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Asset Integration and Attitudes to Risk: Theory and Evidence

by

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ABSTRACT. We provide evidence that choices over small stakes bets are consistent with assumptions of some payoff calibration paradoxes. We then exploit the existence of detailed information on individual wealth of our experimental subjects in Denmark, and directly estimate risk attitudes and the degree of asset integration. We discover that behavior is consistent with partial, rather than full, asset integration. The implied risk attitudes from estimating these specifications indicate risk premia and certainty equivalents that are *a priori* plausible. This theory and evidence suggest one constructive solution to payoff calibration paradoxes.

JEL CODES: D01, D81, C91, C93, C55, C81

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Debate surrounding theories of decisions under risk and uncertainty has renewed interest in the arguments of the utility function over event outcomes. The local measure of risk aversion proposed by Arrow [1971] and Pratt [1964] for expected utility theory (EUT) is based on terminal wealth being the argument. However, there is nothing in the axiomatic foundation of EUT that requires one to use terminal wealth as the argument: Vickrey [1945] used income instead of terminal wealth; von Neumann and Morgenstern [1944; p. 15-20][1953; p. 15-31] were agnostic; and Luce and Raiffa [1957; ch.2] discussed alternatives such as scalar amounts of terminal wealth or income or, alternatively, vectors of commodities. Arrow [1964], Debreu [1959; ch.7] and Hirshleifer [1965] developed models in which the arguments of utility functions are vectors of contingent commodities.

The choice of arguments of the utility function can have significant consequences for the inferences one can plausibly draw from empirical estimates of risk attitudes. Many economics experiments present participants with gambles over relatively small stakes and find that such gambles are frequently turned down in favor of less risky gambles with smaller expected values: modest risk aversion is the general finding. If the argument of the utility function is terminal wealth, then some specific patterns of small stakes risk aversion have implausible implications for preferences over gambles where the stakes are no longer small. One example from Rabin [2000] is that the expected utility of terminal wealth model implies that an agent who turns down a 50/50 bet of losing \$100 or gaining \$110, at all initial wealth levels between \$100 and \$300,000, will also turn down a 50/50 bet with possible loss of \$2,000 even when the gain is as large as \$12 million if they have an initial wealth of \$290,000. However, if the argument of the utility function is not terminal wealth, but rather the stakes offered in the gamble itself, or some other non-additive aggregation of initial wealth and the stakes, implications of this assumed pattern of small stakes risk aversion are no longer ridiculous (implausible) risk aversion (Cox and Sadiraj [2006]).

Given the importance of understanding the arguments of the utility function, the absence of empirical tests is remarkable. We initially provide evidence that choices over small stakes bets in Denmark are consistent with suppositions in the payoff calibration paradoxes. We then present evidence from a unique data source that allows us to confront the question of whether integration of wealth with income in risk preferences is full, partial or null, when agents are making choices over gambles with more modest stakes. We combine field experimental data on lottery choices from a sample of the Danish population and individual-level information on “personal wealth” from a confidential database maintained by Statistics Denmark. Using these data we are able to *identify a measure of personal wealth for the very same individuals who participated in standard experimental tasks*. This allows us to explore theoretical specifications that measure the extent to which individuals integrate their wealth with the prizes on offer in the experimental lottery tasks.

We find no support for the terminal wealth model. We consider the evidence pooling over all subjects, assuming homogeneous preferences. Our subjects behave as if they integrate only a tiny fraction of their personal wealth with the lottery prizes they are asked to make choices over. In effect, this “weighted wealth” is indistinguishable statistically and economically from zero.

In section 1 we briefly review the theoretical literature on the arguments of utility over vectors of outcomes and implications for the measurement of risk attitudes. We note that calibration issues apply to a wide range of decision models. Moreover, extreme assumptions about the nature of asset integration can be seen as special cases of a more flexible specification that admits both wealth and experimental income as arguments of some non-linear function. These results are not new, but they are not widely known. They are important because they serve up a menu of theoretically coherent alternatives to the extreme, “all or nothing” assumptions about asset integration that are often subsumed in the literature.

In section 2 we describe the data we have assembled from a combination of experimental

tasks and links to Danish Registry databases maintained by Statistics Denmark (SD). The sense in which our measure of “personal wealth” deserves quotation marks is explained. It does not include everything that a theorist might want to see in there, such as the present subjective value of human capital, nor does it include every category of financial wealth. On the other hand, it is arguably the most comprehensive wealth measure available to those who are interested in testing the theories of decision under risk.

In section 3 we present the structural model and econometric assumptions used to evaluate the extent of asset integration inferred from our data, and implications for risk attitudes. Section 4 presents estimates and implications. Section 5 outlines some issues that arise in the general case in which experimental choices and non-experimental choices are evaluated jointly. Section 6 draws conclusions.

We make two contributions. The first is to develop a general framework that clarifies that the core issue in the payoff calibration debate is the extent to which preferences are over income or final wealth, and embeds the two extremes that have characterized the debate as special cases. The second contribution is to use this framework to estimate the extent to which risk attitudes to income variation in the lab are integrated with wealth using two sources of wealth variation. One source is a within-subject manipulation of variations in wealth, and demonstrates that *perfect* asset integration would indeed be consistent with payoff calibration puzzles for our sample. The second source is cross-sectional variation of wealth in the Danish population, under the assumption of homogeneous preferences, and implies very little asset integration. Experimental subjects’ choices under the first source of wealth variation establishes the need for examination of the second source of wealth variation.

1. Theory

A. Calibration Critiques

Some seemingly plausible patterns of small-stakes risk aversion can be shown, through concavity calibration arguments, to have implausible implications for large stakes gambles under the terminal wealth specification, where initial wealth and income are integrated perfectly. Alternative empirical identifications of small-stakes patterns have implausible large-stakes implications for models defined on income, in which there is no integration of wealth with income. A different type of (convexity) calibration analysis applies to models with nonlinear probability transformations. From this literature, the theories that are now known to be subject to calibration critique include expected utility theory, dual theory, rank dependent utility (RDU) theory, cumulative prospect theory, and weighted utility and betweenness theories.

There are two types of calibration critiques that one needs to be cognizant of: we refer to these as “payoff calibration” critiques and “probability calibration” critiques. We consider the implications of the payoff calibration critiques. Within that category of critiques, the same risky (low-stakes) lottery choices can have quite different implications depending on the extent to which wealth is integrated with income in risk preferences. This is our principal focus, once we consider the empirical validity of the “seemingly plausible patterns of risk aversion” that underpin the calibration critique.

B. Small Stakes Risk Aversion

The payoff calibration critique may be stated in terms of four suppositions:

- P = “the agent is a risk averse EUT maximizer”
- Q = “the agent fully asset integrates”
- R = “the agent (weakly) turns down small-stakes gambles in favor of a certain amount with a

slightly lower expected value, and does so over a large enough range of wealth levels W ”

- $S \equiv$ “the agent turns down large-stakes gambles in favor of a certain amount with a significantly lower value, and looks silly.”

The calibration puzzle is the claim that if P, Q and R are true, then S follows. Since the behavior implied by supposition S is *a priori* implausible from a thought experiment, something must be inconsistent with these suppositions. Rabin [2000] draws the implication that P must then be false, and that one should employ models of decision-making under risk that relax supposition Q, such as Cumulative Prospect Theory. As a purely logical matter, of course, this is just one way of many ways to resolve this calibration puzzle.

Evidence claimed to support the premise in statement R that decision makers in experiments exhibit small stakes risk aversion for a large enough finite interval of wealth levels comes from designs in which subjects come to the experiment with potentially varying levels of wealth and each makes a single decision about a small-stakes lottery (Barberis, Huang and Thaler [2006]). This is weak, indirect evidence, although it might be suggestive. Interpretation of these data as providing a test of supposition R requires that we assume *no* variation of risk preferences between subjects and full asset integration (FAI), and accept guesses rather than data about wealth levels.¹ What is needed to evaluate supposition R is an experimental design that varies the wealth of a *given* decision-maker, who makes multiple decisions, and who can be presumed to behave consistently with one utility function during the lab session. Cox and Sadiraj [2008] propose a simple experimental design that does just this.

Cox and Sadiraj [2008; p.33] propose that one give subjects choices between a safe lottery of

¹ A common alternative assumption in the experimental literature is to assume no asset integration and interpret variation across wealth and observed choices across subjects as heterogeneity of risk preferences. It is apparent that both interpretations rest on previously untested, and extreme, assumptions about the degree of asset integration (“full” or “none,” respectively).

w for sure, and a risky lottery of a 50:50 chance of $w-x$ or $w+y$, where $w-x \geq 0$ and $y > x$. The key idea is to vary w *in the lab*, and ask each subject to make lottery choice *decisions at different levels of w* . Consider values of w from the ordered set, $S = \{w, w, w, w, w, w, W\}$, where smaller values of the letter w denote smaller values of lab wealth. These values of lab wealth may be plausibly much less than the W that the subject has in the field prior to the experiment. The experimenter does not need to know W for a given subject, but by varying “lab wealth” from S for that subject the experimenter has considered small-stakes lottery choices over 50-50 probabilities of a low prize of $w-x$ and a high prize of $w+y$ against “lab wealth” w for sure, or “field + lab” wealth levels $W+w$, with w from S , for that subject. This step of the design presumes that we vary lab wealth for a given subject, since then we can plausibly presume that field wealth W is constant for that subject during the experimental session. Integration of field wealth W with data from the experiment in analysis of calibration paradoxes depends on existence of good data about field wealth and also assumes a version of supposition Q for which the agent perfectly asset integrates field wealth and lab wealth.

If the agent prefers the safe lottery over the risky lottery for all of the lab wealth values used in the experiment, then we have verification of supposition R, at least for the range of variation in wealth proscribed by the experimenter’s budget. If we observe the agent choosing the safe lottery for small levels of lab wealth but the risky lottery for larger levels of lab wealth, then supposition R is rejected for that agent. Of course, we do not expect deterministic patterns of choice, so one ought to make some claim about the statistical significance of these choice patterns. This is one of the reasons for having multiple choices for each subject. An attractive feature of this experimental design is that we need not structurally model the EUT decision process for the agent: we can rely on simple statistical models such as (panel) probit, conditioned on lab wealth.

Building on this design, there have been “lab” tests of the premises of the calibration claims by Cox, Sadiraj, Vogt and Dasgupta [2013] and Harrison, Lau, Ross and Swarthout [2017] that do

not require integration of field wealth with lab wealth.

C. Partial Asset Integration within EUT

If supposition R cannot be rejected for the population under study, we must consider the implications of the payoff calibration critique in a constructive manner, and for that we turn to the idea of partial asset integration of wealth and income. We develop our analysis for a class of expected utility models that includes as special cases models with full asset integration (FAI), models with no asset integration (NAI), and models with partial asset integration (PAI). Models with full asset integration are possibly subject to the payoff calibration critique of Hansson [1988] and Rabin [2000]. Models with no asset integration or partial asset integration are possibly subject to the payoff calibration critique of Cox and Sadiraj [2006] and Rieger and Wang [2006]. Rather than engage in *a priori* arguments or thought experiments about paradoxes of risky choice, we develop a general theoretical model and let real data do some “real talking” in combination with that theoretical structure.

Cox and Sadiraj [2006] discuss the expected utility of initial wealth and income model with utility functional

$$\int u(w, y) dG = E_G(u(w, y)), \quad (1)$$

where G is an integrable probability distribution function and u is a utility function of initial wealth w and income y . We refer to this as the PAI-EUT model. Two standard models included in the PAI-EUT model are the expected utility of terminal wealth model with full asset integration (FAI-EUT), for which $u(w, y) = v(w+y)$, and the expected utility of income model with no asset integration (NAI-EUT), for which $u(w, y) = \xi(y)$.² These two standard models are polar cases in the class of

² Any utility function of the form $u(w,y) = \xi(y) + h(w)$ would exhibit the same risk preferences over income y as does $\xi(y)$.

models of PAI.

In our application, we treat w as deterministic and known, and of course y is stochastic by experimental design. This is consistent with the usual way in which asset integration is discussed in the literature. We discuss this issue further in section 5.

We begin with a quasiconcave utility function $u(w, y)$ defined over money payoff in the lab, y , and a measure of wealth, w . In a typical experiment subjects' payoffs are paid in amounts of cash that may not be a perfect substitute for outside the laboratory wealth because of differences in liquidity and transaction costs. For example, \$100 in housing equity is not a perfect substitute for \$100 in cash received from participation in an experiment. Therefore, we consider the possibility that money payoffs in an experiment and wealth outside the laboratory may not be perfect substitutes.³ There is then a need to distinguish curvature of indifference curves due to preferences over (w, y) from the preferences over risk.

D. Parametric Structure

A Constant Elasticity of Substitution (CES) function can be used to aggregate wealth w and

³ It is the case that if w and y are allowed to be imperfect substitutes then we have to assume the possibility of imperfect markets in w and y , or else some elementary no-arbitrage conditions would be violated. We do not view this as particularly problematic, for three reasons. First, if behavior is better characterized by assuming that w and y *are* indeed imperfect substitutes, then we have to assume imperfect markets. But then that assumption is one that is in effect supported by the data, even if it runs counter to some stylized model of behavior. That is, imagine that w and y are imperfect substitutes in preferences, but perfect substitutes at some relative price in the market. Then we would never observe behavior suggesting that they are perfect substitutes, hence we would never observe full asset integration behavior. The second reason that we do not view the assumption of imperfect markets as problematic is that there are transactions costs in converting one asset to another, at least for the assets we consider. These transactions costs might be larger or smaller for different individuals, or for different asset classes when one considers generalizations (as we do in §5), but those have to be evaluated on a case-by-case basis. The third reason is related to the second: we could imagine an even more general model in which the degree of asset integration emerges endogenously as a function of circumstances: these could be the transactions costs faced in substituting assets in the market, but it could also be the cognitive burden of thinking of the assets as perfect substitutes in preferences. That is, for some unstated reason the agent might prefer to keep w and y in distinct “mental accounts,” but still think of them as substitutable to some degree.

money payoff y when there is no risk. The terminal wealth model is found at one extreme of parameter values and the pure income model at the other. But the real interest is in between these extremes, and the point is to let the behavior of our subjects tell us the extent to which they (behave *as if* they) are integrating wealth with income from the experiment in making their choices.

Assume that all agents have the same ordinal preferences (when there is no risk), but can differ in their cardinal preferences (over risky outcomes).⁴ We begin with studying homothetic preferences. Following Debreu [1976; p.122], there exists a least concave function, u^* , which is a cardinal utility that represents the same ordinal preferences. In case of homothetic preferences, the least concave function is a homogenous function of degree one. So we use the CES specification

$$v(w, y) = [\omega w^\sigma + y^\sigma]^{1/\sigma} \quad (2)$$

where $w \geq 0$ is a measure of individual wealth, $y \geq 0$ is the prize in the money payoff in the experimental task, ω is a distributive share parameter to be estimated, $\sigma = 1/(1-\rho)$ is the revealed “elasticity of substitution” between wealth and experimental money payoff, and is also to be estimated, and $-\infty < \rho \leq 1$ to ensure that $v(\cdot)$ is quasiconcave. Risk averse preferences over (w, y) are represented by concave transformations of this function, and the EUT assumption that objective probabilities are not modified to generate decision weights. An often used specification of such transformation is the power function

$$U(v) = v^{1-r}/(1-r) \quad (3)$$

where $r \neq 1$ and v is defined by (2). In effect, (2) and (3) define a two-level, nested utility function, where (2) is an “aggregator function” defining a composite good, and (3) is the utility function

⁴ In a uni-variate model with either income or wealth as the only argument, cardinality is modeled entirely through the concavity of the utility function over the single argument. Here, however, cardinality depends also on the convexity of the contour functions over the two imperfectly substitutable utility arguments.

defined directly over that composite.⁵ Thus we can rewrite (3) more compactly as

$$U(w, y) = [(\omega w^\epsilon + y^\epsilon)^{(1-r)/\epsilon}] / (1-r) \quad (3')$$

where $\omega w^\epsilon + y^\epsilon > 0$.⁶ This generalized CES function blends together full, partial, and null asset integration on (w, y) space with risk preferences on composite good, $v(w, y)$, space.

With these parametric assumptions, the familiar one-dimensional Arrow-Pratt measure of *relative* risk aversion with respect to y , evaluated at w , is then

$$[r y^\epsilon - w^\epsilon (r-1) \omega] / [y^\epsilon + w^\epsilon \omega] \quad (4)$$

We discuss the need for measures of *multivariate* risk aversion in section 5 if one is to generalize our approach to allow both arguments of the utility function to be random.

Perfect asset integration with the “utility of terminal wealth” EUT model is the special case in which $\omega > 0$ and $\sigma = \infty$. The usual case in the literature assumes further that $\omega=1$, so that income and wealth are added together on a 1:1 basis. Zero asset integration with the “utility of income” EUT model, where income is interpreted tightly to mean the income from this specific experimental choice,⁷ is the special case in which $\omega = 0$.⁸ Note that we say nothing in this case about σ , because

⁵ This power function is unbounded, so it is useful to be clear on the implications for concavity calibration puzzles under FAI and EUT on a bounded or unbounded domain. If the utility function is bounded on $(0, \infty)$ then that is a sufficient condition for implausible risk aversion in large stakes (e.g., Cox and Sadiraj [2008; Proposition 2, p.20]); global small-stakes risk aversion is not needed for this result. It is not a necessary condition. Small-stakes risk aversion over all $(0, \infty)$ is a sufficient condition for the utility function to be bounded (e.g., Rabin [2000; p.1283] or Cox and Sadiraj [2006; p.59, §C.4]); it is not, however, a necessary condition. Being bounded on $(0, \infty)$ is a necessary condition for small-stakes risk aversion over the open interval $(0, \infty)$, but it is not sufficient. An increasing power function is unbounded and hence violates the necessary condition on boundedness; therefore it cannot represent risk attitudes that exhibit small-stakes risk aversion over all $(0, \infty)$. The sufficiency part can be illustrated by considering a CARA function with parameter 0.0003; it is bounded, however the small-stakes risk aversion pattern in Cox and Sadiraj [2006] is not satisfied, since \$100 for sure is *rejected* in favor of an equal chance of \$210 or \$0. Small-stakes risk aversion defined on a finite interval implies nothing at all about the boundedness of the utility function. Finally, small-stakes risk aversion over a *large enough finite* interval is a sufficient condition for implausible risk aversion for large stakes, whether or not the utility function is bounded or unbounded.

⁶ For negative prizes in income, write it as: $\omega w^\epsilon + \text{sign}(y) \text{abs}(y)^\epsilon > 0$.

⁷ This interpretation is “tight” in the sense that one might also consider income from the set of experimental tasks that this binary choice is embedded in, or the income from the whole experimental session. For example, is income the lottery prize in one binary choice pair, the income from the 60 choices, or the income from the whole session since there were additional paid choices in addition to these lottery choice questions? One could undertake an exactly parallel discussion of partial asset integration within the

any value of σ would generate the same observed choices if $\omega=0$. Our main hypothesis is that subjects perfectly asset integrate with their actual wealth.

2. Data

Our data consist of observations of choice behavior in experimental tasks and wealth data for 442 individuals. The sample is representative of the adult Danish population residing in Greater Copenhagen as of January 2015. Our sample consists of 52% men, aged 47 on average, 43% of whom were married, with an average household size of 1.4, and with average income of 434,085 kroner per year. Comparing to the 1,455,772 comparable Danes in the Registry, our subjects are not statistically significantly different except for household size and income: the population averages were 1.54 and 338,859 kroner, respectively.

All experiments were run in February and March, 2015. The experimental data are of the standard type, and employ procedures described in Andersen, Harrison, Lau and Rutström [2014].

The wealth data are novel, and involve matching the experimental subjects with data collected by SD. The matching process, and all statistical analyses with those data, occur “remotely” at the statistical agency, to ensure privacy.

A. Experimental Data

Each of our 442 subjects was asked to make choices for each of 60 pairs of lotteries in the gain domain, designed to provide evidence of risk aversion as well as the tendency to make decisions

experimental session, evaluating what might be called “local asset integration” issues. Our focus here is on “global asset integration issues” between the usual interpretations of experimental data and the implications of the calibration critiques.

⁸ And, to visualize these intuitively as perfectly complementary Leontief preferences, one might further assume $\sigma = 0$. This assumption, although often implicit, is not necessary for NAI.

consistently with EUT or RDU models.⁹ An online appendix lists these lottery parameters, and the logic behind them. In general each lottery has 3 prizes, although there are some lotteries with 4 prizes, 2 prizes or just 1 prize. The battery is based on ingenious designs from Wakker, Erev and Weber [1994], Loomes and Sugden [1998] and Wilcox [2015], as well as the direct test of supposition R proposed by Cox and Sadiraj [2008; p.33] reviewed earlier. The analysis of risk attitudes given these choices follows Harrison and Rutström [2008].

There were 4 batteries used across the 442 subjects. Each battery included the 24 lottery pairs from Wakker, Erev and Weber [1994]. One battery also included 36 lottery pairs from Wilcox [2015], and this full set of 60 lottery pairs was administered to 222 subjects. The remaining three batteries included the lotteries inspired by Loomes and Sugden [1998] and Cox and Sadiraj [2008], for another set of 60 lottery pairs administered to 220 subjects; the three versions of this battery differed by varying the scale of payoffs.

We carefully selected these lotteries to ensure considerable variation in prizes and probabilities, to facilitate identification of the full structural model. Over all batteries there are 90 distinct prizes and 16 distinct probabilities. At the individual subject level the number of distinct prizes is either 37 or 26, and the number of distinct probabilities is either 16 or 12.

Apart from the tests of supposition R, these choices themselves are not the direct basis for our evaluation of the payoff calibration paradoxes. Combined with the wealth data for each subject, these *choices* allow us to estimate the risk preferences implied by EUT and RDU models, and those *estimates* are then used to evaluate the paradoxes with counterfactual lottery choices. The many variations in wealth, lottery payoffs and lottery probabilities implied by our design allow us to

⁹ The subjects were also presented with other decision tasks in the experiment, which are not analyzed here. For each type of decision task the subjects had a 10% chance of getting paid. If they were paid in the part of the experiment analyzed, one of the 60 decision tasks was randomly selected and the chosen lottery was played out for payment. Average earnings for those who got paid from these 60 decision tasks was 1,923 DKK. Average earnings including recruitment fees across all 442 subjects was 954 DKK.

identify all the required theory parameters.

B. Wealth Data

Wealth data are based on register data from SD. Our data contain economic, financial, and personal information on each individual from relevant official registers. The data set was constructed based on two sources made available from SD and matched with our experimental data: these sources are the Danish Civil Registration Office and the Danish Tax Authorities. All permanent residents in Denmark, and all Danish citizens, have a unique social security number given at birth or the date of formal residence, known as the CPR number, and this number allows us to match data across data sources. The CPR number follows every individual throughout the entire life and all information on an individual is registered on this number. We had access to the CPR number of every subject in our experiments.

Individual and family data are taken from the records in the Danish Civil Registration. These data contain the entire Danish population and provide unique identification across individuals and households over time. Each record includes the personal identification number (CPR), name, gender, date of birth, as well as the CPR numbers of nuclear family members (parents, siblings, and children) and marital history (number of marriages, divorces, and widowhoods). In addition to providing extra control variables, such as age, gender, and marital status, these data enable us to identify the subjects who participated in the artefactual field experiment described above.

Income and wealth information are retrieved from the official tax records at the Danish Tax Authorities (SKAT). This data set contains personal income and wealth information by CPR numbers on the entire Danish population. SKAT receives this information directly from the relevant sources: financial institutions supply information to SKAT on their customers' deposits, on financial

market assets, on interest paid or received, and on security investments and dividends. Employers similarly supply statements of wages paid to their employees.

The *wealth* variable in our analysis is constructed from data reported by SD that represent net individual wealth.¹⁰ Total assets are the market value of domestic real estate, shares and mutual funds, bonds, assets deposited in domestic and foreign financial institutions, pensions and the value of automobiles. Total liabilities are the value of debt in domestic and foreign financial institutions and mortgages. All values of shares, bonds and pensions are reported by financial institutions as of December 31, 2014; values of real estate are estimated by SD as the market value on December 31, 2014; and the value of automobiles is calculated by SD with a one-year lag.¹¹ All values are in 2015 Danish kroner, and values are reported for the full sample of 442 subjects (conversions to USD use the exchange rate 1 DKK \approx 6.643 USD applicable during most of the experiment).

Our wealth measure does not include cash, value of yachts, paintings, equity in privately held companies, nor the market value of shareholder equity in *privately* held companies and *unlisted* mutual funds. Our wealth measure *does* include shareholder equity in *publically* traded companies and *listed* mutual funds. The wealth measure does not include non-traded assets such as human capital, which means that borrowing for assets such as education is seen as debt without any corresponding assets. This is arguably one of the most comprehensive measures of private financial wealth for an entire population that one can get, although we realize that some important non-financial components are left out.

Table 1 provides a tabulation of wealth and its components for our sample. The positive

¹⁰ An alternative is to use household wealth rather than individual wealth, exploiting further the ability of our data to identify other members of the household of the subject in our experiments. On the other hand, one then opens up subtle issues about whose risk attitudes were on display in the experiments (i.e., those of the individual, or those of the household) and how households pool income from individuals.

¹¹ All foreign assets and debt are self-reported to SKAT, and are zero for every subject in our sample.

skew of the distribution of wealth is no surprise. For 4.7% of our subjects, or 21 out of 442, there is negative net wealth, reflecting the fact that some assets are not fully accounted for. For all calculations we assume that wealth cannot be negative and truncate it to zero. Individuals with zero field wealth having nothing to integrate with lab income: in a formal sense, of course, they do integrate, but the *effect* is as if they do not since they have zero wealth.

Access to these unique data is an important issue, both in terms of the ability of others to replicate our findings and for their ability to extend our analysis. Researchers at authorized Danish institutions can gain access to de-identified micro data provided by SD through remote access connections. SD manages most of Danish micro data. The fundamental authorization principle of SD is that data will not be disclosed where there is an imminent risk that an individual person or individual enterprise can be identified. This applies not only to identified data, such as CPR numbers, but also to de-identified data, since such data are usually so detailed that identification can be made. An online appendix documents procedures to access these data.

3. Econometric Model

A. Expected Utility Theory

Although the concerns about implausible risk attitudes under terminal wealth specifications apply to all decision theories that are additive over states, we initially focus on EUT because it is parsimonious. Under EUT the probabilities for each outcome y_j , $p(y_j)$, are those that are induced by the experimenter, so expected utility is simply the probability weighted utility of each outcome in each lottery $i \in \{A, B\}$, where A and B denote left and right lottery, respectively. Using $U(w, y)$ from (3'), we then have:

$$EU_i = \sum_{j=1, J} [(p(y_j)) \times U(w, y_j)] = \sum_{j=1, J} [p_j \times U(w, y_j)] \quad (6)$$

for a lottery with J prizes. To capture behavioral errors we employ a Fechner specification with “contextual utility,” so that we assume the latent index

$$\nabla EU = [(EU_B - EU_A)/\tau]/\mu \quad (7)$$

where τ is a normalizing term described in a moment, μ is the Fechner behavioral error parameter to be estimated, and EU_B and EU_A are the expected utilities of the right and left lottery as presented to subjects. The normalizing term τ is defined as the difference between the maximum utility over all of the prizes in that lottery pair minus the minimum utility over all of the prizes in that lottery pair.

Thus it varies from choice context to choice context, depends on the parameters of the utility function, and normalizes the difference in EU to lie between 0 and 1. This results in a more theoretically coherent concept of risk aversion when one allows for a behavioral error such as with μ (Wilcox [2011]).

The latent index (7), based on latent preferences, is then linked to the observed experimental choices using a standard cumulative normal distribution function $\Phi(\nabla EU)$. This “probit” function takes any argument between $\pm\infty$ and transforms it into a number between 0 and 1 using this familiar function. Thus we have the probit link function,

$$\text{prob}(\text{choose lottery B}) = \Phi(\nabla EU) \quad (8)$$

The index defined by (7) is linked to the observed choices by assuming that the probability that the B lottery is chosen depends on ∇EU in the manner specified by (8).

Thus the likelihood of the observed responses, conditional on the EUT and utility specifications being true, depends on the estimates of the utility function given the above statistical specification and the observed choices. The log-likelihood for the utility function (3') is

$$\ln L(\tau, \omega, \varrho, \mu; \mathbf{c}, \mathbf{w}) = \sum_i [(\ln \Phi(\nabla EU) \times \mathbf{I}(c_i = 1)) + (\ln \Phi(1 - \nabla EU) \times \mathbf{I}(c_i = -1))] \quad (9)$$

where $\mathbf{I}(\cdot)$ is the indicator function, $c_i = 1(-1)$ denotes the choice of the Option B (A) lottery in risk

aversion task i , and $\nabla E U$ is defined using the parameters r , ω , ρ and μ .¹² All estimates employ clustering at the level of the individual, since errors for a given individual may be correlated.

B. Rank Dependent Utility Theory

One popular alternative to EUT is to allow the decision-maker to transform the objective probabilities presented in lotteries and to use these weighted probabilities as decision weights when evaluating lotteries. To calculate decision weights under RDU one replaces expected utility defined by (6) with RDU

$$RDU_i = \sum_{j=1}^J [(d(y_j)) \times U(w, y_j)] = \sum_{j=1}^J [d_j \times U(w, y_j)] \quad (10)$$

where

$$d_j = \pi(p_j + \dots + p_j) - \pi(p_{j+1} + \dots + p_j) \quad (11a)$$

for $j=1, \dots, J-1$, and

$$d_j = \pi(p_j) \quad (11b)$$

for $j=J$, with the subscript j ranking outcomes from worst to best, $\pi(\cdot)$ is some probability weighting function, d_j is the decision weight on the j^{th} -ranked outcome, and RDU refers to the Rank-Dependent Utility model. Of course, one then has to specify the functional form for $\pi(p)$ and estimate additional parameters, but the logic extends naturally.

We use the general functional form proposed by Prelec [1998] for probability, since it exhibits considerable flexibility. This function is

$$\pi(p) = \exp\{-\eta(-\ln p)^\rho\}, \quad (12)$$

¹² One of the core hypotheses to be tested is that $\omega = 0$, and one can run into issues with such hypothesis tests where the parameter in question is close to the boundary of an admissible region. In fact, we are estimating a likelihood function that is already highly non-linear in the parameters (e.g., the curvature of the utility function). Hence we can use a standard numerical method to constrain parameters such as ω to lie in the unit interval by estimating a different parameter $\tilde{\omega}$ which is then, in the function evaluator, converted to $\omega = 1/(1+\exp(\tilde{\omega}))$. In this manner the algorithm evaluating the likelihood can vary $\tilde{\omega}$ between $\pm\infty$ and still keep ω constrained to the unit interval. All hypothesis tests defined over ω are numerically undertaken on the estimated parameter $\tilde{\omega}$, which by definition never gets close to a boundary (the hypothesis tests are therefore non-linear in nature, and use the “delta method” to correctly infer test statistics and p -values).

and is defined for $0 < p \leq 1$, $\eta > 0$ and $\varphi > 0$; note that we do *not* require $0 < \varphi < 1$. When $\varphi = 1$ this function collapses to the familiar Power function $\pi(p) = p^\eta$, and EUT assumes the identity function $\pi(p) = p$, which is the case when $\eta = \varphi = 1$. With (12) included, the log-likelihood then becomes

$$\ln L(r, \omega, \rho, \eta, \varphi, \mu; c, w) = \sum_i [(\ln \Phi(\nabla RDU) \times \mathbf{I}(c_i = 1)) + (\ln \Phi(1 - \nabla RDU) \times \mathbf{I}(c_i = -1))] \quad (13)$$

and we estimate the model with two extra parameters for the probability weighting function.

Estimating the RDU model from experiments that employ the Random Lottery Incentive Method (RLIM) requires that one assumes that individuals isolate each pairwise lottery choice within the series from each other. This implies the compound independence axiom, even though the RDU model allows independence to be violated when subjects evaluate each simple lottery. The vast majority of incentivized lottery choice experiments use RLIM and rely on this axiom. Thus, the RDU model applied to RLIM data inconsistently relaxes that axiom when it comes to evaluating individual lotteries, but assumes that it is valid when applying the RLIM payment protocol (Harrison and Swarthout [2014] and Cox, Sadiraj and Schmidt [2015]).

4. Results and Implications

A. Tests of the Small Stakes Risk Aversion Premis

Using the test proposed by Cox and Sadiraj [2008] for a sub-sample of 220 adult Danes from our complete sample of 442, we actually find evidence of the *relevant* type of “small stakes risk aversion” for the range of lab wealth we considered. The experimental design involved them each making 6 binary choices in the wider battery of binary choices we consider below. Subjects were randomized to 6 lottery choice pairs from a set of 18 possible pairs, spanning 17 different levels of lab wealth. Hence the lab wealth varied for each subject over their 6 choices, and we have pooled data spanning the 17 lab wealth levels. The gains and losses in absolute value were paired for each

subject over different lab wealth levels: for example, +180 and -160 for lab wealth levels of 300 ($\approx \$45$) and 2700 ($\approx \406). Although we refer to “lab wealth,” all that the subject saw was a lottery that had one outcome with a probability of 1, and another lottery with the usual risky outcomes.¹³ Hence we did not use language or framing that would lead subjects to be more or less inclined to integrate it into their extra-lab, “field wealth.” Nor were the outcomes in the risky lottery presented as deviations from the certain outcome of the non-risky lottery, which might also encourage framing. For example, subjects were asked to choose between 2,700 kroner for sure and the risky lottery with outcomes 2,540 kroner and 2,880 kroner. With perfect asset integration, 2,540 kroner adds to the subject’s wealth no matter what the subject’s choice is, so we refer to 2,540 kroner as “lab wealth.”

Figure 1 shows the findings with a random effects panel probit model, since there is no need here for structural estimation of risk preferences. We find no significant evidence of a decline in risk aversion for lab wealth levels over the range considered here. The solid line shows the average prediction, and the shaded area shows the 95% confidence interval around that prediction. Subjects exhibit risk aversion for all levels of lab wealth considered here. So we conclude that the evidence for these adult Danes and these levels of lab wealth does *not* lead us to reject supposition R, that “the agent turns down small-stakes gambles in favor of a certain amount with a slightly lower expected value, and does so over a large enough range of wealth levels W.”

Since supposition R, one of the premises of the calibration critique, is not rejected, there is a need to examine the partial asset integration specification proposed earlier.

¹³ An alternative way to add a lab wealth component might be to randomly add it to the show-up fee for participating in the experiment. The problem with this approach is that it would raise a potential confound due to sample selection issues.

B. Basic Results on Asset Integration for Representative Agents

We now employ the full sample of 442 Danes, and all of the 60 binary choices each of them made. Panel A of Table 2 shows maximum likelihood estimates of the utility function (3'). We assume here that every adult Dane in our sample has the same ordinal preferences over w and y (when there is no risk), as well as the same coefficient r ; an online appendix considers estimates for each individual. The coefficient r is estimated precisely, as is the parameter ω reflecting the weight attached to wealth. We find that the weight attached to wealth is virtually zero, and statistically *not* different from zero. This is a fundamental result, since it means that the PAI specification collapses to the NAI specification in this pooled estimation, and we reject the FAI hypothesis. It also means that it is virtually impossible, for sensible economic reasons, to identify the substitutability between w and y . We find an estimate of ρ of 0.89, implying an estimate of σ of 9.1, but since there is virtually no weighted wealth to substitute with, these values have little economic meaning.

Average net wealth in the estimation sample is 3,074,678 kroner (\approx \$462,845), so these estimates imply that individuals behave as if they evaluate experimental income relative to a weighted baseline wealth of $\omega \times w = 3074678 \times 0.00000625 = 19$ kroner (\approx \$2.86). This is effectively zero in economic terms: for example, it would currently only get half of an Egg McMuffin Value Meal in a Danish *McDonalds*. Another way to evaluate this weighted baseline wealth estimate of 19 kroner is by comparison with the lottery prizes, which ranged between 0 kroner and 6,750 kroner (\approx \$1,016). Needless to say, we can easily reject the hypothesis of FAI since $\omega \approx 0$, and the p -value on the test of the hypothesis that $\omega = 0$ is 0.77.

Another way to see these results, perhaps more intuitively, is to see if measures of Net Wealth correlate with risk attitudes in a reduced form manner. We do this by estimating the EUT-NAI model and asking if the coefficient r is significantly affected by Net Wealth: in this case we

model r as a linear function of some covariates. Our structural results suggest that they should not, since Net Wealth is “zeroed” out by a very low estimate of ω , *at least when we assume homogeneous risk preferences*. If we include Net Wealth the effect on r is -0.004 with a p -value of 0.45 ; if we include a dummy for the top quartile of Net Wealth the effect on r is $+0.004$ with a p -value of 0.93 ; if we include the 5 major components of Net Wealth we have a joint effect that has a p -value of 0.45 , and no component has an individual effect with a p -value below 0.23 . On the other hand, when we include the components of Net Wealth *and some basic demographics* (gender, age, marital status, household size, and net income) we do find a significant joint effect of the components of Net Wealth with a p -value of 0.005 , and the individual component net deposits (with financial institutions) has a significant individual effect of -0.07 with a p -value of 0.003 . These results point to the importance of controlling for heterogeneity, and we do that in an online appendix by estimating the model for each individual, thereby allowing implicitly for *all* observable *and unobservable* individual characteristics.

C. Payoff Calibration Implications for EUT

Using these estimates and the average value of wealth in Denmark we can evaluate the Certainty Equivalents (CE) of a range of lotteries varying in the scale of the stakes. Implausible implications for large stakes can be detected through an extremely low ratio of CE to the Expected Value (EV).¹⁴

Panel B of Table 2 shows implied CE values using the CRRA utility function (3') and the parameter estimates in Panel A. Let H denote a high prize and L denote a low prize, for $H > L$. The CE in Table 2 is then the sure amount of money that has the same expected utility to the individual

¹⁴ Similar results are obtained with median wealth instead of average wealth. The ratio of EV to CE is slightly lower, but close to those reported here.

as the lottery that pays H with probability p and L with probability $(1-p)$. The CE is defined by

$$U(w, CE) = p \times U(w, H) + (1-p) \times U(w, L). \quad (14)$$

So this CE solves for risky income in the experiment, and the stakes are chosen to be within the payoff domain in our experiments. The smallest ratio of CE to EV is 0.362, and most are much higher: these ratios are hardly implausible in the sense of the term used by Hansson [1988], Rabin [2000], Neilson [2001], Rieger and Wang [2006], Cox and Sadiraj [2006] and Safra and Segal [2008].

Figure 2 evaluates the traditional Arrow-Pratt measure of relative risk aversion (RRA) in (4) for the estimated EUT-PAI model. The wealth levels in each panel range up to 5 million kroner. Panel A displays RRA for low stakes lottery prizes up to 10,000 kroner, and Panel B displays RRA for high stakes lottery prizes up to 1 million kroner. Both Panel A and Panel B shows modest levels of risk aversion for a wide range of wealth and experimental payoffs.

These PAI estimates allow us to verify that (a) getting 190 with probability $\frac{1}{2}$ and 0 with probability $\frac{1}{2}$ is rejected in favor of getting 75 for sure, for all wealth amounts smaller than 35 million; and (b) the same utility function exhibits plausible risk aversion for large stakes. Under FAI, no EUT-consistent agent can exhibit both (a) and (b).

It is, however, possible to come up with some “edge cases” in which the predictions of EUT-PAI are implausible. For example, at a wealth level of 307 kroner, a low prize of 0, and a high prize of 5,000 kroner, we get very low ratios of CE to EV, between 0.0004 and 0.12, for probabilities between 0.01 and 0.3 on the large prize. As the wealth level increases to the mean wealth level of 3,074,678 kroner, the same example generates low ratios between 0.02 and 0.12 for probabilities between 0.01 and 0.2 on the high prize. We return to compare results for these special cases when we allow for RDU risk preferences.

D. Probability Weighting

The RDU model estimates with the PAI specification are shown in Panel A of Table 3, and show evidence of slight probability weighting pessimism. Compared to the EUT estimates for the PAI specification, there is less curvature on the utility of outcomes once the possibility of probability pessimism is allowed for.¹⁵ We can easily reject the assumption that there is no probability weighting ($\eta = \varphi=1$), and this is reflected in the improved log-likelihood with the RDU model over EUT. In terms of PAI, the estimates are similar to those under EUT except that there is slightly more substitutability between wealth and lab payoffs: in particular, the fundamental finding that $\omega \approx 0$ is the same.

The overall log-likelihood of the RDU-PAI model is the best of the RDU specifications considered (RDU-NAI, RDU-PAI and RDU-FAI). We can formally reject the FAI hypotheses since ω is estimated precisely, $\omega \approx 0$, and we cannot formally reject the null hypothesis that $\omega = 0$ at any conventional statistical level. For the same reasons, we cannot reject the NAI hypothesis either.

For reasons already noted for the EUT-PAI model, when $\omega \rightarrow 0$ the economic meaning of the parameters defining the substitutability if w and y disappears. We formally estimate ρ to be 0.9999927, with a standard error that spans 1, so it is no surprise that the estimate of $\sigma = 1/(1-\rho)$ is extremely high, at 137,913, and with a large standard error. Again, these wild numerical values follow directly from the economics of the CES function (2) when $\omega \rightarrow 0$, and have no substantive significance or effect on the other parameter estimates (i.e., one could just as easily have constrained $\rho = 1$ and inferred essentially the same estimates).

¹⁵ In other words, for the same choice data, the EUT and RDU models decompose the same risk premium in a different way. The EUT model ascribes all of the risk premium to $U'' < 0$, and the RDU model explains the risk premium with $U'' < 0$ as well as probability pessimism. Since probability pessimism, *ceteris paribus* U'' , generates a risk premium itself, the net effect must be for there to be less diminishing marginal utility under RDU than there is under EUT.

E. Payoff Calibration Implications for RDU

Using the RDU-PAI estimates from Table 3, we can again evaluate the ratio of the CE to the EV for a range of low stakes and high stakes lotteries. Using the same lotteries as in Panel B of Table 2, in Panel B of Table 3 the CE now solves

$$U(w, CE) = h(p) \times U(w, H) + (1-h(p)) \times U(w, L). \quad (15)$$

The smallest ratio of CE to EV in Table 3 is 0.478, and most are much higher, exactly as in Table 2. In general the ratios in Tables 2 and 3 are similar. It is easy to verify that the RDU-PAI model also satisfies the payoff calibration conditions noted earlier for the EUT-PAI model.

Again, as with the EUT-PAI estimates, using these RDU-PAI estimates one can verify that (a) getting 190 with probability $\frac{1}{2}$ and 0 with probability $\frac{1}{2}$ is rejected in favor of getting 75 for sure, for all wealth amounts smaller than 15.8 million; and (b) the same utility function exhibits plausible risk aversion for large stakes. Under FAI, no RDU-consistent agent can exhibit both (a) and (b).¹⁶

Using these RDU estimates, we can reconsider the edge cases noted earlier, under EUT-PAI, in which the PAI predictions are implausible. Under EUT-PAI, at the low wealth level of 307, the ratio of CE to EV was between 0.0004 and 0.12 for probabilities between 0.01 and 0.3 on the large prize: with RDU-PAI these ratios are between 0.04 and 0.27, which range from implausible to plausible. The ratio is 0.09, 0.13 and 0.20 for probabilities on the large prize of 0.05, 0.1 and 0.2, respectively. As the wealth level increases to the mean wealth level of 3,074,678 kroner, the same

¹⁶ Although these exercises showing how a representative agent would react to various risky contexts are informative about average behavior, they do not allow for heterogeneity in preferences. In fact, the estimate of ω may, in part, reflect heterogeneity in risk attitudes that just happens to be correlated with wealth, rather than some true relation between risk attitudes and wealth. Under CRRA, for any given value of r , a higher wealth level would predict more risk taking choices in the lottery tasks. Without having observations where wealth varies at the individual subject level, this possibility cannot be ruled out. Thus, if the true preferences are NAI, a positive ω could just be reflecting the possibility that, in our sample, the subjects with higher wealth are less risk averse. Or, if the true preferences are FAI, $\omega < 1$ could just be reflecting the possibility that, in our sample, the subjects with higher wealth are more risk averse.

example generates plausible ratios under RDU-PAI between 0.26 and 0.31 for probabilities between 0.01 and 0.2 on the high prize.

These edge cases show that although the PAI model can accommodate risk version at small and large stakes at the same time, there remain cases falsifying the model. These edge cases allow us to identify the limits of the PAI approach as it is specified here. However, considering a more flexible specification of ω , where it varies with context, could accommodate these edge cases. When RDU-PAI fails to work in these edge cases, so does RDU-NAI. However, the RDU-PAI prediction becomes plausible at wealth levels that are large enough to make baseline wealth $\omega \times w$ meaningful for predictions with stochastic income. In contrast, the performance of RDU-NAI cannot improve with increasing wealth levels. This also applies to Cumulative Prospect Theory, which is equivalent to RDU-NAI when all choices are made on the gain domain. With the exception of the edge cases, the PAI model does well, as illustrated by the examples in Tables 2 and 3. It does particularly well when paired with the RDU model of decision-making under risk.

5. Generalizations

As flexible as our approach is in comparison to the full integration and no integration special cases that have dominated the discussion, it is still something of a “reduced form” approach to the structural question of the joint determination of lab and non-lab choices. In effect, we take the myriad of decisions underlying w to be given, implicitly assuming that all components of w are symmetric in their relation to y . Given the importance of the issue, we sketch several deeper issues that must be addressed as one generalizes our approach.

In general, it need not be the case that there is symmetry with respect to *components* of w and experimental choices over y . This is immediately problematic when one considers experimental

interventions in the field that offer choices over vectors of commodities rather than just money. For example, the experimental provision of a subsidized microinsurance product over one type of stochastic outcome, such as the weather, might be expected to interact with cropping choices differently than family planning decisions or retirement decisions. Closer to our setting, some components of w , such as more liquid components of wealth, might be viewed as closer substitutes to experimental income than others.¹⁷ These extensions can be immediately captured with nested-CES aggregator functions, of the kind that are common in demand analysis and computable general equilibrium modeling.¹⁸

In a related vein, individual wealth might be viewed as a closer substitute to experimental income that the *individual* is choosing over, and other household wealth as not perfectly fungible with individual wealth. Or we might consider an intertemporal utility function defined over stochastic prizes to be paid today and stochastic prizes to be paid in the future (Kihlstrom [2009] and Andersen, Harrison, Lau and Rutström [2016]).¹⁹ In essence, wealth held as financial assets are simply claims on future income in this manner, thus motivating interest in such intertemporal utility functions.

¹⁷ We can consider those subjects who have more than the median fraction of Net Wealth in relatively liquid form, which in our case refers to net assets in financial institutions, bonds, and shares. For simplicity of interpretation, we focus just on point estimates for individual subjects, without conditioning on the statistical significance of the estimate. Around 77% of these subjects are RDU-consistent. Just over 92% of these subjects have an ω less than 0.05, and 85% have an ω less than 0.001; 79%, 83% and 90%, respectively, have a weighted baseline wealth $\omega \times w$ less than 10 kroner, 1,000 kroner, and 100,000 kroner, respectively. Just over 86% of these subjects have a coefficient of relative risk aversion for the composite, r , greater than 0 and less than 0.5. Hence we conclude that these subjects are actually closer to NAI than the typical subject.

¹⁸ The nested-CES class allows global regularity *and local flexibility* in the specification proposed by Perroni and Rutherford [1995]. Many specifications that allow local flexibility trade off global regularity, an important property for calibration critiques.

¹⁹ One might argue that some of these examples of imperfect substitutes derive from the absence of perfect capital markets. For example, in the intertemporal case the existence of perfect capital markets implies the familiar Fisherian (non-)separation theorem. In these cases one would simply restate results in terms of indirect utility functions.

Once we consider multiple arguments of the utility function there are a number of theoretical subtleties to consider. One issue is to consider multivariate measures of risk aversion. Kihlstrom and Mirman [1974] proposed such an approach under the restrictive assumption that the ordinal preferences underlying two expected utility functions exhibit the same preferences over non-stochastic outcomes. In this case they propose a scalar measure of total risk aversion that allows one to make statements about whether one person is more risk averse than another in several dimensions, or if the same person is more risk averse after some event than before.

If one relaxes this assumption, which is not an attractive one, Duncan [1977] shows that the Kihlstrom and Mirman [1974] multivariate measure of risk aversion naturally becomes matrix-valued. Hence one has vector-valued risk premia, and this vector is not “direction dependent” in terms of evaluation. Karni [1979] shows that one can define the risk premia in terms of the expenditure function, rather than the direct utility function, and then evaluate it “uniquely” by further specifying some statistic of the stochastic process. For example, if one is considering risk attitudes towards a vector of stochastic price shocks, then one could use the mean of those shocks.

A closely related literature defines multi-attribute risk aversion where the utility function is defined over more than one attribute. In our case one attribute would be experimental payoffs y and the other attribute would be extra-experimental wealth w . In this context, Keeney [1973] first defined the concept of conditional risk aversion, Richard [1975] defined the same concept as bivariate risk aversion, and Epstein and Tanny [1980] defined it as correlation aversion. There are several ways to extend these pairwise concepts of risk aversion over two attributes to more than two attributes, as reviewed by Dorfleitner and Krapp [2007].

One attraction of the concept of multiattribute risk aversion is that it allows a relatively simple characterization of the functional forms for utility that rule out multiattribute risk attitudes:

additivity. One can have an additive multiattribute utility function and still exhibit partial, or single-attribute, risk aversion. Similarly, one can generate results that do not depend on partial, single-attribute risk aversion, but could still depend on multiattribute risk aversion.²⁰

A simple, but important, application of the concept of multiattribute aversion, referred to above as correlation aversion, is when considering intertemporal utility functions. In this case allowing for a non-additive intertemporal utility function allows one to tease apart “a-temporal risk preferences” from “time preferences,” especially temporally correlated risk preferences. In this application one attribute is the amount of money involved (more or less) and the other attribute is when it is paid (sooner or later). This approach can be directly implemented in controlled experiments, as illustrated by Andersen, Harrison, Lau and Rutström [2016]. For present purposes, it can be viewed as another application of the idea of bivariate risk aversion, which is the same idea as our concept of partial asset integration over a-temporal w and y .

A second broad set of issues is the characterization of behavior when portfolio choices are disaggregated, and when they are integrated with consumption and leisure choices. Within the field of insurance economics, Mayers and Smith [1983] and Doherty [1984] have stressed the confounding effect that allowing for non-traded assets can have on the demand for insurance. For example, if risks in one domain are perfectly correlated with risks in another domain, but traded insurance is only available in one domain, the rational risk-averse agent would tend to “over-insure.” A large part of the theory of risk management derives from the complementarity and substitutability of “self protection” and “self insurance” activities with formal insurance purchases identified by Ehrlich and Becker [1972]. The joint modeling of consumption behavior, leisure demand and

²⁰ For multivariate risk aversion, the Hessian should be negative semidefinite under the Kihlstrom and Mirman [1974] definition. For positive r , our utility function (3') is a composition of increasing, concave functions, hence its Hessian is negative semidefinite. Applying the matrix-valued measures of Duncan [1977] and Karni [1979] would be more involved, of course.

portfolio choices begun, with non-additive utility functions, by Cox [1975] and Ingersoll [1992], identifies numerous avenues for testable propositions about the unexpected spillover effects of policy interventions. There is also a large literature on the effects of consumption “commitments” on behavior towards risk, starting with Grossman and Laroque [1990] and applied directly to the issue of risk calibration by Chetty and Szeidl [2007]. Finally, the partial asset integration approach could provide a rigorous bridge to characterizing the manner in which decision makers employ “mental accounts” to structure the tradeoffs between components of w and y , in the spirit of Thaler [1985] and Thaler and Johnson [1990].²¹ The hypothesis of mental accounts involves testable statements about the nested nature of substitutability between different components of w and/or y , and the possibility that ω is context dependent. Once we consider a wider range of stakes, both for income and wealth, there are many ways of characterizing the relationship between risk attitudes over these utility arguments. Such specifications are discussed in the broader literature on multivariate and multi-attribute risk aversion.

A third set of broad issues has to do with the treatment of wealth as being deterministic and

²¹ Thaler and Johnson [1990] focused directly on the question of how risk-taking behavior is affected by prior gains or losses, and do not directly consider integration with wealth. But the issues they examine with respect to the “components of y ” have direct application to the generalization we propose. They view choices from the perspective of Prospect Theory (PT), but allow for interesting variations in the manner in which the “editing phase” of PT is applied. They provide (p.646) a simple example in which the subject is told that they have just won \$30, and must then choose between (a) no further gain or loss, or (b) a 50-50 chance of winning \$9 or losing \$9. Three representations of this problem are suggested: (a) $u(\$21) + w^{1/2} [u(\$39) - u(\$21)]$; (b) $u(\$30) + w^{1/2} u(\$9) + w^{1/2} u(-\$9)$; and (c) $u(\$21) + w^{1/2} u(\$18)$. The representation in (a) assumes that prior outcomes are embedded into the choice problem. In effect, it adds “memory” to the standard PT representation of the task, and then applies the PT editing rule that the prospect is broken into the certain part and then the residual uncertain part (Kahneman and Tversky [1979; p.276]). The representation in (b) assumes that prior outcomes, in this case the \$30 of cumulative income, has no effect on the framing of the task. This is the standard PT formulation. The difference between (a) and (b) has something of the flavor of the asset integration parameter ω that we introduced. But it also has something of the flavor of an endogenous reference point for PT. The representation in (c) assumes that subjects actively deform the prospect to make it appear more attractive. Thus the possibility of a \$9 loss is integrated into the \$30 on hand, to be evaluated as a certain \$21, and the risky part of the gamble is evaluated as a potential gain of \$18.

known, while experimental income is stochastic by experimental design. Although consistent with the manner in which asset integration is discussed in the literature, our PAI approach formally allows for there to be a joint probability distribution over wealth and experimental income. An important extension would be to elicit subjective beliefs from individuals about the value of their net wealth at the time of the experiment (or as of some very recent date). After all, who knows with certainty the current value of their net wealth? Since the correlation between subjective beliefs about own-wealth and experimental income is zero, again by design, one can just elicit beliefs about wealth (Harrison, Martínez-Correa, Swarthout and Ulm [2017]) and then construct the joint distribution as a mixture of subjective beliefs about own-wealth and objective probabilities in the experimental lotteries.

This extension connects our approach to the logic of Barberis, Huang and Thaler [2006], who emphasize the role of risks from gambles such as one confronts in an experiment being merged with pre-existing risks from extra-experimental income or wealth. If the risks in the experimental lottery are independent of these pre-existing risks, the diversification benefits of the combination might offset any first-order risk aversion towards the experimental lottery evaluated in isolation. Barberis, Huang and Thaler [2006] then posit that the individual evaluates small-stakes gambles in isolation, and is driven to exhibit first-order risk aversion, but that the same agent evaluates large-stakes gambles as part of this broader portfolio, tempering the small-stakes risk aversion. Our approach does not require this state-dependent utility specification to account for small-stakes risks and large-stakes risks, although we certainly agree that the riskiness of wealth and experimental income ought to be considered jointly in a complete treatment.

This extension also connects our approach to the logic of Kőszegi and Rabin [2007], who consider the implications of loss aversion relative to a *stochastic reference point*, defined in terms of *subjective beliefs* about outcomes of the lottery. Recognizing that “... relatively little evidence on the

determinants of reference points currently exists,” (p. 1051), they make this notion operational by assuming that individuals use the EV of the lottery as their subjective belief about the lottery outcome. Our approach immediately extends to include this specification, since we formally allow a joint probability distribution over wealth and experimental income.

The theme of these comments is that our approach is much more general than the resolution of a puzzle about the calibration of choices over risky y in the lab when one takes into account extra lab w . In effect, the rigorous evaluation of seemingly arcane calibration puzzles via models of partial asset integration opens up many areas for research that have tended to be neglected in the calibration debate.

6. Conclusions

The experimental behavior of adult Danes that have any personal wealth is consistent with partial asset integration, in the dual sense that they behave as if some fraction of personal wealth is combined with experimental prizes in a utility function, and that the combination entails less than perfect substitution. Of course, those who have no wealth cannot, as a matter of definition, integrate it with experimental income. Overall, we conclude that our subjects do not perfectly asset integrate.

The implied risk attitudes from estimating these partial asset integration specifications imply risk premia and certainty equivalents under EUT that are *a priori* plausible when confronted with the payoff calibration paradox. Hence our EUT-PAI specification is promising, by surviving the payoff calibration paradox.

Extending the analysis to an RDU model, we find evidence of modest probability weighting and diminishing marginal utility under partial asset integration. Only when one insists *a priori*, and contrary to the inferences we draw about behavior, that decisions are best characterized with full

asset integration does probability weighting come to dominate the characterization of risk attitudes over experimental payoffs. Nonetheless, the RDU-PAI specification also seem to survive the payoff calibration paradox.

These are constructive solutions to the payoff calibration paradoxes. In addition, the rigorous, structural modeling of partial asset integration points to a rich array of neglected questions in risk management and policy evaluation in important field settings.

Table 1: Individual Wealth in Denmark

All currency values in Danish Kroner (1 DKK \approx \$6.643 in September 2015).
 All valuations as of December 31, 2014, except for Automobiles, which has a 1 year lag.

Variable	Mean	Median	Std. Dev.
Total assets	3,844,104	2,985,522	4,521,335
Real estate	1,427,395	1,000,828	2,734,828
Shares and mutual funds	185,023	2,859	562,243
Bonds	4,006	0	28,118
Assets in financial institutions	186,747	65,762	311,192
Pensions	1,969,176	1,162,490	2,504,648
Automobiles	71,758	27,400	105,166
Total liabilities	769,426	352,192	2,212,928
Debt in financial institutions	190,558	26,133	439,769
Mortgages	578,869	0	2,023,922
Net wealth	3,074,678	2,165,847	3,470,853
Net wealth truncated at zero	3,097,435	2,165,847	3,439,401

Note: Total assets are the market value of domestic real estate, shares and mutual funds, bonds, assets deposited in domestic and foreign financial institutions, pensions and the value of automobiles. Total liabilities are the value of debt in domestic and foreign financial institutions and mortgages. All values of shares, bonds and pensions are reported by financial institutions as of December 31st. Values of real estate are estimated by Statistics Denmark as the market value on December 31st. The value of automobiles is calculated with a one-year lag. All foreign assets and debt are self reported and equal to 0 for every subject in the sample. All values are in 2015 Danish kroner, and values are reported for the full sample of 442 subjects.

Figure 1: Predicted Probability of a Safe Choice by Adult Danes at Varying Levels of Lab Wealth

N=220 subjects each making 6 choices for varying lab wealth
Predictions from a Random Effects Panel Probit model

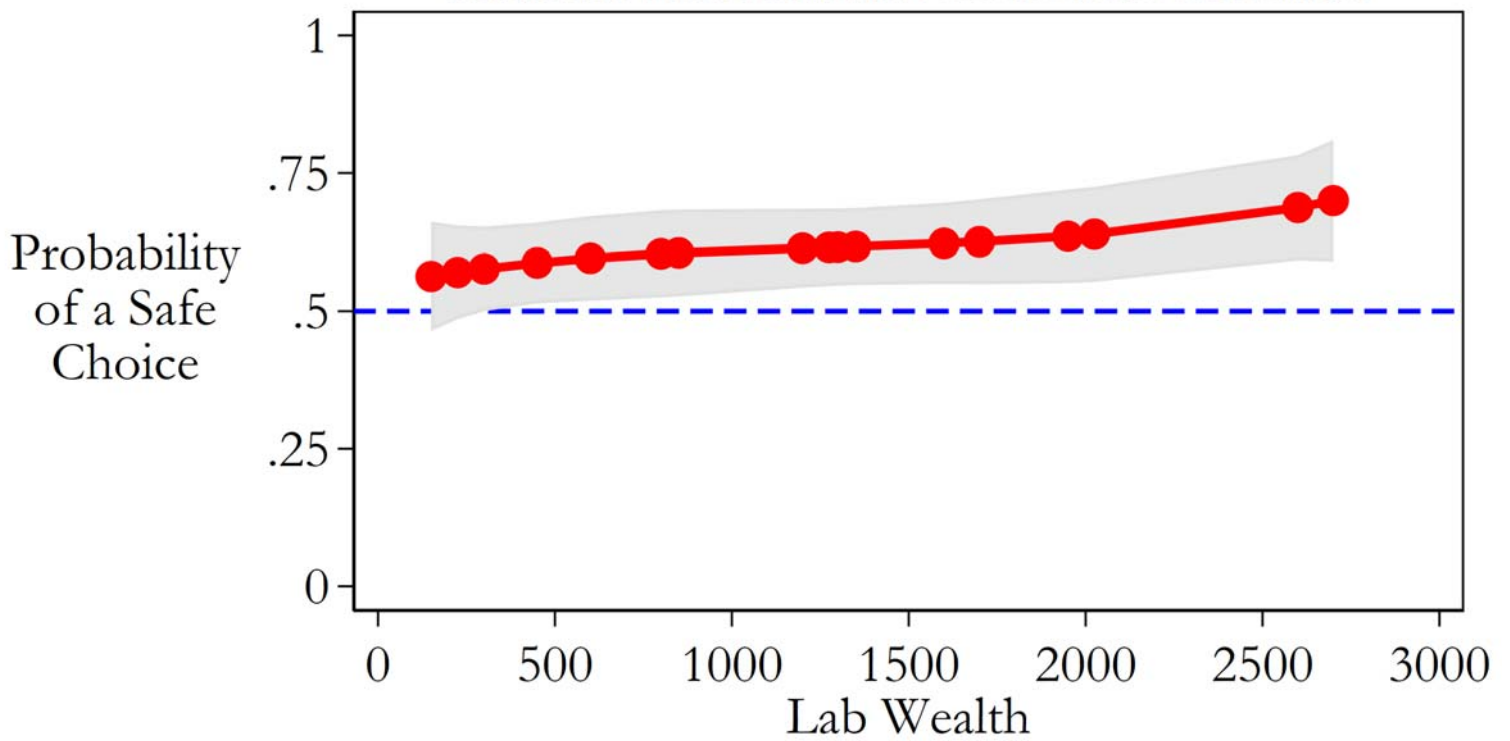


Table 2: Estimates and Implied Certainty-Equivalents Using EUT-PAI Model

A. Estimates

Sample of 442 individuals making 26,520 choices of strict preference
 Log-Likelihood = -17,025 (-17,028 for NAI and -17,436 for FAI)
 Null hypothesis for p -value results is that the coefficient estimates is 0.

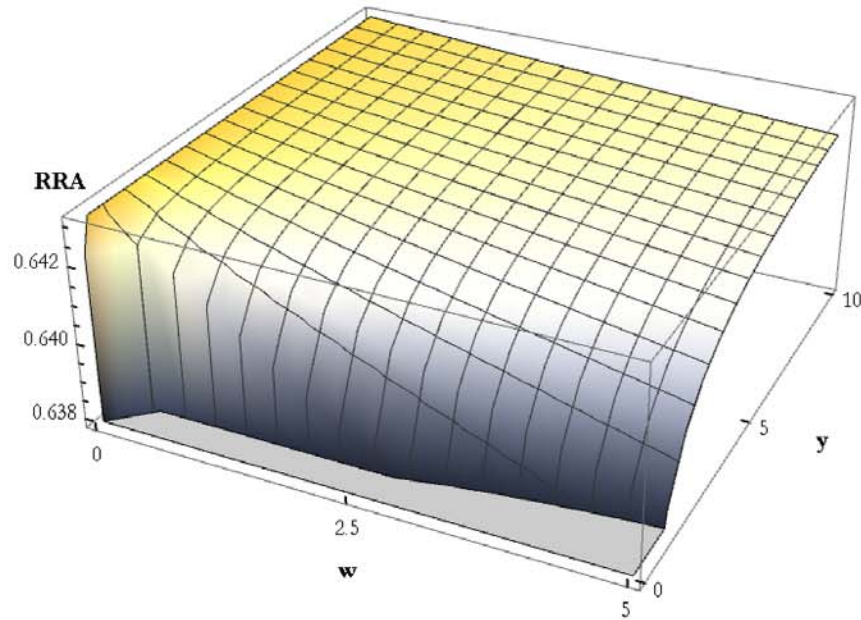
Parameter	Point Estimate	Standard Error	p -value	95% Confidence Interval	
r	0.64	0.04	<0.001	0.57	0.71
ρ	0.89	0.15	<0.001	0.6	1.19
ω	0.000006	0.00002	0.77	-0.00004	0.00005
μ	0.08	0.005	<0.001	0.07	0.09

B. Certainty-Equivalent Calculations with Average Wealth

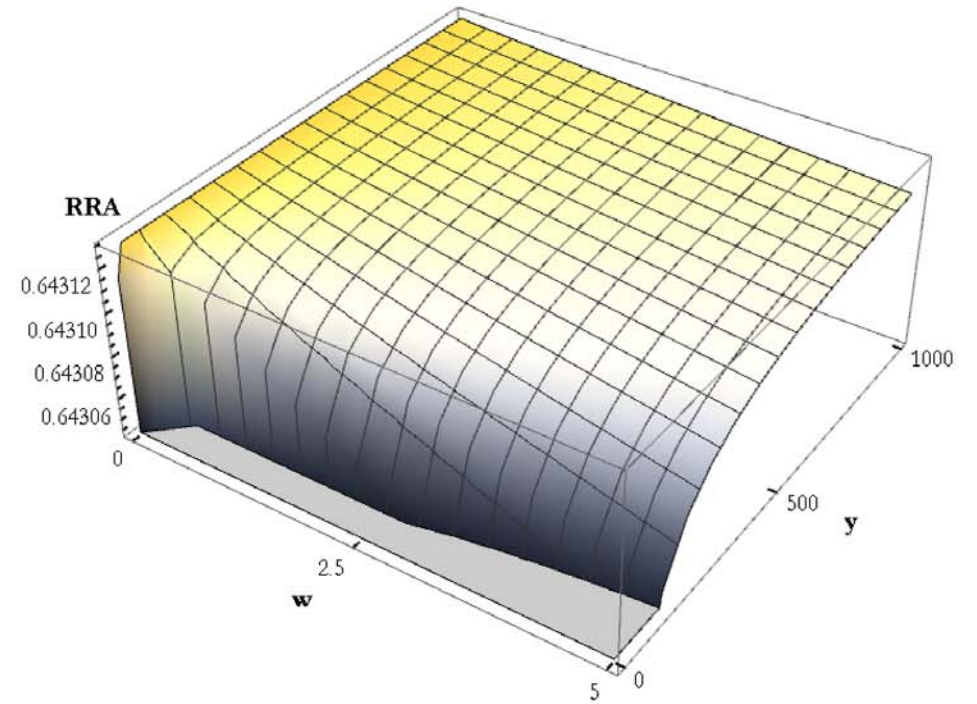
High Prize (DKK)	Probability of High Prize	Low Prize (DKK)	Expected Value (DKK)	Certainty Equivalent (DKK)	Ratio
200	0.5	100	150	145	0.965
500	0.5	100	300	252	0.84
1000	0.5	100	550	402	0.73
2000	0.5	100	1,050	663	0.631
5000	0.5	100	2,550	1,350	0.529
5000	0.01	100	149	109	0.732
5000	0.1	100	590	214	0.362
5000	0.3	100	1,570	626	0.399
5000	0.7	100	3,530	2,459	0.697
5000	0.9	100	4,510	4,025	0.892

Figure 2: Arrow-Pratt Relative Risk Aversion for Estimated EUT-PAI Model

A. Low Stakes



Income measured in thousands of kroner
Wealth measured in millions of kroner



B. High Stakes

Table 3: Estimates Using RDU-PAI Model

A. Estimates

Sample of 442 individuals making 26,520 choices of strict preference
 Log-Likelihood = -16,973 (-16,976 for NAI and -17,049 for FAI)
 Null hypothesis for p -value results is that the coefficient estimates is 0.

Parameter	Point Estimate	Standard Error	p -value	95% Confidence Interval	
r	0.48	0.05	<0.001	0.38	0.57
η	1.12	0.04	<0.001	1.04	1.2
φ	0.84	0.02	<0.001	0.8	0.88
ω	0.0000106	0.00001	0.46	-0.00002	0.00001
ϱ	1	0.00003	<0.001	0.999	1
μ	0.1	0.005	<0.001	0.09	0.11

B. Certainty-Equivalent Calculations with Average Wealth

Large Prize (DKK)	Probability of Large Prize	Small Prize (DKK)	Expected Value (DKK)	Certainty Equivalent (DKK)	Ratio
200	0.5	100	150	141	0.937
500	0.5	100	300	244	0.813
1,000	0.5	100	550	395	0.717
2,000	0.5	100	1,050	668	0.636
5,000	0.5	100	2,550	1,418	0.556
5,000	0.01	100	149	126	0.848
5,000	0.1	100	590	290	0.492
5,000	0.3	100	1,570	751	0.478
5,000	0.7	100	3,530	2,371	0.672
5,000	0.9	100	4,510	3,800	0.842

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ONLINE APPENDICES

Asset Integration and Attitudes to Risk: Theory and Evidence

by

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Appendix A: Experimental Lottery Parameters

The variable “qid” is the label used to identify the specific lottery pair. The initial stem of the qid defines the type of lottery, as explained in the text:

1. The Loomes and Sugden [1998] lotteries are defined by the text “ls”;
2. The Wakker, Erev and Weber [1994] lotteries are defined by the text “rWEW”;
3. The calibration premise lotteries, from the logic of Cox and Sadiraj [2008], are defined here by the text “CandS” (they are referred to by the text “wilcox” in our data files).
4. The Wilcox [2015] lotteries are defined by the text “r” followed immediately by a number.

All prizes are defined in Danish kroner. Each subject was presented with all 60 binary choices, presented in a random order that varied from subject to subject.

Wakker, Erev and Weber [1994] constructed lotteries to carefully test the “comonotonic independence” axiom of RDU. Their main lottery pairs consist of 6 sets of 4 pairs. The logic of their design can be understood by considering the first set, from Wakker, Erev and Weber [1994; Figure 3.1]. The second and third prizes in each pair stay the same within the set of 4 choice pairs. The only thing that varies from pair to pair is the monetary value of the first prize, and that is common to the “safe” and “risky” lottery within each pair. Since the first listed prize is a common consequence in both lotteries within a pair, it should not affect choices under EUT. In the 1st pair the first prize is only \$0.50, and is the lowest ranked prize for both lotteries. The first prize increases to \$3.50 for the 2nd pair, and is again the lowest ranked prize for both lotteries: so rank-dependence should have no effect on choice patterns as the subject moves from the 1st to the 2nd pair. But when we come to the 3rd pair the first prize is \$6.50, which makes it the second highest ranked prize for both lotteries; this is where RDU *could* have a different prediction than EUT, depending on the extent and nature of probability weighting. Finally, in the 4th pair the common consequence is the highest ranked prize for both lotteries, again *allowing* RDU to predict something different from EUT (and from the choices in the 3rd pair). Note that this design does not formally *require* an RDU decision-maker to choose differently than an EUT decision-maker; it simply encourages it for *a priori* reasonable levels of probability weighting. We employ all 24 of their main lottery pairs, and scale the prizes up considerably.

Loomes and Sugden [1998] pose an important design feature for common ratio tests: variation in the “gradient” of the EUT-consistent indifference curves within a Marschak-Machina (MM) triangle. The reason is to generate some choice patterns that are more powerful tests of EUT for any given risk attitude. Under EUT the slope of the indifference curve within a MM triangle is a measure of risk aversion. So there always exists some risk attitude such that the subject is indifferent, and evidence of common ratio violations has virtually zero power; their logic avoids this problem. We use 30 lottery pairs from their design, with slightly different prizes.

Wilcox [2015] designed lottery tasks for the purpose of robust estimation of EUT and RDU models at the level of the individual. These lottery pairs span five monetary prize amounts, 300, 600, 1200, 2100 and 4200 kroner, and five probabilities, 0, 1/4, 1/2, 3/4 and 1. The prizes are combined in ten “contexts,” defined as a particular triple of prizes. These lotteries also contain a number of pairs in which the “EUT-safe” lottery has a *higher* EV than the “EUT-risky” lottery: this is designed

deliberately to evaluate the extent of risk premia deriving from probability pessimism rather than diminishing marginal utility. Wilcox [2015] documents a wide variety of probability weighting functions from choices from his complete battery, based on estimates at the individual level. We use 36 lottery pairs from his wider battery.

A final battery of 6 lottery pairs is designed to test the premise of the calibration puzzle posed by Hansson [1988] and Rabin [2000], using the logic proposed by Cox and Sadiraj [2008; p.33].

Batteries I, II and III are the same lotteries apart from a scaling of prizes, and were jointly administered to 220 subjects. Battery IV was administered to the remaining 222 subjects.

Table A1 shows the parameters of the test of the calibration premiss in a format that is easy to read. As explained in the text, the Risky lotteries were presented to subjects in the gain domain, adding or subtracting from the Lab Wealth value shown, rather than as positive or negative deviations from what we refer to as Lab Wealth.

Table A1: Experimental Parameters for Test of Calibration Premis
All currency values in Danish Kroner (1 DKK \approx \$6.643 in September 2015).

Lab Wealth	Risky Lottery		
	Loss	Gain	Expected Value
150	-80	90	155
225	-120	135	232.5
300	-150	160	305
300	-160	180	310
450	-225	240	457.5
600	-300	320	610
800	-150	160	805
850`	-80	90	855
1200	-225	240	1207.5
1275	-120	135	1282.5
1300	-150	160	1305
1350	-80	90	1355
1600	-300	320	1610
1700	-160	180	1710
1950	-225	240	1957.5
2025	-120	135	2032.5
2600	-300	320	2610
2700	-160	180	2710

Battery I: Low Scale

qid	prizeA1	probA1	prizeA2	probA2	prizeA3	probA3	prizeB1	probB1	prizeB2	probB2	prizeB3	probB3
ls10	300	.5	900	0	1500	.5	300	.1	900	.8	1500	.1
ls11	300	.5	900	0	1500	.5	300	0	900	1	1500	0
ls12	300	.1	900	.8	1500	.1	300	0	900	1	1500	0
ls13	300	.7	900	0	1500	.3	300	.5	900	.4	1500	.1
ls14	300	.7	900	0	1500	.3	300	.4	900	.6	1500	0
ls15	300	.5	900	.4	1500	.1	300	.4	900	.6	1500	0
ls18	300	.4	900	0	1500	.6	300	.1	900	.75	1500	.15
ls19	300	.4	900	0	1500	.6	300	0	900	1	1500	0
ls2	300	.3	900	0	1500	.7	300	.15	900	.25	1500	.6
ls20	300	.1	900	.75	1500	.15	300	0	900	1	1500	0
ls21	300	.7	900	0	1500	.3	300	.6	900	.25	1500	.15
ls22	300	.7	900	0	1500	.3	300	.5	900	.5	1500	0
ls23	300	.6	900	.25	1500	.15	300	.5	900	.5	1500	0
ls26	300	.4	900	0	1500	.6	300	.2	900	.6	1500	.2
ls27	300	.4	900	0	1500	.6	300	.1	900	.9	1500	0
ls28	300	.2	900	.6	1500	.2	300	.1	900	.9	1500	0
ls29	300	.6	900	0	1500	.4	300	.5	900	.3	1500	.2
ls3	300	.3	900	0	1500	.7	300	0	900	.5	1500	.5
ls30	300	.6	900	0	1500	.4	300	.4	900	.6	1500	0
ls31	300	.5	900	.3	1500	.2	300	.4	900	.6	1500	0
ls34	300	.25	900	0	1500	.75	300	.1	900	.6	1500	.3
ls35	300	.25	900	0	1500	.75	300	0	900	1	1500	0
ls36	300	.1	900	.6	1500	.3	300	0	900	1	1500	0
ls37	300	.5	900	.2	1500	.3	300	.4	900	.6	1500	0
ls38	300	.55	900	0	1500	.45	300	.4	900	.6	1500	0
ls39	300	.55	900	0	1500	.45	300	.5	900	.2	1500	.3
ls4	300	.15	900	.25	1500	.6	300	0	900	.5	1500	.5
ls5	300	.15	900	.75	1500	.1	300	0	900	1	1500	0
ls6	300	.6	900	0	1500	.4	300	0	900	1	1500	0
ls7	300	.6	900	0	1500	.4	300	.15	900	.75	1500	.1
rWEW1	90	.55	1080	.25	1260	.2	90	.55	810	.25	1620	.2
rWEW10	450	.4	540	.4	1080	.2	270	.4	540	.4	1350	.2
rWEW11	450	.4	990	.4	1080	.2	270	.4	990	.4	1350	.2
rWEW12	450	.4	1080	.2	1440	.4	270	.4	1350	.2	1440	.4
rWEW13	450	.7	990	.1	1890	.2	450	.7	630	.1	2250	.2
rWEW14	990	.1	1080	.7	1890	.2	630	.1	1080	.7	2250	.2
rWEW15	990	.1	1710	.7	1890	.2	630	.1	1710	.7	2250	.2
rWEW16	990	.1	1890	.2	2340	.7	630	.1	2250	.2	2340	.7
rWEW17	0	.5	360	.1	360	.4	0	.5	0	.1	540	.4
rWEW18	360	.5	360	.1	360	.4	0	.1	360	.5	540	.4
rWEW19	360	.1	360	.4	720	.5	0	.1	540	.4	720	.5
rWEW2	630	.55	1080	.25	1260	.2	630	.55	810	.25	1620	.2
rWEW20	360	.1	360	.4	1080	.5	0	.1	540	.4	1080	.5
rWEW21	360	.5	720	.1	720	.4	360	.5	360	.1	900	.4
rWEW22	720	.5	720	.1	720	.4	360	.1	720	.5	900	.4
rWEW23	720	.1	720	.4	1080	.5	360	.1	900	.4	1080	.5
rWEW24	720	.1	720	.4	1440	.5	360	.1	900	.4	1440	.5
rWEW3	1080	.25	1170	.55	1260	.2	810	.25	1170	.55	1620	.2
rWEW4	1080	.25	1260	.2	1710	.55	810	.25	1620	.2	1710	.55
rWEW5	90	.65	630	.2	990	.15	90	.65	540	.2	1080	.15
rWEW6	450	.65	630	.2	990	.15	450	.65	540	.2	1080	.15
rWEW7	630	.2	990	.15	810	.65	540	.2	810	.65	1080	.15
rWEW8	630	.2	990	.15	1170	.65	540	.2	1080	.15	1170	.65
rWEW9	90	.4	450	.4	1080	.2	90	.4	270	.4	1350	.2
CandS12	0	0	0	0	800	1	0	0	650	.5	960	.5
CandS14	0	0	0	0	850	1	0	0	770	.5	940	.5
CandS2	0	0	0	0	300	1	0	0	150	.5	460	.5
CandS22	0	0	0	0	1300	1	0	0	1150	.5	1460	.5
CandS24	0	0	0	0	1350	1	0	0	1270	.5	1440	.5
CandS4	0	0	0	0	150	1	0	0	70	.5	240	.5

Battery II: Middle Scale

qid	prizeA1	probA1	prizeA2	probA2	prizeA3	probA3	prizeB1	probB1	prizeB2	probB2	prizeB3	probB3
ls10	450	.5	1350	0	2250	.5	450	.1	1350	.8	2250	.1
ls11	450	.5	1350	0	2250	.5	450	0	1350	1	2250	0
ls12	450	.1	1350	.8	2250	.1	450	0	1350	1	2250	0
ls13	450	.7	1350	0	2250	.3	450	.5	1350	.4	2250	.1
ls14	450	.7	1350	0	2250	.3	450	.4	1350	.6	2250	0
ls15	450	.5	1350	.4	2250	.1	450	.4	1350	.6	2250	0
ls18	450	.4	1350	0	2250	.6	450	.1	1350	.75	2250	.15
ls19	450	.4	1350	0	2250	.6	450	0	1350	1	2250	0
ls2	450	.3	1350	0	2250	.7	450	.15	1350	.25	2250	.6
ls20	450	.1	1350	.75	2250	.15	450	0	1350	1	2250	0
ls21	450	.7	1350	0	2250	.3	450	.6	1350	.25	2250	.15
ls22	450	.7	1350	0	2250	.3	450	.5	1350	.5	2250	0
ls23	450	.6	1350	.25	2250	.15	450	.5	1350	.5	2250	0
ls26	450	.4	1350	0	2250	.6	450	.2	1350	.6	2250	.2
ls27	450	.4	1350	0	2250	.6	450	.1	1350	.9	2250	0
ls28	450	.2	1350	.6	2250	.2	450	.1	1350	.9	2250	0
ls29	450	.6	1350	0	2250	.4	450	.5	1350	.3	2250	.2
ls3	450	.3	1350	0	2250	.7	450	0	1350	.5	2250	.5
ls30	450	.6	1350	0	2250	.4	450	.4	1350	.6	2250	0
ls31	450	.5	1350	.3	2250	.2	450	.4	1350	.6	2250	0
ls34	450	.25	1350	0	2250	.75	450	.1	1350	.6	2250	.3
ls35	450	.25	1350	0	2250	.75	450	0	1350	1	2250	0
ls36	450	.1	1350	.6	2250	.3	450	0	1350	1	2250	0
ls37	450	.5	1350	.2	2250	.3	450	.4	1350	.6	2250	0
ls38	450	.55	1350	0	2250	.45	450	.4	1350	.6	2250	0
ls39	450	.55	1350	0	2250	.45	450	.5	1350	.2	2250	.3
ls4	450	.15	1350	.25	2250	.6	450	0	1350	.5	2250	.5
ls5	450	.15	1350	.75	2250	.1	450	0	1350	1	2250	0
ls6	450	.6	1350	0	2250	.4	450	0	1350	1	2250	0
ls7	450	.6	1350	0	2250	.4	450	.15	1350	.75	2250	.1
rWEW1	135	.55	1620	.25	1890	.2	135	.55	1215	.25	2430	.2
rWEW10	675	.4	810	.4	1620	.2	405	.4	810	.4	2025	.2
rWEW11	675	.4	1485	.4	1620	.2	405	.4	1485	.4	2025	.2
rWEW12	675	.4	1620	.2	2160	.4	405	.4	2025	.2	2160	.4
rWEW13	675	.7	1485	.1	2835	.2	675	.7	945	.1	3375	.2
rWEW14	1485	.1	1620	.7	2835	.2	945	.1	1620	.7	3375	.2
rWEW15	1485	.1	2565	.7	2835	.2	945	.1	2565	.7	3375	.2
rWEW16	1485	.1	2835	.2	3510	.7	945	.1	3375	.2	3510	.7
rWEW17	0	.5	540	.1	540	.4	0	.5	0	.1	810	.4
rWEW18	540	.5	540	.1	540	.4	0	.1	540	.5	810	.4
rWEW19	540	.1	540	.4	1080	.5	0	.1	810	.4	1080	.5
rWEW2	945	.55	1620	.25	1890	.2	945	.55	1215	.25	2430	.2
rWEW20	540	.1	540	.4	1620	.5	0	.1	810	.4	1620	.5
rWEW21	540	.5	1080	.1	1080	.4	540	.5	540	.1	1350	.4
rWEW22	1080	.5	1080	.1	1080	.4	540	.1	1080	.5	1350	.4
rWEW23	1080	.1	1080	.4	1620	.5	540	.1	1350	.4	1620	.5
rWEW24	1080	.1	1080	.4	2160	.5	540	.1	1350	.4	2160	.5
rWEW3	1620	.25	1755	.55	1890	.2	1215	.25	1755	.55	2430	.2
rWEW4	1620	.25	1890	.2	2565	.55	1215	.25	2430	.2	2565	.55
rWEW5	135	.65	945	.2	1485	.15	135	.65	810	.2	1620	.15
rWEW6	675	.65	945	.2	1485	.15	675	.65	810	.2	1620	.15
rWEW7	945	.2	1485	.15	1215	.65	810	.2	1215	.65	1620	.15
rWEW8	945	.2	1485	.15	1755	.65	810	.2	1620	.15	1755	.65
rWEW9	135	.4	675	.4	1620	.2	135	.4	405	.4	2025	.2
CandS12	0	0	0	0	1200	1	0	0	975	.5	1440	.5
CandS14	0	0	0	0	1275	1	0	0	1155	.5	1410	.5
CandS2	0	0	0	0	450	1	0	0	225	.5	690	.5
CandS22	0	0	0	0	1950	1	0	0	1725	.5	2190	.5
CandS24	0	0	0	0	2025	1	0	0	1905	.5	2160	.5
CandS4	0	0	0	0	225	1	0	0	105	.5	360	.5

Battery III: High Scale

qid	prizeA1	probA1	prizeA2	probA2	prizeA3	probA3	prizeB1	probB1	prizeB2	probB2	prizeB3	probB3
ls10	600	.5	1800	0	3000	.5	600	.1	1800	.8	3000	.1
ls11	600	.5	1800	0	3000	.5	600	0	1800	1	3000	0
ls12	600	.1	1800	.8	3000	.1	600	0	1800	1	3000	0
ls13	600	.7	1800	0	3000	.3	600	.5	1800	.4	3000	.1
ls14	600	.7	1800	0	3000	.3	600	.4	1800	.6	3000	0
ls15	600	.5	1800	.4	3000	.1	600	.4	1800	.6	3000	0
ls18	600	.4	1800	0	3000	.6	600	.1	1800	.75	3000	.15
ls19	600	.4	1800	0	3000	.6	600	0	1800	1	3000	0
ls2	600	.3	1800	0	3000	.7	600	.15	1800	.25	3000	.6
ls20	600	.1	1800	.75	3000	.15	600	0	1800	1	3000	0
ls21	600	.7	1800	0	3000	.3	600	.6	1800	.25	3000	.15
ls22	600	.7	1800	0	3000	.3	600	.5	1800	.5	3000	0
ls23	600	.6	1800	.25	3000	.15	600	.5	1800	.5	3000	0
ls26	600	.4	1800	0	3000	.6	600	.2	1800	.6	3000	.2
ls27	600	.4	1800	0	3000	.6	600	.1	1800	.9	3000	0
ls28	600	.2	1800	.6	3000	.2	600	.1	1800	.9	3000	0
ls29	600	.6	1800	0	3000	.4	600	.5	1800	.3	3000	.2
ls3	600	.3	1800	0	3000	.7	600	0	1800	.5	3000	.5
ls30	600	.6	1800	0	3000	.4	600	.4	1800	.6	3000	0
ls31	600	.5	1800	.3	3000	.2	600	.4	1800	.6	3000	0
ls34	600	.25	1800	0	3000	.75	600	.1	1800	.6	3000	.3
ls35	600	.25	1800	0	3000	.75	600	0	1800	1	3000	0
ls36	600	.1	1800	.6	3000	.3	600	0	1800	1	3000	0
ls37	600	.5	1800	.2	3000	.3	600	.4	1800	.6	3000	0
ls38	600	.55	1800	0	3000	.45	600	.4	1800	.6	3000	0
ls39	600	.55	1800	0	3000	.45	600	.5	1800	.2	3000	.3
ls4	600	.15	1800	.25	3000	.6	600	0	1800	.5	3000	.5
ls5	600	.15	1800	.75	3000	.1	600	0	1800	1	3000	0
ls6	600	.6	1800	0	3000	.4	600	0	1800	1	3000	0
ls7	600	.6	1800	0	3000	.4	600	.15	1800	.75	3000	.1
rWEW1	180	.55	2160	.25	2520	.2	180	.55	1620	.25	3240	.2
rWEW10	900	.4	1080	.4	2160	.2	540	.4	1080	.4	2700	.2
rWEW11	900	.4	1980	.4	2160	.2	540	.4	1980	.4	2700	.2
rWEW12	900	.4	2160	.2	2880	.4	540	.4	2700	.2	2880	.4
rWEW13	900	.7	1980	.1	3780	.2	900	.7	1260	.1	4500	.2
rWEW14	1980	.1	2160	.7	3780	.2	1260	.1	2160	.7	4500	.2
rWEW15	1980	.1	3420	.7	3780	.2	1260	.1	3420	.7	4500	.2
rWEW16	1980	.1	3780	.2	4680	.7	1260	.1	4500	.2	4680	.7
rWEW17	0	.5	720	.1	720	.4	0	.5	0	.1	1080	.4
rWEW18	720	.5	720	.1	720	.4	0	.1	720	.5	1080	.4
rWEW19	720	.1	720	.4	1440	.5	0	.1	1080	.4	1440	.5
rWEW2	1260	.55	2160	.25	2520	.2	1260	.55	1620	.25	3240	.2
rWEW20	720	.1	720	.4	2160	.5	0	.1	1080	.4	2160	.5
rWEW21	720	.5	1440	.1	1440	.4	720	.5	720	.1	1800	.4
rWEW22	1440	.5	1440	.1	1440	.4	720	.1	1440	.5	1800	.4
rWEW23	1440	.1	1440	.4	2160	.5	720	.1	1800	.4	2160	.5
rWEW24	1440	.1	1440	.4	2880	.5	720	.1	1800	.4	2880	.5
rWEW3	2160	.25	2340	.55	2520	.2	1620	.25	2340	.55	3240	.2
rWEW4	2160	.25	2520	.2	3420	.55	1620	.25	3240	.2	3420	.55
rWEW5	180	.65	1260	.2	1980	.15	180	.65	1080	.2	2160	.15
rWEW6	900	.65	1260	.2	1980	.15	900	.65	1080	.2	2160	.15
rWEW7	1260	.2	1980	.15	1620	.65	1080	.2	1620	.65	2160	.15
rWEW8	1260	.2	1980	.15	2340	.65	1080	.2	2160	.15	2340	.65
rWEW9	180	.4	900	.4	2160	.2	180	.4	540	.4	2700	.2
CandS12	0	0	0	0	1600	1	0	0	1300	.5	1920	.5
CandS14	0	0	0	0	1700	1	0	0	1540	.5	1880	.5
CandS2	0	0	0	0	600	1	0	0	300	.5	920	.5
CandS22	0	0	0	0	2600	1	0	0	2300	.5	2920	.5
CandS24	0	0	0	0	2700	1	0	0	2540	.5	2880	.5
CandS4	0	0	0	0	300	1	0	0	140	.5	480	.5

Battery IV

qid	prizeA1	probA1	prizeA2	probA2	prizeA3	probA3	prizeB1	probB1	prizeB2	probB2	prizeB3	probB3
r11	300	0	600	1	2100	0	300	.5	600	0	2100	.5
r12	300	0	600	.75	2100	.25	300	.25	600	0	2100	.75
r13	300	.25	600	.75	2100	0	300	.75	600	0	2100	.25
r14	300	0	600	.5	2100	.5	300	.25	600	0	2100	.75
r15	300	0	600	.75	2100	.25	300	.5	600	0	2100	.5
r16	300	0	600	1	2100	0	300	.75	600	0	2100	.25
r17	300	.25	600	.75	4200	0	300	.5	600	0	4200	.5
r18	300	0	600	1	4200	0	300	.5	600	0	4200	.5
r19	300	.5	600	.5	4200	0	300	.75	600	0	4200	.25
r20	300	0	600	1	4200	0	300	.75	600	0	4200	.25
r21	300	0	1200	1	2100	0	300	.25	1200	0	2100	.75
r26	300	0	1200	.75	2100	.25	300	.5	1200	0	2100	.5
r28	300	.25	1200	.75	4200	0	300	.5	1200	0	4200	.5
r29	300	0	1200	.75	4200	.25	300	.25	1200	0	4200	.75
r30	300	.5	1200	.5	4200	0	300	.75	1200	0	4200	.25
r31	300	.25	1200	.5	4200	.25	300	.5	1200	0	4200	.5
r32	300	.25	1200	.75	4200	0	300	.75	1200	0	4200	.25
r33	300	0	1200	.5	4200	.5	300	.25	1200	0	4200	.75
r34	300	0	2100	1	4200	0	300	.25	2100	0	4200	.75
r35	300	.25	2100	.75	4200	0	300	.5	2100	0	4200	.5
r39	300	0	2100	.75	4200	.25	300	.5	2100	0	4200	.5
r40	300	.25	2100	.75	4200	0	300	.75	2100	0	4200	.25
r41	300	0	2100	1	4200	0	300	.75	2100	0	4200	.25
r42	600	0	1200	1	2100	0	600	.25	1200	0	2100	.75
r43	600	.25	1200	.75	2100	0	600	.5	1200	0	2100	.5
r44	600	0	1200	1	2100	0	600	.25	1200	.25	2100	.5
r50	600	0	1200	1	2100	0	600	.75	1200	0	2100	.25
r51	600	.25	1200	.75	4200	0	600	.5	1200	0	4200	.5
r52	600	.5	1200	.5	4200	0	600	.75	1200	0	4200	.25
r53	600	.25	1200	.75	4200	0	600	.75	1200	0	4200	.25
r54	600	0	2100	1	4200	0	600	.25	2100	0	4200	.75
r55	600	.25	2100	.75	4200	0	600	.5	2100	0	4200	.5
r58	1200	0	2100	1	4200	0	1200	.25	2100	0	4200	.75
r59	1200	.25	2100	.75	4200	0	1200	.5	2100	0	4200	.5
r60	1200	0	2100	.75	4200	.25	1200	.25	2100	0	4200	.75
r61	1200	0	2100	1	4200	0	1200	.5	2100	0	4200	.5
rWEW1	180	.55	2160	.25	2520	.2	180	.55	1620	.25	3240	.2
rWEW10	900	.4	1080	.4	2160	.2	540	.4	1080	.4	2700	.2
rWEW11	900	.4	1980	.4	2160	.2	540	.4	1980	.4	2700	.2
rWEW12	900	.4	2160	.2	2880	.4	540	.4	2700	.2	2880	.4
rWEW13	900	.7	1980	.1	3780	.2	900	.7	1260	.1	4500	.2
rWEW14	1980	.1	2160	.7	3780	.2	1260	.1	2160	.7	4500	.2
rWEW15	1980	.1	3420	.7	3780	.2	1260	.1	3420	.7	4500	.2
rWEW16	1980	.1	3780	.2	4680	.7	1260	.1	4500	.2	4680	.7
rWEW17	0	.5	720	.5	0	0	0	.6	0	0	1080	.4
rWEW18	0	0	720	1	0	0	0	.1	720	.5	1080	.4
rWEW19	720	.5	0	0	1440	.5	0	.1	1080	.4	1440	.5
rWEW2	1260	.55	2160	.25	2520	.2	1260	.55	1620	.25	3240	.2
rWEW20	720	.5	0	0	2160	.5	0	.1	1080	.4	2160	.5
rWEW21	720	.5	1440	.5	0	0	720	.6	0	0	1800	.4
rWEW22	1440	1	0	0	0	0	720	.1	1440	.5	1800	.4
rWEW23	1440	.5	0	0	2160	.5	720	.1	1800	.4	2160	.5
rWEW24	1440	.5	0	0	2880	.5	720	.1	1800	.4	2880	.5
rWEW3	2160	.25	2340	.55	2520	.2	1620	.25	2340	.55	3240	.2
rWEW4	2160	.25	2520	.2	3420	.55	1620	.25	3240	.2	3420	.55
rWEW5	180	.65	1260	.2	1980	.15	180	.65	1080	.2	2160	.15
rWEW6	900	.65	1260	.2	1980	.15	900	.65	1080	.2	2160	.15
rWEW7	1260	.2	1980	.15	1620	.65	1080	.2	1620	.65	2160	.15
rWEW8	1260	.2	1980	.15	2340	.65	1080	.2	2160	.15	2340	.65
rWEW9	180	.4	900	.4	2160	.2	180	.4	540	.4	2700	.2

In Battery I there are 37 distinct prizes, with positive probability, for each subject and 16 distinct probabilities, distributed as follows:

prize	Freq.	Percent	Cum.
0	6	2.04	2.04
70	1	0.34	2.38
90	6	2.04	4.42
150	2	0.68	5.10
240	1	0.34	5.44
270	4	1.36	6.80
300	51	17.35	24.15
360	16	5.44	29.59
450	8	2.72	32.31
460	1	0.34	32.65
540	10	3.40	36.05
630	10	3.40	39.46
650	1	0.34	39.80
720	12	4.08	43.88
770	1	0.34	44.22
800	1	0.34	44.56
810	6	2.04	46.60
850	1	0.34	46.94
900	44	14.97	61.90
940	1	0.34	62.24
960	1	0.34	62.59
990	10	3.40	65.99
1080	18	6.12	72.11
1150	1	0.34	72.45
1170	4	1.36	73.81
1260	4	1.36	75.17
1270	1	0.34	75.51
1300	1	0.34	75.85
1350	5	1.70	77.55
1440	5	1.70	79.25
1460	1	0.34	79.59
1500	42	14.29	93.88
1620	4	1.36	95.24
1710	4	1.36	96.60
1890	4	1.36	97.96
2250	4	1.36	99.32
2340	2	0.68	100.00
Total	294	100.00	

prob	Freq.	Percent	Cum.
.1	38	12.93	12.93
.15	16	5.44	18.37
.2	40	13.61	31.97
.25	14	4.76	36.73
.3	12	4.08	40.82
.4	48	16.33	57.14
.45	2	0.68	57.82
.5	46	15.65	73.47
.55	10	3.40	76.87
.6	22	7.48	84.35
.65	8	2.72	87.07
.7	14	4.76	91.84
.75	6	2.04	93.88
.8	2	0.68	94.56
.9	2	0.68	95.24
1	14	4.76	100.00
Total	294	100.00	

The same pattern applies to Battery II and Battery III, where the prize levels are multiplied by 1.5 and 2, respectively. The probability batteries are therefore identical to Battery I, above. In Battery II there is this distribution of (37) distinct prizes:

prize	Freq.	Percent	Cum.
0	6	2.04	2.04
105	1	0.34	2.38
135	6	2.04	4.42
225	2	0.68	5.10

360	1	0.34	5.44
405	4	1.36	6.80
450	51	17.35	24.15
540	16	5.44	29.59
675	8	2.72	32.31
690	1	0.34	32.65
810	10	3.40	36.05
945	10	3.40	39.46
975	1	0.34	39.80
1080	12	4.08	43.88
1155	1	0.34	44.22
1200	1	0.34	44.56
1215	6	2.04	46.60
1275	1	0.34	46.94
1350	44	14.97	61.90
1410	1	0.34	62.24
1440	1	0.34	62.59
1485	10	3.40	65.99
1620	18	6.12	72.11
1725	1	0.34	72.45
1755	4	1.36	73.81
1890	4	1.36	75.17
1905	1	0.34	75.51
1950	1	0.34	75.85
2025	5	1.70	77.55
2160	5	1.70	79.25
2190	1	0.34	79.59
2250	42	14.29	93.88
2430	4	1.36	95.24
2565	4	1.36	96.60
2835	4	1.36	97.96
3375	4	1.36	99.32
3510	2	0.68	100.00

Total	294	100.00	

In Battery III there are these tabulations of prizes:

prize	Freq.	Percent	Cum.
0	6	2.04	2.04
140	1	0.34	2.38
180	6	2.04	4.42
300	2	0.68	5.10
480	1	0.34	5.44
540	4	1.36	6.80
600	51	17.35	24.15
720	16	5.44	29.59
900	8	2.72	32.31
920	1	0.34	32.65
1080	10	3.40	36.05
1260	10	3.40	39.46
1300	1	0.34	39.80
1440	12	4.08	43.88
1540	1	0.34	44.22
1600	1	0.34	44.56
1620	6	2.04	46.60
1700	1	0.34	46.94
1800	44	14.97	61.90
1880	1	0.34	62.24
1920	1	0.34	62.59
1980	10	3.40	65.99
2160	18	6.12	72.11
2300	1	0.34	72.45
2340	4	1.36	73.81
2520	4	1.36	75.17
2540	1	0.34	75.51
2600	1	0.34	75.85
2700	5	1.70	77.55
2880	5	1.70	79.25
2920	1	0.34	79.59
3000	42	14.29	93.88
3240	4	1.36	95.24
3420	4	1.36	96.60
3780	4	1.36	97.96
4500	4	1.36	99.32
4680	2	0.68	100.00

Total	294	100.00	

In Battery IV there are 26 distinct prizes, and 12 distinct probabilities:

prize	Freq.	Percent	Cum.
0	5	1.89	1.89
180	6	2.26	4.15
300	32	12.08	16.23
540	4	1.51	17.74
600	24	9.06	26.79
720	10	3.77	30.57
900	8	3.02	33.58
1080	10	3.77	37.36
1200	21	7.92	45.28
1260	10	3.77	49.06
1440	7	2.64	51.70
1620	6	2.26	53.96
1800	4	1.51	55.47
1980	10	3.77	59.25
2100	27	10.19	69.43
2160	18	6.79	76.23
2340	4	1.51	77.74
2520	4	1.51	79.25
2700	4	1.51	80.75
2880	4	1.51	82.26
3240	4	1.51	83.77
3420	4	1.51	85.28
3780	4	1.51	86.79
4200	29	10.94	97.74
4500	4	1.51	99.25
4680	2	0.75	100.00
Total	265	100.00	

prob	Freq.	Percent	Cum.
.1	14	5.28	5.28
.15	8	3.02	8.30
.2	32	12.08	20.38
.25	50	18.87	39.25
.4	24	9.06	48.30
.5	58	21.89	70.19
.55	8	3.02	73.21
.6	2	0.75	73.96
.65	8	3.02	76.98
.7	8	3.02	80.00
.75	38	14.34	94.34
1	15	5.66	100.00
Total	265	100.00	

For the pooled analyses over all subjects, there are 90 distinct prizes and 16 distinct probabilities:

prize	Freq.	Percent	Cum.
0	41	2.02	2.02
70	2	0.10	2.12
90	12	0.59	2.71
105	2	0.10	2.81
135	12	0.59	3.40
140	2	0.10	3.50
150	4	0.20	3.70
180	18	0.89	4.58
225	4	0.20	4.78
240	2	0.10	4.88
270	8	0.39	5.27
300	138	6.80	12.07
360	34	1.68	13.75
405	8	0.39	14.14
450	118	5.82	19.96
460	2	0.10	20.06
480	2	0.10	20.16
540	64	3.15	23.31
600	126	6.21	29.52
630	20	0.99	30.51
650	2	0.10	30.61
675	16	0.79	31.39

690	2	0.10	31.49
720	66	3.25	34.75
770	2	0.10	34.84
800	2	0.10	34.94
810	32	1.58	36.52
850	2	0.10	36.62
900	112	5.52	42.14
920	2	0.10	42.24
940	2	0.10	42.34
945	20	0.99	43.32
960	2	0.10	43.42
975	2	0.10	43.52
990	20	0.99	44.50
1080	90	4.44	48.94
1150	2	0.10	49.04
1155	2	0.10	49.14
1170	8	0.39	49.53
1200	23	1.13	50.67
1215	12	0.59	51.26
1260	38	1.87	53.13
1270	2	0.10	53.23
1275	2	0.10	53.33
1300	4	0.20	53.52
1350	98	4.83	58.35
1410	2	0.10	58.45
1440	43	2.12	60.57
1460	2	0.10	60.67
1485	20	0.99	61.66
1500	84	4.14	65.80
1540	2	0.10	65.89
1600	2	0.10	65.99
1620	62	3.06	69.05
1700	2	0.10	69.15
1710	8	0.39	69.54
1725	2	0.10	69.64
1755	8	0.39	70.03
1800	92	4.53	74.57
1880	2	0.10	74.67
1890	16	0.79	75.46
1905	2	0.10	75.55
1920	2	0.10	75.65
1950	2	0.10	75.75
1980	30	1.48	77.23
2025	10	0.49	77.72
2100	27	1.33	79.05
2160	64	3.15	82.21
2190	2	0.10	82.31
2250	92	4.53	86.84
2300	2	0.10	86.94
2340	16	0.79	87.73
2430	8	0.39	88.12
2520	12	0.59	88.71
2540	2	0.10	88.81
2565	8	0.39	89.21
2600	2	0.10	89.31
2700	14	0.69	90.00
2835	8	0.39	90.39
2880	14	0.69	91.08
2920	2	0.10	91.18
3000	84	4.14	95.32
3240	12	0.59	95.91
3375	8	0.39	96.30
3420	12	0.59	96.90
3510	4	0.20	97.09
3780	12	0.59	97.68
4200	29	1.43	99.11
4500	12	0.59	99.70
4680	6	0.30	100.00

Total	2,029	100.00	

prob	Freq.	Percent	Cum.

.1	242	11.93	11.93
.15	104	5.13	17.05
.2	272	13.41	30.46
.25	134	6.60	37.06
.3	72	3.55	40.61
.4	312	15.38	55.99
.45	12	0.59	56.58
.5	334	16.46	73.04
.55	68	3.35	76.39
.6	134	6.60	83.00

.65	56	2.76	85.76
.7	92	4.53	90.29
.75	74	3.65	93.94
.8	12	0.59	94.53
.9	12	0.59	95.12
1	99	4.88	100.00
<hr/>			
Total	2,029	100.00	

Figure A1 displays the raw choices against the difference in EV, and Figure A2 repeats that display differentiated by quartiles of Net Wealth. The pattern of choices does not differ with Net Wealth, at least at this level of description.

Figure A1: Relationship Between EV Difference and Raw Choice Probabilities

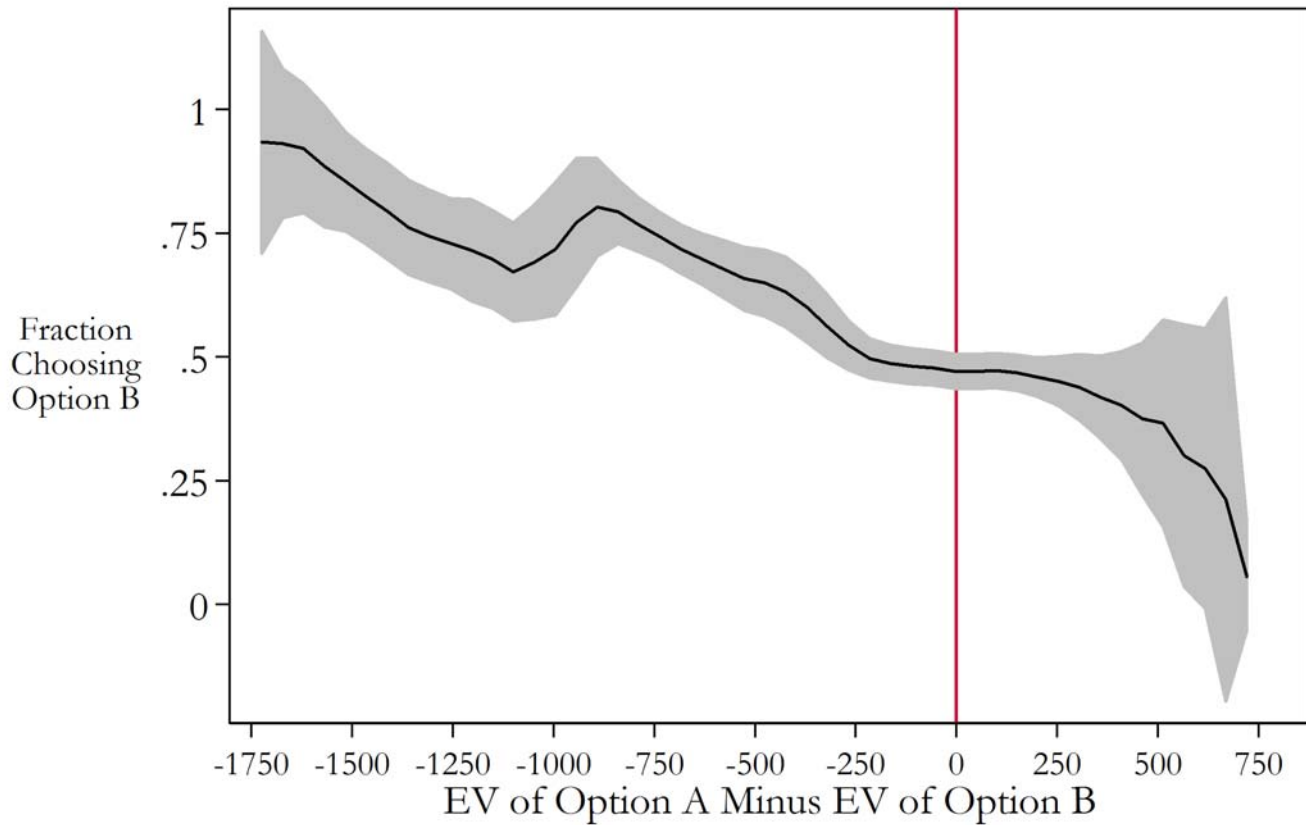
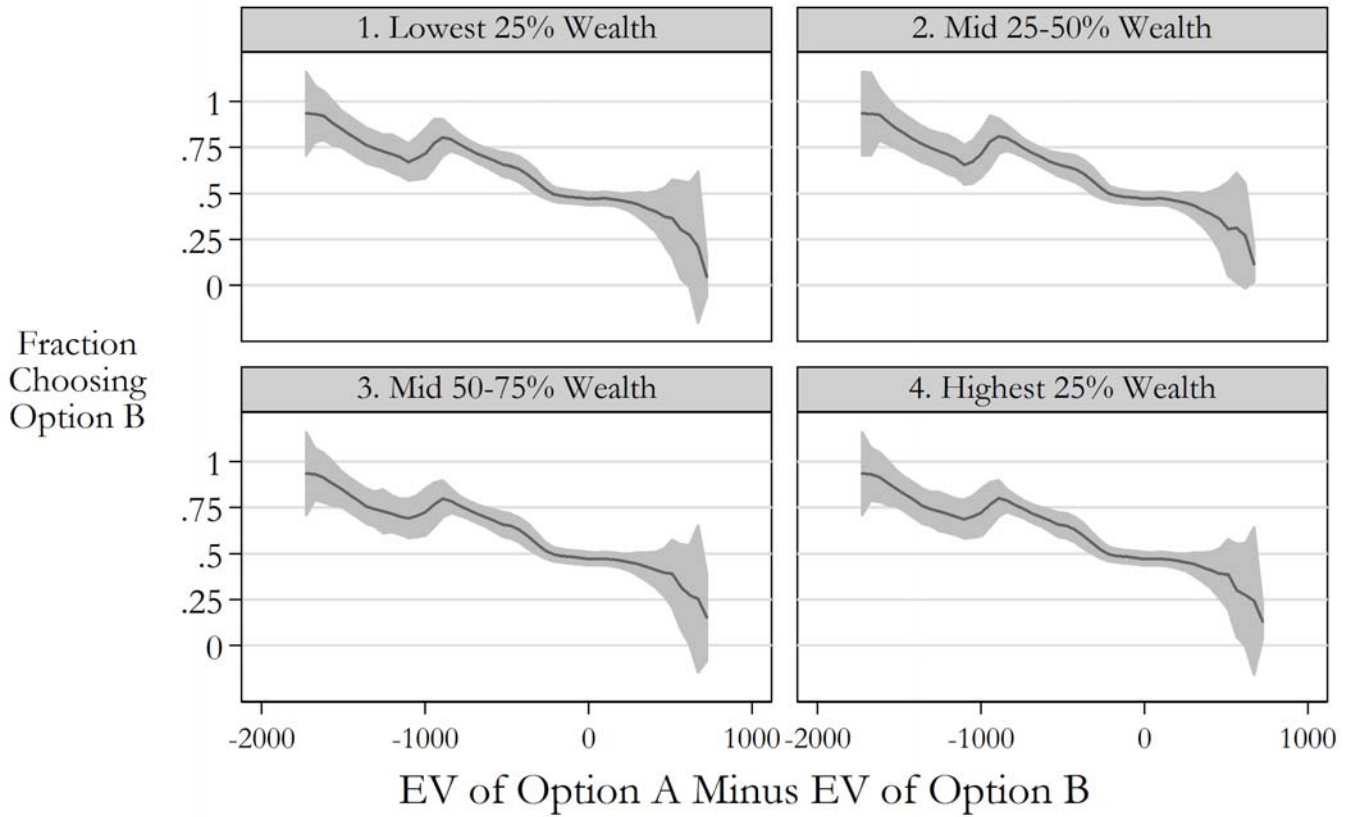


Figure A2: Relationship Between EV Difference and Raw Choice Probabilities



Appendix B: Additional Results at the Level of the Individual Subject

By and large the estimates at the level of the individual are consistent with the conclusions from the pooled models. We continue to find considerable support for the PAI specification converging to the NAI specification, and virtually no support for the FAI specification. But we do observe some considerable heterogeneity, and some interesting special cases.

Unconditional Tests of Null Asset Integration

If we assume that *every* individual is characterized by EUT-PAI, we find that 220 out of 330 valid estimates, or 67%, have p -values on the test of the hypothesis $\omega = 0$ that are below 5%. Hence we conclude that 67% of the sample *deviate* from NAI. Of course, recall from the text that only 30% of the sample (or 32% of the solved cases) were deemed consistent with EUT at the 5% level.

If we assume that *every* individual is characterized by RDU-PAI, we find that 264 out of 338 valid estimates, or 78%, have p -values on the test of the hypothesis $\omega = 0$ that are below 5%. Hence we conclude that 78% of the sample *deviate* from NAI. As noted, we find that 70% of the sample (or 68% of the solved cases) were deemed consistent with RDU at the 5% level.

Unconditional Estimates of Parameters

Table B1 shows tabulations of estimates assuming that every subject is characterized by an EUT-PAI specification, although the estimates are specific to that individual. Table B2 does likewise, assuming that every subject is characterized by a RDU-PAI specification, again where the estimates are specific to that individual. We report estimates in intervals, because the confidentiality of access to individual data within Statistics Denmark does not allow us to report estimates for any one individual. Nonetheless, these tabulations provide a rich characterization.

Panel A within each table show the range of estimates of ω . We find that 71% of the subjects have an estimate of ω that is less than 0.05 under the EUT-PAI specification, and 62% under the RDU-PAI specification. Again, these are the same subjects, just with different specifications assumed: the main text presents comparable results when we condition on whether an individual is better characterized as EUT or RDU. However, even these unconditional estimates confirm the tendency clear from the pooled estimates, that many individuals are approximating NAI. Of course, we need to match these ω estimates with the Net Wealth w that each subject has, and we do that below.

Panel B within each table shows the range of estimates of r . Under EUT, we find 36.9% (= 11.3 + 25.6) with estimates signifying modest risk aversion: $0 < r < 1$. On the other hand, 34% have estimates of r that exceed 1, and some of those estimates could be quite large. We have not in these tabulations conditioned on the statistical significance of these estimates; that is done in the main text. Under RDU the comparable fractions are 26.9% and 30.5%, respectively. Of course, under RDU the coefficient r is only a part of the characterization of risk attitudes, and one has to attend to the effects of probability weighting as well.

Panel C within each table shows a cross-tabulation of these estimates of ω and r . We immediately

observe that 129 (= 107 + 22) of the 150 estimates of $r > 1$ under EUT are for individuals with extremely low ω estimates below 0.05. For RDU we find that 108 (= 35 + 73) of the 135 estimates of $r > 1$ are for individuals with ω below 0.05.

Panel D is an important complement to Panel A, since it multiplies the estimate of ω for the individual by the Net Wealth w of the same individual, telling us in effect what “weighted baseline wealth” the individual aggregates with experimental income. Under EUT we have 25% of the subjects with weighted baseline wealth less than 10 kroner, which is effectively zero. But we have 27% with weighted baseline wealth between 10 and 1,000 kroner, 21% between 1,000 and 100,000 kroner, and 14% over 100,000 kroner. So this is an important pattern of heterogeneity, illustrating why it is important to look at the interaction of the ω parameter with individual wealth w . Under RDU we get even higher weighted baseline wealth levels: only 8% below 10 kroner, only 14% between 10 and 1,000 kroner, but 40% between 10,000 and 100,000 kroner, and 17% over 100,000 kroner.

Panel E provides a cross-tabulation of these estimates of $\omega \times w$ and r , akin to Panel C.

Estimates of Parameters Conditional on Model of Risk Preferences and Statistical Significance

Here we document the findings *after* conditioning on which model of decision-making under risk best characterizes each individual (EUT or RDU), *and then conditioning on the statistical significance of parameter estimates* (e.g., if the estimate of $\omega > 0$ but is not statistically significantly different from 0, we set it to 0). This way of presenting results is more intuitive than ignoring the statistical significance of estimates: one should not look at EUT results for an individual better characterized as RDU, and one should not ignore the statistical significance of results when reporting findings. As it happens, for completeness we later report estimates at the level of the individual that do not condition on statistical significance.

All statements about statistical significance will be using a 5% two-sided test, but we have complete results using a 1% or 10% level, and nothing changes with respect to the qualitative conclusions. We also have to condition statements on the fact that, as always happens with individual-level estimation, there is a fraction of individuals and model specifications that do not solve numerically.

We find that a relatively high 68% of the sample are better characterized as RDU decision-makers than EUT decision-makers. The formal test here is that $\pi(p)=p$, which is the case when $\eta = \varphi = 1$ from the probability weighting function (12). We say that this fraction is relatively high given our priors from the same calculations with university student pools from the United States (Harrison and Ng [2016]). This 68% refers to 287 subjects out of the 421 for which we had valid estimates; there were only 21 subjects for which we had no estimates of either the EUT or RDU specification. So we have a clear majority of subjects for whom we should *not* look at results that assume EUT.

Table B3 collates the individual estimates. Panel A show the range of estimates of ω , and we find that 82% [89%] of the subjects have an estimate of ω that is less than 0.001 [0.05] under the preferred PAI specification (*viz.*, EUT-PAI or RDU-PAI). Recall that this includes all subjects with statistically insignificant estimates of ω , irrespective of the point estimate of ω , which we set to zero. Of course, before we conclude that these individuals are approximating NAI, we need to match these ω estimates with the Net Wealth w that each subject has.

Panel B shows the range of estimates of r , and we see that 39% (= 19% + 20%) of subjects have an estimate that lies between 0 and 1, reflecting modest risk aversion.¹ Some of these cases reflect estimates of r that are not statistically significantly different from zero. Of course, under RDU the coefficient r is only a part of the characterization of risk attitudes, and one has to attend to the effects of probability weighting as well.

Panel C shows a cross-tabulation of these estimates of ω and r . We find that 152 (= 61+77+9+5) of the 442 subjects have estimates of ω between 0 and 0.05 and estimates of r between 0 and 1, reflecting modest risk aversion. We find that 310 (= 152+147+11) of the 442 subjects have estimates of ω between 0 and 0.05 and estimates of r above or equal to 0 reflecting risk aversion.

Panel D is an important complement to Panel A, since it multiplies the estimate of ω for the individual by the Net Wealth w of the same individual, telling us in effect what weighted baseline wealth the individual aggregates with experimental income. We find that 77% of subjects behave as if employing a weighted baseline wealth less than 10 kroner, which is effectively zero in terms of implications for calibration. A further 6% of the sample have weighted baseline wealth between 10 kroner and 1,000 kroner (\approx \$150), 8% have weighted baseline wealth between 1,000 and 100,000 kroner (\approx \$15,053), and 3% have weighted baseline wealth over 100,000 kroner. So this is an important pattern of heterogeneity, illustrating, in contrast to Panel A, why it is important to look at the interaction of the ω parameter with individual Net Wealth w . Panel E provides a cross-tabulation of these estimates of $\omega \times w$ and r , akin to Panel C.

Figures B1 and B2 display the implications for the Arrow-Pratt measure of RRA in (4) for the average parameter values of representative individuals of two sub-samples of subjects with non-trivial levels of weighted baseline wealth $\omega \times w$. We focus on the implications for RRA from the utility function, and set aside implications from any probability weighting.

As it happens, there is relatively little probability weighting from a substantive point of view, even if it is statistically significant. Figures B3 and B4 display the estimated probability weighting functions and implications for decision weights, corresponding to the utility function estimates reflected in Figures B1 and B2, respectively. The left panel of Figures B3 and B4 shows the probability weighting function. The decision weight for the top prize is read directly off the probability weighting function, and the decision weights for the smaller prizes are then derived according to (11a) and (11b). The right panel of Figures B3 and B4 shows an example in which the objective probabilities on three prizes are each assumed to be $\frac{1}{3}$, in order to illustrate the pure effect of probability weighting, and the dashed line then shows the effect of the probability weighting curvature in the left panel. So we see in Figure B4, for example, that the weight given to the best prize increases slightly from 0.33, while the weight given to the worst prize decreases slightly from 0.33.

Returning to Figures B1 and B2, Figure B1 shows results for a representative agent with weighted baseline wealth between 10,000 kroner and 100,000 kroner. Figure B2 shows results for a representative agent with weighted baseline wealth between 100,000 kroner and 1 million kroner. Figures B1 and B2 each pool sub-samples of individual estimates, spanning EUT-PAI and RDU-PAI subjects. In each case we observe considerable variation in RRA as wealth varies, but no levels of RRA that would seem implausible in the sense

¹ We merge the cells for the case in which $0.5 \leq r < 1$ and $r \geq 1$ to avoid reporting cells referring to individuals that have too small a frequency count, so as to ensure confidentiality. We retain the detailed rows to facilitate comparison with unconditional tabulations below.

of the calibration critiques. These are important sub-samples, because their levels of weighted baseline wealth mean that they do not collapse to NAI, where we know that small stakes and large stakes risk aversion are plausible for our subjects. In the case of the individual agents included in Figure B1 we have an example of PAI, with $\omega = 0.045$ (hence weighted baseline wealth of between 450 kroner and 4,500 kroner), $\rho = 0.66$ and $\sigma = 7.7$. This elasticity of substitution is not ∞ , but it is very high for all practical purposes. But the fact that only 4.5% of wealth is integrated with experimental prizes, and this represents an amount in the range of the experimental prizes, points to PAI. For the individual agents in Figure B2 we have another example of PAI, with $\omega = 0.13$, $\rho = 0.31$ and $\sigma = 1.5$.

It is possible to characterize the payoff calibration implications for the complete set of individual estimates by calculating the CE values for a wide range of lotteries that are comparable to those in Panel B of Table 3 in the text, with varying values of H and L and probability $\frac{1}{2}$. For each subject we can undertake these calculations using the estimates for that individual and the Net Wealth for that individual. EUT subjects are simply RDU subjects for whom $\eta = \varphi = 1$, so can be included in the same simulations correctly. We can then summarize the ratio of CE to EV across all subjects and simulated choices: there is an average of 0.83 and a median of 0.96. The complete distribution of these ratios, shown in Figure B5, does reveal *some* low ratios, and *some* ratios indicating risk-loving choices, reflecting the heterogeneity of risk preferences and lotteries evaluated. But the overall pattern confirms our general finding of plausible patterns of risk aversion.

Estimates of Parameters Conditional Only on Model of Risk Preferences

Table B4 shows tabulations of individual estimates in which we condition *only* on the descriptively best model of risk preferences for each subject. As noted above, we find that 68% of the sample are better characterized as RDU decision-makers than EUT decision-makers. The formal test here is that $\pi(p)=p$, which is the case when $\eta = \varphi = 1$ from the probability weighting function (12). In Table B4 the estimates of ω and r for each individual are taken from the estimates for that individual conditional on the preferred model (EUT or RDU) for that individual. Hence they tend to look more like the tabulations in Table B2 than the tabulations in Table B1.

Again, the tabulations in Table B4 are provided to allow one to see the effect of conditioning *solely* on the model of risk preferences, in contrast to Table B3 which conditions on the model or risk preferences *and* the statistical significance of parameter estimates.

Additional Reference

Harrison, Glenn W., and Ng, Jia Min, "Evaluating the Expected Welfare Gain from Insurance," *Journal of Risk and Insurance*, 83(1), 2016, 91-120.

Table B1: EUT-PAI Estimates of Individual Parameters*Panel A: Tabulation of ω Point Estimates*

Range for ω	Frequency	Percent	Cumulative Percent
$0 < \omega \leq 0.001$	232	52.5	52.5
$0.001 < \omega \leq 0.05$	82	18.6	71
$0.05 < \omega \leq 0.3$	49	11.1	82.1
$\omega > 0.3$	26	5.9	88
Missing	53	12	100
Total	442	100	

Panel B: Tabulation of r Point Estimates

Range for r	Frequency	Percent	Cumulative Percent
$r < 0$	76	17.2	17.2
$0 \leq r < 0.5$	50	11.3	28.5
$0.5 \leq r < 1$	113	25.6	54.1
$r \geq 1$	150	34	87.1
Missing	53	12	100
Total	442	100	

Panel C: Cross-Tabulation of ω and r Point Estimates

Range for ω	Range for r				Missing	Total
	$r < 0$	$0 \leq r < 0.5$	$0.5 \leq r < 1$	$r \geq 1$		
$0 < \omega \leq 0.001$	34	20	71	107	0	232
$0.001 < \omega \leq 0.05$	33	16	11	22	0	82
$0.05 < \omega \leq 0.3$	6	10	20	13	0	49
$\omega > 0.3$	3	4	11	8	0	26
Missing	0	0	0	0	53	53
Total	76	50	113	150	53	442

Panel D: Tabulation of $\omega \times w$ Point Estimates

Range for $\omega \times w$ in DKK	Frequency	Percent	Cumulative Percent
$0 < \omega \times w \leq 10$	110	24.9	34.9
$10 < \omega \times w \leq 1,000$	121	27.4	52.3
$1,000 < \omega \times w \leq 100,000$	94	21.3	75.5
$\omega \times w > 100,000$	64	14.5	88
Missing	53	12	100
Total	442	100	

Panel E: Cross-Tabulation of $\omega \times w$ and r Point Estimates

Range for $\omega \times w$ in DKK	Range for r				Missing	Total
	$r < 0$	$0 \leq r < 0.5$	$0.5 \leq r < 1$	$r \geq 1$		
$0 < \omega \times w \leq 10$	17	5	32	56	0	110
$10 < \omega \times w \leq 1,000$	17	17	44	43	0	121
$1,000 < \omega \times w \leq 100,000$	32	17	14	31	0	94
$\omega \times w > 100,000$	10	11	23	20	0	64
Missing	0	0	0	0	53	53
Total	76	50	113	150	53	442

Table B2: RDU-PAI Estimates of Individual Parameters*Panel A: Tabulation of ω Point Estimates*

Range for ω	Frequency	Percent	Cumulative Percent
$0 < \omega \leq 0.001$	95	21.5	21.5
$0.001 < \omega \leq 0.05$	178	40.3	61.8
$0.05 < \omega \leq 0.3$	30	6.8	68.6
$\omega > 0.3$	50	11.3	79.9
Missing	89	20.1	100
Total	442	100	

Panel B: Tabulation of r Point Estimates

Range for r	Frequency	Percent	Cumulative Percent
$r < 0$	99	22.4	22.4
$0 \leq r < 0.5$	38	8.6	31
$0.5 \leq r < 1$	81	18.3	49.3
$r \geq 1$	135	30.5	79.9
Missing	89	20.1	100
Total	442	100	

Panel C: Cross-Tabulation of ω and r Point Estimates

Range for ω	Range for r				Missing	Total
	$r < 0$	$0 \leq r < 0.5$	$0.5 \leq r < 1$	$r \geq 1$		
$0 < \omega \leq 0.001$	19	8	33	35	0	95
$0.001 < \omega \leq 0.05$	53	14	38	73	0	178
$0.05 < \omega \leq 0.3$	9	6	5	10	0	30
$\omega > 0.3$	18	10	5	17	0	50
Missing	0	0	0	0	89	89
Total	99	38	81	135	89	442

Panel D: Tabulation of $\omega \times w$ Point Estimates

Range for $\omega \times w$ in DKK	Frequency	Percent	Cumulative Percent
$0 < \omega \times w \leq 10$	37	8.4	8.4
$10 < \omega \times w \leq 1,000$	63	14.2	22.6
$1,000 < \omega \times w \leq 100,000$	178	40.3	62.9
$\omega \times w > 100,000$	75	17	79.9
Missing	89	20.1	100
Total	442	100	

Panel E: Cross-Tabulation of $\omega \times w$ and r Point Estimates

Range for $\omega \times w$ in DKK	Range for r				Missing	Total
	$r < 0$	$0 \leq r < 0.5$	$0.5 \leq r < 1$	$r \geq 1$		
$0 < \omega \times w \leq 10$	9	4	18	6	0	37
$10 < \omega \times w \leq 1,000$	15	4	18	26	0	63
$1,000 < \omega \times w \leq 100,000$	53	11	37	77	0	178
$\omega \times w > 100,000$	22	19	8	26	0	75
Missing	0	0	0	0	53	53
Total	99	38	81	135	53	442

Table B3: PAI Estimates of Individual Parameters Conditional on the Model of Risk Preferences and Statistical Significance

Panel A: Tabulation of ω Point Estimates

Range for ω	Frequency	Percent	Cumulative Percent
$0 < \omega \leq 0.001$	362	82	82
$0.001 < \omega \leq 0.05$	33	7	89
$0.05 < \omega \leq 0.3$	13	3	92
$\omega > 0.3$	5	1	93
Missing	29	7	100
Total	442	100	

Panel B: Tabulation of r Point Estimates

Range for r	Frequency	Percent	Cumulative Percent
$r < 0$	85	19	19
$0 \leq r < 0.5$	84	19	38
$0.5 \leq r < 1$	90	20	58
$r \geq 1$	162	37	95
Missing	21	5	100
Total	442	100	

Panel C: Cross-Tabulation of ω and r Point Estimates

Range for ω	Range for r				Missing	Total
	$r < 0$	$0 \leq r < 0.5$	$0.5 \leq r < 1$	$r \geq 1$		
$0 < \omega \leq 0.001$	77	61	77	147	0	362
$0.001 < \omega \leq 0.05$	8	9	5	11	0	33
$0.05 < \omega \leq 0.3$	0	0	7	4	2	13
$\omega > 0.3$	0	4	0	0	1	5
Missing	0	10	1	0	18	29
Total	85	84	90	162	21	442

Panel D: Tabulation of $\omega \times w$ Point Estimates

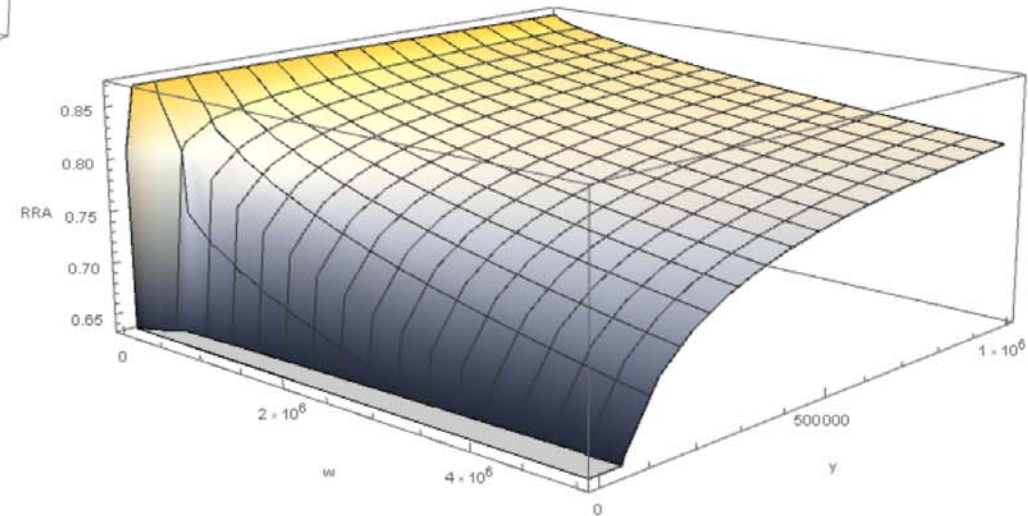
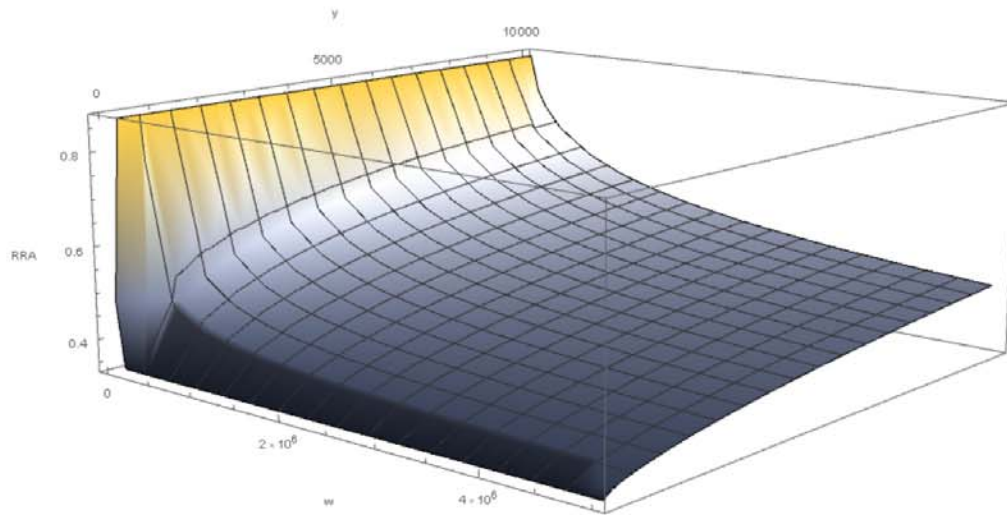
Range for $\omega \times w$ in DKK	Frequency	Percent	Cumulative Percent
$0 < \omega \times w \leq 10$	339	77	77
$10 < \omega \times w \leq 1,000$	25	6	83
$1,000 < \omega \times w \leq 100,000$	34	8	90
$\omega \times w > 100,000$	15	3	93
Missing	29	7	100
Total	442	100	

Panel E: Cross-Tabulation of $\omega \times w$ and r Point Estimates

Range for $\omega \times w$ in DKK	Range for r				Missing	Total
	$r < 0$	$0 \leq r < 0.5$	$0.5 \leq r < 1$	$r \geq 1$		
$0 < \omega \times w \leq 10$	76	50	73	140	0	339
$10 < \omega \times w \leq 1,000$	0	10	7	6	2	25
$1,000 < \omega \times w \leq 100,000$	7	11	6	10	0	34
$\omega \times w > 100,000$	0	15	4	6	0	15
Missing	1	8	0	0	29	38
Total	85	84	90	162	31	442

Figure B1: Arrow-Pratt Relative Risk Aversion
for Baseline Wealth Between 10,000 kroner and 100,000 kroner

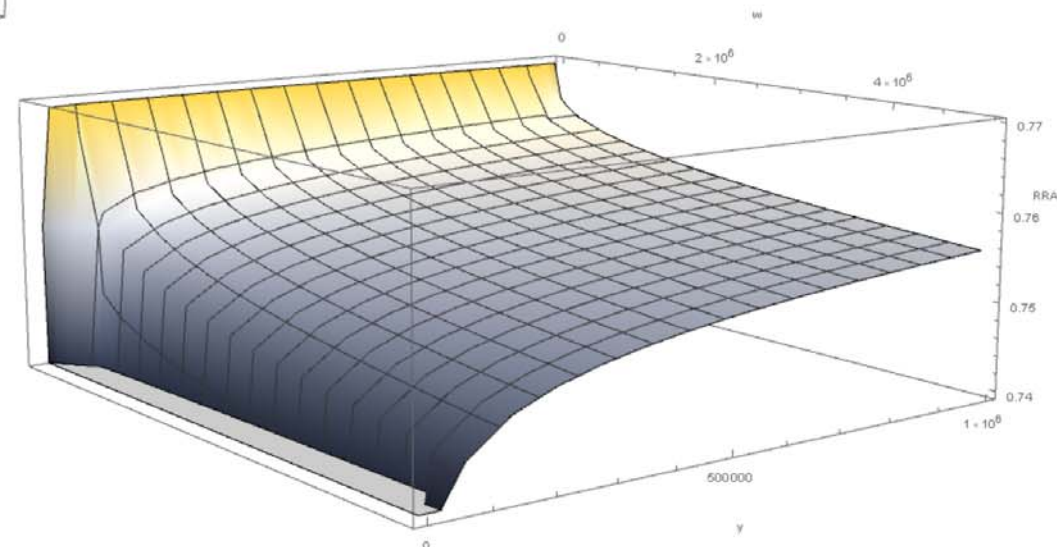
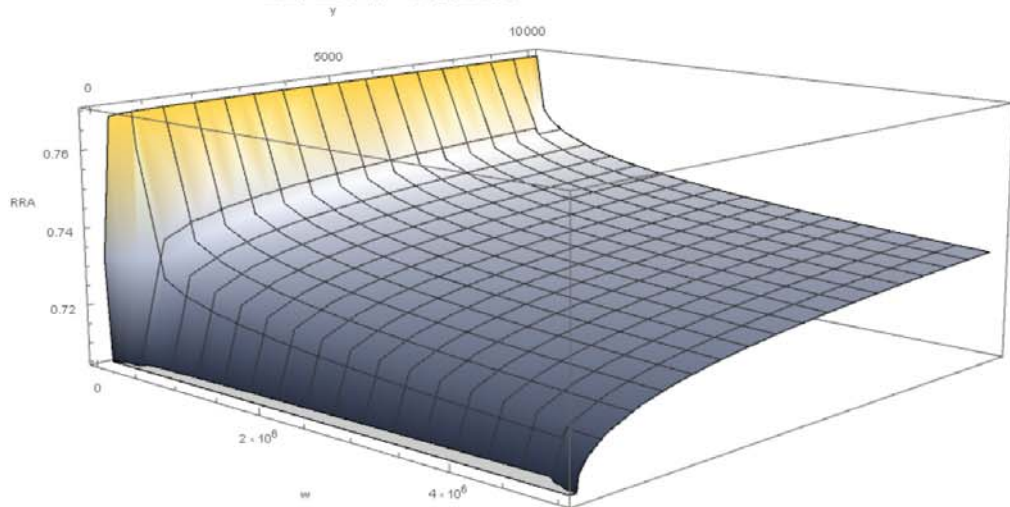
A. Low Stakes



B. High Stakes

Figure B2: Arrow-Pratt Relative Risk Aversion
for Baseline Wealth Between 100,000 kroner and 500,000 kroner

A. Low Stakes



B. High Stakes

Figure B3: Probability Weighting and Decision Weights for Baseline Wealth Between 10,000 kroner and 100,000 kroner

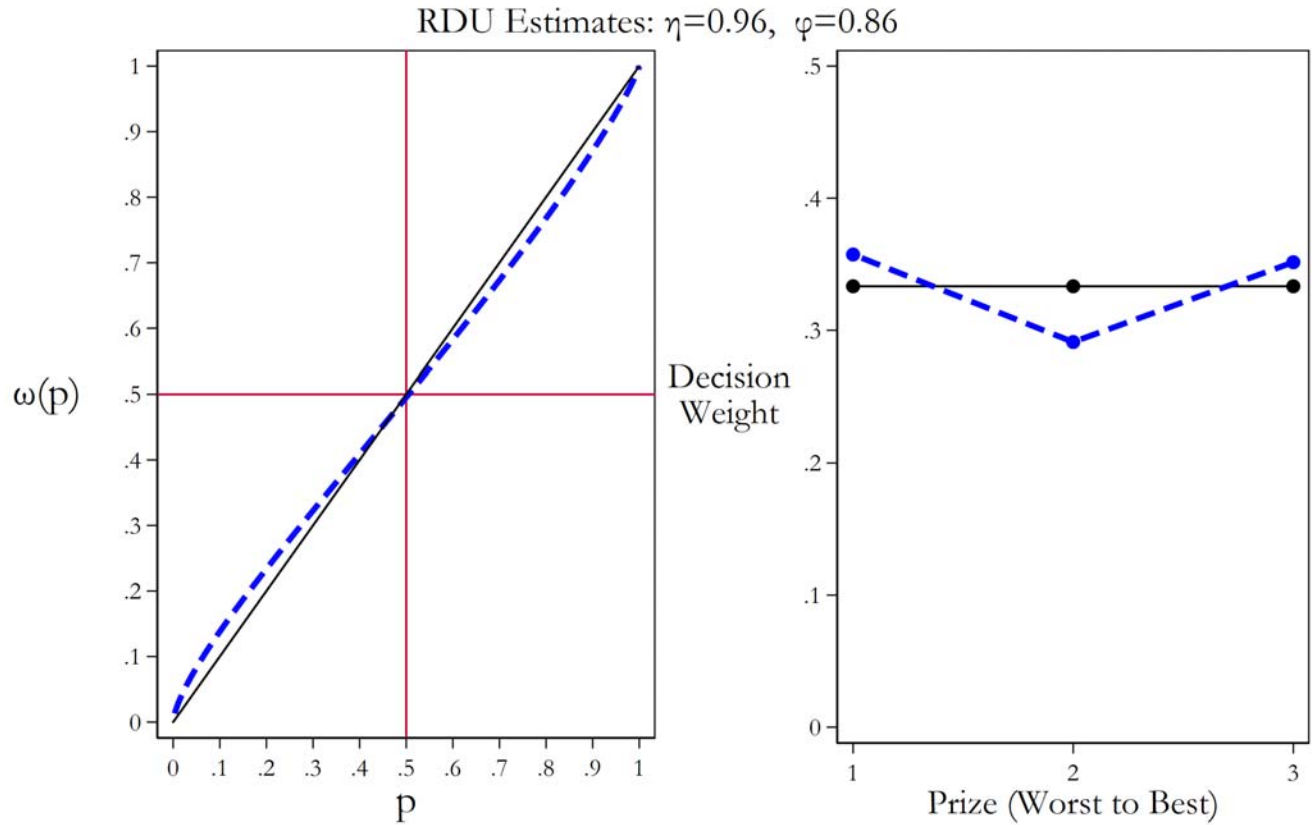


Figure B4: Probability Weighting and Decision Weights for Baseline Wealth Between 100,000 kroner and 500,000 kroner

RDU Estimates: $\eta=0.94$, $\varphi=1.02$

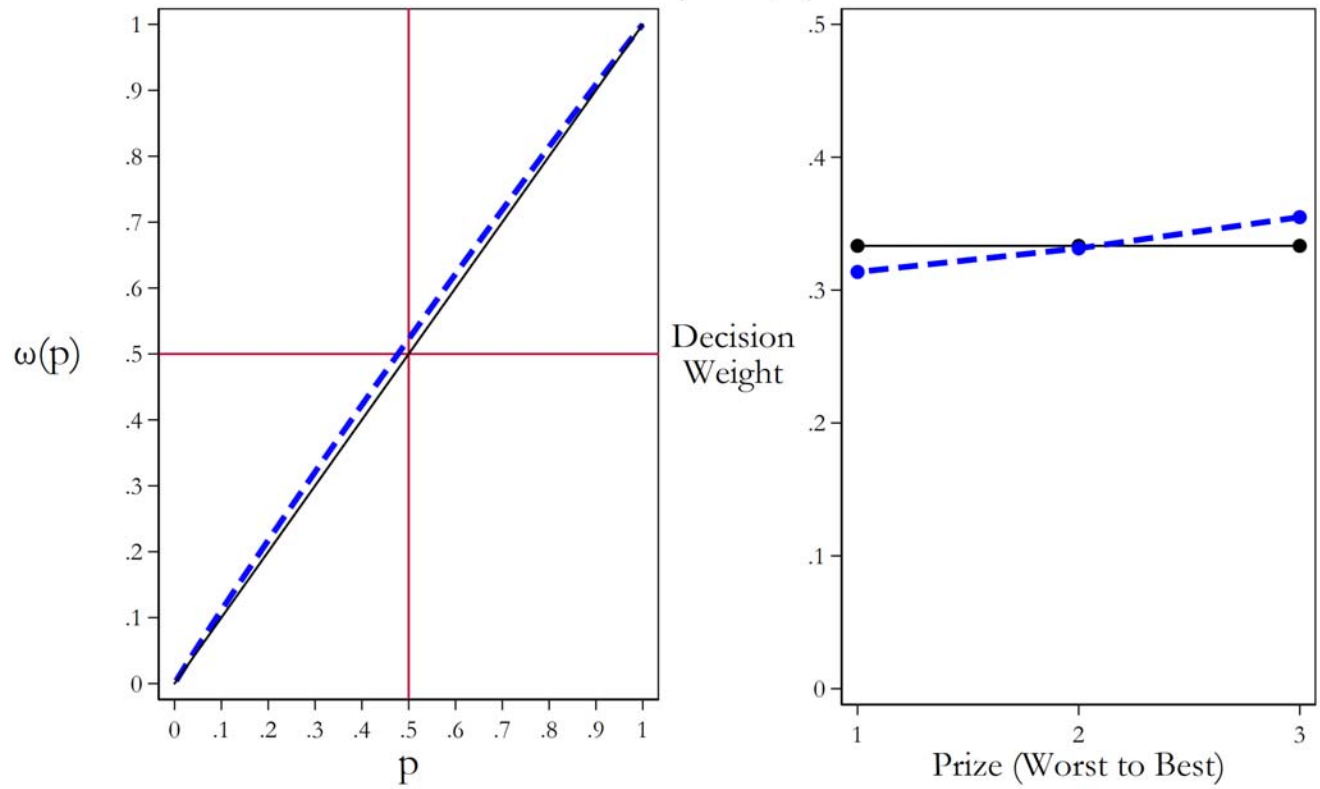


Figure B5: Distribution of Ratios of Certainty Equivalent to Expected Value

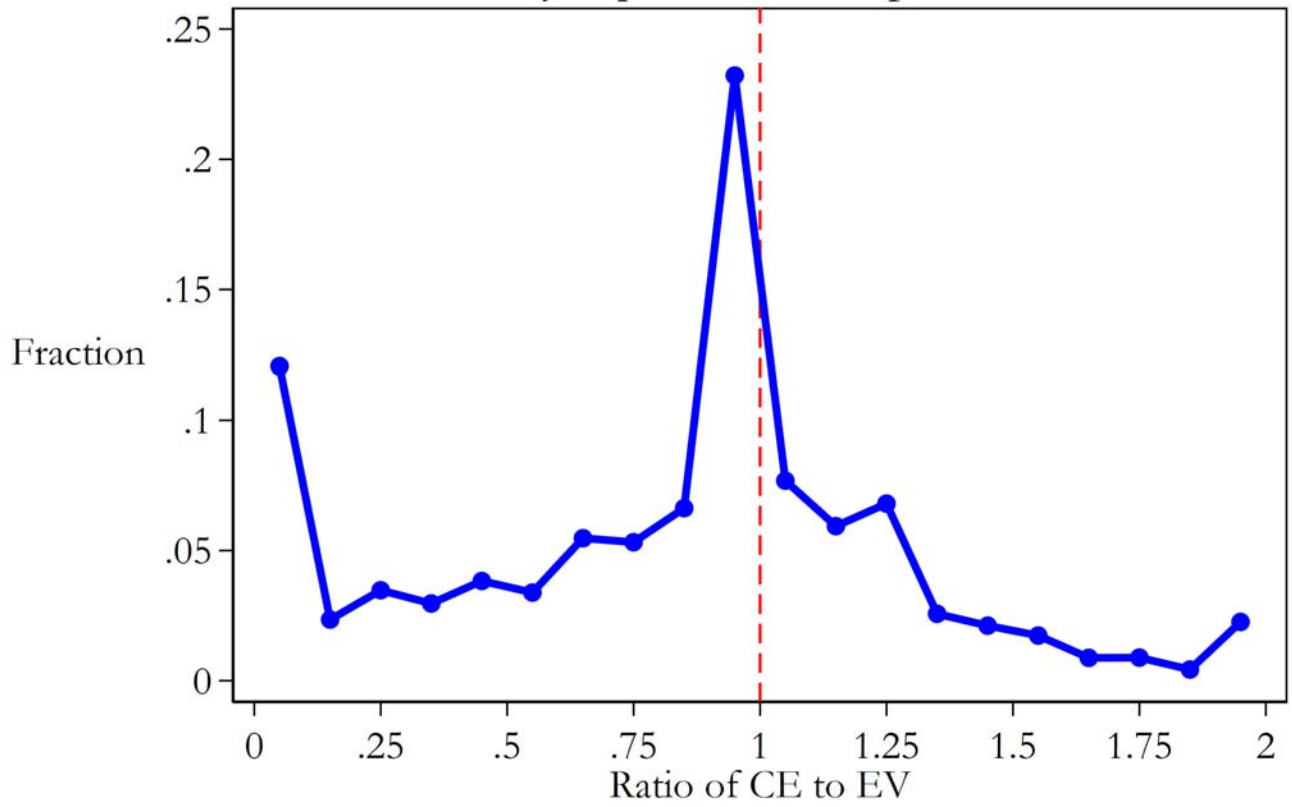


Table B4: PAI Estimates of Individual Parameters Conditional Only on the Model of Risk Preferences

Panel A: Tabulation of ω Point Estimates

Range for ω	Frequency	Percent	Cumulative Percent
$0 < \omega \leq 0.001$	154	34.8	34.8
$0.001 < \omega \leq 0.05$	168	38	72.8
$0.05 < \omega \leq 0.3$	43	9.7	82.6
$\omega > 0.3$	48	10.9	93.4
Missing	29	6.6	100
Total	442	100	

Panel B: Tabulation of r Point Estimates

Range for r	Frequency	Percent	Cumulative Percent
$r < 0$	102	23.1	23.1
$0 \leq r < 0.5$	45	10.2	33.3
$0.5 \leq r < 1$	93	21	54.3
$r \geq 1$	173	39.1	93.4
Missing	20	6.6	100
Total	442	100	

Panel C: Cross-Tabulation of ω and r Point Estimates

Range for ω	Range for r					Total
	$r < 0$	$0 \leq r < 0.5$	$0.5 \leq r < 1$	$r \geq 1$	Missing	
$0 < \omega \leq 0.001$	25	12	44	73	0	154
$0.001 < \omega \leq 0.05$	57	14	31	66	0	168
$0.05 < \omega \leq 0.3$	5	10	11	17	0	43
$\omega > 0.3$	15	9	7	17	0	48
Missing	0	0	0	0	29	29
Total	102	45	93	173	29	442

Panel D: Tabulation of $\omega \times w$ Point Estimates

Range for $\omega \times w$ in DKK	Frequency	Percent	Cumulative Percent
$0 < \omega \times w \leq 10$	77	17.4	17.4
$10 < \omega \times w \leq 1,000$	86	19.5	36.9
$1,000 < \omega \times w \leq 100,000$	164	37.1	74
$\omega \times w > 100,000$	86	19.5	93.4
Missing	29	6.6	100
Total	442	100	

Panel E: Cross-Tabulation of $\omega \times w$ and r Point Estimates

Range for $\omega \times w$ in DKK	Range for r				Missing	Total
	$r < 0$	$0 \leq r < 0.5$	$0.5 \leq r < 1$	$r \geq 1$		
$0 < \omega \times w \leq 10$	11	6	26	34	0	77
$10 < \omega \times w \leq 1,000$	18	7	25	36	0	86
$1,000 < \omega \times w \leq 100,000$	51	12	30	71	0	164
$\omega \times w > 100,000$	22	20	12	32	0	86
Missing	0	0	0	0	29	29
Total	102	45	93	173	29	442

Appendix C: Additional Results with a Different Definition of the Certainty Equivalent

In the text the definition of a Certainty Equivalent (CE) when one allows for partial asset integration is provided by (14), repeated below. An alternative definition is formally possible, and although we do not believe it to be a “natural definition” in terms of the economic implications, we demonstrate here that it does not change our general conclusions if we use it. The analysis here uses the pooled estimates, not the estimates at the level of individuals.

Let H denote a high prize and L denote a low prize, for $H > L$. The CE in Panel A of Table C1 is then the sure amount of money that has the same expected utility to the individual as the lottery that pays H with probability p and L with probability (1-p). In Panel A of Table C1, and in Panel B of Table 2 of the main text, the CE solves

$$U(w, CE) = p \times U(w, H) + (1-p) \times U(w, L). \quad (14)$$

So this CE solves for risky income in the experiment, and the stakes are chosen to be within the payoff domain in our experiments.

In Panel B of Table C1 the CE solves instead for risky wealth, holding constant the experimental income at zero, and the stakes are chosen to span “life-changing” changes in wealth for most Danes. Formally, for Panel B of Table C1 the CE solves

$$U(w+CE, 0) = p \times U(w+H, 0) + (1-p) \times U(w+L, 0). \quad (14')$$

This is the alternative definition of CE referred to above. We believe this definition of the CE to be less interesting, since it amounts to us saying to the subjects, as they come in to the session: “We can give you a certain amount of money, CE, to add to the wealth w that you have outside of the lab, and agree not to participate in the experiment and hence get \$0 payoffs from the experiment with certainty.” The definition of CE in (14), on the other hand, implies a much more natural economic scenario.

Another issue with this CE calculation, raised by a referee, may be stated as follows: if one posed the CE thought experiment in question to the subject, would we change the way they evaluate lab income and wealth? Could there be something contextual in behavioral responses to that sort of task, rather than the one we posed (pick between to lotteries over lab income)? This is a reasonable concern, and although the formal mathematics of the calculation defined by (14') allow us to compute CE, it requires us to assume that there is no context-effect from posing these sorts of questions with respect to changes in wealth. This assumption is not obvious *a priori*, and at the very least can and should be tested behaviorally.

The smallest ratio of CE to EV in Table C1 is 0.362, and most are much higher: these ratios are hardly implausible in the sense of the term used by Hansson [1988], Rabin [2000], Neilson [2001], Rieger and Wang [2006], Cox and Sadiraj [2006] and Safra and Segal [2008].

The same point applies for our evaluation of the CE in the RDU case. Using the same lotteries as in Panel B of Table 3 and Panel A of Table C1, in Panel A of Table C2 the CE now

solves

$$U(w, CE) = h(p) \times U(w, H) + (1-h(p)) \times U(w, L), \quad (15)$$

and in Panel B of Table C2 the CE solves

$$U(w+CE, 0) = h(p) \times U(w+H, 0) + (1-h(p)) \times U(w+L, 0). \quad (15')$$

The smallest ratio of CE to EV in Table C2 is 0.478, and most are much higher, exactly as in Table C1. In fact, in one case the CE exceeds the EV, but this is completely intuitive: the probability on the high prize of 400,000 kroner ($\approx \$60,214$) is 0.010, and the low prize is only 100 kroner.² In general the ratios in Tables C1 and C2 are similar.

² From the left panel of Figure C1 we can, just, see that the probability weighting is “optimistic” for very small probabilities, so this probability of 0.010 becomes 0.0171, which is in turn the decision weight on the top prize. Assuming a linear utility function for simplicity, the *decision-weighted* EV is then $0.0171 \times 400,000 + (1-0.0171) \times 100 = 6,962$, which is 1.698 times the EV of 4,099 kroner. The actual CE is slightly less, at 6,937 kroner, taking into account the fact that $U'' < 0$ for the RDU specification in Table 3.

Table C1: Implied Certainty Equivalents Using EUT-PAI Model

Calculations with average wealth

High Prize (DKK)	Probability of High Prize	Low Prize (DKK)	Expected Value (DKK)	Certainty Equivalent (DKK)	Ratio
<i>A. Risky Lottery in Experiment</i>					
200	0.5	100	150	145	0.965
500	0.5	100	300	252	0.84
1000	0.5	100	550	402	0.73
2000	0.5	100	1,050	663	0.631
5000	0.5	100	2,550	1,350	0.529
5000	0.01	100	149	109	0.732
5000	0.1	100	590	214	0.362
5000	0.3	100	1,570	626	0.399
5000	0.7	100	3,530	2,459	0.697
5000	0.9	100	4,510	4,025	0.892
<i>B. Risky Lottery in Wealth</i>					
200	0.5	100	150	150	0.999
1000	0.5	100	550	550	0.999
5000	0.5	100	2,550	2,549	0.999
100000	0.5	100	50,050	49,794	0.995
11000	0.5	10,000	10,500	10,500	0.999
50000	0.5	10,000	30,000	29,959	0.999
100000	0.5	10,000	55,000	54,792	0.996
400000	0.5	10,000	205,000	201,286	0.982
400000	0.01	100	4,099	3,945	0.962
400000	0.3	100	120,070	116,793	0.973
400000	0.5	100	200,050	196,139	0.98
400000	0.7	100	280,030	276,738	0.988
400000	0.9	100	360,010	358,596	0.996

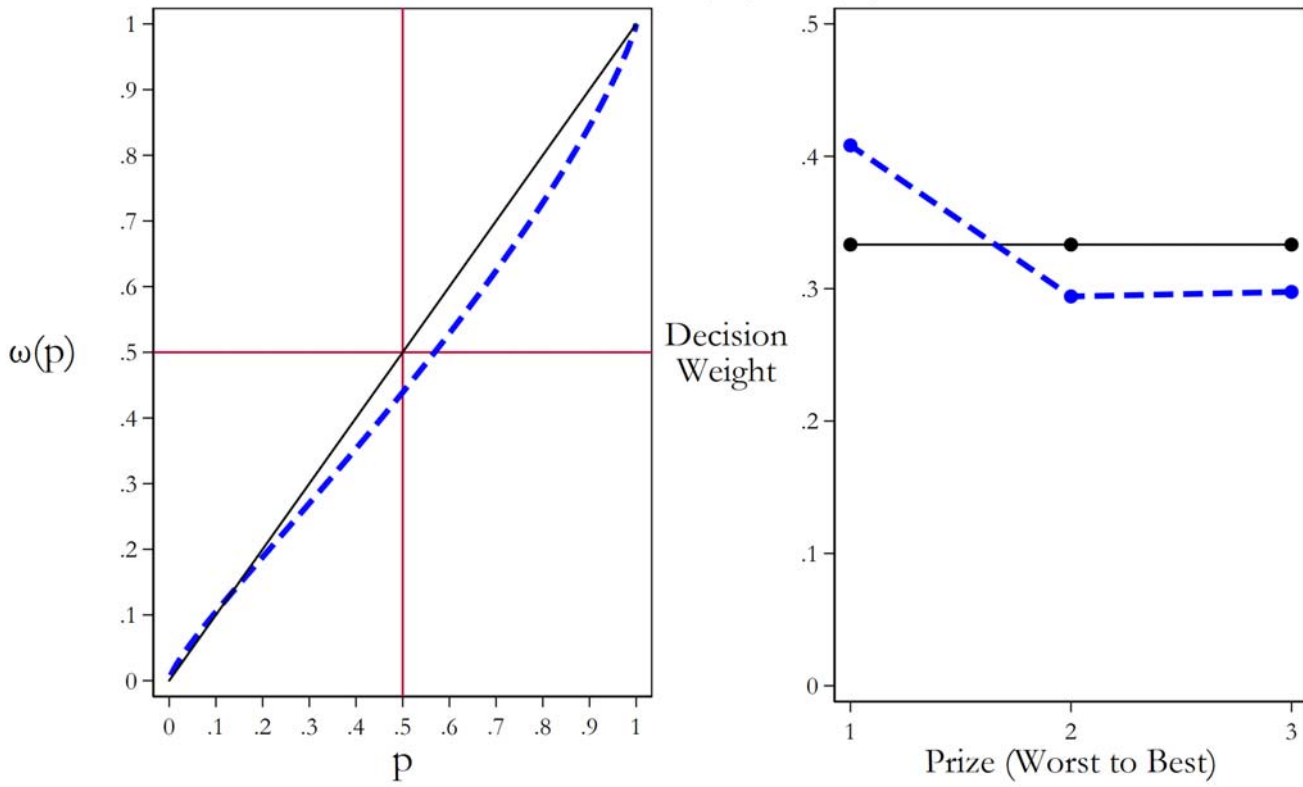
Table C2: Implied Certainty Equivalents Using RDU-PAI Model

Calculations with average wealth

Large Prize (DKK)	Probability of Large Prize	Small Prize (DKK)	Expected Value (DKK)	Certainty Equivalent (DKK)	Ratio
<i>A. Risky Lottery in Experiment</i>					
200	0.5	\$100	\$150	141	0.937
500	0.5	\$100	\$300	244	0.813
1,000	0.5	\$100	\$550	395	0.717
2,000	0.5	\$100	\$1,050	668	0.636
5,000	0.5	\$100	\$2,550	1,418	0.556
5,000	0.01	\$100	\$149	126	0.848
5,000	0.1	\$100	\$590	290	0.492
5,000	0.3	\$100	\$1,570	751	0.478
5,000	0.7	\$100	\$3,530	2,371	0.672
5,000	0.9	\$100	\$4,510	3,800	0.842
<i>B. Risky Lottery in Wealth</i>					
200	0.5	\$100	\$150	144	0.959
1,000	0.5	\$100	\$550	495	0.9
5,000	0.5	\$100	\$2,550	2,251	0.883
100,000	0.5	\$100	\$50,050	43,769	0.875
11,000	0.5	\$10,000	\$10,500	10,439	0.994
50,000	0.5	\$10,000	\$30,000	27,530	0.918
100,000	0.5	\$10,000	\$55,000	49,359	0.897
400,000	0.5	\$10,000	\$205,000	178,473	0.871
400,000	0.01	\$100	\$4,099	6,937	1.692
400,000	0.3	\$100	\$120,070	105,797	0.881
400,000	0.5	\$100	\$200,050	172,774	0.864
400,000	0.7	\$100	\$280,030	247,009	0.882
400,000	0.9	\$100	\$360,010	336,228	0.934

Figure C1: Probability Weighting and Decision Weights
With Partial Asset Integration

RDU Estimates: $r = 0.48$, $\eta = 1.12$, $\varphi = 0.84$



Appendix D: Related Literature

The closest data source is compiled by Schechter [2007], based on a sample of 188 rural Paraguayan households that made one lottery choice in an experiment and provided self-reported measures of daily income. She focuses on the integration of experimental payoffs with daily income on the day of the experiment, assuming it is all consumed on that day, and also with the integration of experimental payoffs with the present value of that daily income when inter-day savings are allowed. In each case she only considers *full asset integration*, in which experimental payoffs are added to daily income, and the intertemporal utility function is linear in current and future utility. She also reports the availability of a measure of household physical wealth, given by the self-reported value of land, animals and tools. She does not report any measures of financial wealth, which may have been negligible for this population.

Several studies of insurance data have attempted to estimate large-stakes risk aversion. The problem with naturally occurring data, of course, is identification. This is where the trade-off between controlled lab or field experiments and naturally occurring data is most clearly seen. In our case we have artefactual field experiments with non-students that are representative of a broader population, so we have “complete control” over the design of the lotteries. This permits us to conduct direct tests of one of the premises of the calibration critique, as well as ensure that we obtain well-identified estimates for each individual of EUT and RDU models of risk preferences. We also have the unusual advantage of being able to merge in naturally occurring data, the Net Wealth of the same individuals that made these lottery choices. Reliance on naturally occurring data generally makes it impossible to draw the sorts of inferences we can, but of course has the advantage of referring to non-artefactual choices over risk. We see the two approaches as complementary, each with strengths and limitations. In some cases naturally occurring data allows relatively refined inferences about large-stakes risk aversion, as illustrated in several classic studies looking at behavior towards insurance deductibles.

Cohen and Einav [2007] examine a rich data-set of choices over menus of deductibles and premium payments for auto insurance that varied across individuals. They know the premium offered, but do not know the subjective perception of the risk of a claim, or the risk that the claim will be paid in full. To proxy the latter they assume that individuals have accurate point estimates of the true distribution, a tenuous assumption, even for experienced drivers. Moreover, they must assume EUT, since they have no way to identify non-EUT models of risk preferences, and hence the calibration implications of such preferences.³ Certain non-EUT models of risk preferences, such as RDU, have been shown to dramatically affect the valuation of insurance when calibrated to estimates from real choices (Hansen, Jacobsen and Lau [2016]).

The same confounding issue arises in the evaluation by Sydnor [2010] of choices over deductibles on home insurance. By choosing lower deductibles the individual is paying a lower, certain premium, in return for a risky return given by the claim rate, and the *subjective* perception of

³ Cohen and Einav [2007] explicitly “take a neutral position” (p. 746) with respect to the calibration implications of their analysis, recognizing that “avoiding this debate is also a drawback” (p. 747) of their approach. Of course, their analysis was not intended to contribute to the debate over the calibration critique.

how often the individual expects to make a claim in the next year. Since these are lower deductibles, there is no risk attached to the *amount* that is saved by the lower deductible, so risk preferences do not play a role in this decision under EUT. But it is easy to imagine an RDU agent viewing the actual claims rate “optimistically” enough to justify these deductibles.⁴ Again, nothing in these data allow one to identify the parameters of the simplest RDU model, hence identify the calibration implications for such a specification.

Barseghyan, Molinari, O’Donoghue and Teitelbaum [2013] is an important advance in the analysis of insurance deductible choice. They exploit the fact that the decision-makers in their sample had a choice from multiple deductibles, and recognize that this allows them to identify the role of diminishing marginal utility and probability weighting, since these two channels for a risk premium have different implications at different deductible levels. They also recognize that what they call probability weighting might also be simply subjective risk perceptions that differ from the true claims rate, an important issue we return to later. Their striking result is that probability overweighting with respect to claims is, along with diminishing marginal utility, a central determinant of the risk preferences of these deductible choices. They use semi-parametric methods to infer the probability weighting function. Although such methods have some obvious attractions, they can lead to *a priori* implausible results, such as the massive jump discontinuity from the infamous probability weighting function sketch of Kahneman and Tversky [1979; Figure 4, p. 283]: claims rates of zero imply weighted claims rates of 6.5%, with 95% confidence intervals spanning 6% and 10% (Figure 1). They also estimate CRRA coefficients of 0.37 and 0.21 (p. 2524), comparable to the 0.48 we estimate in our RDU-PAI specification (Table 5).

When it comes to implications for the calibration critique, Barseghyan et al. [2013; p. 2527] hedge, suggesting that their relatively low estimate of U'' “suggests that it may be possible” to explain low-stakes and high-stakes risk aversion while maintaining “standard risk aversion,” by which they mean some degree of diminishing marginal utility. If one interprets their probability weighting in terms of an RDU model, they still require a deviation from EUT. On the other hand, they openly acknowledge that their analysis “does not enable us to say whether households are engaging in probability weighting *per se* or whether their subjective beliefs about risk simply do not correspond to the objective probabilities.” (p. 2527). The latter explanation when it requires additivity is just Subjective Expected Utility, which does not require that subjective beliefs be correct or even updated according to Bayes Rule.⁵ We return to the role of subjective beliefs below.

⁴ For example, the modal choice from the sample was to pay \$100 to get a \$500 reduction in the deductible. The actual claims rate was 0.043 in this case, at least for the claims that resulted in a payout. An RDU decision-maker with a power probability weighting function $\pi(p) = p^\gamma$ would only need $\gamma = 0.5$ to have a weighted probability and decision weight of 0.21, exceeding the 0.2 needed to justify the purchase. And it is reasonable to expect that some households might perceive the true probability as higher than 0.043, requiring even less optimism to justify the purchase. The estimated probability weighting function of Barseghyan et al. [2013; Figure 2 or Figure 4], for comparable choices by samples from comparable populations, implies a weighted probability of roughly 0.11 if one uses the actual claims rate of 0.043. Of course, this is still a violation of EUT, which is the general point being made by Sydnor [2010].

⁵ Some economists view Bayes Rule as a part of Subjective Expected Utility, but it is not. The literature in behavioral finance is clear about these two being separate, even if it challenges the descriptive

It is possible to write down non-EUT models that can explain small-stakes risk aversion as well as large-stakes risk aversion. For instance, Ang, Bekaert and Liu [2005], building on Epstein and Zin [1990], show that a recursive utility specification with a non-EU, first-order⁶ risk averse certainty equivalent, can account for both types of risk aversion. Our approach does not *require* than one adopt a non-EU specification, but of course allows for that as we illustrate with our RDU-PAI specification.

An important feature of Barberis et al. [2006] is the evaluation of small-stakes risks that are delayed, rather than resolved immediately. This requirement differentiates their specification from the model of Ang et al. [2005], who implicitly require these risks to be resolved immediately. Modeling risk over time raises many new issues, which we discussed earlier: in effect it takes us to the generalization of our approach to model multiattribute or multivariate risks.

Loss aversion was suggested by Rabin [2000] and Rabin and Thaler [2001] as a possible explanation for first-order risk aversion over small-stakes lotteries. These suggestions are more formally developed in Barberis, Huang and Thaler [2006], discussed below. Our results show that loss aversion is not necessary to account for small-stakes risk aversion and large-stakes risk aversion: none of the lotteries our subjects faced were in the loss domain or mixed domain, if one views the status quo as the reference point. Just as we were able to extend our PAI approach to consider RDU, one could extend it to Cumulative Prospect Theory, with appropriate formal modifications noted earlier.

An alternative approach to allowing for partial asset integration, adopted by Harrison, List and Towe [2007] and Heinemann [2008], is to assume $\sigma = \infty$ and estimate the composite Ω such that $v = \Omega + y$ is employed by the decision-maker using a utility function such as (3). This approach is useful, as far as it goes, to move away from the pure “utility of income” EUT model. However, it does not address the manner in which experimental prizes are integrated with wealth, which is the focus of our analysis.

validity of both. Barberis and Thaler [2005; p.1] open their survey by noting that “The traditional finance paradigm [...] seeks to understand financial markets using models in which agents are ‘rational.’ Rationality means two things. First, when they receive new information, agents update their beliefs correctly, in the manner described by Bayes’s law. Second, given their beliefs, agents make choices that are normatively acceptable, in the sense that they are consistent with Savage’s notion of Subjective Expected Utility (SEU).”

⁶ First-order risk aversion refers to a utility functional that can exhibit risk aversion for small prizes. Under FAI, and assuming wealth is significant, a differentiable utility function does not exhibit first-order risk aversion, though it can at non-differentiable points (Segal and Spivak [1990]). Under NAI it does. In context, the reference in the text is to a “disappointment aversion” model.

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Appendix E: Data Access

All computer code, and data from the laboratory experiments, is available on request. It may also be downloaded from <http://cear.gsu.edu/gwh/>. All estimations were undertaken with version 14 of *Stata*.

Access to Danish micro data (“the Registry”) follows the Act on Processing of Personal Data (in Danish, the *Lov om Behandling af Personoplysninger*). This requires a notification to the Danish Data Protection Agency whenever data are made available to researchers. Access can only be granted to researchers in authorised environments. Authorizations can be granted to public research and analysts environments (e.g., in universities, research institutes, and ministries) and to research organizations as a part of a charitable organization. Certain groups in the private sector can also obtain authorization. Only Danish institutions are granted authorization. Foreign researchers can have access to Danish micro data if they are affiliated with an appropriate Danish institution. Visiting researchers can have remote access from a workplace in the Danish research institution during their stay in Denmark, and under the Danish authorization.

Generally data for a new Registry project is obtained by the following steps:

- an appropriate Danish institution grants permission for the research project;
- the Department of Data for Research at *Statistics Denmark* is contacted at <http://www.dst.dk/en/TilSalg/Forskningservice>;
- a Project description is delivered to Statistics Denmark for approval;
- data costs are calculated and a contract is signed; and
- data is extracted to the project.

The procedure is described in detail at:

<http://www.dst.dk/-/media/Kontorer/13-Forskning-og-Metode/Step-by-step-procedures-for-researchers-access-to-Microdata.pdf?la=en>

We append this document at the end of this appendix.

The additional experimental data, which we have collected ourselves, is subsequently obtained by being transferred from our project at *Statistics Denmark* to the new project.

In practice, another way to gain access is for researchers to contact a researcher at an appropriate Danish institution that already has an approved project and is willing to provide access. That researcher would then facilitate contact with *Statistics Denmark*, and access would only be allowed if the new project was subsequently approved. So the only way that this path differs is that the “first contact” is through an existing researcher, rather than *Statistics Denmark*. In the end, direct approval from *Statistics Denmark* is always needed.

Step-by- step practices for national register-based research projects at Statistics Denmark

Below, you will find a brief – step-by-step overview for the practices and procedures when applying for access to register data through statistics Denmark’s research services.

The description is designed to outline the typical main steps seen from a researcher’s point of view from the first initial contact with statistics Denmark until the researcher gets access to register data.

Step 1: Contact:

The first step is typically that you contact one of the staff members from the Research Unit at Statistics Denmark for an initial discussion and presentation of the project. The staff members can be contacted by phone +45 3917 3130 or by E-mail. E-mail addresses can be found on the internet. <http://www.dst.dk/en/TilSalg/Forskningservice.aspx>

Step 2: Authorization:

In order to get access to micro data through Statistics Denmark, it is a pre-requisite that you are affiliated by a research environment pre-approved by Statistics Denmark. In case you are already affiliated with an authorized institution you can move directly to step 2.

In case your institution is not authorized, please contact the Research Service Unit for further information. In general, authorizations can be granted to public research and analysis environments (e.g. universities, ministries, etc.), to research organizations as a part of a non-profit foundation and under certain circumstances companies within the private sector. Statistics Denmark will not grant authorization to single persons.

In order get an authorization an application need to be submitted to the Research Service Unit at Statistics Denmark. The application should include 1) a description of the research environment; 2) name and contact information for a responsible manager of the authorization (often the Head of the Department). Statistics Denmark will evaluate the application carefully and make a concrete assessment of the applicant’s reliability as a data recipient (e.g.: ownership, educational standard among the staff and the research experience). If the institution is approved for access to micro data an authorization contract between Statistics Denmark and the research institution/environment is signed by the National Chief Statistician – Mr Jørgen Elmeskov and the responsible research manager at the institution (For details. please see appendix A).

The most important point in the authorization contracts is that the person signing the authorization agreement on behalf of the research/analysis environment undertakes personally to supervise that the confidentiality of data is always kept. The researcher may not, directly or indirectly, download the dataset

or any datasets derived there from. Furthermore, individual records may not be printed, and all output must be aggregated to an extent that eliminates any risk of direct or indirect identification of persons or enterprises. The researcher may not attempt to make such identification.

Step 3 – Project description and approval

The next step in the process is to submit a project proposal to Statistics Denmark (1-2 pages, a template can be found in appendix C). It is important to stress that access to data is given according to a so-called “need to know”-principle, implying that you can only get access to the data needed to fulfil your research purpose.

The proposal needs to cover the following paragraphs:

- Description of purpose of the research project
- Population to be studied (Need to know)
- DST registers included in the study (Need to know)
- External data (survey data or data from external registers)
- Register year
- Who will have access to data

In order to assist you in selecting the registers of relevance for your project, you can find comprehensive descriptions of most of the registers provided by Statistics Denmark in the quality declaration compiled for each statistic. The declarations describe quality and content of data as well as information about purpose, history, collection method, accuracy, reference period, and comparability over time. All quality declarations can be found on the internet on the following page: <http://www.dst.dk/en/Statistik/dokumentation/Declarations.aspx>

Most of the data derive from the administrative registers of governmental agencies, offering high data quality for the entire Danish populations of persons, buildings and companies. Data can be combined in infinite ways, allowing you to produce unique analysis of dynamic processes and fluctuations, using the Danish population. Altogether, data from 250 subject-areas are available for research purposes through Statistics Denmark. The majority of the registers fall into the following headlines:

- Population
- Education
- Labour market
- Earnings
- Social conditions
- Health
- Justice
- Income
- Business enterprises
- Housing
- Environment
- External trade

If there are external data – taken to mean data not produced by Statistics Denmark - such as survey data or data from other register owners, permission must be granted by the Danish Data Protection Agency. It is your responsibility that the permission is granted.

Step 4 – Data extraction protocol

Based on the project description the Research Service Unit will prepare a detailed data extraction protocol, including detailed lists of variables as well as sets of values, if they are of relevance. Descriptions of all variables can be found in our documentation system TIMES, which is also published on the internet, so far in Danish only.

The 600 mostly used variables have very detailed variable descriptions, including data breaches as well as appendices with special interest. This documenta-

tion is called High Quality Documentation and can be found on the following home page:
<http://www.dst.dk/da/TilSalg/Forskningservice/Dokumentation/hoekvalitet/svariable.aspx>

Documentation of other variables produced by Statistics Denmark can be found on the following pages:

Current variables:

<http://www.dst.dk/da/Statistik/dokumentation/times.aspx>

Historical variables:

<http://www.dst.dk/extranet/staticsites/TIMES3/html/Start000-0000-0000-0000-000000000000.htm>

Step 5 – Price calculation and contract is signed

Based on the data extraction protocol a price will be calculated. At present, DKK 1,229.00 (EURO ~150) is charged per hour for extraction of data (January 2013). 10 hours of free initial advisory services are given for new projects managed by public research and analysis environment. Additional charge for disk storage in the case of large-scale projects and log-on charge for private companies will be charged. Finally, a contract stating price and expected time of delivery is sent to you.

Step 6 – Data extraction and working with micro data on the servers

Data extraction will be initiated shortly after Statistic Denmark has received a signed contract. Data are anonymized by means of a project-specific key before data are entered into the researcher computer (all identifying variables such as CPR numbers, addresses etc. are replaced by project specific random numbers). When research data for a project have been prepared by the Research Service Unit, the data are transferred to the research server where remote access is given via the Internet. Communications via the Internet is protected and encrypted, which secures against unauthorized access.

However, before getting access you have to sign an agreement with Statistics Denmark., cf. the agreement in Appendix B. The agreement states that all work on micro data must take place on the research server and no attempts to identify people or enterprises – or to remove micro data must be made. It is considered a very serious breach of the agreement between the researcher and Statistics Denmark if you transfer micro data from the research server to your own computer or make attempt to identify individual persons. A breach of these rules may exclude you from using any research schemes of Statistics Denmark for a period of time or permanently. In worst cases, the authorization of your research institution might be withdrawn for a period of time.

All aggregated results from the researchers' computer can be stored in a special file and such printouts are sent to the researchers by e-mail. This is a continuous process (every five minutes) and has shown to be quite effective. The advantage for Statistics Denmark is that all e-mails are logged at Statistics Denmark and checked by the Research Service Unit. Several computer packets are available on the research server, such as SAS, SPSS, STATA, GAUSS and R. The programs are frequently updated with new versions.

APPENDIX A:

AUTHORISATION

Statistics Denmark hereby grants

Authorisation for

Remote electronic access to selected datasets at Statistics Denmark

Remote access via the Internet is subject to the following terms:

1. A project description must be submitted, which states the project objectives and renders it possible to select the data required for successful project execution.
2. Based on the project and data description, Statistics Denmark decides whether external electronic access to data can be granted for the specified project.
3. The researcher to whom external electronic access is granted shall sign a special agreement with Statistics Denmark, cf. appendix.
4. All datasets are confidential, cf. subsection 3 of section 27 of the Danish Public Administration Act and section 152 of the Danish Criminal Code.
5. The researcher obtains access to make batch runs on Statistics Denmark's special researcher machines from the research/analysis environment. Access can also via the research/analysis environment be switched to linked-up home computers in accordance with the guidelines determined by Statistics Denmark.
6. Only the client software assigned by Statistics Denmark may be applied in connection with the RSA SecurID card provided. A PC connected to Statistics Denmark may not be made available to unauthorised persons, and when the user leaves the PC, the PC must be either shut down or disconnected, i.e. protected from any unauthorised use.
7. The password of the individual researcher is personal and strictly confidential.
8. The researcher may not, directly or indirectly, download the dataset or any datasets derived there from. All transfers of output for printing or further statistical processing (in spreadsheets or similar) must be executed in accordance with the guidelines and methods laid down by Statistics Denmark. Statistics Denmark will create a log file of such authorised transfers. Furthermore, individual records may not be printed, and all output must be aggregated to an extent that eliminates any risk of direct or indirect identification of persons or enterprises. The researcher may not attempt to make such identification.
9. Statistics Denmark shall be entitled at unannounced visits to check that the rules of this agreement are observed.
10. The person signing this agreement on behalf of the research/analysis environment shall ensure that publications by the environment do not contain any information that may identify individual persons or individual enterprises.
11. The person signing this agreement on behalf of the research/analysis environment undertakes personally to supervise or to appoint a person to supervise that the provisions of this agreement are observed.
12. In case of breach of the provisions of this agreement, the researcher in breach will be excluded from using any research schemes of Statistics Denmark permanently or for a period of not less than three years. Furthermore, in the case of breach hereof, this authorisation will be withdrawn for a period.

This agreement, which is signed in two copies, enters into force on (date) and may be terminated by either party at three months' notice.

Copenhagen, date

Jørgen Elmeskov

Copenhagen, date

APPENDIX B:

Statistics Denmark

AGREEMENT

Between Statistics Denmark and **Name** of researcher has been concluded on establishing direct electronic access to selected datasets for research.

Access is given in accordance with the authorisation granted to **(Name of Institution)** by Statistics Denmark in connection with special agreement dated **(Date)**

The agreement is subject to the following terms:

1. The data sets to which access is given shall be treated as confidential information in accordance with Section 27, subsection 3 of the Danish Administration Act and Section 152 of the Danish Penal Code.
2. Processing of the basic data may only be conducted from the research environment for which the authorisation been granted, or access can also via the authorized research/analysis environment be switched to linked-up home computers in accordance with the guidelines determined by Statistics Denmark.
3. A computer linked up to Statistics Denmark may not be placed at the disposal of other persons, and the connection shall be completely turned off or disconnected, when the computer is not used, i.e. protected against unauthorized use.
4. Passwords, which are supplied by Statistics Denmark for the project are strictly personal and shall not be passed on to any third party.
5. Basic data as well as derived data sets shall not, neither directly nor indirectly, be downloaded.
6. All transfers of output (tables, analytical results), etc. for printing or for further statistical processing shall only take place in accordance with the guidelines and methods determined by Statistics Denmark. A logging of these transfers is conducted by Statistics Denmark.
7. Confidential data shall not be printed, including data at the level of individuals or firms, and all output shall be aggregated in such a manner that it is impossible to identify individual persons or individual firms directly or indirectly. Attempts at identifying individual persons or firms are not permissible.
- 8. Access to the data is given for the period: Two years with the possibility of extension.**
9. No information from the project in which it is possible to identify an individual person or individual firm may be published.
10. Published information from the project shall be submitted to Statistics Denmark for scrutiny.
11. If a "token", has been provided for the project, it shall be returned to Statistics Denmark when the agreement expires.

On attached researchers in particular.

12. The responsible person signing the agreement of authorisation for the authorised Danish institution, shall approve and assume the responsibility that all existing rules governing access to micro data are observed by the associated researcher.
13. It shall be the responsibility of the authorised Danish institution to inform the attached researcher of the rules governing the use of micro data, including the rules of confidentiality in force as well as the rules governing downloading of data

14. The associated researcher's access to micro data shall pass through the authorised Danish institution and can also be switched to linked-up home computers in accordance with the rules governing work from home
15. The authorised Danish institution appoints a contact person undertaking the responsibility for all contact with the attached researcher and Statistics Denmark.
16. All invoices concerning the attached researcher are forwarded to and paid by the authorised Danish institution in question in accordance with the terms of invoicing applicable to the institution.

A breach of the provisions of this agreement will imply that access to the data is immediately denied. Furthermore, the person who has signed this agreement will in future be excluded from using any of Statistics Denmark's research schemes. In the case of minor breaches, the person will be excluded from Statistics Denmark's research schemes temporarily for a period of not less than three years.

This agreement may be terminated by either party at 3 months' notice. If the authorisation of the research/analysis environment expires or is changed, this agreement is simultaneously cancelled.

Signatures and date:

Statistics Denmark

Researcher

The researcher's manager, who has signed the agreement of authorisation

APPENDIX C:

Project no. xxxxxx

Recommendation of Project Approval

Authorised institution

Write the name of the institution

Project title

Write the project title

Project description

Write a short description of the project including the objectives of the project.

Population

Define the population(s)

Variables included

List the registers and if possibly the variables you want to access

Please also include information on external data or own survey data if such data is part of the project. In such cases please include approval from the Danish Protection Agency.

Authorised scientists

Name, Institution, E-mail and Cell phone number of the scientists, whom will be working with the data.

Research Service, Date

Approved for external access

Ivan Thaulow