MARTIJN J. VAN DEN ASSEM

Deal or No Deal?

Decision Making under Risk in a Large-Stake TV Game Show and Related Experiments

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Deal or No Deal? Risicogedrag in een televisieshow met grote geldbedragen en gerelateerde experimenten

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To my parents

Contents

Introduction

This thesis analyzes the decisions of subjects in a TV game show named "Deal or No Deal" (DOND). This show has received substantial attention from researchers, because it has such desirable features that it almost appears to be designed to be a risky choice experiment rather than a TV game show.

Risky choice is fundamental to virtually every branch of economics. A wide range of theories of have been developed in this field, including the normative expected utility theory of von Neumann and Morgenstern (1944) and the descriptive prospect theory of Kahneman and Tversky (1979). Unfortunately, empirical testing has proven to be difficult.

Early tests of theories of risky choice were mostly based on either thought experiments or answers to hypothetical questions. With the rising popularity of experimental economics, risky choice experiments with real monetary stakes have become more popular, but because of limited budgets most experiments are still limited to small stakes. Outside the laboratory, empirical research is typically plagued by what amounts to "joint hypothesis" problems. Researchers cannot directly observe risk preferences for most real-life problems, because the true probability distribution is not known to the subjects and the subjects' beliefs are not known to the researcher.

Despite the short history of DOND, it is already widely recognized as a natural laboratory for studying risky choice behavior. We discovered DOND and its prospects at the time the format was on the eve of an international breakthrough. DOND is developed by Endemol, a Dutch production company. It was first aired in the Netherlands in December 2002. In 2003, it was successfully exported to Australia, and in the following years, the game show was launched in dozens of other countries. By 2006, DOND was aired in a total of 46 countries. The stakes in DOND are very high and wide-ranging: contestants can go home as multimillionaires or practically empty-handed. Analyzing risky choice in TV game shows is not new, but unlike other game shows, DOND involves only simple stop-go decisions ("Deal" or "No Deal") that require minimal skill, knowledge or strategy, and the probability distribution is simple and known with near-certainty. Also, the game show involves multiple rounds, and consequently seems particularly interesting for analyzing path-dependence, or the role of earlier outcomes.

Of course, the behavior of contestants in game shows cannot always be generalized to what an ordinary person does in her everyday life when making risky decisions. While the contestants have to make decisions in just a few minutes in front of millions of viewers, many real-life decisions involving large sums of money are neither made in a hurry nor in the limelight. Still, we believe that the choices in this particular game show are worthy of study, because the decision problems are simple and well-defined, and the amounts at stake are very large. Furthermore, prior to the show, contestants have had considerable time to think about what they might do in various situations, and during the show they are encouraged to discuss those contingencies with a friend or relative who sits in the audience. In this sense, the choices may be more deliberate and considered than might appear at first glance. Indeed, it seems plausible that our contestants have given more thought to their choices on the show than to some of the other financial choices they have made in their lives such as selecting a mortgage or retirement savings investment strategy.

Chapter 1 of this thesis examines the risky choices of 151 contestants from the Netherlands (51), Germany (47) and the United States (53), and of 80 students in related classroom experiments. To analyze their "Deal or No Deal" decisions, we use, among others, structural choice models and a maximum-likelihood methodology. For each sample, a simple implementation of prospect theory explains the choices substantially better than expected utility does. The biggest losers and the biggest winners appear to have an abnormally low degree of risk aversion, consistent with the "break-even" and "house-money" effects that occur when the reference point sticks to earlier expectations (Thaler and Johnson, 1990). Although "Deal or No Deal" contestants never have to pay money out of their own pockets, they can suffer significant "paper" losses if they eliminate the largest prizes (causing the expected winnings to fall), and we find that such losses influence their subsequent choices. Many losers even appear to be risk seeking by rejecting bank offers that exceed the average remaining prize. The results point in the direction of reference-dependent choice theories, such as prospect theory, and indicate that path-dependence is relevant, even when large real monetary amounts are at stake. This first chapter is based on the paper "Deal or No Deal? Decision Making under Risk in a Large-Payoff Game Show", co-authored by Thierry Post, Guido Baltussen and Richard H. Thaler, and published in the American Economic Review.

Chapter 2 examines how risky choices in DOND depend on the context of the initial set of prizes in the game. It uses a much larger data set of ten editions, with large differences in the set of initial prizes. The ten editions originate from seven different countries and together cover approximately 6,400 risky choices from over 1,100 different contestants. The large data set and the differences in the set of prizes at the start of a game allow us to analyze framing effects by analyzing choices across editions, and reduces the need for fully specified structural models such as those employed in Chapter 1. Using probit regression analysis, we compare how the absolute and relative magnitudes of the amounts at stake affect risky choice. Our analyses within and across the different editions suggest that risky choice is highly sensitive to the context, as defined by the initial set of prizes in the game. Decisions are mainly driven by the relative size of the stakes rather than the absolute monetary size. For a given edition of DOND, changes in the amounts at stake have a strong effect on risk attitudes and choice behavior, but differences in the initial amounts across the shows have only a weak effect. Our results suggest that amounts are primarily evaluated in proportion to a subjective frame of reference rather than in terms of their absolute monetary value. This chapter is based on the paper "Risky Choice and the Relative Size of Stakes", co-authored by Guido Baltussen and Thierry Post.

Chapter 3 analyzes the effects of random task incentive systems (RTISs). A RTIS is an incentive method that is often applied in economic experiments, where only one randomly selected task is played for real. The laboratory experiments in this chapter mimic the game of DOND. We perform three different treatments, where the only factor that we vary is the incentive system that is used. In the first, or guaranteed-payment treatment, each subject plays the game once and for real. In the second, or within-subjects RTIS treatment, subjects play the game ten times, of which one is randomly selected for real payment. In the third, or between-subjects RTIS treatment, each subject plays the game once, with a ten percent chance of real payment. We investigate three potential effects that can occur in a RTIS experiment: biased risk aversion, increased decision errors, and carry-over effects from outcomes of previously performed tasks. The properties of DOND allow us to study the three effects on the basis of this single type of task. Our results suggest that caution is warranted when applying RTIS designs. In the RTIS treatments, we find a significant frequency of errors that are unrelated to the characteristics of the choice problem. Subjects appear to have lapses of concentration and/or refrain from efforts to seriously evaluate the decision problem, resulting in choices that seem to be completely at random. Furthermore, in the within-subjects RTIS design, we also observe strong carry-over effects of prior tasks: risk aversion increases after unfavorable outcomes in the two most recent previous games, and decreases after favorable outcomes. On average, risk aversion in the within-subjects RTIS experiment is not significantly different from that in the guaranteed-payment design. The between-subjects RTIS design is based on one task per subject and therefore avoids carry-over effects from prior tasks altogether. However, in this design, risk aversion is substantially lower than in the guaranteed-payment treatment, suggesting a serious bias. This chapter is based on the paper “Random Task Incentive Systems in Risky Choice Experiments”, co-authored by Guido Baltussen, Thierry Post, and Peter P. Wakker.

Chapter 1:
Deal or No Deal? Decision Making under Risk in a Large-Payoff Game Show[1]

Abstract

We examine the risky choices of contestants in the popular TV game show "Deal or No Deal" and related classroom experiments. Contrary to the traditional view of expected utility theory, the choices can be explained in large part by previous outcomes experienced during the game. Risk aversion decreases after earlier expectations have been shattered by unfavorable outcomes or surpassed by favorable outcomes. Our results point to reference-dependent choice theories such as prospect theory, and suggest that path-dependence is relevant, even when the choice problems are simple and well-defined, and when large real monetary amounts are at stake.

1.1 Introduction

A wide range of theories of risky choice have been developed, including the normative expected utility theory of von Neumann and Morgenstern (1944) and the descriptive prospect theory of Kahneman and Tversky (1979). Although risky choice is fundamental to virtually every branch of economics, empirical testing of these theories has proven to be difficult.

Many of the earliest tests such as those by Allais (1953), Ellsberg (1961), and the early work by Kahneman and Tversky were based on either thought experiments or

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[1] This chapter is based on the paper "Deal or No Deal? Decision Making under Risk in a Large-Payoff Game Show", co-authored by Thierry Post, Guido Baltussen and Richard H. Thaler, and published in the American Economic Review. We thank Nick Barberis, Ingolf Dittmann, Glenn Harrison, Phil Maymin and Peter Wakker, conference participants at BDRM X 2006 Santa Monica, FUR XII 2006 Rome, EFA XXXIII 2006 Zurich, EEA XXI 2006 Vienna and EWGFM XL 2007 Rotterdam, seminar participants at the Erasmus University of Rotterdam, the University of Zurich, the University of Groningen, the University of Amsterdam and the University of Lugano, and anonymous referees for useful comments and suggestions. We thank Monique de Koning, Endemol, TROS and Sat.1 for providing us with information and/or recordings of "Deal or No Deal", Marc Schauten for acting as game show host in the experiments, and Nick de Heer and Jan-Hein Paes for their skillful research assistance. The support by Tinbergen Institute, Erasmus Research Institute of Management and Erasmus Center for Financial Research is gratefully acknowledged.

answers to hypothetical questions. With the rising popularity of experimental economics, risky choice experiments with real monetary stakes have become more popular, but because of limited budgets most experiments are limited to small stakes. Some experimental studies try to circumvent this problem by using small nominal amounts in developing countries, so that the subjects face large amounts in real terms; see, for example, Binswanger (1980, 1981), and Kachelmeier and Shehata (1992). Still, the stakes in these experiments are typically not larger than one month's income and thus do not provide evidence about risk attitudes regarding prospects that are significant in relation to lifetime wealth.

Nonexperimental empirical research is typically plagued by what amounts to "joint hypothesis" problems. Researchers cannot directly observe risk preferences for most real-life problems, because the true probability distribution is not known to the subjects and the subjects' beliefs are not known to the researcher. For example, to infer the risk attitudes of investors from their investment portfolios, one needs to know what their beliefs are regarding the joint return distribution of the relevant asset classes. Were investors really so risk averse that they required an equity premium of 7 percent per year, or were they surprised by an unexpected number of favorable events or worried about catastrophic events that never occurred? An additional complication arises because of the possible difference between risk and uncertainty: real-life choices rarely come with precise probabilities.

In order to circumvent these problems, some researchers analyze the behavior of contestants in TV game shows, for example "Card Sharks" (Gertner, 1993), "Jeopardy!" (Metrick, 1995), "Illinois Instant Riches" (Hersch and McDougall, 1997), "Lingo" (Beetsma and Schotman, 2001), "Hoosier Millionaire" (Fullenkamp, Tenorio and Battalio, 2003) and "Who Wants to be a Millionaire?" (Hartley, Lanot and Walker, 2006). The advantage of game shows is that the amounts at stake are larger than in experiments and that the decision problems are often simpler and better defined than in real life.

The game show we use in this study, "Deal or No Deal", has such desirable features that it almost appears to be designed to be an economics experiment rather than a TV show. Here is the essence of the game. A contestant is shown 26 briefcases which each contain a hidden amount of money, ranging from €0.01 to €5,000,000 (in the

Dutch edition). The contestant picks one of the briefcases and then owns its unknown contents. Next, she selects 6 of the other 25 briefcases to open. Each opened briefcase reveals one of the 26 prizes that are *not* in her own briefcase. The contestant is then presented a “bank offer” – the opportunity to walk away with a sure amount of money – and asked the simple question: “Deal or No Deal?” If she says “No Deal”, she has to open five more briefcases, followed by a new bank offer. The game continues in this fashion until the contestant either accepts a bank offer, or rejects all offers and receives the contents of her own briefcase. The bank offers depend on the value of the unopened briefcases; if, for example, the contestant opens high-value briefcases, the bank offer falls.

This game show seems well-suited for analyzing risky choice. The stakes are very high and wide-ranging: contestants can go home as multimillionaires or practically empty-handed. Unlike other game shows, “Deal or No Deal” involves only simple stop-go decisions (“Deal” or “No Deal”) that require minimal skill, knowledge or strategy, and the probability distribution is simple and known with near-certainty (the bank offers are highly predictable, as discussed later). Finally, the game show involves multiple game rounds, and consequently seems particularly interesting for analyzing path-dependence, or the role of earlier outcomes. Thaler and Johnson (1990) conclude that risky choice is affected by prior outcomes in addition to incremental outcomes due to decision makers incompletely adapting to recent losses and gains. Although “Deal or No Deal” contestants never have to pay money out of their own pockets, they can suffer significant “paper” losses if they open high-value briefcases (causing the expected winnings to fall), and such losses may influence their subsequent choices. (Throughout this thesis we will use the term “outcomes” to indicate not only monetary pay-offs, but also new information or changed expectations.)

We examine the games of 151 contestants from the Netherlands, Germany and the United States in 2002 – 2007. The game originated in the Netherlands and is now broadcast around the world. Although the format of “Deal or No Deal” is generally similar across all editions, there are some noteworthy differences. For example, in the daily versions from Italy, France and Spain, the banker knows the amounts in the briefcases and may make informative offers, leading to strategic interaction between the banker and the contestant. In the daily edition from Australia, special game options known as “Chance” and “Supercase” are sometimes offered at the discretion of the game-show producer after a contestant has made a “Deal”. These options would complicate our analysis, because the associated probability distribution is not known, introducing a layer of uncertainty in addition to the pure risk of the game. For these reasons, we limit our analysis to the games played in the Netherlands, Germany and the United States.

The three editions have a very similar game format, apart from substantial variation in the amounts at stake. While the average prize that can be won in the Dutch edition is roughly €400,000, the averages in the German and US edition are roughly €25,000 and €100,000, respectively. At first sight, this makes the pooled data set useful for separating the effect of the amounts at stake from the effect of prior outcomes. (Within one edition, the stakes are strongly confounded with prior outcomes.) However, cross-country differences in culture, wealth and contestant selection procedure could confound the effect of stakes across the three editions. To isolate the effect of stakes on risky choice, we therefore conduct classroom experiments with a homogeneous student population. In these experiments, we vary the prizes with a factor of ten, so that we can determine if, for example, €100 has the same subjective value when it lies below or above the initial expectations.

Our findings are difficult to reconcile with expected utility theory. The contestants’ choices appear to be driven in large part by the previous outcomes experienced during the game. Risk aversion seems to decrease after earlier expectations have been shattered by opening high-value briefcases, consistent with a “break-even effect”. Similarly, risk aversion seems to decrease after earlier expectations have been surpassed by opening low-value briefcases, consistent with a “house-money effect”.

The orthodox interpretation of expected utility of wealth theory does not allow for these effects, because subjects are assumed to have the same preferences for a given choice problem, irrespective of the path traveled before arriving at this problem. Our results point in the direction of reference-dependent choice theories, such as prospect theory, and indicate that path-dependence is relevant, even when large real monetary amounts are at stake. We therefore propose a version of prospect theory with a path-dependent reference point as an alternative to expected utility theory.

Of course, we must be careful with rejecting expected utility theory and embracing alternatives like prospect theory. Although the standard implementation of expected utility theory is unable to explain the choices of losers and winners, a better fit could be achieved with a nonstandard utility function that has convex segments (as proposed by, for example, Friedman and Savage, 1948, and Markowitz, 1952), and depends on prior outcomes. Therefore, this study does not reject or accept any theory. Rather, our main finding is the important role of reference-dependence and path-dependence, phenomena that are not standard in typical implementations of expected utility, but common in prospect theory. Any plausible explanation of the choice behavior in the game will have to account for these phenomena. A theory with static preferences cannot explain why variation of the stakes due to the subject's fortune during the game has a much stronger effect than variation in the initial stakes across different editions of the TV show and experiments.

The remainder of this chapter is organized as follows. In Section 1.2, we describe the game show in greater detail. Section 1.3 discusses the data we use. Section 1.4 provides a first analysis of the risk attitudes in "Deal or No Deal" by examining the bank offers and the contestants' decisions to accept ("Deal") or reject ("No Deal") these offers. Section 1.5 analyzes the decisions using expected utility theory with a general, flexible-form expo-power utility function. Section 1.6 analyzes the decisions using prospect theory with a simple specification that allows for partial adjustment of the subjective reference point that separates losses from gains. This implementation of prospect theory explains a material part of what expected utility theory leaves unexplained. Section 1.7 reports results from classroom experiments in which students play "Deal or No Deal". The experiments confirm the important role of previous outcomes and suggest that the isolated effect of the amounts at stake is limited compared to the isolated effect of previous outcomes. Section 1.8 offers concluding remarks and suggestions for future research. Finally, in Section 1.9, an epilogue gives a synopsis of other "Deal or No Deal" studies that became available after the study in this chapter was first submitted to the American Economic Review in October 2005.

1.2 Description of the Game Show

The TV game show "Deal or No Deal" was developed by the Dutch production company Endemol and was first aired in the Netherlands in December 2002. The game show soon became very popular and was exported to dozens of other countries, including Germany and the United States. The following description applies to the Dutch episodes of "Deal or No Deal". Except for the monetary amounts, the structure of the main game is similar in the German and US versions used in this study.

Each episode consists of two parts: an elimination game based on quiz questions in order to select one finalist from the audience, and a main game in which this finalist plays "Deal or No Deal". Audience members have not been subjected to an extensive selection procedure: tickets are randomly distributed to players in the national lottery sponsoring the show. Only the main game is the subject of our study. Except for determining the identity of the finalist, the elimination game does not influence the course of the main game. The selected contestant has not won any prize before entering the main game.

The main game starts with a fixed and known set of 26 monetary amounts ranging from €0.01 to €5,000,000, which have been randomly allocated over 26 numbered and closed briefcases. One of the briefcases is selected by the contestant and this briefcase is not to be opened until the end of the game.

The game is played over a maximum of nine rounds. In each round, the finalist chooses one or more of the other 25 briefcases to be opened, revealing the prizes inside. Next, a "banker" tries to buy the briefcase from the contestant by making her an offer. Contestants have a few minutes to evaluate the offer and to decide between "Deal" and "No Deal", and may consult a friend or relative who sits nearby.[2] The remaining prizes and the current bank offer are displayed on a scoreboard and need not be memorized by the contestant. If the contestant accepts the offer ("Deal"), she walks away with this sure amount and the game ends; if the contestant rejects the offer ("No Deal"), the game continues and she enters the next round.

In the first round, the finalist has to select six briefcases to be opened, and the first bank offer is based on the remaining 20 prizes. The numbers of briefcases to be opened

[2] In the US version and in the second German series, three or four friends and/or relatives sit on stage nearby the contestant. In the Dutch version and in the first German series, only one person accompanies the contestant.

in the maximum of eight subsequent rounds are 5, 4, 3, 2, 1, 1, 1, and 1. Accordingly, the number of prizes left in the game decreases to 15, 11, 8, 6, 5, 4, 3, and 2. If the contestant rejects all nine offers she receives the prize in her own briefcase. Figure 1.1 illustrates the basic structure of the main game.

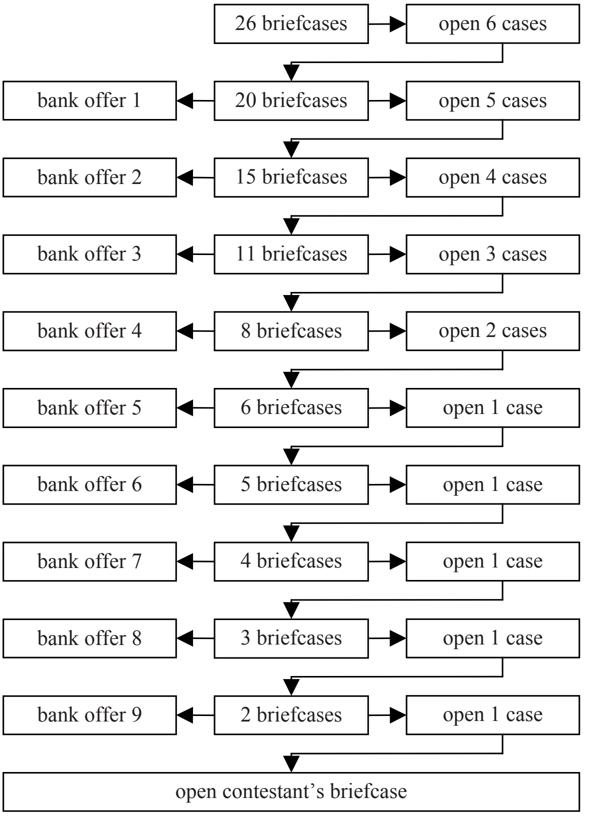

Figure 1.1: Flow chart of the main game. In each round, the finalist chooses a number of briefcases to be opened, each giving new information about the unknown prize in the contestant's own briefcase. After the prizes in the chosen briefcases are revealed, a "bank offer" is presented to the finalist. If the contestant accepts the offer ("Deal"), she walks away with the amount offered and the game ends; if the contestant rejects the offer ("No Deal"), play continues and she enters the next round. If the contestant decides "No Deal" in the ninth round, she receives the prize in her own briefcase. The flow chart applies to the Dutch and US editions and the second German series. The first German series involves one fewer game round and starts with 20 briefcases.

To provide further intuition for the game, Figure 1.2 shows a typical example of how the main game is displayed on the TV screen. A close-up of the contestant is shown in the center and the original prizes are listed to the left and the right of the contestant. Eliminated prizes are shown in a dark color and remaining prizes are in a bright color. The bank offer is displayed at the top of the screen.

As can be seen on the scoreboard, the initial prizes are highly dispersed and positively skewed. During the course of the game, the dispersion and the skewness generally fall as more and more briefcases are opened. In fact, in the ninth round, the distribution is perfectly symmetric, because the contestant then faces a 50/50 gamble with two remaining briefcases.

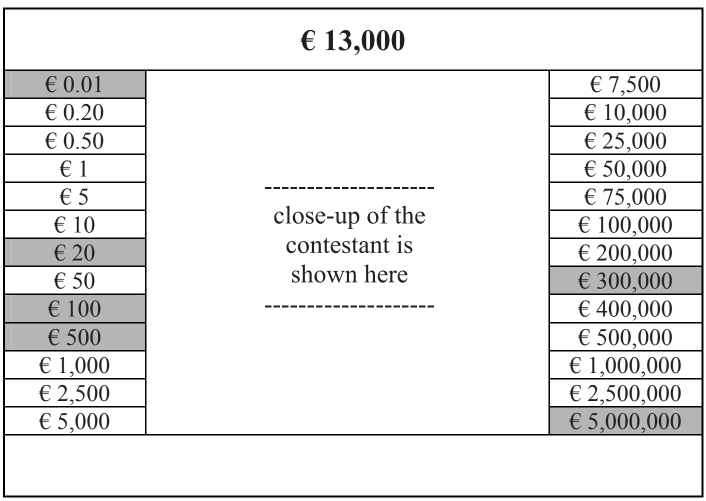

Figure 1.2: Example of the main game as displayed on the TV screen. A close-up of the contestant is shown in the center of the screen. The possible prizes are listed in the columns to the left and right of the contestant. Prizes eliminated in earlier rounds are shown in a dark color and remaining prizes are in a bright color. The top bar above the contestant shows the bank offer. This example demonstrates the two options open to the contestant after opening six briefcases in the first round: accept a bank offer of €13,000 or continue to play with the remaining 20 briefcases, one of which is the contestant's own. This example reflects the prizes in the Dutch episodes.

Bank Behavior

Although the contestants do not know the exact bank offers in advance, the banker behaves consistently according to a clear pattern. Four simple rules of thumb summarize this pattern:

Rule 1. Bank offers depend on the value of the unopened briefcases: when the lower (higher) prizes are eliminated, the average remaining prize increases (decreases) and the banker makes a better (worse) offer.

Rule 2. The offer typically starts at a low percentage (usually less than 10 percent) of the average remaining prize in the first round and gradually increases to 100 percent in the later rounds. This strategy obviously serves to encourage contestants to continue playing the game and to gradually increase excitement.

Rule 3. The offers are not informative, that is, they cannot be used to determine which of the remaining prizes is in the contestant's briefcase. Only an independent auditor knows the distribution of the prizes over the briefcases. Indeed, there is no correlation between the percentage bank offer and the relative value of the prize in the contestant's own briefcase.

Rule 4. The banker is generous to losers by offering a relatively high percentage of the average remaining prize. This pattern is consistent with path-dependent risk attitudes. If the game-show producer understands that risk aversion falls after large losses, he may understand that high offers are needed to avoid trivial choices and to keep the game entertaining to watch. Using the same reasoning, we may also expect a premium after large gains; this, however, does not occur, perhaps because with large stakes, the game is already entertaining.

Section 1.4 gives descriptive statistics on the bank offers in the sample and Section 1.5 presents a simple model that captures the rules of thumb noted above. The key finding is that the bank offers are highly predictable.

1.3 Data

We examine all "Deal or No Deal" decisions of 151 contestants appearing in episodes aired in the Netherlands (51), Germany (47), and the United States (53).

The Dutch edition of "Deal or No Deal" is called "*Miljoenenjacht*" (or "Chasing Millions"). The first Dutch episode was aired on December 22, 2002 and the last in the sample used in this chapter dates from January 1, 2007. In this time span, the game show was aired 51 times, divided over eight series of weekly episodes and four individual episodes aired on New Year's Day, with one contestant per episode. A distinguishing feature of the Dutch edition is the high amounts at stake: the average prize equals roughly €400,000 (€391,411 in episode 1 – 47 and €419,696 in episode 48 – 51). Contestants may even go home with €5,000,000. The fact that the Dutch edition is sponsored by a national lottery probably explains why the Dutch format has such large prizes. The large prizes may also have been preferred to stimulate a successful launch of the show and to pave the way for exporting the formula abroad. Part of the 51 shows were recorded on videotape by the authors and tapes of the remaining shows were obtained from the Dutch broadcasting company *TROS*.

In Germany, a first series of "*Deal or No Deal – Die Show der GlücksSpirale*" started on June 23, 2005 and a second series began on June 28, 2006.[3] Apart from the number of prizes, the two series are very similar. The first series uses 20 prizes instead of 26 and is played over a maximum of 8 game rounds instead of 9. Because these 8 rounds are exactly equal to round 2 – 9 of the regular format in terms of the number of remaining prizes and in terms of the number of briefcases that have to be opened, we can analyze this series as if the first round has been skipped. Both series have the same maximum prize (€250,000) and the averages of the initial set of prizes are practically equal (€26,347 versus €25,003 respectively). In the remainder of the chapter we will consider the two German series as one combined subsample. The first series was broadcast weekly and lasted for 10 episodes, each with two contestants playing the game sequentially. The second series was aired either once or twice a week and lasted for 27 episodes, with one contestant per episode, bringing the total number of German contestants in our sample to 47. Copies of the first series were obtained from TV station

[3] An earlier edition called "*Der MillionenDeal*" started on May 1, 2004. The initial average prize was €237,565 and the largest prize was €2,000,000. This edition however lasted for only 6 episodes and is therefore not included here.

Sat.1 and from Endemol's local production company *Endemol Deutschland*. The second series was recorded by a friend of the authors.

In the United States, the game show debuted on December 19, 2005, for five consecutive nights and returned on TV on February 27, 2006. This second series lasted for 34 episodes until early June 2006. The 39 episodes combined covered the games of 53 contestants, with some contestants starting in one episode and continuing their game in the next. The regular US format has a maximum initial prize of $1,000,000 (roughly €800,000) and an average of $131,478 (€105,182). In the games of six contestants, however, the top prizes and averages were larger to mark the launch and the finale of the second series. All US shows were recorded by the authors. US Dollars are translated into Euros by using a single fixed rate of €0.80 per $ (the actual exchange rate was within 5 percent of this rate for both the 2005 and 2006 periods).

For each contestant, we collected data on the eliminated and remaining prizes, the bank offers, and the "Deal or No Deal" decisions in every game round, leading to a panel data set with a time-series dimension (the game rounds) and a cross-section dimension (the contestants).

We also collected data on each contestant's gender, age and education. Age and education are often revealed in an introductory talk or in other conversations during the game. The level of education is coded as a dummy variable, with a value of 1 assigned to contestants with a bachelor degree level or higher (including students) or equivalent work experience. Although a contestant's level of education is usually not explicitly mentioned, it is often clear from the stated profession. We estimate the missing values for age based on the physical appearance of the contestant and information revealed in the introductory talk, for example, the age of children. However, age, gender and education do not have significant explanatory power in our analysis. In part or in whole, this may reflect a lack of sampling variation. For example, during the game, the contestant is permitted to consult with friends, family members, or spouse, and therefore decisions in this game are in effect taken by a couple or a group, mitigating the role of the individual contestant's age, gender or education. For the sake of brevity, we will pay no further attention to the role of contestant characteristics. Moreover, prior outcomes are random and unrelated to characteristics and therefore the characteristics probably would not affect our main conclusions about path-dependence, even if they would affect the level of risk aversion.

Table 1.1 shows summary statistics for our sample. Compared to the German and US contestants, the Dutch contestants on average accept lower percentage bank offers (76.3 percent versus 91.8 and 91.4 percent) and play roughly three fewer game rounds (5.2 versus 8.2 and 7.7 rounds). These differences may reflect unobserved differences in risk aversion due to differences in wealth, culture or contestant selection procedure. In addition, increasing relative risk aversion (IRRA) may help to explain the differences. As the Dutch edition involves much larger stakes than the German and US editions, a modest increase in relative risk aversion suffices to yield sizeable differences in the accepted percentages. Furthermore, the observed differences in the number of rounds played are inflated by the behavior of the banker. The percentage bank offer increases with relatively small steps in the later game rounds and consequently a modest increase in relative risk aversion can yield a large reduction in the number of game rounds played. Thus, the differences between the Dutch contestants on the one hand and the German and US contestants on the other hand are consistent with moderate IRRA.

Cross-Country Analysis

Apart from the amounts at stake, the game show format is very similar in the three countries. Still, there are some differences in how contestants are chosen to play that may create differences in the contestant pool. In the Dutch and German episodes in our sample there is a preliminary game in which contestants answer quiz questions, the winner of which gets to play the main game we study. One special feature of the Dutch edition is the existence of a "bail-out offer" at the end of the elimination game: just before a last, decisive question, the two remaining contestants can avoid losing and leaving empty-handed by accepting an unknown prize that is announced to be worth at least €20,000 (approximately 5 percent of the average prize in the main game) and typically turns out to be a prize such as a world trip or a car. If the more risk-averse pre-finalists are more likely to exit the game at this stage, the Dutch finalists might be expected to be less risk averse on average. In the United States, contestants are not selected based on an elimination game but rather the producer selects each contestant

Table 1.1: Summary statistics. The table shows descriptive statistics for our sample of 151 contestants from the Netherlands (51; panel A), Germany (47; panel B) and the United States (53; panel C). The contestants' characteristics age and education are revealed in an introduction talk or in other conversations between the host and the contestant. Age is measured in years. Gender is a dummy variable with a value of one assigned to females. Education is a dummy variable that takes a value of one for contestants with a bachelor degree or higher (including students) or equivalent work experience. Stop Round is the round number in which the bank offer is accepted. The round numbers from the first series of German episodes are adjusted by +1 to correct for the lower initial number of briefcases and game rounds; for contestants who played the game to the end, the stop round is set equal to 10. Best Offer Rejected is the highest percentage bank offer the contestant chose to reject ("No Deal"). Offer Accepted is the percentage bank offer accepted by the contestant ("Deal"), or 100 percent for contestants who rejected all offers. Amount Won equals the accepted bank offer in monetary terms, or the prize in the contestant's own briefcase for contestants who rejected all offers.

	Mean	Stdev	Min	Median	Max
A. Netherlands ($N = 51$)					
Age (years)	45.31	11.51	21.00	43.00	70.00
Gender (female = 1)	0.27	0.45	0.00	0.00	1.00
Education (high = 1)	0.55	0.50	0.00	1.00	1.00
Stop Round	5.22	1.75	3.00	5.00	10.00
Best Offer Rejected (%)	55.89	32.73	10.17	55.32	119.88
Offer Accepted (%)	76.27	30.99	20.77	79.29	165.50
Amount Won (€)	227,264.90	270,443.20	10.00	148,000.00	1,495,000.00
B. Germany ($N = 47$)					
Age (years)	36.47	8.17	20.00	35.00	55.00
Gender (female = 1)	0.34	0.48	0.00	0.00	1.00
Education (high = 1)	0.47	0.50	0.00	0.00	1.00
Stop Round	8.21	1.53	5.00	8.00	10.00
Best Offer Rejected (%)	89.07	33.90	37.31	88.22	190.40
Offer Accepted (%)	91.79	19.15	52.78	95.99	149.97
Amount Won (€)	20,602.56	25,946.69	0.01	14,700.00	150,000.00
C. United States ($N = 53$)					
Age (years)	34.98	10.03	22.00	33.00	76.00
Gender (female = 1)	0.57	0.50	0.00	1.00	1.00
Education (high = 1)	0.49	0.50	0.00	0.00	1.00
Stop Round	7.70	1.29	5.00	8.00	10.00
Best Offer Rejected (%)	80.98	17.57	44.04	83.52	112.00
Offer Accepted (%)	91.43	15.31	49.16	97.83	112.50
Amount Won ($)	122,544.58	119,446.18	5.00	94,000.00	464,000.00

individually, and the selection process appears to be based at least in part on the appearance and personalities of the contestants. (The Web site for the show tells prospective contestants to send a video of themselves and their proposed accompanying friends and relatives. The show also conducts open "casting calls".) Contestants (and their friends) thus tend to be attractive and lively. Another concern is that richer and more risk-seeking people may be more willing to spend time attempting to get onto large-stake editions than onto small-stake editions. To circumvent these problems, Section 1.7 complements the analysis of the TV shows with classroom experiments that use a homogeneous student population.

1.4 Preliminary Analysis

To get a first glimpse of the risk preferences in “Deal or No Deal”, we analyze the offers made by the banker and the contestants’ decisions to accept or reject these offers.

Several notable features of the game can be seen in Table 1.2. First, the banker becomes more generous by offering higher percentages as the game progresses (“Rule 2”). The offers typically start at a small fraction of the average prize and approach 100 percent in the later rounds. The strong similarity between the percentages in the Dutch edition (panel A), the German edition (panel B) and the US edition (panel C) suggest that the banker behaves in a similar way across the three editions. (A spokesman from Endemol, the production company, confirmed that the guidelines for bank offers are the same for all three editions included in our sample.) The number of remaining contestants in every round clearly shows that the Dutch contestants tend to stop earlier and accept relatively lower bank offers than the German and US contestants do. Again, this may reflect the substantially larger stakes in the Dutch edition, or, alternatively, unobserved differences in risk aversion due to differences in wealth, culture or contestant selection procedure. Third, the contestants generally exhibit what might be called only “moderate” risk aversion. In the US and German sample, all contestants keep playing until the bank offer is at least half the expected value of the prizes in the unopened briefcases. In round 3 in the Netherlands, 20 percent of the contestants (10 out of 51) do accept deals that average only 36 percent of the expected value of the unopened briefcases, albeit at stakes that exceed €400,000. Many contestants turn down offers of 70 percent or more of amounts exceeding €100,000. Fourth, there can be wide discrepancies, even within a country, in the stakes that contestants face. In the Dutch show, contestants can be playing for many hundreds of thousands of Euros, down to a thousand or less. In the later rounds, the contestant is likely to face relatively small stakes, as a consequence of the skewness of the initial set of prizes.

Table 1.2: Bank offers and contestants' decisions. The table shows summary statistics for the percentage bank offers and contestants' decisions in our sample of 151 contestants from the Netherlands (51; panel A), Germany (47; panel B) and the United States (53; panel C). The average bank offer as a percentage of the average remaining prize (%BO), the average remaining prize in Euros (Stakes) and the number of contestants (No.) are reported for each game round ($r = 1,\cdots,9$). The statistics are also shown separately for contestants accepting the bank offer ("Deal") and for contestants rejecting the bank offer ("No Deal"). The round numbers from the first series of German episodes are adjusted by +1 to correct for the lower initial number of briefcases and game rounds.

	Unconditional			"Deal"			"No Deal"		
Round	%BO	Stakes	No.	%BO	Stakes	No.	%BO	Stakes	No.
A. Netherlands ($N = 51$)									
1	6%	387,867	51	-	-	0	6%	387,867	51
2	14%	376,664	51	-	-	0	14%	376,664	51
3	34%	369,070	51	36%	409,802	10	33%	359,135	41
4	61%	348,820	41	69%	394,860	11	58%	331,939	30
5	77%	317,618	30	82%	557,680	7	76%	244,555	23
6	88%	234,877	23	90%	237,416	12	87%	232,107	11
7	98%	243,868	11	104%	414,106	6	91%	39,582	5
8	96%	50,376	5	100%	78,401	3	90%	8,338	2
9	106%	11,253	2	91%	17,500	1	120%	5,005	1
B. Germany ($N = 47$)									
1	8%	24,277	27	-	-	0	8%	24,277	27
2	15%	24,915	47	-	-	0	15%	24,915	47
3	34%	23,642	47	-	-	0	34%	23,642	47
4	46%	21,218	47	-	-	0	46%	21,218	47
5	59%	22,304	47	59%	29,976	2	59%	21,963	45
6	72%	20,557	45	67%	48,038	7	73%	15,494	38
7	88%	15,231	38	85%	21,216	5	88%	14,324	33
8	98%	15,545	33	91%	28,813	10	101%	9,776	23
9	103%	14,017	23	109%	13,925	11	99%	14,101	12
C. United States ($N = 53$)									
1	11%	152,551	53	-	-	0	11%	152,551	53
2	21%	151,885	53	-	-	0	21%	151,885	53
3	36%	147,103	53	-	-	0	36%	147,103	53
4	50%	148,299	53	-	-	0	50%	148,299	53
5	62%	148,832	53	79%	118,517	1	61%	150,434	52
6	73%	150,549	52	74%	139,421	9	73%	152,879	43
7	88%	154,875	43	91%	204,263	15	86%	128,416	28
8	92%	114,281	28	96%	183,917	14	88%	44,644	14
9	98%	39,922	14	99%	53,825	8	97%	21,384	6

It is not apparent from this table what effect the particular path a player takes can have on the choices she makes. To give an example of the decisions faced by an unlucky player, consider poor Frank, who appeared in the Dutch episode of January 1, 2005 (see Table 1.3).

Table 1.3: Example "Frank". The table shows the gambles presented to a Dutch contestant named Frank and the "Deal or No Deal" decisions made by him in game rounds 1 – 9. This particular episode was broadcast on January 1, 2005. For each game round, the table shows the remaining prizes, the average remaining prize, the bank offer, the percentage bank offer and the "Deal or No Deal" decision. Frank ended up with a prize of €10.

	Game Round (r)								
Prize (€)	1	2	3	4	5	6	7	8	9
0.01	x	x							
0.20	x	x							
0.50	x	x	x	x	x	x	x		
1	x	x	x	x	x				
5									
10	x	x	x	x	x	x	x	x	x
20	x	x	x	x	x	x	x	x	
50									
100									
500									
1,000	x								
2,500	x	x	x						
5,000	x	x							
7,500									
10,000	x	x	x	x	x	x	x	x	x
25,000	x	x							
50,000	x	x	x	x					
75,000	x	x	x						
100,000	x	x	x						
200,000	x	x	x	x					
300,000	x								
400,000	x								
500,000	x	x	x	x	x	x			
1,000,000	x								
2,500,000									
5,000,000	x								
Average (€)	383,427	64,502	85,230	95,004	85,005	102,006	2,508	3,343	5,005
Offer (€)	17,000	8,000	23,000	44,000	52,000	75,000	2,400	3,500	6,000
Offer (%)	4%	12%	27%	46%	61%	74%	96%	105%	120%
Decision	No Deal	No Deal	No Deal	No Deal	No Deal	No Deal	No Deal	No Deal	No Deal

In round 7, after several unlucky picks, Frank opened the briefcase with the last remaining large prize (€500,000) and saw the expected prize tumble from €102,006 to €2,508. The banker then offered him €2,400, or 96 percent of the average remaining prize. Frank rejected this offer and play continued. In the subsequent rounds, Frank deliberately chose to enter unfair gambles, to finally end up with a briefcase worth only €10. Specifically, in round 8, he rejected an offer of 105 percent of the average remaining prize; in round 9, he even rejected a certain €6,000 in favor of a 50/50 gamble of €10 or €10,000. We feel confident to classify this last decision as risk-seeking behavior, because it involves a single, simple, symmetric gamble with thousands of Euros at stake. Also, unless we are willing to assume that Frank would always accept unfair gambles of this magnitude, the only reasonable explanation for his choice behavior seems to be a reaction to his misfortune experienced earlier in the game.

In contrast, consider the exhilarating ride of Susanne, an extremely fortunate contestant who appeared in the German episode of August 23, 2006 (see Table 1.4).

Table 1.4: Example "Susanne". The table shows the gambles presented to a German contestant named Susanne and the "Deal or No Deal" decisions made by her in game rounds 1 – 9. This particular episode was broadcast on August 23, 2006. For each game round, the table shows the remaining prizes, the average remaining prize, the bank offer, the percentage bank offer, and the "Deal or No Deal" decision. Susanne ended up with a prize of €150,000.

	Game Round (*r*)								
Prize (€)	1	2	3	4	5	6	7	8	9
0.01	X	X	X	X					
0.20	X	X	X						
0.50	X	X	X	X	X	X	X		
1									
5									
10									
20	X	X							
50	X	X							
100	X	X	X	X					
200									
300	X	X	X						
400	X								
500									
1,000	X	X	X	X	X	X	X	X	
2,500	X	X	X	X	X	X			
5,000	X								
7,500									
10,000	X	X							
12,500	X	X	X						
15,000	X								
20,000	X	X							
25,000	X	X	X	X	X				
50,000	X								
100,000	X	X	X	X	X	X	X	X	X
150,000	X	X	X	X	X	X	X	X	X
250,000	X								
Average (€)	32,094	21,431	26,491	34,825	46,417	50,700	62,750	83,667	125,000
Offer (€)	3,400	4,350	10,000	15,600	25,000	31,400	46,000	75,300	125,000
Offer (%)	11%	20%	38%	45%	54%	62%	73%	90%	100%
Decision	No Deal	No Deal	No Deal	No Deal	No Deal	No Deal	No Deal	No Deal	No Deal

After a series of very lucky picks, she eliminated the last small prize of €1,000 in round 8. In round 9, she then faced a 50/50 gamble of €100,000 or €150,000, two of the three largest prizes in the German edition. While she was concerned and hesitant in the earlier game rounds, she decidedly rejected the bank offer of €125,000, the expected value of the gamble; a clear display of risk-seeking behavior and one that proved fortuitous in this case as she finally ended up winning €150,000.

Thus both unlucky Frank and lucky Susanne exhibit very low levels of risk aversion, even risk-seeking, whereas most of the contestants in the shows are at least moderately risk averse. Frank's behavior is consistent with a "break-even" effect, a willingness to gamble in order to get back to some perceived reference point. Susanne's behavior is consistent with a "house-money" effect, an increased willingness to gamble when someone thinks she is playing with "someone else's money".

To systematically analyze the effect of prior outcomes such as the extreme ones experienced by Frank and Suzanne, we first develop a rough classification of game situations in which the contestant is classified as a "loser" or a "winner", and analyze the decisions of contestants in these categories separately.

Our classification takes into account the downside risk and upside potential of rejecting the current bank offer. A contestant is a loser if her average remaining prize after opening one additional briefcase is low, even if the best-case scenario of eliminating the lowest remaining prize would occur. Using $\bar{x}_r$ for the current average, the average remaining prize in the best-case scenario is:

$$BC_r = \frac{n_r\,\bar{x}_r - x_r^{\min}}{n_r - 1} \tag{1.1}$$

where n_r stands for the number of remaining briefcases in game round $r = 1, \cdots, 9$ and $x_r^{\min}$ for the smallest remaining prize. Similarly, winners are classified by the average remaining prize in the worst-case scenario of eliminating the largest remaining prize, $x_r^{\max}$:

(1.2) $$WC_r = \frac{n_r\,\overline{x}_r - x_r^{\max}}{n_r - 1}$$

More specifically, we classify a contestant in a given game round as a "loser" if BC_r belongs to the worst one-third for all contestants in that game round and as a "winner" if WC_r belongs to the best one-third.[4] Game situations that satisfy neither of the two conditions (or both in rare occasions) are classified as "neutral".

Of course, there are numerous ways one could allocate players into winner and loser categories. The results we show are robust to other classification schemes, provided that the classification of winners accounts for the downside risk of continuing play: the house-money effect – a decreased risk aversion after prior gains – is weak if incremental losses can exceed prior gains. For example, partitioning on just the current average ($\overline{x}_r$) does not distinguish between situations with different dispersion around that average, and therefore takes no account of the downside risk of continuing play.

Table 1.5 illustrates the effect of previous outcomes on the contestants' choice behavior. We see that, compared to contestants who are in the neutral category, both winners and losers have a stronger tendency to continue play. While 31 percent of all "Deal or No Deal" choices in the neutral group are "Deal" in the Dutch sample, the "Deal" percentage is only 14 percent for losers – despite the generous offers they are presented ("Rule 4"). The low "Deal" percentage for losers suggests that risk aversion decreases when contestants have been unlucky in selecting which briefcases to open. In fact, the strong losers in our sample generally exhibit risk-seeking behavior by rejecting bank offers in excess of the average remaining prize.

The low "Deal" percentage could be explained in part by the smaller stakes faced by losers and a lower risk aversion for small stakes, or increasing relative risk aversion (IRRA). However, the losers generally still have at least thousands or tens of thousands of Euros at stake and gambles of this magnitude are typically associated with risk aversion in other empirical studies (including other game show studies and experimental studies). Also, if the stakes explained the low risk aversion of losers, we would expect a

[4] To account for the variation in the initial set of prizes within an edition (see Section 1.3), BC_r and BW_r are scaled by the initial average prize.

Table 1.5: "Deal or No Deal" decisions after bad and good fortune. The table summarizes the "Deal or No Deal" decisions for our sample of 151 contestants from the Netherlands (51; panel A), Germany (47; panel B) and the United States (53; panel C). The samples are split based on the fortune experienced by contestants during the game. A contestant is classified as a "loser" if her average remaining prize after eliminating the lowest remaining prize is among the worst one-third for all contestants in the same game round; she is a "winner" if the average after eliminating the largest remaining prize is among the best one-third. For each category and game round, the table displays the percentage bank offer (%BO), the number of contestants (No.) and the percentage of contestants choosing "Deal" (%D). The round numbers from the first series of German episodes are adjusted by +1 to correct for the lower initial number of briefcases and game rounds.

	Loser			Neutral			Winner		
Round	%BO	No.	%D	%BO	No.	%D	%BO	No.	%D
A. Netherlands ($N = 51$)									
1	6%	17	0%	6%	17	0%	6%	17	0%
2	15%	17	0%	12%	17	0%	15%	17	0%
3	40%	17	12%	29%	17	41%	31%	17	6%
4	69%	14	14%	58%	13	46%	54%	14	21%
5	82%	10	10%	71%	10	20%	78%	10	40%
6	94%	8	50%	85%	7	43%	86%	8	63%
7	99%	4	25%	97%	3	67%	99%	4	75%
8	105%	1	0%	91%	3	67%	100%	1	100%
9	120%	1	0%	-	0	-	91%	1	100%
2 - 9		72	14%		70	31%		72	25%
B. Germany ($N = 47$)									
1	7%	9	0%	7%	9	0%	8%	9	0%
2	16%	16	0%	13%	15	0%	14%	16	0%
3	35%	16	0%	33%	15	0%	33%	16	0%
4	46%	16	0%	44%	15	0%	47%	16	0%
5	65%	16	0%	54%	15	13%	57%	16	0%
6	83%	15	0%	67%	15	20%	66%	15	27%
7	107%	13	0%	80%	12	25%	76%	13	15%
8	117%	11	0%	89%	11	55%	86%	11	36%
9	107%	8	38%	106%	7	57%	98%	8	50%
2 - 9		111	3%		105	17%		111	13%
C. United States ($N = 53$)									
1	9%	18	0%	10%	17	0%	13%	18	0%
2	19%	18	0%	19%	17	0%	25%	18	0%
3	41%	18	0%	29%	17	0%	39%	18	0%
4	57%	18	0%	42%	17	0%	51%	18	0%
5	69%	18	0%	55%	17	6%	62%	18	0%
6	78%	18	11%	68%	16	31%	73%	18	11%
7	92%	15	27%	87%	13	23%	84%	15	53%
8	94%	9	22%	95%	10	70%	87%	9	56%
9	92%	4	50%	101%	6	67%	99%	4	50%
2 - 9		118	8%		113	18%		118	14%

higher risk aversion for winners. However, risk aversion seems to decrease when contestants are lucky and have eliminated low-value briefcases. The "Deal" percentage for winners is 25 percent, below the 31 percent for the neutral group.

Interestingly, the same pattern arises in all three countries. The overall "Deal" percentages in the German and US editions are lower than in the Dutch edition, consistent with moderate IRRA and the substantially smaller stakes. Within every edition, however, the losers and winners have relatively low "Deal" percentages.

These results suggest that prior outcomes are an important determinant of risky choice. This is inconsistent with the traditional interpretation of expected utility theory in which the preferences for a given choice problem do not depend on the path traveled before arriving at the choice problem. By contrast, path-dependence can be incorporated quite naturally in prospect theory. The lower risk aversion after misfortune is reminiscent of the break-even effect, or decision makers being more willing to take risks due to incomplete adaptation to previous losses. Similarly, the relatively low "Deal" percentage for winners is consistent with the house-money effect, or a lower risk aversion after earlier gains.

Obviously, this preliminary analysis of "Deal" percentages is rather crude. It does not specify an explicit model of risky choice and it does not account for the precise choices (bank offers and remaining prizes) the contestants face. Furthermore, there is no attempt at statistical inference or controlling for confounding effects at this stage of our analysis. The next two sections use a structural choice model and a maximum-likelihood methodology to analyze the "Deal or No Deal" choices in greater detail.

1.5 Expected Utility Theory

This section analyzes the observed "Deal or No Deal" choices with the standard expected utility of wealth theory. The choice of the appropriate class of utility functions is important, because preferences are evaluated on an interval from cents to millions. We do not want to restrict our analysis to a classical power or exponential utility function, because it seems too restrictive to assume constant relative risk aversion (CRRA) or constant absolute risk aversion (CARA) for this interval. To allow for the plausible combination of increasing relative risk aversion (IRRA) and decreasing absolute risk aversion (DARA), we employ a variant of the flexible expo-power family of Saha (1993) that was used by Abdellaoui, Barrios and Wakker (2007) and by Holt and Laury (2002):

(1.3) $$u(x) = \frac{1 - \exp(-\alpha(W + x)^{1-\beta})}{\alpha}$$

In this function, three parameters are unknown: the risk aversion coefficients α and β, and the initial wealth parameter W. The classical CRRA power function arises as the limiting case where $\alpha \to 0$ and the CARA exponential function arises as the special case where $\beta = 0$. Theoretically, the correct measure of wealth should be lifetime wealth, including the present value of future income. However, lifetime wealth is not observable and it is possible that contestants do not integrate their existing wealth with the payoffs of the game. Therefore, we include initial wealth as a free parameter in our model.

We will estimate the three unknown parameters using a maximum likelihood procedure that measures the likelihood of the observed "Deal or No Deal" decisions based on the "stop value," or the utility of the current bank offer, and the "continuation value," or the expected utility of the unknown winnings when rejecting the offer. In a given round r, $B(x_r)$ denotes the bank offer as a function of the set of remaining prizes x_r. The stop value is simply:

(1.4) $$sv(x_r) = u(B(x_r))$$

Analyzing the continuation value is more complicated. We elaborate on the continuation value, the bank offer model and the estimation procedure below.

Continuation Value

The game involves multiple rounds and the continuation value has to account for the bank offers and optimal decisions in all later rounds. In theory, we can solve the entire dynamic optimization problem by means of backward induction, using Bellman's principle of optimality. Starting with the ninth round, we can determine the optimal "Deal or No Deal" decision in each preceding game round, accounting for the possible scenarios and the optimal decisions in subsequent rounds. This approach assumes, however, that the contestant takes into account all possible outcomes and decisions in all subsequent game rounds. Studies on backward induction in simple alternating-offers bargaining experiments suggest that subjects generally do only one or two steps of strategic reasoning and ignore further steps of the backward induction process; see, for example, Johnson *et al.* (2002) and Binmore *et al.* (2002). This pleads for assuming that the contestants adopt a simplified mental frame of the game.

Our video material indeed suggests that contestants generally look only one round ahead. The game-show host tends to stress what will happen to the bank offer in the next round should particular briefcases be eliminated and the contestants themselves often comment that they will play "just one more round" (although they often change their minds and continue to play later on). We therefore assume a simple "myopic" frame. Using this frame, the contestant compares the current bank offer with the unknown offer in the next round, and ignores the option to continue play thereafter.

Given the current set of prizes (x_r), the statistical distribution of the set of prizes in the next round (x_{r+1}) is known:

(1.5) $$\Pr[x_{r+1} = y \mid x_r] = \binom{n_r}{n_{r+1}}^{-1} = p_r$$

for any given subset y of n_{r+1} elements from x_r. In words, the probability is simply one divided by the number of possible combinations of n_{r+1} out of n_r. Thus, using $\mathrm{X}(x_r)$ for all such subsets, the continuation value for a myopic contestant is given by:

(1.6) $$cv(x_r) = \sum_{y \in \mathrm{X}(x_r)} u(B(y))\, p_r$$

Given the cognitive burden of multi-stage induction, this frame seems the appropriate choice for this game. However, as a robustness check, we have also replicated our estimates using the rational model of full backward induction and have found that our parameter estimates and the empirical fit did not change materially. In the early game rounds, when backward induction appears most relevant, the myopic model underestimates the continuation value. Still, the myopic model generally correctly predicts "No Deal", because the expected bank offers usually increase substantially during the early rounds, so even the myopic continuation value is generally greater than the stop value. In the later game rounds, backward induction is of less importance, because fewer game rounds remain to be played and because the rate of increase in the expected bank offers slows down. For contestants who reach round nine, such as Frank and Susanne, the decision problem involves just one stage and the myopic model coincides with the rational model. The low propensity of losers and winners in later game rounds to "Deal" is therefore equally puzzling under the assumption of full backward induction.

Bank Offers

To apply the myopic model, we need to quantify the behavior of the banker. Section 1.2 discussed the bank offers in a qualitative manner. For a contestant who currently faces remaining prizes x_r and percentage bank offer b_r in game round $r = 1, \cdots, 9$, we quantify this behavior using the following simple model:

(1.7) $$B(x_{r+1}) = b_{r+1}\overline{x}_{r+1}$$

(1.8) $$b_{r+1} = b_r + (1 - b_r)\rho^{(9-r)}$$

where ρ, $0 \leq \rho \leq 1$, measures the speed at which the percentage offer goes to 100 percent. Since myopic contestants are assumed to look just one round ahead, the model predicts the offer in the next round only. The bank offer in the first round needs not be predicted, because it is shown on the scoreboard when the first "Deal or No Deal" choice has to be made. $B(x_{10}) = x_{10}$ and $b_{10} = 1$ refer to the prize in the contestant's own briefcase.

The model does not include an explicit premium for losers. However, before misfortune arises, the continuation value is driven mostly by the favorable scenarios and the precise percentage offers for unfavorable scenarios do not materially affect the results. After bad luck, the premium is included in the current percentage and extrapolated to the next game round.

For each edition, we estimate the value of ρ by fitting the model to the sample of percentage offers made to all contestants in all relevant game rounds using least squares regression analysis. The resulting estimates are very similar for each edition: 0.832 for the Dutch edition, 0.815 for the first German series, 0.735 for the second German series and 0.777 for the US shows. The model gives a remarkably good fit. Figure 1.3 illustrates the goodness-of-fit by plotting the predicted bank offers against the actual offers. The results are highly comparable for the three editions in our study and therefore the figure shows the pooled results. For each individual sample, the model explains well over 70 percent of the total variation in the individual percentage offers. The explanatory power is even higher for monetary offers, with an R-squared of roughly 95 percent for each sample. Arguably, accurate monetary offers are more relevant for accurate risk aversion estimates than accurate percentage offers, because the favorable scenarios with high monetary offers weigh heavily on expected utility. On the other hand, to analyze risk behavior following the elimination of the largest prizes, accurate estimates for low monetary offers are also needed. It is therefore comforting that the fit is good in terms of both percentages and monetary amounts. In addition, if ρ is used as a free parameter in our structural choice models, the optimal values are approximately the same as our estimates, further confirming the goodness.

Since the principle behind the bank offers becomes clear after seeing a few shows, the bank offer model (7) – (8) is treated as deterministic and known to the contestants. Using a stochastic bank offer model would introduce an extra layer of uncertainty, yielding lower continuation values. For losers, the bank offers are hardest to predict, making it even more difficult to rationalize why these contestants continue play.

Maximum Likelihood Estimation

In the spirit of Becker, DeGroot and Marschak (1963), and Hey and Orme (1994), we assume that the "Deal or No Deal" decision of a given contestant $i = 1, \cdots, N$ in a given game round $r = 1, \cdots, 9$ is based on the difference between the continuation value and the stop value, or $cv(x_{i,r}) - sv(x_{i,r})$, plus some error. The errors are treated as

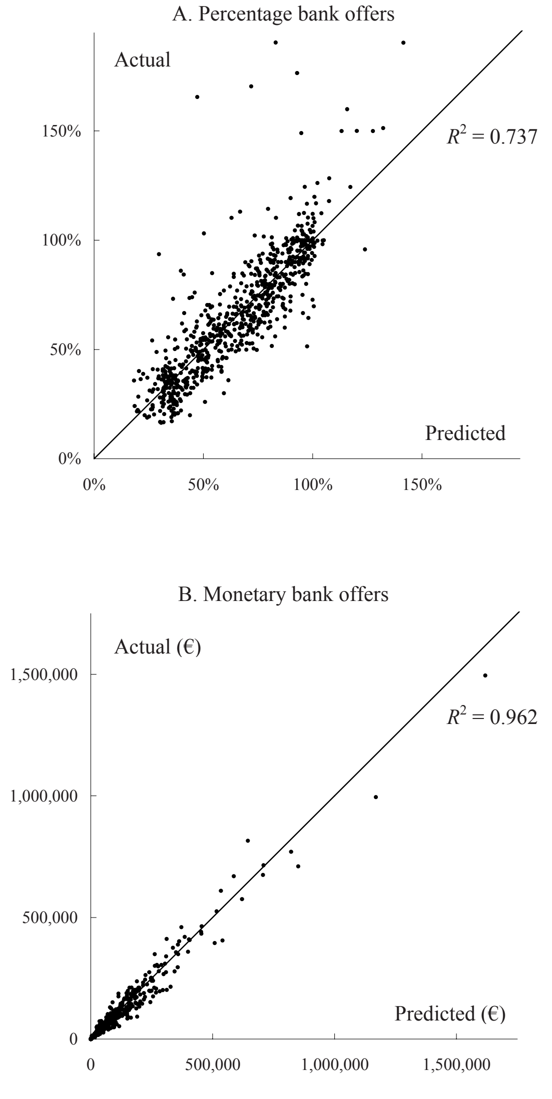

Figure 3: Predicted bank offers versus actual bank offers. The figure displays the goodness of our bank offer model by plotting the predicted bank offers versus the actual bank offers for all relevant game rounds in our pooled sample of 151 contestants from the Netherlands, Germany and the United States. Panel A shows the fit for the percentage bank offers and panel B shows the fit for the monetary bank offers (in Euros). A 45-degree line (perfect fit) is added for ease of interpretation.

independent, normally distributed random variables with zero mean and standard deviation $\sigma_{i,r}$. Arguably, the error standard deviation should be higher for difficult choices than for simple choices. A natural indicator of the difficulty of a decision is the standard deviation of the utility of the outcomes used to compute the continuation value:

$$\delta(x_{i,r}) = \sqrt{\sum_{y \in X(x_{i,r})} (u(B(y)) - cv(x_{i,r}))^2 p_r} \tag{1.9}$$

We assume that the error standard deviation is proportional to this indicator, that is, $\sigma_{i,r} = \delta(x_{i,r})\sigma$, where σ is a constant noise parameter. As a result of this assumption, the simple choices effectively receive a larger weight in the analysis than the difficult ones. We also investigated the data without weighting. The (unreported) results show that the overall fit in the three samples deteriorates. In addition, without weighting, the estimated noise parameters in the three editions strongly diverge, with the Dutch edition having a substantially higher noise level than the German and US editions. The increase in the noise level seems to reflect the higher difficulty of the decisions in the Dutch edition relative to the German and US editions; contestants in the Dutch edition typically face (i) larger stakes because of the large initial prizes and (ii) more remaining prizes because they exit the game at an earlier stage. The standard deviation of the outcomes (9) picks up these two factors. The deterioration of the fit and the divergence of the estimated noise levels provide additional, empirical arguments for our weighting scheme.

Given these assumptions, we may compute the likelihood of the "Deal or No Deal" decision as:

$$l(x_{i,r}) = \begin{cases} \Phi\left(\dfrac{cv(x_{i,r}) - sv(x_{i,r})}{\delta(x_{i,r})\sigma}\right) & \text{if "No Deal"} \\ \Phi\left(\dfrac{sv(x_{i,r}) - cv(x_{i,r})}{\delta(x_{i,r})\sigma}\right) & \text{if "Deal"} \end{cases} \tag{1.10}$$

where $\Phi(\cdot)$ is the cumulative standard normal distribution function.[5]

Aggregating the likelihood across contestants, the overall log-likelihood function of the "Deal or No Deal" decisions is given by:

$$\ln(L) = \sum_{i=1}^{N} \sum_{r=2}^{R_i} \ln(l(x_{i,r})) \tag{1.11}$$

where R_i is the last game round played by contestant i.

To allow for the possibility that the errors of individual contestants are correlated, we perform a cluster correction on the standard errors (see, for example, Wooldridge, 2003). Note that the summation starts in the second game round ($r = 2$). The early German episodes with only eight game rounds effectively start in this game round and in order to align these episodes with the rest of the sample, we exclude the first round ($r = 1$) of the editions with nine game rounds. Due to the very conservative bank offers, the choices in the first round are always trivial (no contestant in our sample ever said "Deal"); including these choices does not affect the results, but it would falsely make the early German episodes look more "noisy" than the rest of the sample.

The unknown parameters in our model (α, β, W, and σ) are selected to maximize the overall log-likelihood. To determine if the model works significantly better than a naïve model of risk neutrality, we perform a likelihood ratio test.

Results

Table 1.6 summarizes our estimation results. Apart from coefficient estimates and p-values, we have also computed the implied certainty equivalent as a fraction of the expected value, or certainty coefficient (CC), for 50/50 gambles of €0 or €10^z, $z = 1,\cdots,6$. These values help to interpret the coefficient estimates by illustrating the shape of the utility function. Notably, the CC can be interpreted as the critical bank offer (as a fraction of the expected value of the 50/50 gamble) that would make the contestant

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[5] This error model allows for violations of first-order stochastic dominance (FSD). The probability of "Deal" is predicted to be larger than zero and smaller than unity, even when the bank offer is smaller than the smallest outcome ("No Deal" dominates "Deal") or larger than the largest outcome ("Deal" dominates "No Deal"). As pointed out by an anonymous referee, a truncated error model can avoid such violations of FSD. In our data set, however, the bank offer is always substantially larger than the smallest and substantially smaller than the largest outcome, and violations of FSD do not occur.

indifferent between "Deal" and "No Deal". If CC = 1, the contestant is risk neutral. When CC > 1, the contestant is risk seeking, and as CC approaches zero, the contestant becomes extremely risk averse. To help interpret the goodness of the model, we have added the "hit percentage", or the percentage of correctly predicted "Deal or No Deal" decisions.

In the Dutch sample, the risk aversion parameters α and β are both significantly different from zero, suggesting that IRRA and DARA are relevant and the classical CRRA power function and CARA exponential function are too restrictive to explain the choices in this game show. The estimated wealth level of €75,203 significantly exceeds zero. Still, given that the median Dutch household income is roughly €25,000 per annum, the initial wealth level seems substantially lower than lifetime wealth and integration seems incomplete. This deviates from the classical approach of defining utility over wealth and is more in line with utility of income or the type of narrow framing that is typically assumed in prospect theory. A low wealth estimate is also consistent with Rabin's (2000) observation that plausible risk aversion for small and medium outcomes implies implausibly strong risk aversion for large outcomes if the outcomes are integrated with lifetime wealth. Indeed, the estimates imply near risk neutrality for small stakes, witness the CC of 0.994 for a 50/50 gamble of €0 or €1,000, and increasing the wealth level would imply near risk neutrality for even larger gambles.

Rabin's point is reinforced by comparing our results for large stakes with the laboratory experiments conducted by Holt and Laury (2002) using the lower stakes typical in the lab. Holt and Laury's subjects display significant risk aversion for modest stakes, which, as Rabin notes, implies extreme risk aversion for much larger stakes – behavior our contestants do not display. Indeed, contestants with Holt and Laury's parameter estimates for the utility function would generally accept a "Deal" in the first game round, in contrast to the actual behavior we observe. We conclude, agreeing with Rabin, that expected utility of wealth models have difficulty explaining behavior for both small and large stakes.

The model also does not seem flexible enough to explain the choices for losers and winners simultaneously. The estimated utility function exhibits very strong IRRA, leading to an implausibly low CC of 0.141 for a 50/50 gamble of €0 or €1,000,000.

Table 1.6: Expected utility theory results. The table displays the estimation results of expected utility theory for our sample of 151 contestants from The Netherlands (51), Germany (47) and the United States (53). Shown are maximum likelihood estimators for the α and β parameters and the wealth level (W, in Euros) of the utility function (3), and the noise parameter σ. The table also shows the overall mean log-likelihood (MLL), the likelihood ratio (LR) relative to the naïve model of risk neutrality, the percentage of correctly predicted "Deal or No Deal" decisions (Hits), and the total number of "Deal or No Deal" decisions in the sample (No.). Finally, the implied certainty coefficient (CC; certainty equivalent as a fraction of the expected value) is shown for 50/50 gambles of €0 or €10^z, $z = 1,\cdots,6$. p-values are shown in parentheses.

	Netherlands		Germany		United States	
α	0.424	(0.000)	1.58e-5	(0.049)	4.18e-5	(0.000)
β	0.791	(0.000)	0.000	(1.000)	0.171	(0.000)
W	75,203	(0.034)	544	(0.481)	101,898	(0.782)
σ	0.428	(0.000)	0.467	(0.000)	0.277	(0.000)
MLL	-0.365		-0.340		-0.260	
LR	24.29	(0.000)	3.95	(0.267)	15.10	(0.002)
Hits	76%		85%		89%	
No.	214		327		349	
CC ($0/10^1$)	1.000		1.000		1.000	
CC ($0/10^2$)	0.999		1.000		1.000	
CC ($0/10^3$)	0.994		0.996		0.998	
CC ($0/10^4$)	0.946		0.960		0.984	
CC ($0/10^5$)	0.637		0.640		0.859	
CC ($0/10^6$)	0.141		0.088		0.302	

Indeed, the model errs by predicting that winners would stop earlier than they actually do. If risk aversion increases with stakes, winners are predicted to have a stronger propensity to accept a bank offer, the opposite of what we observe; witness for example the "Deal" percentages in Table 1.5. However, strong IRRA is needed in order to explain the behavior of losers, who reject generous bank offers and continue play even with tens of thousands of Euros at stake. Still, the model does not predict risk seeking at small stakes; witness the CC of 0.946 for a 50/50 gamble of €0 or €10,000 – roughly Frank's risky choice in round 9. Thus, the model also errs by predicting that losers would stop earlier than they actually do.

Interestingly, the estimated coefficients for the German edition are quite different from the Dutch values. The optimal utility function reduces to the CARA exponential function ($\beta = 0$) and the estimated initial wealth level becomes insignificantly different from zero. Still, on the observed domain of prizes, the two utility functions exhibit a similar pattern of unreasonably strong IRRA and high risk aversion for winners. Again, the model errs by predicting that losers and winners would stop earlier than they actually do. These errors are so substantial in this edition that the fit of the expected utility model is not significantly better than the fit of a naive model that assumes that all contestants are risk neutral and simply "Deal" whenever the bank offer exceeds the average remaining prize.

Contrary to the Dutch and German utility functions, the US utility function approximates the limiting case of the CRRA power function ($\alpha \approx 0$). The CC is again very high for small stakes. For larger stakes, the coefficient decreases but at a slower pace than in the other two countries, reflecting the relatively low propensity to "Deal" for US contestants with relatively large amounts at stake. The decreasing pattern stems from the estimated initial wealth level of €101,898, which yields near risk neutrality for small stakes. Still, initial wealth is not significantly different from zero, because a similar pattern can be obtained if we lower the value of beta relative to alpha and move in the direction of the CARA exponential function.

To further illustrate the effect of prior outcomes, Table 1.7 shows separate results for losers and winners (as defined in Section 1.4). Confirming the low "Deal" percentages found earlier, the losers and winners are less risk averse and have higher CCs than the neutral group. The losers are in fact best described by a model of risk seeking, which is not surprising given that the losers in our sample often reject bank offers in excess of the average remaining prize. The same pattern arises in each of the three editions, despite sizeable differences in the set of prizes. For example, the Dutch losers on average face larger stakes than the contestants in the US and German neutral groups. Still, risk seeking (CC > 1) arises only in the loser group. Overall, these results suggest that the expected utility model fails to capture the strong effect of previous outcomes.

1.6 Prospect Theory

In this section, we use prospect theory to analyze the observed "Deal or No Deal" choices. Contestants are assumed to have a narrow focus and evaluate the outcomes in the game without integrating their initial wealth – a typical assumption in prospect theory. Furthermore, we will again use the myopic frame that compares the current bank offer with the unknown offer in the next round. Although myopia is commonly assumed

Table 1.7: Path dependence. The table shows the maximum likelihood estimation results of expected utility theory for our sample of 151 contestants from the Netherlands (51; panel A), Germany (47; panel B) and the United States (53; panel C). The samples are split based on the fortune experienced during the game. A contestant is classified as a "loser" ("winner") if her average remaining prize after eliminating the lowest (highest) remaining prize is among the worst (best) one-third for all contestants in the same game round. The results are presented in a format similar to the full-sample results in Table 1.6.

	Loser		Neutral		Winner	
A. Netherlands						
α	-244.904	(0.022)	0.044	(0.204)	0.125	(0.831)
β	0.993	(0.000)	0.687	(0.000)	0.736	(0.011)
W	0	(1.000)	304	(0.671)	3,061	(0.824)
σ	0.627	(0.000)	0.323	(0.000)	0.309	(0.000)
MLL	-0.300		-0.383		-0.325	
Hits	89%		81%		83%	
No.	72		70		72	
CC ($0/10^1$)	1.330		0.994		0.999	
CC ($0/10^2$)	1.338		0.945		0.992	
CC ($0/10^3$)	1.347		0.723		0.928	
CC ($0/10^4$)	1.355		0.392		0.630	
CC ($0/10^5$)	1.363		0.150		0.216	
CC ($0/10^6$)	1.371		0.032		0.035	
B. Germany						
α	-7.914	(0.117)	0.364	(0.000)	0.087	(0.000)
β	0.814	(0.000)	0.759	(0.000)	0.651	(0.000)
W	930	(0.825)	50,926	(0.481)	113,582	(0.180)
σ	0.659	(0.000)	0.241	(0.000)	0.454	(0.000)
MLL	-0.276		-0.257		-0.278	
Hits	90%		87%		88%	
No.	111		105		111	
CC ($0/10^1$)	1.012		1.000		1.000	
CC ($0/10^2$)	1.113		0.999		0.999	
CC ($0/10^3$)	1.584		0.990		0.995	
CC ($0/10^4$)	1.823		0.911		0.949	
CC ($0/10^5$)	1.891		0.485		0.614	
CC ($0/10^6$)	1.929		0.072		0.101	
C. United States						
α	-203.512	(0.006)	1.96e-5	(0.000)	0.938	(0.000)
β	0.995	(0.000)	0.086	(0.000)	0.998	(0.000)
W	54	(0.691)	934,904	(0.331)	29,468	(0.107)
σ	0.193	(0.000)	0.308	(0.000)	0.326	(0.000)
MLL	-0.194		-0.275		-0.253	
Hits	92%		86%		91%	
No.	118		113		118	
CC ($0/10^1$)	1.004		1.000		1.000	
CC ($0/10^2$)	1.023		1.000		0.999	
CC ($0/10^3$)	1.054		0.999		0.992	
CC ($0/10^4$)	1.071		0.986		0.927	
CC ($0/10^5$)	1.081		0.863		0.646	
CC ($0/10^6$)	1.089		0.252		0.289	

in prospect theory, the choice of the relevant frame in this game is actually more important than for expected utility theory. As discussed in Section 1.5, the myopic frame seems appropriate for expected utility theory. For prospect theory, however, it can be rather restrictive. Prospect theory allows for risk-seeking behavior when in the domain of losses and risk seekers have a strong incentive to look ahead multiple game rounds to allow for the possibility of winning the largest remaining prize. Indeed, contestants who reject high bank offers often explicitly state that they are playing for the largest remaining prize (rather than a large amount offered by the banker offer in the next round). Preliminary computations revealed that prospect theory generally performs better if we allow contestants to look ahead multiple game rounds. The improvements are limited, however, because risk seeking typically arises at the end of the game. At that stage, only a few or no further game rounds remain and the myopic model then gives a good approximation. Thus, we report only the results with the myopic model in order to be consistent with the previous analysis using expected utility theory.

The stop value and continuation value for prospect theory are defined in the same way as for expected utility theory, with the only difference that the expo-power utility function (1.3) is replaced by the prospect theory value function, which is defined on changes relative to some reference point:

(1.12) $$v(x \mid RP) = \begin{cases} -\lambda(RP - x)^{\alpha} & x \le RP \\ (x - RP)^{\alpha} & x > RP \end{cases}$$

where $\lambda > 0$ is the loss-aversion parameter, RP is the reference point that separates losses from gains, and $\alpha > 0$ measures the curvature of the value function. The original formulation of prospect theory allows for different curvature parameters for the domain of losses ($x \le RP$) and the domain of gains ($x > RP$). To reduce the number of free parameters, we assume here that the curvature is equal for both domains.[6]

[6] Empirical curvature estimates are often very similar for gains and losses. Tversky and Kahneman (1992), for example, find a median value of 0.88 for both domains. Furthermore, the curvature needs to be the same for both domains in order to be consistent with the definition of loss aversion; see Köbberling and Wakker (2005).

Reference Point Specification

Kahneman and Tversky's (1979) original treatment of prospect theory equates the reference point with the status quo. Since "Deal or No Deal" contestants never have to pay money out of their own pockets, the reference point would then equal zero and contestants would never experience any losses. Kahneman and Tversky recognize, however, that "there are situations in which gains and losses are coded relative to an expectation or aspiration level that differs from the status quo" (286). They point out that "a person who has not made peace with his losses is likely to accept gambles that would be unacceptable to him otherwise" (287). This point is elaborated by Thaler and Johnson (1990), though neither team offers a formal model of how the reference point changes over time. One recent effort along these lines is by Kőszegi and Rabin (2006, 2007).

The specification of the subjective reference point (or the underlying set of expectations) and how it varies during the game is crucial for our analysis, as it determines whether outcomes enter as gain or loss in the value function and with what magnitude. Slow adjustment or stickiness of the reference point can yield break-even and house-money effects, or a lower risk aversion after losses and after gains. If the reference point adjusts slowly after losses, relatively many remaining outcomes are placed in the domain of losses, where risk seeking applies. Similarly, if the reference point sticks to an earlier, less favorable value after gains, relatively many remaining prizes are placed in the domain of gains, reducing the role of loss aversion.

Figure 1.4 illustrates these two effects using a 50/50 gamble of €25,000 or €75,000. Contestants in "Deal or No Deal" face this type of gamble in round 9. The figure shows the value function using the parameter estimates of Tversky and Kahneman (1992), or $\alpha = 0.88$ and $\lambda = 2.25$, and three alternative specifications for the reference point. In a neutral situation without prior outcomes, the reference point may equal the expected value (RP_N = €50,000). In this case, the contestant frames the gamble as losing €25,000 (€50,000 − €25,000) or winning €25,000 (€75,000 − €50,000). The certainty equivalent of the gamble is CE_N = €44,169, meaning that bank offers below this level would be rejected and higher offers would be accepted. The risk premium of €5,831 is caused by loss aversion, which assigns a larger weight to losses than to gains.

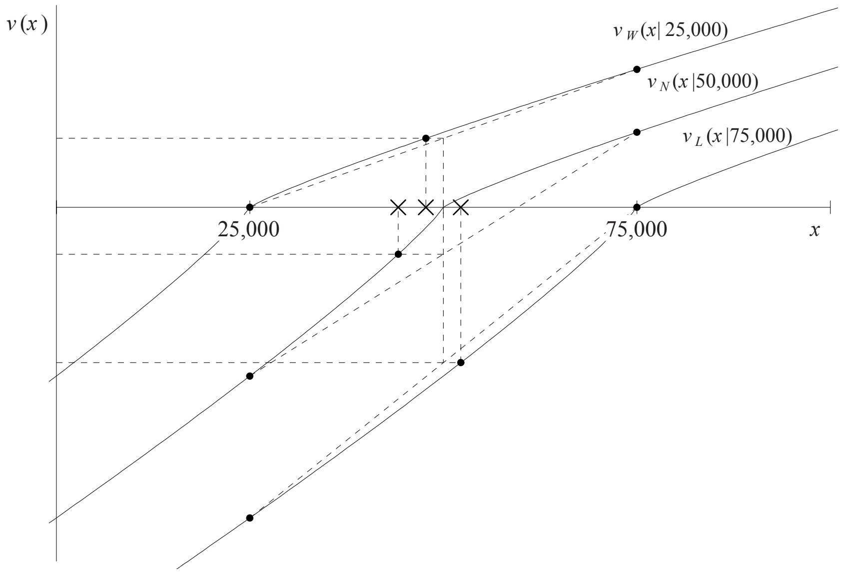

Figure 1.4: Break-even and house-money effects in prospect theory. The figure displays the prospect value function (1.12) for three different levels of the reference point (*RP*) and the associated certainty equivalents (*CE*s) for a 50/50 gamble of €25,000 or €75,000. Value function $v_N(x|50{,}000)$ refers to a neutral situation with RP_N = €50,000 and CE_N = €44,169, $v_W(x|25{,}000)$ to a winner with RP_W = €25,000 and CE_W = €47,745, and $v_L(x|75{,}000)$ to a loser with RP_L = €75,000 and CE_L = €52,255. All three value functions are based on the parameter estimates of Tversky and Kahneman (1992), or $\alpha = 0.88$ and $\lambda = 2.25$. The crosses indicate the certainty equivalents for the 50/50 gamble.

Now consider contestant L, who initially faced much larger stakes than €50,000 and incurred large losses before arriving at the 50/50 gamble in round 9. Suppose that L slowly adjusts to these earlier losses and places his reference point at the largest remaining prize (RP_L = €75,000). In this case, L does not frame the gamble as losing €25,000 or winning €25,000 but rather as losing €50,000 (€75,000 – €25,000) or breaking even (€75,000 – €75,000). Both prizes are placed in the domain of losses where risk seeking applies. Indeed, L would reject all bank offers below the certainty equivalent of the gamble, CE_L = €52,255, which implies a negative risk premium of €2,255.

Finally, consider contestant W, who initially faced much smaller stakes than €50,000 and incurred large gains before arriving at the 50/50 gamble. Due to slow adjustment, W employs a reference point equal to the smallest remaining prize (RP_W = €25,000) and places both remaining prizes in the domain of gains. In this case, W frames the gamble as one of either breaking even (€25,000 – €25,000) or gaining €50,000 (€75,000 – €25,000). Since loss aversion does not apply in the domain of gains, the risk aversion of W is lower than in the neutral case and W would reject all bank offers below CE_W = €47,745, implying a risk premium of €2,255, less than the value of €5,831 in the neutral case.

It should be clear from the examples above that a proper specification of the reference point and its dynamics is essential for our analysis. In fact, without slow adjustment, prospect theory does not yield the path-dependence found in this study. Unfortunately, the reference point is not directly observable and prospect theory alone provides minimal guidance for selecting the relevant specification. We therefore need to give the model some freedom and rely on the data to inform us about the relevant specification. To reduce the risk of data mining and to simplify the interpretation of the results, we develop a simple structural model based on elementary assumptions and restrictions for the reference point.

If contestants were confronted with the isolated problem of choosing between the current bank offer and the risky bank offer in the next round, it would seem natural to link the reference point to the current bank offer. The bank offer represents the sure alternative and the opportunity cost of the risky alternative. Furthermore, the bank offer is linked to the average remaining prize and therefore to current expectations regarding future outcomes. A simple specification would be $RP_r = \theta_1 B(x_r)$. If $\theta_1 = 0$, then the reference point equals the status quo ($RP_r = 0$) and all possible outcomes are evaluated as gains; if $\theta_1 > 0$, the reference point is strictly positive and contestant may experience (paper) losses, even though they never have to pay money out of their own pockets. A reference point below the current bank offer, or $\theta_1 < 1$, is conservative (pessimistic) in the sense that relatively few possible bank offers in the next round are classified as losses and relatively many possible outcomes are classified as gains. By contrast, an "optimistic" reference point, or $\theta_1 > 1$, involves relatively many possible losses and few possible gains.

The actual game is dynamic and the bank offer changes in every round, introducing the need to update the reference point. Due to slow adjustment, however, the reference point may be affected by earlier game situations. We may measure the effect of outcomes after earlier round j, $0 \leq j < r$, by the relative increase in the average remaining prize, or $d_r^{(j)} = (\bar{x}_r - \bar{x}_j)/\bar{x}_r$. For $j = 0$, $d_r^{(j)}$ measures the change relative to the initial average, or $\bar{x}_0$.

Ideally, our model would include this measure for all earlier game rounds (and possibly also interaction terms). However, due to the strong correlation between the lagged terms and the limited number of observations, we have to limit the number of free parameters. We restrict ourselves to just two terms: $d_r^{(r-2)}$ and $d_r^{(0)}$. The term $d_r^{(r-2)}$ is the longest fixed lag that can be included for all observations (our analysis starts in the second round) and measures recent changes; $d_r^{(0)}$, or the longest variable lag, captures all changes relative to the initial game situation. Adding these two lagged terms to the static model, our dynamic model for the reference point is:

$$RP_r = (\theta_1 + \theta_2 d_r^{(r-2)} + \theta_3 d_r^{(0)}) B(x_r) \tag{1.13}$$

In this model, $\theta_2 < 0$ or $\theta_3 < 0$ implies that the reference points sticks to earlier values and that it is higher than the neutral value $\theta_1 B(x_r)$ after decreases in the average remaining prize and lower after increases.

It is not immediately clear how strong the adjustment would be, or if the adjustment parameters would be constant, but it seems realistic to assume that the adjustment is always sufficiently strong to ensure that the reference point is feasible in the next round, i.e., not lower than the smallest possible bank offer and not higher than the largest possible bank offer. We therefore truncate the reference point at the minimum and maximum bank offer, i.e. $\min_{y \in X(x_r)} B(y) \leq RP_r \leq \max_{y \in X(x_r)} B(y)$. This truncation improves the empirical fit of our model and the robustness to the specification of the reference point and its dynamics.

Our complete prospect theory model involves five free parameters: loss aversion λ, curvature α, and the three parameters of the reference point model θ_1, θ_2 and θ_3. We estimate these parameters and the noise parameter σ with the same maximum likelihood procedure used for the expected utility analysis. We also apply the same bank offer model.

Our analysis ignores subjective probability transformation and uses the true probabilities as decision weights. The fit of prospect theory could improve if we allow for probability transformation. If losers have a sticky reference point and treat all possible outcomes as losses, they will overweight the probability of the smallest possible loss, strengthening the risk seeking that stems from the convexity of the value function in the domain of losses. For example, applying the Tversky and Kahneman (1992) weighting function and parameter estimates to a gamble with two equally likely losses, the decision weight of the smallest loss is 55 percent rather than 50 percent. Still, we prefer to focus on the effect of the reference point in this study and we ignore probability weighting for the sake of parsimony. This simplification is unlikely to be material, especially in the most important later rounds, when the relevant probabilities are medium to large and the decision weights would be relatively close to the actual probabilities (as illustrated by the 50/50 gamble).

Results

Table 1.8 summarizes our results. For the Dutch edition, the curvature and loss aversion parameters are significantly different from unity. The curvature of the value function is needed to explain why some contestants reject bank offers in excess of the average remaining prize; loss aversion explains why the average contestant accepts a bank offer below the average prize. Both parameters take values that are comparable with the typical results in experimental studies. Indeed, setting these parameters equal to the Tversky and Kahneman (1992) parameter values does not change our conclusions.

The parameter θ_1 is significantly larger than zero, implying that contestants do experience (paper) losses, consistent with the idea that the reference point is based on expectations and that diminished expectations represent losses. The parameter is also significantly smaller than unity, indicating that the reference point generally takes a conservative value below the current bank offer.

The adjustment parameters θ_2 and θ_3 are significantly smaller than zero, meaning that the reference point tends to stick to earlier values and is higher than the neutral

Table 1.8: Prospect theory results. The table shows the estimation results of prospect theory for our sample of 151 contestants from The Netherlands (51), Germany (47) and the United States (53). Shown are maximum likelihood estimators for the loss aversion (λ) and curvature (α) of the value function, the three parameters of the reference point model θ_1, θ_2 and θ_3, and the noise parameter σ. The table also shows the overall mean log-likelihood (MLL), the likelihood ratio (LR) relative to the naïve model of risk neutrality, the percentage of correctly predicted "Deal or No Deal" decisions (Hits), and the total number of "Deal or No Deal" decisions in the sample (No.). p-values are shown in parentheses.

	Netherlands		Germany		United States	
λ	2.375	(0.013)	4.501	(0.008)	4.528	(0.001)
α	0.516	(0.000)	0.486	(0.000)	0.836	(0.000)
θ_1	0.474	(0.000)	1.096	(0.000)	1.163	(0.000)
θ_2	-0.285	(0.000)	-0.026	(0.000)	0.031	(0.329)
θ_3	-0.028	(0.000)	-0.052	(0.000)	-0.093	(0.023)
σ	0.345	(0.000)	0.533	(0.000)	0.193	(0.000)
MLL	-0.309		-0.303		-0.228	
LR	48.41	(0.000)	27.44	(0.000)	37.28	(0.000)
Hits	85%		89%		91%	
No.	214		327		349	

value after losses and lower after gains. In magnitude, θ_2 is much larger than θ_3, suggesting that the effect of recent outcomes is much stronger than the effect of initial expectations. However, the changes in the average remaining prize during the last two game rounds are generally much smaller than the changes during the entire game, limiting the effect of the parameter value. In addition, in case of large changes, the reference point often falls outside the range of feasible outcomes. In these cases, the reference point is set equal to the smallest or largest possible bank offer (see above), further limiting the effect of the parameter value.

The slow adjustment of the reference point lowers the propensity of losers and winners to "Deal". Not surprisingly, the prospect theory model yields substantially smaller errors for losers and winners and the overall log-likelihood is significantly higher than for the expected utility model. While the expected utility model correctly predicted 76 percent of the "Deal or No Deal" decisions, the hit percentage of the prospect theory model is 85 percent.

The results for the German and US samples are somewhat different from the results for the Dutch sample, but still confirm the important role of slow adjustment. The difference seems related to the relatively large stakes and the associated high propensity to "Deal" in the Dutch edition. In the German and US samples, the reference point is substantially higher in relative terms than in the Dutch sample. The relatively high reference point helps explain why the German and US contestants stop in later rounds and demand higher percentage bank offers than the Dutch contestants. Relatively many outcomes are placed in the domain of losses, where risk seeking applies. In such a situation, a relatively strong loss aversion is needed to explain "Deals". Indeed, the loss aversion estimates are substantially higher than for the Dutch sample. Again, stickiness is highly significant. However, the most recent outcomes seem less important and the reference point now sticks primarily to the initial situation. This seems related to the German and US contestants on average playing more game rounds than the Dutch contestants. In later rounds, many briefcases have already been opened, but relatively few briefcases have been opened in the last few rounds. The last two game rounds played in the German and US edition therefore generally reveal less information than in the Dutch edition. The model again materially reduces the errors for losers and winners and fits the data significantly better than the expected utility model in these two samples.

The results are consistent with our earlier finding that the losers and winners have a low propensity to "Deal" (see Table 1.5). Clearly, prospect theory with a dynamic but sticky reference point is a plausible explanation for this path-dependent pattern. Still, we stress that our analysis of prospect theory serves merely to explore and illustrate one possible explanation, and that it leaves several questions unanswered. For example, we have assumed homogeneous preferences and no subjective probability transformation. The empirical fit may improve even further if we would allow for heterogeneous preferences and probability weighting. Further improvements may come from allowing for a different curvature in the domains of losses and gains, from allowing for different partial adjustment after gains and losses, and from stakes-dependent curvature and loss aversion. We leave these issues for further research.

1.7 Experiments

The previous sections have demonstrated the strong effect of prior outcomes or path-dependence of risk attitudes. Also, the amounts at stake seem to be important, with a stronger propensity to deal for larger stakes levels. Prior outcomes and stakes are, however, highly confounded within every edition of the game show: unfavorable outcomes (opening high-value briefcases) lower the stakes and favorable outcomes (opening low-value briefcases) raise the stakes. The stronger the effect of stakes, the easier it is to explain the weak propensity to "Deal" of losers, but the more difficult it is to explain the low "Deal" percentage of winners. To analyze the isolated effect of the amounts at stake, we conduct a series of classroom experiments in which students at Erasmus University play "Deal or No Deal". We consider two variations to the same experiment that use monetary amounts that differ by a factor of ten, but draw from the same student population.

Both experiments use real monetary payoffs to avoid incentive problems (see, for example, Holt and Laury, 2002). In order to compare the choices in the experiments with those in the original TV show and to provide a common basis for comparisons between the two experiments, each experiment uses the original scenarios from the Dutch edition.[7] At the time of the experiments, only the first 40 episodes are available. The original monetary amounts are scaled down by a factor of 1,000 or 10,000, with the smallest amounts rounded up to one cent. Despite the strong scaling, the resulting stakes are still unusually high for experimental research. Although the scenarios are predetermined, the subjects are not "deceived" in the sense that the game is not manipulated to encourage or avoid particular situations or behaviors. Rather, the subjects are randomly assigned to a scenario generated by chance at an earlier point in time (in the original episode). The risk that the students would recognize the original episodes seems small, because the scenarios are not easy to remember and the original episodes are broadcast at least six months earlier. Indeed, the experimental "Deal or No Deal" decisions are statistically unrelated to which of the remaining prizes is in the contestant's own briefcase.

We replicate the original game show as closely as possible in a classroom, using a game show host (a popular lecturer at Erasmus University) and live audience (the student subjects and our research team). Video cameras are pointed at the contestant, recording all her actions. The game situation (unopened briefcases, remaining prizes and bank offers) is displayed on a computer monitor in front of the stage (for the host and the contestant) and projected on a large screen in front of the classroom (for the audience). This setup is intended to create the type of distress that contestants must experience in the TV studio. Our approach seems effective, because the audience is very

[7] Original prizes and offers are not available when a subject continues play after a "Deal" in the TV episode. The "missing outcomes" for the prizes are selected randomly (but held constant across the experiments), and the bank offers are set according to the pattern observed in the original episodes.

excited and enthusiastic during the experiment, applauding and shouting hints, and most contestants show clear symptoms of distress.

All our subjects are students, about 20 years of age. A total of 160 business or economics students are randomly selected from a larger population of students at Erasmus University who applied to participate in experiments during the academic year 2005 – 2006. Although each experiment requires only 40 subjects, 80 students are invited to guarantee a large audience and to ensure that a sufficient number of subjects are available in the event that some subjects do not show up. Thus, approximately half of the students are selected to play the game. To control for a possible gender effect, we ensure that the gender of the subjects matches the gender of the contestants in the original episodes.

At the beginning of both experiments we hand out the instructions to each subject, consisting of the original instructions to contestants in the TV show plus a cover sheet explaining our experiment. Next, the games start. Each individual game lasts about 5 to 10 minutes, and each experiment (40 games) lasts roughly 5 hours, equally divided in an afternoon session with one half of the subjects and games, and an evening session with the other half.

Small-Stake Experiment

In the first experiment, the original prizes and bank offers from the Dutch edition are divided by 10,000, resulting in an average prize of roughly €40 and a maximum prize of €500.

The overall level of risk aversion in this experiment is lower than in the original TV show. Contestants on average stop later (round 6.9 versus 5.2 for the TV show) and reject higher percentage bank offers. Still, the changes seem modest given that the initial stakes are 10,000 times smaller than in the TV show. In the TV show, contestants generally become risk neutral or risk seeking when "only" thousands or tens of thousands of Euros remain at stake. In the experiment, the stakes are much smaller, but the average contestant is clearly risk averse. This suggests that the effect of stakes on risk attitudes in this game is relatively weak. By contrast, the effect of prior outcomes is very strong; witness for example the (untabulated) "Deal" percentages (for round 2 – 9 combined) of 3, 21 and 19 for "loser", "neutral" and "winner", respectively.

The first column of Table 1.9 shows the maximum likelihood estimation results. The estimated utility function exhibits the same pattern of extreme IRRA as for the original shows, but now at a much smaller scale. See, for example, the CC of 0.072 for a 50/50 gamble of €0 or €1,000. It follows from Rabin's (2000) observation that plausible levels of risk aversion require much lower initial wealth levels for small-stake gambles than for large-stake gambles. Indeed, initial wealth is estimated to be €11 in this experiment, roughly a factor of 10,000 lower than for the original TV sample. As for the original episodes, the model errs by predicting that the losers and winners would stop earlier than they actually do. Prospect theory with a sticky reference point fits the data substantially better than the expected utility model, both in terms of the log-likelihood and in terms of the hit percentage.

Large-Stake Experiment

The modest change in the choices in the first experiment relative to the large-stake TV show suggests that the effect of stakes is limited in this game. Of course, the classroom experiment is not directly comparable with the TV version, because, for example, the experiment is not broadcast on TV and uses a different type of contestant (students). Our second experiment therefore investigates the effect of stakes by replicating the first experiment with larger stakes.

The experiment uses the same design as before, with the only difference being that the original monetary amounts are divided by 1,000 rather than by 10,000, resulting in an average prize of roughly €400 and a maximum prize of €5,000 – extraordinarily large amounts for experiments. For this experiment, 80 new subjects were drawn from the same population, excluding students involved in the first experiment.

Based on the strong IRRA in the first experiment, the expected utility model would predict a much higher risk aversion in this experiment. However, the average stop round is exactly equal to the average for the small-stake experiment (round 6.9), and subjects reject similar percentage bank offers (the highest rejected bank offer averages 82.5 percent versus 82.4 percent for the small-stake experiment). Therefore, the isolated effect of stakes seems much weaker than suggested by the estimated IRRA in the individual experiments.

Table 1.9: Experimental results. The table shows the maximum likelihood estimation results for our choice experiments. The first column (Small stakes) displays the results for the experiment with the original monetary amounts in the Dutch TV format of "Deal or No Deal" divided by 10,000, the second column (Large stakes) displays the results for the experiment with prizes scaled down by a factor of 1,000, and the third column (Pooled) displays the results for the two samples combined. Panel A shows the results for expected utility theory. Panel B shows the results for prospect theory. The results are presented in the same format as the results in Table 1.6 and Table 1.8, respectively.

	Small stakes		Large stakes		Pooled	
A. Expected utility theory						
α	0.019	(0.000)	0.002	(0.001)	0.002	(0.001)
β	0.000	(1.000)	0.000	(1.000)	0.000	(1.000)
W	11	(0.920)	50	(0.930)	0	(1.000)
σ	0.306	(0.000)	0.294	(0.000)	0.354	(0.000)
MLL	-0.342		-0.337		-0.351	
LR	10.17	(0.017)	10.14	(0.017)	9.37	(0.025)
Hits	81%		83%		80%	
No.	231		234		465	
CC ($0/10^1$)	0.953		0.995		0.995	
CC ($0/10^2$)	0.583		0.953		0.953	
CC ($0/10^3$)	0.072		0.588		0.586	
CC ($0/10^4$)	0.007		0.074		0.074	
CC ($0/10^5$)	0.001		0.007		0.007	
CC ($0/10^6$)	0.000		0.001		0.001	
B. Prospect theory						
λ	2.307	(0.000)	2.678	(0.000)	2.518	(0.000)
α	0.732	(0.000)	0.695	(0.000)	0.693	(0.000)
θ_1	1.045	(0.000)	1.024	(0.000)	1.023	(0.000)
θ_2	-0.119	(0.000)	0.019	(0.000)	0.013	(0.250)
θ_3	-0.086	(0.000)	-0.046	(0.000)	-0.049	(0.000)
σ	0.267	(0.000)	0.196	(0.000)	0.218	(0.000)
MLL	-0.275		-0.265		-0.272	
LR	40.94	(0.000)	44.04	(0.000)	83.29	(0.000)
Hits	87%		88%		87%	
No.	231		234		465	

The second column of Table 1.9 displays the maximum likelihood estimation results. With increased stakes but similar choices, the expected utility model needs a different utility function to rationalize the choices. In fact, the estimated utility function seems scaled in proportion to the stakes, so that the 50/50 gamble of €0 or €1,000 now involves approximately the same CC as the 50/50 gamble of €0 or €100 in the small-stake experiment. By contrast, for prospect theory, the estimated parameters are roughly the same as for the small-stake version and a substantially better fit is achieved relative to the implementation of expected utility theory.

In both experiments, risk aversion is strongly affected by prior outcomes, which are strongly related to the level of stakes *within* the experiments, but the stakes do not materially affect risk aversion *across* the experiments. Since the stakes are increased by a factor of ten and all other conditions are held constant, the only plausible explanation seems that prior outcomes rather than stakes are the main driver of risk aversion in this game.

Pooled Sample

The last column of Table 1.9 shows the results for the pooled sample of the two experiments. As noted above, the choice behavior in the two samples is very similar, despite the large differences in the stakes. The important role of the stakes in the individual samples and the weak role across the two samples lead to two very different utility functions. Stakes appear to matter more in relative terms than in absolute terms. Combining both samples will cause problems for the expected utility model, since the model assigns an important role to the absolute level of stakes. Using a single utility function for the pooled sample indeed significantly worsens the fit relative to the individual samples. The prospect theory model does not suffer from this problem because it attributes the low “Deal” propensity of losers and winners in each sample to the slow adjustment of a reference point that is proportional to the stakes in each sample. In this way, the model relies on changes in the relative level of the stakes rather than the absolute level of the stakes. Whether outcomes are gains or losses depends on the context. An amount of €100 is likely to be placed in the domain of gains in the small-stake experiment (where the average prize is roughly €40), but the same amount is probably placed in the domain of losses in the large-stake experiment (with an average prize of roughly €400).

1.8 Conclusions

The behavior of contestants in game shows cannot always be generalized to what an ordinary person does in her everyday life when making risky decisions. While the contestants have to make decisions in just a few minutes in front of millions of viewers, many real-life decisions involving large sums of money are neither made in a hurry nor in the limelight. Still, we believe that the choices in this particular game show are worthy of study, because the decision problems are simple and well-defined, and the amounts at stake are very large. Furthermore, prior to the show, contestants have had considerable time to think about what they might do in various situations, and during the show they are encouraged to discuss those contingencies with a friend or relative who sits in the audience. In this sense, the choices may be more deliberate and considered than might appear at first glance. Indeed, it seems plausible that our contestants have given more thought to their choices on the show than to some of the other financial choices they have made in their lives such as selecting a mortgage or retirement savings investment strategy.

What does our analysis tell us? First, we observe, on average, what might be called "moderate" levels of risk aversion. Even when hundreds of thousands of Euros are at stake, many contestants are rejecting offers in excess of 75 percent of the expected value. In an expected utility of wealth framework, this level of risk aversion for large stakes is hard to reconcile with the same moderate level of risk aversion found in small-stake experiments – both ours, and those conducted by other experimentalists. Second, although risk aversion is moderate on average, the offers people accept vary greatly among the contestants; some demonstrate strong risk aversion by stopping in the early game rounds and accepting relatively conservative bank offers, while others exhibit clear risk-seeking behavior by rejecting offers above the average remaining prize and thus deliberately entering "unfair gambles". While some of this variation is undoubtedly due to differences in individual risk attitudes, a considerable part of the variation can be explained by the outcomes experienced by the contestants in the previous rounds of the game. Most notably, risk aversion generally decreases after prior expectations have been shattered by eliminating high-value briefcases or after earlier expectations have been surpassed by opening low-value briefcases. This path-dependent pattern occurs in all three editions of the game, despite sizeable differences in the initial stakes across the editions. "Losers" and "winners" generally have a weaker propensity to "Deal" than their "neutral" counterparts.

The relatively low risk aversion of losers and winners is hard to explain with expected utility theory and points in the direction of reference-dependent choice theories such as prospect theory. Indeed, our findings seem consistent with the break-even effect (losers becoming more willing to take risk due to incomplete adaptation to prior losses), and the house-money effect (a low risk aversion for winners due to incomplete adaptation to prior gains). A simple version of prospect theory with a sticky reference point explains the "Deal or No Deal" decisions substantially better than expected utility theory. These findings suggest that reference-dependence and path-dependence are important, even when the decision problems are simple and well-defined, and when large real monetary amounts are at stake.

Of course, we must be careful with rejecting expected utility theory and embracing prospect theory. We use the flexible expo-power utility function, which embeds the most popular implementations of expected utility theory, and find that this function is unable to provide an explanation for the choices of losers and winners in this game show. However, a (nonstandard) utility function that has risk seeking segments and depends on prior outcomes could achieve a better fit. Such exotic specifications blur the boundary between the two theories, and we therefore do not reject or accept one of the two.

Our main finding is the important role of reference-dependence and path-dependence, phenomena that are often ignored in implementations of expected utility theory. Previous choice problems are a key determinant of the framing of a given choice problem. An amount is likely to be considered as "large" in the context of a game where it lies above prior expectations, but the same amount is probably evaluated as "small" in a game where it lies below prior expectations. For contestants who expected to win hundreds of thousands, an amount of €10,000 probably seems "small"; the same amount is likely to appear much "larger" when thousands or tens of thousands were expected.

To isolate the effect of the amounts at stake, we conducted two series of choice experiments that use a homogeneous student population and mimic the TV show as closely as possible in a classroom. We find that a tenfold increase of the initial stakes does not materially affect the choices. Moreover, the choices in the experiments are remarkably similar to those in the original TV show, despite the fact that the experimental stakes are only a small fraction of the original stakes. Consistent with the TV version, the break-even effect and the house-money effect also emerge in the experiments. These experimental findings reinforce our conclusion that choices are strongly affected by previous outcomes. The combination of (i) a strong effect of variation in stakes caused by a subject's fortune within a game and (ii) a weak effect of variation in the initial stakes across games calls for a choice model that properly accounts for the context of the choice problem and its dynamics.

This study has focused on episodes from the Netherlands, Germany, and the United States, because these episodes have a very similar game format. For further research, it would be interesting to collect more international data in order to obtain more degrees of freedom to analyze the effect of prior outcomes in greater detail and to examine the role of the cultural, social or economic background of the contestant. It would also be interesting to further extend our choice experiments. While the stakes are much smaller, experiments do allow the researcher to control contestant characteristics, rules and situations, and to more closely monitor contestants and their behavior. Our experiments were designed to mimic the TV studio and used real monetary payoffs, but further experiments may also take place in the behavioral laboratory and employ some sort of random-lottery incentive system to reduce the costs.

1.9 Epilogue

Following the success of “Deal or No Deal” in the Netherlands, the game show was sold to dozens of countries worldwide. Other research groups have investigated episodes of editions other than those used in this chapter. Their analyses employ not only different data sets, but also different research methodologies and different (implementations of) decision theories, and the results sometimes seem contradictory. Reconciling the seemingly disparate results will be a valuable exercise, but is beyond the scope of this study. We will limit ourselves at this point to a synopsis of the available studies, which are presented below in alphabetical order, and some concluding remarks.

Using the UK edition, Andersen *et al*. (2006a) estimate various structural choice models, assuming a homoskedastic error structure and accounting for forward-looking behavior. Their expected utility estimates suggest CRRA and initial wealth roughly equal to average annual UK income; their rank-dependent expected utility estimates indicate modest probability weighting along with a concave utility function; their prospect theory estimates indicate no loss aversion and modest probability weighting for gains, using several plausible specifications of the reference point. Andersen *et al*. (2006b) study the UK television shows and related lab experiments using a mixture model in which decision makers use two criteria: one is essentially rank-dependent expected utility, and the other is essentially a probabilistic income threshold. They find evidence that both criteria are used in the game show and that lab subjects place a much greater weight on the income threshold.

Baltussen, Post and Van den Assem (2007) compare various editions of DOND. Their sample includes editions from the same country that employ very different initial sets of prizes. Comparing editions from the same country can separate the effect of current stakes and prior outcomes without introducing cross-country effects, in the same way as changing the initial stakes in our experiments. Consistent with reference-dependence and path-dependence, they find that contestants in large- and small-stake editions respond in a similar way to the stakes relative to their initial level, even though the initial stakes are widely different across the various editions.

Blavatskyy and Pogrebna (2007a) show that Italian and UK contestants do not exhibit lower risk aversion when the probability of a large prize is small, and they interpret this as evidence against the overweighting of small probabilities. Blavatskyy and Pogrebna (2007b) find that the fit and relative performance of alternative decision theories depends heavily on the assumed error structure in the Italian and UK data sets. Pogrebna (2008) finds that Italian contestants generally do not follow naïve advice from the audience. Blavatskyy and Pogrebna (2006a) analyze the UK, French and Italian editions, which sometimes include a swap option that allows contestants to exchange their briefcase for another unopened briefcase. Blavatskyy and Pogrebna (2006b) conduct a nonparametric test of ten popular decision theories using the UK and Italian edition.

Bombardini and Trebbi (2007) use the Italian edition to estimate a structural dynamic CRRA expected utility model and find that the risk aversion is moderate on average and shows substantial variation across individual contestants. They also find that contestants are practically risk neutral when faced with small stakes and risk averse when faced with large stakes. Accounting for strategic interaction between the banker and the contestant (the Italian banker knows the contents of the unopened briefcases) does not change their conclusions.

Botti *et al*. (2007) estimate various structural expected utility models for the Italian edition, assuming that contestants ignore subsequent bank offers and compare the current bank offer with the set of remaining prizes. They find that the CARA specification fits the data significantly better than the CRRA and expo-power specifications, and they also report a gender effect (males are more risk averse) and substantial unobserved heterogeneity in risk aversion.

Deck, Lee and Reyes (2008) estimate structural CRRA and CARA expected utility models for Mexican episodes of “Deal or No Deal”. They consider both forward-looking contestants and myopic contestants who look forward only one game round, and they vary the level of forecasting sophistication by the contestants. They find a moderate level of average risk aversion and considerable individual variation in risk attitudes, with some contestants being extremely risk averse while others are risk seeking.

Using the Australian edition, De Roos and Sarafidis (2006) estimate structural dynamic CARA and CRRA expected utility models using random effects and random coefficients models. Their models produce plausible estimates of risk aversion, and suggest substantial heterogeneity in decision making, both between contestants and between decisions made by the same contestant. They also find that rank-dependent expected utility substantially improves the explanatory power. In addition to these main-game results, they also investigate contestants’ choices in special “Chance” and “Supercase” game rounds, which are specific for the Australian edition. Risk attitudes elicited in these additional game rounds seem to be similar to risk attitudes elicited in the main game. Also using Australian data, Mulino *et al*. (2006) estimate a structural dynamic CRRA expected utility model. Their estimates reveal moderate risk aversion on average and considerable variation across contestants. They also find that risk aversion depends on contestant characteristics such as age and gender, but not on wealth. Like De Roos and Sarafidis, they investigate the choices in the “Chance” and “Supercase” rounds, but they do find a difference in risk attitudes between these special rounds and the main game.

Clearly, “Deal or No Deal” can be studied for several research purposes and with a variety of methodologies and theories, and different studies can lead to different, sometimes opposing conclusions. Some final remarks may be useful to evaluate the existing studies and to guide further research. First, to analyze risk attitudes without the confounding effect of ambiguity and strategic insight, it is useful to analyze the basic version of the game. Of course, the more exotic versions with special game options and informative bank offers are interesting for other purposes, as demonstrated in some of the above studies. Second, to disentangle the effect of the amounts at stake and the effect of previous outcomes, it is useful to analyze multiple game show editions or choice experiments with different initial amounts at stake. Within one edition or experiment, current stakes and prior outcomes are perfectly correlated, and the two effects cannot be separated. Third, when using parametric structural models, it seems important to analyze the robustness for the assumed mental frame and error structure. For example, we found a relatively poor fit for models that assume that contestants focus on the set of remaining prizes rather than the next round's bank offer, and also for models that assume that the error variance is equal for all choice problems, irrespective of the level of the stakes or the variation in the prizes.

Chapter 2:
Risky Choice and the Relative Size of Stakes[8]

Abstract

We examine framing effects by analyzing how risky choice depends on the absolute and relative size of the amounts at stake, using an extensive sample of choices from ten different editions of the large-stake TV game show "Deal or No Deal". Our analyses within and across the samples suggest that risky choice is highly sensitive to the context, as defined by the initial set of prizes in the game. In each sample, contestants respond in a similar way to the stakes relative to their initial level, even though the initial level differs widely across the various editions. Amounts therefore appear to be primarily evaluated in proportion to a subjective frame of reference rather than in terms of their absolute monetary value.

2.1 Introduction

Normative theories of judgment and decision making assume that the context in which a decision occurs does not affect the choice. However, a wealth of experimental evidence, starting with the early work of Kahneman and Tversky (1979) and Tversky and Kahneman (1981), suggests that decision makers deviate from normative behavior by making decisions that depend on subjective anchors or reference points such as earlier expectations or aspirations. In many cases also real-life decision making can only be understood by accounting for the decision maker's subjective frame of reference. For example, Simonsohn and Loewenstein (2006) show that movers arriving from more expensive cities spend more money on housing than movers to the same city arriving from cheaper cities, controlling for wealth and other confounding effects. We add to the

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[8] This chapter is based on the paper "Risky Choice and the Relative Size of Stakes", co-authored by Guido Baltussen and Thierry Post. We thank conference participants at AEA 2008 New Orleans, BDRM 2008 San Diego, and FUR 2008 for useful comments and suggestions. We are very grateful to the people at Endemol, TROS, VTM, Sat.1, Endemol Southern Star, Seven Network and Schweizer Fernsehen and to Monique de Koning and Chris Powney for providing us with information and/or recordings of "Deal or No Deal", and to Sylvie Nahuijs and Jan-Hein Paes for their skillful research assistance. The support by Tinbergen Institute, Erasmus Research Institute of Management and Erasmus Center for Financial Research is also gratefully acknowledged.

literature on this subject by analyzing context effects in risky-choice behavior across various editions of the large-stake TV game show "Deal or No Deal" (DOND).

The problems in this game show resemble the simple and well-defined problems in laboratory choice experiments, but the stakes are substantially larger. Not surprisingly, DOND has recently attracted substantial research interest as a natural laboratory for analyzing risky choice; see for example the survey in Post, Van den Assem, Baltussen and Thaler (2008; henceforth PVBT). The game show originated in the Netherlands in 2002 and is now broadcast in dozens of countries.[9] The rules are similar in each edition. The game is played over several rounds by one single contestant, and starts with a known set of twenty or more monetary prizes that are randomly distributed over the same number of closed cases (boxes or briefcases). One of these cases is set aside for the contestant, who then "owns" the unknown prize it contains. At the beginning of each game round, the contestant opens one or more of the other cases, thereby revealing which prizes are not in her own case. Next, a "banker" offers a riskless amount of money to buy the contestant's case. If the contestant says "Deal", the game ends there. A contestant who says "No Deal" enters the next round and has to open additional cases, followed by a new offer. The game continues in this way until the contestant either accepts the banker's offer, or until all the cases have been opened and the contestant wins the contents of her own case. Although the game format is similar all over the world, the set of prizes that is distributed over the cases varies substantially.[10]

In a recent study of Dutch, German, US and experimental samples of DOND, PVBT find strong indications of the reference dependence of risk attitudes. For each sample, a simple implementation of prospect theory explains the choices substantially better than expected utility does. The biggest losers and the biggest winners appear to have an abnormally low degree of risk aversion, consistent with the "break-even" and "house-money" effects that occur when the reference point sticks to earlier expectations

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[9] According to an official press release by production company Endemol (February 22, 2007), DOND was aired in a total of 46 countries in 2006. In some countries, DOND is known by a local name. The original Dutch edition, for example, is named "*Miljoenenjacht*" ("Chasing Millions"). We will refer to the game show by the Anglo-Saxon name, the name that is most used worldwide.

[10] In all editions, the statistical distribution of the prizes is typically strongly positively skewed. The original Dutch version involves the largest stakes, with an average prize of roughly €400,000 and a top prize of €5,000,000.

(Thaler and Johnson, 1990). Many losers even appear to be risk seeking by rejecting bank offers that exceed the average remaining prize. It seems difficult to explain these choices without taking the context into account, because the losers generally still have thousands or tens of thousands of Euros at stake. In other empirical studies (including other game show studies and experimental studies), gambles of this magnitude are typically associated with risk aversion. In the context of a game that commences with an average prize of hundreds of thousands however, amounts of thousands or tens of thousands may seem small and are probably relatively easily put at risk in an attempt to escape from the uneasy feelings of experiencing a loss. Still, the PVBT results provide only indirect evidence for such a framing effect. Like other DOND studies, PVBT analyze choices within individual editions; within a given edition, the initial set of prizes is fixed and one can only analyze its effect by assuming a structural choice model that forwards the relevant specification of the reference point, introducing a risk of specification error.

This study examines how risky choices in DOND depend on the context of the initial set of prizes in a game. To compare how the absolute and relative magnitudes of the amounts at stake affect risky choice, we analyze ten editions of DOND with large differences in the set of initial prizes, originating from seven different countries: the Netherlands (2), Belgium (2), Australia (2), the US, the UK, Germany and Switzerland.[11] The ten samples together cover approximately 6,400 risky choices from over 1,100 different contestants. The differences in the set of prizes at the start of a game allow us to analyze framing effects by analyzing choices across editions, and reduces the need for fully specified structural models.

We first pay attention to our samples from the Netherlands, Belgium and Australia. In each of these three countries, both a large-stake version and a small-stake version of the game show are broadcast. Using editions with different stakes from the same country may allow for a more appropriate comparison than using editions from different countries, because it controls for systematic differences between countries in, for example, wealth and culture. Nevertheless, we will conclude this study with a large cross-country analysis.

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[11] Some of these editions are also used in other studies. The small-stake version from Australia is analyzed by Brooks *et al*. (2008a, 2008b) and De Roos and Sarafidis (2006); the UK version is analyzed by Andersen *et al*. (2006a, 2006b) and Blavatskyy and Pogrebna (2006a, 2006b, 2007a, 2007b).

Our probit estimation results suggest that the average contestant in each sample is roughly equally sensitive to changes in the relative level of the amounts at stake (the current average remaining prize relative to the initial average), even though the absolute level of the stakes (the current average remaining prize in monetary terms) is very different. For a given edition of DOND, changes in the amounts at stake have a strong effect on risk attitudes and choice behavior, but differences in the initial amounts across the shows have only a weak effect. We therefore conclude that risk aversion in DOND depends strongly on the context of the initial set of prizes. In contrast to normative theories of risky choice, amounts appear to be primarily evaluated in proportion to a subjective frame of reference rather than in terms of their absolute monetary value. A contestant who may initially expect to win tens of thousands in one edition will consider an amount of, say, €50,000, to be a larger amount than a contestant in another edition who may expect to win hundreds of thousands, and will behave accordingly.

The remainder of this chapter is organized as follows. In Section 2.2, we describe the editions of DOND from the Netherlands, Belgium and Australia, and we present a summary of our data from these countries. Section 2.3 analyzes how contestant characteristics and game situations affect the risky choices in DOND using probit regression analyses for each individual edition separately and for the combination of each pair of editions from the same country. Section 2.4 adds analyses of samples from the US, the UK, Germany and Switzerland, and shows the results of probit regression analysis for the large, pooled sample of all ten international editions. Finally, Section 2.5 contains concluding remarks and suggestions for future research.

2.2 Descriptions of Editions and Data

The general setup of DOND as described in the introduction applies to all editions worldwide. There are however some noteworthy differences. This section discusses the details of the six Dutch, Belgian and Australian versions of DOND, explains the sample periods, and presents summary statistics for each of the six samples. The four editions from the US, the UK, Germany and Switzerland will be discussed in Section 2.4, where we include the samples from these countries in a large cross-country analysis.

The Dutch large-stake edition is called "*Miljoenenjacht*" (or "Chasing Millions"), and starts off with 26 cases. The prizes in the cases range from €0.01 to €5,000,000 with an average prize of roughly €400,000. A game is played over a maximum of 9 rounds, and the number of cases opened each round is 6, 5, 4, 3, 2, 1, 1, 1 and 1, respectively, reducing the number of remaining cases from 26 to 20, 15, 11, 8, 6, 5, 4, 3, and 2 in the last round. Figure 2.1 presents a schematic overview of the course of the game.

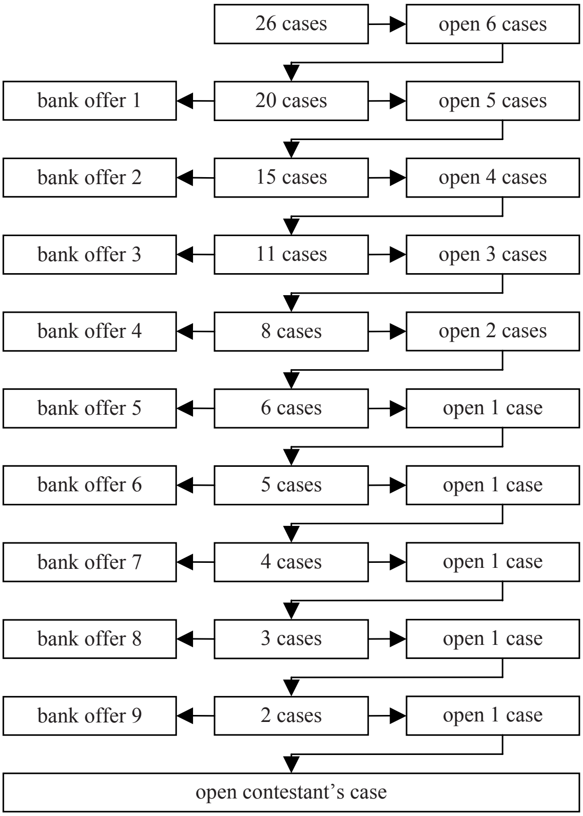

Figure 2.1: Flow chart of the Australian editions and the large-stake editions from the Netherlands and Belgium. In each round, the contestant chooses a number of cases to be opened, each opened case giving new information about the unknown prize in the contestant's own case. After the prizes in the chosen cases are revealed, a "bank offer" is made. If the contestant accepts the offer ("Deal"), she walks away with the amount offered and the game ends; if the contestant rejects the offer ("No Deal"), play continues and she enters the next round. If the contestant opts for "No Deal" in the last round, she receives the prize in her own case.

Contestants are selected from an audience composed of 500 prospects by means of an elimination game that precedes the main game and that is based on quiz questions. As in most editions, the actual game is played by one contestant per episode. We use an updated sample of the Dutch large-stake episodes analyzed in PVBT, consisting of all the 56 episodes (292 choice observations) aired up to June 3, 2007. Because the €7,500 prize was replaced with a €750,000 prize after episode 47, the average prize is not exactly constant across all episodes, and equals €391,411 in episodes 1 – 47 and €419,696 in episodes 48 – 56. There were no further material changes.

The Dutch small-stake edition uses only 20 prizes instead of 26. The prizes are considerably smaller than in the large-stake edition, and range from €1 to €250,000 with an average prize of €31,629. The game starts with 20 potential contestants, all randomly assigned a case with one of the 20 prizes hidden inside. A multiple choice question determines which contestants qualify for a pool from which one contestant is then randomly chosen to play the game. The 19 contestants that are not selected return in the subsequent episode (together with one new contestant), to form the group from which the next episode's contestant is chosen. The small-stake edition has a maximum of 5 rounds. In the first round, 6 cases are opened, followed by 3 cases in each subsequent round. Figure 2.2 presents an overview of the small-stake game.

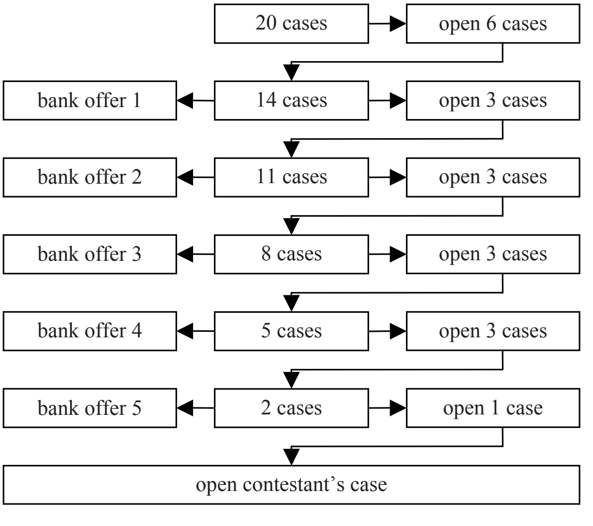

Figure 2.2: Flow chart of the small-stake edition from the Netherlands.

The first season of the small-stake edition started on August 27, 2006 and episodes were then aired nearly every weekday until June 8, 2007. Our sample consists of the full set of 204 episodes (904 choice observations) aired during this first season. All the episodes were recorded by the authors. There were no changes to the game format during the sample period. An occasional special feature in the small-stake edition is the "swap option". In the fifth game round of 24 episodes, when two cases or prizes are left, the contestant was offered the opportunity to swap her case for the other remaining case. Whenever this option was offered, it was always offered in addition to and not instead of a regular bank offer. In most instances (16), the contestant preferred to stick with her own case. Obviously, the opportunity to swap cases does not change the contestant's optimal strategy, because she has no information about which of the two remaining prizes is her own case, and therefore we do not pursue this option further.

The Belgian large-stake edition is named after the Dutch large-stake edition, or "*Miljoenenjacht*". The game uses 26 prizes with an average value of €85,972, including a top prize of €1,000,000. The game is played in exactly the same manner as the Dutch large-stake version (see also Figure 2.1). Contestant selection also occurs in the same way, although the elimination game starts with only 150 prospects in the audience. The Belgian large-stake version was aired 19 times, in two seasons of weekly episodes. The first series of 11 shows was launched on October 16, 2004 and the second series of 8 shows started on October 15, 2005. Copies of the 19 shows (114 choice observations) were obtained from Endemol's local production company *Endemol België*.

A small-stake Belgian version called "*Te Nemen of Te Laten*" was launched on August 21, 2006 and aired on most weekdays until April 12, 2007. Unlike other editions, in this edition the contestant has to split her winnings with an anonymous viewer. After adjustment, the top prize is €100,000 and the average €11,492. Games are played over a maximum of 6 rounds, during which the number of cases, 22 at the start of the game, decreases to 16, 13, 10, 7, 4, and finally 2; see Figure 2.3. The contestant is randomly selected from a pool of 22 potential contestants, and, as in the Dutch small-stake version, contestants that are not selected return in the subsequent episode to form part of the group from which the next episode's contestant is selected. A typical feature of the Belgian small-stake game is that, at some point during the game, most contestants are offered the opportunity to exchange their case for any other remaining case. Contrary to the swap option in the Dutch small-stake version, the offer to swap cases replaces the regular bank offer for that particular round of the game. It is the decision of the game-show producer as to if and when this offer is made. As a result, contestants in the Belgian small-stake version may experience some ambiguity beyond the normal risk of the game. A Belgian colleague of the authors recorded all the 130 episodes. We deliberately excluded one contestant from our sample (a Belgian celebrity playing on behalf of a charitable institution on New Year's Day; inclusion would not have affected our results), leaving a sample size of 129 contestants (613 choice observations).

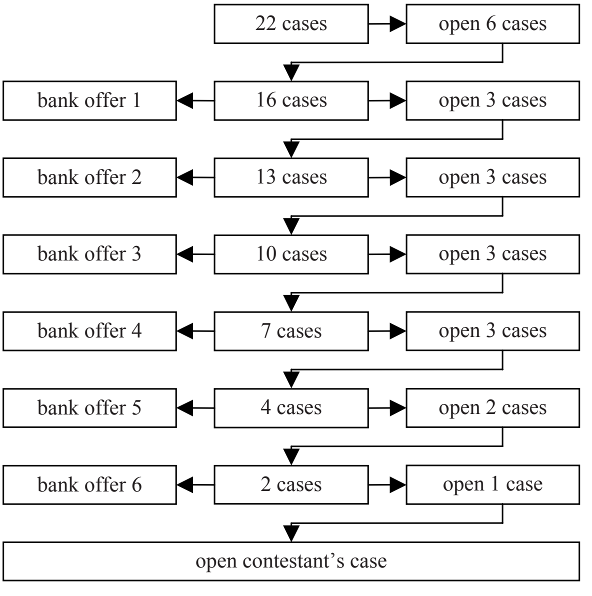

Figure 2.3: Flow chart of the small-stake edition from Belgium.

In Australia, DOND made its debut on July 13, 2003. The first season consisted of 14 weekly episodes, covering the games of 16 contestants (the games of some contestants started in one episode and continued in the next). The top prize of the 26 prizes in this large-stake version is A$2,000,000 (€1,200,000 using a rate of €0.60 per A$), and the average equals A$154,885 (€92,931). The structure of the game is exactly the same as that of the large-stake versions in the Netherlands and Belgium (see also Figure 2.1), and the contestant is selected in a similar way (by means of quiz questions) from a large group of prospects (200) in the audience. Four additional Australian shows with large prizes were aired as "special episodes" in August and September 2004. We have added two of these shows to our sample: one with a couple playing the game and one with an unlucky contestant from an earlier episode. A special episode with hypothetical stakes (a former, lucky contestant playing on behalf of an anonymous, ex-post selected viewer) and a show named "test of the psychics" (where decisions were based primarily on clairvoyance) have been intentionally omitted, although their inclusion would not affect our results. Our Australian large-stake sample totals 18 contestants (100 choice observations); copies of the episodes were obtained from an Australian game-show collector.

DOND returned to Australian TV on February 2, 2004, but as a shorter, daily edition with considerably reduced prizes. The structure of the game remained unchanged, but the top prize and the average prize were scaled down to A$200,000 (€120,000) and A$19,112 (€11,467), respectively. The contestant is selected from a group of 26 by means of three multiple choice quiz questions. Our sample covers the complete set of 140 episodes (993 choice observations) aired up to August 13, 2004. After that date, game options known as "Chance" and "Supercase" were introduced. These options add ambiguity to the game that we prefer to avoid. We are grateful to *Endemol Southern Star* and TV station *Seven* for providing us with the recordings.

For all episodes analyzed in this study, we collected data on the eliminated and remaining prizes, the bank offers and the DOND decisions. We also collected data on each contestant's gender, age and education. Gender and education are coded as dummy variables, with a value of 1 assigned to females, and to contestants with either a bachelor degree level of education or higher (including students) or equivalent work experience, respectively. The contestant characteristics are in some instances obtained from the producer, but mostly extracted from the introductory talk, from other conversations between the host and the contestants, or from a short movie about the life of the contestant at the beginning of some editions. The contestant's level of education is usually not explicitly mentioned, but this characteristic can often be inferred from her stated profession. Missing values for the contestant's age are estimated on the basis of both the contestant's physical appearance and other helpful information such as the age of children.

Table 2.1 presents summary statistics for the six samples. The average age of the contestants varies from 32 (Australian large-stake edition) to 46 (Dutch large-stake edition), the proportion of women in the samples varies from 21 (Belgian large-stake edition) to 67 percent (Belgian small-stake edition), and the average percentage of highly-educated contestants ranges between 26 (Belgian large-stake edition) and 72 (Australian large-stake edition). Despite some differences in the composition of the samples, the variation within each sample is large, and the next section shows that the contestant characteristics do not have a systematic and significant effect across the different analyses.

Apart from one single exception in the Australian large-stake edition, contestants always reject the first two offers. Since the bank offer is only a small fraction of the average remaining prize in the first rounds (as illustrated in Table 2.2 discussed below), the choices in these rounds are generally trivial. Statistics on the round in which contestants accept the bank offer in each sample cannot be directly compared due to differences in the maximum number of rounds in each edition. However, contestants generally play for longer both in editions with smaller stakes and in editions with fewer cases to open per round. The same pattern emerges in comparisons of the percentage bank offers that contestants choose to reject and accept, suggesting a pattern of increased risk aversion for larger stakes.[12] In the next sections, we will show that this pattern *across* editions is rather weak compared to the strong pattern of increased risk aversion for larger stakes *within* each edition.

Performing the natural experiment with the same monetary prizes in the lab would far exceed any experimental research budget as the combined total prize money in the six samples equals nearly €85 million. The biggest winner is a Dutch contestant named Helma: in the episode aired on November 13, 2005, she accepted a bank offer of €1,495,000 in round 7, while amounts of €1,000, €75,000, €2,500,000 and €5,000,000 were remaining. Her case turned out to contain €1,000.

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[12] In Table 2, the average percentage bank offer accepted in the Australian small-stake edition is heavily influenced by one extremely appealing offer of 2,963 percent. Omitting this one observation reduces the average from 111.54 to 91.03 percent. In Table 3, omission reduces the reported average bank offer for round 9 from 198 to 103 percent.

Table 2.1: Summary statistics. The table shows descriptive statistics for our six samples from the Netherlands (Panel A and B), Belgium (C and D) and Australia (E and F). Age is measured in years. Gender is a dummy variable with a value of 1 assigned to females. Education is a dummy variable that takes a value of 1 for contestants with bachelor-degree level or higher (including students) or equivalent work experience. Stop Round is the number of the round in which the bank offer is accepted, or the maximum number of rounds + 1 for contestants who rejected all offers. Best Offer Rejected is the highest percentage bank offer the contestant chose to reject ("No Deal"). Offer Accepted is the percentage bank offer accepted by the contestant ("Deal"), or 100 percent for contestants who rejected all offers. Amount Won equals the accepted bank offer in Euros, or the prize in the contestant's own case for those contestants who rejected all offers. Australian Dollars are converted into Euros by using a single fixed exchange rate of €0.60 per A$.

	Mean	Stdev	Min	Median	Max
A. Dutch large-stake edition (56 contestants)					
Age (years)	45.80	11.53	21.00	43.50	70.00
Gender (female = 1)	0.30	0.46	0.00	0.00	1.00
Education (high = 1)	0.52	0.50	0.00	1.00	1.00
Stop Round	5.23	1.73	3.00	5.00	10.00
Best Offer Rejected (%)	54.62	32.46	10.17	53.76	119.88
Offer Accepted (%)	74.68	30.92	20.77	79.23	165.50
Amount Won (€)	231,241.25	261,212.29	10.00	156,500.00	1,495,000.00
B. Dutch small-stake edition (204 contestants)					
Age (years)	34.42	10.57	19.00	33.00	67.00
Gender (female = 1)	0.49	0.50	0.00	0.00	1.00
Education (high = 1)	0.38	0.49	0.00	0.00	1.00
Stop Round	4.74	0.95	3.00	4.00	6.00
Best Offer Rejected (%)	41.83	33.54	7.95	26.50	223.48
Offer Accepted (%)	64.82	34.60	9.51	53.69	188.94
Amount Won (€)	11,762.09	12,350.22	1.00	9,140.00	69,000.00
C. Belgian large-stake edition (19 contestants)					
Age (years)	41.89	10.11	30.00	42.00	65.00
Gender (female = 1)	0.21	0.42	0.00	0.00	1.00
Education (high = 1)	0.26	0.45	0.00	0.00	1.00
Stop Round	6.11	2.00	4.00	6.00	10.00
Best Offer Rejected (%)	55.96	26.56	20.70	49.83	114.29
Offer Accepted (%)	65.48	23.69	37.22	62.76	100.00
Amount Won (€)	50,263.16	48,886.88	500.00	30,000.00	200,000.00
D. Belgian small-stake edition (129 contestants)					
Age (years)	32.43	9.03	19.00	30.00	59.00
Gender (female = 1)	0.67	0.47	0.00	1.00	1.00
Education (high = 1)	0.33	0.47	0.00	0.00	1.00
Stop Round	5.71	1.11	4.00	6.00	7.00
Best Offer Rejected (%)	54.41	41.48	11.39	43.00	346.90
Offer Accepted (%)	74.08	28.50	14.56	79.79	140.00
Amount Won (€)	5,134.75	6,154.72	0.01	3,750.00	37,500.00

Table 2.1 (continued)

	Mean	Stdev	Min	Median	Max
E. Australian large-stake edition (18 contestants)					
Age (years)	32.33	6.32	20.00	32.50	43.00
Gender (female = 1)	0.25	0.43	0.00	0.00	1.00
Education (high = 1)	0.72	0.46	0.00	1.00	1.00
Stop Round	5.78	2.39	2.00	5.00	10.00
Best Offer Rejected (%)	63.53	47.55	8.99	53.70	178.09
Offer Accepted (%)	92.92	63.32	14.51	70.90	228.01
Amount Won (€)	56,922.00	83,917.34	3.00	27,075.00	309,000.00
F. Australian small-stake edition (140 contestants)					
Age (years)	36.04	11.89	18.00	35.00	75.00
Gender (female = 1)	0.50	0.50	0.00	0.50	1.00
Education (high = 1)	0.56	0.50	0.00	1.00	1.00
Stop Round	7.20	1.71	3.00	7.00	10.00
Best Offer Rejected (%)	77.38	30.15	15.69	75.59	184.62
Offer Accepted (%)	111.54	245.13	26.72	88.53	2.962.96
Amount Won (€)	9,899.96	12,766.19	0.30	6,783.00	120,000.00

Table 2.2 summarizes the offers made by the banker and the contestants' decisions to accept or reject the offers in the various rounds. The offers are related to the set of remaining prizes and typically start at a small fraction of the average prize and approach 100 percent in the later rounds. As the offers become more generous, more and more contestants are persuaded to accept them ("Deal") and exit the game. The bank offers in the last few rounds of the Dutch and Belgian small-stake editions are relatively conservative compared to those in the other editions. For example, in round 6 of the Dutch large-stake edition and in round 4 of the Dutch small-stake edition, five cases remain unopened. While in the large-stake edition the banker on average offers 87 percent of the average remaining prize, this percentage is only 41 in the small-stake edition. This difference may reflect the fact that contestants in the small-stake edition generally face more risk, because they have to open more cases. (For example, in round 4, three out of five remaining cases have to be opened after a "No Deal" decision, while this is only one out of five in round 6 of the large-stake edition.) Alternatively, if there are fewer rounds and smaller stakes, there may be more incentive for the producer to discourage a "Deal" in order to enhance the show's entertainment value.

Table 2.2: Bank offers, stakes and decisions. The table summarizes the percentage bank offers, the stakes and contestants' decisions for our six samples from the Netherlands (Panel A and B), Belgium (C and D) and Australia (E and F). The number of unopened cases when the bank offer is presented (Cases), the average bank offer as a percentage of the average remaining prize (%BO), the average remaining prize in Euros (Stakes), the number of contestants in the given round (No.), the number of those contestants that accept the bank offer ("D") and the number of those contestants that reject the bank offer ("ND") are reported for each round. Australian Dollars are converted into Euros by using a single fixed rate of €0.60 per A$. Observations with no bank offer (which occasionally occur when only insignificant amounts remain or due to a substituting swap offer) are excluded.

Round	Cases	%BO	Stakes	No.	"D"	"ND"
A. Dutch large-stake edition						
0	26	-	396,001	56	-	-
1	20	6%	397,392	56	0	56
2	15	14%	386,686	56	0	56
3	11	33%	384,339	56	10	46
4	8	58%	360,429	46	13	33
5	6	75%	316,743	33	8	25
6	5	87%	238,631	25	13	12
7	4	96%	268,389	12	6	6
8	3	94%	139,341	6	4	2
9	2	106%	11,253	2	1	1
B. Dutch small-stake edition						
0	20	-	31,629	204	-	-
1	14	6%	30,888	204	0	204
2	11	12%	30,138	204	0	204
3	8	21%	30,473	204	11	193
4	5	41%	29,835	193	92	101
5	2	80%	18,203	99	40	59
C. Belgian large-stake edition						
0	26	-	85,972	19	-	-
1	20	6%	85,299	19	0	19
2	15	17%	79,160	19	0	19
3	11	34%	79,785	19	0	19
4	8	48%	87,566	19	5	14
5	6	60%	84,975	14	4	10
6	5	70%	73,546	10	3	7
7	4	78%	88,542	7	3	4
8	3	93%	5,809	4	1	3
9	2	101%	4,883	3	1	2
D. Belgian small-stake edition						
0	22	-	11,492	129	-	-
1	16	7%	11,441	129	0	129
2	13	16%	11,480	129	0	129
3	10	30%	11,482	127	0	127
4	7	39%	11,558	104	23	81
5	4	62%	10,205	71	34	37
6	2	87%	7,031	53	29	24

Table 2.2 (continued)

Round	Cases	%BO	Stakes	No.	"D"	"ND"
E. Australian large-stake edition						
0	26	-	92,934	18	-	-
1	20	9%	84,804	18	0	18
2	15	15%	92,287	18	1	17
3	11	27%	107,131	17	2	15
4	8	65%	97,144	15	3	12
5	6	87%	109,683	12	4	8
6	5	95%	125,259	8	1	7
7	4	94%	112,308	7	3	4
8	3	120%	4,335	3	1	2
9	2	140%	2,254	2	1	1
F. Australian small-stake edition						
0	26	-	11,467	140	-	-
1	20	10%	11,132	140	0	140
2	15	22%	11,089	140	0	140
3	11	31%	11,678	140	1	139
4	8	49%	11,850	139	10	129
5	6	62%	11,557	129	14	115
6	5	78%	11,338	115	19	96
7	4	86%	11,447	96	32	64
8	3	102%	10,543	64	34	30
9	2	198%	9,649	30	15	15

The next section uses probit regression analysis to analyze the DOND choices in greater detail. We will perform regressions for each edition separately, and for the combination of each pair of editions from the same country. Comparing two editions from one country circumvents the problems that may arise as a result of using editions from different countries, like differences in wealth and culture. Although the small-stake editions in both the Netherlands and Belgium have a somewhat different structure than their large-stake counterparts, the effect of the differences on the risk of continuing play are simple to model. The next section shows that the choices in the various editions are not significantly different after correcting for the percentage bank offers and the risk of continuing play.

In addition to differences in the structure, there are also the possible differences in the contestant pools. One concern is that richer and less risk-averse people may be more willing to spend time attempting to get onto a large-stake show than onto a small-stake one. Also, small-stake editions generally feature contestants that are explicitly selected on an individual basis by the producer, whereas chance plays a larger role in selecting the people that play the elimination game in a large-stake show. In the Dutch large-stake version for example, a national lottery sponsors the show and the original 500 people in the audience are randomly invited lottery players. Indeed, as shown above, the contestant characteristics show non-trivial variation across the various editions. To account for the contestant pool, we included these characteristics as control variables in the regressions. The characteristics do not however show a systematic and significant effect on the risk attitudes within the editions and it therefore seems unlikely that they have a material effect across the editions.

2.3 Probit Regression Analysis

This section uses probit regression analysis to explain the DOND choices within and across the various samples.[13] The DOND choices of the different contestants in different editions, different games and different rounds cannot be directly compared without accounting for differences in game situations and contestant characteristics. Therefore, earlier DOND-based studies like PVBT use structural choice models. The results of these models are however sensitive to their precise specifications (for example, the shape of the utility function and the dynamics of the reference point). To avoid this problem, we estimate reduced-form models using probit regression analysis rather than full-blown structural models, allowing for a more compact presentation and more robust results.

The dependent variable is the contestant's decision, with a value of 1 for "Deal" and 0 for "No Deal". We try to explain the decisions we observed with the following set of contestant- and game-related variables:

- Age;
- Gender (0 = male, 1 = female);
- Education (0 = low, 1 = high);
- $EV/10^5$: absolute stakes, measured as the current average remaining prize in Euros divided by 100,000;
- EV/EV_0: relative stakes, measured as the current average remaining prize divided by the initial average;

[13] Using logit instead of probit yields similar results.

- BO/EV: percentage bank offer, or bank offer divided by the average remaining prize;
- Stdev/EV: standard deviation ratio, or standard deviation of the distribution of the average remaining prize in the next round divided by the average remaining prize.

The standard deviation ratio measures the risk of continuing play ("No Deal") for one additional round, and in this way accounts for the differences between rounds and editions with respect to the number of cases that have to be opened. Our video material suggests that the typical contestant generally looks ahead to the bank offer in the next game round, rather than to the prize in their own case; the game-show host, for example, tends to stress what will happen to the bank offer in the next round should particular cases be eliminated and the contestants themselves often comment that they will play "just one more round". The use of other risk measures, such as the standard deviation of the remaining prizes, tends to lower the empirical fit, but does not change our conclusions regarding the relevance of relative stakes compared to absolute stakes. Although the distributions of bank offers and prizes are generally highly skewed, skewness does not add significant explanatory power, because it is very strongly correlated with the standard deviation in this game.

To control for the attractiveness of the bank offer, we include the percentage bank offer, which is defined as the bank offer divided by the average remaining prize. The use of the inverse of the percentage bank offer (which measures the expected return from rejecting the current and subsequent bank offers) yields similar results and does not change any of our conclusions.

Our focus is on the effect of absolute and relative stakes. The other variables are included as control variables. Fortunately, the control variables are only weakly correlated with the stakes variables, so it is not difficult to separate their effect. For example, the level of the stakes within a given edition is determined by chance and therefore uncorrelated with the contestant characteristics. Since absolute and relative stakes are perfectly correlated within each individual edition, only the absolute-stakes variable is included as regressor in the one-sample analyses.[14] Using relative stakes in those analyses would yield the same results, the only difference being that the absolute-stakes coefficient is multiplied by the constant term $EV_0/10^5$.

To allow for the possibility that the errors of individual contestants are correlated, we perform a cluster correction on the standard errors (see, for example, Wooldridge, 2003).

Table 2.3 shows the probit estimation results. The contestant characteristics generally do not have significant explanatory power or at least not consistently across all editions and countries, confirming the results of PVBT (2008). In contrast, game characteristics have a significant effect (with a few exceptions in the smaller samples) and show a consistent pattern across all editions and countries. Our discussion therefore focuses on the game characteristics.

Panel A shows the results for the Netherlands. As expected for non-satiable and risk averse individuals, the "Deal" propensity increases with the generosity of the bank offer and the dispersion of the outcomes. The "Deal" propensity also increases with the stakes, consistent with increasing relative risk aversion.

The results for the Dutch small-stake edition are very similar to those for the large-stake edition, supporting our assumption that the two versions of DOND are indeed comparable after a proper correction for game characteristics. However, for the small-stake edition, the coefficient for the absolute-stakes term changes from 0.153 to 2.179, an increase of roughly a factor of 14. Interestingly, this change is of the same order of magnitude as the difference in the initial average prize of the two editions (roughly a factor of 12.5). Replacing absolute stakes with relative stakes yields comparable coefficients for the two samples: 0.596 for the large-stake sample and 0.689 for the small-stake sample (not reported in the table). This is a first, strong indication that the relative size of the stakes in this game matter more than the absolute size.

The last three columns show the pooled results. If the stakes are included in absolute terms, the empirical fit of the pooled sample (a log-likelihood of -347.5) deteriorates significantly relative to the individual samples (LL = -93.6 + -217.7 =

[14] In fact, for some of the editions analyzed in this study the correlation is marginally below unity due to small changes in the initial set of prizes.

Table 2.3: Probit regression results within countries. The table displays the results from the probit regression analyses of the DOND decisions in our large- and small-stake samples from the Netherlands (Panel A), Belgium (B) and Australia (C). The dependent variable is the contestant's decision, with a value of 1 for "Deal" and 0 for "No Deal". Age is measured in years. Gender is a dummy variable with a value of 1 assigned to females. Education is a dummy variable that takes a value of 1 for contestants with bachelor-degree level or higher (including students) or equivalent work experience. EV is the current average remaining prize in Euros and EV_0 is the initial average. BO is the bank offer. Stdev measures the standard deviation of the distribution of the average remaining prize in the next game round. Australian Dollars are converted into Euros by using a single fixed rate of €0.60 per A$. Observations with no bank offer (which occasionally occur when only insignificant amounts remain or due to a substituting swap offer) are excluded. The first column shows the large-stake results, the second column shows the small-stake results and the last three columns show the results for the large- and small-stake samples from one country combined. Apart from the maximum likelihood estimates for the regression coefficients, the table reports the log-likelihood (LL), the mean log-likelihood (MLL), McFadden's R-squared, and the number of observations. The *p*-values (within parentheses) are corrected for correlation between the responses of a given contestant (contestant-level cluster correction).

	Large stakes	Small stakes	Pooled	Pooled	Pooled
A. The Netherlands					
Constant	-5.229 (0.000)	-4.191 (0.000)	-3.600 (0.000)	-4.311 (0.000)	-4.251 (0.000)
Age	0.021 (0.085)	-0.001 (0.890)	0.008 (0.130)	0.014 (0.009)	0.012 (0.042)
Gender	0.057 (0.780)	0.045 (0.740)	-0.043 (0.710)	-0.002 (0.990)	-0.019 (0.870)
Education	0.009 (0.970)	-0.255 (0.097)	-0.078 (0.490)	-0.035 (0.760)	-0.068 (0.560)
$EV/10^5$	0.153 (0.000)	2.179 (0.000)	0.148 (0.000)		0.057 (0.110)
EV/EV_0				0.624 (0.000)	0.538 (0.000)
BO/EV	2.677 (0.000)	1.148 (0.000)	1.390 (0.000)	1.868 (0.000)	1.802 (0.000)
Stdev/EV	3.597 (0.066)	3.345 (0.000)	2.787 (0.000)	2.567 (0.000)	2.690 (0.000)
LL	-93.6	-217.7	-347.5	-333.0	-331.3
MLL	-0.321	-0.241	-0.291	-0.278	-0.277
McFadden R^2	0.337	0.448	0.353	0.380	0.383
No. obs.	292	904	1196	1196	1196
B. Belgium					
Constant	-5.158 (0.001)	-4.399 (0.000)	-3.692 (0.000)	-4.256 (0.000)	-4.194 (0.000)
Age	0.006 (0.650)	-0.004 (0.730)	-0.000 (0.990)	0.005 (0.490)	0.002 (0.810)
Gender	-0.204 (0.430)	0.033 (0.860)	-0.033 (0.820)	-0.139 (0.350)	-0.080 (0.590)
Education	0.596 (0.017)	0.340 (0.076)	0.312 (0.043)	0.308 (0.051)	0.324 (0.038)
$EV/10^5$	0.848 (0.000)	5.510 (0.000)	0.799 (0.000)		0.387 (0.040)
EV/EV_0				0.633 (0.000)	0.482 (0.001)
BO/EV	2.044 (0.018)	0.510 (0.220)	0.389 (0.250)	0.834 (0.064)	0.736 (0.097)
Stdev/EV	4.813 (0.031)	4.260 (0.000)	3.963 (0.000)	3.693 (0.000)	3.855 (0.000)
LL	-34.5	-134.2	-181.5	-177.2	-175.5
MLL	-0.303	-0.219	-0.250	-0.244	-0.241
McFadden R^2	0.282	0.460	0.388	0.403	0.408
No. obs.	114	613	727	727	727

Table 2.3 (continued)

	Large stakes	Small stakes	Pooled	Pooled	Pooled
C. Australia					
Constant	-4.052 (0.007)	-4.119 (0.000)	-3.638 (0.000)	-4.023 (0.000)	-4.032 (0.000)
Age	-0.012 (0.786)	-0.001 (0.890)	-0.001 (0.778)	-0.000 (0.959)	-0.000 (0.987)
Gender	-0.772 (0.321)	0.339 (0.007)	0.261 (0.030)	0.255 (0.033)	0.270 (0.024)
Education	-1.415 (0.001)	-0.162 (0.160)	-0.246 (0.027)	-0.213 (0.049)	-0.231 (0.038)
$EV/10^5$	0.636 (0.000)	2.976 (0.002)	0.478 (0.000)		0.209 (0.175)
EV/EV_0				0.342 (0.001)	0.294 (0.010)
BO/EV	1.767 (0.000)	1.845 (0.000)	1.504 (0.000)	1.649 (0.000)	1.665 (0.000)
Stdev/EV	6.439 (0.007)	3.219 (0.000)	3.544 (0.000)	3.494 (0.000)	3.495 (0.000)
LL	-27.1	-244.8	-284.7	-278.5	-277.5
MLL	-0.271	-0.246	-0.261	-0.255	-0.254
McFadden R^2	0.384	0.349	0.322	0.337	0.340
No. obs.	100	993	1093	1093	1093

-311.3), reflecting the very different absolute-stakes coefficients in the two samples. However, if the stakes are measured in relative terms, the fit of the pooled sample (LL = -333.0) is much more comparable to the fit of the individual samples. Including both the absolute and the relative-stakes variables hardly improves the explanatory power (LL = -331.3) compared to using the relative-stakes variable only.

To summarize, while the sensitivity of the "Deal" propensity to the absolute level of the stakes is much higher in the small-stake edition, the sensitivity to the relative level of the stakes is comparable. The contestants respond in a similar way to changes in the relative stakes in each sample, even though the absolute stakes differ by a factor of 12.5. This suggests that the choice behavior in DOND is highly reference dependent. Decisions appear not to be based on an evaluation of the absolute amounts that are at stake, as we would expect an expected utility maximizer to do, but on the relative size of the amounts. A given bank offer or prize appears to be considered as "large" in the context of a game where it lies in the upper range of the initial set of prizes, and the same amount seems to be evaluated as "small" in a game where it belongs to the lower range of prizes.

Panel B and C show the results for Belgium and Australia, respectively. For both pairs of samples from these countries the same pattern arises. The sensitivity for absolute stakes is much lower in the large-stake edition than in the small-stake edition (0.848 vs. 5.510 for Belgium, and 0.636 vs. 2.976 for Australia), while the sensitivity for relative stakes is comparable (0.728 vs. 0.633 for Belgium, and 0.591 vs. 0.341 for

Australia; not shown in Table 2.3).[15] Note that the relative-stakes coefficients are not only comparable within each country, but also across the three countries included in the analysis so far. Compared to the relative magnitude of the stakes, the absolute-stakes variable hardly provides any contribution to the empirical fit of the pooled samples. These findings strengthen our interpretation that decisions are primarily based on the relative values of the amounts at stake.

Various robustness checks did not change our results. For example, we added a quadratic stakes term to the regression to analyze if the relationship between deal propensity and stakes is non-linear. The linear and quadratic terms combined can give a second-order Taylor series approximation to an arbitrary (twice continuously differentiable) function. In each sample, the quadratic term is significantly negative, suggesting that the deal propensity becomes less sensitive to stakes at higher stakes levels. Nonetheless, the main conclusion about the importance of the relative level of the stakes remains the same. When the stakes are measured in absolute amounts, the differences between the quadratic terms are of the same order of magnitude as the differences in the squared initial average prize. Consequently, the empirical fit in the pooled samples deteriorates significantly relative to the individual samples if the stakes are measured in absolute terms, and is comparable to the fit in the individual samples if the stakes are measured in relative terms. Although including a squared stakes term improves the empirical fit, the additional term is highly correlated with the linear term and the linear term is more important. For the sake of parsimony, we therefore do not include the quadratic terms in the reported analyses.

2.4 Evidence from Other Countries and Cross-Country Analyses

The analyses in the last two sections used data from different editions from the Netherlands, Belgium and Australia. Comparing different versions from only one country mitigates the potential confounding effects of wealth and culture. However, DOND has been aired in many other countries too, including several other developed,

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[15] The absolute- and relative-stakes coefficients for the Australian small-stake sample are rather small compared to the coefficients for the Australian large-stake sample. This is partly attributable to the choices of contestant Dean Cartechini, a very lucky contestant in the June 17, 2004 episode, who played the game to the end only to discover that he had the top prize of A$200,000 in his case. If we exclude the choices of this particular contestant from the regression, the absolute stakes coefficient increases from 2.976 to 4.009 and the relative stakes coefficient increases from 0.341 to 0.460.

Western countries, and it would be interesting to see if we find similar effects of absolute and relative stakes. In this section, we will therefore repeat our probit analysis for samples from the US, the UK, Germany and Switzerland, and we will also perform large, pooled-sample analyses across the ten different editions. Table 2.4 shows summary statistics for the four additional editions.

The US, German and Swiss editions are similar to the Dutch and Belgian large-stake version and to the two Australian versions analyzed in the previous sections: the shows start with 26 cases, last for a maximum of 9 rounds, and the number of cases opened in each round is 6, 5, 4, 3, 2, 1, 1, 1 and 1 respectively; see Figure 2.1.[16] The UK version is somewhat different, and resembles the small-stake versions from Belgium and the Netherlands. Some US, UK and Swiss contestants are offered the opportunity to swap their case at some point during the game. We ignore this option, as, in common with the Dutch small-stake edition, it is offered only sporadically, does not replace a monetary bank offer (apart from a few exceptions in the UK), and is often rejected.

For the US, we use the same sample as in PVBT (2008), which covers the first 53 contestants (402 choice observations) after the premiere on December 19, 2005. The stakes in the US are relatively large: the regular format has a maximum prize of $1,000,000 (€800,000 using €0.80 per $) and an average prize of $131,478 (€105,182). In our sample, the average prize is even larger ($142,435), because six games were played with higher amounts to mark the beginning and end of a series of episodes.

We updated the German sample of PVBT with data from the series aired from November 2006 to May 2007. This adds an extra 34 contestants and brings the total to 81 (628 choice observations). Most new episodes were recorded by a friend of the authors, and the remainder were kindly provided by *Endemol Deutschland*. The initial stakes across the 81 games amount to €25,335 (€26,347 for the first 20 contestants, €25,003 for the other 61).

DOND was introduced on Swiss TV on September 1, 2004. We obtained the first two seasons of 44 and 45 weekly episodes (691 choice observations) from *Schweizer Fernsehen*. The initial average prize in each of the 89 episodes of "*Deal or No Deal –*

[16] The first 20 contestants in our sample from Germany actually started with 20 cases. Effectively, their games can be analyzed as if the first round was skipped.

Table 2.4: Summary statistics for the US, UK, German and Swiss edition. The table shows descriptive statistics for our samples from the US, the UK, Germany and Switzerland. Definitions are similar to Table 2.1. US Dollars, UK Pounds and Swiss Francs are converted into Euros by using single fixed rates of €0.80, €1.50 and €0.65, respectively.

	Mean	Stdev	Min	Median	Max
A. US edition (53 contestants)					
Age (years)	34.98	10.03	22.00	33.00	76.00
Gender (female = 1)	0.57	0.50	0.00	1.00	1.00
Education (high = 1)	0.49	0.50	0.00	0.00	1.00
Stop Round	7.70	1.29	5.00	8.00	10.00
Best Offer Rejected (%)	80.98	17.57	44.04	83.52	112.00
Offer Accepted (%)	91.43	15.31	49.16	97.83	112.50
Amount Won (€)	98,035.66	95,556.94	4.00	75,200.00	371,200.00
B. UK edition (326 contestants)					
Age (years)	42.42	14.93	19.00	38.00	83.00
Gender (female = 1)	0.50	0.50	0.00	0.50	1.00
Education (high = 1)	0.27	0.45	0.00	0.00	1.00
Stop Round	5.38	1.23	3.00	5.00	7.00
Best Offer Rejected (%)	55.10	20.60	8.00	53.43	120.63
Offer Accepted (%)	72.45	23.56	12.66	74.23	104.00
Amount Won (€)	24,287.14	25,496.60	0.02	19,425.00	180,000.00
C. German edition (81 contestants)					
Age (years)	35.96	9.86	18.00	34.00	62.00
Gender (female = 1)	0.42	0.50	0.00	0.00	1.00
Education (high = 1)	0.47	0.50	0.00	0.00	1.00
Stop Round	7.95	1.57	4.00	8.00	10.00
Best Offer Rejected (%)	85.49	31.51	35.24	81.82	190.40
Offer Accepted (%)	93.17	19.19	51.52	94.08	159.84
Amount Won (€)	20,528.91	23,503.69	0.01	15,000.00	150,000.00
D. Swiss edition (89 contestants)					
Age (years)	40.20	10.06	18.00	40.00	60.00
Gender (female = 1)	0.49	0.50	0.00	0.00	1.00
Education (high = 1)	0.30	0.46	0.00	0.00	1.00
Stop Round	8.20	1.78	4.00	8.00	10.00
Best Offer Rejected (%)	79.25	16.71	35.25	84.05	104.89
Offer Accepted (%)	87.36	14.54	48.75	90.94	108.70
Amount Won (€)	11,413.64	13,634.22	0.03	7,150.00	78,000.00

Das Risiko" is Fr.26,898 (€17,583 using €0.65 per Fr.) and the largest prize is Fr.250,000 (€162,500).

The UK version started on October 31, 2005 and was normally aired six times a week. The game commences with 22 prizes averaging £25,712 (€38,568 using €1.50 per £) and includes a top prize of £250,000 (€375,000). Over the maximum number of 6 rounds, 5, 3, 3, 3, 3 and 3 cases are opened, respectively. Thanks to the frequency with

Table 2.5: Probit regression results for the US, UK, German and Swiss edition. The table displays the results from the probit regression analyses of the DOND decisions in our four samples from the US (first column), the UK (second column), Germany (third column) and Switzerland (fourth column). Definitions are similar to Table 2.3. US Dollars, UK Pounds and Swiss Francs are converted into Euros by using single fixed rates of €0.80, €1.50 and €0.65, respectively.

	US		UK		Germany		Switzerland	
Constant	-5.239	(0.000)	-4.470	(0.000)	-4.867	(0.000)	-4.403	(0.000)
Age	-0.006	(0.740)	0.004	(0.220)	0.009	(0.258)	0.002	(0.812)
Gender	-0.228	(0.370)	0.020	(0.850)	0.146	(0.451)	-0.465	(0.008)
Education	-0.279	(0.210)	-0.263	(0.025)	0.081	(0.679)	0.042	(0.821)
$EV/10^5$	0.515	(0.000)	1.706	(0.000)	2.460	(0.000)	3.336	(0.000)
EV/EV_0								
BO/EV	3.447	(0.000)	2.827	(0.000)	1.545	(0.000)	2.430	(0.000)
Stdev/EV	3.377	(0.000)	2.166	(0.000)	3.248	(0.000)	2.079	(0.000)
LL	-77.9		-417.6		-128.2		-135.2	
MLL	-0.194		-0.252		-0.204		-0.196	
McFadden R^2	0.463		0.387		0.386		0.287	
No. obs.	402		1656		628		691	

which the program was aired and the readiness of *Endemol UK* to provide us with recordings, we have collected a large sample covering all 326 episodes (1656 choice observations) up to December 11, 2006.

Table 2.5 shows the probit estimation results. Again, contestant characteristics are generally insignificant, while the game characteristics are highly important in every sample. The "Deal" propensity increases with the relative bank offer, with the dispersion of the prizes, and with the level of the stakes.

To facilitate a comparison of the absolute- and relative-stakes coefficients across the various editions, Table 2.6 summarizes the results for all samples by listing the absolute-stakes and relative-stakes coefficients from each one-sample regression, including the earlier regressions for the Netherlands, Belgium and Australia. Clearly, the variation in the absolute-stakes coefficients is much larger than the variation in the relative-stakes coefficients, consistent with the notion of relative valuation. For example, the absolute-stakes coefficient in the Dutch large-stake edition is roughly 36 times smaller than that in the Belgian small-stake edition; since the absolute stakes differ between the two editions by roughly the same factor (34.5), the two relative-stakes coefficients are almost identical. The sensitivity to the absolute level of the

Table 2.6: Overview of absolute- and relative-stakes effects. The table provides an overview of the probit regression coefficients for the absolute- and relative-stakes variables in multivariate probit regressions of the DOND decisions in the various editions of the game show. Age, Gender, Education, BO/EV and Stdev/EV are included as control variables. The results for the absolute-stakes variable $EV/10^5$ are taken from Table 2.3 and Table 2.5. The coefficients for the relative-stakes variable EV/EV_0 are obtained by substituting the absolute-stakes variable for this variable. Also included for each edition are the mean initial average of the prizes in the game ($\overline{EV}_0$; in Euros), the number of contestants (No. cont.) and the number of DOND decisions (No. obs.). Editions are arranged in order of initial average prize.

Sample	$\overline{EV}_0$	No. cont.	No. obs.	Stakes: Absolute	Stakes: Relative
Australia, small	11,467	140	993	2.976	0.341
Belgium, small	11,492	129	613	5.510	0.633
Switzerland	17,483	89	691	3.336	0.583
Germany	25,335	81	628	2.460	0.614
The Netherlands, small	31,629	204	904	2.179	0.689
UK	38,568	326	1,656	1.706	0.658
Belgium, large	85,792	19	114	0.848	0.728
Australia, large	92,934	18	100	0.636	0.591
US	113,948	53	402	0.515	0.635
The Netherlands, large	396,001	56	292	0.153	0.596

amounts at stake is clearly negatively related to the initial average prize (EV_0), whereas the relative-stakes coefficient is roughly equal (≈ 0.6) across all editions. (The results for the Australian small-stake edition appear somewhat different from the rest, but, as explained in Footnote 15, the low values can be attributed in part to the choices of only one single contestant.)

We conclude with a probit regression analysis for the large sample of editions combined. Note that this analysis is based on a very large data set of nearly 6,400 choices made by more than 1,100 different contestants. The results are presented in Table 2.7. If the stakes are included in absolute terms (first column), the empirical fit of the pooled sample (a log-likelihood of -1726.0) is clearly worse than the fit of the model that includes the stakes in relative terms (second column; LL = -1629.9). This result reflects the differences in absolute-stakes coefficients across the samples and the similarity of the relative-stakes coefficients that we observed earlier. Including both stakes terms substantially improves the fit (last column; LL = -1620.5) compared to the absolute-stakes model, whereas the improvement is rather limited compared to the

Table 2.7: Probit regression results across countries. The table displays the results from the pooled probit regression analyses across the ten different editions of DOND used in this study. Definitions are the same as those in Table 2.3.

	All samples		All samples		All samples	
Constant	-3.287	(0.000)	-4.069	(0.000)	-4.062	(0.000)
Age	0.001	(0.640)	0.004	(0.063)	0.003	(0.143)
Gender	-0.025	(0.630)	-0.011	(0.827)	-0.010	(0.839)
Education	-0.098	(0.055)	-0.072	(0.180)	-0.088	(0.099)
$EV/10^5$	0.208	(0.000)			0.100	(0.001)
EV/EV_0			0.548	(0.000)	0.499	(0.000)
BO/EV	1.481	(0.000)	1.730	(0.000)	1.735	(0.000)
Stdev/EV	2.491	(0.000)	2.648	(0.000)	2.687	(0.000)
LL	-1726.0		-1629.9		-1620.5	
MLL	-0.270		-0.255		-0.253	
McFadden R^2	0.309		0.348		0.351	
No. obs.	6393		6393		6393	

relative-stakes model, strengthening our interpretation that decisions are primarily driven by the relative values of the amounts that are at stake, and to a much lesser extent by their absolute monetary values.

2.5 Summary and Concluding Remarks

The TV game show DOND is a natural laboratory for studying risky choice. This chapter examines a unique data set of approximately 6,400 choices from ten different editions and six different countries. The editions employ different sets of prizes, and we use these differences to separate the effects of the absolute and relative amounts at stake on risky choice. In the first part of the analysis, we restrict ourselves to three comparisons of a large-stake edition with a small-stake edition from the same country. This type of analysis avoids the possible systematic differences between countries that may arise when comparing editions from different countries. In the second part of the analysis, we combine the data from the ten different editions and six different countries. Both types of analysis suggest that the choices in DOND are highly sensitive to the context in which they occur, as defined by the initial set of prizes in the game. Contestants respond in a similar way to the relative level of stakes, even though the absolute level of the stakes differs significantly across the various editions. Amounts therefore appear to be primarily evaluated in proportion to a subjective frame of reference rather than in terms of their absolute monetary value.

In an unreported analysis, we find similar results for DOND samples from Hungary, India, Mexico and Thailand. We left these samples out of the original analysis, because effects of culture and wealth are likely to confound the results. It is however comforting to note that the results are in line with the results presented in this study. The estimated coefficients for relative stakes are very similar to those reported here for our editions from developed Western countries. Similar results were also found in classroom experiments with business and economics students and relatively small stakes.

We hope that our results provide a stimulus for the further development and proliferation of reference-dependent choice theory. Our results seem, for example, consistent with the use of a subjective reference point that is proportional to the average prize at the outset of the game. Unfortunately, testing specific choice theories is difficult, among other things, because they involve many degrees of freedom and because it is not immediately clear how the known results for static choice problems generalize to the type of dynamic problems seen in this game show.

Chapter 3:
Random Task Incentive Systems in Risky Choice Experiments[17]

Abstract

This chapter examines the effects of random task incentive systems (RTISs), where only one randomly selected task is played for real, in a dynamic risky choice experiment. We perform three variations of the experiment: one with one task per subject played for real, one with a within-subjects RTIS, and one with a between-subjects RTIS. In the two RTIS designs, a significantly larger proportion of choices are made completely at random. Further, in the within-subjects RTIS design, we find strong carry-over effects from outcomes of previously performed tasks. Risk aversion increases after unfavorable results in the two most recent tasks and decreases after favorable results. The between-subjects design avoids carry-over effects from other tasks, but leads to substantially lower risk aversion than the guaranteed-payment design, implying that there is a bias. The results suggest that caution is warranted when applying RTISs.

3.1 Introduction

Risky choice experiments commonly use a random task incentive system (RTIS) to implement real incentives while avoiding income effects. Early studies that implemented an RTIS include Allais (1953), Tversky (1967a, 1967b), Yaari (1965), Rosett (1971), Smith (1976), Grether and Plott (1979), and Reilly (1982). It is the only incentive system known today that avoids income effects.

In the most common RTIS, each subject performs a series of tasks, knowing that only one of these tasks will be randomly selected at the end to be for real. Although there have been several debates about the validity of the method, it has by now been

[17] This chapter is based on the paper "Random Task Incentive Systems in Risky Choice Experiments", co-authored by Guido Baltussen, Thierry Post, and Peter P. Wakker. We thank conference participants at FUR 2008 Barcelona for useful comments and suggestions, and Nick de Heer for his skillful research assistance. The support by Tinbergen Institute, Erasmus Research Institute of Management and Erasmus Center for Financial Research is gratefully acknowledged.

widely accepted in studies of individual choice in experimental economics (Holt, 1986; Starmer and Sugden, 1991; Lee, 2008).

A second, more radical type of RTIS has been used recently, where not every subject is paid. A subset of the subjects is selected randomly, and only for these subjects one of their tasks, randomly selected, is for real. A drawback of this procedure is that the probability of real play is further reduced, leading to a lower motivation on the part of the subjects. In return, however, higher prizes can be awarded to the subjects who do play for real, which will improve motivation. Examples of studies in the field of risky choice that apply this incentive method are Camerer and Ho (1994), Harrison, Lau and Rutström (2007), and Harrison, List and Towe (2007).

In the latter type of RTIS, there is selection both regarding the tasks and regarding the subjects. To investigate the effects of these two elements of the selection process in isolation, our study will consider, besides the most typical RTIS, a RTIS where there is no selection of tasks. That is, each subject performs only one single task, after which some subjects are randomly selected for whom the outcome of their task will be for real. We call this design the between-subjects (BS) RTIS, and the other, typical design the within-subjects (WS) RTIS. The case where both tasks and subjects are selected is called the hybrid RTIS. An example of a BS RTIS from the field of risky choice experiments is in Tversky and Kahneman (1981), who use it in a robustness check of a hypothetical-stakes experiment.

Often, experiments use real incentives to motivate subjects. Several studies however, particularly those by psychologists, use no monetary incentives at all. Evidence on the validity of such thought experiments for measuring risk aversion generally indicates that subjects are less risk averse. See, for example, Camerer and Hogarth (1999), Holt and Laury (2002), and Rubinstein (2001).

We analyze the effects of RTISs in the setting of a dynamic risky choice experiment that is based on the popular TV game show Deal or No Deal (DOND). This show has received substantial attention from researchers. DOND has been widely recognized as a natural laboratory for studying risky choice behavior (Andersen, Harrison, Lau and Rutström, 2008). The game is dynamic because it uses multiple game rounds and the choice problem in each round depends on the outcomes of earlier rounds. Section 3.3 explains the game in more detail.

We investigate three different treatments. In the first, basic, treatment each subject plays the game once and for real. We will refer to this treatment as the guaranteed-payment design. In the second treatment, subjects play the game ten times, of which one is randomly selected for real payment (WS RTIS design). In the third treatment, each subject plays the game once, with a ten percent chance of real payment (BS RTIS design). We vary only the incentive system. Other factors, including for example the face values of the prizes in the game, are held constant across the treatments. We investigate three potential effects that can occur in a RTIS experiment: biased risk aversion, increased decision errors, and carry-over effects from outcomes of previously performed tasks. The properties of DOND allow us to study the three effects on the basis of this one single type of task. To analyze the two former effects, we contrast the choices in the two RTIS treatments with those in the guaranteed-payment treatment. The carry-over effect appears from comparing the various choices in a WS design. In RTIS experiments, payments are individual and independent of other subjects, and effects of incentive systems based on team or relative performance (van Dijk, Sonnemans and van Winden, 2001) therefore play no role.

We find a significant frequency of errors in the RTIS treatments that are unrelated to the characteristics of the choice problem. Subjects appear to have lapses of concentration and/or refrain from efforts to seriously evaluate the decision problem, resulting in choices that seem to be completely at random. Furthermore, in the WS design, we also observe strong carry-over effects of prior tasks: risk aversion increases after unfavorable outcomes in the two most recent previous games, and decreases after favorable outcomes. On average, risk aversion in the WS RTIS experiment is not significantly different from that in the guaranteed-payment design. The BS design is based on one task per subject and therefore avoids carry-over effects from prior tasks altogether. However, in this design, risk aversion is substantially lower than in the guaranteed-payment treatment, suggesting a serious bias.

This chapter is structured as follows. Section 3.2 provides background and motivation. In Section 3.3, we describe the game of DOND and other aspects of our experiments. Section 3.4 presents results, Section 3.5 discusses these results, and Section 3.6 concludes.

3.2 Background and Motivation

RTISs are known under several other names, including “random lottery incentive system” (Starmer and Sugden, 1991, and several other papers), “random lottery selection method” (Holt, 1986), “random problem selection procedure” (Beattie and Loomes, 1997), and “random round payoff mechanism” (Lee, 2008). The different names apply to particular types of experiments (risky choice or social dilemma), rewards (lotteries or outcomes) or tasks (composite or single-choice) only. Clearly, there is no convention about the name of the method. We use “random task incentive system”, because this name describes the method for any type of experiment, reward and task.

First, we will discuss whether RTISs influence subjects’ degree of risk aversion. Holt (1986) raised a serious concern against WS RTISs that will hold at least in theory. Subjects may not perceive each choice in the experiment in isolation. Rather, they may perceive the whole experiment as a meta-lottery, entailing a probability distribution over the different choices and their resulting outcomes. Such a perception may lead to distortions if the subjects violate the independence condition of expected utility. Violations of the latter condition have been widely documented (Allais, 1953; Starmer, 2000). Isolation entails that subjects, to the contrary, take each choice in the experiment for real and as the only real, leading to proper experimental measurements.

Milder forms of the problem of reduction are also conceivable, where subjects do not fully integrate all choices and the RTIS lottery but nevertheless the choices do affect each other, leading to some distortions still (partial reduction). Cubitt, Starmer and Sugden (1998a) also argue that there can be differences between subjects in the way they treat the experiment, and that even if most subjects may treat each task in isolation, a minority may do reduce the whole experiment to a single problem.

The validity of the WS RTIS method has been empirically analyzed in several studies. In a cleverly designed experiment based on simple, pairwise decision problems, Starmer and Sugden (1991) find isolation verified. However, in a direct comparison of the choices in a RTIS treatment with those in a guaranteed-payment sample they find a marginally significant difference. This difference is not confirmed in later studies: in further investigations with more subjects, Beattie and Loomes (1997), Cubitt, Starmer and Sugden (1998a), and Laury (2006) conclude that, for simple, pairwise decisions, there is no evidence of reduction or different risk aversion in RTIS tasks than in tasks with guaranteed payment. Camerer (1989) also finds that WS RTISs elicit true preferences. Virtually every subject in his experiment held on to her earlier choice when she was allowed to change her decision after the gamble to be played for real had been determined. Hey and Lee (2005a, 2005b) compare the empirical fit of various preference specifications under reduction with the fit under isolation, and conclude in favor of isolation.[18] All in all, the afore-mentioned studies are supportive of WS RTIS for simple binary choices.

In a pure BS experiment, each subject performs only one single task. In his lecture for the Nobel Foundation, Kahneman (2002) argues for the use of this design to study preferences. When a RTIS is employed in a BS experiment, only a fraction of the subjects are paid for their task. The concern of Holt (1986) also applies to BS RTISs, and biased risk preferences may result if subjects integrate the choice problem they face with the RTIS lottery. Relative to the WS design, the BS design may be more susceptible to reduction. Reduction now only requires straightforward multiplication of the probabilities of one task with the probability of real payment and no other tasks have to be taken into account. There have not been many investigations into the performance of the BS RTIS for risky choices. The only test we are aware of is in Cubitt, Starmer and Sugden (1998b). Using a simple binary choice problem, they find a marginally significant difference, with lower risk aversion in the RTIS treatment.

In our experiments, we hold the face values of the prizes constant and only vary the incentive system that is used. Because payment of the outcome of a task is not sure in the RTIS designs, a subject's expected reward for solving a decision problem is smaller than her reward in the case of guaranteed payment. Besides possibly biased risk attitudes, another effect might therefore occur in our RTIS treatments: when the benefits of incurring decision costs are smaller, subjects may be less motivated to consider choice problems profoundly, resulting in more choices that are driven by errors (for theoretical models and literature surveys, see Camerer and Hogarth, 1999, Smith and

[18] The two papers of Hey and Lee differ mainly in the assumptions about the range of tasks that subjects combine into one composite gamble if reduction would occur. In the first paper they assume that subjects are only aware of previous tasks and the choices they made so far, and that subjects therefore only take those previous decisions into account when making their current decision. In the second paper they assume that subjects are aware of all tasks and therefore that subjects can also take future decisions into account.

Walker, 1993, and Wilcox, 1993). For tasks with, for example, a one-in-ten chance of being selected, the expected reward is ten times smaller when the nominal stakes are held constant, whereas subjects' costs of discovering optimal choices are not affected. Most prior investigations of RTISs used simple choice problems that require little mental effort, and therefore they may have avoided the potential effects of reward dilution. Many risky choice problems (including the dynamic choice problems used in our experiment) are however cognitively more demanding and therefore increase the importance of incentives as a means to raise subjects' willingness to incur decision costs and reduce errors.

In experiments, Wilcox (1993) found that the probability of task selection (i.e., the strength of incentives) in a WS RTIS design is not important if choices concern simple, one-stage lotteries. For more complex two-stage lotteries (that are distributionally equivalent to the one-stage lotteries), an increased probability did, on the contrary, improve decisions. Apparently, the higher expected payoff per task encourages subjects to spend more efforts. Moffatt (2005) also confirms that higher incentives bring about an increase in efforts and that subjects are discouraged by complex tasks.

Unfortunately, the effects of decision errors do not always cancel out in a large sample. For example, if the least risky alternative is optimal for a given subject, errors can only increase observed risk aversion. Harrison (1989, 1994) raises concerns that diluted rewards may even have resulted in inaccurate inferences in the literature about the (in)validity of expected utility theory.

Further, if reward dilution is an issue in RTIS experiments, the effect may not be constant across the different tasks of a WS design. With every task, subjects gain experience in making decisions, and, with experience, decision costs decrease, resulting in fewer errors (Smith and Walker, 1993). Subjects also become more familiar with the software and/or devices, further decreasing the likelihood of errors. On the other hand, subjects in experiments with repeated tasks may become bored, resulting in reduced concentration and increased error variance.

Prior analyses of the validity of RTISs almost exclusively concerned static choice problems, in which each task requires the subjects to choose between two simple lotteries. Many actual choice problems are, however, dynamic, consisting of multiple sub-problems with intermediate decisions and outcomes. In an experimental setting where each subject performs a series of dynamic choice tasks and where each task is paid for real, outcomes of prior tasks may distort the following choices because of an income effect (see, for example, Cox and Epstein, 1989). In this respect, WS RTIS has an additional advantage over using real monetary rewards for each task. Because only one task is randomly chosen at the end of the experiment, there is no accumulation of payoffs from the different tasks, implying that potential income effects are eschewed. Modern reference-dependent decision theories such as prospect theory suggest that outcomes of earlier tasks may generate a reference-point effect. Thaler and Johnson (1990) show that risk aversion indeed depends on previous gains and losses. Also, changes in the expected value of a subject's payment from the experiment resulting from favorable or unfavorable earlier tasks may change a subject's risk aversion (Grether and Plott, 1979, p.630). Lee (2008) finds that income effects are present in his guaranteed-payment design, and absent in his treatment that employs a WS RTIS. However, Lee required subjects to perform two different types of tasks in each of ten subsequent rounds of his experiment, and only analyzed the impact of the incentive method used for the second type of tasks on decisions in the first type. Thus, he did not investigate potential correlation between prior outcomes and decisions in one given and repeated type of task. Whether or not prior intermediate outcomes in a WS RTIS experiment affect choice behavior will be examined in our study.

We examine the effects of RTISs for risky choices. Various other studies analyze this incentive method in other fields. Bolle (1990) and Sefton (1992) analyze the BS RTIS. Bolle considers ultimatum games, and finds that behavior in the BS RTIS treatment is not different from real tasks. Sefton, on the contrary, finds that a BS RTIS does affect behavior in experiments with dictator games. Papers analyzing hybrid RTISs in other fields than risky choice are Armantier (2006), Harrison, Lau and Williams (2002), and Stahl and Haruvy (2006). Armantier performs an ultimatum-game experiment and finds that behavior is not different from behavior in a WS RTIS design. Harrison, Lau and Williams conduct an experiment designed to elicit discount rates and find no effect of the magnitude of the probability of selection across subjects. For dictator experiments, Stahl and Haruvy find that a hybrid RTIS leads to significant differences.

3.3 The Experiment

Our experiment mimics the choice problems in the TV game show Deal or No Deal (DOND). Other studies using experiments based on this TV show include Andersen, Harrison, Lau and Rutström (2008), Deck, Lee, Reyes and Rosen (2008), and Post, Van den Assem, Baltussen and Thaler (2008).

In every game round of DOND, the subject has to choose between a sure alternative and a risky lottery with known probabilities. DOND requires no special skills or knowledge. At the start of a game, the contestant chooses one case out of a total of 26 numbered cases, each hiding one out of 26 randomly distributed amounts.[19] The contestant does not know the content of the chosen case. Next, she has to select 6 of the other 25 cases to be opened, revealing their prizes, and revealing that these prizes are not in the case chosen by the contestant. Then, the banker specifies a price for which he is willing to buy the contestant's case. If the contestant decides "No Deal", she enters the second round and has to open 5 additional cases, followed by a new bank offer. The game continues this way until the contestant either accepts an offer, or rejects all offers and receives the contents of her own case. The maximum number of game rounds to be played is 9, and the number of cases to be opened in each round is 6, 5, 4, 3, 2, 1, 1, 1, and 1, reducing the number of remaining cases from 26 to 20, 15, 11, 8, 6, 5, 4, 3, and, finally, 2.

Our experiment uses the 26 prizes of the original Dutch edition, scaled down by a factor of 10,000, with the lowest amounts rounded up to one cent. The resulting set of prizes is: €0.01 (9 times); €0.05; €0.10; €0.25; €0.50; €0.75; €1; €2.50; €5; €7.50; €10; €20; €30; €40; €50; €100; €250; €500. The distribution of prizes is clearly positively skewed, and has a median of €0.63 and a mean of €39.14.

In the TV episodes, the bank offer starts from a small fraction of the average remaining prize in the early game rounds and approaches the average remaining prize in the last few game rounds. Although the bank offers can be predicted accurately (Post *et al*., 2008), we eliminate any remaining ambiguity by fixing the percentage bank offer in

[19] Deal or No Deal is aired in dozens of countries, sometimes under a different name. There are many editions of the show, each with its own set of prizes, but the basic structure and the game rules are always similar. Some editions use a different number of cases and game rounds (for example, 22 cases and 6 game rounds in the UK). Baltussen, Post and Van den Assem (2007) includes descriptions and analyses of ten different editions.

each game round and by including these percentages in the instructions handed out to the subjects. The percentages for round 1 to 9 are 15, 30, 45, 60, 70, 80, 90, 100 and 100, respectively. The resulting monetary offers are rounded to the nearest cent.

We consider three variations to the same experiment that differ only in the incentive system used. The first experiment uses guaranteed payment. Each subject plays the game once and for real. In the second, or WS RTIS treatment, subjects play the game ten times. At the end of this treatment, one out of the ten different outcomes is randomly selected for real payment. In the third, or BS RTIS treatment, subjects play the game once with a ten percent chance of real payment.

The different treatments all draw from the same student population. For each experiment we randomly selected first-year Economics students at the Erasmus University of Rotterdam. Students were not allowed to participate in more than one treatment. Given the random allocation of subjects to the different subject pools and the homogeneous population of first-year economics students, the different groups are likely to be very similar.

A research assistant developed computer software that generates random scenarios, displays the game situations, provides a user interface, and stores the game situations and choices of the subjects. All treatments of the experiment were conducted in a computerized laboratory and run in sessions with about 20 students sitting behind 20 computer terminals simultaneously. To mitigate the ability to see what other subjects are doing and to provide a quiet environment, subjects were separated from each other with at least one unoccupied computer in between them.

Before the experiment actually started, subjects were given ample time to read the instructions and they were offered the opportunity to pose questions (we allowed subjects to consult the instructions during the experiment). We next explained the relevant payment procedure. In the case of an RTIS treatment, we explained that choices were to be made as if payment would be for sure with each task. At the end of each session, the relevant payment procedure was carried out. In both RTIS treatments, a ten-sided die was thrown individually by each subject to determine her payment. The instructions of the WS RTIS treatment are in the appendix. The instructions to the other two treatments are similar, apart from the details about the number of games to be played and the incentive scheme.

3.4 Results

A total of 97 subjects participated in the guaranteed-payment treatment, 100 participated in the BS RTIS treatment, and 88 participated in the WS RTIS treatment. In the analyses below, we removed all games that ended up with trivial choice problems involving prizes of one cent only.

Analysis of the Stop Round

A rough way to compare the risk aversion across the different treatments is obtained by comparing the round in which contestants accepted a bank offer ("Deal"). After all, the bank offer becomes increasingly attractive as the game progresses, and therefore the longer a subject continues to play, the less risk averse she is likely to be. Table 3.1 reports summary statistics of the stop round for the three treatments (Panel A), including separate statistics for each of the ten successive games of the WS treatment (Panel B). For subjects who rejected all offers and played the game until the end, we set the stop round equal to 10. Figure 3.1 shows histograms of the stop round in the guaranteed-payment, WS, and BS design, respectively.

To investigate the effects of the various RTIS designs on the degree of risk aversion, we compare the choices in the two RTIS treatments with those in the guaranteed-payment treatment.

The averages of the stop round in the WS and guaranteed-payment treatment are roughly similar. Measured across the ten different games in the WS treatment, the average subject accepts a bank offer ("Deal") in round 7.41, compared to 7.87 in the single-task design with guaranteed payment. Using a two-sided independent t-test shows that the difference is only marginally significant at the 10 percent level ($t_{112.5} = 1.794$, $p = 0.076$). Arguably, the decisions in the last nine games of the WS design may be contaminated by carry-over effects from the outcomes of earlier games, and/or affected by more familiarity with the task or boredom. If we drop the last nine games from the comparison and use the first game only, the difference (7.13 vs. 7.87) is larger but still statistically insignificant ($t_{153.5} = 1.931$, $p = 0.055$). (The lowest average stop round is recorded for the first game, but the average of this game is not significantly different from the rest.) Strictly speaking, this first analysis of the WS RTIS design yields no statistically significant evidence of biasedness.

Table 3.1: Summary statistics of the stop round. The table shows summary statistics of the stop round for the various treatments (Panel A), separate statistics for the ten successive games in the WS treatment (Panel B), and separate statistics for games in the WS treatment subdivided on the basis of the outcome of a prior task (Panel C). The stop round is the round number in which the bank offer is accepted ("Deal"), or 10 for subjects who rejected all offers. In the guaranteed-payment treatment (Guaranteed), subjects play the game once and for real. In the between-subjects RTIS treatment (BS RTIS), subjects play the game once with a ten percent chance of real payment. In the within-subjects RTIS treatment (WS RTIS), subjects play the game ten times with a random selection of one of the ten outcomes for real payment. Shown are the mean, the standard deviation (Stdev), the 25th percentile (P25), the median, the 75th percentile (P75) and the number of observations (No. obs.).

	Mean	Stdev	P25	Median	P75	No. obs.
A. Overall						
Guaranteed	7.87	2.29	6.00	8.00	10.00	91
BS RTIS	8.61	1.84	8.00	9.00	10.00	94
WS RTIS	7.41	2.35	6.00	8.00	10.00	808
B. WS RTIS conditional on the task number						
Game 1	7.13	2.64	5.00	8.00	9.00	78
Game 2	7.33	2.50	5.00	8.00	10.00	82
Game 3	7.37	2.31	6.00	7.50	10.00	76
Game 4	7.36	2.37	6.00	8.00	10.00	80
Game 5	7.28	2.41	5.00	8.00	10.00	78
Game 6	7.31	2.28	5.50	8.00	9.00	84
Game 7	7.81	2.38	7.00	8.00	10.00	80
Game 8	7.77	2.14	6.00	8.00	10.00	82
Game 9	7.35	2.18	6.00	8.00	9.00	83
Game 10	7.41	2.29	6.00	8.00	9.00	85
C. WS RTIS conditional on the outcome of a prior task						
$EV_{-1} \leq 39.14$	7.27	2.31	5.00	8.00	9.00	376
$EV_{-1} > 39.14$	7.66	2.34	6.00	8.00	10.00	291
$EV_{-2} \leq 39.14$	7.36	2.29	6.00	8.00	9.00	327
$EV_{-2} > 39.14$	7.68	2.29	6.00	8.00	10.00	265
$EV_{-3} \leq 39.14$	7.48	2.29	6.00	8.00	10.00	288
$EV_{-3} > 39.14$	7.45	2.37	6.00	8.00	10.00	232
$EV_{-4} \leq 39.14$	7.49	2.32	6.00	8.00	10.00	240
$EV_{-4} > 39.14$	7.39	2.27	6.00	8.00	9.00	207

The BS design, to the contrary, does seem to yield significantly biased measurements of risk aversion. Subjects in the BS design choose to play longer: on average they accept a bank offer ("Deal") in round 8.61, compared to 7.87 for the treatment with guaranteed payment ($t_{172.5} = 2.416$, $p = 0.017$). Nearly two-thirds of the subjects display risk-seeking behavior by rejecting actuarially fair offers. This rough way of analyzing suggests that the BS design generates a lower degree of risk aversion than the guaranteed-payment experiment.

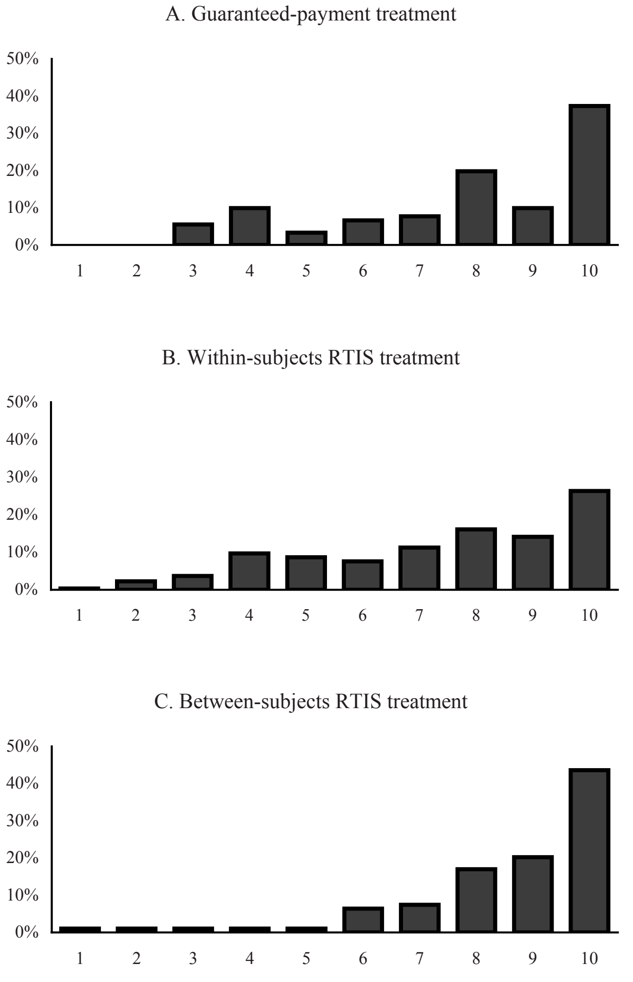


Figure 3.1: Histograms of the stop round. The figure shows histograms of the stop round in the guaranteed-payment treatment (Panel A), in the WS RTIS treatment (Panel B) and in the BS RTIS treatment (Panel C). The stop round is the round number in which the bank offer is accepted ("Deal"), or 10 for subjects who rejected all offers. In the guaranteed-payment treatment, subjects play the game once and for real. In the WS RTIS treatment, subjects play the game ten times with a random selection of one of the ten outcomes for real payment. In the BS RTIS treatment, subjects play the game once with a ten percent chance of real payment.

The above analyses of the average stop round do not correct for the potential influence of errors. Errors are likely to have an asymmetric effect in our experiment and to reduce the average stop round. Erroneous "Deal" decisions immediately end the game, whereas erroneous "No Deal" decisions may lead to only one extra round because the subject can stop the round after. If errors are more likely to occur in a RTIS design, this may bias our comparison of the average stop round. For the BS design, controlling for an asymmetric effect of errors would aggravate the difference with the guaranteed-payment design. For the WS design, however, doing so would reduce the difference. The histograms (Figure 3.1) do indeed yield some indications of increased errors in the treatments with a RTIS. The dispersion of the stop round seems to be larger in both the WS and the BS design. Some decisions appear to be made without any regard to the attractiveness of the alternative choice option. Accepting, for example, a bank offer of 15 or 30 percent (in round 1 and 2, respectively) of the mean of the remaining prizes implies an implausible degree of risk aversion if we would assume that this decision is really carefully considered. In the probit regression analyses of the next paragraph we will analyze the likelihood and effect of increased errors more thoroughly.

To obtain a first indication of whether subjects' choices are dependent on the outcomes of previous tasks, we compare the stop round of tasks preceded by tasks that unfolded favorably with those that unfolded less favorably. To classify the outcomes of prior tasks, we focus on the average remaining prize in the last game round (or the prize in the subject's own briefcase if she rejected all bank offers), and we use the statistical average of the prizes present at the start of each game (€39.14) to create a division into "good" and "bad" outcomes.

Panel C of Table 3.1 presents the results. The outcome of the latest prior task appears to have a significant influence on a subject's choices in the current task. When the previous task ended with stakes below €39.14, the average stop round is 7.27, whereas the average stop round after a previous task that ended with larger stakes is 7.66. The difference is significant ($t_{619.6} = 2.106$, $p = 0.036$). The sign of the effect of the penultimate prior task is similar, but the effect is weaker and only marginally significant (7.36 versus 7.68; $t_{564.9} = 1.669$, $p = 0.096$). The outcomes of games played more than two games earlier do not seem to have any effect on choice behavior in the current game.

Our approach to classify subjects into groups characterized by favorable and unfavorable prior outcomes may also pick up differences in risk aversion across subjects. More adventurous subjects play longer, and, due to the skewness of the prizes, subjects who play longer are more likely to end up with below-average stakes. However, based on this argument, we would expect subjects with below-average prior outcomes to take more risk, i.e., play more game rounds, which is opposite to what we observe.

To summarize, the preliminary stop-round analysis suggests that the average degree of risk aversion in the WS design is roughly unaffected, but differences in risk aversion seem to be related to the outcomes of preceding tasks. Furthermore, errors appear to occur more frequently. The alternative BS design is likely to be biased towards lower risk aversion, and appears to suffer from a similar increase in errors. The next subsection presents a more sophisticated analysis of the effects of RTISs, and confirms these findings.

Probit Regression Analysis

An analysis of the stop round is crude and does not control for differences between the various choice problems such as differences in the amounts at stake. In this subsection, we use probit regression analysis to explain the DOND choices in the various samples, while correcting for the characteristics of the choice problems. The dependent variable is the subject's decision, with a value of 1 for "Deal" and 0 for "No Deal". We estimate two regression models. Both models use the dummy variables D^{WS} and D^{BS} to measure the effects of the different treatments. In the first model, we try to explain the various choices using the following set of variables:

- D^{WS}: dummy variable indicating that the choice is made in the WS treatment (1 = WS RTIS);
- D^{BS}: dummy variable indicating that the choice is made in the BS treatment (1 = BS RTIS);
- EV/100: stakes, measured as the current average remaining prize divided by 100 Euros;

- EV/BO: expected relative return (+1) from rejecting the current and subsequent bank offers, or the average remaining prize divided by the bank offer;
- Stdev/EV: standard deviation ratio, or standard deviation of the distribution of the average remaining prize in the next round divided by the average remaining prize.

The stakes are divided by 100 Euros to obtain more convenient regression coefficients. To control for the attractiveness of the bank offer, we use the expected return from rejecting the current and subsequent bank offers. The standard deviation ratio measures the risk of continuing to play ("No Deal") for one additional round. To allow for the possibility that the errors of individual subjects are correlated, we perform a cluster correction on the standard errors (Wooldridge, 2003). In order to avoid confounding effects of outcomes of prior tasks, we only use the first game of the WS design. The sample used for the regression consists of a total of 1977 choice observations, of which 677 observations are from the guaranteed-payment treatment, 766 from the BS treatment and 534 from the WS treatment.

The first column of Table 3.2 shows the probit estimation results. As expected for non-satiable and risk averse individuals, the "Deal" propensity increases with the generosity of the bank offer and with the dispersion of the outcomes. The "Deal" propensity also increases with the stakes, consistent with increasing relative risk aversion.

The WS dummy is significantly larger than zero ($z = 2.551$, $p = 0.011$), indicating a higher deal propensity in this treatment than in the guaranteed-payment treatment. The BS dummy is negative, but statistically only marginally significant ($z = -1.676$, $p = 0.094$). Although the signs of the treatment effects are similar to the preliminary findings in the stop-round analysis, the statistical significance has now been reversed.

The above analysis does not account for the possible effects of tremble, or the subject losing concentration and choosing completely at random. Such errors would introduce a relatively large number of "Deals" in the early game rounds when "Deals" cannot reasonably be explained by risk aversion or errors in weighing the attractiveness of "Deal" and "No Deal" against each other, because the bank offers are very

Table 3.2: Probit regression results: treatment effects. The table displays the results from the probit regression analyses of the DOND decisions in the three different treatments. In the guaranteed-payment treatment, subjects play the game once and for real. In the BS RTIS treatment, subjects play the game once with a ten percent chance of real payment. In the WS RTIS treatment, subjects play the game ten times with a random selection of one of the ten outcomes for real payment. The dependent variable is the contestant's decision, with a value of 1 for "Deal" and 0 for "No Deal". EV is the current average remaining prize in Euros. BO is the bank offer. Stdev measures the standard deviation of the distribution of the average remaining prize in the next game round. D^{WS} (D^{BS}) is a dummy variable that takes a value of 1 for observations from the WS (BS) RTIS treatment. Tremble is the estimated probability that a choice is made at random. Apart from the maximum likelihood estimates for the regression coefficients, the table reports the log-likelihood (LL), the mean log-likelihood (MLL), McFadden's R-squared, and the number of observations (No. obs.). The *p*-values (within parentheses) for the regression coefficients are corrected for correlation between the responses of a given subject (subject-level cluster correction). The *p*-values for the tremble probabilities are based on likelihood ratio tests.

	Model 1	Model 2
Constant	-2.284 (0.000)	-1.843 (0.000)
D^{WS}	0.288 (0.011)	0.230 (0.104)
D^{BS}	-0.184 (0.094)	-0.258 (0.030)
EV/100	0.753 (0.000)	0.767 (0.000)
EV/BO	-0.244 (0.002)	-0.496 (0.001)
Stdev/EV	1.964 (0.000)	1.801 (0.000)
Tremble: Constant		0.000 (1.000)
D^{WS}		0.027 (0.003)
D^{BS}		0.009 (0.007)
LL	-458.2	-453.4
MLL	-0.232	-0.229
McFadden R^2	0.213	0.221
No. obs.	1977	1977

conservative in those rounds. The relatively large number of early "Deals" in the preliminary analysis of the stop round indeed suggests that tremble is relevant in this experiment.

For discussions of the interpretation and modeling of stochastic elements in risky choice experiments we refer to Harless and Camerer (1994), Hey (1995), Hey and Orme (1994), Loomes and Sugden (1995), Luce and Suppes (1965), and Wilcox (2008).

To account for tremble, we extend our probit model by adding a tremble probability (Harless and Camerer, 1994). For a discussion of the method of including a tremble probability in a binary choice model, see Loomes, Moffatt and Sugden (2002) and Moffatt and Peters (2001). We allow for different tremble probabilities in the three treatments by modeling the tremble probability, ω, as $\omega = \omega_0 + \omega_1 D^{WS} + \omega_2 D^{BS}$. The constant ω_0 represents the tremble probability in the guaranteed-payment design, and the parameters ω_1 and ω_2 represent the deviations of the tremble probabilities for the WS and BS design, respectively. Following the recommendation of Moffatt and Peters, we calculate the *p*-values for the tremble probabilities on the basis of likelihood ratio tests. Since the tremble parameter is restricted to be nonnegative, a test for tremble is one-sided, and the restricted *p*-value is obtained by dividing the unrestricted *p*-value by a value of two.

The second column of Table 3.2 presents the results of this second model. The tremble probability in the guaranteed-payment design is virtually zero ($4.83 \cdot 10^{-7}$). In both RTIS treatments, however, the presence of a tremble probability is statistically significant. In the WS treatment, the tremble probability equals 2.7 percent ($LR_1 = 7.653$, $p = 0.003$), implying that about 14 of the 534 choices in this design can be labeled as random decisions. In the BS design, the tremble probability is smaller, 0.9 percent ($LR_1 = 6.115$, $p = 0.007$), and corresponds with 7 out of 766 choices. The difference between the tremble probabilities in the RTIS treatments is statistically significant ($LR_1 = 3.078$, $p = 0.040$). Adding the three tremble probabilities significantly improves the fit of the regression model. A likelihood ratio test yields a *p*-value of 0.011 ($LR_3 = 9.622$) for their combined effect.

Interestingly, after correcting for trembles, the WS dummy is no longer significantly larger than zero ($z = 1.624$, $p = 0.104$), indicating unbiased risk aversion in this treatment. The BS dummy on the other hand gains statistical significance and is now significantly negative ($z = -2.169$, $p = 0.030$), implying that this RTIS stimulates less risk averse choices. (Note that the absolute values of both coefficients are roughly equal, suggesting opposite biases of similar strength. Statistical inference for the WS dummy seems to be affected by overlap in the effects of increasing the dummy coefficient and increasing the tremble probability. Both yield a decrease in the predicted stop round.) Clearly, correcting for the asymmetric impact of errors is important in our experimental analysis.

We also estimated a model that allows for differences in the standard noise term between the different treatments, but found no significant improvement of the fit. The standard noise term represents errors in weighing the attractiveness of “Deal” and “No Deal” against each other and particularly helps to understand decision errors that occur when the subject is almost indifferent between the two choices. It can explain for example why a subject stops in game round 6 or 8 when stopping in round 7 would be optimal. Tremble helps to understand why a moderately risk averse subject would sometimes stop in the early game rounds when the bank offers are very conservative. In our experiments, the RTIS seems to generate additional tremble rather than standard noise. The loss of concentration arguably reflects the lower expected monetary reward per trial in the RTIS treatments compared to guaranteed payment.

A typical feature of dynamic choice problems is that intermediate outcomes have to be shown. An experiment that uses a WS RTIS consists of multiple tasks, and, as discussed in Section 3.2, intermediate outcomes of earlier tasks may influence a subject's choices in a given task. (In our experiment, the information available to subjects is not limited to intermediate outcomes. Also the final outcome of a prior task is known.) The preliminary analysis of the stop round indeed yielded some indications of such an effect. Using choice data from the WS treatment, we will further analyze whether carry-over effects occur between subsequent tasks. We therefore expand the probit regression model with variables that capture the outcomes of previous tasks.

To quantify the outcome of a prior task, we employ a proxy for the winnings. Using the actual winnings (the accepted bank offer) would introduce a bias in the regression coefficients, because this variable picks up heterogeneity in risk attitudes between subjects: more risk averse subjects are more likely to say "Deal" in a given round of the current game, and, at the same time, more risk averse subjects have won smaller amounts in prior games because they have accepted lower percentage bank offers. To avoid spurious correlation, we quantify a subject's winnings in the k^{th} preceding game by the average remaining prize in the last game round (or the prize in the subject's own briefcase if she rejected all bank offers), EV_{-k}. To obtain convenient coefficients, we divide this variable by 100 Euros. We include the outcomes of the four most recent prior tasks, i.e., $k = 1,\cdots,4$. Missing values in the case of early tasks are set equal to the sample average. (We also ran the regression on a smaller sample that excludes the observations with missing values for prior outcomes. The results are similar.)

The first column of Table 3.3 presents the probit regression results. The outcomes

Table 3.3: Probit regression results: carry-over and tremble effects across tasks. The table displays the results from the probit regression analyses of the DOND decisions in the ten different games of the WS RTIS treatment. EV_{-k} (k = 1,2,3,4) is the average remaining prize in the last round of the k^{th} game preceding the current game. Missing values for this variable are set equal to the sample average. Model 1 assumes a constant tremble probability across the different tasks, and Model 2 assumes a log-linear pattern. Other definitions are similar to Table 3.2.

	Model 1	Model 2
Constant	-1.182 (0.000)	-1.170 (0.000)
EV/100	0.812 (0.000)	0.805 (0.000)
EV/BO	-0.613 (0.000)	-0.616 (0.005)
Stdev/EV	1.610 (0.000)	1.594 (0.000)
$EV_{-1}/100$	-0.204 (0.001)	-0.200 (0.001)
$EV_{-2}/100$	-0.197 (0.001)	-0.193 (0.001)
$EV_{-3}/100$	-0.052 (0.390)	-0.053 (0.384)
$EV_{-4}/100$	-0.029 (0.683)	-0.025 (0.720)
Tremble: Constant	0.011 (0.000)	0.028 (0.000)
Ln(Task)		-0.011 (0.029)
LL	-1531.2	-1529.4
MLL	-0.266	-0.266
McFadden R^2	0.200	0.201
No. obs.	5756	5756

of the two most recent tasks, EV_{-1} and EV_{-2}, strongly influence the "Deal" propensity in the current task: the larger the prior winnings, the less a subject is inclined to accept the sure alternative, i.e., the more risk she takes. The effects of the other two lags are economically and statistically insignificant.

Note that the tremble probability (1.1 percent) is again significant ($LR_1 = 20.887$, $p = 0.000$), but clearly smaller than the tremble probability estimated for the first task (2.7 percent, see Table 3.2). Because the tremble probability is now estimated across the ten different tasks, this difference suggests that the likelihood of trembles decreases during the experiment. To further investigate this possibility, we decompose the tremble probability into a constant that represents the tremble in the first task, and a term that varies log-linearly with the task number, i.e., $\omega = \omega_0 + \omega_1 \log(\text{Task})$. Column 2 shows the estimation results. The log-linear term is negative and significant ($LR_1 = 3.608$, $p = 0.029$), confirming a decreasing pattern of the tremble probability. The constant (2.8 percent) is almost equal to the tremble probability estimated for the first task separately (2.7 percent). If we replace the log-linear term by a linear term, i.e., $\omega = \omega_0 + \omega_1 (\text{Task})$, the significance of the linear component deteriorates ($LR_1 = 2.711$, $p = 0.050$;

untabulated), suggesting that the largest effect of gaining experience occurs during the first few tasks.

3.5 Discussion

Our results suggest that the likelihood of random choice is larger in the RTIS designs than in the guaranteed-payment design. We find that both the WS and the BS RTIS treatment yield decisions that can be described as trembles, or errors of which the likelihood is independent of the characteristics of the decision problem. Not correcting for these errors would increase the estimated degree of risk aversion in our experiment. Of course, the trembles in the RTIS treatments are not likely to be a consequence of RTIS *per se*, and they will probably also emerge if we would decimate the prizes in our guaranteed-payment design. The tremble probabilities in the WS and BS design are significantly different and estimated to be about three and one percent, respectively. This difference may result from the different number of tasks that subjects face. In the single task of the BS design, a subject's performance depends on that one task only. The WS design consists of a much larger number of tasks, which may weaken a subject's attention, and subjects may also regard the first few tasks as learning opportunities. If we look at the tremble probability across the series of repetitions of the task, then we find that the probability decreases with the number of tasks performed. Apparently, the frequency of trembles is negatively related to subjects' decision-making experience, a conclusion that confirms the prediction of Smith and Walker (1993, p.251), and earlier findings of Moffatt and Peters (2001), and Loomes, Moffatt and Peters (2002). The effect of boredom from repetition would work in the opposite direction, but seems to be absent or mitigated due to the popularity of the game and/or the random course and therefore different properties of each task.

Consistent with the findings of most earlier investigations of the WS RTIS, the general level of risk-aversion in our WS design appears to be roughly similar to that in our guaranteed-payment design. Only at marginal levels of significance, our analyses show signs of a bias in the direction of increased risk aversion. In contrast, the BS treatment yields results that are significantly biased towards less risk averse behavior. Subjects in this design play the game longer, and the probit analysis also shows that subjects display less risk averse behavior. Reduction can affect behavior if independence is violated, especially if reduction strongly affects the probabilities. In a BS design, integration of the choice problem and the RTIS lottery would be relatively easy, because only one choice problem is involved and the reduction of each alternative requires only straightforward multiplication of the probabilities in the choice problem with the probability of payout. With a payout ratio of 10 percent, a 50 percent chance at €100 would be perceived as a 5% gamble at €100. Since small probabilities tend to be overweighed, this generally has the effect of encouraging risk taking in BS RTIS experiments. In a WS design, reduction is substantially more complex. It is unlikely that the average subject could perform this complex task by heart in a few seconds. It seems more likely that the complexity of the meta-lottery introduces a sense of ambiguity in a WS design, and that this ambiguity explains the weak indication of increased risk aversion in our results.

Subjects in our WS treatment are clearly influenced by the outcomes of prior tasks: a substantial part of the variation in risk attitudes across subjects and tasks in our WS treatment can be explained by the outcomes of the two most recent previous games. The larger the winnings in those earlier tasks, the lower a subject's aversion to risk. A possible explanation for these cross-task contamination effects can be found in the work of Thaler and Johnson (1990). Thaler and Johnson show that subjects display less risk aversion after previous losses when they have a chance to break even, the so-called "break-even effect", and higher risk aversion when they cannot break even. Subjects in our experiment cannot recover from the unfavorable outcome of a previous game, and they will have to accept the possibility that this unfavorable outcome is selected for payout. Thus, the higher risk aversion after unfavorable outcomes in earlier games seems consistent with the results of Thaler and Johnson. Similarly, Thaler and Johnson also show that subjects display a relatively low degree of risk aversion after gains when these gains cannot be entirely lost, the so-called "house-money effect". The low risk aversion that we observed after favorable outcomes in previous games is consistent with this phenomenon. Alternatively, one could argue that the cross-task contamination that we observe can be regarded as an income effect. However, income effects cannot explain why only the last two previous games affect decisions in the current game, since a subject's expected income from the experiment is determined by the outcomes of every game.

In a WS experiment with static choice problems, carry-over effects from outcomes of previously performed tasks can easily be avoided by simply postponing the presentation of outcomes until all tasks have been completed. Intermediate outcomes are, however, an inevitable feature of dynamic choice problems. Subjects in our DOND experiment, for example, need to know at any stage of the game which prizes are eliminated and which prizes remain. Our results indicate that the effects of prior tasks are short-lived. A researcher who wants to analyze dynamic risky choice in a WS design may therefore attempt to avoid carry-over effects by, for example, interposing dummy tasks between the tasks of interest.

We did not explicitly analyze a design that combines the WS and the BS variant of the RTIS. In such a hybrid design, one of each subject's tasks is randomly selected and a subset of the contestants is then randomly selected for real payment. Assuming that the same number of tasks is performed, the costs of the hybrid design are only a fraction of the costs of the WS design. Harrison, Lau and Rutström (2007, footnote 16) deal briefly with a comparison of risky choices in a hybrid and a WS scheme, and find that the two designs yield no differences in revealed risk preferences for simple pairwise static choice problems. Abdellaoui, Baillon and Wakker (2007) interviewed subjects in a pilot study and asked which RTIS motivates them better: one large prize for one subject in a hybrid design or one moderate prize per subject in a WS design, holding the total prize pool constant. The majority pointed out that the hybrid design motivates them best, which is why the authors chose this design even though they could also have implemented the WS design. Future research may further analyze the hybrid RTIS. Based on our results, it seems likely that the hybrid design suffers from the same path-dependence as the WS design, from a similar bias towards lower risk aversion as the BS design, and from the higher frequency of trembles we observed in both the WS and the BS design.

3.6 Conclusions

When we compare RTIS treatments with a guaranteed-payment treatment and hold the face values of the prizes constant, a BS RTIS design generates downwardly biased measurements of risk aversion and increases the likelihood of decision errors. The increased errors are likely to reflect lapses of concentration, caused by the less favorable tradeoff between the cost and benefits of decision efforts. Of course, the advantages of using a guaranteed-payment approach in single choice designs should be balanced against the smaller number of choice observations that can be obtained with a given budget. Experimentalists who consider using the WS RTIS to study dynamic risky choice problems should be aware of possible carry-over effects from showing intermediate outcomes of previous tasks. Risk aversion increases after recent unfavorable results and decreases after recent favorable results. Like the BS design, the WS RTIS also seems to generate more trembles than a guaranteed-payment design. Still, after controlling for the effect of such errors, the WS RTIS delivers measurements of risk aversion that are comparable to using a single-task design with guaranteed payment. Our results call for caution when applying RTISs in dynamic designs.

Appendix: Instructions to the Within-Subjects RTIS Experiment

Instructions to participants in the DEAL OR NO DEAL experiment

Dear participant in the *Deal or No Deal* experiment,

You are about to participate in an experiment based on the popular TV game show *Deal or No Deal*. Depending on a chance factor and your own decisions during the game, you will be paid a monetary amount.

You will play the game 10 times. One out of the 10 amounts won will be truly paid to you. At the end of this session, a 10-sided dice will determine completely at random the winnings from which game that will be. Play every game as if payment is for sure!

A game consists of a maximum of 9 rounds, in which you have to open one or more briefcases. At the end of each round, you have to decide if you want to accept a displayed "bank" offer or if you prefer to play on and open one or more additional briefcases instead. The exact rules and explanations are on the next pages. Please read these instructions carefully. People who are familiar with the TV game show "*Deal or No Deal*" or "*Miljoenenjacht*" will recognize many parts of the instructions; however, there are some noteworthy differences.

The choice problems presented to you are simple and well-defined, and not designed to test your intelligence. Decisions to quit or continue playing depend on your own personal preferences. There are no "right" or "wrong" decisions. The way others play is completely irrelevant to you. You make your own decisions. You will be granted plenty of time, so take your time to decide on each choice.

You are kindly requested to switch off your mobile telephone during the experiment. Please do not to leave the room before the end of the session. If you do, we are forced to exclude you from the experiment and from payment.

If you have any questions after reading this document, do not hesitate to consult one of the organizers.

Good luck!

The Erasmus University *Deal or No Deal* team:

Guido Baltussen
Thierry Post
Martijn J. van den Assem
Peter P. Wakker

i

Instructions DEAL OR NO DEAL Experiment

General
The game starts with 26 monetary amounts distributed over 26 closed briefcases. First, you have to select one briefcase, which will be your "personal" briefcase. This briefcase remains closed until the end of the game. No one knows which of the 26 amounts is in your briefcase, even the organizers do not know. The following amounts are distributed over the 26 cases:

€ 0,01	€ 0,75
€ 0,01	€ 1,00
€ 0,01	€ 2,50
€ 0,01	€ 5,00
€ 0,01	€ 7,50
€ 0,01	€ 10
€ 0,01	€ 20
€ 0,01	€ 30
€ 0,01	€ 40
€ 0,05	€ 50
€ 0,10	€ 100
€ 0,25	€ 250
€ 0,50	€ 500

Note that a comma separates decimals and a dot separates thousands.

During the game, the screen shows which briefcases and which amounts are still in play. One of the remaining prizes is in your own briefcase. You can always win the amount in your own briefcase, unless you decide to accept a bank offer in the course of the game. In that case, you give up your own briefcase and you win the amount offered. Of course, you can no longer claim the contents of your personal briefcase after you accepted an offer. The bank's offer is always shown at the end of a round. Each time, you have to choose between making a "Deal" with the bank and opening additional briefcases to find out more about which amounts are not in your own case.

To satisfy your curiosity, the contents of your own briefcase are always revealed after a "Deal".

The course of the game
The following summarizes the general course of the game.

Round 1. Number of briefcases to be opened: 6. The bank makes an offer based on the 20 remaining prizes. You decide: "Deal" or "No Deal". If you choose "Deal", the game ends and you win the amount offered; if you choose "No Deal", you will advance to the next round.

Round 2. Number of briefcases to be opened: 5. The bank makes an offer based on the 15 remaining prizes. You decide: "Deal" or "No Deal". If you choose "Deal", the game ends and you win the amount offered; if you choose "No Deal", you will advance to the next round.

Round 3. Number of briefcases to be opened: 4. The bank makes an offer based on the 11 remaining prizes. You decide: "Deal" or "No Deal". If you choose "Deal", the game ends and you win the amount offered; if you choose "No Deal", you will advance to the next round.

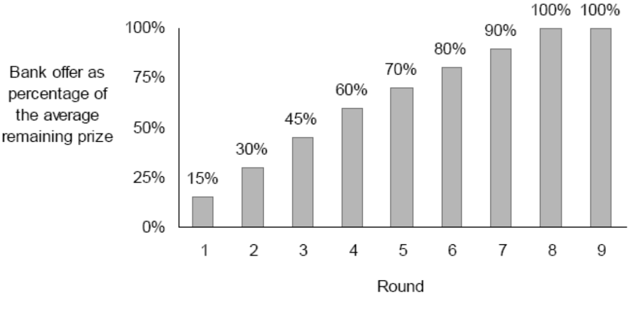
Round 4. Number of briefcases to be opened: 3. The bank makes an offer based on the 8 remaining prizes. You decide: "Deal" or "No Deal". If you choose "Deal", the game ends and you win the amount offered; if you choose "No Deal", you will advance to the next round.

Round 5. Number of briefcases to be opened: 2. The bank makes an offer based on the 6 remaining prizes. You decide: "Deal" or "No Deal". If you choose "Deal", the game ends and you win the amount offered; if you choose "No Deal", you will advance to the next round.

Round 6 up to and including 9. Each round, 1 briefcase has to be opened. The bank makes an offer based on the remaining prizes. You decide: "Deal" or "No Deal". If you choose "Deal", the game ends and you win the amount offered; if you choose "No Deal", you will advance to the next round. If you reject the bank's offer in round 9 ("No Deal"), you win the amount in your personal briefcase.

The bank offers
No one knows which of the remaining prizes is in your own briefcase, even the bank does not know. Each round, the bank will try to buy your briefcase. The more large amounts remain, the larger will be the bank's offer. In this experiment, the bank calculates each offer on the basis of two factors: the round number and the average of the remaining prizes. The round number is always known and the average remaining prize is relatively easy to calculate.

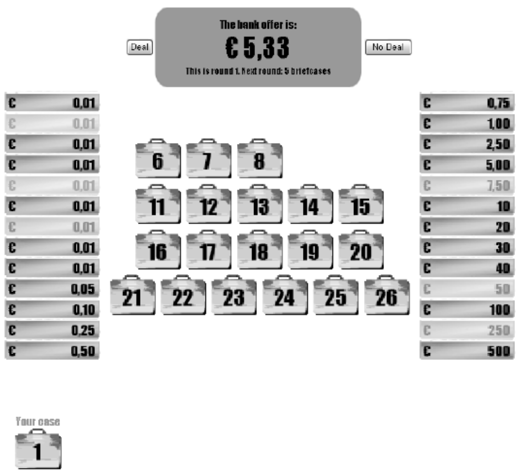
The figure below shows the percentage bank offer for each game round. In late rounds, the bank offers a large percentage of the average remaining prize compared to the early rounds. Note that the average remaining prize is important for the offer expressed in Euros. For example, an offer equal to 90% of a small average remaining prize implies a small bank offer in Euros, whereas an offer of 60% of a large average remaining prize corresponds to a large bank offer in Euros. The more small prizes you eliminate from the game and the more large prizes remain in play, the larger the offers will be.



Screenshots
To further illustrate the game, we have included some screenshots. The figure below shows the opening screen. The 26 briefcases are displayed in the middle and the 26 prizes are listed to the left and the right. Your first task is to select your own, personal briefcase.

iii

After picking your personal briefcase, the first round starts. A total of 6 briefcases have to be opened. The figure below shows a random situation at the end of the first round. The prizes that were in the opened briefcases are eliminated and displayed transparent (0,01; 0,01; 0,01; 7,50; 50 and 250).



Below, on the left side of the screen, you see the personal briefcase chosen at the start (in this case briefcase no. 1). The opened briefcases in this example are 2, 3, 4, 5, 9 and 10. At the end of each round, the bank offer is shown. In this case, the offer equals €5,33. If you decide "Deal", the game ends and the amount is stored. If you choose "No Deal", the next round commences and you will have to open one or more additional briefcases, followed by a new bank offer. For your convenience, the number of cases to be opened in the next round is shown just below the bank offer. After you select a briefcase to be opened, the revealed, eliminated amount is shown at the bottom of the screen.

iv

Final remarks
Some remarks we would like to repeat:

- We will finalize the experiment only after all participants have finished their games. It makes no sense to hurry. Take your time to evaluate each choice problem, you will be granted plenty of time.
- There are no “right” or “wrong” decisions in this game. Decisions to quit or continue playing are dependent on your own personal preferences.
- You will play the game 10 times. One out of the 10 amounts won will be truly paid to you. At the end of this session, a 10-sided dice will determine completely at random the winnings from which game that will be. Play every game as if payment is for sure!

Good luck!

Conclusions

The TV game show Deal or No Deal (DOND) is a natural laboratory for studying risky choice. In this game show we observe, on average, what might be called "moderate" levels of risk aversion. Even when hundreds of thousands of Euros are at stake, many contestants are rejecting offers in excess of 75 percent of the expected value. In an expected utility of wealth framework, this level of risk aversion for large stakes is hard to reconcile with the same moderate level of risk aversion found in small-stake experiments – both ours, and those conducted by other experimentalists. Although risk aversion is moderate on average, the offers people accept vary greatly among the contestants; some demonstrate strong risk aversion by stopping in the early game rounds and accepting relatively conservative bank offers, while others exhibit clear risk-seeking behavior by rejecting offers above the average remaining prize and thus deliberately entering "unfair gambles". While some of this variation is undoubtedly due to differences in individual risk attitudes, a considerable part of the variation can be explained by the outcomes experienced by the contestants in the previous rounds of the game.

In the first chapter of this thesis, we estimate structural choice models. For each of the three editions of DOND analyzed in this chapter, we find that a simple version of prospect theory with a sticky reference point explains the "Deal or No Deal" decisions substantially better than expected utility theory. In the second chapter, we do not limit ourselves to investigating contestants' choice behavior *within* editions, and also look *across* editions. Using probit regression analysis and a very large data set of nearly 6,400 choices made by more than 1,100 different contestants in ten different editions, we find that contestants respond in a similar way to the relative level of the stakes, even though the absolute level differs significantly across the various editions. Differences in the stakes across the various editions have only a weak effect.

Our main finding in the first two chapters is the important role of reference-dependence and path-dependence, phenomena that are often ignored in implementations of expected utility theory. The context of earlier game situations or choice problems appears to be a key determinant of the framing of a given choice problem. Amounts are likely to be considered as "large" in the context of a game where they lie above prior expectations, but the same amounts are evaluated as “small” in a game where they lie below prior expectations. For contestants who expected to win hundreds of thousands, an amount of €10,000 probably seems “small”; the same amount is likely to appear much “larger” when thousands or tens of thousands were expected. We hope that the results provide a stimulus for the further development and proliferation of reference-dependent choice theory.

In the third chapter, we analyze the effects of the random task incentive systems (RTISs) in risky choice experiments that mimic the game of DOND. When we compare our two RTIS treatments with our guaranteed-payment treatment and hold the face values of the prizes constant, we find that applying a RTIS in a between-subjects design generates downwardly biased measurements of risk aversion and increases the likelihood of decision errors. The increased errors are likely to reflect lapses of concentration, caused by the less favorable tradeoff between the cost and benefits of decision efforts. Experimentalists who consider using a RTIS in a within-subjects design should be aware of possible carry-over effects from showing intermediate outcomes of previous tasks. Risk aversion increases after recent unfavorable results and decreases after recent favorable results. Like the BS design, the within-subjects RTIS also seems to generate more trembles than a guaranteed-payment design. Still, after controlling for the effect of such errors, the within-subjects design delivers measurements of risk aversion that are comparable to using a single-choice design with guaranteed payment. The results of the third chapter call for caution when applying RTISs.

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Nederlandse Samenvatting

Summary in Dutch

Het tv-spelprogramma Deal or No Deal (DOND) – in Nederland ook bekend onder de naam Miljoenenjacht – werd in 2006 in 46 landen uitgezonden en is daarmee één van de succesvolste televisieprogramma's van producent Endemol. Ook voor economen is DOND interessant, omdat het spelprogramma kan worden beschouwd als een uniek natuurlijk experiment dat de mogelijkheid biedt individueel risicogedrag te analyseren in een situatie waarin grote geldbedragen op het spel staan.

In de meeste landen zijn de spelregels vergelijkbaar. DOND wordt gespeeld over meerdere rondes, en begint met enkele tientallen koffers (of dozen) waarover evenveel prijzen willekeurig zijn verdeeld. De samenstelling van de verzameling geldprijzen is bekend, maar uiteraard is niet zichtbaar welke prijs zich in welke koffer bevindt. De kandidaat kiest één van de koffers, en mag zich daarmee eigenaar noemen van de vooralsnog onbekende inhoud. Aan het begin van iedere spelronde wijst de kandidaat een gegeven aantal van de overige koffers aan, die vervolgens worden geopend. Zodoende komt de kandidaat er stap-voor-stap achter welke prijzen zich niet in zijn of haar eigen koffer bevinden. Aan het einde van iedere ronde doet een "bankier" een bod op de koffer van de kandidaat. Dit bod is vooral gerelateerd aan het gemiddelde van de resterende prijzen, en de aantrekkelijkheid van het bod loopt op naarmate het spel vordert. Als de kandidaat het bod accepteert, "Deal", dan is het spel afgelopen. Als de kandidaat het bod weigert, "No Deal", dan gaat hij of zij verder naar de volgende ronde en moet er wederom een aantal koffers worden geopend. Het spel gaat op deze wijze door, totdat de kandidaat ofwel een bankbod accepteert, ofwel alle andere koffers heeft geopend en de inhoud van zijn of haar eigen koffer ontvangt.

In dit proefschrift analyseren we het risicogedrag van kandidaten in televisieafleveringen en van studenten die het spel spelen achter de computer of voorin de collegezaal. De vraag hoe mensen zich gedragen in situaties van risico raakt aan de basis van praktisch iedere discipline binnen de economische wetenschap. Er bestaat momenteel een breed scala aan theorieën op dit gebied. Bekende voorbeelden zijn de normatieve verwachte-nutstheorie van Von Neumann and Morgenstern (1944) en de beschrijvende *prospect theory* van Kahneman and Tversky (1979). Het empirisch toetsen van dergelijke theorieën is echter niet eenvoudig gebleken. De eerste onderzoeken waren meestal gebaseerd op gedachte-experimenten of antwoorden op hypothetische vragen. Met de opkomst van de experimentele economie nam de populariteit van experimenten met echte geldbedragen toe, maar door beperkte onderzoeksbudgetten bleven de gehanteerde bedragen klein. Buiten het onderzoekslaboratorium wordt empirisch onderzoek vaak gehinderd door wat men wel het *joint hypothesis* probleem noemt. Het ondubbelzinnig waarnemen van de risicohouding is niet mogelijk, omdat de mensen waarvan het gedrag wordt bestudeerd niet weten wat de werkelijke kansverdeling is (een situatie die men vaak omschrijft als een situatie van onzekerheid, in plaats van risico), en omdat de ideeën die zij hebben over de kansverdeling niet bekend zijn bij de onderzoeker.

Ondanks de nog maar korte historie van DOND wordt de spelshow nu al in brede academische kring erkend als een “natuurlijk onderzoekslaboratorium” voor de analyse van risicogedrag. Wij ontdekten DOND en de mogelijkheden die de show biedt toen deze aan de vooravond stond van een internationale doorbraak. De show is ontwikkeld door Endemol, een Nederlandse programmamaker, en in Nederland voor het eerst uitgezonden. De allereerste uitzending was in december 2002. In 2003 werd het concept succesvol geëxporteerd naar Australië. In de daaropvolgende jaren werd DOND gelanceerd in vele tientallen andere landen. In 2006 verscheen de show reeds in 46 verschillende landen op nationale tv-zenders. De geldbedragen die op het spel staan in DOND zijn zeer groot en variëren sterk: in een aflevering kan een kandidaat naar huis gaan als multimiljonair, maar ook met vrijwel lege handen. De analyse van risicogedrag aan de hand van televisieshows is niet nieuw, maar anders dan bij andere shows gaat het bij DOND om zeer simpele ja/nee beslissingen (“Deal” of “No Deal”). Deze beslissingen vereisen nauwelijks of geen vaardigheden, kennis of strategie, en de verdeling van kansen en uitkomsten is eenvoudig en met nagenoeg volledige zekerheid bekend. Bovendien bestaat het spel uit meerdere rondes, waardoor DOND interessant kan zijn voor de analyse van padafhankelijkheid in risicogedrag, of, anders gezegd, van de invloed van voorafgaande uitkomsten op de risicohouding.

Natuurlijk kan het gedrag van deelnemers aan spelshows niet altijd worden gegeneraliseerd naar de beslissingen die men maakt in het alledaagse leven. Waar de deelnemers aan televisiespelletjes beslissingen moeten nemen in een tijdsbestek van enkele minuten en voor het oog van de camera en miljoenen televisiekijkers, worden veel beslissingen in het dagelijks leven die betrekking hebben op grote bedragen zelden gehaast of in het licht van schijnwerpers genomen. Toch zijn wij er van overtuigd dat de keuzes in deze bijzondere spelshow de moeite van het bestuderen waard zijn, omdat de keuzeproblemen als gezegd eenvoudig en duidelijk geformuleerd zijn en omdat de geldbedragen enorm groot zijn. Bovendien hebben kandidaten voorafgaand aan de show onbeperkt de tijd gehad om zich af te vragen wat men zou doen in verschillende situaties, en tijdens de show wordt hen regelmatig en uitgebreid de mogelijkheid geboden om de keuzes te overleggen met één of meerdere vrienden of familieleden die in de studio aanwezig zijn. Zo bezien lijken de keuzes meer weloverwogen dan men op het eerste gezicht zou denken. Het heeft er zelfs schijn van dat kandidaten beter nadenken over hun keuzes tijdens de show dan over andere financiële beslissingen die ze nemen in hun leven, bijvoorbeeld bij het selecteren van een hypotheek, het beleggen van hun vermogen, of het afsluiten van verzekeringen.

In hoofdstuk 1 van dit proefschrift analyseren we het risicogedrag van 151 kandidaten uit Nederland (51), Duitsland (47) en Amerika (53), en van 80 studenten die deelnamen aan een DOND-experiment in een collegezaal. Eén van de analyses die we uitvoeren betreft de schatting van structurele keuze-modellen aan de hand van *maximum-likelihood* methodologie. Voor iedere steekproef vinden we dat een eenvoudige implementatie van *prospect theory* de keuzes van de deelnemers duidelijk beter verklaart dan verwachte-nutstheorie. De grootste verliezers en de grootste winnaars blijken een abnormaal lage risicoaversie te hebben. Deze constatering komt overeen met de zogenaamde *break-even* en *house-money* effecten die zich voor doen wanneer een referentiepunt gehanteerd wordt dat zich traag aanpast aan nieuwe situaties en blijft hangen bij oude verwachtingen. Hoewel kandidaten bij DOND nooit armer de studio uit komen dan zij erin gaan, kunnen ze wel degelijk substantiële "papieren" verliezen lijden wanneer de dozen of koffertjes met grote bedragen afvallen (waardoor de verwachte spelopbrengst daalt). Wat wij laten zien, is dat dergelijke verliezen hun navolgende keuzes beïnvloeden. Veel verliezers gedragen zich zelfs risicozoekend door bankbiedingen te weigeren die de gemiddelde resterende prijs overstijgen. De resultaten wijzen in de richting van referentie-afhankelijke beslissingstheorieën als *prospect theory*, en geven aan dat padafhankelijkheid relevant is, zelfs wanneer grote, echte geldbedragen op het spel staan. Dit eerste hoofdstuk is gebaseerd op het artikel *Deal or No Deal? Decision Making under Risk in a Large-Payoff Game Show*, dat mede geschreven is door Thierry Post, Guido Baltussen en Richard H. Thaler, en gepubliceerd is in de American Economic Review.

In hoofdstuk 2 onderzoeken we hoe het risicogedrag in DOND af hangt van de context, in de zin van de grootte van de prijzen waarmee het spel begint. We maken in dit hoofdstuk gebruik van een grotere dataset, die bestaat uit tien edities die onderling sterk verschillen in de grootte van de prijzen. De tien edities zijn afkomstig uit zeven verschillende landen en vertegenwoordigen gezamenlijk ongeveer 6.400 keuzes die gemaakt zijn door ongeveer 1.100 verschillende kandidaten. Deze grote dataset en de verschillen in de grootte van de prijzen waarmee het spel begint maken het mogelijk om *framing* effecten te analyseren door vergelijkingen te maken tussen edities, en verminderen daarnaast de noodzaak om volledig gespecificeerde structurele modellen te schatten zoals in hoofdstuk 1. Aan de hand van probit regressie analyse vergelijken we hoe de absolute en relatieve groottes van de bedragen die op het spel staan van invloed zijn op het risicogedrag. Onze analyses binnen en tussen de verschillende edities wekken de indruk dat risicogedrag zeer gevoelig is voor de context van het keuzeprobleem. Beslissingen worden voornamelijk gedreven door de relatieve grootte van de geldbedragen, en niet of nauwelijks door de absolute grootte. Voor een gegeven editie van DOND hebben veranderingen in de bedragen die op het spel staan een sterk effect op de risicohouding en het keuzegedrag, terwijl verschillen in de bedragen bij de start van het spel nauwelijks van invloed zijn. Onze resultaten geven aan dat bedragen vooral worden geëvalueerd in verhouding tot een subjectief referentiekader in plaats van in termen van de absolute waarde. Dit hoofdstuk is gebaseerd op het artikel *Risky Choice and the Relative Size of Stakes*, dat mede is geschreven door Guido Baltussen en Thierry Post.

Het laatste hoofdstuk betreft een onderzoek naar de effecten van *Random Task Incentive Systems* (RTISs). Een RTIS is een beloningsmethode die vaak wordt gehanteerd bij economische experimenten, waarbij slechts één willekeurig geselecteerde taak voor echt geld wordt gespeeld. De laboratoriumexperimenten in dit hoofdstuk imiteren het DOND spel. We voeren drie varianten uit van het experiment, waarbij alleen de beloningsmethode verschilt. In de eerste variant speelt iedere deelnemer het spel één keer en voor echt geld. In de tweede variant speelt iedere deelnemer het spel tien keer en wordt willekeurig één van de tien uitkomsten geselecteerd en uitbetaald. In de derde variant speelt iedere deelnemer één keer, en heeft hij of zij een kans van tien procent dat de uitkomst ook daadwerkelijk wordt uitbetaald. We onderzoeken drie mogelijke effecten die kunnen optreden in een RTIS experiment: een vertekend niveau van risicoaversie, frequentere beslissingsfouten, en beïnvloeding van keuzes door uitkomsten van voorafgaande taken. De kenmerken van DOND maken het mogelijk om de drie verschillende effecten te onderzoeken op basis van één en dezelfde taak. Onze resultaten geven aan dat voorzichtigheid betracht dient te worden bij het gebruik van RTISs. We constateren dat er sprake is van een significante frequentie van fouten die ongerelateerd zijn aan de karakteristieken van het keuzeprobleem. Deelnemers lijken last te hebben van concentratieverlies en/of af te zien van de inspanningen die nodig zijn om het probleem serieus te overdenken, waardoor keuzes gemaakt worden die volstrekt willekeurig zijn. Daarnaast blijkt dat deelnemers in de variant waarbij één van de tien taken voor betaling wordt geselecteerd, sterk worden beïnvloed door de uitkomsten van voorafgaande taken: hun risicoaversie is hoger na onaantrekkelijke recente uitkomsten en lager na aantrekkelijke recente uitkomsten. Gemiddeld genomen is de mate van risicoaversie niet significant anders dan bij de variant met één taak en gegarandeerde betaling. De methode waarbij de deelnemer één taak uitvoert en een kans heeft van tien procent op uitbetaling omzeilt uiteraard beïnvloeding door uitkomsten van voorafgaande taken, maar bij deze variant is de mate van risicoaversie aanmerkelijk lager dan in het geval van zekere uitbetaling, wat wijst op een serieuze vertekening. Dit laatste hoofdstuk is gebaseerd op het artikel *Random Task Incentive Systems in Risky Choice Experiments*, mede geschreven door Guido Baltussen, Thierry Post en Peter P. Wakker.

About the Author

Martijn J. van den Assem (1976) obtained his Master's degree in Economics from the Erasmus University of Rotterdam, the Netherlands, in August 2000. Immediately thereafter, he joined the Finance Group of the Erasmus School of Economics as an Assistant Professor of Finance. In 2005, he became a member of the Executive Committee of this department. In 2004, Martijn started to carry out the research in this PhD thesis. His work has been presented at many international conferences and received a great deal of media attention, appearing for example on the front page of the Wall Street Journal and being featured twice on National Public Radio in the US. The first paper from his doctoral research has been published in the American Economic Review, and is among the most frequently downloaded manuscripts all-time in the field of social sciences on the Internet. His wide range of research interests include decision making under risk, behavioral finance and behavioral economics, initial public offerings, regulatory issues of stock markets, market micro-structure, capital structure and dividend policy, ex-dividend day effects, and cost of capital issues. He served as a referee for various scholarly journals, including multiple times for the American Economic Review. Martijn supervised a large number of bachelor and master theses, and has taught several introductory courses on finance and seminars on corporate finance. He was nominated by students for the 2003/2004 best lecturer award of the Erasmus School of Economics and received an honorable mention in the 2004/2005 election.

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DEAL OR NO DEAL? DECISION MAKING UNDER RISK IN A LARGE-STAKE TV GAME SHOW AND RELATED EXPERIMENTS

Risky choice is fundamental to virtually every branch of economics. Unfortunately, empirical testing of theories developed in this field has proven to be difficult because of limited budgets in experiments and joint hypothesis problems outside the laboratory.

The first two chapters of this thesis analyze the risky choices of contestants in a TV game show named "Deal or No Deal" (DOND). DOND provides a unique opportunity to study risk behavior, because it is characterized by very large and wide-ranging stakes, by a simple probability distribution, and by stop-go decisions that require minimal skill or strategy. The results are hard to reconcile with expected utility theory and point to reference-dependent alternatives such as prospect theory. The first chapter uses editions from the Netherlands, Germany and the US. In each sample, the choices of contestants can be explained in large part by previous outcomes experienced during the game. Risk aversion decreases after earlier expectations have been shattered by unfavorable outcomes or surpassed by favorable outcomes. The second chapter not only studies risk behavior in individual editions, but also compares across editions. We demonstrate that risky choice is highly sensitive to the context, as defined by the initial prizes in the game. Even though the initial stakes of the various editions are widely different, contestants respond in a similar way to the stakes relative to their initial level.

The third chapter of this thesis analyzes random task incentive systems (RTISs), using experiments that mimic DOND. RTISs are commonly applied in economic experiments to implement real incentives when research budgets are limited and to avoid income effects. We find that caution is warranted when applying RTISs.

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