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Risk Aversion and Expected-Utility Theory: A Calibration Theorem

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Abstract

Within the expected-utility framework, the only explanation for risk aversion is that the utility function for wealth is concave: A person has lower marginal utility for additional wealth when she is wealthy than when she is poor. This paper provides a theorem showing that expected-utility theory is an utterly implausible explanation for appreciable risk aversion over modest stakes: Within expected-utility theory, for *any* concave utility function, even very little risk aversion over modest stakes implies an absurd degree of risk aversion over large stakes. Illustrative calibrations are provided.

Keywords: Diminishing Marginal Utility, Expected Utility, Risk Aversion

JEL Classifications: B49, D11, D81

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1. Introduction

Using expected-utility theory, economists model risk aversion as arising solely because the utility function over wealth is concave. This diminishing-marginal-utility-of-wealth theory of risk aversion is psychologically intuitive, and surely helps explain some of our aversion to large-scale risk: We dislike vast uncertainty in lifetime wealth because a dollar that helps us avoid poverty is more valuable than a dollar that helps us become very rich.

Yet this theory also implies that people are approximately risk neutral when stakes are small. Arrow (1971, p. 100) shows that an expected-utility maximizer with a differentiable utility function will always want to take a sufficiently small stake in any positive-expected-value bet. That is, expected-utility maximizers are (almost everywhere) arbitrarily close to risk neutral when stakes are arbitrarily small. While most economists understand this formal limit result, fewer appreciate that the approximate risk-neutrality prediction holds not just for negligible stakes, but for quite sizable and economically important stakes. Economists often invoke expected-utility theory to explain substantial (observed or posited) risk aversion over stakes where the theory actually predicts virtual risk neutrality.

While not broadly appreciated, the inability of expected-utility theory to provide a plausible account of risk aversion over modest stakes has become oral tradition among some subsets of researchers, and has been illustrated in writing in a variety of different contexts using standard utility functions.¹ In this paper, I reinforce this previous research by presenting a theorem which calibrates a relationship between risk attitudes over small and large stakes. The theorem shows that, within the expected-utility model, anything but virtual risk neutrality over modest stakes implies manifestly unrealistic risk aversion over large stakes. The theorem is entirely “non-parametric”, assuming nothing about the utility function except concavity.

In the next section I illustrate implications of the theorem with examples of the form “If an expected-utility maximizer always turns down modest-stakes gamble X, she will always turn down large-stakes gamble Y.” Suppose that, from any initial wealth level, a person turns down gambles where she loses \$100 or gains \$110, each with 50% probability. Then she will turn down 50-50 bets

¹ See Epstein (1992), Epstein and Zin (1990), Hansson (1988), Kandel and Stambaugh (1991), Loomes and Segal (1994), and Segal and Spivak (1990). Hansson’s (1988) discussion is most similar to the themes raised in this paper. He illustrates how a person who for all initial wealth levels is exactly indifferent between gaining \$7 for sure and a 50-50 gamble of gaining either \$0 or \$21 prefers a sure gain of \$7 to *any* lottery where the chance of gaining positive amounts of money is less than 40% – no matter how large the potential gain is.

of losing \$1,000 or gaining *any* sum of money. A person who would always turn down 50-50 lose \$1,000/gain \$1,050 bets would always turn down 50-50 bets of losing \$20,000 or gaining *any* sum. These are implausible degrees of risk aversion. The theorem not only yields implications if we know somebody will turn down a bet for *all* initial wealth levels. Suppose we knew a risk-averse person turns down 50-50 lose \$100/gain \$105 bets for any lifetime wealth level less than \$350,000, but knew nothing about the degree of her risk aversion for wealth levels above \$350,000. Then we know that from an initial wealth level of \$340,000 the person will turn down a 50-50 bet of losing \$4,000 and gaining \$635,670.

The intuition for such examples, and for the theorem itself, is that within the expected-utility framework turning down a modest-stakes gamble means that the marginal utility of money must diminish very quickly for small changes in wealth. For instance, if you reject a 50-50 lose \$10/gain \$11 gamble because of diminishing marginal utility, it must be that you value the 11th dollar above your current wealth by *at most* $\frac{10}{11}$ as much as you valued the 10th-to-last-dollar of your current wealth.² Iterating this observation, if you have the same aversion to the lose \$10/gain \$11 bet if you were \$21 wealthier, you value the 32nd dollar above your current wealth by at most $\frac{10}{11} \times \frac{10}{11} \approx \frac{5}{6}$ as much as your 10th-to-last dollar. You will value your 220th dollar by at most $\frac{3}{20}$ as much as your last dollar, and your 880th dollar by at most $\frac{1}{2,000}$ of your last dollar. This is an absurd rate for the value of money to deteriorate — and the theorem shows the rate of deterioration implied by expected-utility theory is actually quicker than this. Indeed, the theorem is really just an algebraic articulation of how implausible it is that the consumption value of a dollar changes significantly as a function of whether your lifetime wealth is \$10, \$100, or even \$1,000 higher or lower. From such observations we should conclude that aversion to modest-stakes risk has nothing to do with the diminishing marginal utility of wealth.

Expected-utility theory seems to be a useful and adequate model of risk aversion for many purposes, and it is especially attractive in lieu of an equally tractable alternative model. “Extremely-concave expected utility” may even be useful as a parsimonious tool for modeling aversion to modest-scale risk. But this and previous papers make clear that expected-utility theory is mani-

² My wording here, as in the opening paragraph and elsewhere, gives a psychological interpretation to the concavity of the utility function. Yet a referee has reminded me that a common perspective among economists studying choice under uncertainty has been that the concavity of the utility function need be given no psychological interpretation. I add such psychological interpretation throughout the paper as an aid to those readers who, like me, find this approach to be the natural way to think about utility theory, but of course the mathematical results and behavioral analysis in this paper hold without such interpretations.

festly not close to the right explanation of risk attitudes over modest stakes. Moreover, when the specific structure of expected-utility theory is used to analyze situations involving modest stakes — such as in research that assumes that large-stake and modest-stake risk attitudes derive from the same utility-for-wealth function — it can be very misleading. In the concluding section, I discuss a few examples of such research where the expected-utility hypothesis is detrimentally maintained, and speculate very briefly on what set of ingredients may be needed to provide a better account of risk attitudes. In the next section, I discuss the theorem and illustrate its implications.

2. Some Calibrations Based on a Theorem

Consider an expected-utility maximizer over wealth, w , with Von Neumann-Morgenstern preferences $U(w)$. Assume that the person likes money and is risk-averse: For all w , $U(w)$ is (strictly) increasing and (weakly) concave. Suppose further that, for some range of initial wealth levels and for some $g > l > 0$, she will reject bets losing $\$l$ or gaining $\$g$, each with 50% chance.³ From the assumption that these bets will be rejected, the theorem presented in this paper places an upper bound on the rate at which utility increases above a given wealth level, and a lower bound on the rate at which utility decreases below that wealth level. Its proof is a short series of algebraic manipulations; both the theorem and proof are in the appendix. Its basic intuition is straightforward, as described briefly in the introduction.

The theorem handles cases where we know a person to be averse to a gamble only for some ranges of initial wealth. A simpler corollary, also in the appendix, holds when we know a lower bound on risk aversion for *all* wealth levels. Table 1 illustrates some of the corollary’s implications: Consider an individual who is known to reject, for all initial wealth levels, 50-50, lose $\$100$ /gain g bets, for $g = \$101, \$105, \$110, \text{ and } \125 . The table presents implications of the form “the person will turn down 50-50 gambles of losing L and gaining G ,” where each L is a row in the table and the highest G (using the bounds established by the corollary) making the statement true is the entry

³ The assumption that U is concave is not implied by the fact that an agent always turns down a given better-than-fair bet; if you know that a person always turns down 50-50 lose $\$10$ / gain $\$11$ bets, you don’t know that her utility function is concave everywhere – it could be convex over small ranges. (For instance, let $U(w) = 1 - (\frac{1}{2})^w$ for $w \notin (190, 200)$, but $U(w) = 1 - (\frac{1}{2})^{19} + [(\frac{1}{2})^{19} - (\frac{1}{2})^{20}] (w - 19)^2$ for $w \in (190, 200)$.) Concavity is an additional assumption, but I am confident that results hold approximately if we allow such small and silly non-convexities.

in the table.⁴ All entries are rounded down to an even dollar amount.

L	g			
	\$101	\$105	\$110	\$125
\$400	400	420	550	1,250
\$600	600	730	990	∞
\$800	800	1,050	2,090	∞
\$1,000	1,010	1,570	∞	∞
\$2,000	2,320	∞	∞	∞
\$4,000	5,750	∞	∞	∞
\$6,000	11,810	∞	∞	∞
\$8,000	34,940	∞	∞	∞
\$10,000	∞	∞	∞	∞
\$20,000	∞	∞	∞	∞

Table 1

If averse to 50-50 lose \$100/gain g bets for all wealth levels, will turn down 50-50 lose L /gain G bets; G 's entered in table.

So, for instance, if a person always turns down a 50-50 lose \$100/gain \$110 gamble, she will always turn down a 50-50 lose \$800/gain \$2,090 gamble. Entries of ∞ are literal: Somebody who always turns down 50-50 lose \$100/gain \$125 gambles will turn down *any* gamble with a 50% chance of losing \$600. This is because the fact that the bound on risk aversion holds everywhere implies that $U(w)$ is bounded above.

The theorem and corollary are homogenous of degree 1: If we know that turning down 50-50 lose l /gain g gambles implies you will turn down 50-50 lose L /gain G , then for all $x > 0$, turning down 50-50 lose xl /gain xg gambles implies you will turn down 50-50 lose xL /gain xG . Hence the $L = \$10,000$, $g = \$101$ entry in Table 1 tells us that turning down 50-50 lose \$10/gain \$10.10 gambles implies you will turn down 50-50 lose \$1,000/gain ∞ gambles.

The reader may worry that the extreme risk aversion shown in Table 1 relies heavily on the assumption that the person will turn down the given gamble for *all* initial wealth levels. It doesn't. While without knowing a global lower bound on a person's modest-stakes risk aversion we cannot assert that she'll turn down gambles with infinite expected return, Table 2 indicates that the lack of a lower bound does not salvage the plausibility of expected-utility theory. Table 2 shows calibrations if we know the person will turn down 50-50 lose \$100/gain g gambles for initial wealth levels

⁴ The theorem provides a lower bound on the concavity of the utility function, and its proof indicates an obvious way to obtain a stronger (but uglier) result. Also, while the theorem and applications focus on "50-50 bets", the point is applicable to more general bets. For instance, if an expected-utility maximizer dislikes a bet with a 25% chance of losing \$100 and a 75% chance of winning \$100, then she would turn down 50-50 lose \$100/gain \$300 bets, and we could apply the theorem from there.

less than \$300,000, indicating which gambles she'll turn down starting from initial wealth level of \$290,000. Large entries are approximate.

	<i>g</i>			
<i>L</i>	\$101	\$105	\$110	\$125
\$400	400	420	550	1,250
\$600	600	730	990	36,000,000,000
\$800	800	1,050	2,090	90,000,000,000
\$1,000	1,010	1,570	718,190	160,000,000,000
\$2,000	2,320	69,930	12,210,880	850,000,000,000
\$4,000	5,750	635,670	60,528,930	9,400,000,000,000
\$6,000	11,510	1,557,360	180,000,000	89,000,000,000,000
\$8,000	19,290	3,058,540	510,000,000	830,000,000,000,000
\$10,000	27,780	5,503,790	1,300,000,000	7,700,000,000,000,000
\$20,000	85,750	71,799,110	160,000,000,000	540,000,000,000,000,000,000

Table 2

Table 1 replicated, for initial wealth level \$290,000, when l/g behavior is only known to hold for $w \leq \$300,000$.

If we only know that a person turns down 50-50 lose \$100/gain \$125 bets when her lifetime wealth is below \$300,000, we also know she will turn down a 50-50 lose \$600/gain \$36 billion bet beginning from lifetime \$290,000.⁵ The intuition is that the extreme concavity of the utility function between \$290,000 and \$300,000 assures that the marginal utility at \$300,000 is tiny compared to the marginal utility at wealth levels below \$290,000; hence, even if the marginal utility does not diminish at all above \$300,000, a person won't care nearly as much about money above \$300,000 as she does about amounts below \$290,000. The choice of \$290,000 and \$300,000 as the two focal wealth levels is arbitrary; all that matters is that they are \$10,000 apart. As with Table 1, Table 2 is homogenous of degree 1, where the wedge between the two wealth levels must be multiplied by the same factor as the other entries. Hence — multiplying Table 2 by 10 — if an expected-utility maximizer would turn down a 50-50 lose \$1,000/gain \$1,050 gamble whenever her lifetime wealth is below \$300,000, then from an initial wealth level of \$200,000 she will turn down a 50-50 lose \$40,000/gain \$6,356,700 gamble.

While these “non-parametric” calibrations are less conducive to analyzing more complex questions, Table 3 provides similar calibrations for decisions that resemble real-world investment choices

⁵ Careful examination of Tables 1 and 2 show that most entries that are not ∞ in Table 1 show up exactly the same in Table 2. The two exceptions are those entries that are above \$10,000 — since Table 2 implicitly makes no assumptions about concavity for gains of more than \$10,000, it yields lower numbers.

by assuming conventional functional forms of utility functions. It shows what aversion to various gambles implies for the maximum amount of money a person with a constant-absolute-risk-aversion (CARA) utility function would be willing to keep invested in the stock market, for reasonable assumptions about the distribution of returns for stocks and bonds.

l/g	ρ	X
\$100/ \$101	.0000990	\$14,899
\$100/ \$105	.0004760	\$3,099
\$100/ \$110	.0009084	\$1,639
\$100/ \$125	.0019917	\$741
\$100/ \$150	.0032886	\$449
\$1,000/ \$1,050	.0000476	\$30,987
\$1,000/ \$1,100	.0000908	\$16,389
\$1,000/ \$1,200	.0001662	\$8,886
\$1,000/ \$1,500	.0003288	\$4,497
\$1,000/ \$2,000	.0004812	\$3,067
\$10,000/ \$11,000	.0000090	\$163,889
\$10,000/ \$12,000	.0000166	\$88,855
\$10,000/ \$15,000	.0000328	\$44,970
\$10,000/ \$20,000	.0000481	\$30,665

Table 3

If a person has CARA utility function and is averse to 50/50 lose l /gain g bets for all wealth levels, then
 1) she has coefficient of absolute risk aversion no smaller than ρ and 2) invests X in the stock market when stock yields are normally distributed with mean real return 6.4% and standard deviation 20%, and bonds yield a riskless return of 0.5%.

Hence, an expected-utility maximizer with CARA preferences who turns down 50/50 lose \$1,000/gain \$1,200 gambles will only be willing to keep \$8,875 of her portfolio in the stock market, no matter how large her total investments in stocks and bonds. If she turns down a 50/50 lose \$100/gain \$110 bet, she will be willing to keep only \$1,600 of her portfolio in the stock market — keeping the rest in bonds (which average 6% lower annual return). While it is widely believed that investors are too cautious in their investment behavior, no one believes they are *this* risk averse.

3. Discussion and Conclusion

Expected-utility theory may well be a useful model of the taste for very-large-scale insurance.⁶ Despite its usefulness, however, there are reasons why it is important for economists to recognize

⁶ While there is also much evidence for some limits of its applicability for large-scale risks, and the results of this paper suggest an important flaw with the expected-utility model, the specific results do not of course demonstrate that the model is unuseful in all domains.

how miscalibrated expected-utility theory is as an explanation of modest-scale risk aversion. For instance, some research methods economists currently employ should be abandoned because they rely crucially on the expected-utility interpretation of modest-scale risk aversion. One example arises in experimental economics. In recent years, there has been extensive laboratory research in economics in which subjects interact to generate outcomes with payoffs on the order of \$10 to \$20. Researchers are often interested in inferring subjects' beliefs from their behavior. Doing so often requires knowing the relative value subjects hold for different money prizes; if a person chooses \$5 in event A over \$10 in event B, we know that she believes A is at least twice as likely as B only if we can assume the person likes \$10 at least twice as much as \$5. Yet economic theory tells us that, because of diminishing marginal utility of wealth, we should not assume people like \$10 exactly twice as much as \$5. Experimentalists (e.g., Davis and Holt (1993, pp. 472-6)) have developed a clever scheme to avoid this problem: Instead of prizes of \$10 and \$5, subjects are given prizes such as 10% chance of winning \$100 vs. 5% chance of winning \$100. Expected-utility theory tells us that, irrespective of the utility function, a subject values the 10% chance of a prize exactly twice as much as the 5% chance of winning the same prize.

The problem with this lottery procedure is that it is known to be sufficient only when we maintain the expected-utility hypothesis. But then it is not necessary — since expected-utility theory tells us that people will be virtually risk neutral in decisions on the scale of laboratory stakes. If expected-utility theory is right, these procedures are at best redundant, and are probably harmful.⁷ On the other hand, if we think that subjects in experiments *are* risk averse, then we know they are not expected-utility maximizers. Hence the lottery procedure, which is motivated solely by expected-utility theory's assumptions that preferences are linear in probabilities and that risk attitudes come only from the curvature of the utility-of-wealth function, has little presumptive value in "neutralizing" risk aversion. Perhaps there are theories of risk attitudes such that the lottery

⁷ If expected-utility theory explained behavior, these procedures would surely not be worth the extra expense, nor the loss in reliability of the data from making experiments more complicated. Nor should experimentalists who believe in expected utility theory ever be cautious about inferences made from existing experiments that don't use the lottery methods out of fear that the results are confounded by the subjects' risk attitudes.

procedure is useful for neutralizing risk aversion — but expected-utility theory isn't one of them.⁸

A second example of problematic research methods relates to the logic underlying the theorem: Expected-utility theory makes wrong predictions about the relationship between risk aversion over modest stakes and risk aversion over large stakes. Hence, when measuring risk attitudes maintaining the expected-utility hypothesis, differences in estimates of risk attitudes may come from differences in the scale of risk comprising data sets, rather than from differences in risk attitudes of the people being studied.⁹ Data sets dominated by modest-risk investment opportunities are likely to yield much higher estimates of risk aversion than data sets dominated by larger-scale investment opportunities. So not only are standard measures of risk aversion somewhat hard to interpret given that people are not expected-utility maximizers, but even attempts to compare risk attitudes so as to compare across groups will be misleading unless economists pay due attention to the theory's calibrational problems.

The problems with assuming that risk attitudes over modest and large stakes derive from the same utility-of-wealth function relates to a long-standing debate in economics. Expected-utility theory makes a powerful prediction that economic actors don't see an amalgamation of independent gambles as significant insurance against the risk of those gambles; they are either barely less willing or barely more willing to accept risks when clumped together than when apart. This observation was introduced in a famous article by Samuelson (1963), who showed that expected-utility theory implies that if (for some sufficiently wide range of initial wealth levels) a person turns down a particular gamble, then she should also turn down an offer to play $n > 1$ of those gambles. Hence, in his example, if Samuelson's colleague is unwilling to accept a 50-50 lose \$100/gain \$200 gamble, then he should be unwilling to accept 100 of those gambles taken together. Though Samuelson's theorem is "weaker" than the one in this paper, it makes manifest the fact that expected-utility

⁸ Indeed, the observation that diminishing marginal utility of wealth is irrelevant in laboratory experiments raises questions about interpreting experimental tests of the adequacy of expected-utility theory. For instance, while showing that existing alternative models better fit experimental data than does expected-utility theory, Harless and Camerer (1994) show that expected-utility theory better fits experimental data than does "expected-value theory" — risk-neutral expected-utility theory. But because expected-utility implies that laboratory subjects should be risk neutral, such evidence that expected-utility theory explains behavior better than expected-value theory is evidence *against* expected-utility theory.

⁹ Indeed, Kandel and Stambaugh (1991, pp. 68-69) discuss how the plausibility of estimates for the coefficient of relative risk aversion may be very sensitive to the scale of risk being examined. Assuming constant risk aversion, they illustrate how a coefficient of relative risk aversion needed to avoid predicting absurdly large aversion to a 50/50 lose \$25,000/gain \$25,000 gamble generates absurdly little aversion to a 50/50 lose \$400/gain \$400 gamble. They summarize such examples as saying that "Inferences about [the coefficient of relative risk aversion] are perhaps most elusive when pursued in the introspective context of thought experiments." But precisely the same problem makes inferences from real data elusive.

theory predicts that adding together a lot of independent risks should not appreciably alter attitudes towards those risks.

Yet virtually everybody would find the aggregated gamble of 100 50-50 lose \$100/gain \$200 bets attractive. It has an expected yield of \$5,000, with negligible risk: There is only a $\frac{1}{700}$ chance of losing any money and merely a $\frac{1}{25,000}$ chance of losing more than \$1,000.¹⁰ While nobody would turn down this gamble, many people, such as Samuelson's colleague, might reject the single 50-50 lose \$100/gain \$200 bet.¹¹ Hence, using expected-utility theory to make inferences about the risk attitudes towards the amalgamated bet from the reaction to the one bet — or vice versa — would be misleading.

What *does* explain risk aversion over modest stakes? While this paper provides a “proof by calibration” that expected-utility theory does not help explain some risk attitudes, there are of course more direct tests showing that alternative models better capture risk attitudes. There is a large literature (see Machina (1987) and Camerer (1992) for reviews) of formal models of such alternatives. Many of these models seem to provide a more plausible account of modest-scale risk attitudes, allowing both substantial risk aversion over modest stakes and non-ridiculous risk aversion over large stakes, and researchers (e.g., Segal and Spivak (1990), Loomes and Segal (1994), Epstein and Zin (1990)) have explicitly addressed how non-expected-utility theory can help account for small-stake risk aversion.

Indeed, what is empirically the most firmly established feature of risk preferences, *loss aversion*, is a departure from expected-utility theory that provides a direct explanation for modest-scale risk aversion. Loss aversion says that people are significantly more averse to losses relative to the status quo than they are attracted by gains, and more generally that people's utilities are determined by changes in wealth rather than absolute levels.¹² Preferences incorporating loss aversion can reconcile significant small-scale risk aversion with reasonable degrees of large-scale risk aver-

¹⁰ The theorem in this paper predicts that, under exactly the same assumptions invoked by Samuelson, turning down a 50-50 lose \$100/gain \$200 gamble implies the person turns down a 50-50 lose \$200/gain \$20,000 gamble. This has an expected return of \$9,900 – with zero chance of losing more than \$200.

¹¹ As Samuelson noted, the strong statement that somebody should turn down the many bets if and only if she turns down the one is not strictly true if a person's risk attitudes change at different wealth levels. Indeed, many researchers (e.g., Hellwig (1995) and Pratt and Zeckhauser (1987)) have explored features of the utility function such that an expected-utility maximizer might take a multiple of a favorable bet that they would turn down in isolation. But characterizing such instances isn't relevant to examples of the sort discussed by Samuelson. We know from the unwillingness to accept a 50/50 lose \$100/ gain \$200 gamble that Samuelson's colleague was not an expected-utility maximizer.

¹² Loss aversion was introduced by Kahneman and Tversky (1979) as part of the more general “prospect theory”, and is reviewed in Kahneman, Knetsch, and Thaler (1991). Tversky and Kahneman (1991) and others have estimated the loss-aversion-to-gain-attraction ratio to be about 2:1.

sion.¹³ A loss-averse person will, for instance, be likely to turn down the one 50/50 lose \$100/gain \$200 gamble Samuelson's colleague turned down, but will surely accept one hundred such gambles pooled together. Variants of this or other models of risk attitudes can provide useful alternatives to expected-utility theory that can reconcile plausible risk attitudes over large stakes with non-trivial risk aversion over modest stakes.¹⁴

Appendix: The Theorem and a Corollary

Theorem: Suppose that for all w , $U(w)$ is strictly increasing and weakly concave. Suppose that there exists $\bar{w} > \underline{w}$, $g > l > 0$ such that for all $w \in [\underline{w}, \bar{w}]$, $.5U(w - l) + .5U(w + g) < U(w)$.

Then for all $w \in [\underline{w}, \bar{w}]$, for all $x > 0$,

$$1) \text{ If } g \leq 2l \text{ then } U(w) - U(w - x) \geq \begin{cases} 2 \sum_{i=1}^{k^*(x)} \left(\frac{g}{l}\right)^{i-1} r(w) & \text{if } w - \underline{w} + 2l \geq x \geq 2l \\ 2 \left[\sum_{i=1}^{k^*(w - \underline{w} + 2l)} \left(\frac{g}{l}\right)^{i-1} r(w) \right] + [x - (w - \underline{w} + l)] \left(\frac{g}{l}\right)^{k^*(w - \underline{w} + 2l)} r(w) & \text{if } x \geq \bar{w} - w + 2l \end{cases}$$

$$2) U(w + x) - U(w) \leq \begin{cases} \sum_{i=0}^{k^{**}(x)} \left(\frac{l}{g}\right)^i r(w) & \text{if } x \leq \bar{w} \\ \sum_{i=0}^{k^{**}(\bar{w})} \left(\frac{l}{g}\right)^i r(w) + [x - \bar{w}] \left(\frac{l}{g}\right)^{k^{**}(\bar{w})} r(w) & \text{if } x \geq \bar{w} \end{cases}$$

where, letting $\text{int}(y)$ denotes the smallest integer less than or equal to y , $k^*(x) \equiv \text{int}\left(\frac{x}{2l}\right)$, $k^{**}(x) \equiv \text{int}\left(\frac{x}{g} + 1\right)$, and $r(w) \equiv U(w) - U(w - l)$.

Proof of Part 1 of Theorem: For notational ease and without loss of generality, let $r(w) \equiv U(w) - U(w - l) = 1$. Then clearly $U(w - l) - U(w - 2l) \geq 1$, by the concavity of $U(\cdot)$. Also, since

¹³ While most formal definitions of loss aversion have not made explicit the assumption that people are substantially risk averse even for very small risks (but see Bowman, Minehart, and Rabin (1999) for an explicit treatment of this issue), most examples and calibrations of loss aversion imply such small-scale risk aversion.

¹⁴ But Kahneman and Lovallo (1993), Benartzi and Thaler (1995), and Read, Loewenstein, and Rabin (1998) argue that an additional departure from standard assumptions is implicated in many risk attitudes is that people tend to isolate different risky choices from each other in ways that lead to different behavior than would ensue if these risks were considered jointly. Samuelson's colleague, for instance, might reject each 50/50 lose \$100/gain \$200 gamble if on each of 100 days he were offered one such gamble, whereas he might accept all of these gambles if they were offered to him at once. Benartzi and Thaler (1995) argue that a related type of myopia is an explanation for the "equity premium puzzle" — the mystery about the curiously large premium on returns that investors seem to demand to compensate for riskiness associated with investing in stocks. Such risk aversion can be explained with plausible (loss-averse) preferences — if investors are assumed to assess gains and losses over a short-run (yearly) horizon rather than the longer-term horizon for which they are actually investing.

$2l > g > l$, we know that $w - 2l + g \in (w - l, w)$, and by the concavity of $U(\cdot)$ we know that $U(w - 2l + g) - U(w - l) \geq \frac{g-l}{l} = \frac{g}{l} - 1$. Hence, $U(w - 2l + g) - U(w - 2l) \geq \frac{g}{l} - 1 + 1 = \frac{g}{l}$.

Hence, if $w - 2l \geq \underline{w}$, we know that $U(w - 2l) - U(w - 3l) \geq \frac{g}{l}$ since by assumption, $U(w - 2l - l) + U(w - 2l + g) \leq 2u(w - 2l)$. By concavity, we also know that $U(w - 3l) - U(w - 4l) \geq \frac{g}{l}$. More generally, if $w - 2kl \geq \underline{w}$, then

$$\begin{aligned} U(w - (2k - 1)l) - U(w - 2kl) &\geq U(w - 2(k - 1)l) - U(w - (2k - 1)l) \\ \Rightarrow U(w - 2kl + g) - U(w - 2kl) &\geq \frac{g}{l}[U(w - 2(k - 1)l) - U(w - (2k - 1)l)] \\ \Rightarrow U(w - 2kl) - U(w - (2k + 1)l) &\geq \frac{g}{l}[U(w - 2(k - 1)l) - U(w - (2k - 1)l)] \end{aligned}$$

These lower bounds on marginal utilities yield the lower bound on total utilities $U(w) - U(w - x)$ in part 1 of the theorem.¹⁵

Proof of Part 2 of Theorem: Again let $r(w) \equiv U(w) - U(w - l) = 1$. Then $U(w + g) - U(w) \leq 1$. By the concavity of U , $U(w + g) - U(w + g - l) \leq \frac{l}{g}$. But if $w + g \leq \bar{w}$, this implies by assumption that $U(w + 2g) - U(w + g) \leq \frac{l}{g}$ (since $U(w + g - l) + U(w + 2g) \leq 2U(w + g)$).

More generally, we know that if $w + mg \leq \bar{w}$, then $U(w + mg + g) - U(w + mg) \geq \frac{l}{g}[U(w + mg) - U(w + mg - g)]$. These upper bounds on marginal utilities yield the upper bound on utilities $U(w + x) - U(w)$ in part 2 of the theorem.

Corollary: Suppose that for all w , $U'(w) > 0$ and $U''(w) < 0$. Suppose there exists $g > l > 0$ such that, for all w , $.5U(w - l) + .5U(w + g) < U(w)$. Then for all positive integers k , $\forall m < m(k)$, $.5(w - 2kl) + .5U(w + mg) < U(w)$, where $m(k) \equiv$

$$\begin{cases} \frac{\ln \left[1 - \left(1 - \frac{l}{g}\right) 2 \sum_{i=1}^k \left(\frac{g}{l}\right)^i \right]}{\ln \frac{l}{g}} - 1 & \text{if } 1 - \left(1 - \frac{l}{g}\right) 2 \sum_{i=1}^k \left(\frac{g}{l}\right)^i > 0 \\ \infty & \text{if } 1 - \left(1 - \frac{l}{g}\right) 2 \sum_{i=1}^k \left(\frac{g}{l}\right)^i \leq 0 \end{cases}$$

Proof of Corollary: From the proof of the Theorem, we know $U(w) - U(w - 2kl) \geq 2 \sum_{i=1}^k \left(\frac{g}{l}\right)^{i-1} r(w)$ and $U(w + mg) - U(w) \leq \sum_{i=0}^{m+1} \left(\frac{l}{g}\right)^i r(w)$. Therefore, if $U(w) - U(w - 2kl) < U(w + mg) - U(w)$,

¹⁵ The theorem is weaker than it could be. If we observe, for all m such that $2 \leq m \leq w - \bar{w} + 1$, that $U(w - m) - U(w - m - 1) \geq U(w - m + 1) - U(w - m) + \left(\frac{g}{l} - 1\right) [U(w - m + 2) - U(w - m + 1)]$, we can prove a stronger (but far messier) theorem. (The current theorem merely invokes $U(w - m) - U(w - m - 1) \geq U(w - m + 1) - U(w - m)$ for even m 's.)

then $2 \sum_{i=1}^k \left(\frac{g}{l}\right)^{i-1} < \sum_{i=0}^{m+1} \left(\frac{l}{g}\right)^i$. Solving for m yields the formula. Note that if $g > 2l$, we only need $U(w) - U(w - 2kl) \geq 2k(U(w) - U(w - 1))$ to get the result.

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