucciones: Ahora, explica al encuestado que a pesar que hayan 20 rondas solo una se pagará, y se determinará al final al azar, así que tendrá todo el incentivo de hacer lo que cree sea lo mejor en cada ronda. Registre la información de la misma manera que en la 10 rondas de práctica.

5.2.1 Ronda	5.2.2. Escenario (0 a 4)	5.2.3. Producción de papas (0 a 20)	5.2.4. Precio (5 a 9)	5.2.5. Ganancias (-47.58 a 32.61)
1				
2				
3				
4				
5				
6				
7				
8				
9				

10				
11				
12				
13				
14				
15				
16				
17				
18				
19				
20				

Instrucciones: Realiza la siguiente pregunta al encuestado:

“Fue el juego fácil de entender?”

Registra la respuesta aquí:____(1=Sí; 2 = No.)

Instrucciones: Si tú has jugado el juego de producción primero, ve a la siguiente página del juego de lotería. Si tú has jugado primero el juego de lotería, ve a la página 9, la sesión de pago.

6. Juego de Lotería

Instrucciones: Muestre la hoja con las bolsas de colores correspondientes. Lea lo siguiente al encuestado.

En este set de juego, se le presentara una tabla de 10 pares de loterías, opción A y opción B, donde se le preguntara que escoja la que prefiere.

Aquí hay un ejemplo de las opciones que se te dará (muestra la primera fila al encuestado). Si escoges la opción A, hay un 10% de probabilidad que tu recibirás S/. 6, y un 90% de probabilidad que tu recibas S/.4.8. Si escoges la opción B, hay un 10 % de probabilidad que tu recibirás S/. 11.55 lo cual es mucho más grande que S/.6 en la opción A. Sin embargo, también hay un 90% de probabilidad que tú puedas recibir solo S/. 0.3.

Si escoges la opción A, muévete a la siguiente fila. Para una vez que escojas la opción B.

Tu paga por este juego de lotería se determinara de la siguiente forma: un número al azar entre 1 a 10 se realizara para determinar el número de fila de una de tus elecciones. Después, de acuerdo a la elección que tú realices, una de la lotería A o B será extraída, y esto determinará tu pago.

El pago de esta ronda será entre el rango de S/. 0.3 y S/. 11.55."

<u>Instrucciones:</u> Ahora, ve línea por línea. Por cada línea, pregunta al sujeto si prefiere la lotería A o la lotería B. Recuerda: ana vez que el sujeto escoja B, el juego para - nosotros asumimos que escogen B por el resto de líneas. Tú simplemente necesitas registrar la línea en la que se dé el cambio de A a B.

	Opción A	**Opción B**	**Su elección** **(Marque una)**
1	1/10 de S/. 6.00, 9/10 de S/. 4.80	1/10 de S/. 11.55, 9/10 de S/. 0.30	A B
2	2/10 de S/. 6.00, 8/10 de S/. 4.80	2/10 de S/. 11.55, 8/10 de S/. 0.30	A B

3	3/10 de S/. 6.00, 7/10 de S/. 4.80	3/10 de S/. 11.55, 7/10 de S/. 0.30	A B
4	4/10 de S/. 6.00, 6/10 de S/. 4.80	4/10 de S/. 11.55, 6/10 de S/. 0.30	A B
5	5/10 de S/. 6.00, 5/10 de S/. 4.80	5/10 de S/. 11.55, 5/10 de S/. 0.30	A B
6	6/10 de S/. 6.00, 4/10 de S/. 4.80	6/10 de S/. 11.55, 4/10 de S/. 0.30	A B
7	7/10 de S/. 6.00, 3/10 de S/. 4.80	7/10 de S/. 11.55, 3/10 de S/. 0.30	A B
8	8/10 de S/. 6.00, 2/10 de S/. 4.80	8/10 de S/. 11.55, 2/10 de S/. 0.30	A B
9	9/10 de S/. 6.00, 1/10 de S/. 4.80	9/10 de S/. 11.55, 1/10 de S/. 0.30	A B
10	10/10 de S/. 6.00, 0/10 de S/. 4.80	10/10 de S/. 11.55, 0/10 de S/. 0.30	A B

6.1 En que línea el concursante empieza a escoger B en vez de A?

Instrucciones: Realiza la siguiente pregunta al encuestado:

“El juego fue fácil de entender?”

Registra la respuesta aquí:____(1=Sí; 2 = No.)

Instrucciones: Instrucciones: si has jugado el juego de producción primero, ve a la siguiente página e inicia la sesión de pago. Si has jugado primero el juego de lotería, ve a la página 4 donde encuentras el juego de producción.

Pago

Instrucciones: lea lo siguiente al encuestado: "Por participar en estos juego y contestar unas breves preguntas demográficas, usted ha ganado S/. 45 como cuota de participación. Ahora nosotros determinaremos su pago por el juego de producción y el juego de la Lotería."

Instrucciones: Haga que el participante tire el dado de 20 lados para determinar en cuál ronda del juego de producción será la ronda de pago.

7.1 Registre el tiro de dado del encuestado aquí: _____

Instrucciones: Mirando la cantidad producida por el encuestado y el precio generado en la ronda seleccionada aleatoriamente arriba, y del precio en las tablas, asegúrese de registrar correctamente la ganancia.

Después de haber confirmado la ganancia, lea al encuestado lo siguiente: "Por el juego de producción, su pago será de S/. 25 más la mitad de (indicar la pérdidas o ganancias en la ronda de paga)."

7.2 Registra el pago del juego de producción del encuestado aquí: ______

Instrucciones: Haga que el encuestado tire un dado de 10 lados para determinar en cuál de las 10 rondas de juego de lotería estará la ronda de pago.

7.3 Registra el tiro del dado del encuestado aquí: ____

Instrucciones: Mira la línea del juego de la lotería que corresponde al número que salió en el tirar el dado. Si el encuestado escogió A para esa línea, hágale jugar en la lotería A; si el encuestado escogió B para esa línea, hágale jugar en la lotería B. Recuerde al participante que el cero en el dado de 10 lados significa "10". Mira la tabla de producción por visualizar la ganancia por cada cantidad.

Después de determinar cuánto el encuestado ha ganado, léale lo siguiente: "Por el juego de la lotería, su pago será (indicar la ganancia o pérdida en el juego de la lotería)."

7.4 Registre el pago por el juego de la lotería del encuestado aquí: ____.

Instrucciones: Dígale al encuestado: "Sus ganancias de hoy día son S/. 45 por su participación más S/. X por su desempeño en el juego de producción más S/. Y por su desempeño en el juego de la lotería. Todo esto por un monto total de S/. ¡Felicitaciones, y gracias por su ayuda!"

7.5 Registre el pago final por todo el experimento del encuestado aquí: ______

Instrucciones: Haga que el encuestado firme el recibo.

FIN

Exhibit C4. Visual Aid for the Holt and Laury (2002) List Experiment

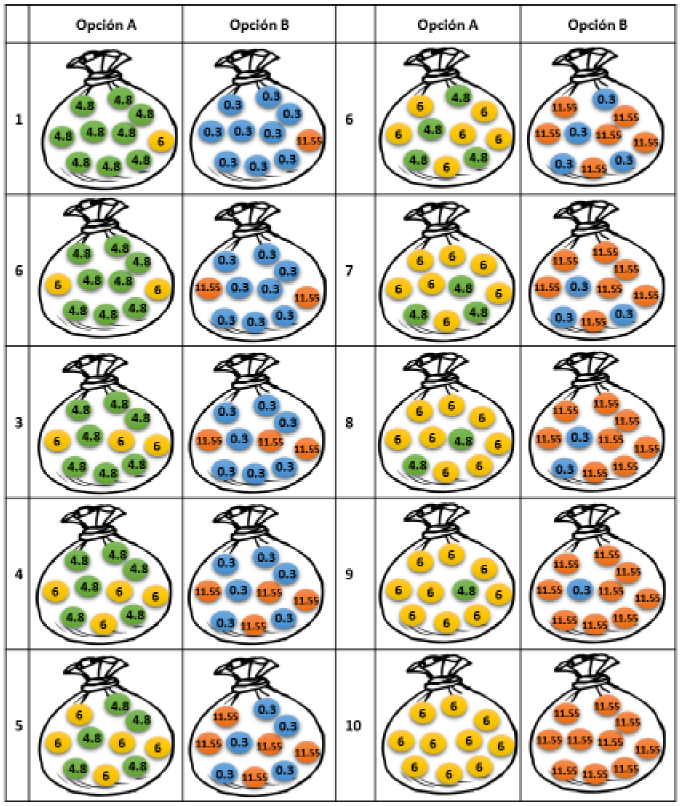


Exhibit C5. Visual Aid for the Price Risk Game I

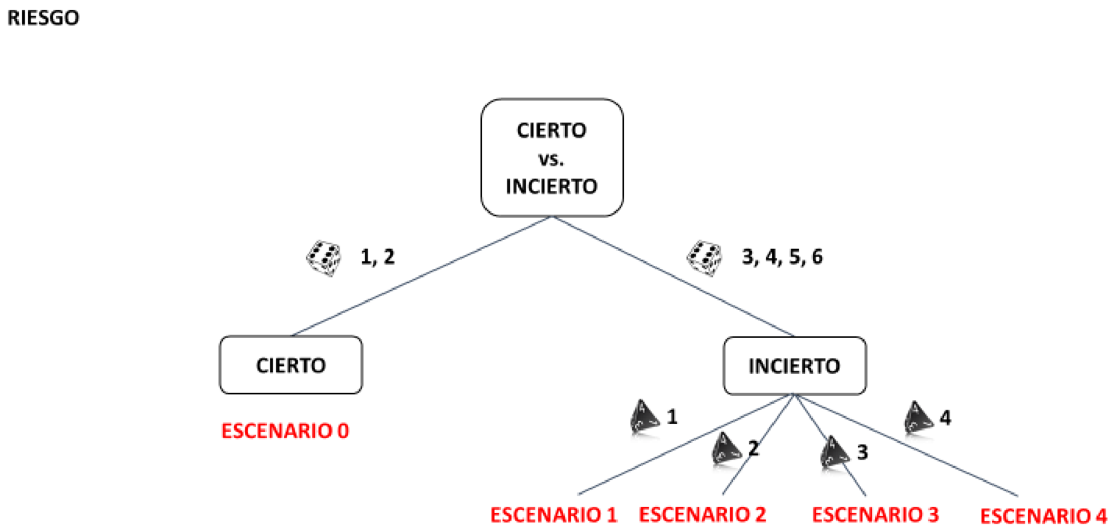
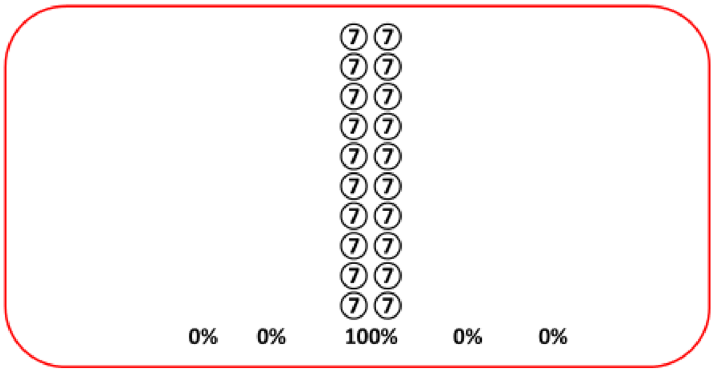


Exhibit C6. Visual Aids for the Price Risk Game II

Escenario 0

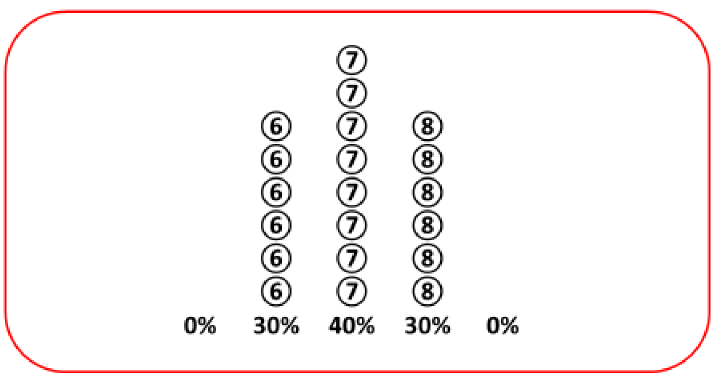
- Hay 20 bolas en la bolsa marcados con precios de S/. 5, S/. 6, S/. 7, S/. 8 y S/. 9. El número de bolas marcadas con cada precio se muestra en la siguiente imagen.



- Escriba su opción de entrada (0-20) en la hoja de respuestas .

Escenario 1

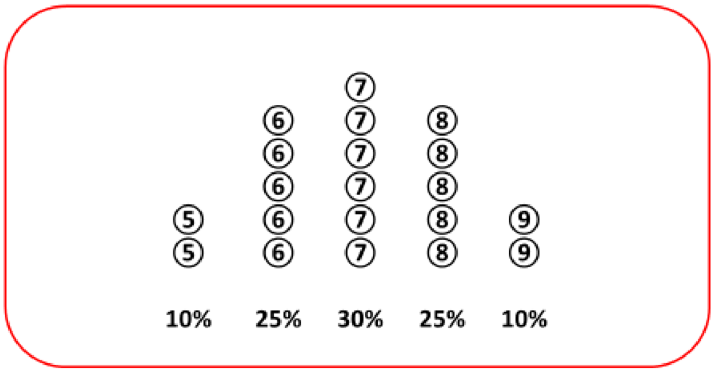
- Hay 20 bolas en la bolsa marcados con precios de S/. 5, S/. 6, S/. 7, S/. 8 y S/. 9. El número de bolas marcadas con cada precio se muestra en la siguiente imagen.



- Escriba su opción de entrada (0-20) en la hoja de respuestas .

Escenario 2

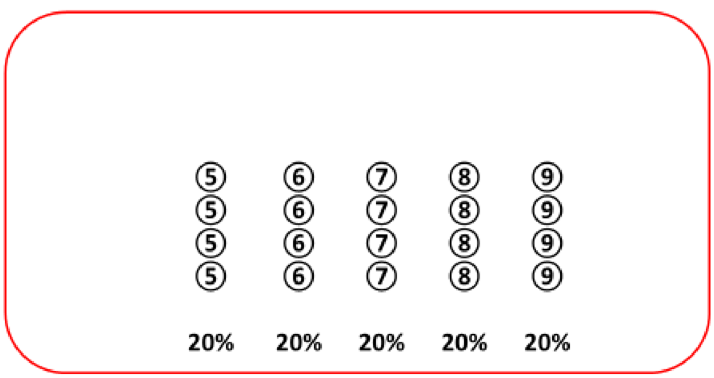
- Hay 20 bolas en la bolsa marcados con precios de S/. 5, S/. 6, S/. 7, S/. 8 y S/. 9. El número de bolas marcadas con cada precio se muestra en la siguiente imagen.



- Escriba su opción de entrada (0-20) en la hoja de respuestas .

Escenario 3

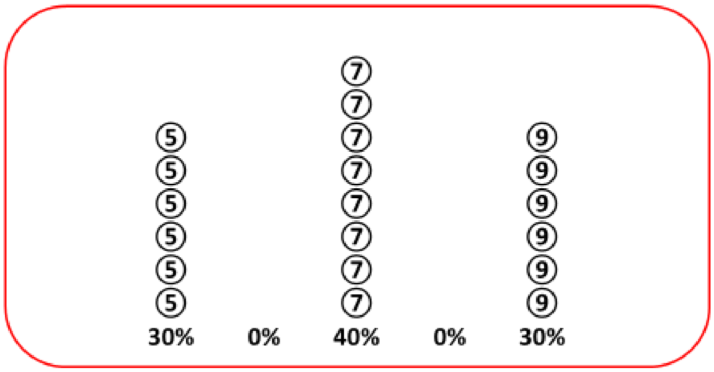
- Hay 20 bolas en la bolsa marcados con precios de S/. 5, S/. 6, S/. 7, S/. 8 y S/. 9. El número de bolas marcadas con cada precio se muestra en la siguiente imagen.



- Escriba su opción de entrada (0-20) en la hoja de respuestas .

Escenario 4

- Hay 20 bolas en la bolsa marcados con precios de S/. 5, S/. 6, S/. 7, S/. 8 y S/. 9. El número de bolas marcadas con cada precio se muestra en la siguiente imagen.



- Escriba su opción de entrada (0-20) en la hoja de respuestas .

Appendix D. Prospect Theory

One possible explanation for why the behavior of the average subject in our data is at odds with the predictions of the canonical model is that expected utility theory might not be well-suited to explain our subjects' behavior. In the remainder of this section, we explore whether prospect theory---a popular alternative to EU theory---might be better suited than EU theory to explain their behavior (Kahneman and Tversky, 1979).[1]

Recall that prospect theory differs from EU theory in three important ways. First, subjects have a reference point above which they act as if risk-averse and below which they act as if risk-loving. Second, subjects are loss-averse so that, in terms of welfare changes, a loss is felt more strongly than an equivalent gain. Finally, subjects tend to react more strongly to small-probability events.

Reader beware: Because we expected to find support for the canonical predictions of Sandmo (1971) when we embarked on this project, our experimental protocol was not designed to test prospect theory, and so what follows is not a formal test of prospect theory. We can nevertheless conduct a test of prospect theory *of sorts* with the data we have, one whose conclusions can be suggestive of support for prospect theory rather than being definitive about the same theory. The reader should keep in mind that some of the results that follow are not causal, and that by virtue of not being identified, they are merely suggestive that prospect theory might be a better explanation for our subjects' behavior.

Specifically, by assuming that our subject's reference point is the zero-profit point,[2] we can look at whether subjects expose themselves to less risk (i.e., act as if risk-averse) over gains, whether they expose themselves to more risk (i.e., act as if risk-loving) over losses, whether they are loss-averse by testing whether they react more strongly to expected losses than to expected gains, and we can look at whether they respond more strongly to small-probability events.

To do so, we define the following new variables, on which we regress quantity produced:

1. A variable $Pr(S)$, where S stands for "small," equal to $Pr(p = 5)$ and capturing the likelihood that the price drawn will be equal to 5. By symmetry of all price distributions in this paper, this is identical to $Pr(p = 9)$. We refer to this likelihood as ``tail risk,'' i.e., the probability of a small-probability event, which is randomly assigned within each round, and thus strictly exogenous.
2. A variable $Pr(O)$, where O stands for "other," equal to $Pr(p = 7 \lor p = 6)$ and capturing the likelihood that the price drawn will be equal to 6 or 7. By symmetry of all price distributions in

[1] In an earlier version of this article, we also looked at other alternative theories that might have explained our subjects' behavior. Looking at whether safety-first (Roy 1952, Telser 1955) or expected profit maximization (Lin et al., 1974) could explain our results, we found subjects for neither of those theories.
[2] This is only one of several candidate reference points. As Mattos and Zinn (2016) and Tonsor (2018) show, farmers tend to set their best experienced outcome as a reference point.

this paper, this is identical $Pr(p = 7 \vee p = 8)$. We refer to this likelihood as the probability of any other event. This is also randomly assigned within each round, and thus strictly exogenous.

3. A variable $g \geq 0$ equal to the expected gain if expected profit is positive and equal to zero otherwise, and
4. A variable $\ell \geq 0$ equal to the expected loss if expected profit is negative and equal to zero otherwise.

Note that by definition, the likelihood of a small-probability event and the likelihood of another event are not collinear. Similarly, by definition, g and ℓ are also not collinear.

We then estimate the following equation:

$$y_{it} = \alpha_3 + \sum_{k \in \{S,O\}} \pi_{3k} \Pr(k)_{it} + \kappa_3 g_{it} + \lambda_3 \ell_{it} + \tau_3 t_t + \upsilon_{3i} + \epsilon_{3it}, \quad \text{(D1)}$$

In all cases, $Pr(S) \leq Pr(O)$, so our test that subjects respond more sharply to small-probability events tests the null hypothesis that $\pi_{3S} = \pi_{3O}$. Under the assumption that a change in the probability of observing a tail event (i.e., a price of 5 or 9) should not have an effect different than a change in the probability of observing other events (i.e., a price of 6, 7, or 8), ceteris paribus, rejection of the null in favor of finding that $|\pi_{3S}| > |\pi_{3O}|$. would suggest that our subjects react more strongly to small-probability events (i.e., tail risk) than to more likely—and more central, from a distributional perspective—events.

Our test that subjects act as if risk-averse over gains consists in testing the hypothesis that $\kappa_3 = 0$. Because a decrease in quantity produced translates into a decrease in one's exposure to price risk, rejecting that null in favor of the alternative that the coefficient on expected gains is negative suggests that our subjects expose themselves to less risk in response to greater expected gains, i.e., it supports the hypothesis that subjects act as if risk-averse over gains.

Similarly, our test that subjects act as if risk-loving over losses consists in testing the null that $\lambda_3 = 0$. Because an increase in quantity produced translates into an increase in one's exposure to price risk, rejecting that null in favor of the alternative that the coefficient on expected losses is positive suggests that our subjects expose themselves to more risk in response to greater expected losses, i.e., it supports the hypothesis that subjects act as if risk-loving over losses.

Lastly, a test comparing the magnitude of the latter two coefficients by testing the null that $|\kappa_3| = |\lambda_3|$ allows testing for loss-aversion. A rejection in favor of the alternative hypothesis that $|\kappa_3| > |\lambda_3|$ would support the hypothesis that our subjects are loss-averse, as it would indicate that they respond more strongly to (expected) losses than to (expected) gains.

We reiterate that, because the data were not collected with the goal of testing prospect theory in mind, the results of these tests cannot be taken as definitive evidence in favor of or against prospect theory. One fruitful avenue for future research might be to use the experimental methodology developed by

Tanaka et al. (2010) to look at whether prospect theory does a better job of explaining behavior in this context than EU theory does, similarly to what Liu (2013) does to explain the adoption of *Bt* in China.

Table D1 below shows estimation results for equation D1. Again, the reader should keep in mind that the coefficients κ_3 and λ_3 are not causal, given that they depend on the value of the dependent variable, and so they are likely biased due to the statistical endogeneity problems that stems from reverse causality.

The results in table 5 show that our US subjects respond more strongly to small-probability events (i.e., tail risk) than they do to other events. Because those subjects react more strongly to tail risk (i.e., they adjust their production level more to tail risk than to more central risk), this supports the notion that they tend to overweight small-probability events. Peruvian subjects, for their part, seem to react more strongly to non-tail risk than to tail risk.

All subjects appear to expose themselves to more risk in response to expected losses, and thus they appear to act as if risk-loving over losses. Additionally, Peruvian subjects seem to expose themselves to less risk in response to expected gains, and thus they appear to act as if risk-averse over gains; US subjects appear to act as if risk-neutral over gains.

Comparing subjects' behavior in the face of expected gains with their behavior in the face of expected losses by looking at whether they respond more strongly to the latter in absolute value, we find that the average subject does respond more strongly to expected losses than to equivalent expected gains, but this is entirely driven by subjects at Cornell. This suggests that the average subject is loss-averse, a finding that is driven by the Cornell sub-samples.

At the risk of repeating ourselves, we wish to reiterate that our experimental setup was not designed to conduct a formal test of prospect theory, and so the findings just discussed are merely suggestive that prospect theory can explain our subjects' behavior. In the face of our data's lack of support for EU theory, we wanted to look at alternative explanations for behavior, one of which was prospect theory. We caution the reader against treating the results in this section as a formal test of prospect theory, but we also encourage future researchers to formally test whether prospect theory is an apt description of how people behave in the face of price risk.

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Table D1. Fixed Effects Regression Results for Prospect Theory, All Subjects.

Variables	Pooled	Cornell 1	Cornell 2	Minnesota	Peru
		Dependent Variable: Output Level			
Pr(Small-Probability Event)	-2.432**	-8.099***	-5.627***	-3.648**	2.870
	(1.206)	(2.730)	(1.955)	(1.670)	(1.802)
Pr(Other Event)	-0.244	-2.734*	-2.000**	-0.432	1.689**
	(0.519)	(1.344)	(0.954)	(0.792)	(0.698)
Expected Gain	-0.234	-0.004	0.313	-0.186	-0.419**
	(0.151)	(0.326)	(0.346)	(0.476)	(0.194)
Expected Loss	0.898***	1.548***	2.002***	1.471***	0.663**
	(0.237)	(0.374)	(0.278)	(0.392)	(0.253)
Round	-0.013	0.011	-0.000	-0.001	-0.041**
	(0.010)	(0.018)	(0.018)	(0.019)	(0.017)
Constant	11.715***	12.489***	10.600***	11.611***	11.221***
	(0.673)	(1.649)	(1.549)	(1.842)	(0.910)
Observations	2,339	480	480	459	920
p-value for Pr(Small) = Pr(Other)	0.005	0.002	0.006	0.007	0.376
p-value for |Expected Gain| = |Expected Loss|	0.053	0.035	0.000	0.147	0.528
R-squared	0.183	0.144	0.251	0.258	0.225

Standard errors clustered at the subject level in parentheses
*** p<0.01, ** p<0.05, * p<0.1