Experimenting with Measurement Error: Techniques with Applications to the Caltech Cohort Study*

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Abstract

Measurement error is ubiquitous in experimental work. It leads to imperfect statistical controls, attenuated estimated effects of elicited behaviors, and biased correlations between characteristics. We develop statistical techniques for handling experimental measurement error. These techniques are applied to data from the Caltech Cohort Study, which conducts repeated incentivized surveys of the Caltech student body. We replicate three classic experiments, demonstrating that results change substantially when measurement error is accounted for. Collectively, these results show that failing to properly account for measurement error may cause a field-wide bias leading scholars to identify "new" phenomena.

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1 Introduction

Measurement error is ubiquitous in experimental work. Lab elicitations of attitudes are subject to random variation in participants' attention and focus, as well as rounding due to finite choice menus. Moreover, there is an imperfect link between elicited proxies and the attitudes they intend to capture. Despite the ubiquity of measurement error, fewer than 10% of experimental papers published in the last decade in leading economics journals mention measurement error as a concern (see Section 1.2 for details). This is due, in large part, to the relatively crude tools for dealing with it—most commonly improved elicitation techniques and multiple rounds. Instead, we focus on developing statistical techniques.

At the heart of our approach is the combination of duplicate elicitations (usually two) of behavioral proxies and methods from the econometrics literature, particularly the instrumental variables approach to errors-in-variables (Reiersøl, 1941, 1945, 1950). While multiple elicitations would be impossible for a researcher using, say, the Current Population Survey, in experimental economics they are very easy to obtain.

The statistical tools we develop deal with three types of inference breakdowns that arise from different uses of experimental proxies measured with error: as controls, as causal variables, or to estimate correlations between latent preference characteristics. We demonstrate the potential perils of measurement error, and the effectiveness of our techniques, using a unique new data set tracking behavioral proxies of the entire Caltech undergraduate student body, the Caltech Cohort Study (CCS). We replicate within the CCS three classic and influential studies, and observe that 30–40% of variance in choices is attributable to measurement error. In all three of the experiments we have examined, accounting for measurement error substantially alters conclusions and implications.

First, we examine the most influential experimental study of the last decade, Niederle and Vesterlund (2007). That paper found that men are more likely to select into competition, due to a preference for competitive situations that is distinct from risk attitudes and overconfidence. We replicate, as many have before us, the fact that men choose to compete more

frequently than women. We show that the gender gap in competition is well explained by risk attitudes and overconfidence once measurement error is treated appropriately. Second, Friedman et al. (2014), summarizing their own research and that of many other scholars, find low correlations between different lab-based methods of measuring risk attitudes. As risk attitudes are fundamental to many economic theories, the failure to reliably measure them has troubling implications for lab experiments. In contrast, we find that many commonly used measures of risk attitudes are highly correlated once measurement error is taken into account. Third, we examine the relationship between attitudes towards ambiguous and compound lotteries, following the setup of Halevy (2007). Ambiguity aversion has been a rich field of theoretical exploration, and has been used to explain an array of behaviors, ranging from stock market investments to voting patterns. While Halevy finds a substantial correlation between attitudes towards compound risk and ambiguity, we find that accounting for measurement error leads to the conclusion that they are virtually identical.

Generally, measurement error biases estimates of effects and correlations towards zero. This attenuation bias is considered conservative, as it "goes against finding anything"—that is, it reduces the probability of false positives. However, as our results demonstrate, it may also lead to the over-identification of "new" effects and phenomena that are actually already documented.

1.1 Simulated Examples

Here we present simulated examples to illustrate, for the unfamiliar reader, the problems created by measurement error, and summarize our approaches. In our first example, a researcher is interested in estimating the effects of a variable D—say, gambling—on some outcome variable Y—say, participation in dangerous sports—using an experimentally measured variable X—say, elicited risk attitudes—as a control. The model that we use to simulate data is

$$Y^* = X^* \quad \text{with} \quad D = 0.5 \times X^* + \eta \quad \text{and} \quad X = X^* + \nu, \tag{1}$$

where $\eta \sim \mathcal{N}[0, 0.9]$ (so the variance of D is ≈ 1), $X^* \sim \mathcal{N}[0, 1]$, and $\nu \sim \mathcal{N}[0, \sigma_\nu^2]$. That is, risk attitudes drive both gambling and participation in dangerous sports, but that attitude is measured through a lab-based elicitation technique that contains error. We assume the researcher only has access to $Y = Y^* - \varepsilon$, a noisy measure of Y^*, where $\varepsilon \sim \mathcal{N}[0, 1]$.

A diligent researcher would fit a regression model of the form

$$Y = \alpha D + \beta X + \epsilon, \tag{2}$$

hoping to control for the role of risk attitudes in the effect of gambling on participation in dangerous sports. Table 1 shows, from simulations, how the estimates, $\hat{\alpha}$ and $\hat{\beta}$, depend on how much measurement error there is in the variance of X, that is $\frac{\sigma_\nu^2}{\sigma_\nu^2 + \sigma_{X^*}^2}$.

The estimated coefficients depend strongly on the amount of measurement error in X. With $N = 100$—a typical sample size for an experiment—the coefficient on gambling D becomes statistically significant in the average simulation when measurement error reaches approximately 1/3 of the variance in X. Intuitively, this occurs because the measurement error in X attenuates $\hat{\beta}$, allowing the $\hat{\alpha}$ to pick up the variation in D related to X^*. To put this in perspective, we estimate that measurement error accounts for 30–40% of the variance of elicited proxies for risk attitudes (see (4) and surrounding text).

Depressingly, adding more observations does nothing to reduce the bias in the estimated coefficients. In fact, when $N = 1{,}000$, the approximate size of the CCS, the coefficient on gambling $\hat{\alpha}$ appears statistically significant in the average simulation when measurement error accounts for only 10% of the variance in X. This emphasizes that issues with measurement error will not "wash out" once a study is large enough, and highlights the benefit of using the CCS to explore these issues.

Correcting this is simple—elicit more controls. With the moderate-size data sets used in experiments, five to ten elicitations will generally be sufficient, although if there are multiple behaviors one wishes to control for, or more categories of participants, this may entail the

Table 1: Simulated regressions of (2), with controls X measured with error. True model: $\alpha = 0$, $\beta = 1$.

Error as a percent of Var[X]:	0	10%	20%	30%	40%	50%
Panel A: $N = 100$						
$\hat{\alpha}$	0.00	0.06	0.11	0.16	0.21*	0.26***
	(0.11)	(0.11)	(0.12)	(0.12)	(0.12)	(0.12)
$\hat{\beta}$	1.00***	0.87***	0.75***	0.64***	0.54***	0.44***
	(0.12)	(0.11)	(0.11)	(0.10)	(0.10)	(0.09)
Percent of time $\alpha = 0$ rejected at the 5% level with:						
1 noisy measure of X^*	5%	8%	15%	25%	37%	50%
5 noisy measures of X^*	5%	6%	6%	7%	9%	11%
10 noisy measures of X^*	5%	5%	5%	5%	6%	7%
20 noisy measures of X^*	5%	5%	5%	5%	5%	6%
Panel B: $N = 1{,}000$						
$\hat{\alpha}$	0.00	0.06*	0.11***	0.16***	0.21***	0.26***
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
$\hat{\beta}$	1.00***	0.87***	0.75***	0.64***	0.54***	0.43***
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Percent of time $\alpha = 0$ rejected at the 5% level with:						
1 noisy measure of X^*	5%	31%	81%	98%	100%	100%
5 noisy measures of X^*	5%	6%	11%	23%	42%	66%
10 noisy measures of X^*	5%	5%	7%	10%	16%	28%
20 noisy measures of X^*	5%	5%	5%	6%	8%	11%

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with standard errors in parentheses. Coefficients and standard errors are averages from 10,000 simulated regressions.

loss of too many degrees of freedom to be practical. Thus, in Section 3, we show how principal component analysis allows for the use of a small, but informative, set of controls. These ideas are applied, in the same section, to show that the gender gap in competitiveness can be explained by risk attitudes and overconfidence, although Niederle and Vesterlund (2007) concluded that it was a disjoint phenomenon. Indeed the original paper included a

Table 2: Correlations with X and Y measured with error. True model: $\text{Corr}[X^*, Y^*] = 1$.

Error as a percent of Var[X] and Var[Y]:	0	10%	20%	30%	40%	50%
Panel A: $N = 100$						
$\widehat{\text{Corr}}[X, Y]$	1.00	0.90***	0.80***	0.70***	0.60***	0.50***
	(0.00)	(0.02)	(0.04)	(0.05)	(0.06)	(0.08)
$\widehat{\text{Corr}}[\mathbb{E}[X], \mathbb{E}[Y]]$	1.00	0.95***	0.89***	0.82***	0.75***	0.66***
	(0.00)	(0.01)	(0.02)	(0.03)	(0.04)	(0.06)
ORIV $\widehat{\text{Corr}}[X, Y]$	1.00	1.00	1.00	1.00	1.00	1.00
	(0.00)	(0.01)	(0.02)	(0.04)	(0.06)	(0.10)
Panel B: $N = 1{,}000$						
$\widehat{\text{Corr}}[X, Y]$	1.00	0.90***	0.80***	0.70***	0.60***	0.50***
	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
$\widehat{\text{Corr}}[\mathbb{E}[X], \mathbb{E}[Y]]$	1.00	0.95***	0.89***	0.82***	0.75***	0.66***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)
ORIV $\widehat{\text{Corr}}[X, Y]$	1.00	1.00	1.00	1.00	1.00	1.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.03)

Notes: ***, **, * denote statistically significantly different from 1 at the 1%, 5%, and 10% level, with standard errors in parentheses. Coefficients and standard errors are averages from 10,000 simulated regressions.

control cleverly designed to capture risk-aversion and overconfidence. However, that control exhibits the issue shown in Table 1: the coefficient on it is too small. A modification of their analysis that accounts for noise in the control brings their result in line with ours. That is, in both their data and ours, there is no statistically significant relationship between gender and competitiveness once risk-aversion and overconfidence are properly accounted for.

The problem of measurement error biasing coefficients is particularly acute when researchers estimate correlations between X and Y, as shown in the simulated results in Table 2. In this table, we simultaneously vary the proportion of measurement error in both variables. The results speak for themselves: even a bit of measurement error causes significant deviations from the proper correlation of one. As the scale of measurement error in our data is around 30–40%, it is extremely unlikely that one would ever estimate a correlation close

to one, even if that were the correct relationship.[1]

To correct for measurement error in correlations, we draw inspiration from instrumental variables. Our approach, which we call *Obviously Related Instrumental Variables* (ORIV) uses duplicate elicitations of X and Y as instruments. Specifically, we obtain duplicate measures of X, denoted X^a and X^b, which are both proxies for X^* that are measured with error. If measurement error in the two elicitations is orthogonal—as we assume—then the predicted values $\hat{X}^a(X^b)$ from a regression of X^a on X^b contain only information about X^*.[2] We then use a stacked regression to combine the information from both $\hat{X}^a(X^b)$ and $\hat{X}^b(X^a)$, resulting in an efficient use of the data. This technique is easily extended to allow for multiple measures of the outcome Y. This is particularly useful in estimating correlations, where there is no clear distinction between outcome and explanatory variables, and measurement error in either can attenuate estimates.

As demonstrated in Table 2, and proved in Appendix A, ORIV produces consistent coefficients, correlations, and standard errors. This is in contrast to one common way experimenters deal with multiple and potentially noisy elicitations: averaging the measures. As can be seen from the table, while averaging produces some reduction in inaccuracy, it still leads to incorrect conclusions in the presence of small amounts of measurement error.

We apply ORIV, in Section 4, to show that various risk elicitation methods are more correlated than previously thought, and that the patterns of correlations between them are indicative of phenomena outside the lab. We further use this technique to show, in Section 5, that ambiguity aversion and reaction to compound lotteries are very close to perfectly correlated—once we account for measurement error. This leads us to conclude, in Section 6, that failing to correct for measurement error has led the field to over-identify "new" phenomena.

[1]Note that the standard errors are smaller in Table 2, as Var[ε], set to 1 in all columns of Table 1, now varies across the columns: starting at 0 in the first column, and climbing up to 1 in the final column.

[2]If measurement error is not orthogonal, then instrumented coefficients will still be biased downwards, although less so than without instrumenting. In our experimental design, we tried to weaken any possible correlation by varying the choice parameters, the grid of possible responses, and so on. See Section 2.1 for details.

1.2 Related Literature

Mis-measurement of data has been an important concern for statisticians and econometricians since the late $19^{\underline{th}}$ century (Adcock, 1878). Indeed, estimating the relationship between two variables when both are measured with error is a foundational problem in the statistics literature (Frisch, 1934; Koopmans, 1939; Wald, 1940). The use of instrumental variables to address the classical errors-in-variables problem was proposed by Reiersøl (1941, 1945, 1950), with notable developments by Durbin (1954) and Sargan (1958) (see Hausman, 2001, for a review). These techniques were first applied to economic problems by Friedman (1957), who estimated consumption functions, and noted that annual income is a noisy measure of permanent income, which would attenuate estimates of the marginal propensity to consume from permanent income.[3] Since then, instrumental variables have been used to account for measurement error in an assortment of fields including medicine (Carroll and Stefanski, 1994), psychology (Fiske, Gilbert, and Lindzey, 2010), and epidemiology (Greenland, 2000).

The experimental literature has considered noise in lab data and its consequences, going back to at least Kahneman (1965). Nonetheless, in the decade from 2006–2015 only 9% of the 283 experimental (field and lab) papers in Economics' top 5 journals explicitly tried to deal with measurement error. One-fifth of these papers either used an experimental design aimed at reducing noise, or averaged multiple elicitations, a technique that may do little to reduce bias, as shown in Section 1.1. About one-half of these papers estimate structural models of participants' "mistakes." In particular, two-fifths estimate *Quantal Response Equilibrium* models. Other than the few exceptions described below, most of the remaining papers use indirect methods to deal with noise, such as the elimination of outliers, or the informal derivation of additional hypotheses about the effects of noise, which are then tested.[4]

[3]For a review of the history and applications of instrumental variables more generally, see Angrist and Krueger (2001).

[4]We examined full, refereed papers published in *The American Economic Review, Econometrica, Journal of Political Economy, The Quarterly Journal of Economics,* and *The Review of Economic Studies.* A research assistant found all experimental papers, then searched for a comprehensive list of keywords pertaining to measurement error.

There are very few instances in which measurement error played an explicit role in the analysis of experimental economics data. An early example, Battalio et al. (1973), shows that even small reporting errors can lead to a rejection of the generalized axiom of revealed preferences. In subsequent work, some scholars argued for a "theory of errors" under which observed violations of expected utility are an artifact of human error (Hey, 1991).[5]

Recent experimental papers have taken a renewed interest in the problems caused by measurement error. Quantal Response Equilibrium posits a structural model in which agents make mistakes that are inversely related to the payoff losses they generate (see McKelvey and Palfrey, 1995, 1998; and applications described in Goeree, Holt, and Palfrey, 2016). Using a related approach, Castillo, Jordan, and Petrie (2015), posit a structural model of measurement error, following Harless and Camerer (1994). They use several risk elicitation methods to study the effects of risk attitudes, accounting for measurement error, on disciplinary referrals of children. Coffman and Niehaus (2015) adjust for measurement error in self-interest and other-regard by projecting both on a common set of explanatory variables. Blattman et al. (2015), in a field setting, focus on gaining the trust of respondents in order to quantify the amount of measurement error in responses to sensitive questions.[6] [7] Our paper is, to our knowledge, the first to offer simple, yet general, experimental techniques for mitigating the effects of measurement error.[8]

[5]Drerup, Enke, and von Gaudecker (2016) do not correct for measurement error per-se, but rather observe that imprecise belief proxies may indicate behavioral rules that are less sensitive to beliefs. They test this hypothesis using data on stock market expectations and investment decisions.

[6]There is extensive literature dealing with measurement error in survey data (see, for example, Bertrand and Mullainathan, 2001; Bound, Brown, and Mathiowetz, 2001, and references therein). This literature acknowledges some of the issues we discuss, but offers limited techniques for overcoming them. An important exception is Beauchamp, Cesarini, and Johannesson (2015), who consider measurement error in survey-based risk elicitations. They use a latent variable model that allows them to make inferences about the component of measured risk attitudes that is not due to measurement error.

[7]Recently, some experimental studies have included instrumental variables to deal with endogeneity (Fong and Luttmer, 2011; Hart and Middleton, 2014), and measurement error (Ambuehl and Li, 2015). Weizsäcker (2010) considers errors in one explanatory variable in the context of social learning experiments. He treats participants in the same experimental condition as replicants of each other, splits the sample of participants in two, and uses one subset as an instrument for the other.

Ortoleva and Dean (2015) are the closest to the approach in our work. However, they estimate correlations using duplicates only on the right side, and without using them to properly estimate the variances of X^* and Y^*. Section 4 describes the issues with this approach.

[8]List, Shaikh, and Xu (2016) also propose general techniques for dealing with a particular issue in exper-

More often than not, however, experimental scholars limit their efforts at reducing the effects of measurement error through repetition of tasks across multiple rounds of an experiment.

2 The Caltech Cohort Study

Caltech is an independent, privately supported university located in Pasadena, California. It has around 900 undergraduate students, of which approximately 40% are women.

In the Fall of 2013, 2014, and Spring of 2015, we administered an incentivized, online survey to the entire undergraduate student body. We used incentivized tasks to elicit an array of attributes, including: risk aversion, ambiguity aversion, competitiveness, cognitive sophistication, implicit attitudes toward gender and race, generosity, honesty, overconfidence, overprecision, and optimism. Students were also asked a large set of questions addressing their lifestyle and social habits: sleep patterns, study routines, social networks, study networks, physical attributes, and so on.[9]

The data used in this paper comes from the Fall 2014 and Spring 2015 installments. In the Fall of 2014, 92% of the entire student body (893/972) responded to the survey. Of those, 39% were female (349/893), and the average payment was $24.34. In the Spring of 2015, 91% of the entire student body (819/899) responded to the survey. Of those, 39% were female (322/819), and the average payment was $29.08. The difference in average payments across years was due to the inclusion of several additional incentivized items in 2015.[10] Of those who had taken the survey in 2015, 96% (786/819) also took the survey in 2014. As Section 4 requires data from both surveys, for consistency we use this subsample of 786 throughout.[11]

iments, in their case multiple hypothesis testing in experimental work.

[9] For screenshots of the 2015 survey, go to: people.hss.caltech.edu/~lyariv/ScreenshotsSpring2015.pdf.

[10] The number of overall students was substantially lower in the Spring of 2015, as about 50 students departed the institute due to hardship or early graduation. Further, we did not approach students who had spent more than four years at Caltech, accounting for approximately 25 students.

[11] In the Fall of 2013 88% of the student body (806/916) responded to the survey, of which 38.5% (310/806) were female. The average payment was $20.58. Of those who took the survey in 2013 and did not graduate, 89% (546/615) also took the survey in the Fall of 2014.

There are several advantages to using the CCS to address questions of measurement error. The large size of the study allows us to document the non-existence of certain previously identified "distinct" behaviors with unusual precision. Furthermore, the inflation of standard errors that comes with using instrumental variables techniques does not threaten the validity of our inferences. Last, unlike most experimental settings, there is little concern about self-selection into our experiments from the participant population, due to our 90%+ response rates (Cleave, Nikiforakis, and Slonim, 2013; Falk, Meier, and Zehnder, 2013; Harrison, Lau, and Rutström, 2009). Thus, the issues we identify are due solely to measurement error, and not due to a small sample or self-selection.

Nonetheless, Caltech is highly selective, which may cause one to worry that the overall population is different from the pool used in most lab experiments. Three points should mitigate this concern. First, the raw results of the replications yield virtually identical to those reported in the original papers.[12] Second, responses from our survey to several standard elicitations—of risk, altruism in the dictator game, etc.—are similar to those reported in several other pools (see Appendix D for details). Third, while top-10 schools account for 0.32% of the college age population in the U.S., top-50 schools enroll only 3.77% of that population (using the *U.S. News and World Report* rankings). Thus, there seems to be little cause for concern that our participant pool is more "special" than that used in many other lab experiments. As the results reported in this paper are replications of other studies, these points suggest that our conclusions are likely due to our more sophisticated treatment of measurement error, rather than an artifact of the participant population.

2.1 Measures Used

Our results deal with a subset of the measured attributes, which we detail here. Question wordings can be found in Appendix E. Throughout, 100 survey tokens were valued at $1.

[12]This should increase confidence in the original studies that we replicate, as it implies that it is not participants' self-selection into the lab that is driving the results in those studies.

2.1.1 Overconfidence

We break overconfidence into three categories, following Moore and Healy (2008). These measures are used in Section 3 as controls.

Overestimation and Overplacement: Participants complete two tasks: a five-question cognitive reflection test (CRT; see Frederick, 2005), and five Raven's matrices (Raven, 1936). Participants are given a maximum of 20 seconds per CRT item, and 30 seconds per Raven's matrix. After each block of five questions, each participant is asked how many they think they answered correctly. This, minus the participant's true performance, gives a measure of overestimation. Each participant is also asked where they think they are in the performance distribution of all participants. This, minus the participant's true percentile, gives a measure of overplacement. This gives three co-linear measures: performance, expected performance, and overconfidence; two of which can be used to control for confidence and overconfidence.

Overprecision: Participants are shown a random picture of a jar of jellybeans, and asked to guess how many jellybeans the jar contains. They are then asked—on a six point qualitative scale from "Not confident at all" to "Certain"—how confident they are of their guess. This is repeated three times. Following Ortoleva and Snowberg (2015), each of these measures is interpreted as a measure of overprecision.

Perception of Academic Performance: A final measure of overconfidence asks participants to state where in the grade distribution of their entering cohort they believe they would fall over the next year. This is treated as a measure of confidence in placement.

2.1.2 Risk

Risk measures are used in Section 3 as controls, and in Section 4 as an outcome of interest. Further, the Risk MPL described below is used as an outcome of interest in Section 5.[13]

[13]For an overview of risk elicitation techniques, see Charness, Gneezy, and Imas (2013).

Projects: Following Gneezy and Potters (1997), participants are asked to allocate 200 tokens between a safe option (keeping them), and a project that returns some multiple of the tokens with probability p, otherwise returning nothing. In Fall 2014, two projects were used: the first returning 3 tokens per token invested where $p = 40\%$ of the time, and the second returning 2.5 tokens 50% of the time. In the Spring of 2015, the first project was modified to return 3 tokens 35% of the time.

Qualitative: Following Dohmen et al. (2011), participants are asked to rate themselves, on a scale of 0–10, in terms of their willingness to take risks. As this question was only asked once in the Fall of 2014, the elicitation from the Spring of 2015 is used as a duplicate measure in Section 4.

Lottery Menu: Following Eckel and Grossman (2002), participants are asked to choose between six 50/50 lotteries with different stakes.[14] The first lottery contained the same payoff in each state, and thus corresponded to a sure amount. The remaining lotteries contain increasing means and variances, allowing for an estimation of risk aversion.

Risk MPL: Participants respond to two Multiple Price Lists (MPLs) that ask them to choose between a lottery over a draw from an urn, and sure amounts. The lottery would pay off if a ball of the color of the participant's choosing was drawn. The first urn contained 20 balls—10 black and 10 red—and paid 100 tokens. The second contained 30 balls—15 black and 15 red—and paid 150 tokens. Taking the first MPL as an example, participants are first asked to choose the color (red or black) that they want to pay off, if drawn. They are then presented with a list of choices between a certainty equivalent that increases in units of 10 tokens from 0 to 100 or the gamble on the urn.[15]

[14]The variant we use comes from Dave et al. (2010).

[15]In order to prevent multiple crossovers, the online form automatically selected the lottery over a 0 token certainty equivalent, and 100 tokens over the lottery. Additionally, participants needed only to make one choice and all other rows were automatically filled in to be consistent with that choice.

2.1.3 Ambiguity and Compound Lotteries

Reactions to ambiguous and compound lotteries are considered in Section 5.

Compound MPL: This follows the same protocol as the Risk MPLs described above, except participants are told that the number of red balls would be uniformly drawn between 0 and 20 for the first urn, and between 0 and 30 for the second. As this is a measure of risk attitudes, it is also used as a control in Section 4.

Ambiguous MPL: This elicitation emulates the standard Ellsberg (1961) urn. It follows the same protocol as the two other MPLs. Participants were informed that the composition of the urn was chosen by the Dean of Undergraduate Students at Caltech.

To reduce instructions, both of the MPLs for a given attitude (Risk, Compound, Ambiguity) are run sequentially, in random order. These three blocks are spread across the survey, and which block is given first, second, and third is randomly determined. As no order effects were observed, we aggregate results across the different possible orderings.

3 Controls Measured with Error

To make the claim that an estimated effect is independent of other factors, many studies attempt to control for those other factors. If those other factors are measured with error, one control, or even a few, may be insufficient to reliably assert the claim, as illustrated in Section 1.1. That section also showed that more controls can ameliorate this issue. Here we illustrate that properly dealing with controls measured with error has important substantive consequences. We do so by replicating the competitiveness and gender study of Niederle and Vesterlund (2007)—henceforth NV—within the CCS. Like NV, we find a robust difference in the rates at which men and women compete. However, NV conclude that:

> Finally, controlling for gender differences in general factors such as overconfidence, risk, and feedback aversion, we estimate the size of the residual gender difference in the tournament-entry decision. Including these controls, gender differences are still significant and large. Hence, we conclude that, in addition to gender differences in overconfidence, a sizeable part of the gender difference in tournament entry is explained by men and women having different preferences for performing in a competitive environment.

In contrast, we show that the gender gap is well explained by risk aversion and overconfidence.

Using the notation of Section 1.1, measurement error in X, in this case controls for risk aversion and overconfidence, can result in a biased estimate of the coefficient on D, in this case gender, on competition Y. To understand this intuitively, consider the model in (1) where $Y^* = X^*$, D and X^* are correlated, and $X = X^* + \nu$ is a noisy measure of X^*. For illustration, consider an extreme case where the variance of ν is very large, so that X is almost entirely noise. If a researcher ignores that noise in X, standard regression analysis could then lead to the erroneous conclusion that Y and D are correlated, even when controlling for X.[16]

To put this in terms of our substantive example, it is well known that overconfidence is correlated with gender (see, for example, Moore and Healy, 2007, 2008), and, depending on the elicitation method, risk aversion may be correlated with gender as well (see Holt and Laury, 2014, for a survey, and our discussion in Section 4.6). Thus, if competitiveness is driven by overconfidence or risk aversion, mis-measurement of these traits will lead to an overestimate of the effect of gender.

What can be done to mitigate this issue? The simplest approach, which we take, is to include multiple measures for each of the possible controls X.

3.1 Measuring Competitiveness in the Caltech Cohort Study

Part of the Spring 2015 survey mimicked the essential elements of NV's design. Participants first had three minutes to complete as many sums of five two-digit numbers as they could.

[16] It is well known that measurement error in left-side variables may bias estimated coefficients in discrete choice models, see Hausman (2001). Thus, as Y may also be measured with error, we use linear probability models to avoid bias.

The participants were informed that they would be randomly grouped with three others at the end of the survey. If they completed the most sums in that group of four, they would receive 40 experimental tokens (or $0.40) for each sum correctly solved, and would otherwise receive no payment for the task. Ties were broken randomly. As in NV, at the end of this task, participants were asked to guess their rank, from 1 to 4, within the group of four participants. They were paid 50 tokens (or $0.50) if their guess was correct.

Next, in the central task of NV's design, participants were told they would have an additional three minutes to complete sums. However, before doing so, they chose whether to be paid according to a piece-rate scheme or a tournament. The piece-rate scheme paid 10 tokens for each correctly solved sum. The tournament had a similar payment scheme to the first three-minute task. The difference was that the participant's performance in the tournament would be compared to the performance of three randomly chosen participants in the *first* task. This ensured that the participant would not need to be concerned about the motivation, or other characteristics, that might drive someone to compete in the second task. Otherwise, the payment structure was identical to that in the first task.

There are a few ways in which our implementation differs from NV's:

Time and Payments: We gave participants three, rather than five, minutes to complete sums. Per-sum payments were scaled down by a factor of four. As with the rest of the CCS, participants were paid for all tasks, rather than a randomly selected one. This could have caused participants to hedge by choosing the piece-rate scheme.

Grouping of Participants: In NV, paticipants were assigned to groups of four where they could visibly see that there were two men and two women. This created an imbalance in the expected number of female competitors: for men this was 2/3 of the group, versus 1/3 for women. In the CCS, we randomly selected groups after the survey was administered, and created different groups for the first and second task. Thus, both genders faced the same expected profile of competitors.

Experimental Setting: Our elicitation was done on a survey, whereas NV used a lab setting. Administering our survey several months later in a lab environment to 98 Caltech students produced very similar results, but with larger standard errors driven by the smaller sample.

Additional Tasks: NV include two additional parts: a preliminary task allowing participants to try out the piece-rate scheme, and a final choice that allows participants to select either an additional piece-rate or tournament payment scheme for their performance in the preliminary task. This final choice served as a control for risk aversion and overconfidence. As the CCS has multiple other controls for both of these traits (see Section 2.1), we omitted these two parts to reduce the complexity and time taken to elicit competitiveness.

The first three of these factors would only change interpretation if they affected men and women differently. However, as we replicate the gender gap in tournament entry found by NV, these do not seem to be of particular importance. The final factor implies that we cannot use an analogous control to that of NV's with the CCS data. Nonetheless, using NV's data and an analysis accounting for measurement error in their control, we find that the gender gap in tournament entry can be explained by risk aversion and overconfidence (see Section 3.3). More details about our, and NV's, implementation can be found in Appendix E.1.

3.2 Gender, Competition, and Controls

This subsection analyzes the extent to which risk aversion and overconfidence drive the gender gap in competitiveness. Table 3 summarizes specifications meant to illustrate different points. These are linear probability models, and hence, the coefficient on gender is directly interpretable as the percentage-point gap between men and women in choosing to compete.[17]

[17]As noted in Footnote 16, discrete choice models may produce biased estimates of coefficients when the left-side variable is measured with error. Nonetheless, in our data, Probit and Logit specifications produce almost identical levels of statistical significance as in Table 3.

The first column shows the baseline difference in competition: Women choose tournament incentives 21.4% of the time, while men choose them 40.4% of the time, for a difference of 19.0 percentage points. This difference is highly statistically significant. While these numbers are somewhat lower than those reported in NV, their relative sizes are quite similar. The second column controls for participants' estimates of their own rank, as well as their performance, linearly, as in NV's main specification. Similar to their results, the inclusion of these controls reduces the coefficient on gender by approximately 1/3.

There is, however, a non-linear relationship between expected rank and perceived probability of victory in a competition.[18] Therefore, the third column enters participants' subjective ranks non-parametrically, by including a dummy variable for each possible response (three categories). This estimation confirms that the effect of perceived rank in a competition is, indeed, non-linear, although the coefficient on gender remains unchanged.[19] The third column also enters performance non-linearly (29 and 26 categories, respectively), as there is also a non-linear relationship between performance and competition. The coefficient on gender in the third column is lower than in the second.[20]

The fourth column begins introducing additional controls for risk aversion and overconfidence. In this column, two (non-randomly) selected controls are entered, one for each attribute. As can be seen, this does not affect the coefficient on gender, despite the fact that both controls have statistically significant coefficients. The fifth column contains a different two (non-randomly) selected controls; this cuts the coefficient on gender by more than half, and renders it statistically insignificant. Taken together, these columns show that the statistical significance of controls is not a good indicator of whether or not a trait is

[18] If an individual believes a random participant is inferior to her with probability p, then her probability of winning is p^3. Furthermore, her expected rank is given by $\sum_{i=0}^{3}(i+1)\binom{3}{i}(1-p)^i p^{3-i} = 3(1-p)+1$. We can therefore back out the probability p from any reported rank r (ignoring rounding). With a reported rank of r, the probability of winning the competition is $\left(\frac{4-r}{3}\right)^3$.

[19] Rates of competition are 65.6% for participants who predicted they would come in first (in a random group of 4), and 31.4%, 15.3%, and 5.0% for participants predicting they would come in second, third, and fourth (last), respectively. The distribution of guessed ranks differs from that reported in NV: our participants were better calibrated, and this likely resulted in the lower observed rates of tournament entry.

[20] This is entirely driven by including performance in the first task non-parametrically, as there are small differences in male and female performance in this task, as shown in Figure 4 of Appendix E.1.

Table 3: Gender, competition, and controls

Dependent Variable	Chose to Compete ($N = 783$)							
Male	0.19***	0.13***	0.11***	0.11***	0.048	0.050	0.041	0.0020
	(.034)	(.031)	(.031)	(.031)	(.031)	(.034)	(.033)	(.054)
Guessed Tournament Rank		-0.15***	$F = 29$	$F = 28$	$F = 23$	$F = 21$		$\chi^2_3 = 6.9$
		(.017)	$p = 0.00$	$p = 0.00$	$p = 0.00$	$p = 0.00$		$p = 0.08$
Tournament Performance		0.086***	$F = 1.6$	$F = 1.6$	$F = 1.6$	$F = 1.5$		$\chi^2_{30} = 30$
		(.020)	$p = 0.02$	$p = 0.02$	$p = 0.02$	$p = 0.05$		$p = 0.47$
Performance Difference		-0.021	$F = 1.4$	$F = 1.5$	$F = 1.4$	$F = 1.4$		$\chi^2_{25} = 24$
		(.017)	$p = 0.09$	$p = 0.07$	$p = 0.11$	$p = 0.11$		$p = 0.53$
Risk Aversion: MPL #1				0.042***				
				(.015)				
Overplacement: CRT				0.026*				
				(.015)				
Risk Aversion: Project #2					0.067***			
					(.016)			
Perceived Performance (pctile.): CRT					-0.042***			
					(.016)			
All Risk Aversion Controls						$F = 3.6$		
						$p = 0.02$		
All Overconfidence Controls						$F = 1.7$		
						$p = 0.05$		
First 5 Principal Components							$F = 34$	
							$p = 0.00$	
Instrumental Variables (IV)								$\chi^2_8 = 20$
								$p = 0.01$
Adjusted R^2	0.038	0.23	0.26	0.27	0.28	0.29	0.20	

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with standard errors in parentheses. Coefficients and standard errors on all non-dichotomous measures are standardized. F-statistics and p-values are presented when variables are entered categorically rather than linearly. There are 3 categories for Guessed Competition Rank, 29 categories for Tournament Performance, 26 categories for Performance Difference, 6 variables for Risk Aversion Controls, and 12 variables for overconfidence controls. $N = 783$.

fully controlled for. Moreover, it suggests that measurement error in the controls themselves allows for the perception of competitiveness as a separate trait.

It is worth noting that these conclusions are not driven by our unusually large sample size. If anything, the size of our dataset helps reduce standard errors and identify weak effects that, with a smaller dataset, would appear insignificant. To see this, we draw a random sample of 40 women and 40 men (the size and gender composition of NV's experiment) from our data 10,000 times, and regress the competition decision on two overconfidence controls, two risk controls, and perceived rank in the first competition task. The coefficient on gender is significant at the 1% level 2.2% of the time, at the 5% level 7.6% of the time, and at the 10% level 13% of the time.

The sixth column enters all available controls for risk (6 controls) and overconfidence (an additional 12 controls). The coefficient on gender is relatively unchanged, which masks the fact that additional controls first cause the coefficient to fall, and then rise, although never by much. If we enter these controls separately, we find that much of the decrease in this coefficient, as compared to the third column, is due to the controls for risk aversion. We revisit the relationship between gender and risk aversion in Section 4.6.

The number of controls in the sixth column (76, including categorical controls for performance) approaches the number of data points in a normally sized study—such as NV, which had 80 participants. Thus, we examine ways to preserve degrees of freedom. The simplest is to perform a principal components analysis of all 76 of the controls. The first few principal components will contain most of the information in those controls—in this case entering just 5 of them produces a very similar point estimate to entering all 76 controls. More on this technique can be found in Appendix B.1.

As discussed in Section 1.1, the potential bias in the coefficient on gender comes from the fact that the coefficients on the noisy controls are biased towards zero. Thus, in the final column, we instrument the risk aversion and overconfidence controls for which we have multiple elicitations. While the point estimate of the coefficient on gender is consistent (and

statistically insignificant), it is also accompanied by the higher standard errors that come with an IV specification. We exploit these multiple elicitations, and IV strategies, more fully in Sections 4 and 5. However, we first turn to another avenue for achieving the correct coefficient on controls that can be applied to those designed with a specific purpose in mind.

3.3 Using Designed Controls

NV control for risk aversion and overconfidence with another tournament entry choice. Namely, in the last stage of their experiment, participants are given a second opportunity to be paid for their performance in the piece-rate task from the beginning of the experiment. They can choose to be paid again as a piece rate, or to enter their performance into a tournament. The clever idea behind this additional choice is that it controls for all aspects determining tournament entry that are not directly related to a preference for competing—explicitly, risk aversion and overconfidence. We show that a specification that accounts for measurement error in this control, generates very different conclusions than those of NV, using their data. For compactness, we refer to the choice in the main task as Y_i^a, and the choice in final task as Y_i^b.

NV regress Y_i^a on gender, performance, guessed tournament rank, and Y_i^b, as in the third Column of Table 4. This reduces the coefficient on gender compared to their main specifications, which are displayed in the first two columns.[21] However, if Y_i^b is measured with error, this will bias the coefficient downwards, and the coefficient on gender upwards, exactly as shown in the simulations of Table 1 in the Introduction.

As Y_i^b is designed to measure every part of the tournament entry choice *except* a preference for competition, it should enter the regression with a coefficient of 1. This can be implemented by regressing $Y_i^a - Y_i^b$ on gender, which will produce an unbiased estimate of the effect of gender on tournament entry, controlling for Y_i^b. Intuitively, the only difference

[21] We thank Muriel Niederle and Lise Vesterlund for generously sharing their data. The coefficients in Table 4 differ due to our use of OLS—which is preferred to Probit for reasons described above—although p-values are very similar.

Table 4: Re-Analysis of Niederle and Vesterlund's Data

Dependent Variable:	Choose To Compete (Y_i^a)			$Y_i^a - Y_i^b$	
Male	0.37***	0.27***	0.21**	0.075	0.053
	(.11)	(.11)	(.10)	(.12)	(.12)
Tournament Performance	0.016	-0.003	-0.012***		-0.037
	(.019)	(.019)	(.018)		(.023)
Performance Difference	0.016	-0.005	0.012		0.056**
	(.023)	(.023)	(.023)		(.027)
Guessed Tournament Rank		-0.24***	-0.20***		-0.11
		(.066)	(.066)		(.080)
Y_i^b			0.27**		
			(.11)		
Adjusted R^2	0.13	0.24	0.29	0.00	0.054

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with standard errors in parentheses. Coefficients and standard errors on all non-dichotomous measures are standardized. $N = 80$ for all regressions. $N = 80$.

between these two variables is, by construction, a desire to compete. To see if this desire is correlated with gender, it should be regressed on gender. Doing so results in an insignificant coefficient on gender of 0.075. The inclusion of additional controls on the right side reduces the coefficient even further.[22] Thus, had NV used a specification that accounts for measurement error in Y_i^b, they would have come to the same conclusion: risk aversion and overconfidence explain the gender gap in tournament entry.

3.4 Substantive Interpretation

Our analysis shows, using both new data, and data from NV, that although men are more likely to select into competition, this is not due to a distinct *preference* for performing in a competitive environment. Rather, it is driven by differences in risk aversion and overconfidence. It is important to note that using multiple controls for risk aversion and overconfi-

[22]The inclusion of additional controls should make the test more efficient in small samples, see Appendix B.2 for a formal exposition of this, and other points, in this sub-section.

dence, or using principal components, does not allow us to say how important either of these factors is in explaining competition, only that together they explain much of the effect.[23]

Our results do not, by any means, imply that it is better to elicit risk attitudes and overconfidence instead of competitiveness. There is a tradeoff: competition is potentially more directly relevant for an array of economically important decisions, and is definitely a more parsimonious measure. Indeed, competition has been shown to explain several interesting behaviors, such as choice of college major (see, for example, Buser, Niederle, and Oosterbeek, 2014). However, risk aversion and overconfidence feature in many theories, and are therefore of potential use in bringing theory to bear on gender differences.

There are also practical considerations. NV report that their experiment had an average runtime of approximately 45 minutes. By using two tasks (rather than four) and allowing participants to solve sums for three minutes (rather than five), we reduced the average time participants spent on the competition task to around 8 minutes. Naturally, eliciting multiple measures of risk and overconfidence may be time consuming as well. Nevertheless, our entire survey had an average runtime of less than 30 minutes, including the competition task.

4 Measurement Error Left and Right

We now shift to a situation where the variables of interest, rather than controls, are measured with error. This attenuates the estimated relationship between different variables. We introduce a simple method, *Obviously Related Instrumental Variables* (ORIV), to correct for this. We apply this technique to the estimation of the correlation between different measures of risk attitudes in this section, and between risk and ambiguity aversion in the next.

It is well known that measurement error in outcome, or dependent, variables does not bias estimated relationships, although it increases standard errors. Measurement error in explana-

[23]Recent work by van Veldhuizen (2016) uses clever experimental design to add refined versions of NV's final task, and specifications following the previous subsection, to estimate the effect of risk-aversion, overconfidence, and a taste for competition on tournament entry. He arrives at a similar conclusion to ours: the gender gap in tournament entry is entirely driven by differences in risk aversion and overconfidence.

tory, or dependent, variables is a much more serious problem, biasing estimated coefficients towards zero and distorting standard errors. This leads to an improper understanding of the relationship between explanatory variables and outcomes. These problems are compounded when estimating a correlation: the distinction between outcome and explanatory variables is blurred, and measurement error in either biases estimates towards zero.

The following subsections develop the ORIV approach gradually. The first subsection introduces the substantive question of investigating correlations between different risk measures. The next gives a simple treatment of the standard application of instrumental variables to correct for measurement error in explanatory variables. The following three subsections show, theoretically and empirically, how to combine information from multiple instruments, and how to consistently estimate correlation coefficients when both explanatory and outcome variables are measured with error. The discussion in this section focuses on implementation, with the formal properties of the estimators developed in Appendix A.

4.1 Risk Elicitation Techniques

There is a substantial experimental literature assessing the validity of common experimental techniques for eliciting attitudes towards risk and uncertainty (see the literature review in Holt and Laury, 2014). These studies often elicit risk attitudes in the same set of participants using different techniques. By using a within-participant design, researchers attempt to understand technique-driven differences in elicited proxies for risk aversion. This type of work has generally found small correlations between different techniques, making it difficult to study the individual correlates of risk preferences. To mention a few examples, Dave et al. (2010) compare the Lottery Menu task with the Holt and Laury (2002) task—in which participants choose one lottery in each of a sequence of lottery pairs, where means and variances change from pair to pair. Participants appear to be more risk averse in the Holt and Laury task. Deck et al. (2008) compare behavior in the Holt and Laury task to that in a task that was a variation on the game show "Deal or No Deal," and report a correlation

between risk attitudes from the two tasks at only 0.008, with a p-value of 0.94. Deck et al. (2010) compare the same two tasks, adding two others (including the Lottery Menu task used here), as well as survey questions touching upon risk in six different domains. The highest pairwise correlations they find are lower than 0.3. Similarly, Anderson and Mellor (2009) compare responses to the Holt and Laury task to survey questions about hypothetical gambles. They find small correlations, and provide a survey of the literature with similar results.

Ultimately, the literature concludes that risk elicitation is a "risky business"—the pun is not ours, see Friedman et al. (2014) for a survey. Indeed, those authors conclude that:

> Estimated parameters exhibit remarkably little stability outside the context in which they are fitted. Their power to predict out-of-sample is in the poor-to-nonexistent range, and we have seen no convincing victories over naive alternatives.

However, none of the studies on which this conclusion is based account for measurement error when estimating correlations between elicitation techniques. In what follows, we inspect several commonly used risk-attitude elicitation techniques, and estimate their within-participant correlations using an IV strategy to account for measurement error. This generates much higher within-participant correlations than previously reported. Moreover, the corrected correlations suggest that elicitation techniques fall into one of two sets: those that elicit certainty equivalents for lotteries, and those that elicit allocations of assets between safe and risky options. The latter category exhibits greater corrected correlations with other measures, and more stability over time. Further, elicitations based on allocation decisions display substantial gender effects—which are consistent with investment behavior in the field—while certainty equivalent elicitations do not.

This section uses four measures of risk as described in Section 2.1: Qualitative, Risk MPL, Project, and Lottery Menu. Before we proceed, we note a few details about how we handle the data from those elicitations to standardize estimated quantities for easy comparison. First, when using two measures from the same form of elicitation, we put these on a common

scale. In particular, the certainty equivalents from the 30-ball urn Risk MPL (which go up to 150) are divided by 1.5 to be on the same scale as the certainty equivalents from the 20-ball urn Risk MPL (which go up to 100).[24] Second, when comparing objects like estimated CRRA coefficients or derived certainty equivalents, these are also put on the same scale. For example, when examining the relationship between certainty equivalents from the Risk MPLs and Projects—the former allowing for risk-loving answers and the latter not—those who gave risk-loving answers on the urns are re-coded to give a risk-neutral answer. Without this censoring, results are qualitatively similar.[25]

4.2 A First Take on Measurement Error Correction

It is well known that measurement error attenuates estimated coefficients (see, for example, Greene, 2011). Here we review that basic finding to set up a framework for our estimator.

To estimate the relationship between two variables measured with independent i.i.d. error, $Y = Y^* + \nu_Y$ and $X = X^* + \nu_X$ (with $\mathbb{E}[\nu_Y \nu_X] = 0$ and $\text{Var}[\nu_k] = \sigma^2_{\nu_k}$), the ideal regression model would be $Y^* = \alpha^* + \beta^* X^* + \varepsilon^*$. Instead, we can only estimate $Y = \alpha + \beta X + \varepsilon$, where α is a constant and ε is a mean-zero random noise. Annotating finite-sample estimates with hats and population moments without hats, this results in an estimated relationship of

$$\begin{aligned} \hat{\beta} &= \frac{\widehat{\text{Cov}}[Y,X]}{\widehat{\text{Var}}[X]} = \frac{\widehat{\text{Cov}}[\alpha + \beta^* X^* + \varepsilon + \nu_Y, X^* + \nu_X]}{\widehat{\text{Var}}[X^* + \nu_X]} \\ \mathbb{E}\Big[\hat{\beta}\Big] &= \operatorname*{plim}_{n\to\infty} \hat{\beta} = \beta^* \frac{\sigma^2_{X^*}}{\sigma^2_{X^*} + \sigma^2_{\nu_X}} < \beta^*. \end{aligned} \tag{3}$$

The estimated coefficient $\hat{\beta}$ is thus biased towards zero. Importantly, the bias in (3) depends on the amount of information about the true explanatory variable X^* in X.

In a lab experiment, it is relatively easy to elicit two replicated measures of the same underlying parameter X^*. That is, suppose we have $X^a = X^* + \nu^a_X$ and $X^b = X^* + \nu^b_X$,

[24] This implicitly assumes a CRRA utility function.

[25] This censoring affects 22% of the responses in the 20-ball urn, and 32% of responses in the 30-ball urn.

with ν_X^a, ν_X^b i.i.d. random variables, and $\mathbb{E}\left[\nu_X^a \nu_X^b\right] = 0$—that is, measurement errors are independent of each other, and thus uncorrelated. With the additional assumption that $\text{Var}[\nu_X^a] = \text{Var}[\nu_X^b] \equiv \text{Var}[\nu_X]$, we have that

$$\widehat{\text{Corr}}[X^a, X^b] \to_p \text{Corr}[X^a, X^b] = \frac{\sigma_{X^*}^2}{\sigma_{X^*}^2 + \sigma_{\nu_X}^2}, \tag{4}$$

which allows us to ballpark the degree of bias in estimated coefficients. The modal correlation between two elicitations of the same risk measure is approximately 0.6, suggesting that the variance of measurement error is of the order of 2/3 of the variance of X^*.[26]

Using instrumental variables (IV), the second noisy measure of X^* can be used to recover a consistent estimate of the true coefficient β^*. Recalling from the derivation of (3) that $\widehat{\text{Cov}}[X^a, X^b] = \widehat{\text{Var}}[X^*]$, we use two-stage-least-squares (2SLS) to instrument X^a with X^b

$$X^a = \pi_0 + \pi_1 X^b + \varepsilon_X \quad \Rightarrow \quad \hat{\pi}_1 = \frac{\widehat{\text{Cov}}[X^a, X^b]}{\widehat{\text{Var}}[X^b]} = \frac{\widehat{\text{Var}}[X^*]}{\widehat{\text{Var}}[X^b]}, \tag{5}$$

and then condition on this instrumented relationship to estimate $Y = \alpha + \beta(\hat{\pi}_0 + \hat{\pi}_1 X^b) + \varepsilon_Y$. This second stage regression provides

$$\hat{\beta} = \frac{\widehat{\text{Cov}}[\alpha^* + \beta^* X^* + \varepsilon^* + \nu_Y, \hat{\pi}_0 + \hat{\pi}_1 X^b]}{\widehat{\text{Var}}[\hat{\pi}_0 + \hat{\pi}_1 X^b]} = \frac{\beta^* \hat{\pi}_1 \widehat{\text{Var}}[X^*]}{\hat{\pi}_1^2 \widehat{\text{Var}}[X^b]} \to_p \beta^*,$$

a consistent estimate of β^*, the true relationship between Y^* and X^*.

[26] This correlation also provides a way to derive a correction factor for the attenuation bias in the regression estimates from (3) dating back to Spearman (1904). Defining the "disattenuated" estimator of β as $\tilde{\beta} = \frac{\hat{\beta}}{\widehat{\text{Corr}}[X^a, X^b]}$ and invoking the continuous mapping theorem, it is clear that $\tilde{\beta}$ provides a consistent estimator for β. This approach, while consistent, may be inefficient in the presence of multiple replicates. As illustrated in Appendix A, our ORIV approach provides a simple formulation for consolidating the information from multiple replicates of both X and Y.

Table 5: Correlation between different risk measures is understated due to measurement error.

Dependent Variable	Qualitative Assessment				Lottery Menu			
Project #1	0.31***				0.24***			
	(.034)				(.034)			
Project #2		0.29***				0.29***		
		(.034)				(.034)		
Project #1 (Instrumented)			0.55***				0.59***	
			(.069)				(.073)	
Project #2 (Instrumented)				0.58***				0.50***
				(.067)				(.070)
Risk MPL #1	0.15***				0.19***			
	(.036)				(.035)			
Risk MPL #2		0.17***				0.23***		
		(.035)				(.035)		
Risk MPL #1 (Instrumented)			0.22***				0.44***	
			(.048)				(.067)	
Risk MPL #2 (Instrumented)				0.21***				0.37***
				(.047)				(.067)

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with standard errors in parentheses. Coefficients are from regressions where both the right and left-side variables are standardized, and thus are correlations. $N = 776$.

4.3 Two Instrumentation Strategies

Multiple measures for X^* admit multiple instrumentation strategies that will only produce the same estimate with infinite data. The ORIV estimator consolidates the information from these different formulations of the problem. In our working example, we have two equally valid elicitations and two possible instrumentation strategies: one may instrument X^a with X^b, or X^b with X^a. In this subsection, we illustrate the divergent results these two strategies may produce. The next subsections show how to deal with this issue by combining these sources of information into a single estimated relationship.

Table 5 shows estimated relationships between different elicitation techniques. These are first estimated using a standard regression, and then the two different IV strategies discussed above. The coefficients are from regressions where both the left- and right-side variables are standardized, which removes scale effects and provides for easy comparison.

Although different instrumentation strategies may produce similar results—as in the third and fourth columns of Table 5—they can also produce different results—as in the seventh and eighth columns. Moreover, given that estimated standard deviations, which are inflated by measurement error, are used to standardize the variables in Table 5, neither strategy is likely to produce an accurate result. The next subsection deals with both of these issues.

4.4 Obviously Related Instrumental Variables

We construct ORIV estimates and corrected correlations in three steps. First, we consider the case where only explanatory variables are measured with error. We then extend the analysis to the case where both the outcome and explanatory variables are measured with error. Finally, we show how to derive consistent correlations from the consistent and asymptotically efficient ORIV estimates of the regression coefficient β. Throughout, we focus on designs in which there are at most two replications for each measure. This is done for simplicity, and because it fits precisely the implementation carried out using the Caltech Cohort Study.[27]

[27]Appendix A extends the ORIV estimator to settings where more than one replicate is available.

4.4.1 Errors in Explanatory Variables

We continue with the model stated in Section 4.2, noting that unlike the analysis in Section 4.3, these measures are not standardized. The ORIV regression estimates a stacked model to consolidate the information from the two available instrumentation strategies:

$$\begin{pmatrix} Y \\ Y \end{pmatrix} = \begin{pmatrix} \alpha_1 \\ \alpha_2 \end{pmatrix} + \beta \begin{pmatrix} X^a \\ X^b \end{pmatrix} + \varepsilon, \quad \text{instrumenting } \begin{pmatrix} X^a \\ X^b \end{pmatrix} \text{ with } W = \begin{pmatrix} X^b & 0_N \\ 0_N & X^a \end{pmatrix}, \tag{6}$$

where N is the number of participants, and 0_N is an $N \times N$ matrix of zeroes. To implement this, one need only to create a stacked dataset and run a 2SLS regression. This can be thought of as estimating a first stage, as in (5), for both instrumentation strategies, and then estimating

$$\begin{pmatrix} Y \\ Y \end{pmatrix} = \begin{pmatrix} \alpha_1 \\ \alpha_2 \end{pmatrix} + \beta \begin{pmatrix} \hat{X}^a \\ \hat{X}^b \end{pmatrix} + \varepsilon, \tag{7}$$

where $\hat{X}^a$ and $\hat{X}^b$ are the predicted values derived from the two first-stage regressions. Sample Stata code illustrating this estimation procedure appears in Appendix C.2.[28]

With a single replication, the stacked regression will produce an estimate of β^* that is the average of the estimates from the two instrumentation approaches in prior subsections. Intuitively, with no theoretical reason to favor one estimate or the other, it is equally likely that the smaller is too small as it is that the larger is too large.[29] The estimator splits the difference, leading to a consistent estimate of β^*, the true relationship between Y^* and X^*.

Proposition 1. *ORIV produces consistent estimates of* β^*.

This technique uses each individual twice, which results in standard errors that are too small, as the regression appears to have twice as much data as it really does. Many practi-

[28] Note that if one estimates 2SLS in stages, the estimated standard errors from the second stage, in (7), would be incorrect, as they do not take into account the fact that the $\hat{X}^a$ and $\hat{X}^b$ are estimated. Therefore, it is preferable to estimate (6) directly, using a statistical package's 2SLS command, as this will give correct asymptotic standard errors.

[29] Over-identification can be addressed using generalized method of moments (GMM). Indeed, ORIV is asymptotically equivalent to equal-weighted GMM, but is simpler to implement, and easier to extend to include other nuances, such as sample weights on individuals or optimal weighting of different measures, using standard statistical software. See Appendix A.6.

tioners understand intuitively the idea that one should use clustered standard errors to treat multiple observations as having the same source.[30]

Proposition 2. *The ORIV estimator satisfies asymptotic normality under standard conditions. The estimated standard errors, when clustered by participant, are consistent estimates of the asymptotic standard errors.*

Simulations suggest that block-bootstrapped standard errors are, if anything, slightly smaller, implying that asymptotic standard errors are slightly conservative.

4.4.2 Errors in Outcome and Explanatory Variables

When estimating the relationship between two elicited variables, there is no reason to believe that one is measured with error (X), but the other is not (Y). The existence of measurement error in Y does not change Propositions 1 and 2, although estimated standard errors will, of course, increase, reflecting the degree of uncertainty in the estimated coefficient. Still, if one has access to two estimates of Y there is no reason not to use them, as they will increase efficiency. Moreover, when estimating correlations, where neither variable can be said to be the explanatory or outcome variable, measurement error in either will bias the estimated correlation towards zero.

To incorporate two measures of Y^* with measurement error ($Y^a = Y^* + \nu_Y^a,\ Y^b = Y^* + \nu_Y^b$,

[30] Mathematically, clustering is needed as $\text{Cov}[\varepsilon_i, \varepsilon_{N+i}] = \text{Var}[\varepsilon_i^*]$ for $i \in \{1, 2, 3, \ldots, N\}$. This implies that the variance-covariance matrix of residuals is given by

$$\begin{pmatrix} (\text{Var}[\varepsilon^*] + \beta^2 \text{Var}[\nu^a]) I_N & \text{Var}[\varepsilon^*] I_N \\ \text{Var}[\varepsilon^*] I_N & (\text{Var}[\varepsilon^*] + \beta^2 \text{Var}[\nu^b]) I_N \end{pmatrix}, \text{ where } I_N \text{ is an } N \times N \text{ identity matrix.}$$

Clustering takes care of the common ε_i^* for participant i on- and off-diagonal.

As the diagonal terms differ in whether $\text{Var}[\nu^a]$ or $\text{Var}[\nu^b]$ remains, this suggests that a different weighting of X^a and X^b is optimal. This could be implemented using Feasible-Generalized Least Squares (FGLS) for our ORIV estimators. However, FGLS tends to have poor small sample properties, and would likely produce worse estimates in small to moderate-sized experimental datasets. In our dataset, which is an order of magnitude larger than most, FGLS has no effect. For more detail, see Appendix A.

$\mathbb{E}[\nu_Y^a] = \mathbb{E}\left[\nu_Y^b\right] = 0$) in the ORIV estimation procedure, one would simply estimate

$$\begin{pmatrix} Y^a \\ Y^a \\ Y^b \\ Y^b \end{pmatrix} = \begin{pmatrix} \alpha_1 \\ \alpha_2 \\ \alpha_3 \\ \alpha_4 \end{pmatrix} + \beta \begin{pmatrix} X^a \\ X^b \\ X^a \\ X^b \end{pmatrix} + \varepsilon \text{ with instruments } W = \begin{pmatrix} X^b & 0_N & 0_N & 0_N \\ 0_N & X^a & 0_N & 0_N \\ 0_N & 0_N & X^b & 0_N \\ 0_N & 0_N & 0_N & X^a \end{pmatrix}.$$

4.4.3 Estimating Correlations from Consistent Coefficients

ORIV produces $\hat{\beta}^*$, a consistent estimate of β^*. Notice that

$$\hat{\beta} = \frac{\widehat{\text{Cov}}[X,Y]}{\widehat{\text{Var}}[X]} \text{ implying } \hat{\rho}_{XY} = \hat{\beta}\sqrt{\frac{\widehat{\text{Var}}[X]}{\widehat{\text{Var}}[Y]}},$$

where ρ_{XY} is the correlation. Thus, we need consistent estimates of $\text{Var}[X^*]$ and $\text{Var}[Y^*]$ to recover $\hat{\rho}^*_{XY}$. The problem is that $\text{Var}[X] = \text{Var}[X^*] + \text{Var}[\nu_X]$. As $\text{Var}[Y]$ is biased as well, it is not clear if transforming the regression coefficient into a correlation will generate an estimate that is biased up or down (although overall, the correlation will be biased towards zero by measurement error). Nonetheless, we have

$$\text{Cov}[X^a, X^b] = \text{Cov}[X^* + \nu_X^a, X^* + \nu_X^b] = \text{Var}[X^*] \text{ so } \hat{\rho}^*_{XY} = \hat{\beta}^*\sqrt{\frac{\widehat{\text{Cov}}[X^a, X^b]}{\widehat{\text{Cov}}[Y^a, Y^b]}}.$$

To determine the proper asymptotic standard errors, simply multiply those estimated from the ORIV procedure by $\sqrt{\widehat{\text{Cov}}[X^a, X^b]/\widehat{\text{Cov}}[Y^a, Y^b]}$. An example of how to estimate correlations using ORIV in STATA can be found in Appendix C.3.

Proposition 3. *$\hat{\rho}^*_{XY}$ is consistent. Standard errors estimated from ORIV, multiplied by $\sqrt{\widehat{Cov}[X^a, X^b]/\widehat{Cov}[Y^a, Y^b]}$, are consistent.*

Table 6: Correlation matrices before and after accounting for measurement error

In Units Given by the Questions

	Raw Correlations			Corrected for Measurement Error		
	Project	Qualitative	Lottery Menu	Project	Qualitative	Lottery Menu
Qualitative	0.26*** (.030)			0.41*** (.046)		
Lottery Menu	0.47*** (.030)	0.25*** (.033)		0.71*** (.047)	0.40*** (.054)	
Risk MPL	0.19*** (.031)	0.13*** (.033)	0.22*** (.029)	0.30*** (.053)	0.19*** (.048)	0.38*** (.060)

Measured in CRRA coefficients

	Raw Correlations			Corrected for Measurement Error		
	Project	Qualitative	Lottery Menu	Project	Qualitative	Lottery Menu
Qualitative	0.21*** (.043)			0.36*** (.052)		
Lottery Menu	0.27*** (.057)	0.24*** (.035)		0.55*** (.071)	0.38*** (.057)	
Risk MPL	0.18*** (.041)	0.069** (.033)	0.22*** (.038)	0.37*** (.084)	0.10** (.048)	0.42*** (.076)

Measured in certainty equivalent of a 50/50 lottery over 0/100 tokens

	Raw Correlations			Corrected for Measurement Error		
	Project	Qualitative	Lottery Menu	Project	Qualitative	Lottery Menu
Qualitative	0.25*** (.029)			0.44*** (.053)		
Lottery Menu	0.38*** (.032)	0.23*** (.031)		0.73*** (.086)	0.36*** (.051)	
Risk MPL	0.24*** (.044)	0.13*** (.033)	0.20*** (.025)	0.43*** (.080)	0.19*** (.048)	0.34*** (.052)

Notes: ***, **, * denote statistical significance at the 1%, 5% and 10% level, with standard errors in parentheses. $N = 776$.

4.5 Corrected Correlations between Risk Elicitation Techniques

We now use ORIV estimators to examine the correlations between different risk measures. Table 6 contains both the raw and corrected correlations. Both the Risk MPL task and the Project task were elicited twice. The qualitative risk measure was elicited only once in the Fall of 2014, but again in the Spring of 2015, which serves as its second measure.[31]

Previous work comparing different risk-elicitation techniques often transforms them into a common scale (see Deck et al., 2010, for an example). For comparability, we do the same in Table 6. This is not theoretically advisable as it introduces a non-linear change in the structure of measurement error, which may lead to inconsistent estimates. However, for the question at hand, this makes little difference in the results.

In the top panel, we consider correlations between the unaltered measures. That is, we use units given by the elicitation techniques. The second panel translates these various measures into CRRA coefficients, except for the qualitative assessment, which does not lend itself to transformation. The third panel uses the imputed CRRA coefficients to calculate the implied certainty equivalent with a 50% probability of 100 tokens and a 50% probability of 0 tokens. Note that in the case of the Risk MPLs, this is the same as the questions' natural units, as these are elicitations of certainty equivalents over 50/50 lotteries.

All three panels suggest similar conclusions. First, the corrected correlations are substantially higher. While the raw correlations are arguably low, never exceeding 0.5 (and uniformly below 0.27 when considering imputed CRRA coefficients), corrected correlations are dramatically higher, reaching levels as high as 0.73. Whether this correlation is "high" or "low" is largely a judgement call. However, the literature seems to consistently suggest that correlations above 0.7 are very high (see, for example, Cohen, 1988; Evans, 1996). Moreover, many perceived strong links correspond to correlations that are 0.7 or below. For exam-

[31] For correlations involving the Lottery Menu task, we multiply by $\sqrt{\widehat{\text{Var}}[((X^a)' \ (X^b)')']/\widehat{\text{Var}}[Y]}$. This is valid so long as the measurement error in the measures of X and Y are equal—that is, so long as $\text{Corr}[X^a, X^b] = \text{Corr}[Y^a, Y^b]$, as shown in (4). Having only one measure of Y, we cannot test this. However, the correlation between measures for the three that we do have are 0.67 (s.e. 0.026, projects), 0.62 (s.e. 0.028, qualitative), and 0.58 (s.e. 0.29, Risk MPLs), so this seems reasonable.

ple, the correlation between parents' and their childrens' heights hovers around 0.5 (Wright and Cheetham, 1999), the correlation between height and foot length for individuals over the age of 30 is about 0.6 for females and 0.7 for males (Pawar and Dadhich, 2012), the correlation between average parents' education and their children's education ranges from around 0.30 in Denmark and 0.54 in Italy, with most western countries falling somewhere in-between (Hertz et al., 2007), and the correlation between Body Mass Index (BMI) and insulin resistance—which underlies the link between obesity and type-2 diabetes—is around 0.46 (Abbasi et al., 2002). Second, some measures are noticeably more correlated. Namely, the Project measure appears to be most correlated with the other elicitation techniques. It is most highly correlated with the Lottery Menu measure, with corrected correlations of 0.55–0.73, depending on the units of measurement. The Lottery Menu also exhibits relatively high correlations with the other measures. The lowest correlations are observed between the Risk MPL and the Qualitative measure.

4.6 Substantive Implications

There are good reasons to suspect that the Risk MPL measure captures risk attitudes over a different domain than the other measures: its smaller correlation with other measures, and the fact that, unlike other risk measures, it is uncorrelated with gender, as shown in Figure 1. In that figure, the average risk attitudes of men and women are very close when considering the Risk MPLs, but all other measures show that women are substantially more risk averse than men.[32] These differences account for the explanatory power of risk controls on the gender gap in competitiveness in Section 3.2.

The fact that different measures yield different conclusions about the relationship between gender and risk attitudes is reflected in the behavioral literature, which reaches mixed conclusions about the general relationship between gender and risk (see, for example, Byrnes,

[32]In addition to the usual concerns with the Kolmogorov-Smirnov test, it is not valid for discrete distributions. In such cases, the p-value is only approximate, and may lead to extreme implications, such as the p-values of 1.000 found in Figure 2. As such, we provide p-values only to aid visual inspection.

Miller, and Schafer, 1999, for a review of the relevant experimental work in psychology, and Croson and Gneezy, 2009; Eckel and Grossman, 2008b; and Niederle, 2015, for reviews of related experimental work in economics).[33] On the other hand, the finance literature has found a more consistent difference between men and women when considering risky financial investments (see, for example, Barber and Odean, 2013; Embrey and Fox, 1997; Farrell, 2011). The Project measure intentionally mimics a stock/bond portfolio choice (or risky/safe assets), and the gender-based behavior in this task is similar to that seen in real financial investments: men invest more aggressively than women.[34]

There is also variation in the consistency of responses to the different risk elicitation techniques across time. The Project task, Risk MPL, and Qualitative assessment were all elicited in both the Fall 2014 and Spring 2015 installments of the survey. The risk attitudes elicited by the Project task exhibit more stability than the Risk MPLs—a correlation of 0.65 (0.044) for the Project measure(s), compared with 0.42 (0.063) for the Risk MPL (both corrected for measurement error). The Qualitative elicitation was performed only once per survey, and the uncorrected correlation between these elicitations was 0.62.

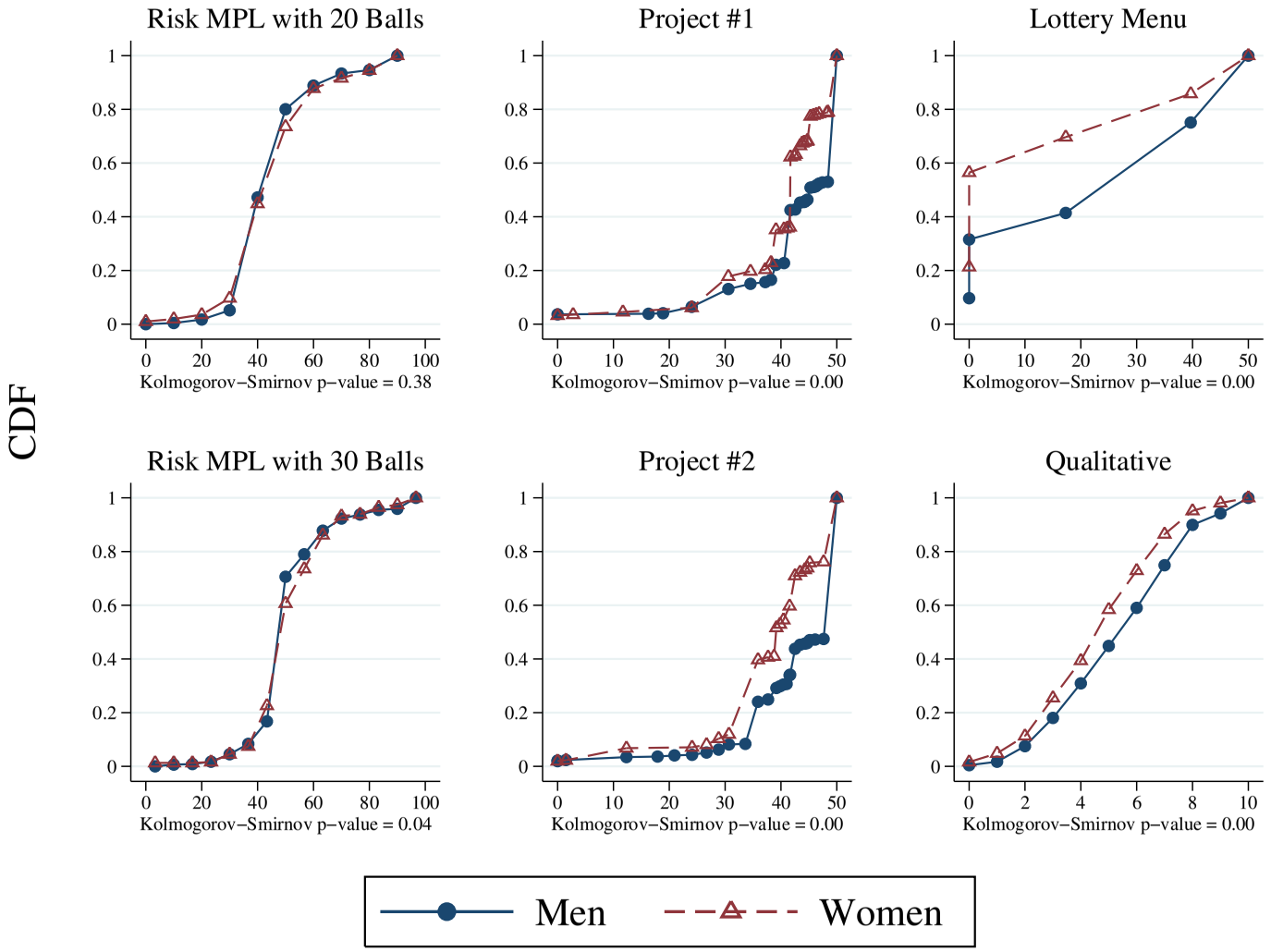
Taken together, these different measures may be representative of risk attitudes in different settings. This would not be surprising, as psychologists have found that risk attitudes do differ across contexts (Slovic, 1964; see Kruger, Wang, and Wilke, 2007; Weber, Blais, and Betz, 2002; and references therein for more recent work). While there are many criteria on which one might evaluate such measures, the Project-based measure seems particularly attractive due to its stability, correlation with other popular measures, and the fact that the literal interpretation of the measure is consistent with evidence from the field, in finance.[35]

[33] Our findings are consistent with some observations in Holt and Laury (2014).

[34] The lotteries in the Lottery Menu task can be viewed as corresponding to different investment allocations between a safe option and a risky one that pays three times the amount invested with 50% probability, see Eckel and Grossman (2002). Eckel and Grossman (2008a) observed that results are not sensitive to whether or not lotteries are described as risky investments to participants, which is consistent with the Lottery Menu task exhibiting similar patterns to the Project measure.

[35] Recent evidence suggests the presence of a global risk component that explains individuals' choices across investment domains. This component exhibits some of the features of the Project measure (Einav et al., 2012).

Figure 1: Risk aversion differs by gender, except when elicited using Risk MPLs.



Notes: All panels except for the qualitative assessment are put on the same scale: a certainty equivalent of a 50/50 lottery paying 0/100 tokens, via a CRRA utility function.

The next section uses the ORIV technique to examine risk in a particular domain—compound lotteries—and how it relates to ambiguous (uncertain) lotteries.

5 New Traits

Measurement error may lead researchers to believe that an observed behavior is not well explained by current theory. We have already shown one example of this in Section 3: using controls that are measured with error may lead researchers to believe a behavior is independent of other behaviors. It is natural to think that bias in correlations, explored in the last section, may similarly cause researchers to underestimate the relationship between two variables, and thus declare them distinct when they are, in fact, not. In this section we provide a potential example of this phenomenon by examining attitudes towards ambiguity.

Ambiguity aversion refers to a preference for known risks over unknown risks. First introduced by Ellsberg (1961), this preference implies that an ambiguity averse individual would prefer a lottery with a known probability distribution of rewards over a similar lottery in which the probability distribution of rewards is unknown. This behavior is expressed in the *Ellsberg Paradox*, where participants prefer a bet on the draw of a black ball from an urn with, say, 10 red and 10 black balls than on one with 20 total balls, but with unknown composition. Ambiguity aversion is widely studied, and used to explain incomplete contracts, volatility in stock markets, selective abstention in elections, and so on (Mukerji, 2000).

Segal (1987, 1990) suggests that choices under ambiguity may come from improperly compounding a sequence of lotteries. For instance, in the Ellsberg Paradox scenario above, a participant might view a draw from the ambiguous urn as having two stages: First, the number of red balls is randomly determined, according to some subjective probability; second, a ball is drawn from the urn. If an individual fails to properly reduce these two lotteries into one, a bias will result. Halevy (2007) experimentally tests this proposition. In his study, participants face both an Ellsberg urn, and an urn where the number of red balls is uniformly

determined. In his results, Halevy reports correlations of around 0.5 between behaviors in both treatments. Nonetheless, his results suggest that half the variation in the responses to ambiguous and compound lotteries is independent. This implies a strong, but imperfect, link between ambiguity aversion and (negative) reactions to compound lotteries.

In this section, we replicate Halevy's exercise, adding duplicate measures of certainty equivalents of both ambiguous and compound lotteries. This allows us to correct for measurement error using ORIV. As a result, ambiguity aversion and reaction to compound lotteries appear almost identical.

5.1 Ambiguity Aversion and Reaction to Compound Lotteries

As described in Section 3.1, the Risk MPL, Compound MPL, and Ambiguous MPL are all implemented in a very similar way. All ask for a participant's certainty equivalent value of a draw from an urn if a certain color ball is drawn. All allow the participant to select the color of the ball associated with positive payment. All have the same number of balls, and the same payoff. The only difference is how the distribution of balls in the urn is specified: half black and half red for Risk; drawn from a uniform distribution for Compound; or unknown, but selected by the Dean of Undergraduate Students for Ambiguous. Each measure is replicated twice: once with a 20-ball urn and a payoff of 100 tokens if the correct color ball is drawn, and once with a 30-ball urn and a 150-token payoff.

Our data show evidence of ambiguity aversion, as well as a negative reaction to compound lotteries. In particular, the certainty equivalents of the ambiguous urns are 2.5 (0.48) and 2.5 (0.46) percentage points lower than those of the risky urns for the 20 and 30 ball urns, respectively; and the certainty equivalents of the compound lotteries are 2.9 (0.51) and 2.8 (0.51) percentage points lower than those of the risky urns. Note that these differences are statistically significant, but they are not statistically significantly different from *each other*. On average, ambiguity aversion and reaction to compound lotteries are identical.[36]

[36]On the individual level, 30% of our respondents prefer the uncertain lottery over the ambiguous one, 20% prefer the ambiguous lottery, and 50% are neutral. Halevy (2007) used the BDM method to elicit certainty

Table 7: The correlation between certainty equivalents is substantial.

	Raw Correlations			Corrected for Measurement Error		
	Risk CE	Compound CE	Compound Reaction	Risk CE	Compound CE	Compound Reaction
Compound CE	0.55*** (.041)			0.74*** (.057)		
Ambiguous CE	0.60*** (.038)	0.65*** (.029)		0.78*** (.048)	0.85*** (.035)	
Ambiguity Aversion			0.44*** (.042)			0.85*** (.086)

Notes: ***, **, * denote statistical significance at the 1%, 5%, and 10% level, with standard errors in parentheses. $N = 786$.

Table 7 reports the raw and corrected correlations between the three measures. The raw correlation between ambiguous and compound certainty equivalents are 0.65. This is in line with Halevy (2007), who reports a correlation of 0.45 in the first round of his experiment, and a correlation of 0.71 in his robustness round. However, once measurement error is corrected for in our study, the correlation is much higher: 0.85.

Corrected correlations between certainty equivalents of risky and compound or ambiguous urns are substantial as well: estimated at 0.74 and 0.78, respectively. This leads to an important point: the high correlation between ambiguity aversion and reaction to compound lotteries may be because the certainty equivalents of both reflect risk attitudes as well. Thus, we subtract the risk certainty equivalents from each of the compound and ambiguous certainty equivalents, leaving measures of ambiguity aversion, and (negative) reaction to compound lotteries. This results in a smaller raw correlation of 0.44, but the same correlation of 0.85 when measurement error is taken into account. Moreover, the 90% confidence interval for this value is (0.71, 0.995). Thus, while we can reject the null hypothesis that the

equivalents, whereas we use an MPL. This choice was made because MPLs are easier to explain on a survey, when participants are not able to ask the experimenter for help. Once we take into account the fact that participants in our experiment express certainty equivalents in units 20 times as large as Halevy (2007), his data, which he graciously provided to us, is reasonably close to ours.

correlation is 1, we cannot reject the null that the correlation is very close to 1.[37]

Here, unlike in Section 3, our large sample size is likely the reason we find any difference at all. Drawing a random sample of 104 observations (the size of Halevy's experiment) 10,000 times, the correlation between ambiguity aversion and reaction to compound lotteries differs from 1 only 1.2% of the time at the 1% level, 4.8% of the time at the 5% level, and 9.0% of the time at the 10% level. It should be noted that these are the results from standard confidence intervals that are well-calibrated. While correlations have an upper bound of 1, and thus suffer from the Andrews's (2001) problem, our estimator can take on values greater than 1, and is normally distributed around 1 when that is the true correlation. Thus, any time a correlation greater than 1 is calculated using ORIV, this should be interpreted as strong evidence that the correlation is actually 1, rather than as a statistical issue.

As a final way of showing how closely ambiguity aversion and reaction to compound lotteries are related, we plot the CDFs of the various measures in Figure 2. The left-most panels show certainty equivalents for risky urns and ambiguous urns—the fact that these diverge below 50 is evidence of ambiguity aversion. The center panels show the certainty equivalents for ambiguous urns and compound urns: the distributions appear identical. The final panels show the distributions of ambiguity aversion and reaction to compound lotteries. Once again, these appear identical.

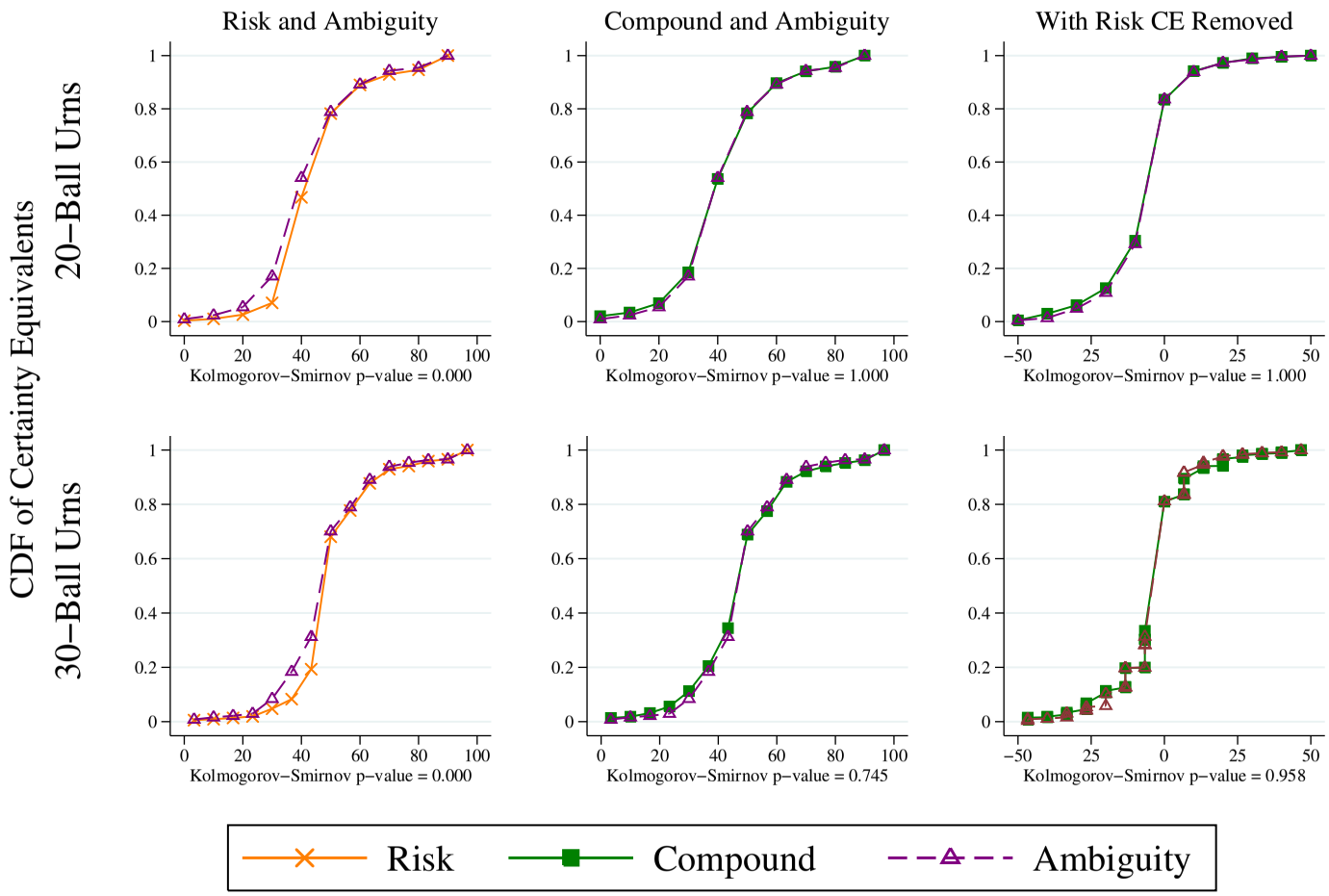
5.2 Substantive Implications

Ambiguity aversion and reaction to compound lotteries appear remarkably similar once measurement error is accounted for. It is worth noting that this is compatible with the original description of Knightian (1921) Uncertainty,

> [T]he essential fact is that "risk" means in some cases a quantity susceptible of measurement, while at other times it is something distinctly not of this character; and there are far-reaching and crucial differences in the bearings of the phenomena depending on which of the two is really present and operating.

[37] The 95% confidence interval is (0.68, 1.02) and the 99% confidence interval is (0.63, 1.08). Halevy's data has a correlation 0.47 in the main experiment, and 0.75 in the robustness round.

Figure 2: Population responses to risky and ambiguous lotteries differ, but are identical for ambiguous and compound lotteries.



Notes: The first two columns are certainty equivalents for a lottery paying 0/100 points. This is the natural scale for the 20-ball urns; for the 30-ball urns the expressed certainty equivalent is divided by 1.5, implying a CRRA utility function. The final column subtracts the certainty equivalent of the risky lottery from those of the compound and ambiguous lotteries.

That is, compound lotteries may be no more "susceptible to measurement" for individuals—even Caltech students, who are mathematically inclined—than those that are ambiguous.

This suggests to us that the defining characteristic—in addition to risk attitudes—in determining valuations of these lotteries is some notion of complexity. Regardless of the philosophical interpretation of these results, it is clear that any successful model of ambiguity aversion should also predict behavior in complex, but fully specified, risky environments.

6 Conclusion

If measurement error is such a ubiquitous, but easily correctable, issue, why has the experimental literature paid so little attention to it? The answer likely lies in the fact that attenuation bias driven by measurement error is a conservative bias. That is, it biases the researcher against false positives. So when asked about measurement error, a researcher can confidently answer that it would "go against finding anything."

However, measurement error creates another, field-wide, issue that has been little appreciated. It leads to the over-identification of "new" phenomena. Indeed, our paper shows two examples of this: previously, competitiveness was thought to have a component unconnected to risk aversion and overconfidence, and ambiguity aversion and reaction to compound lotteries (the latter a form of risk aversion) were thought to be distinct. Our results show that both are unlikely to be true. Moreover, our finding that elicitations of risk attitudes are more highly correlated than previously appreciated suggests that fewer elicitation methods are needed than previously thought. Given that these are the only three results we have examined in trying to understand the influence of measurement error in experiments, it seems likely that the over-identification of new phenomena is a substantial problem.

That measurement error may lead to the identification of new phenomena where none exist may feed into the recent mushrooming of methodological work suggesting the high rates of non-replicability of research discoveries (see Ioannidis, 2005; Simonsohn, 2015, and refer-

ences therein). Using the techniques developed here to account for measurement error may help researchers discover, in a more robust fashion, the deep connections between different attitudes and effects.

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