A Truth-Serum for Non-Bayesians: Correcting Proper Scoring Rules for Risk Attitudes[1]

Theo Offerman[a], Joep Sonnemans[a], Gijs van de Kuilen[b], & Peter P. Wakker[c]
a: CREED, Dept. of Economics, University of Amsterdam, Roetersstraat 11, Amsterdam, 1018 WB, The Netherlands
b: TIBER, Dept. of Economics, Tilburg University, P.O. Box 90153, Tilburg, 5000 LE, The Netherlands
c: Econometric Institute, Erasmus University, P.O. Box 1738, Rotterdam, 3000 DR, the Netherlands

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ABSTRACT. Proper scoring rules provide convenient and highly efficient tools for incentive compatible elicitations of subjective beliefs. As traditionally used, however, they are valid only under expected value maximization. This paper shows how they can be generalized to modern ("nonexpected utility") theories of risk and ambiguity, yielding mutual benefits: people using proper scoring rules can benefit from the empirical realism of nonexpected utility, and people analyzing ambiguity attitudes can benefit from the efficient measurements through proper scoring rules. An experiment demonstrates the feasibility of our generalized proper scoring rule.

KEY WORDS: belief measurement, proper scoring rules, ambiguity, Knightian uncertainty, subjective probability, nonexpected utility
JEL-CLASSIFICATION: D81, C60, C91

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1. Introduction

An important problem in mechanism design concerns the elicitation of private information. A design is incentive compatible if the actions of agents, motivated solely by self interest, will nevertheless reveal their true private information (Hurwicz 1960). A social planner can then use all relevant information to devise the most efficient social allocation. This paper considers the case where the private information concerns subjective beliefs about the likelihood of uncertain events, often modelled through subjective probabilities. This case arises, for instance, when principals rely on the judgments of specialized agents. In the absence of proper incentives, agents may pretend to be more confident about their judgment than they really are, and may not update beliefs sufficiently, so as to suggest greater ability than they really have (Li 2007). Manski (2004) presented an historical survey of belief measurement, and gave many economic applications.

For belief measurement, incentive compatible mechanisms had been known at an early stage in the form of proper scoring rules (Brier 1950; Good 1952). These scoring rules are particularly efficient mechanisms for eliciting subjective beliefs in an incentive compatible manner. They use cleverly designed optimization problems where the observation of one single choice suffices to determine the exact quantitative degree of belief of an agent in an uncertain event. Hence, they have recently become popular in experimental economics and game theory (Nyarko & Schotter 2002). Proper scoring rules have been used in many other fields in the social sciences, including accounting (Wright 1988), Bayesian statistics (Savage 1971), business (Staël von Holstein 1972), education (Echternacht 1972), finance (Johnstone 2007a,b; Shiller, Kon-Ya, & Tsutsui 1996), medicine (Spiegelhalter 1986), meteorology (Palmer & Hagedorn 2006; Yates 1990), politics (Tetlock 2005), psychology (McClelland & Bolger 1994), and other fields (Hanson 2002; Prelec 2004).

An alternative way to elicit beliefs that has recently become popular concerns prediction markets on internet (Wolfers & Zitzewitz 2004). Here people trade event-contingent payments regarding uncertain events, such as a guarantee to receive €100 if a Democrat becomes the next president of the United States. If this guarantee is now traded at a price P, then P/100 is taken as the market-probability of the event. This inference assumes expected value. Gjerstad (2004) discussed deviations from expected value. Johnstone (2007a) explained that a financial market can, for many purposes, be analyzed as if being a rational

individual, and discussed the role of proper scoring rules in such settings. In a market with log-utility maximizing agents, good forecasters are better identified through their performance in terms of proper scoring rules than in terms of their average earnings (Johnstone 2007b).

Whereas all applications of proper scoring rules that we are aware of assume expected value maximization ("risk neutrality"), many deviations have been observed empirically. Under expected utility, risk aversion is the common finding (Bernoulli 1738). Johnstone (2007a) and Winkler & Murphy (1970) discussed the implications thereof for proper scoring rules. Further, many deviations from expected utility have been found, both when probabilities exist ("risk"; Allais 1953; Kahneman & Tversky 1979) and when probabilities cannot even be specified ("ambiguity"; Ellsberg 1961; Keynes 1921; Knight 1921).

This paper updates proper scoring rules from the expected value model as assumed in the 1950s, when proper scoring rules were introduced, to the current state of the art in decision theory. Thus we can, on the one hand, improve the validity of belief measurement through proper scoring rules. On the other hand, we can use proper scoring rules to obtain more efficient methods for measuring risk and ambiguity attitudes. In economics, probabilities are usually not known, and the importance of quantitative measurements of ambiguity attitudes has been widely understood (Gilboa 2004; Greenspan 2004). We will show how subjective beliefs and ambiguity attitudes can be isolated from risk attitude in a surprisingly easy way by means of proper scoring rules. Thus we can correct measurements of subjective beliefs and ambiguity attitudes for nonneutral risk attitudes.

We illustrate the feasibility of our method through an experiment where we measure the subjective beliefs of participants about the future performance of stocks after provision of information about past performance. The empirical findings confirm the usefulness of our method. Violations of additivity of subjective beliefs are reduced but not eliminated by our corrections. Thus, the classical measurements will contain violations of additivity that are partly due to the incorrect assumption of expected value maximization, but partly they are genuine. Subjective beliefs are genuinely nonadditive.

The analysis of this paper consists of three parts. The first part (§§2-4) considers various modern theories of risk and ambiguity, and derives implications for proper scoring rules. The second part of the paper, §5 and §6, applies the revealed-preference technique to the results of the first part. That is, we do not assume theoretical models to derive implications for empirical observations, but we use empirical observations to derive implications for theoretical models. §5 presents the main result of this paper, showing how subjective beliefs

can easily be derived from observed choices using so-called risk-corrections. §6 presents an example illustrating such a derivation at the individual level. Readers solely interested in applying our method empirically can skip most of §§3-5, only reading Corollary 5.4.

The third part of the paper (§§7-11) implements our correction method in an experiment. To illustrate the applicability of our method, we obtain some implications for nonadditive beliefs and for different implementations of real incentives. §7 contains methodological details. §8 presents results regarding the biases that we correct for, §9 presents some implications of the corrections of such biases, and §10 presents an additional control treatment. The experimental results are discussed in §11. A general discussion and conclusions are in §§12-13. Appendix A presents technical results, Appendix B presents proofs, Appendix C surveys the implications of modern decision theories for our measurements, and Appendix D presents details of the experimental instructions.

2. Proper Scoring Rules; Definitions

Let E denote an event of which an agent is uncertain about whether or not it obtains, such as whether a stock's value will decrease during the next six months. The degree of uncertainty of the agent about E will obviously depend on the information that the agent possesses about E. For most uncertain events, no objective probabilities of occurrence are known, and decisions have to be based on subjective likelihood assessments.

Prospects designate event-contingent payments. We use the general notation $x_E y$ for a *prospect* that yields outcome x if event E obtains and outcome y if E^c obtains, with E^c the *complementary event* not-E. Outcomes designate money amounts. *Risk* concerns the special case of known probabilities. Then, for a prospect $x_E y$, the probability p of event E is known. We identify this prospect with a probability distribution $x_p y$ over money, yielding x with probability p and y with probability 1−p.

This paper considers the *quadratic scoring rule* (*QSR*), the most commonly used proper scoring rule (McKelvey & Page 1990; Nyarko & Schotter 2002; Palfrey & Wang 2007). A *qsr-prospect*

$$(1-(1-r)^2)_E(1-r^2) \qquad (2.1)$$

is offered to the agent, where $0 \le r \le 1$ is chosen at the agent's discretion. This number r is a function of E, sometimes denoted r_E, and is called the (*uncorrected*) *reported probability* of E. The reasons for using this term will be explained later. If event E has an (objective or subjective) probability p, then according to all theories considered r_E will depend only on p, so that we can write it as a function R(p). More general prospects $(a–b(1–r)^2)_E(a–br^2)$ for any $b > 0$ and $a \in \mathbb{R}$ can be considered, but for simplicity we restrict our attention to $a = b = 1$. No negative payments can occur, so that the agent never loses money. Under the event that happens, the QSR in fact yields 1 minus the squared distance between the reported probability of a clairvoyant (who assigns probability 1 to the event that happens) and the reported probability of the agent (r under E, 1–r under E^c). The following observation about a symmetry of the QSR will be useful.

OBSERVATION 2.1. The quadratic scoring rule for event E presents the same choice of prospects as the quadratic scoring rule for event E^c, with each prospect resulting from r as reported probability of E identical to the prospect resulting from 1–r as reported probability of E^c. □

Because of Observation 2.1, we have

$$r_{E^c} = 1– r_E \qquad (2.2)$$

and

$$R(1–p) = 1 – R(p). \qquad (2.3)$$

Hence, we will state many results only for $r \ge 0.5$. The case $r < 0.5$ then follows from these equations applied to E^c.

3. A Theoretical Analysis of Proper Scoring Rules

In this section we consider modern decision models for decision making under uncertainty, and derive implications for proper scoring rules. As explained in detail in Appendix C, virtually all presently existing models, including multiple priors (Gilboa & Schmeidler 1989) and Choquet expected utility (Gilboa 1987; Schmeidler 1989) evaluate the qsr-prospect of Eq. 2.1 through the following formula.

For $r \geq 0.5$: $W(E)U(1-(1-r)^2) + (1-W(E))U(1-r^2)$. (3.1)

Comments on the case $r < 0.5$ follow later. U is the *utility function*, assumed to be continuous and strictly increasing, and scaled such that $U(0) = 0$. We present a number of cases for W, with each case generalizing the preceding one. Cases 1 and 2 are well known.

CASE 1 [*Expected Value*]. U is the identity function and W is a probability measure P.

CASE 2 [*Expected Utility*]. W is a probability measure P.

CASE 3 [*Probabilistic Sophistication* (with nonexpected utility)]. There exist a probability measure P and a continuous strictly increasing function w, the *probability weighting function*, such that $W(.) = w(P(.))$, $w(0) = 0$, and $w(1) = 1$.

CASE 4 [*General Model*]. W satisfies: (i) $W(\varnothing) = 0$; (ii) $W = 1$ for the universal event; (iii) $C \supset D$ implies $W(C) \geq W(D)$.

We distinguish two subcases for Case 3 and, hence, also for Cases 2 and 1.

SUBCASE a. [*Objective Probabilities*]. The probability measure P is objective, based on statistical data that everyone agrees on.

SUBCASE b. [*Subjective Probabilities*]. The probability measure P may be subjective, and can be revealed from preferences.[2]

De Finetti (1937), Savage (1954), and Machina & Schmeidler (1992) give preference foundations for Cases 1.b, 2.b, and 3.b. Case 3 is an interesting intermediate case, with the Bayesian principles violated at the level of decisions but not at the level of beliefs. In the general Case 4, the Bayesian principles are also violated at the level of beliefs. The well-known Allais (1953) paradox shows that expected utility is often violated, so that w and W are nonadditive and we cannot restrict attention to classical Cases 1 and 2. The well-known

[2] In this paper, the term *subjective probability* is used only for probability judgments that are Bayesian in the sense of satisfying the laws of probability. In the literature, the term subjective probability has sometimes been used for judgments that deviate from the laws of probability, including cases where these judgments are nonlinear transformations of objective probabilities when the latter are given. We use the term (probability) weights or beliefs, depending on the way of generalization, to designate the latter.

Ellsberg (1961) paradox, discussed in detail later, shows that probabilistic sophistication is often violated, so that the general Case 4 has to be considered.

For the general model, the formula to evaluate the qsr-prospect of Eq. 2.1 for $r < 0.5$ follows from Observation 2.1:

$$\text{For } r < 0.5\text{: } (1-W(E^c))U(1-(1-r)^2) + W(E^c)U(1-r^2). \tag{3.2}$$

For expected value and expected utility, Eq. 3.2 agrees with Eq. 3.1 and the two formulas can be used interchangeably, but for probabilistic sophistication and the general model Eq. 3.2 can be different. The latter separate, "rank-dependent," way of weighting the outcomes, with weights always summing to 1, was discovered independently by Quiggin (1982) for the special case of risk with given probabilities, and by Schmeidler (1989; first version 1982) for the general model. This idea was the key to the development of the modern nonexpected utility theories. It was incorporated in the new version of prospect theory (Tversky & Kahneman 1992).

Objective probabilities can best be interpreted as a special limiting case of subjective probabilities, a point formalized by Machina (2004). The hypothetical situation of an agent using a subjective probability different than an objective probability if the latter is given cannot arise under plausible assumptions.[3]

We now analyze which optimal values r_E are predicted under the various cases considered.

THEOREM 3.1. In the general model, the optimal choice r in Eq. 2.1 satisfies:

$$\text{If } r > 0.5, \text{ then } r = r_E = \frac{W(E)}{W(E) + (1-W(E))\dfrac{U'(1-r^2)}{U'(1-(1-r)^2)}}\ . \tag{3.3}$$

□

[3] The first plausible assumption is what defines decision under risk: that the only relevant aspect of an event is its objective probability, and the second is that we have sufficient richness of events to carry out the following reasoning. The claim follows first for equally-probable n-fold partitions of the universal event, where because of symmetry all events must have both objective and subjective probabilities equal to 1/n. It then follows for all events with rational probabilities because they are unions of the former events. Finally, it follows for all remaining events by proper continuity or monotonicity conditions. There have been several misunderstandings about this point (Edwards 1954, p. 396; Schoemaker 1982, Table 1).

The optimality result for $r < 0.5$ follows from Observation 2.1 applied to E^c. If $r = 0.5$ is optimal then it can be a boundary solution for which Eq. 3.3 need not hold. We will discuss this case later. Theorem 3.1 generalizes results, obtained by Winkler & Murphy (1970) for expected utility, to general nonexpected utility. The following corollary, first found by Brier (1950), is highly appealing, and is to the best of our knowledge the first incentive compatible result provided in the literature.

COROLLARY 3.2. Under expected value, Eq. 3.3 holds for all r and $r = r_E = P(E)$. □

Thus, under expected value, it is in the agent's best interest to report true subjective probabilities. Proper scoring rules have been widely used to elicit subjective beliefs. In virtually all such studies, expected value is assumed. Given the widespread empirical violations of expected value, known since Bernoulli (1738), an empirically more realistic analysis of proper scoring rules is warranted. The following section illustrates the extent to which reported probabilities, still commonly equated with subjective probability in virtually all applications today, can deviate from subjective probabilities due to empirical deviations from expected value. We next consider the case $r = 0.5$ under expected utility.

OBSERVATION 3.3. Under expected utility with probability measure P, $r_E = 0.5$ implies $P(E) = 0.5$. Conversely, $P(E) = 0.5$ implies $r_E = 0.5$ if risk aversion holds. Under risk seeking, $r_E \neq 0.5$ is possible if $P(E) = 0.5$. □

4. Discrepancies between Subjective Probabilities and Proper Scoring Rules; Numerical Examples

The solutions r presented in this section can be verified through substitution in the implicit Eq. 3.3. We will later provide explicit expressions for $R^{-1}(p)$, which we used to find the solutions and to draw Figure 4.1. We consider two urns each containing 100 balls that are Crimson, Green, Silver, or Yellow. Urn K ("known") contains 25 balls of each colour, and urn A ("ambiguous") contains the balls in an unknown proportion. One ball will be drawn at random from each urn. C designates the event of a crimson ball drawn from urn K, and G, S,

and Y are similar. E is the event that the ball drawn from K is not crimson, i.e. it is the event $C^c = \{G,S,Y\}$. C_a designates the event of a crimson ball drawn from urn A, with K_a, G_a, and S_a similar, and $E_a = C_a^c$. Subjects are asked to report their belief in event E and are rewarded with a QSR (Eq. 2.1). We consider the four cases presented in the preceding section.

CASE 1 [*Expected Value*]. Expected value holds for urn K. Then $r_{E_K} = R(0.75) = 0.75$ is optimal in Eq. 2.1. The point r_E is depicted as r^{EV} in Figure 4.1, at p = 0.75. Corollary 3.2 implies that $r_G = r_S = r_Y = 0.25$. The reported probabilities satisfy additivity: $r_G + r_S + r_Y = r_E$.

CASE 2 [*Expected Utility*]. Expected utility holds for urn K, with $U(x) = x^{0.5}$. We obtain $r_E = R(0.75) = 0.69$, depicted as r^{EU} in Figure 4.1, at p = 0.75. The expected value of the resulting qsr-prospect is 0.0031 (i.e., 0.8125 – 0.8094) less than it was in Case 1. This difference can be interpreted as a risk premium, designating a profit margin for an insurance company. By Eq. 2.2, $r_C = 0.31$, and by symmetry $r_G = r_S = r_Y = 0.31$ too. The reported probabilities violate additivity, with $r_G + r_S + r_Y = 0.93 > 0.69 = r_E$. Through this violation, data can directly reveal that expected value, the common assumption in applications of proper scoring rules, does not hold. □

CASE 3 [*Nonexpected Utility with Probabilistic Sophistication*]. Probabilistic sophistication holds for urn K, with $U(x) = x^{0.5}$ and

$$w(p) = \left(exp(-(-ln(p))^{\alpha})\right) \qquad (4.1)$$

with parameter $\alpha = 0.65$ (Prelec 1998). This function agrees with the prevailing empirical findings (Tversky & Kahneman 1992; Abdellaoui 2000; Bleichrodt & Pinto 2000; Gonzalez & Wu 1999). We obtain $r_E = R(0.75) = 0.61$, depicted as r^{nonEU} in Figure 4.1 at p = 0.75. The extra expected-value loss (and, hence, the extra risk premium) relative to Case 2 is 0.0174 (i.e., 0.8094 – 0.7920). By Eq. 2.3, $r_C = 0.39$, and by symmetry $r_G = r_S = r_Y = 0.39$ too. The reported probabilities strongly violate additivity, because $r_G + r_S + r_Y = 1.17 > 0.61 = r_E$. □

The following case describes the most fundamental deviation from expected value and expected utility, driven by ambiguity, a central topic in decision theory today. The case concerns a version of Ellsberg's (1961) paradox.

CASE 4 [*General Case; Violation of Probabilistic Sophistication*]. We assume probabilistic sophistication for urn K but consider, in addition, urn A using the general model. If probabilities were assigned to drawings from urn A and probabilistic sophistication were to hold also for this urn, then, in view of symmetry, we should have $P(C_a) = P(G_a) = P(S_a) = P(Y_a)$. Then these probabilities would be 0.25. $P(E_a)$ then would be 0.75, as was P(E) in Case 3. Under probabilistic sophistication combined with nonexpected utility as in Case 3, r_{E_a} would be the same as r_E in Case 3 for the known urn, i.e. $r_{E_a} = 0.61$. It implies that people would be indifferent between $x_E y$ and $x_{E_a} y$ for all x and y. The latter condition is, however, typically violated empirically. People usually have a strict preference for known probabilities, implying for instance

$$100_E 0 \succ 100_{E_a} 0 .^{4} \qquad (4.2)$$

Consequently, it is impossible to model beliefs about uncertain events E_a through probabilities, and probabilistic sophistication fails. Eq. 4.2 implies that $W(E_a) < W(E)$. By Eq. 3.1, $r_{E_a} < r_E$.[5] Given the strong aversion to unknown probabilities that is often found empirically (Camerer & Weber 1992), we will assume that $r_{E_a} = 0.52$. It is depicted as r^{nonEUa} in Figure 4.1. The extra expected-value loss relative to Case 3 is 0.7920 – 0.7596 = 0.0324. This amount can be interpreted as the ambiguity-premium. By Eq. 2.3, $r_C = 0.48$, and by symmetry $r_G = r_S = r_Y = 0.48$ too. The reported probabilities violate additivity to an extreme degree, with $r_G + r_S + r_Y = 1.44 > 0.52 = r_{E_a}$. □

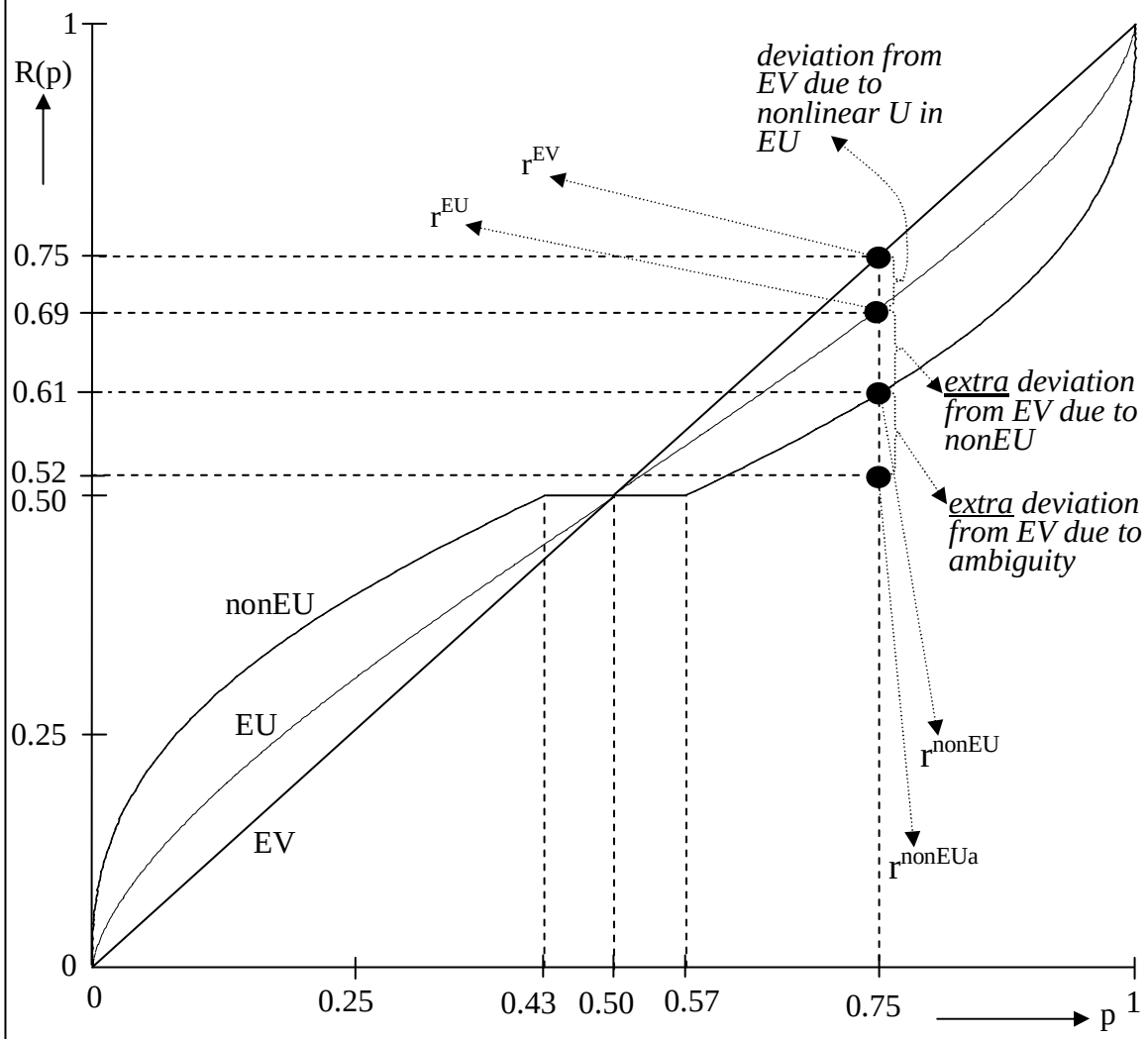
Figure 4.1 illustrates the extent to which reported probabilities can deviate from subjective probabilities, due to violations of expected value. The cases presented in this section considered p = 0.75, but Figure 4.1 considers all probabilities p under probabilistic sophistication. In the general model there are only events and no probabilities, so that the latter cannot be put on the x-axis and no graph can be drawn. For expected utility, a similar figure is in Winkler & Murphy (1970, Figure 3). Its pattern was confirmed empirically by Huck & Weizsäcker (2002). The figure illustrates the errors generated by the assumption of expected value maximization, still generally used in applications of proper scoring rules

[4] This holds also if people can choose the three colours to bet on in the ambiguous urn, so that there is no reason to suspect unfavourable compositions.

[5] It is easiest to see in Eq. 3.1 that $1/r_E$ is decreasing in W(E).

today, according to the modern views in decision theory. Johnstone (2007a, p. 164) gave similar results based on a mean-variance analysis.

FIGURE 4.1. Reported probability R(p) as a function of probability p



EV: expected value;
EU: expected utility with $U(x) = x^{0.5}$;
nonEU: nonexpected utility for known probabilities, with $U(x) = x^{0.5}$ and with w(p) as in Case 3.
r^{nonEUa}: nonexpected utility for unknown probabilities ("Ambiguity").

5. Revealed Preference Techniques to Elicit Subjective Beliefs from Proper Scoring Rules

In the preceding sections we assumed theoretical decision models and derived predictions about reported probabilities in proper scoring rules. This section presents the usual revealed-preference technique. That is, we assume that we observe reported probabilities, and we then investigate what we can infer about decision models and their parameters. In particular, we will be interested in inferring subjective probabilities and their generalizations from proper scoring rules.

If we could observe enough general decisions under risk beyond proper scoring rules then we could in principle reveal the whole function w. Similarly, we could reveal the whole function W if we could observe enough decisions under uncertainty. Then we could obtain the following concept, which will be central in this paper.

$$B(E) = w^{-1}W(E) . \qquad (5.1)$$

In general, B assigns value 0 to the vacuous event $\varnothing$, value 1 to the universal event, and B is increasing in the sense that $C \supset D$ implies $B(C) \geq B(D)$. These properties similarly hold for the composition $W(\cdot) = w(B(\cdot))$, as we saw above. Under probabilistic sophistication (including expected utility and expected value), B(E) agrees with the probability P(E). In all cases in §4 up to Case 3, $B(E) = 0.75 = P(E)$ indeed. Thus, B(E) is a better candidate for measuring subjective beliefs than r_E, the value still commonly used in applications of proper scoring rules today. Whenever subjective probabilities exist, B measures them correctly, irrespective of what the risk attitude is. B has corrected r_E for nonneutral risk attitudes. We call B the (*risk-*)*corrected reported probability*.

Case 4 showed that sometimes decisions cannot be modelled through subjective probabilities. In particular, B in Eq. 3.3 then will not be a probability measure. Yet we think it is a better candidate to reflect subjective beliefs of likelihood than the uncorrected reported probabilities. Risk attitude is a behavioural component rather than a component reflecting beliefs and it should be filtered out from belief assessments. Many studies of direct judgments of belief have supported the thesis that subjective beliefs cannot be modelled through probabilities (McClelland & Bolger 1994; Shafer 1976; Tversky & Koehler 1994), so that B will violate additivity. Bounded rationality is an extra reason to expect that subjective beliefs will violate the laws of probability (Aragones et al. 2005; Charness & Levin 2005).

EXAMPLE 5.1. Consider Case 4. The belief component $B(E_a)$ is estimated to be $w^{-1}(W(E_a)) = w^{-1}(0.52) = 0.62$. This value implies that B must violate additivity. Under additivity, we would have $B(C_a) = 1 - B(E_a) = 0.38$ and then, by symmetry, $B(G_a) = B(S_a) = B(Y_a) = 0.38$, so that $B(G_a) + B(S_a) + B(Y_a) = 3 \times 0.38 = 1.14$. Under additivity, this value should equal $B\{G_a,S_a,Y_a\} = B(E_a) = 0.62$ but it does not. Additivity is violated and B is no probability measure.

Of the total deviation of $r_{E_a} = 0.52$ from 0.75, being 0.23, a part of 0.06 + 0.08 = 0.14 is the result of deviations from risk neutrality that distorted the measurement of $B(E_a)$. The remaining 0.09 is not a distortion in the measurement of belief. It rather shows that belief is genuinely nonadditive. □

The measurement of B through entire measurements of w and W is laborious, in particular because of interactions with utility (Tversky & Kahneman 1992, p. 311; Abdellaoui, Vossman, & Weber 2005). The following results prepare for a tractable measurement of B. Whereas the expression of r in terms of W in Theorem 3.1 was implicit, we now present an explicit expression of its inverse, i.e. of W in terms of r. For easy later reference we state the result for $B = w^{-1}(W)$ instead of W.

COROLLARY 5.2. For the optimal choice $r = r_E$:

$$\text{If } r > 0.5, \text{ then } B(E) = w^{-1}\left(\frac{r}{r + (1-r)\frac{U'(1-(1-r)^2)}{U'(1-r^2)}}\right). \qquad (5.2)$$

□

We next display the special case of W = w(P) with P objective, in which case $B = P = R^{-1}(r)$.

COROLLARY 5.3. Under probabilistic sophistication, we have for the optimal choice r = R(p):

$$\text{If } r > 0.5, \text{ then } p = w^{-1}\left(\frac{r}{r + (1-r)\frac{U'(1-(1-r)^2)}{U'(1-r^2)}}\right). \tag{5.3}$$

□

As it so happens, the right-hand sides in Eqs. 5.2 and 5.3 are identical. This allows a particularly convenient way to measure B.

COROLLARY 5.4. Assume the general model. For an event E with $r_E = r$ we can find the objective probability p with the same value R(p) = r, and then we can conclude that B(E) = p. That is,

$$\text{If } r_E = r = > 0.5, \text{ then } B(E) = R^{-1}(r). \tag{5.4}$$

□

Two points underlie Corollary 5.4. First, proper scoring rules uniquely identify the underlying subjective factors for the r's specified. Second, the optimal solution of the problem for objective probabilities and the one for event E generate equally preferred prospects. The corollary is useful for empirical applications because all terms involved are easily observable. The corollary is the only implication of our theoretical analysis that is needed for applications.

In practice, we first infer the (for the participant) optimal R(p) for a set of objective probabilities p that is so dense that we obtain a sufficiently accurate estimation of R and R^{-1}. We will consider all values p = j/20 for $j \geq 10$ in our experiment. Then, for all uncertain events E (or E^c if r < 0.5) we derive B(E) from the observed r_E through Eq. 5.4. For $r_E = 0.5$, B(E) and the inverse p may not be uniquely determined because of the flat part of R_{nonEU} in Figure 4.1. The case r < 0.5 follows from Eqs. 5.4 and 2.2, as always. We call the function R^{-1} the *risk-correction* (for proper scoring rules). $R^{-1}(r_E) = B(E)$ is the corrected reported probability.

6. An Illustration of Our Measurement of Belief

This section describes risk corrections for a participant in the experiment so as to illustrate how our method can be applied empirically. It will show that Corollary 5.4 is the only result of the theoretical analysis needed to apply our method. Results and curves for $r < 0.5$ are derived from $r > 0.5$ using Eq. 2.2; we will not mention this point explicitly in what follows.

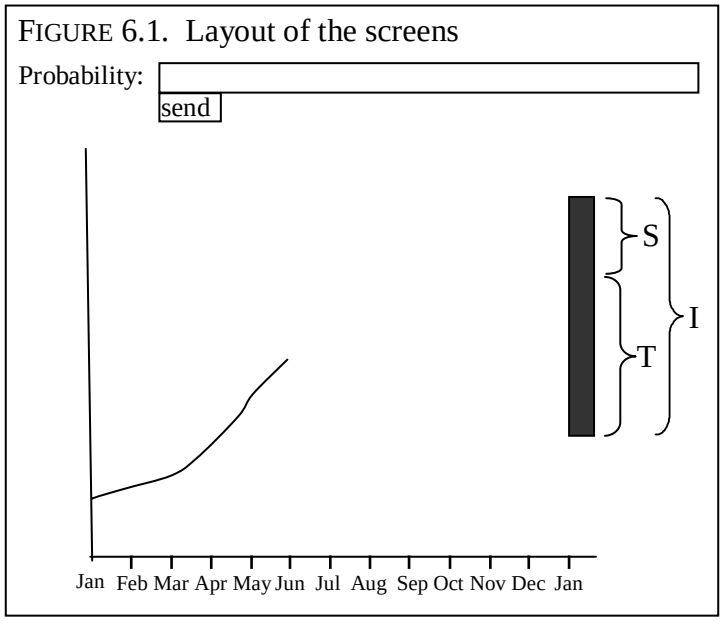


FIGURE 6.1. Layout of the screens

The left side of Figure 6.1 displays the performance of stock 12 (the Royal Begemann Group) in our experiment from January 1, until June 1, 1991, as given to the participants. Further details (such as the absence of a unit on the y-axis) will be explained in §7. The right side of the figure displays two disjoint intervals S and T, and their union $I = S \cup T$. For each of the intervals S,T, and I, participants reported the probability of the stock ending up in that interval on January 1, 1992 (with some other questions in between these three questions). For participant 14, the results are as follows.

$$r_S = 0.35;\ r_T = 0.55;\ r_I = 0.65. \tag{6.1}$$

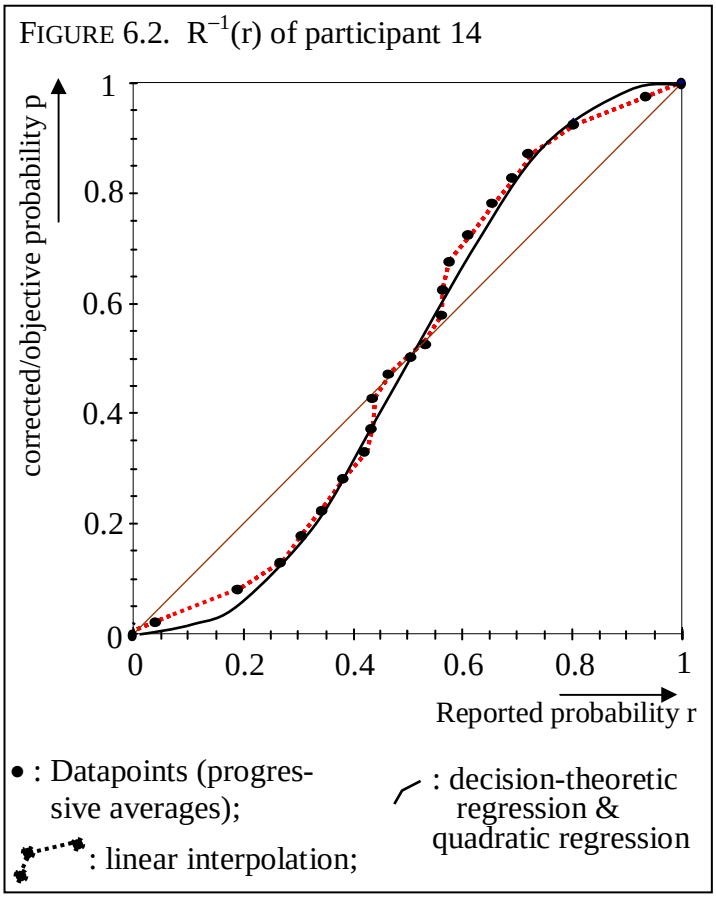
Under additivity of reported probability, $r_S + r_T - r_I$ (the *additivity bias*, defined in general in Eq. 7.5), should be 0, but here it is not and additivity is violated.

$$\text{The additivity bias is } 0.35 + 0.55 - 0.65 = 0.25. \tag{6.2}$$

Table 6.1 and Figure 6.2 (in inverted form) display the reported probabilities R(p) that we measured from this participant, with the curves explained later. We use progressive averages (midpoints between data points) so as to reduce noise.[6]

TABLE 6.1. Progressive average reported probabilities R(p) of participant 14

P	.025	.075	.125	.175	.225	.275	.325	.375	.425	.475	.525	.575	.625	.675	.725	.775	.825	.875	.925	.975
R(p)	.067	.192	.267	.305	.345	.382	.422	.435	.437	.470	.530	.563	.565	.578	.618	.655	.695	.733	.808	.933



For simplicity of presentation, we analyze the data here using linear interpolation. Then R(0.23) = 0.35.[7] Using this value for R(0.23), using the values R(0.56) = 0.55, and R(0.77) = 0.65, and, finally, using Eq. 5.4, we obtain the following corrected reported probabilities.

[6] For each midpoint between two given probabilities p, we calculated the average report for the adjacent probabilities. For instance, to compute the R(p) for p = 0.625, we averaged the reported probabilities for p = 0.6 and those for p = 0.65.

457 $B(S) = R^{-1}(0.35) = 0.23$; $B(T) = R^{-1}(0.55) = 0.56$; $B(I) = R^{-1}(0.65) = 0.77$;
458 the additivity bias is $0.23 + 0.56 - 0.77 = 0.02$. (6.3)

459 The risk-correction has reduced the violation of additivity, which according to Bayesian
460 principles can be interpreted as a desirable move towards rationality. In the experiment
461 described in the following sections we will see that this effect is statistically significant for
462 single evaluations (treatment "t=ONE"), but is not significant for repeated payments and
463 decisions (treatment "t=ALL").

464 It is statistically preferable to fit data with smoother curves than resulting from linear
465 interpolation. We derived "decision-theoretic" parametric curves for R(p) from Corollary
466 5.3, with further assumptions explained at the end of §8.1.[8] The resulting curve for
467 participant 14 is given in the figure. The equality $B = R^{-1}(r)$ and this curve lead to

468 $B(S) = R^{-1}(0.35) = 0.24$; $B(T) = R^{-1}(0.55) = 0.59$; $B(I) = R^{-1}(0.65) = 0.76$; the additivity
469 bias is $0.24 + 0.59 - 0.76 = 0.07$, (6.4)

470 again reducing the uncorrected additivity bias. For this participant the quadratic curve,
471 explained in §12, happens to be indistinguishable from the decision theoretic curve.
472

473 # 7. An Experimental Application of Risk Corrections: Method

474 The following five sections present the third part of this paper, being an experimental
475 implementation of our new measurement method. We first describe the two main treatments
476 in detail. §10 presents a third, control, treatment.
477

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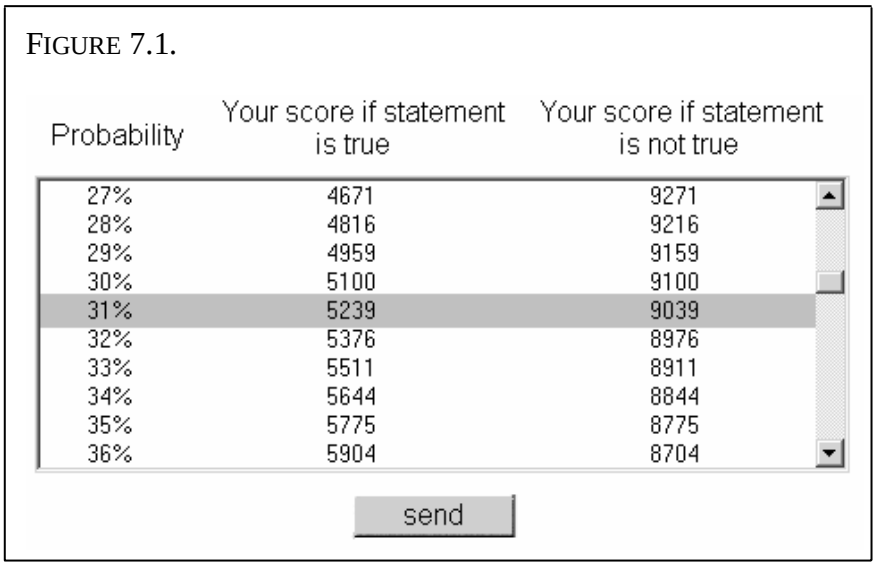
[7] We have $0.23 = 0.865\times0.225 + 0.135\times0.275$, $R(0.225) = 0.345$, and $R(0.275) = 0.382$, so that $R(0.23) = R\big(0.865\times0.225 + 0.135\times0.275\big) = 0.865\times R(0.225) + 0.135\times R(0.275) = 0.865\times0.345 + 0.135\times0.382 = 0.35$.

[8] The decision-theoretic curve in the figure is the function $p = B(E) = \dfrac{r}{r + (1-r)\dfrac{0.26\big(1-(1-r)^2\big)^{-1.26}}{0.26(1-r^2)^{-1.26}}}$, in agreement with Corollaries 5.3 and 5.4, where we estimated $w(p) = p$ and found $\rho = -0.26$ as optimal value for $U(x)$ in Eq. 7.1.

Participants. For the first two treatments, N = 93 students from a wide range of disciplines (45 economics; 13 psychology, 35 other disciplines) participated in the experiment. They were self-selected from a mailing list of approximately 1100 people.

Procedure. Participants were seated in front of personal computers in 6 groups of approximately 16 participants each. They first received an explanation of the QSR, given in Appendix D. Then, for each uncertain event, participants could first report a probability (in percentages) by typing in an integer from 0 to 100. Subsequently, the confirmation screen displayed a list box with probabilities and the corresponding score when the event was (not) true, illustrated in Figure 7.1.

FIGURE 7.1.



All figures (including Figure 6.1) are reproduced here in black and white; in the experiment we used colours to further clarify the figures. The entered probability and the corresponding score were preselected in this list box. The participant could confirm the decision or change to another probability by using the up or down arrow or by scrolling to another probability using the mouse. The event itself was also visible on the confirmation screen. Thus, the reported probability r finally resulted for the uncertain event.

Stimuli. The participants provided 100 reported probabilities r for events with unknown probabilities in the *stock-price part* of the experiment. For these events, we fixed June 1, 1991, as the "evaluation date." The uncertain events always concerned the question whether or not the price of a stock would lie in a target-interval seven months after the evaluation date. For each stock, the participants received a graph depicting the price of the stock on 0, 1,

2, 3, 4, and 5 months prior to the evaluation date, as well as an upper and lower bound to the price of the stock on the evaluation date. Figure 6.1, without the braces and letters, gives an example of the layout. We used 32 different stocks, all real-world stock market data from the 1991 Amsterdam Stock Exchange. After 4 practice questions, the graph of each stock-price was displayed once in the questions 5-36, once in the questions 37-68, and once in the questions 69-100. We, thus, obtained three probabilistic judgments of the performance of each stock, once for a large target-interval and twice for the small target-intervals that partitioned the large target-interval (see Figure 6.1). We partially randomized the order of presentation of the elicitations. Each stock was presented at the same place in the first, second, and third 32-tuple of elicitations, so as to ensure that questions pertaining to the same stock were always far apart. The order of presentation of the one large and the two small intervals for each stock was not randomized stochastically, but was varied systematically, so that all orders of big and small intervals occurred equally often. We also maximized the variation of whether small intervals were both very small, both moderately small, or one very small and one moderately small.

In the *calibration part* of the experiment, participants essentially made the same decisions as in the stock-price part, but now for 20 events with objective probabilities. Thus, participants simply made choices between risky prospects with objective probabilities. We used two 10-sided dice to determine the outcome of the different prospects and obtained measurements of the reported probabilities corresponding to the objective probabilities 0.05, 0.10, 0.15, …, 0.85, 0.90, and 0.95 (we measured the objective probability 0.95 twice). The event with probability 0.25 was, for instance, described as "The outcome of the roll with two 10-sided dice is in the range 01–25." The decision screen was very similar to Figure 7.1, except for the fact that we wrote "row-percentage" instead of "probability" and "your score if the roll of the die is 01-25" instead of "your score if statement is true;" and so on.

Motivating participants. Depending on whether the uncertain event obtained or not and on the reported probability for the uncertain event, a number of points was determined for each question through the QSR (Eq. 2.1), using 10000 points as unit of payment so as to have integer scores with four digits of precision. Thus, the maximum score for one question was 10000, the minimum score was 0, and the certain score resulting from reported probability 0.5 was 7500 points.

In treatment t=ALL, the sum of all points for all questions was calculated for each participant and converted to money through an exchange rate of 60000 points = €1, yielding an average payment of €15.05 per participant. For the calibration part we then used a box with twenty separate compartments containing pairs of 10-sided dice to determine the outcome of each of the twenty prospects at the same time for the treatment t=ALL.

In treatment t=ONE, the random incentive system was used. That is, at the end of the experiment, 1 out of the 120 questions that they answered was selected at random for each participant and the points obtained for this question were converted to money through an exchange rate of 500 points = €1, yielding an average payment of €15.30 per participant.

All payments were done privately at the end of the experiment.

Analysis. For the calibration part we only need to analyze probabilities of 0.5 or higher, by Eq. 2.3 (see also Observation A.2). Every observation for $p < 0.5$ amounts to an observation for $p' = 1-p > 0.5$. It implies that we have two observations for all $p > 0.5$ (and three for $p = 0.95$).

We first analyze the data at the group level, assuming homogeneous participants. We start from general probabilistic sophistication. Notice that this model can be estimated using a non-parametric procedure. If the agent is willing to go through a large series of correction questions, it is possible to measure the corresponding reported probability of each objective probability repeatedly. In this way an accurate estimate of the whole correction curve can be obtained without making assumptions about the utility function or the weighting function. This procedure is appropriate if the goal is to correct an expert, e.g., correct the reports provided by a weatherman. In applications of experimental economics where subjects participate for a limited amount of time, the researcher will only be able to collect a limited number of observations of the correction curve. Then it is more appropriate to follow a parametric approach to elicit the curve that fits the observations best. In this paper, we used parametric fittings. For U we used the *power utility with parameter* ρ, also known as the family of constant relative risk aversion (CRRA)[9], and the most popular parametric family for fitting utility, which is defined as follows:

[9] We avoid the latter term because in nonexpected utility models as relevant for this paper, risk aversion depends not only on curvature of utility.

For $\rho>0$: $U(x) = x^{\rho}$;

for $\rho=0$: $U(x) = ln(x)$;

for $\rho<0$: $U(x) = -x^{\rho}$. (7.1)

It is well-known that the unit of payment is immaterial for this family. The most general family that we consider for w(p) is Prelec's (1998) two-parameter family

$$w(p) = \left(exp(-\beta(-ln(p))^{\alpha})\right), \quad (7.2)$$

chosen for its analytic tractability and good empirical performance. We will mostly use the one-parameter subfamily with $\beta=1$, as in Eq. 4.1, for reasons explained later. Substituting the above functions yields

$$B(E) = \exp\left(-\left(\frac{-ln\left(\frac{r(2r-r^2)^{1-\rho}}{(1-r)(1-r^2)^{1-\rho} + r(2r-r^2)^{1-\rho}}\right)}{\beta}\right)^{1/\alpha}\right).$$

for Eq. 5.2.

The model we estimate for each subject separately is as follows.

$$R_k(j/20) = h(j/20,\alpha,\rho) + \varepsilon_k(j/20). \quad (7.3)$$

Here $R_k(j/20)$ is the reported probability of the participant for known probability $p=j/20$ ($10 \le j \le 19$) in treatment t (t = ALL or t = ONE) for the k^{th} measurement for this probability, with only k=1 for j = 10, k = 1,2 for $11 \le k \le 18$, and k = 1,2,3 for j = 19. With β set equal to 1, α is the remaining probability-weighting parameter (Eq. 7.2), and ρ is the power of utility (Eq. 7.1). The function h is the inverse of Eq. 5.3. Although we have no analytic expression for this inverse, we could calculate it numerically in the analyses. The error terms $\varepsilon_k(j/20)$ are drawn from a truncated normal distribution with mean 0 and variance σ^2. The distribution of the error terms is truncated because reported probabilities below 0 and above 1 are excluded by design. Error terms are identically and independently distributed across choices. We employed maximum likelihood to estimate the parameters of Eq. 7.3.

We also carried out an analysis at the aggregate level of the calibration part, with α_t and ρ_t, i.e. with these parameters depending on the treatment but not on the participant. To correct for individual differences, we added an individual-specific constant $c_{s,t}$ to the equation where s refers to the participant and t to the treatment:

$$R_{s,t,k}(j/20) = h(j/20,\alpha_t, \rho_t) + c_{s,t} + \varepsilon_{s,t,k}(j/20,\sigma_t^2)\,. \qquad (7.4)$$

Here the error terms are independent across subjects, treatments, and choices.

In the stock-price part, violations of additivity were tested. With I the large interval of a stock, being the union S∪T of the two small intervals S and T, additivity of the uncorrected reported probabilities implies

$$r_S + r_T = r_I. \qquad (7.5)$$

Hence, $r_S + r_T - r_I$ is an index of deviation from additivity, which we call the *additivity bias* of r.

Under the null hypothesis of additivity for corrected reported probabilities B, binary additivity holds, and we can obtain $B(S) = 1 - B(S^c)$ for small intervals S in the experiment (cf. Eq. 2.2). Thus, under additivity of B, we have

$$B(S) + B(T) = B(I). \qquad (7.6)$$

Hence, $B(S) + B(T) - B(I)$ is an index of deviation from additivity of B, and is B's *additivity bias*.

We next discuss tests of the additivity bias. For each individual stock, and also for the average over all stocks, we tested for both treatments t=ONE and t=ALL: (a) whether the additivity bias was zero or not, both with and without risk correction; (b) whether the average additivity bias, as relevant for aggregated group behaviour and expert opinions, was enlarged or reduced by correction; (c) whether the absolute value of the additivity bias, as relevant for additivity at the individual level, was enlarged or reduced by correction. We report only the tests for averages over all stocks.

8. Results of the Calibration Part

Risk-corrections and, in general, QSR measurements, do not make sense for participants who are hardly responsive to probabilities, so that R(p) is almost flat on its entire domain. Hence we kept only those participants for whom the correlation between reported probability and objective probability exceeded 0.35. We thus dropped 4 participants. The following analyses are based on the remaining 89 participants.

630 *8.1. Group Averages*

631

632 We did several tests using Eq. 7.2 with β as a free (treatment-dependent or -independent)
633 variable, but β's estimates added little extra explanatory power to the other parameters and
634 usually were close to 1. Hence, we chose to focus on a more parsimonious model in which
635 the restriction $\beta_{ONE} = \beta_{ALL} = 1$ is employed. Table 8.1 lists the estimates for the model of Eq.
636 7.4 for β=1 (Eq. 4.1 instead of Eq. 7.2) together with the estimates of some models with
637 additional restrictions. We first give results at the aggregate level. Because there turns out to
638 be a strong correlation between the α and ρ parameters, estimation results where both
639 parameters are estimated simultaneously cannot be trusted and we only report the results
640 where either α or ρ are estimated.

641

642 TABLE 8.1. Estimation results at the aggregate level

Row	Restrictions	σ_{ONE}	α_{ONE}	ρ_{ONE}	σ_{ALL}	α_{ALL}	ρ_{ALL}	–LogL
1	$\alpha_{ONE} = \alpha_{ALL} = \rho_{ONE} = \rho_{ALL} = 1$	9.00** (0.21)	–	–	8.36** (0.20)	–	–	6373.21
2	$\alpha_{ONE} = \alpha_{ALL} = 1$	8.73** (0.20)	–	0.43** (0.09)	8.36** (0.20)	–	0.94** (0.07)	6345.43
3	$\rho_{ONE} = \rho_{ALL} = 1$	8.82** (0.21)	0.69** (0.03)	–	8.35** (0.20)	1.09** (0.07)	–	6354.14
4	$\alpha_{ONE} = \alpha_{ALL} = \rho_{ONE} = 1$	9.00** (0.21)	–	–	8.36** (0.20)	–	0.94** (0.07)	6372.87
5	$\alpha_{ONE} = \alpha_{ALL} = \rho_{ALL} = 1$	8.73** (0.20)	–	0.43** (0.09)	8.36** (0.20)	–	–	6345.77
6	$\alpha_{ONE} = \alpha_{ALL} = 1$, $\rho_{ONE} = \rho_{ALL}$	8.78** (0.21)	–	0.70** (0.06)	8.41** (0.20)	–	–	6556.48

643 Standard errors in parentheses, ** (*) denotes significance at the 1% (5%) level.

644

645 *Overall need for risk-correction.* The 1st row of Table 8.1 shows the results without any
646 correction. The 2nd row presents the results when utility curvature is introduced. The
647 likelihood improves significantly (Likelihood Ratio test, p = 0.01) and substantially, so that
648 risk-correction is called for. Risk-correction can also be done by probability weighting. This
649 is done in the 3rd row of the Table. Probability weighting also increases the likelihood of
650 observing the data significantly compared to the model without correction, but less so than

utility curvature does. Therefore, in the remainder of the paper we focus on risk-correction obtained through utility curvature.

Comparing the two treatments. At the aggregate level, risk-correction is less needed in treatment ALL than in treatment ONE, as the 4th and 5th rows show. In treatment ONE the likelihood is improved significantly (compare the 5th and the 1st row, Likelihood Ratio test; $p = 0.01$) but in treatment ALL the likelihood is not improved significantly (compare the 4th and the 1st row, Likelihood Ratio test; $p > 0.10$). We obtain $\rho_{ONE} < \rho_{ALL}$: if only one decision is paid out, then participants exhibit more concave curvature of utility than when all decisions are paid out. Given the same degree of probability weighting, it implies more risk aversion for t=ONE than for t=ALL (and R closer to 0.5). The finding is supported by comparing the 6th row of Table 8.1, with the restriction $\rho_{ONE} = \rho_{ALL}$, to the 2nd row. This restriction significantly reduces the likelihood of observing the data (Likelihood Ratio test, $p = 0.01$).

FIGURE 8.1. Corrected versus uncorrected probability

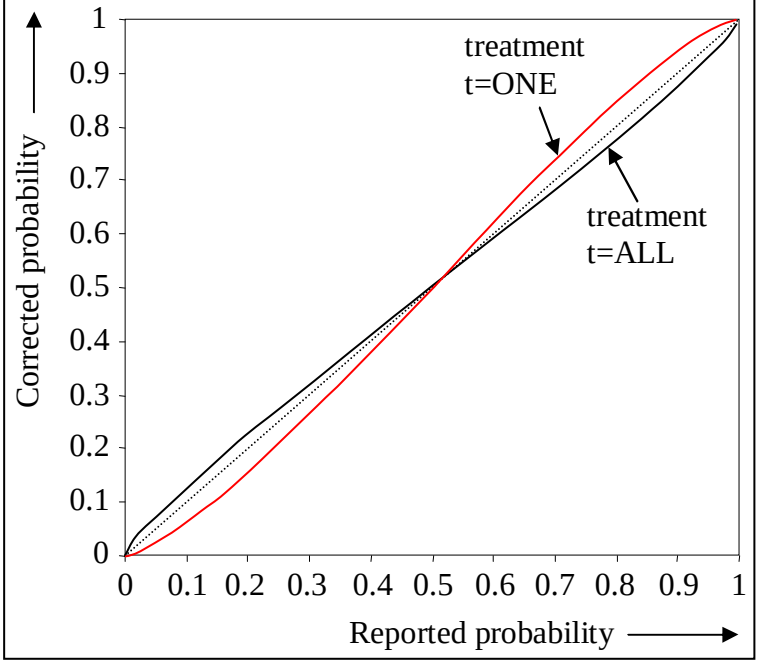
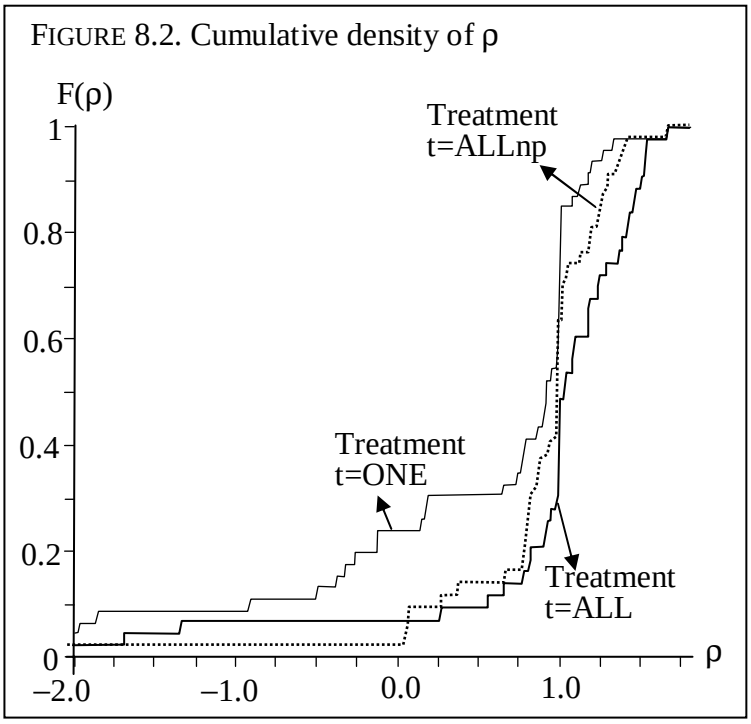


Figure 8.1, based on the estimates reported in the 2nd row of Table 8.1, displays the resulting average risk-correction for the two treatments separately. The figure illustrates that risk correction is clearly needed at the aggregate level in treatment ONE.

8.2. Individual Analyses

Need for risk-correction at the individual level. There is considerable heterogeneity in each treatment. Whereas the corrections required were small at the level of group averages, they are big at the individual level. This appears from Figure 8.2, which displays the cumulative distribution of the (per-subject) estimated ρ-coefficients for each treatment, assuming $\alpha = \beta = 1$. (The figure also displays a treatment t=ALLnp that will be explained in Section 10.) There are wide deviations from the value ρ=1 (i.e., no correction) on both sides. As seen from the group-average analysis, there are more deviations at the risk-averse side of $\rho < 1$.

FIGURE 8.2. Cumulative density of ρ

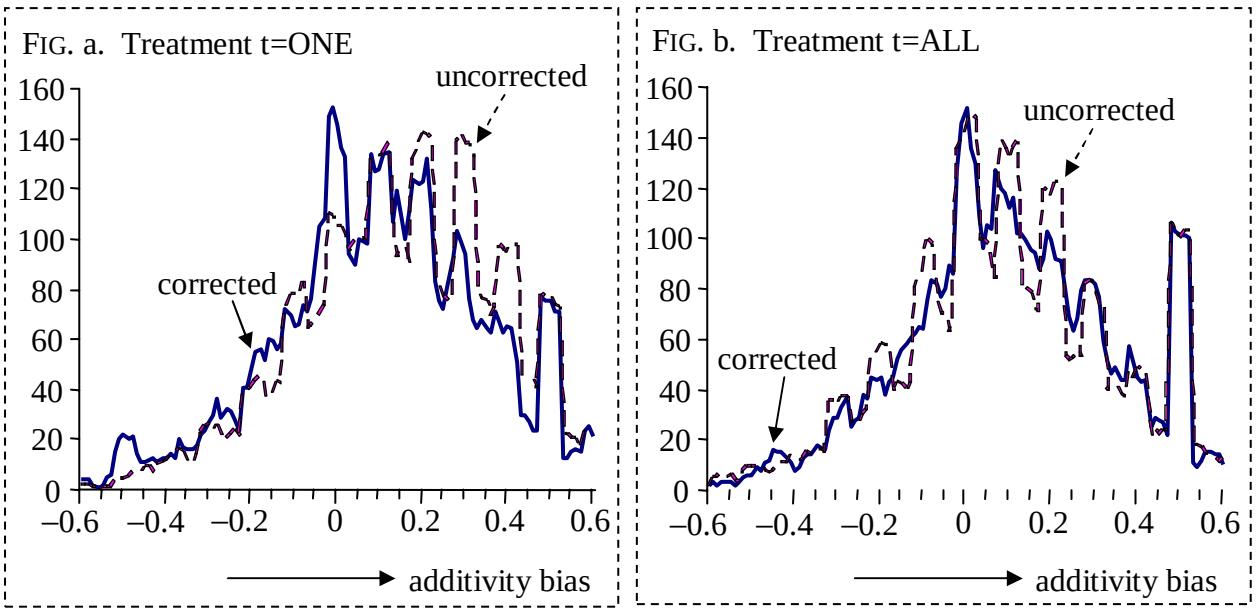


Comparing the two treatments. The ρ-coefficient distribution of treatment t=ONE dominates the ρ-coefficient distribution of treatment t=ALL. Thus, the ρ-coefficients for t=ONE are lower than for t=ALL (p=0.001; two-sided Mann-Whitney test). It confirms the result from Table 8.1 that there is more risk aversion for group averages, moving R in the direction of 0.5, for t=ONE than for t=ALL. The figure also shows that in an absolute sense there is more deviation from ρ=1 for t=ONE than for t=ALL, implying that there are more deviations from expected value and more risk corrections for t=ONE than for t=ALL.

Unlike the median ρ-coefficients that are fairly close to each other for the two treatments (0.92 for t=ONE versus 1.04 for t=ALL), the mean ρ-coefficients are substantially different (0.24 for t=ONE versus 0.91 for t=ALL), which is caused by skewedness to the left for t=ONE. That is, there is a relatively high number of strongly risk-averse participants for t=ONE. Analyses of the individual ρ parameters (two-sided Wilcoxon signed rank sum tests) confirm findings of group-average analyses in the sense that the ρ-coefficients are significantly smaller than 1 for t=ONE ($z = -3.50$, $p = 0.0005$), but not for t=ALL ($z = 1.42$, $p = 0.16$).

9. Results for the Stock-Price Part: Risk-Correction and Additivity

FIGURE 9.1. Empirical density of additivity bias for the two treatments



For each interval $[\frac{j-2.5}{100}, \frac{j+2.5}{100}]$ of length 0.05 around $\frac{j}{100}$, we counted the number of additivity biases in the interval, aggregated over 32 stocks and 89 individuals, for both treatments. With risk-correction, there were 65 additivity biases between 0.375 and 0.425 in the treatment t=ONE, and without risk-correction there were 95 such; etc.

All comparisons in this section are based on two-sided Wilcoxon signed rank sum tests. Figure 9.1 displays data, aggregated over both stocks and individuals, of the additivity biases for t=ONE and for t=ALL. The figures show that the additivity bias is more often positive than negative, in agreement with common findings in the literature (Tversky & Koehler

1994). Indeed, for virtually all stocks the additivity bias is significantly positive for both treatments, showing in particular that additivity does not hold. This also holds when taking the average additivity bias over all stocks as one data point per participant ($z = 5.27$, $p < 0.001$ for t=ONE, $z = 4.35$, $p < 0.001$ for t=ALL). We next consider whether risk corrections reduce the violations of additivity.

Let us first consider treatment t=ONE. Here the risk corrections reduce the average additivity bias significantly for 27 of the 32 stocks, and enlarge it for none. We only report the statistics for the average additivity bias over all stocks as one data point per participant, which has overall averages 0.163 (uncorrected) and 0.120 (corrected), with the latter significantly smaller ($z = 3.21$, $p = 0.001$). For assessing the degree of irrationality (additivity-violation) at the individual level, the absolute values of the additivity bias are relevant. For t=ONE, Figure 9.1 suggests that these are smaller after correction, because on average the corrected curve is closer to 0 on the x-axis. These absolute values were significantly reduced for 9 stocks and enlarged for none. Again, we only report the statistics for the average absolute value of the additivity bias over all stocks taken as one data point per participant, which has overall averages 0.239 (uncorrected) and 0.228 (corrected), with the latter significantly smaller ($z = 2.26$, $p = 0.02$).

For t=ALL, risk corrections did not significantly alter the average additivity bias. More specifically, it gave a significant increase for 3 stocks and a significant decrease for 1 stock, which, for 32 stocks, suggests no systematic effect. The latter was confirmed when we took the average additivity bias over all stocks for each individual, with no significant differences generated by correction (average 0.128 uncorrected and average 0.136 corrected; $z = -1.64$, $p = 0.1$). Similar results hold for absolute values of additivity biases, which gave a significant increase for 1 stock and a significant decrease for no stock. Taking the average additivity bias over all stocks as one data point per participant (average 0.237 uncorrected and average 0.239 corrected; $z = -0.36$, $p = 0.70$) also gave no significant difference.

Risk correction reduces the additivity bias for treatment t=ONE to a level similar to that observed for t=ALL (averages 0.120 and 0.136). The overall pattern is that beliefs for t=ONE after correction, and for t=ALL both before and after correction, exhibit a similar degree of violation of additivity, which is clearly different from zero. The additivity bias is not completely caused by nonlinear risk attitudes when participants report probabilities, but has a genuine basis in beliefs.

10. A Treatment without Explicit Reference to Beliefs or Probability

This section briefly reports the results of a robustness check of our experimental design. In agreement with the current practice of scoring rules, the instructions of our main treatments repeatedly used the terms probability and belief. These terms may have influenced the subjects. To assess such influences, we performed a control treatment in which we did not refer to probabilities or beliefs.[10] Thus, in Figure 7.1 we now used the expression "choose a number" instead of probability. In the instructions of this control treatment, we similarly asked subjects to choose numbers without calling them probabilities, and we dropped all interpretations of likelihood. In this manner we ran the control treatment for t=ALL. We chose t=ALL rather than t=ONE because the former is most commonly used in applications of proper scoring rules. We refer to the control treatment as t=ALLnp (np for no probabilities). N=44 students participated. The number of participants dropped from the analysis because their correlation between reported and objective probability was below 0.35 now was 2. In all other respects the new treatment was identical to the t=ALL treatment in the main experiment.

The results confirmed all patterns and inequalities found for t=ALL. We give some numerical details for individual analyses. The ρ's of t=ALLnp are not significantly different from those of t=ALL ($z = 1.57$, $p=0.12$), with a similar median (1.00 for t=ALLnp versus 1.04 for t=ALL) and mean (0.80 for t=ALLnp versus 0.91 for t=ALL). They, accordingly, are not significantly below 1 either ($z = 0.52$, $p = 0.60$), and they also exceed the ρ for t=ONE ($z = -2.30$, $p = 0.02$).

The additivity bias is, again, positive, showing that additivity is violated, for most individual stocks. It also is when taking the average additivity bias over all stocks per participant ($z = 4.47$, $p < 0.001$). Risk corrections did not significantly in- or decrease the average additivity bias for any stock. For the (absolute) average additivity bias over all stocks per participant, we again found no significant difference between the non-corrected and corrected average additivity bias ($z = 0.378$, $p = 0.71$; $z = 0.265$, $p = 0.79$ for absolute values). The risk-corrected average additivity bias for t=ALLnp being virtually the same as for t=ALL (0.126 versus 0.136) obviously implies that it is also equal to the one for t=ONE

[10] This treatment was recommended to us by a referee and the editor.

(0.120). In summary, all results for the t=ALL treatment were confirmed by the t=Allnp treatment, suggesting that the explicit use of the term probability in our instructions did not alter the results.

11. Discussion of Experiment

Methods. We chose the evaluation date (June 1, 1991) sufficiently long ago to ensure that participants would be unlikely to recognize the stocks or have private information about them. In addition, no numbers were displayed on the vertical axis, making it extra hard for participants to recognize specific stocks. We, thus, ensured that participants based their probability judgments entirely on the prior information about past performance of the stocks given by us. Given the large number of questions it is unlikely that participants noticed that the graphs were presented more than once (three times) for each stock. Indeed, in informal discussions after the experiment no participant showed awareness of this point.

In some studies in the literature, the properness of scoring rules is explained to participants by stating that it is in their best interest to state their true beliefs, either without further explanation, or with the claim added that they will thus maximize their "expected" money. A drawback of this explanation is that expected value maximization is empirically violated, which is the central topic of this paper (§3), so that the recommendation is debatable. We, therefore, used an alternative explanation that relates properness for one-off events to observed frequencies of repeated events (Appendix D).

Optimal Incentive Scheme. After some theoretical debates about the random incentive system (Holt 1986), as in our treatment t=ONE, the system was tested empirically and found to be incentive-compatible (Lee 2008; Starmer & Sugden 1991). It is today the almost exclusively used incentive system for measurements of individual preferences (Holt & Laury 2002; Harrison et al. 2002; Myagkov & Plott 1997). Unlike repeated payments it avoids income effects such as Thaler & Johnson's (1990) house money effect, and the drift towards expected value and linear utility that is commonly generated by repeated choice.[11] For the purpose of measuring individual preference, the treatment t=ONE is, therefore, preferable.

[11] It is required that the repeated choices are perceived as sufficiently uncorrelated. Correlation can enhance the perception of and aversion to ambiguity (Halevy & Feltkamp 2005).

When the purpose is, however, to derive subjective probabilities from proper scoring rules, and no risk-correction is possible, then a drift towards expected value is actually an advantage, because uncorrected proper scoring rules assume expected value. This point agrees with our findings, where less risk-correction was required for the t=ALL treatment. Li (2007) discussed other arguments for and against repeated rewarding when events are not verifiable and binary rewards have to be used.

For some applications group averages of probability estimates are most relevant, such as when aggregating expert judgments or predicting group behaviour. Then our statistical results regarding "non-absolute" values of reported probabilities are most relevant. For the assessment of rationality at the individual level, absolute values of the additivity biases are most relevant.

Choice of Parameters. The lack of extra explanatory power of parameter β in Eq. 7.2 should come as no surprise because β and α imply similar phenomena on [0.5,1], increasing risk aversion there. They mainly deviate from one another on [0,0.5], where β continues to enhance risk aversion but α enhances the inverse-S shape that is mostly found empirically. The domain [0,0.5] is, however, not relevant to our study (Observation A.2).

Pragmatic applications. More tractable families can be used to fit the reported probabilities than the decision-theory-based curves that we used. For example, in Figure 6.2 we also used quadratic regression to find the curve $p = a + br + cr^2$ that best fits the data. For most participants, the curve is virtually indistinguishable from the decision-theoretic curve. This observation, together with Corollary 5.4 which demonstrates that we only need the readily observable reported probabilities and not the actual utility function or probability weighting function to apply our method, shows that applications of our method are straightforward. The theoretical analysis of this paper, and the decision-theory based curve-fitting that we adopted, served to prove that our method is in agreement with modern decision theories. If this thesis is accepted, and the only goal is to obtain corrected reported probabilities, then one may choose the pragmatic shortcuts just described.

General Discussion. We emphasize that the biases due to violations of expected value that we correct for need not concern mistakes or irrationalities in decision making. Deviations from risk neutrality need not be irrational and, according to some, even deviations from

Bayesian beliefs need not be irrational, nor need the corresponding ambiguity attitudes be (Gilboa & Schmeidler 1989). The required corrections concern empirical deficiencies of the model of expected value, i.e. they concern biases on the part of the researchers analyzing the data.

Under proper scoring rules, beliefs are derived solely from decisions, and Eq. 2.1 is taken purely as a decision problem, where the only goal of the agent is to optimize the prospect received. We considered both treatments that explicitly referred to probabilities and beliefs, and a treatment that did not do so, finding no differences between the latter and the former treatments. Thus, this paper has analyzed proper scoring rules purely from the decision-theoretic perspective supported with real incentives, and has corrected only for biases resulting therefrom. Many studies have investigated direct judgments of belief without real incentives, and then many other aspects play a role, leading for instance to the often found overconfidence. Such introspective effects are beyond the scope of this paper.

An immediate advantage of our calibration measurement, prior to any theoretical analysis, is that it helps to identify subjects whose understanding of the concepts to be measured is below what is minimally acceptable. Indeed, subject for whom the correlation between objective and reported probabilities is very low clearly have little clue what likelihood means. Their reported probabilities are of so little interest that we recommend dropping them from the sample. If we are interested in the beliefs of such subjects in more elaborate studies, then further teaching and learning will be called for. In our experiments we, indeed, dropped the subjects with the lowest correlations between reported and objective probabilities.

The experimental data show that for a subset of the subjects a substantial correction of reported probabilities needs to be made. The fraction of the sample that needs substantial corrections is larger when only a single large-stake decision is paid than when repeated small decisions are paid. Our conclusion is that it is desirable to correct agents' reported probabilities elicited with scoring rules, especially if only a single large-stake decision is paid. If it is not possible to obtain individual measurements of the correction curve, then it will be useful to use best-guess corrections, for instance through averages obtained from individuals as similar as possible. Thus, at least, the systematic error for the group average to risk attitude has been corrected for as good as is possible without requiring extra measurements. In this respect the average curves in Figure 8.1 are reassuring for existing studies, because these curves suggest that only small corrections were needed for the group averages in our context.

Several methods have been used in the literature to measure the subjective degree of belief of an agent in an event E. Mostly these have been derived from: (a) binary preferences, which only give inequalities or approximations; (b) binary indifferences, which are hard to elicit, e.g. through the complex Becker-DeGroot-Marschak mechanism (Braga & Starmer 2005; Karni & Safra 1987) or bisection (Abdellaoui, Vossman, & Weber (2005); (c) introspection, which is not revealed preference based let alone incentive compatible. Proper scoring rules provide an efficient manner for measuring subjective beliefs while avoiding the problems mentioned.

12. Theoretical Discussion

A way to reveal B(E) from observed choice, alternative to our method, is by revealing the *matching probability* p of event E, defined through the equivalence

$$x_p y \sim x_E y \qquad (12.1)$$

for some preset x > y, say x = 100 and y = 0. Then $w(B(E))(U(x)-U(y)) = w(p)(U(x)-U(y))$, and B(E) = p follows. Wakker (2004) discussed the interpretation of Eqs. 5.1 and 12.1 as belief. Matching probabilities were commonly used in early decision analysis (Raiffa 1968, §5.3; Yates 1990 pp. 25-27) under the assumption of expected utility. A recent experimental measurement is in Holt (2006, Ch. 30), who also assumed expected utility. Abdellaoui, Vossman, & Weber (2005) measured and analyzed them in terms of prospect theory, as does our paper. A practical difficulty is that the measurement of matching probabilities requires the measurement of indifferences, and these are not easily inferred from choice. For example, Holt (2006) used the Becker-deGroot-Marschak mechanism, and Abdellaoui, Vossman, & Weber (2005) used a bisection method.

A second alternative way to correct reported probabilities is through calibration. Then many reported probabilities are collected over time and are related to observed relative frequencies. Calibration has been studied in game theory (Sandroni, Smorodinsky, & Vohra 2003), and has been applied to weather forecasters (Murphy & Winkler 1974). It needs extensive data, which is especially difficult to obtain for rare events such as earthquakes, and further assumptions such as stability of the characteristics of these events over time. Clemen & Lichtendahl (2005) discussed these drawbacks and proposed correction techniques for

probability estimates in the spirit of our paper, but still based these on traditional calibration techniques. Our correction ("calibration") technique is considerably more efficient than traditional ones. It shares with Prelec's (2004) method the advantage that we need not wait until the truth or untruth of uncertain events has been revealed for implementing it.

Allen (1987) proposed to avoid biases of the QSR due to nonlinear utility by paying in terms of the probability of winning a prize instead of in terms of money, and this procedure was implemented by McKelvey & Page (1990). The procedure, however, only works if expected utility holds, and there is much evidence against this assumption. Indeed, Selten, Sadrieh, & Abbink (1999) showed empirically that payment in probability generates more deviations from risk neutrality.

The decision-based distortion in the direction of 0.5 due to risk aversion in §4 is opposite to the overconfidence (probability judgments too far from 0.5) mostly found in direct judgments of probability without real incentives (McClelland & Bolger 1994), and found among experts seeking to distinguish themselves (Keren 1991, p. 224 and 252; the "expert bias", Clemen & Rolle 2001). Similar optimistic and pessimistic distortions of probability can result from nonlinear utility if the probability considered is a consensus probability for a group of individuals with heterogeneous beliefs (Jouini & Napp 2007).

The curve for nonEU in Figure 4.1 is flat around $p = 0.5$, more precisely, on the probability interval [0.43, 0.57]. For probabilities from this interval the risk aversion generated by nonexpected utility is so strong that the agent goes for maximal safety and chooses $r = 0.5$, corresponding with the sure outcome 0.75 (cf. Manski 2004, footnote 10; Segal & Spivak 1990). Such a degree of risk aversion is not possible under expected utility, where $r = 0.5$ can happen only for $p = 0.5$ (Observation 3.3). This observation cautions against assigning specific levels of belief to observations $r = 0.5$, because proper scoring rules may be insensitive to small changes in the neighbourhood of $p = 0.5$. It in fact means that there the scoring rules, traditionally called proper, are not really proper.

B captures the component of decision making beyond risk attitude. It is common in decision theory to interpret factors beyond risk attitude as ambiguity. Then B reflects ambiguity attitude. There is no consensus about the extent to which ambiguity reflects non-Bayesian beliefs, and to what extent it reflects non-Bayesian decision attitudes beyond belief. If the equality $B(E) + B(E^c) = 1$ (*binary additivity*) is violated, then it can further be debated whether $B(E)$ or $1 - B(E^c)$ is to be taken as an index of belief or of ambiguity. Such interpretations have not yet been settled, and further studies are called for. We have mostly

969 referred to B as reflecting beliefs, to stay as close as possible to the terminology used in the
970 literature on proper scoring rules today. Irrespective of the interpretation of B, it is clear that
971 the behavioural component of risk attitude should be filtered out before an interpretation of
972 belief can be considered. This paper shows how this filtering out can be done. In Schmeidler
973 (1989), the main paper initiating Eqs. 3.1 and 3.2, w was assumed linear, with expected
974 utility for given probabilities, and W coincided with B. Schmeidler interpreted this
975 component as reflecting beliefs. So did the first paper on nonadditive measures for decision
976 making, Shackle (1949).
977 As is common in the mechanism design literature, our correction procedure assumed
978 deterministic choice. A fundamental question is how the mechanism performs when agents
979 may make mistakes, as in the random utility model (Luce 1959; McFadden 1974, 1976).
980 Such mistakes will affect the optimal elicitation procedure. These issues are relevant to the
981 entire mechanism design literature and deserve high priority in future research.
982

983 # 13. Conclusion

984 This paper has applied modern theories of risk and ambiguity to proper scoring rules.
985 Mutual benefits have resulted for people using proper scoring rules and for people studying
986 risk and ambiguity. For the former we have shown which distortions affect their common
987 measurements and how large these distortions are, using theories that are descriptively better
988 than the expected value hypothesis still common in applications of proper scoring rules today.
989 We have provided a procedure to correct for the aforementioned distortions, and a theoretical
990 foundation has been given for interpretations of the resulting measurements as (possibly non-
991 Bayesian) beliefs and ambiguity attitudes. For studies of risk and ambiguity we have shown
992 how the remarkable efficiency of proper scoring rules can be used to measure and analyze
993 subjective beliefs and ambiguity attitudes in ways more tractable than is possible through the
994 binary preferences traditionally used.
995 The feasibility and tractability of our method have been demonstrated in an experiment,
996 where we used it to investigate some properties of beliefs and quadratic proper scoring rules.
997 We found, for instance, that our correction method reduces the violations of additivity in
998 subjective beliefs but does not eliminate them. It confirms that beliefs are genuinely non-
999 Bayesian and that ambiguity attitudes play a central role in proper scoring rules.

Appendix A. Technical Remarks

For qsr-prospects in Eq. 2.1, every choice $r < 0$ is inferior to $r = 0$, and $r > 1$ is inferior to $r = 1$. The optimization problem does not change if we allow all real r, instead of $0 \le r \le 1$. Hence, solutions $r = 0$ or $r = 1$ can be treated as interior solutions, and they satisfy the first-order optimality conditions.

In general, it may not be possible to derive both w and U from R(p) without further assumptions, i.e. U and w may be nonidentifiable for proper scoring rules. Under regular assumptions about U and w, however, they have some different implications. The main difference is that, if we assume that U is differentiable (as done throughout this paper) and concave, then a flat part of R(p) around 0.5 must be caused by w (Observation 4.2.3).

We next discuss in more detail dualities between B(E) and $1 - B(E^c)$. Event A is (*revealed*) *more likely than* event B if, for some positive outcome x, say x = 100, the agent prefers x_A0 to x_B0. This observation is independent of the outcome $x>0$. In view of the symmetry of QSRs in Observation 2.1, for $r \neq 0.5$ the agent will always allocate the highest payment to the most likely of E and E^c. It leads to the following restriction of QSRs.

OBSERVATION A.1. Under the QSR in Eq. 2.1, the highest outcome is always associated with the most likely event of E and E^c. □

Hence, QSRs do not give observations about most likely events when endowed with the worst outcome. Similar restrictions apply to all other proper scoring rules considered in the literature so far. It implies the following result.

OBSERVATION A.2. For the QSR, only the restriction of w to [0.5,1] plays a role, and w's behavior on [0,0.5) is irrelevant. □

For our risk-corrections, we need w only on [0.5,1]. An advantage is that the empirical findings about w are uncontroversial on this domain, the general finding being that w underweights probabilities there. This holds both for the mostly found inverse-S shape (Abdellaoui 2000; Bleichrodt & Pinto 2000; Gonzalez & Wu 1999; Tversky & Kahneman

1992), and for the also often found convex shapes (Goeree, Holt, & Palfrey 2002; van de Kuilen, Wakker, & Zou 2008).[12]

Some details on weak inequalities and corner solutions are as follows. A choice of r = 0.5 may be driven by risk aversion, so that no likelihood ordering between E and E^c can be concluded then. A choice of $r \neq 0.5$ (if close to 0.5) may be driven by risk seeking with equal likelihood of E and E^c. Only interior solutions with a strict inequality $r > 0.5$ combined with E being strictly less likely than E^c are excluded for QSRs.

As with the weighting function w under risk, B is also applied only to the most likely one of E and E^c in the above equations, reflecting again the restriction of the QSR of Observation A.1. Hence, under traditional QSR measurements we cannot test binary additivity directly because we measure B(E) only when E is more likely than E^c. These problems can easily be amended by modifications of the QSR. For instance, we can consider prospects

$$(2-(1-r)^2)_E(1-r^2), \tag{A.1}$$

i.e. qsr-prospects as in Eq. 2.1 but with a unit payment added under event E. The classical proper-scoring-rule properties of §2 are not affected by this modification, and the results of §3 are easily adapted. With this modification, we have the liberty to combine event E with the highest outcome both if E is more likely than E^c and if E is less likely, and we avoid the restriction of Observation A.1. We then can observe w of the preceding subsection, and W(E) and B(E) over their entire domain. Similarly, with prospects

$$(1-(1-r)^2)_E(2-r^2), \tag{A.2}$$

we can measure the duals $1 - W(E^c)$, $1 - w(1-p)$, and $1 - B(E^c)$ over their entire domain. In this study we confine our attention to the QSRs of Eq. 2.1 as they are classically applied throughout the literature. We reveal their biases according to the current state of the art of decision theory, suggest remedies whenever possible, and signal the problems that remain. Further investigations of the, we think promising, modifications of QSRs in the above equations are left to future studies.

The restrictions of the classical QSRs also hold for the experiment in this paper. There an application of the QSR to events less likely than their complements are to be interpreted

[12] On [0,0.5) the patterns is less clear, with both underweighting and overweighting (Abdellaoui 2000, Bleichrodt & Pinto 2000, Gonzalez & Wu 1999).

formally as the measurement of $1 - B(I^c)$. The restrictions also explain why the theorems concerned only the case of $r > 0.5$ (with $r = 0.5$ as a boundary solution).

Appendix B. Proofs

For qsr-prospects in Eq. 2.1, every choice $r < 0$ is inferior to $r = 0$, and $r > 1$ is inferior to $r = 1$. The optimization problem does not change if we allow all real r, instead of $0 \le r \le 1$. Hence, solutions $r = 0$ or $r = 1$ can be treated as interior solutions, and they satisfy the first-order optimality conditions.

PROOF OF OBSERVATION 3.3. If $r = 0.5$ then the marginal utility ratio in Eq. 3.3 is 1, and $p = 0.5$ follows. For the reversed implication, assume risk aversion. Then $r > 0.5$ is not possible for $p = 0.5$ because then the marginal utility ratio in Eq. 3.3 would be at least 1 so that the right-hand side of Eq. 3.3 would at most be 0.5, contradiction $r > 0.5$. Applying this finding to E^c and using Eq. 2.2, $r < 0.5$ is not possible either, and $r = 0.5$ follows.

Under strong risk seeking, r may differ from 0.5 for $p = 0.5$. For example, if $U(x) = e^{2.5x}$, then $r = 0.14$ and $r = 0.86$ are optimal, and $r = 0.5$ is a local infimum, as calculations can show. The same optimal values of r result under nonexpected utility with linear U, and with $w(0.5) = 0.86$. Such large w-values also generate risk seeking.

PROOF OF THEOREM 3.1. We write π for the decision weight W(E). For optimality of interior solutions r, the first-order optimality condition for Eq. 3.1 is that

$$\pi U'(a-b(1-r)^2)2b(1-r) - (1-\pi)U'(a-br^2)2br = 0,$$

implying

$$\pi(1-r)U'(a-b(1-r)^2) = (1-\pi)rU'(a-br^2) \qquad (B.1)$$

or $\pi U'(a-b(1-r)^2) = r \times (\pi U'(a-b(1-r)^2) + (1-\pi)U'(a-br^2))$, and Eq. 3.3 follows.

□

PROOF OF COROLLARY 5.2. Let $r > 0.5$ be optimal, and write $\pi = W(E)$. Then Eq. B.1 implies $\pi \times ((1-r)U'(a-b(1-r)^2) + rU'(a-br^2)) = rU'(a-br^2)$, implying

$$\pi = \frac{r}{r + (1-r)\dfrac{U'(a-b(1-r)^2)}{U'(a-br^2)}} \qquad \text{(B.2)}$$

Applying w^{-1} to both sides yields the theorem. □

In measurements of belief one first observes r, and then derives B(E) from it. Corollary 5.2 gave an explicit expression. In general, it does not seem to be possible to write r as an explicit expression of B(E) or, in the case of objective probabilities with B(E) = p, of the probability p.

PROOF OF COROLLARY 5.4. Theorem 3.1 implies that the right-hand side of Eq. 3.3 is r both as is, and with p substituted for B(E). Because Eq. 3.3 is strictly increasing in w(B(E)), and w is strictly increasing too, p = B(E) follows. □

Appendix C. Models for Decision under Risk and Uncertainty

For binary (two-outcome) prospects with both outcomes nonnegative, as considered in QSRs, Eqs. 3.1 and 3.2 have appeared many times in the literature. Early references include Allais (1953, Eq. 19.1) and Edwards (1954, Figure 3). The convenient feature that binary prospects suffice to identify utility U and the nonadditive $w \circ B = W$ was pointed out by Ghirardato & Marinacci (2001), Gonzalez & Wu (2003), Luce (1991, 2000), Miyamoto (1988), and Wakker & Deneffe (1996, p. 1143 and pp.1144-1145).

The convenient feature that most decision theories agree on the evaluation of binary prospects was pointed out by Miyamoto (1988), calling Eqs. 3.1 and 3.2 generic utility, and Luce (1991), calling these equations binary rank-dependent utility. It was most clearly analyzed by Ghirardato & Marinacci (2001), who called the equations the biseparable model. These three works also axiomatized the model. The agreement for binary prospects was also central in many works by Luce (e.g., Luce, 2000, Ch. 3) and in Gonzalez & Wu (2003). Only for more than two outcomes, the theories diverge (Mosteller & Nogee 1951 p. 398; Luce 2000, introductions to Chs. 3 and 5). Theories that also deviate for two outcomes include betweenness models (Chew & Tan 2005), the variational model (Maccheroni, Marinacci, & Rustichini 2006), and models with underlying multistage decompositions (Halevy &

Feltkamp 2005; Halevy & Ozdenoren 2007; Klibanoff, Marinacchi, & Mukerji 2005; Nau 2006; Olszewski 2007).

We next describe some of the agreeing decision theories. Because we consider only nonnegative outcomes, losses play no role, and we describe prospect theory only for gains.

We begin with decision under risk, with known objective probabilities P(E). Expected utility (von Neumann & Morgenstern 1944) is the special case where w is the identity and B(E) = P(E). Kahneman & Tversky's (1979) original prospect theory, Quiggin's (1982) rank-dependent utility, and Tversky & Kahneman's (1992) new prospect theory concern the special case of B(E) = P(E), where w now can be nonlinear. The case B(E) = P(E) also includes Gul's (1991) disappointment aversion theory.

We next consider the more general case where no objective probabilities need to be given for all events E. Expected utility is the special case where B is an additive, now "subjective," probability and w is the identity. Choquet expected utility (Schmeidler 1989) and cumulative prospect theory (Tversky & Kahneman 1992) start from the general weighting function W, from which B obviously results as $w^{-1}(W)$, with w the probability weighting function for risk. The multiple priors model (Gilboa & Schmeidler 1989) results with W(E) the infimum value P(E) over all priors P. Under Machina & Schmeidler's (1992) probabilistic sophistication, B is an additive probability measure.

Appendix D. Experimental Instructions

Instructions are translated from Dutch. The text between braces in the instructions for treatment ALL concerns the changes made in the instructions for treatment ALLnp.

Instructions treatment ONE

This experiment is about statements of which you do not know whether they are true or not. An example is the statement that snow did fall in Amsterdam in March 1861. You do not know for sure whether this statement is true or not. We will ask you to indicate how likely it is for you that such a statement is true, using probability judgments expressed in percentages. Perhaps you will, for example, attach a probability of 30% to the statement that it snowed in March 1861 in Amsterdam. We will then determine a score for you with the help of the added table on paper.

According to the table, for a probability judgment of 30% you get score 5100 if the statement is true (snow did fall in Amsterdam in March 1861). You get score 9100 if the statement is not true (snow did not fall in Amsterdam in March 1861). If you give a different probability judgment, you get different scores, as shown in the table. For example, if you give a probability judgment of 100%, your score is 10000 if the statement is true (snow did fall), and 0 if the statement is not true (snow did not fall). We now like to check whether the table with the scores is clear.

Practice questions using the table

Your answers were right. We will now explain some further features of the table. If you are certain that the statement is true, then it is best for you to give the maximum probability judgment of 100% because that gives the maximum score 10000 for a true statement. Every other answer then surely yields a lower score. If you are certain that the statement is not true, then it is similarly best to give the minimum probability judgment of 0%, because that gives the maximum score 10000 for a false statement. In many cases you do not know for certain whether a statement is true or not. We will now explain an important feature of the table on the basis of a thought experiment.

The properties of the table can be well illustrated with the help of repeated statements. Imagine, as a thought experiment, that you first have to give your probability judgment about a particular statement (for example, snow in Amsterdam in a particular year, say 1861). Imagine that you give judgment 30%, which means that you earn 5100 points in case of snow and 9100 points in case of no snow. Next however, various repetitions of that statement are being considered (snow in Amsterdam in March 1862, snow in Amsterdam in March 1863, …., snow in Amsterdam in March 1960), leading to a total of 100 of such statements. For all 100 statements (thus every year between 1861 and 1960) your score will be determined according to the table and your probability judgment (that is the same for every 100 statements). Your total score is then equal the sum of those 100 scores. For example, if it did snow in Amsterdam in March 35 times in those 100 years, and it did not snow 65 times, a probability judgment of 30% yields the following total score:

$$35 \times 5100 + 65 \times 9100 = 770000$$

We can also calculate this for other probability judgments, suppose that your probability judgment was 35%, then your total score was:

$$35 \times 5775 + 65 \times 8775 = 772500$$

On the next page we show that your total-score is optimal if your probability judgment is exactly equal to that percentage. Put differently, if for example 35 of the 100 (35%) statements are true, then it is best for you to choose probability judgment 35% because it will give you the highest total-score.

Now suppose that 35 of the 100 statements are true. We will determine what your total-score would have been at different judgments.

Table showing the total score for all possible probability judgments

It looks like judgment 35 is best. We conclude that if 35% of the statements are true, probability judgment 35 is optimal. Something similar holds for every percentage. Conclusion: For every percentage of true statements your total-score is optimal if you choose your probability judgment to be equal to that percentage. Check this for another number by clicking on continue.

Subjects were required to check the conclusion for any other percentage

The experiment for non-repeated statements

The experiment we will perform concerns unique, and not repeated, statements. The various unique statements we consider are all different. For every single one of them you can give a different probability judgment.

There is a big difference between the real experiment and the thought-experiment with repetition. In the thought experiment there was an objective-optimal probability judgment, based on the percentage of true statements. In the real experiment, there are no repetitions and for every probability judgment you get only one score.

The thought experiment does give a guide for your probability judgment in the real experiment, with the percentage true statements as reference point. It is now based on your own subjective judgment however, and not on objective calculations. In the real experiment, there is no right or wrong answer. You purely choose what you like best.

In the experiment, you will encounter all different sorts of statements, more or less probable ones, and you can choose all probability judgments ranging from 0% till 100%. You can only choose whole percentages.

Payoff

This experiment consists of two parts. In both parts you will be asked to give probability judgments, 100 in part 1 and 20 in part 2. At the end of the experiment, one out of 120 statements considered during the experiment will be randomly (with equal probability) selected and on the basis of your score at this statement you will be paid out in euros, where 500 points is equal to 1 euro. Click on continue to read the instructions of the first part of the experiment.

Instructions part 1

In the graph below you see the price of a stock from January till June in a year in the past. We used real stock prices of the Amsterdam Exchange when we made the graphs. The graph is scaled in such a way that the price of the stock always stays between the upper and lower axis. The same holds for the other graphs you will see later in this experiment. We consider the following statement: on the 31st of December in that particular year, the price of the stock in the graph was in the purple area. We ask you to give a probability judgment about the truth of this statement without any further information about the stock or the year. You can only base this on the course of the graph in the first half of the year.

Figure showing an example of a graph of a stock price

Your score at this question depends on your probability judgment and whether the statement is true or not, according to the table.

Figure showing the same graph but with three different end prices at 31st of December

The input of your probability judgment takes place in two phases: first you type in an integer number between 0 and 100, next you will be shown a menu in which your choice is reproduced with the corresponding scores from the table. At that moment you can still alter your choice and choose any other integer between 0 and 100. You can do this by selecting the up or down arrow, or by clicking the mouse in the menu and scroll to another probability judgment. Next, when you click on OK your choice is final and you continue with the next statement. If you have any questions at this moment, raise your hand. The experimenter will come to you.

Instructions part 2

Part 1 of the experiment is now over. The second part of the experiment consists of 20 statements. Also in this part of the experiment you will be asked to give probability judgments. The difference is that it does not concern the prediction of stock prices now, but rolls with two 10-sided dice. On one of the dice are the values 00, 10, 20, 30, 40, 50, 60, 70, 80, 90 and on the other die are the values 1, 2, 3, 4, 5, 6, 7, 8, 9. Both dice will be rolled. The sum of the outcomes has the values 1-100 (we consider the roll 00-0 as if it is 100), where all values have the same probability.

Picture showing the two ten sided dice

An example of a statement is "the outcome is in the range 01-25." This statement is true when the outcome of the dice is indeed between 1 and 25 (including 25), and not true when the outcome is higher than 25. The input of your probability judgment again takes place in two phases: first you type in an integer number between 0 and 100, next you will be shown a menu in which your choice is replicated with the corresponding scores from the table. At that moment you can still alter your choice and choose any other integer number between 0 and 100. You can do this by selecting the up or down arrow, or by clicking the mouse in the menu and scroll to another probability judgment. Next, when you click on OK your choice is final and you continue with the next statement. Also in this part there is no right or wrong answer; you again choose what you want best. At the end of the experiment one statement will be selected and paid out. In case that this is a statement from part 2 of the experiment, you will be asked to roll the two ten sided dice once.

This is the end of part 2. Please raise your hand. The experimenter will come by so that it can be determined which round will be paid out.

Instructions treatment ALL [treatment ALLnp]

This experiment is about statements of which you do not know whether they are true or not. An example is the statement that snow did fall in Amsterdam in March 1861. You do not know for sure whether this statement is true or not. We will ask you to indicate how likely it is for you that such a statement is true, using probability judgments expressed in percentages [we will ask you to report a number]. Perhaps you will, for example, attach a probability of 30% to the statement that it snowed in March 1861 in Amsterdam [sentence deleted]. We will then determine a score for you with the help of the added table on paper. [We will then

determine a score for you depending on the number you have reported with the help of the added table on paper.]

According to the table, for a probability judgment of 30% [number 30] you get score 5100 if the statement is true (snow did fall in Amsterdam in March 1861). You get score 9100 if the statement is not true (snow did not fall in Amsterdam in March 1861). If you give a different probability judgment [report a different number], you get different scores, as shown in the table. For example, if you give a probability judgment of 100% [report number 100], your score is 10000 if the statement is true (snow did fall), and 0 if the statement is not true (snow did not fall). We now like to check whether the table with the scores is clear.

Practice questions using the table

Your answers were right. We will now explain some further features of the table. If you are certain that the statement is true, then it is best for you to give the maximum probability judgment of 100% [maximum number 100] because that gives the maximum score 10000 for a true statement. Every other answer then surely yields a lower score. If you are certain that the statement is not true, then it is similarly best to give the minimum probability judgment of 0% [minimum number 0], because that gives the maximum score 10000 for a false statement. In many cases you do not know for certain whether a statement is true or not. We will now explain an important feature of the table on the basis of a thought experiment.

The properties of the table can be well illustrated with the help of repeated statements. Imagine, as a thought experiment, that you first have to give your probability judgment [number] about a particular statement (for example, snow in Amsterdam in a particular year, say 1861). Imagine that you give judgment 30% [report number 30], which means that you earn 5100 points in case of snow and 9100 points in case of no snow. Next however, various repetitions of that statement are being considered (snow in Amsterdam in March 1862, snow in Amsterdam in March 1863, …., snow in Amsterdam in March 1960), leading to a total of 100 of such statements. For all 100 statements (thus every year between 1861 and 1960) your score will be determined according to the table and your probability judgment [number] (that is the same for every 100 statements). Your total score is then equal the sum of those 100 scores. For example, if it did snow in Amsterdam in March 35 times in those 100 years, and it did not snow 65 times, a probability judgment of 30% [number 30] yields the following total score:

$$35 \times 5100 + 65 \times 9100 = 770000$$

We can also calculate this for other probability judgments [numbers], suppose that your probability judgment was 35% [number was 35], then your total score was:

$$35 \times 5775 + 65 \times 8775 = 772500$$

On the next page we show that your total-score is optimal if your probability judgment [number] is exactly equal to that percentage [the amount of times snow did fall]. Put differently, if for example 35 of the 100 statements (35%) [deleted] are true, then it is best for you to choose probability judgment 35% [report number 35] because it will give you the highest total-score.

Now suppose that 35 of the 100 statements are true. We will determine what your total-score would have been at different judgments [numbers].

Table showing the total score for all possible probability judgments

It looks like judgment [number] 35 is best. We conclude that if 35% of the statements [35 of the 100 statements] are true, probability judgment [number] 35 is optimal. Something similar holds for every percentage [number]. Conclusion: for every percentage of true statements your total-score is optimal if you choose your probability judgment [number] to be equal to that percentage [the amount of true statements]. Check this for another number by clicking on continue.

Subjects were required to check the conclusion for any other percentage [number]

The experiment for non-repeated statements

The experiment we will perform concerns unique, and not repeated, statements. The various unique statements we consider are all different. For every single one of them you can give a different probability judgment [number].

There is a big difference between the real experiment and the thought-experiment with repetition. In the thought experiment there was an objective-optimal probability judgment [number], based on the percentage [amount] of true statements. In the real experiment, there are no repetitions and for every probability judgment [number] you get only one score.

The thought experiment does give a guide for your probability judgment [number] in the real experiment, with the percentage [amount of] true statements as reference point. It is now based on your own subjective judgment however, and not on objective calculations. In the real experiment, there is no right or wrong answer. You purely choose what you like best.

In the experiment, you will encounter all different sorts of statements, more or less probable ones, and you can choose all probability judgments [numbers] ranging from 0% till 100% [0 till 100]. You can only choose whole percentages [numbers].

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Payoff

This experiment consists of two parts. In both parts you will be asked to give probability judgments [report numbers], 100 in part 1 and 20 in part 2. Whether or not a statement was true will be revealed to you at the end of the experiment. Then, all 120 statements will be considered, and all scores will be determined. Your earnings in euro are equal to the sum of all scores divided by 60000. Click on continue to read the instructions of the first part of the experiment.

Instructions part 1

In the graph below you see the price of a stock from January till June in a year in the past. We used real stock prices of the Amsterdam Exchange when we made the graphs. The graph is scaled in such a way that the price of the stock always stays between the upper and lower axis. The same holds for the other graphs you will see later in this experiment. We consider the following statement: on the 31st of December in that particular year, the price of the stock

in the graph was in the purple area. We ask you to give a probability judgment about the truth of this statement [number for this statement] without any further information about the stock or the year. You can only base this on the course of the graph in the first half of the year.

Figure showing an example of a graph of a stock price

Your score at this question depends on your probability judgment[the number you report] and whether the statement is true or not, according to the table.

Figure showing the same graph but with three different end prices at 31st of December

The input of your probability judgment [number] takes place in two phases: first you type in an integer number between 0 and 100, next you will be shown a menu in which your choice is reproduced with the corresponding scores from the table. At that moment you can still alter your choice and choose any other integer between 0 and 100. You can do this by selecting the up or down arrow, or by clicking the mouse in the menu and scroll to another probability judgment [number]. Next, when you click on OK your choice is final and you continue with the next statement. If you have any questions at this moment, raise your hand. The experimenter will come to you.

Instructions part 2

Part 1 of the experiment is now over. The second part of the experiment consists of 20 statements. Also in this part of the experiment you will be asked to give probability judgments [report numbers]. The difference is that it does not concern the prediction of stock prices now, but rolls with two 10-sided dice. On one of the dice are the values 00, 10, 20, 30, 40, 50, 60, 70, 80, 90 and on the other die are the values 1, 2, 3, 4, 5, 6, 7, 8, 9. Both dice will be rolled. The sum of the outcomes has the values 1-100 (we consider the roll 00-0 as if it is 100), where all values have the same probability.

Picture showing the two ten sided dice

An example of a statement is “the outcome is in the range 01-25.” This statement is true when the outcome of the dice is indeed between 1 and 25 (including 25), and not true when

the outcome is higher than 25. The input of your probability judgment [number] again takes place in two phases: first you type in an integer number between 0 and 100, next you will be shown a menu in which your choice is replicated with the corresponding scores from the table. At that moment you can still alter your choice and choose any other integer number between 0 and 100. You can do this by selecting the up or down arrow, or by clicking the mouse in the menu and scroll to another probability judgment. Next, when you click on OK your choice is final and you continue with the next statement. Also in this part there is no right or wrong answer; you again choose what you want best, and also in this part of the experiment, all scores will be summed and paid out. For convenience, you will therefore be asked to shake a box with 20 compartments each containing a pair of 10-sided dice at the end of the experiment. This box will then be opened, the result of each pair of dice will be inspected, and your earnings will be calculated on the basis of these results.

This is the end of part 2. The results of the lotteries will now be determined by shaking the box containing the pairs of dice. Please raise your hand so that the experimenter knows that you are ready.

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