High-Stakes Failures of Backward Induction

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Abstract

We examine high-stakes strategic choice using more than 40 years of data from the American TV game show *The Price Is Right*. In every episode, contestants play the *Showcase Showdown*, a sequential game of perfect information for which the optimal strategy can be found through backward induction. We find that contestants systematically deviate from the subgame perfect Nash equilibrium. These departures from optimality are well explained by a modified agent quantal response model that allows for limited foresight. The results suggest that many contestants simplify the decision problem by adopting a myopic representation and optimize their chances of beating the next contestant only. In line with learning, contestants' choices improve over the course of our sample period.

Keywords: backward induction; limited foresight; omission bias; quantal response equilibrium; subgame perfect Nash equilibrium
JEL Classifications: C72; D01; D91

1 Introduction

Many economic interactions are sequential in nature. A negotiator who makes a bargaining offer, an entrepreneur who considers whether to enter a market, and a corporate manager who decides how many goods to produce, all need to consider the subsequent actions of others. Such situations can be modeled as finite sequential games of perfect information, for which the subgame perfect Nash equilibria can be found through backward induction (von Stackelberg, 1934; Selten, 1978; Dixit, 1982; Rubinstein, 1982).

Unfortunately, the descriptive accuracy of these equilibria is difficult to test in the field, because agents' choice options, payoffs, and the information they have are normally not (or not straightforwardly) observable. Whenever choices deviate from equilibrium play, it then remains unclear whether the behavior is truly suboptimal or whether the deviations are the result of incorrect assumptions. To avoid this joint-hypothesis problem, tests of equilibrium play typically rely on laboratory experiments in which all factors are perfectly controlled. Experimental work generally finds that people often deviate from the equilibrium strategies, casting doubt on the descriptive validity of backward induction as a solution concept (Rosenthal, 1981; McKelvey and Palfrey, 1992; Fey et al., 1996; Binmore et al., 2002; Johnson et al., 2002; Levitt et al., 2011; Dufwenberg and Van Essen, 2018). The generalizability of experimental findings to real world situations, however, is subject to debate (Binmore, 1999; Levitt and List, 2007*a*,*b*; Falk and Heckman, 2009; Camerer, 2015). Critics argue that it is not very surprising that experimental subjects frequently fail to adopt equilibrium strategies, most notably because they typically are not well incentivized and have little experience with the task.

The present paper examines the optimality of strategic decisions in the *Showcase Showdown* (SCSD), a finite sequential game of perfect information that is played twice in every episode of the long-running American TV game show *The Price Is Right*. In this game, described in more detail in Section 2, three contestants take turns to spin a wheel that contains all multiples of 5 in the range 5–100.[1] Immediately after spinning the wheel once, the contestant has to decide whether to spin the wheel again. Their score is the outcome of the first spin if they spin only once, and the sum of the two spin outcomes if they spin twice. The contestant whose score is closest to 100 without going over wins the game and proceeds to the so-called *Showcase* final, where they compete with the winner of another SCSD to win a set of prizes worth tens of thousands of dollars in expectation. If their score

[1]Henceforth we refer to the contestant who spins first as Contestant 1, to the contestant who spins second as Contestant 2, and to the contestant who spins last as Contestant 3.

is exactly 100 they, in addition, win one or two monetary bonus prizes.

To make the optimal choice, the contestants thus need to weigh the possibility of obtaining a more competitive score and having a shot at the bonus prizes against the chance of self-elimination. Coe and Butterworth (1995), Grosjean (1998), and Tenorio and Cason (2002) derive the unique subgame perfect Nash equilibrium (SPNE) for this game for various sets of combinations of bonus prizes and expected showcase values. The three contestants' equilibrium strategies, which can be found through backward induction, take the form of decision rules that dictate whether a contestant should stop or use their second spin.

The characteristics of the SCSD make it an appealing test bed for assessing the descriptive validity of backward induction as a solution concept in the field. First, as in carefully designed lab experiments, the task is well-defined and both the choice options and the choice-relevant information that is available to contestants are known. Second, the prizes that can be won dwarf the payoffs that are typically employed in experiments. Third, the SCSD has been repeated numerous times under similar conditions, creating the opportunity of a large-scale statistical analysis. Other benefits of this long history are that contestants can be expected to be sufficiently familiar with the game and that we can explore potential learning effects.[2]

At the same time, the game show setting may evoke external validity concerns because of selection procedures and the unusual conditions under which choices are made. Section 5 reflects on these concerns. Any possible downside, however, should be evaluated in the light of the availability of better alternatives. Other opportunities for a large-scale, high-stakes test of backward induction are incredibly scarce, if not absent. Hence, following List (2023), the unique setting should be embraced and not dismissed for its idiosyncrasies.

The present paper is not the first to use a TV game show as a real-world naturally-occurring laboratory. Game shows have been used to study a wide range of other topics in economics, such as decision making under risk (Gertner, 1993; Metrick, 1995; Post et al., 2008), discrimination (Levitt, 2004; Belot et al., 2010), bargaining (van Dolder et al., 2015), willingness to compete (Hogarth et al., 2012; Buser et al., 2022), and cooperation (List, 2006; Oberholzer-Gee et al., 2010; van den Assem et al., 2012; Turmunkh et al., 2019).

We analyze a large sample of 10,071 renditions of the SCSD. In every rendition, three contestants make a spin decision, but a substantial fraction of the 30,213 decisions are trivial and of little value to our study. For Contestant 2 and 3, decisions

[2]The SCSD has also been proposed as a useful classroom tool for teaching probability and game theory (Burks and Jaye, 2012; Swenson, 2015).

are trivial when their first-spin outcome is lower than the best preceding score (in which case they always spin again). For Contestant 3, who spins last, the decision is also trivial when their first-spin outcome is higher than the best preceding score (in which case they always stop).[3] We omit such decisions from our empirical analysis, and exclusively focus on the decisions of Contestant 1 and the remaining decisions of Contestant 2.

We start our analysis by examining whether, when, and how contestants deviate from the SPNE. We find that Contestant 1 and 2 frequently make suboptimal decisions, and that the error rate of Contestant 1 is somewhat higher than that of Contestant 2. Moreover, Contestant 1 almost exclusively errs by underspinning: they stop when it is optimal to spin. Contestant 2's mistakes, by contrast, are considerably more symmetric and involve only slightly more underspinning than overspinning.

We then consider several explanations for suboptimal play that are well-rooted in the literature. First, we examine whether contestants depart from the equilibrium strategy because they make random errors in evaluating the expected utility of their two choice options, and expect others to also make such mistakes. To test this explanation, we estimate an agent quantal response equilibrium model (AQRE; McKelvey and Palfrey, 1998). We find that a substantial proportion of the deviations from the SPNE can be explained by random evaluation errors. The decisions of Contestant 2 are largely consistent with the model's probabilistic predictions. However, the model fails to capture most of the underspinning of Contestant 1.

Next, we consider the possible role of omission bias, which is the tendency to favor harmful inactions over harmful actions (Ritov and Baron, 1990, 1992; Spranca et al., 1991; Feldman et al., 2020). Systematic underspinning in the SCSD can be explained by a preference for elimination after not spinning (by an opponent who obtains a higher score) over elimination after spinning (by exceeding 100 points). We find that allowing for omission bias in the AQRE model improves the goodness-of-fit for Contestant 1, but at the same time introduces systematic prediction errors for Contestant 2. Hence, omission bias fails to adequately explain the behavior that we observe.[4]

Another possible explanation is that some contestants do not properly backward induct, and instead adopt a simplified representation of the game. Prior research

[3]Contestant 3 faces a nontrivial decision when they tie with the best preceding score, but such situations are relatively rare.

[4]Walker et al. (2018) propose the related concept of sudden death aversion: the tendency to avoid strategies that can lead to immediate defeat, even if these are optimal. In our setting, sudden death aversion and omission bias are indistinguishable, because spinning (acting) entails the risk of immediate defeat whereas not spinning (not acting) does not.

suggests that people display limited foresight, and look only one or a few steps ahead in multi-stage strategic situations (Jehiel, 1995, 1998, 2001; Johnson et al., 2002; Gabaix and Laibson, 2005; Gabaix et al., 2006; Mantovani, 2015; Ke, 2019; Rampal, 2022; Baranski and Reuben, 2023). We adjust our baseline AQRE model to allow for the possibility that a contestant myopically behaves as if the next stage of the game is also the last. Such a simplified frame lowers Contestant 1's propensity to spin, because beating only one subsequent contestant in expectation requires a lower score than beating two. For Contestant 2, limited foresight coincides with full backward induction because the next stage is also the last stage of the game. Our limited foresight model accurately describes the observed behavior of contestants. According to the estimation results, approximately 38 percent of the contestants simplify the game by looking only one step ahead.

Limited foresight thus provides a better account of contestants' spinning choices than omission bias. To establish whether omission bias adds any explanatory power on top of limited foresight, we also estimate a model that incorporates both these elements. We find that this model does not significantly outperform the one that only includes limited foresight. The overall conclusion therefore is that the deviations from the SPNE in this high-stakes game are well explained by a combination of random evaluation errors and limited foresight, and that the role of omission bias is negligible. This conclusion is robust to various alternative modeling assumptions.

Our conclusion diverges from that of Tenorio and Cason (2002). Tenorio and Cason compare the decisions of contestants in the SCSD with those dictated by the SPNE, using a relatively small sample from 1994 and 1995. Based on the under-spinning of Contestant 1 and a lack of informative observations for Contestant 2, they conclude that omission bias is a plausible explanation for the deviations from the SPNE in their data. The present paper uses a considerably larger sample, with many informative observations for both Contestant 1 and Contestant 2. This allows for the estimation of structural decision models and tests of competing hypotheses, which reveal that random evaluation errors and limited foresight rather than omission bias can adequately explain the deviations.

Our results are striking in the light of the long history of the show and its popularity. A natural question is whether contestants' behavior converges towards the SPNE over time. When we subdivide our sample into four periods, we find that the estimated probability of limited foresight monotonically decreases from 55 percent in the first period to 24 percent in the last. Despite this strong improvement, the results show that many contestants remain unable to properly backward induct, even after several decades of *The Price Is Right*.

The remainder of the paper is structured as follows. Section 2 introduces the game show and the SCSD in more detail and outlines the equilibrium strategies. Section 3 discusses the data and provides a descriptive analysis of deviations from equilibrium play. Section 4 presents the main analyses and results, various robustness checks, and the learning analysis. Section 5 concludes and discusses our findings.

2 The Game and Its Equilibrium Strategies

The Price Is Right was first aired in the United States in 1956. Through the years, the format was introduced in many other countries, but here we exclusively consider the American version. Every episode consists of multiple games. The game that is central in our paper—the *Showcase Showdown* (SCSD)—was introduced in 1975. Apart from a change in 2008 (see below), the SCSD has remained the same since 1979. We exclusively consider episodes from that year onwards.

Every episode contains two renditions of the SCSD, with three contestants each. Prior to the SCSD, every contestant plays two other games: the so-called *One Bid* game, and a pricing game. In the *One Bid* game, four contestants guess the retail price of a consumer product (such as a microwave or television).[5] The contestant whose guess is closest to the actual retail price without going over wins the product, gets to play one of the many different pricing games, and will be one of the SCSD contestants.[6] In their pricing game, the contestant can win one or more prizes, often by guessing the retail prices of consumer goods. After three contestants have won a *One Bid* game and completed their pricing game, the first SCSD is played. In the next part of every episode this combination of three *One Bid* games, three pricing games, and one SCSD is repeated.

The winners of the two SCSDs proceed to the final of the episode. In this so-called *Showcase* round, the two finalists have to guess the retail price of their own respective showcase, which typically consists of multiple valuable prizes such as cars, furniture, electronics, and trips. The contestant whose guess is closest to the retail price without exceeding it wins the contents of their showcase. If the winner's guess is within a specified range below the retail price ($100 until 1997-98, $250 from 1998-99 onwards) they win both showcases; if both finalists' guesses exceed the retail price

[5]Contestants are selected by the producers through interviews with ticketed audience members shortly before to the recording of an episode.

[6]Bennett and Hickman (1993), Berk et al. (1996), and Healy and Noussair (2004) use the *One Bid* game to study strategic decision making. Atanasov et al. (2021) use it to study own-gender favoritism.

both showcases remain unclaimed.

In the SCSD, our game of interest, three contestants take turns to spin a big wheel that contains all multiples of 5 up to 100. The contestant with the lowest (highest) prior winnings spins first (last). Immediately after observing the outcome of their first spin, a contestant has to decide whether to spin the wheel again.[7] Their score is the outcome of the first spin if they spin once, and the sum of the two spin outcomes if they spin twice. The contestant whose score is closest to 100 without exceeding is the winner and proceeds to the *Showcase* round.[8] If two or three contestants tie for the highest score, they enter a spin-off in which each of them spins the wheel once more; the one who spins the highest number is the winner. This procedure is repeated in the case of further ties.

On top of securing a place in the lucrative final, SCSD contestants can also win one or two monetary bonus prizes. If a contestant scores exactly 100 points, they receive $1,000 plus a bonus spin that yields an additional $10,000 ($5,000 before 2008-09) if the wheel lands on 5 or 15, or $25,000 ($10,000 before 2008-09) if it stops at 100. If two or three contestants tie at a score of 100, the outcome of their bonus spin counts as their spin-off score.

The optimal strategy for a contestant depends on the expected showcase value and the bonus prizes. Coe and Butterworth (1995), Grosjean (1998), and Tenorio and Cason (2002) derive the unique subgame perfect Nash equilibrium (SPNE) for a limited set of combinations of these values. The three contestants' equilibrium strategies, which can be found through backward induction, take the form of optimal stopping rules that dictate when a contestant should not use their second spin. Our sample covers a large time span, over which the average stated retail price of the showcases varied considerably, and during which there was a change in the bonus prizes. We therefore derive the optimal stopping thresholds for a large set of combinations of expected showcase values and the two bonus schemes.

In line with previous work, we assume (i) that contestants believe that spin outcomes follow a discrete uniform distribution from 5 to 100 with steps of 5, (ii) that contestants are risk neutral, and (iii) that contestants believe that the chance of winning the *Showcase* round after winning the SCSD is 50 percent. We examine the sensitivity of our results to the latter two assumptions in Section 4.4.

Table 1 shows each contestant's optimal strategy for various ranges of expected

[7]The wheel must be spun for at least one full revolution.

[8]If the third contestant beats the best preceding score with their first spin, or if the first two contestants went over 100, the third contestant automatically advances to the *Showcase* round. In the latter case, Contestant 3 does spin the wheel once to try to win a bonus prize by spinning exactly 100, but they are not given the choice to spin a second time.

Table 1: Optimal strategies

Contestant	First spin	$E(S)$	Stopping threshold
Panel A: Bonus Scheme 1 (until 2008-09)			
C1		$\$1,532 \leq E(S) < \$4,180$	75
C1		$E(S) \geq \$4,180$	70
C2	Better than C1	$\$2,564 \leq E(S) < \$27,826$	60
C2	Better than C1	$E(S) \geq \$27,826$	55
C2	Tied with C1	$\$2,503 \leq E(S) < \$10,702$	75
C2	Tied with C1	$E(S) \geq \$10,702$	70
C3	Tied with C1 or C2	$\$2,000 \leq E(S) < \$4,000$	60
C3	Tied with C1 or C2	$E(S) \geq \$4,000$	55
C3	Tied with C1 and C2	$\$2,400 \leq E(S) < \$6,000$	75
C3	Tied with C1 and C2	$E(S) \geq \$6,000$	70
Panel B: Bonus Scheme 2 (from 2008-09 onwards)			
C1		$\$2,489 \leq E(S) < \$6,792$	75
C1		$E(S) \geq \$6,792$	70
C2	Better than C1	$\$4,167 \leq E(S) < \$45,217$	60
C2	Better than C1	$E(S) \geq \$45,217$	55
C2	Tied with C1	$\$4,068 \leq E(S) < \$17,391$	75
C2	Tied with C1	$E(S) \geq \$17,391$	70
C3	Tied with C1 or C2	$\$3,250 \leq E(S) < \$6,500$	60
C3	Tied with C1 or C2	$E(S) \geq \$6,500$	55
C3	Tied with C1 and C2	$\$3,900 \leq E(S) < \$9,750$	75
C3	Tied with C1 and C2	$E(S) \geq \$9,750$	70

Notes: The table shows the optimal strategies for various ranges of expected showcase values and for the two different bonus schemes. Under Bonus Scheme 1 (Panel A), the bonus prizes are $1,000, $5,000, and $10,000; under Bonus Scheme 2 (Panel B), the bonus prizes are $1,000, $10,000, and $25,000. The first column indicates whether the contestant is the first (C1), second (C2), or third (C3) to spin. The second column indicates whether the contestant's first spin beats or ties the best preceding score. The third column gives the range for the expected showcase value. The last column gives the optimal stopping threshold: the first-spin outcome at or above which the contestant should stop, and below which they should spin again. The table omits the trivial optimal decision of Contestant 2 and Contestant 3 in situations where their first-spin outcome is lower than the best preceding score (always spin), and that of Contestant 3 when their first-spin outcome beats the best preceding score (always stop).

showcase values, denoted $E(S)$, and for the two different bonus schemes. For brevity, the table displays the optimal strategies for empirically relevant ranges of $E(S)$ only.[9] Furthermore, it omits the trivial optimal decision of Contestant 2 and Contestant 3 in situations where their first-spin outcome is lower than the best preceding score (where they should always spin), and that of Contestant 3 when their first-spin outcome beats the best preceding score (where they are automatically declared the winner).

Consider, for example, a rendition of the SCSD with the most recent bonus

[9] When $E(S)$ goes to zero, the optimal stopping threshold converges to 100 for all situations.

scheme and where $E(S) = \$25,000$. Contestant 3 faces a nontrivial decision only when they tie the best preceding score. If they tie with one previous contestant, they should stop when the tie is at 55 or more (and spin otherwise). In the case of a three-way tie, the stopping threshold is 70.

The optimal strategies of the other two contestants can be found through backward induction. Assuming that Contestant 3 strictly adopts the optimal approach, Contestant 2 is best off by stopping at 60 or more if they beat the score of Contestant 1, and by spinning otherwise. In the case of a tie with Contestant 1, Contestant 2's stopping threshold is 70. Contestant 1 has to anticipate the decisions of Contestant 2 and 3. Assuming that these two both follow the optimal strategy, Contestant 1's stopping threshold is 70.

3 Data and Preliminary Results

Our data are from the *The Price Is Right Episode Guide*.[10] We accessed this fan-edited website on 21 June 2021. At that time, it contained $5,834$ detailed recaps of episodes of *The Price Is Right* from 1979 onwards. We successfully scraped the data for one or both SCSDs for 5,307 episodes. After omitting special episodes with a deviating prize structure, and the one available episode from the 1978-79 season, our final sample contains 10,071 SCSDs from 5,235 different episodes that were aired between 1979-80 and 2020-21.[11] In most cases, we additionally obtained contestants' names, the accumulated value of the prizes they earned prior to the SCSD, and the stated retail prices of the showcases. Table A1 in the Appendix shows the numbers of episodes, SCSDs, and showcase prices in our sample for every season.

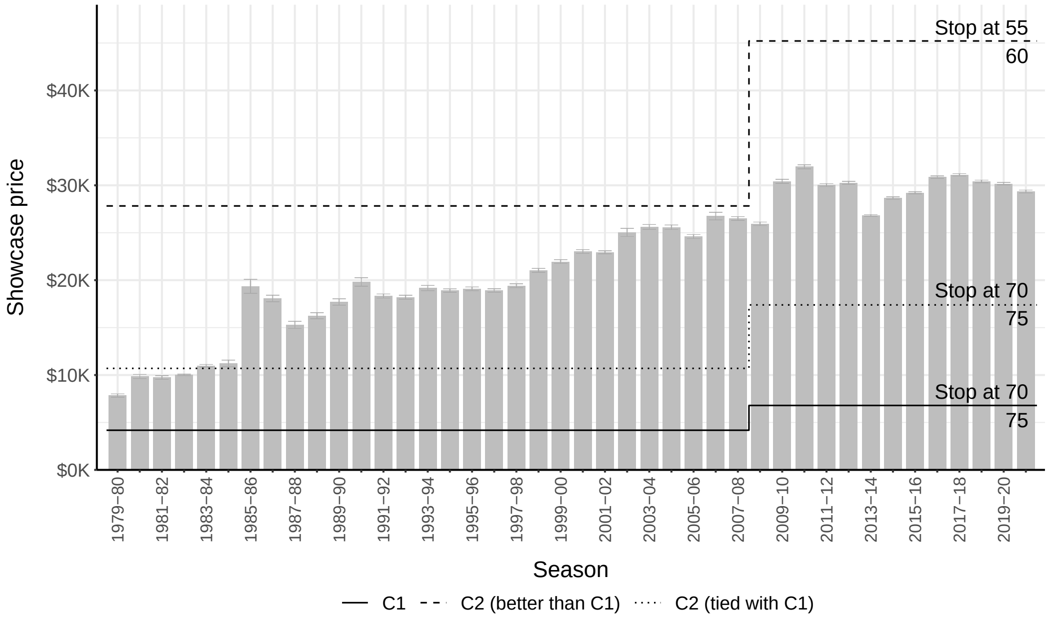
As a first analysis, we explore the extent to which contestants' spinning decisions are consistent with the SPNE. Because almost all Contestant 3's decisions are trivial—they are automatically declared the winner if their first-spin outcome beats the best preceding score, and by default spin again if it is lower—we focus exclusively on Contestant 1 and 2. For the same reason, we omit decisions of Contestant 2 that follow first-spin outcomes that are below the score of Contestant 1. This leaves us with 10,071 spinning decisions for Contestant 1 and 4,488 for Contestant 2.

The previous section showed how the optimal stopping rules depend on a contestant's assessment of the expected showcase value. We make the simplifying assump-

[10]See https://tpirepguide.com.

[11]Some types of special episodes featured a deviating SCSD bonus scheme or extra-valuable prizes in the *Showcase* round. We identified and omitted such episodes using https://www.priceisright.fandom.com, a collaborative website dedicated to *The Price Is Right*. We omit the one episode from the 1978-79 season because we cannot reliably estimate the expected showcase value for that season.

Figure 1: Average stated retail price of showcases across seasons



Notes: The figure shows the average stated retail price of showcases for every season. Error bars depict standard errors around the mean. Horizontal lines indicate the most relevant expected showcase values at which the optimal stopping thresholds change. Table A1 in the Appendix shows the number of included showcases per season.

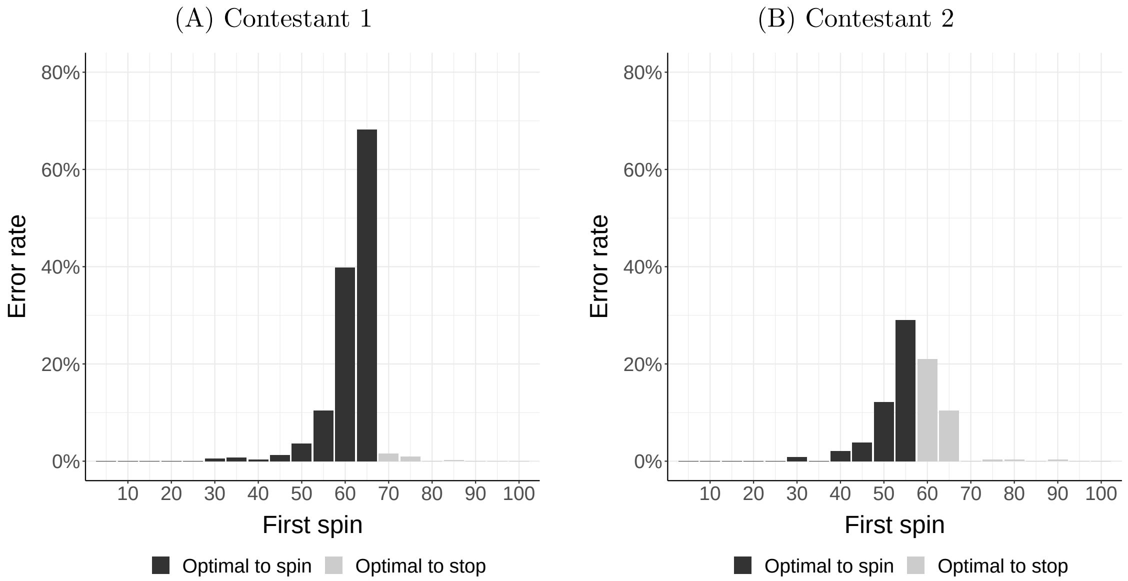
tion that this subjective value equals the average stated retail price of the showcases in the given season, and examine the sensitivity of our results to this assumption in Section 4.4.

Figure 1 shows the average stated retail price per season. Throughout our sample period, this average increased from $7,838 (1979-80) to $29,342 (2020-21), or by approximately 3.3 percent per year. For comparison, the inflation in the US over this period was 3.0 percent per year (US Consumer Price index; OECD, 2021). The horizontal lines indicate the most relevant thresholds at which the optimal stopping rules change. The jumps reflect the change of the bonus prizes. At any expectation higher than these thresholds, the stopping rules remain the same.

For Contestant 1, the average retail price was always well above the threshold values of $\$4,180$ (until 2008-09) and $\$6,792$ (from 2008-09 onwards). Hence, throughout our entire sample period Contestant 1 optimizes their play by stopping if and only if their first spin is 70 or higher. For Contestant 2 we need to distinguish between situations where their first spin beats the score of Contestant 1, and situations where they tie.[12] Contestant 2's optimal stopping rule in situations where their first spin beats Contestant 1's score was also constant over time: the average

[12]Ties are relatively rare. Out of the 4,488 spinning situations that we have for Contestant 2, only 384 (8.6%) are ties.

Figure 2: Deviations from the SPNE



Notes: The figure shows how often the decisions of Contestant 1 (Panel A, N=10,071) and Contestant 2 (Panel B, N=4,104) deviate from the optimal strategy, for every possible first-spin outcome. Panel B omits ties and thus exclusively considers choice situations where the first-spin outcome of Contestant 2 beats the score of Contestant 1. Dark gray bars depict first-spin outcomes at which it is optimal to spin, light gray bars depict first-spin outcomes at which it is optimal to stop.

retail price never exceeded the critical values of $27,826 (until 2008-09) and $45,217 (from 2008-09 onwards), which means that they should stop if and only if their first spin is 60 or higher. For ties the optimal stopping rule did change. Most of the time—from the 1983-84 season onward—Contestant 2 was best off by stopping if and only if the tie was at 70 or higher. Until the start of the 1983-84 season the stopping threshold was 75.

When we compare contestants' actual decisions with the optimal decisions, we observe that only a small proportion deviate. For Contestant 1, 93.4 percent of the 10,071 decisions are in accordance with the equilibrium strategy. For Contestant 2, 95.9 percent of the 4,488 decisions are optimal. These low rates at which contestants depart from optimality are not very surprising, because most decisions are easy. When we exclusively consider "difficult" choice situations—which we define as situations where the first-spin outcome is no more than two steps below the stopping threshold and no more than one step above it—we find that 72.9 percent of the 2,069 decisions of Contestant 1 and 79.5 percent of the 790 decisions of Contestant 2 are in accordance with the equilibrium strategy. Hence, for these more difficult choice situations, the rates of departure from optimality are considerable.

The deviations tend to be in one direction. If Contestant 1 and 2 were to follow the optimal strategy, they would spin in 65.9 and 32.4 percent, respectively, of all situations in our sample. In reality, however, they spin only 59.6 and 30.9 percent of the times. For the more difficult situations, the optimal spinning rates are 49.1 and 46.3 percent, whereas the actual rates are only 23.3 and 39.7 percent. Hence, these global spinning statistics indicate that there is systematic underspinning, especially for Contestant 1.

Figure 2 shows how often Contestant 1 (Panel A) and Contestant 2 (Panel B) deviate from the optimal strategy, for every possible first-spin outcome.[13] The dark grey bars represent the deviations in situations where it is optimal to spin, the light grey bars show the deviations in situations where it is optimal to stop. Clearly, at first-spin outcomes of 60 and 65 Contestant 1 frequently departs from the equilibrium strategy. In these situations, spinning is optimal but many instead choose to stop. In contrast to these underspinning errors, Contestant 1 displays hardly any overspinning errors. For Contestant 2, the pattern looks different. Contestant 2 departs less frequently from the optimal strategy than Contestant 1. Moreover, in comparison with Contestant 1, their deviations from optimality are considerably more symmetric.

4 Analyses and Results

In the current section we propose and test three possible explanations for contestants' deviations from the SPNE. Section 4.1 introduces our baseline structural model, which allows for the possibility that contestants make random evaluation errors. Section 4.2 then extends this model with the possibility of omission bias, whereas Section 4.3 instead extends it to allow for the possibility of limited foresight. Section 4.4 presents several robustness checks, which include tests of various alternative explanations. Last, Section 4.5 exploits the longitudinal dimension of the data to explore whether there is evidence of learning over the years.

4.1 Random Errors

The SPNE is based on the assumption that contestants perfectly maximize their expected utility, and never make mistakes. In reality, people of course will make mistakes. In the SCSD, the costs of mistakes vary between choice situations, and strongly depend on a contestant's first spin outcome. In situations where spinning is

[13]Figure 2 omits the (relatively rare) choice situations of Contestant 2 where they are tied with Contestant 1, because the optimal stopping threshold is different for these situations.

only slightly better than stopping, or vice versa, even a small evaluation error could lead a contestant to deviate from the optimal choice. Depending on the relative costs of over- or underspinning across all choice situations, random errors can lead to a pattern of systematic deviation from the SPNE.

Moreover, a player who realizes that the choices of their opponents are not flawless should take this into account in determining their optimal strategy. Factoring in the mistakes of others may lead to optimal strategies that differ from the SPNE (Goeree and Holt, 2001; Goeree et al., 2002, 2003). In the SCSD, mistakes of subsequent opponents generally lower the incentive to spin again. Therefore, in theory, the anticipation of mistakes could explain the underspinning as compared to the SPNE.

To examine the role of random errors, we adopt the Quantal Response Equilibrium (QRE) concept (McKelvey and Palfrey, 1995; Chen et al., 1997). The QRE is a stochastic generalization of the Nash equilibrium, and commonly used to account for bounded rationality in strategic settings (see, for example, Capra et al., 1999; Anderson et al., 2001; Goeree et al., 2002, 2003; Moinas and Pouget, 2013; Goeree et al., 2016, 2017). The main underlying idea is that people make random mistakes in evaluating the expected utilities of choice alternatives, and that they anticipate that others do the same. Because the SCSD is a sequential game, we consider the Agent Quantal Response Equilibrium (AQRE), a modification of the QRE for extensive-form games (McKelvey and Palfrey, 1998). The AQRE concept has found many applications (see, for example, Fey et al., 1996; McKelvey and Palfrey, 1998; Deck, 2001; Cason and Reynolds, 2005; Cai and Wang, 2006; McKelvey and Patty, 2006; Fehr et al., 2021).

Almost all of Contestant 3's decisions are trivial, and therefore we assume that Contestant 1 and 2 expect Contestant 3 to play their SPNE strategy without error. Similarly, we assume that Contestant 1 does not expect Contestant 2 to err after a first-spin outcome that is worse than Contestant 1's score, because Contestant 2 by default always spins again in such situations.

For all nontrivial choice situations, let $EU_{ij}^{s}(\cdot)$ denote the expected utility of action $s \in \{Spin, Stop\}$ for Contestant $i \in \{1, 2\}$ in SCSD $j \in \{1, 2, \ldots, J\}$. Contestants make random evaluation errors ε_{ij}^{s} and mistakenly consider $\widehat{EU}_{ij}^{s}(\cdot) = EU_{ij}^{s}(\cdot) + \varepsilon_{ij}^{s}$. Following convention, we assume that ε_{ij}^{s} is independently and identically distributed according to an extreme value distribution, which leads to the following predicted spin probabilities (Goeree et al., 2005; Haile et al., 2008; Goeree et al., 2020):

$$P_{ij}^{Spin} = \frac{e^{\lambda EU_{ij}^{\text{Spin}}}}{e^{\lambda EU_{ij}^{\text{Spin}}} + e^{\lambda EU_{ij}^{\text{Stop}}}} \tag{1}$$

λ can be interpreted as contestants' rationality parameter or payoff sensitivity. If $\lambda \to 0$, contestants make completely random choices and spin with a 50 percent likelihood; if $\lambda \to \infty$, they follow the payoff-maximizing strategy with certainty. In Section 4.4, we consider a more flexible specification that allows λ to differ between Contestant 1 and 2.

The expected utilities of spinning and stopping depend on the resulting probability of winning the SCSD, the shape of the utility function, the chance of winning the showcase after winning the SCSD, and the showcase value; for spinning, the expected utility in addition depends on the bonus prizes. In our main analyses we assume risk neutrality. We also assume that contestants believe that they have a 50 percent chance of winning the showcase after winning the SCSD, and that the expected showcase value equals the average stated retail price of all showcases in the entire running season. We examine the sensitivity of the results to these assumptions in Section 4.4.

We convert all nominal monetary values to 2015 dollars using the US Consumer Price Index (OECD, 2021).[14] To obtain more readable coefficients, we divide the monetary values by 1,000. The next subsections expand this baseline model with additional parameters that capture omission bias and limited foresight. We use maximum likelihood techniques to estimate the parameters.

Table 2 presents the results. To compare how well the baseline AQRE model explains contestants' behavior relative to the SPNE, we consider three goodness-of-fit statistics: the hit rate, the Brier score, and the spinning bias.

The hit rate of the model is the fraction of correctly predicted decisions. A prediction is defined as correct if the model assigns a 50 percent or greater probability to the contestant's actual decision. The baseline model correctly predicts 93.4 percent of Contestant 1's decisions and 95.9 percent of Contestant 2's decisions. These high hit rates are not surprising, because most decisions in our sample are easy. For relatively difficult choice situations—where the first-spin outcome is no more than two steps below the stopping threshold and no more than one step above it —the hit rate of the baseline model is 72.9 percent for Contestant 1 and 79.5 percent for Contestant 2. These hit rates are identical to those for the SPNE, suggesting that allowing for evaluation errors does not add any descriptive power. Due to the binary nature of "hits", however, the measure is rather crude. In contrast to the SPNE,

[14]For completeness, Section 4.4 also gives the results without correcting for inflation.

Table 2: Estimation results

	SPNE		Baseline		Omission bias		Limited foresight		OB & LF	
λ	-	-	1.423	(0.027)	1.677	(0.035)	1.627	(0.037)	1.616	(0.038)
γ	-	-	-	-	1.052	(0.046)	-	-	-0.112	(0.090)
β	-	-	-	-	-	-	0.379	(0.014)	0.408	(0.028)
N	-		14,559		14,559		14,559		14,559	
Log-likelihood	-		-2,049		-1,814		-1,701		-1,701	
AIC	-		4,101		3,633		3,407		3,407	
	C1	C2	C1	C2	C1	C2	C1	C2	C1	C2
Hit rate	0.934	0.959	0.934	0.959	0.953	0.943	0.953	0.959	0.953	0.959
Hit rate (difficult)	0.729	0.795	0.729	0.795	0.817	0.705	0.817	0.795	0.817	0.795
Brier score	0.066	0.041	0.046	0.035	0.035	0.039	0.034	0.035	0.034	0.035
Brier score (difficult)	0.271	0.205	0.177	0.173	0.125	0.197	0.120	0.174	0.120	0.174
Spinning bias	-0.063	-0.014	-0.041	0.001	-0.008	0.027	0.003	0.001	0.003	-0.002
Spinning bias (difficult)	-0.258	-0.066	-0.217	0.010	-0.066	0.147	-0.010	0.014	-0.010	-0.001

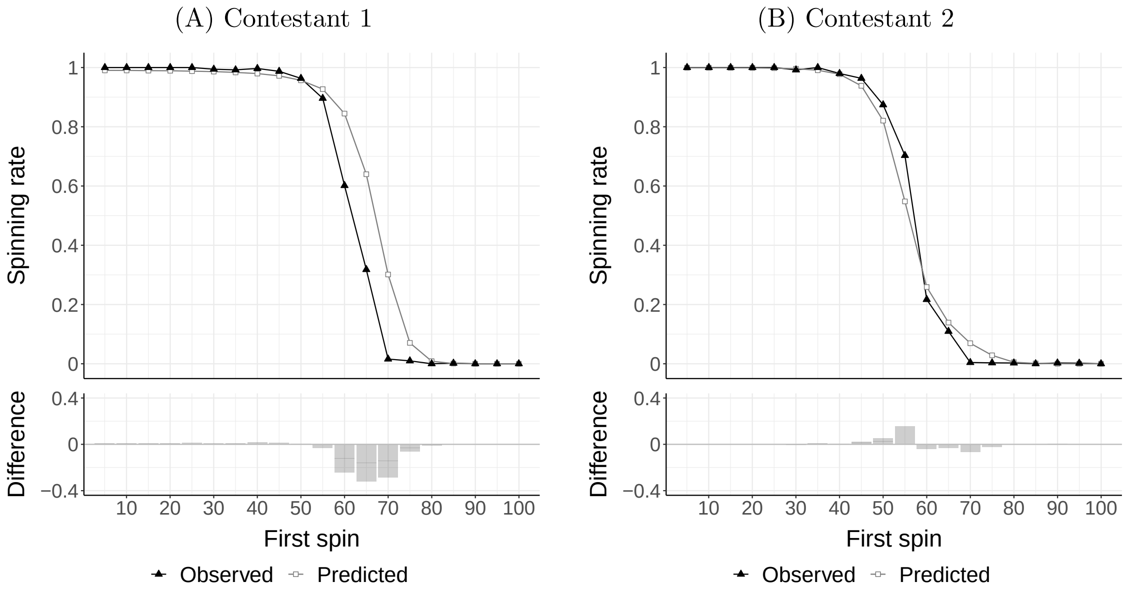
Notes: The table shows the estimated parameters and goodness-of-fit of various structural models of strategic decision making. *SPNE* is the model that adopts the binary predictions from the subgame perfect Nash equilibrium, *Baseline* is the baseline AQRE model, *Omission bias* is the AQRE model that incorporates omission bias, *Limited foresight* is the AQRE model that allows for limited foresight, and *OB & LF* is the AQRE model with both omission bias and limited foresight. λ is the estimated rationality parameter, γ is the estimated disutility of self-elimination, and β is the estimated probability of limited foresight. Standard errors are in parentheses. N is the number of spinning decisions, *Log likelihood* is the log likelihood value of the estimation, and *AIC* is the Akaike Information Criterion value. Other goodness-of-fit measures are given separately for Contestant 1 (*C1*) and Contestant 2 (*C2*), both for all choice situations combined and for relatively difficult choice situations only. Difficult choice situations are choices where the first-spin outcome is no more than two steps below the stopping threshold and no more than one step above it. *Hit rate* is the fraction of correctly predicted decisions, *Brier score* is the mean squared prediction error, and *Spinning bias* is the average difference between contestants' actual spinning decisions and the model's spinning predictions.

the predictions of the AQRE are probabilistic, and much of the variation in these probabilities is not reflected in the hit rate.

To assess the difference between the observed choices and the probabilistic predictions, we calculate the Brier score (Brier, 1950). The Brier score is the mean squared prediction error. For the binary predictions of the SPNE, the Brier score is the complement of the hit rate. Compared to the Brier scores for the SPNE, those for the baseline model are substantially lower. The improvement is especially strong for Contestant 1; for difficult decisions, for example, the statistic declines from 0.271 to 0.177.

The Brier score is a good measure to assess overall predictive accuracy, but it is uninformative of the degree to which the model systematically over- or under-predicts contestants' propensity to spin. To visually explore whether there is any systematic deviation, Figure 3 plots the actual spinning rates against the probabilistic predictions of the baseline AQRE model for every possible first-spin outcome. The figure clearly shows that Contestant 1 underspins relative to the predictions. At first-spin outcomes of 60, 65 and 70, the fraction of contestants who actually use their second spin is 25-30 percentage points lower than predicted. For Contestant 2 the differences are much smaller, with the actual spinning rate on average being

Figure 3: Empirical spinning rates and baseline model predictions



Notes: The figure shows the observed spinning rate and the probabilistic prediction of the baseline model for Contestant 1 (Panel A, N=10,071) and for Contestant 2 (Panel B, N=4,488) for all possible first-spin outcomes. The lower parts of the panels show the differences between the observed and predicted spinning rates.

slightly higher than predicted.

The spinning bias quantifies the degree of systematic deviation, and is calculated as the average difference between contestants' actual spinning decisions, which take a value of either 0 (stop) or 1 (spin), and the model's probabilistic spinning predictions, which can take any value between 0 and 1. A positive value of this goodness-of-fit statistic indicates overspinning, a negative value underspinning. Confirming the pattern in Figure 3, the spinning bias is negative for Contestant 1: -4.1 percentage points at the aggregate level, and -21.7 percentage points for the relatively difficult first-spin outcomes. This degree of contestants' systematic underspinning according to the baseline model is high, but lower than the negative spinning bias of Contestant 1 relative to the SPNE (-6.3 and -25.8 percentage points, respectively). For Contestant 2, the spinning bias is positive and close to zero: 0.1 percentage points across all choices, and 1.0 for the more difficult ones.

Taken together, these findings suggest that random evaluation errors can explain some of the deviations from the SPNE. The systematic underspinning of Contestant 1, however, remains largely unexplained.

4.2 Omission Bias

Tenorio and Cason (2002) analyze a sample of 282 renditions of the SCSD from 1994 and 1995, and similarly report that contestants tend to stop when it is actually optimal to spin. Their evidence derives primarily from Contestant 1, as their sample of informative decisions of Contestant 2 is too small to draw reliable conclusions. Tenorio and Cason propose that the underspinning can be explained by omission bias—the tendency to favor harmful inactions over harmful actions (Ritov and Baron, 1990, 1992; Spranca et al., 1991; Feldman et al., 2020). Other research shows that omission bias can play an important role in settings where decision makers face a choice between action and inaction. Examples include vaccination decisions, debt repayment, blackjack, and sports refereeing (Ritov and Baron, 1990; Asch et al., 1994; Carlin and Robinson, 2009; DiBonaventura and Chapman, 2008; Moskowitz and Wertheim, 2011; Hallsworth et al., 2015).

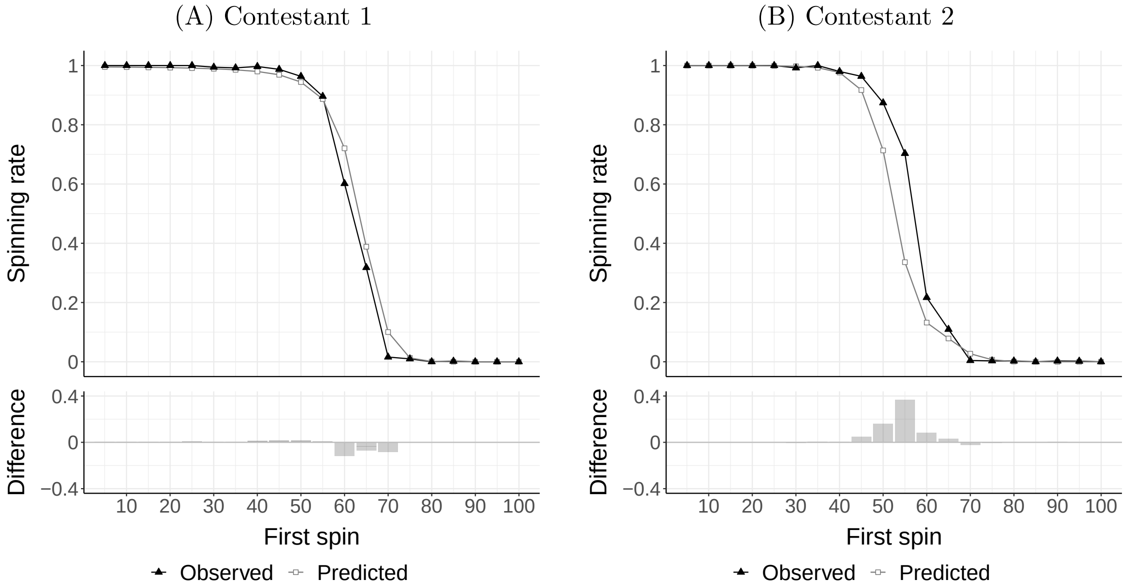
In the SCSD, contestants will be less likely to spin if they prefer elimination after not spinning (by an opponent who obtains a higher score) over elimination after spinning (by exceeding 100 points). To examine whether omission bias can explain the observed behavior, we extend the baseline structural model with γ, a parameter that captures the disutility of self-elimination.

Table 2 shows the results for the AQRE model with omission bias. The estimated value of γ is 1.052, implying that the disutility of losing through self-elimination is equivalent to the disutility of a monetary loss of \$1,052 (in 2015 dollars). This model explains contestants' choices better than the baseline model, also when we account for its additional parameter: both the log-likelihood and the AIC show a substantial improvement. A likelihood-ratio test confirms that the model with omission bias significantly outperforms the baseline model ($\chi^2(1) = 469.77$, $p < 0.001$).

The separate goodness-of-fit measures for the two contestants show that the omission bias model provides a better account of Contestant 1's decisions but a worse account of Contestant 2's decisions, as compared to the baseline model. For Contestant 1, the overall hit rate improves from 93.4 to 95.3 percent, and the hit rate for difficult decisions improves from 72.9 to 81.7 percent. The improved fit for Contestant 1 is also reflected in lower Brier scores. The opposite holds for Contestant 2: the overall hit rate deteriorates from 95.9 to 94.3 percent, the hit rate for more difficult decisions deteriorates from 79.5 to 70.5 percent, and the Brier scores increase.

Figure 4 compares the actual spinning rates and the probabilistic predictions of the omission-bias model for all first-spin outcomes. For Contestant 1, the actual spinning rates are relatively close to the predictions. As shown in Table 2 the

Figure 4: Empirical spinning rates and omission bias model predictions



Notes: The figure shows the observed spinning rate and the probabilistic prediction of the omission bias model for Contestant 1 (Panel A, N=10,071) and for Contestant 2 (Panel B, N=4,488) for all possible first-spin outcomes. The lower parts of the panels show the differences between the observed and predicted spinning rates.

remaining spinning bias of Contestant 1 is a mere -0.8 percentage points, which compares favorably to the -4.1 percentage points of the baseline model. For relatively difficult first-spin outcomes the degree of underspinning decreases from 21.7 to 6.6 percentage points.

The reduction of the systematic prediction error for Contestant 1, however, is largely offset by an increase for Contestant 2. Contestant 2 clearly overspins relative to the predictions of the omission bias model. Their spinning bias increases from 0.1 to 2.7 percentage points across all choices, and from 1.0 to 14.7 percentage points for the more difficult ones.

Altogether, omission bias thus fails to adequately explain contestants' behavior. The additional parameter captures the underspinning of Contestant 1 and improves the overall fit of the model, but at the same time introduces large systematic prediction errors for Contestant 2.

4.3 Limited Foresight

A possible alternative explanation for the suboptimal behavior of contestants is limited foresight. To simplify the decision problem, contestants may adopt a myopic representation and optimize their chances of beating the next contestant only. The notion that people reason only one or a few steps ahead has been proposed in a large body of theoretical research (Jehiel, 1995, 1998, 2001; Jackson and Wolinsky, 1996; Gabaix and Laibson, 2005; Ke, 2019; Bossaerts et al., 2022; Rampal, 2022), and is supported by experimental studies (Johnson et al., 2002; Gabaix et al., 2006; Mantovani, 2015; Rampal, 2022; Baranski and Reuben, 2023). If Contestant 1 only considers Contestant 2 in their spinning choice and ignores the presence of Contestant 3, then Contestant 1 will be less inclined to spin because beating only one subsequent contestant in expectation requires a lower score than beating two. For Contestant 2, limited foresight coincides with full backward induction because the next stage is also the last stage of the game.

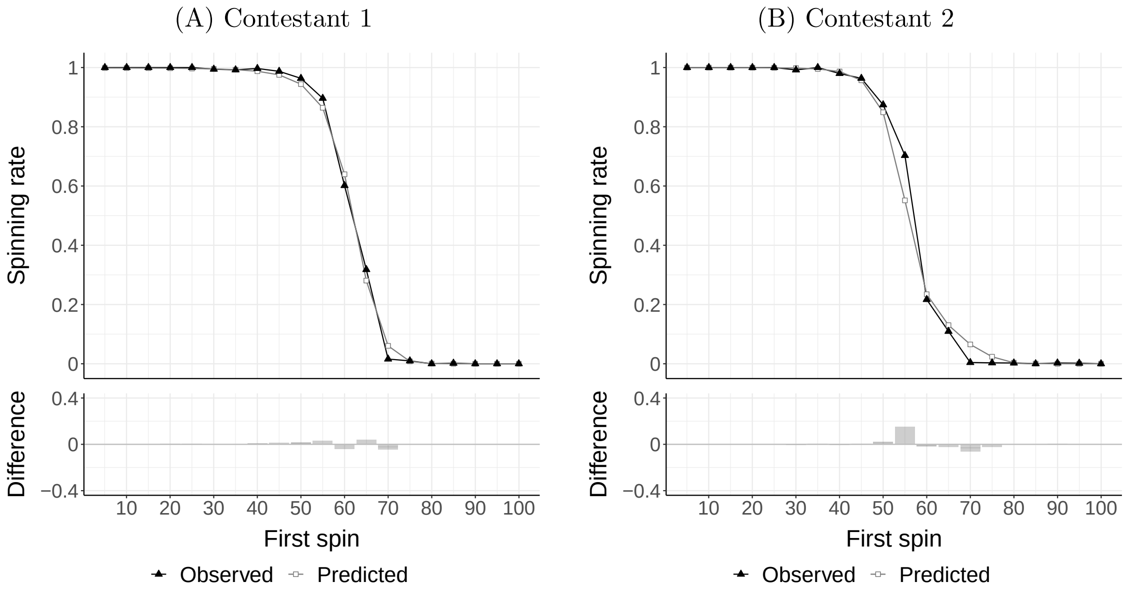
Our limited foresight model expands the baseline model with the possibility that contestants reduce complexity by considering the next contestant only. A contestant adopts this simplified frame with probability β, and correctly considers all future contestants with probability $1 - \beta$. We assume that myopic contestants believe that the next contestant behaves as if they are the last.

The penultimate column of Table 2 shows the results for the limited foresight model. The estimated β coefficient is 0.379, suggesting that 37.9 percent of the spinning decisions are made in accordance with limited foresight, while the remaining 62.1 percent are consistent with full backward induction. The empirical fit is much better than the fit of the baseline and omission bias models: both the log-likelihood and the AIC show considerable improvements. A likelihood-ratio test confirms that the current model outperforms the baseline model ($\chi^2(1) = 696.13$, $p < 0.001$), and a Vuong test for non-nested models confirms that it also outperforms the omission bias model ($Z = 5.56$, $p < 0.001$).

As compared to the omission bias model, the limited foresight model provides a slightly better account of Contestant 1's decisions, and a substantially better account of Contestant 2's decisions. For Contestant 1, the hit rates are identical to those of the omission bias model, and the Brier scores are marginally better. For Contestant 2, the overall hit rate increases from 94.3 to 95.9 percent, the hit rate for difficult first-spin outcomes increases from 70.5 to 79.5 percent, and the Brier scores improve considerably.

Figure 5 plots the actual spinning rates against probabilistic predictions of the limited foresight model, and shows that the model accurately captures the observed behavior. For both Contestant 1 and 2, the actual and predicted spinning rates approximately coincide. As also shown in Table 2, barely any spinning bias remains. Across all choices, Contestant 1 spins a negligible 0.3 percentage points more often than predicted by the model and Contestant 2 spins only 0.1 percentage points

Figure 5: Empirical spinning rates and limited foresight model predictions



Notes: The figure shows the observed spinning rate and the probabilistic prediction of the limited foresight model for Contestant 1 (Panel A, N=10,071) and for Contestant 2 (Panel B, N=4,488) for all possible first-spin outcomes. The lower parts of the panels show the differences between the observed and predicted spinning rates.

more often. For the more difficult choices, the spinning biases are a mere -1.0 and 1.4 percentage points, respectively.

The limited foresight model thus provides an accurate account of contestants' spinning decisions. To examine whether contestants' choices are in addition partly driven by omission bias, we estimate a model that allows for both omission bias and limited foresight. The final column of Table 2 shows the estimation results. The results clearly speak against omission bias as a possible driver. First, the omission bias parameter is negative, relatively small, and statistically insignificant. A negative value implies a preference for harmful actions over harmful inactions, which goes against the hypothesis. Second, the goodness-of-fit of the model is similar to that of the limited foresight model. Neither the log-likelihood nor the AIC value improves. Not surprisingly, a likelihood-ratio test does not reject the hypothesis that the two models explain spinning choices equally well ($\chi^2(1) = 1.55$, $p = 0.213$). The values for the hit rate, Brier score, and spinning bias are also very similar to those of the limited foresight model.

All in all, the conclusion from these analyses is that the behavior of contestants is well described by an AQRE model with limited foresight, where all contestants make random evaluation errors and many simplify the decision problem by myopically considering the next stage of the game only.

4.4 Robustness Checks

The structural models required a variety of assumptions. In the present subsection, we explore the sensitivity of our results to risk aversion (Section 4.4.1), to beliefs about the expected value of winning the SCSD (Section 4.4.2), to the weight attached to the opportunity of winning bonus prizes (Section 4.4.3), and to various other, more minor aspects (Section 4.4.4).[15]

4.4.1 Risk Aversion

The choice between spinning and stopping is essentially a choice between two risky prospects. In the analyses thus far we assumed that contestants are risk neutral. Here we explore the sensitivity of our results to the alternative assumption that contestants are risk averse. We now assume that they have a constant absolute risk aversion (CARA) utility function of the form $U(x) = 1 - \exp^{\theta x}$, where x is the monetary value of their prospective winnings and θ is the risk-aversion coefficient. We set θ such that the certainty equivalent of a 50-50 lottery of winning \$25,000 or \$0 is \$2,500.[16] To obtain more readable coefficients, we scale the utility function such that the utility of \$1,000 equals unity.

Table 3, Panel A presents the results. Introducing a considerable degree of risk aversion worsens the overall fit of the baseline model, and leaves the overall fit of the models with omission bias, limited foresight, and the combination of these largely unaffected. Contestant 1 still underspins compared to the predictions of the baseline model, whereas Contestant 2 still overspins compared to the predictions of the omission bias model. The limited foresight model again provides an accurate account of contestants' spinning decisions. Allowing for both omission bias and limited foresight yields no statistically significant improvement in explanatory power as compared to the limited foresight model (LR test: $\chi^2(1) = 2.16$, $p = 0.142$). The main conclusions from the previous analyses thus do not seem to hinge on the assumption of risk neutrality.

4.4.2 Discounting the Showcase Value

The optimal strategies and stochastic model predictions depend on the relatives sizes of the monetary bonus prizes and the expected value of the showcase. For the main

[15] Under some of the alternative assumptions of the robustness checks in this section, the optimal strategies are slightly different as compared to those in Table 1. For brevity, we do not discuss whether and how the optimal strategies change.

[16] We find similar results for more moderate degrees of risk aversion, for example when we set θ such that the certainty equivalent of the lottery is \$5,000 or \$10,000.

Table 3: Estimation results under alternative modeling choices (1/2)

	SPNE		Baseline		Omission bias		Limited foresight		OB & LF	
Panel A: Risk aversion										
λ	-	-	9.629	(0.181)	11.968	(0.252)	11.445	(0.260)	11.546	(0.268)
γ	-	-	-	-	0.181	(0.006)	-	-	0.019	(0.013)
β	-	-	-	-	-	-	0.447	(0.015)	0.411	(0.029)
N	-		14,559		14,559		14,559		14,559	
Log-likelihood	-		-2,157		-1,812		-1,703		-1,702	
AIC	-		4,316		3,628		3,411		3,411	
	C1	C2	C1	C2	C1	C2	C1	C2	C1	C2
Hit rate	0.934	0.959	0.934	0.955	0.953	0.945	0.953	0.955	0.953	0.959
Hit rate (difficult)	0.729	0.795	0.729	0.789	0.817	0.736	0.817	0.789	0.817	0.813
Brier score	0.066	0.041	0.049	0.035	0.035	0.039	0.034	0.035	0.034	0.035
Brier score (difficult)	0.271	0.205	0.195	0.158	0.126	0.182	0.121	0.158	0.121	0.157
Spinning bias	-0.063	-0.014	-0.048	-0.005	-0.008	0.027	0.002	-0.006	0.003	-0.002
Spinning bias (difficult)	-0.258	-0.066	-0.252	-0.016	-0.065	0.152	-0.009	-0.010	-0.009	0.008
Panel B: Discounted showcase value										
λ	-	-	2.649	(0.050)	3.298	(0.070)	3.148	(0.072)	3.183	(0.074)
γ	-	-	-	-	0.659	(0.023)	-	-	0.087	(0.046)
β	-	-	-	-	-	-	0.447	(0.015)	0.401	(0.028)
N	-		14,559		14,559		14,559		14,559	
Log-likelihood	-		-2,152		-1,806		-1,701		-1,700	
AIC	-		4,306		3,616		3,407		3,405	
	C1	C2	C1	C2	C1	C2	C1	C2	C1	C2
Hit rate	0.934	0.956	0.934	0.956	0.952	0.945	0.953	0.956	0.953	0.959
Hit rate (difficult)	0.729	0.789	0.729	0.789	0.815	0.728	0.817	0.789	0.817	0.809
Brier score	0.066	0.044	0.049	0.035	0.035	0.039	0.034	0.035	0.034	0.035
Brier score (difficult)	0.271	0.211	0.194	0.162	0.125	0.184	0.120	0.162	0.120	0.161
Spinning bias	-0.063	-0.018	-0.048	-0.006	-0.008	0.026	0.002	-0.006	0.002	-0.002
Spinning bias (difficult)	-0.258	-0.076	-0.251	-0.020	-0.064	0.150	-0.010	-0.015	-0.010	0.009
Panel C: No bonus prizes										
λ	-	-	1.525	(0.030)	1.706	(0.036)	1.677	(0.037)	1.640	(0.038)
γ	-	-	-	-	0.785	(0.045)	-	-	-0.400	(0.089)
β	-	-	-	-	-	-	0.311	(0.014)	0.416	(0.028)
N	-		14,559		14,559		14,559		14,559	
Log-likelihood	-		-1,965		-1,826		-1,713		-1,703	
AIC	-		3,932		3,655		3,430		3,412	
	C1	C2	C1	C2	C1	C2	C1	C2	C1	C2
Hit rate	0.934	0.943	0.934	0.943	0.953	0.943	0.953	0.943	0.953	0.957
Hit rate (difficult)	0.729	0.701	0.729	0.701	0.817	0.700	0.817	0.701	0.817	0.787
Brier score	0.066	0.057	0.042	0.036	0.035	0.040	0.034	0.036	0.034	0.035
Brier score (difficult)	0.271	0.299	0.161	0.171	0.125	0.195	0.120	0.172	0.120	0.171
Spinning bias	-0.063	0.026	-0.034	0.009	-0.008	0.028	0.003	0.009	0.003	-0.002
Spinning bias (difficult)	-0.258	0.141	-0.182	0.038	-0.068	0.145	-0.009	0.038	-0.009	-0.019

Notes: The table shows the estimated parameters and goodness-of-fit of the structural models under three alternative modeling choices. Panel A shows the results under the assumption that contestants have CARA utility, with a certainty equivalent of $2,500 for a 50-50 lottery of winning $25,000 or $0. Panel B shows the results under the assumption that contestants value showcases at 50 percent of the retail price. Panel C shows the results under the assumption that contestants ignore the bonus prizes. Other definitions are as in Table 2.

analyses, we assumed that the value of a showcase equals its stated retail price. The stated retail price is a natural and salient value, but in reality contestants will likely discount it. The showcase prizes are selected by the game show producers, not by the contestants themselves, and will therefore mostly not align well with contestants' preferences.[17] As a robustness check, we re-estimate the structural models under the alternative assumption that contestants value showcases at 50 percent of the retail

[17]Contestants should further discount the showcase value because of taxes. Although taxes are levied over both (monetary) bonus prizes and (generally non-monetary) showcase prizes, taxes generally make the showcase prizes relatively less attractive. The reason is that the showcase prizes are taxed on the basis of their (relatively high, non-discounted) retail prices.

price.[18]

Table 3, Panel B presents the results. Discounting the showcase value leads to a worse fit of the baseline model and stronger evidence of underspinning. This is not surprising, because a lower expected showcase value increases the relative attractiveness of the bonus prizes, and thus increases the incentive to spin a second time. Nevertheless, limited foresight still provides a better account of contestants' spinning decisions than omission bias, and extending the limited foresight model by allowing for omission bias does not yield a significant increase in explanatory power (LR test: $\chi^2(1) = 3.54$ and $p = 0.060$).

4.4.3 Ignoring Bonus Prizes

A possible explanation for underspinning is that contestants attach a relatively low weight to the possibility of winning one or two bonus prizes by obtaining a score of exactly 100. In this section, we re-estimate the structural models under the extreme assumption that contestants completely ignore the existence of the bonus prizes.[19]

Table 3, Panel C presents the results. As expected, ignoring the bonus prizes improves the overall fit of the baseline model. The fit of the models with omission bias, limited foresight, and the combination of these, however, is somewhat worse. More importantly, the limited foresight model still explains choices substantially better than both the baseline and the omission bias model. Combining omission bias and limited foresight yields significantly more explanatory power than limited foresight alone (LR test: $\chi^2(1) = 20.39$ and $p < 0.001$). The estimated omission bias parameter, however, is negative, which implies that people would have a preference for harmful actions over harmful inactions. Again, the results thus speak against omission bias as a possible driver.

4.4.4 Other Robustness Checks

We perform four additional analyses to examine the robustness of our results to alternative modeling choices. First, we weigh the observations of Contestant 1 and 2. The number of decisions of Contestant 1 in our sample is more than twice the number of decisions of Contestant 2, and consequently the choices of Contestant 1

[18]The effect of discounting the showcase value is equivalent to the effect of lowering contestants' perceived chance of winning the showcase after winning the SCSD. The present robustness test therefore also captures the possibility that this subjective probability is smaller than the 50 percent that we assumed in the main analyses.

[19]This robustness test also captures the possibility that contestants expect to derive relatively much utility from playing the *Showcase* final, for example because they are overconfident about their chances of winning the showcase, or because of the joy of "winning the episode".

Table 4: Estimation results under alternative modeling choices (2/2)

	SPNE		Baseline		Omission bias		Limited foresight		OB & LF	
Panel A: Weighting										
λ	-	-	1.384	(0.028)	1.508	(0.032)	1.518	(0.035)	1.513	(0.035)
γ	-	-	-	-	0.803	(0.050)	-	-	-0.076	(0.074)
β	-	-	-	-	-	-	0.381	(0.018)	0.401	(0.026)
N	-		14,559		14,559		14,559		14,559	
Log-likelihood	-		-1,964		-1,843		-1,722		-1,722	
AIC	-		3,930		3,689		3,448		3,449	
	C1	C2	C1	C2	C1	C2	C1	C2	C1	C2
Hit rate	0.934	0.959	0.934	0.959	0.952	0.943	0.953	0.959	0.953	0.959
Hit rate (difficult)	0.729	0.795	0.729	0.795	0.815	0.703	0.817	0.795	0.817	0.795
Brier score	0.066	0.041	0.046	0.035	0.037	0.037	0.034	0.035	0.034	0.035
Brier score (difficult)	0.271	0.205	0.176	0.173	0.132	0.185	0.120	0.173	0.120	0.173
Spinning bias	-0.063	-0.014	-0.040	0.001	-0.013	0.021	0.005	0.001	0.005	-0.001
Spinning bias (difficult)	-0.258	-0.066	-0.217	0.009	-0.104	0.112	-0.013	0.012	-0.013	0.002
Panel B: Separate rationality parameters										
λ_1	-	-	1.481	(0.032)	2.043	(0.055)	1.815	(0.049)	1.826	(0.053)
λ_2	-	-	1.204	(0.054)	1.055	(0.044)	1.231	(0.053)	1.227	(0.054)
γ	-	-	-	-	1.254	(0.045)	-	-	0.063	(0.109)
β	-	-	-	-	-	-	0.376	(0.014)	0.359	(0.032)
N	-		14,559		14,559		14,559		14,559	
Log-likelihood	-		-2,041		-1,732		-1,673		-1,673	
AIC	-		4,086		3,470		3,351		3,353	
	C1	C2	C1	C2	C1	C2	C1	C2	C1	C2
Hit rate	0.934	0.959	0.934	0.959	0.953	0.947	0.953	0.959	0.953	0.957
Hit rate (difficult)	0.729	0.795	0.729	0.795	0.817	0.725	0.817	0.795	0.817	0.784
Brier score	0.066	0.041	0.046	0.035	0.034	0.039	0.034	0.035	0.034	0.035
Brier score (difficult)	0.271	0.205	0.177	0.173	0.122	0.190	0.121	0.173	0.121	0.173
Spinning bias	-0.063	-0.014	-0.043	0.001	-0.005	0.031	0.000	0.001	0.000	0.003
Spinning bias (difficult)	-0.258	-0.066	-0.217	0.003	-0.029	0.127	-0.005	0.004	-0.005	0.012
Panel C: Nominal monetary values										
λ	-	-	1.827	(0.036)	2.146	(0.047)	2.103	(0.048)	2.108	(0.049)
γ	-	-	-	-	0.807	(0.036)	-	-	0.034	(0.062)
β	-	-	-	-	-	-	0.364	(0.014)	0.353	(0.024)
N	-		14,559		14,559		14,559		14,559	
Log-likelihood	-		-2,097		-1,866		-1,748		-1,748	
AIC	-		4,196		3,735		3,501		3,503	
	C1	C2	C1	C2	C1	C2	C1	C2	C1	C2
Hit rate	0.934	0.959	0.934	0.959	0.954	0.943	0.953	0.959	0.953	0.957
Hit rate (difficult)	0.729	0.795	0.729	0.795	0.823	0.701	0.817	0.795	0.817	0.784
Brier score	0.066	0.041	0.046	0.036	0.035	0.039	0.034	0.036	0.034	0.036
Brier score (difficult)	0.271	0.205	0.176	0.176	0.122	0.197	0.121	0.177	0.121	0.177
Spinning bias	-0.063	-0.014	-0.039	0.002	-0.005	0.027	0.002	0.001	0.002	0.003
Spinning bias (difficult)	-0.258	-0.066	-0.220	0.009	-0.075	0.142	-0.019	0.013	-0.018	0.019
Panel D: Last season's showcase values										
λ	-	-	1.407	(0.027)	1.657	(0.035)	1.609	(0.036)	1.597	(0.037)
γ	-	-	-	-	1.059	(0.046)	-	-	-0.126	(0.091)
β	-	-	-	-	-	-	0.379	(0.015)	0.411	(0.028)
N	-		14,475		14,475		14,475		14,475	
Log-likelihood	-		-2,043		-1,812		-1,697		-1,696	
AIC	-		4,089		3,628		3,398		3,398	
	C1	C2	C1	C2	C1	C2	C1	C2	C1	C2
Hit rate	0.935	0.959	0.935	0.959	0.952	0.944	0.952	0.959	0.952	0.959
Hit rate (difficult)	0.731	0.794	0.731	0.794	0.817	0.706	0.817	0.794	0.817	0.794
Brier score	0.065	0.041	0.046	0.035	0.035	0.039	0.034	0.035	0.034	0.035
Brier score (difficult)	0.269	0.206	0.176	0.173	0.125	0.197	0.120	0.174	0.120	0.175
Spinning bias	-0.063	-0.015	-0.041	0.001	-0.008	0.027	0.003	0.001	0.003	-0.002
Spinning bias (difficult)	-0.258	-0.066	-0.217	0.011	-0.066	0.148	-0.009	0.015	-0.009	-0.002

Notes: The table shows the estimated parameters and goodness-of-fit under four alternative modeling choices. Panel A shows the results when observations are weighted such that the overall weights of Contestant 1 and 2 are equal and the average weight across individual contestants is unity. Panel B shows the results when the rationality parameter, λ, is allowed to differ between Contestant 1 and 2. Panel C shows the results when nominal instead of real monetary values are used. Panel D shows the results under the assumption that the expected showcase value equals the average stated retail price of all showcases in the previous season instead of the running season. Other definitions are as in Table 2.

may have determined the results of the main analyses more than those of Contestant 2. To correct for this imbalance, we now weigh the observations of Contestant 1 by $(N_1 + N_2)/2N_1$ and those of Contestant 2 by $(N_1 + N_2)/2N_2$, such that the overall weights for the two types of contestants become equal while the average weight across all individual contestants remains unity. Second, we increase the flexibility of the structural models by allowing Contestant 1 and 2 to have different rationality parameters. As a third robustness check, we use the original, nominal monetary values instead of the inflation-corrected, real monetary values. Last, we assume that the expected showcase value equals the average stated retail price of all showcases in the previous season instead of the running season.

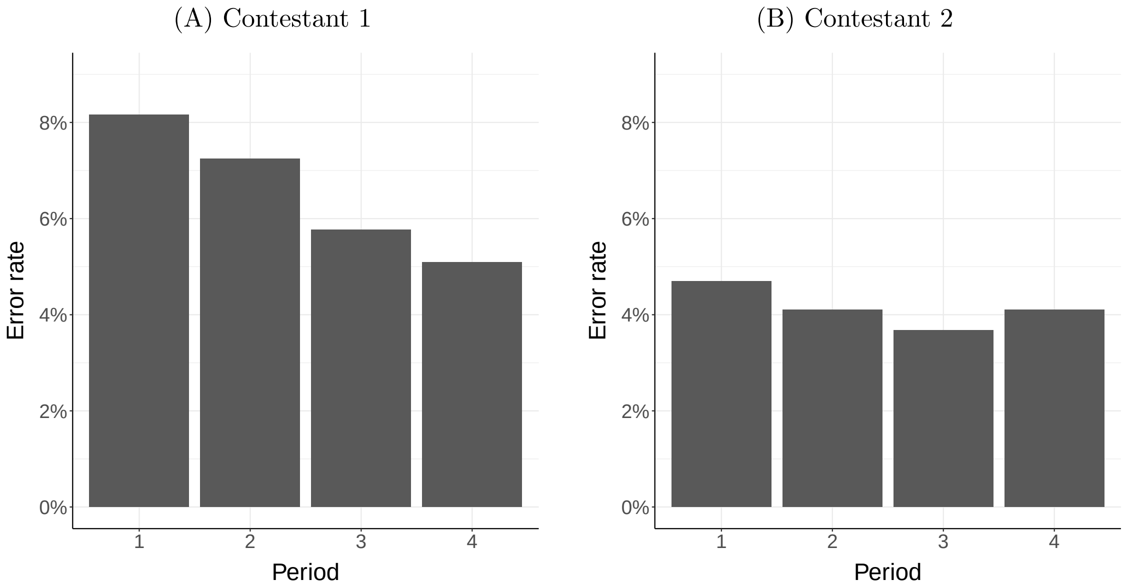
The four sets of results are in Table 4. In all cases, the limited foresight model provides a much better account of contestants' choices than the baseline and omission bias models. Allowing for both omission bias and limited foresight never results in significantly more explanatory power than limited foresight alone (LR tests: all $\chi^2(1) < 1.89$ and all $p > 0.169$). The likelihood that a contestant myopically considers the next stage of the game only is barely affected by the alternative approaches: the limited foresight parameter is always close to the 37.9 percent that we found previously.

Finally, a possible concern may be that the order in which contestants take turns spinning the wheel is not random, but determined by the sum of the prizes they won in the previous games. This can be problematic if there is a relationship between prior winnings and their rationality. Such a relationship, however, is not very likely because the nature of the prior games is such that winnings are to a large extent driven by luck. Moreover, empirically there is no evidence of such a relationship. When we regress the likelihood of departures from the optimal strategy on prior winnings, the regression coefficient is economically and statistically insignificant, regardless of whether we consider a linear or a log-linear relationship, and regardless of whether we consider all choices or difficult choices only. The exact results are in Table A2 in the Appendix.

4.5 Learning

The SCSD has been running uninterruptedly for more than 40 years. This long history opens up the possibility to investigate whether behavior converges towards the rational equilibrium strategies over time, as contestants can learn about the game and the behavior of their opponents. In laboratory experiments, game theory often describes the behavior of experienced subjects better than that of inexperienced subjects (Fudenberg and Levine, 1998, 2009, 2016). Although SCSD contestants cannot

Figure 6: Deviations from the SPNE per period



Notes: The figure shows the fraction of spinning decisions by Contestant 1 (Panel A) and Contestant 2 (Panel B) that deviate from the SPNE, for four different time periods. The first period covers seasons 1979-80 to 1992-93, the second 1993-94 to 2007-08, the third 2008-09 to 2014-15, and the fourth 2015-16 to 2020-21.

gain experience themselves, they can potentially learn by observing the choices and outcomes of others (Duffy and Feltovich, 1999; Armantier, 2004; Simonsohn et al., 2008). Over time, the number of existing episodes has grown, and episodes have become more readily available online. In addition, with the advent of the internet and modern communication technologies, people have become better able to share and discuss the optimal strategies.

To explore whether there is any evidence of learning, we divide our data into four different time periods: (i) seasons 1979-80 to 1992-93, (ii) 1993-94 to 2007-08, (iii) 2008-09 to 2014-15, and (iv) 2015-16 to 2020-21.[20]

For each of the four time periods, Figure 6 shows how often Contestant 1 and 2 deviate from the SPNE. For Contestant 1, there is a clear downward trend in the frequency of mistakes: the rate decreases monotonically from 8.2 percent in Period 1 to 5.1 percent in Period 4. For Contestant 2, by contrast, there is no clear trend in the quality of spinning choices over time.

Of course comparing behavior in different time periods in this way is rather crude, because due to the changing expected showcase value and the two different bonus schemes the costs of mistakes can be very different at different points in time. The

[20]We first separate the data for the two different bonus schemes, and then split the data for each bonus scheme into two periods of roughly equal length.

Table 5: Estimation results per period

	Period 1		Period 2		Period 3		Period 4	
λ	1.469	(0.086)	1.564	(0.057)	1.711	(0.077)	1.857	(0.095)
β	0.554	(0.048)	0.435	(0.023)	0.335	(0.027)	0.238	(0.029)
N	2,012		5,876		3,770		2,901	
Log-likelihood	-255		-683		-410		-326	
AIC	515		1,369		823		656	
	C1	C2	C1	C2	C1	C2	C1	C2
Hit rate	0.942	0.953	0.950	0.959	0.955	0.963	0.960	0.959
Hit rate (difficult)	0.792	0.781	0.807	0.786	0.831	0.798	0.826	0.815
Brier score	0.038	0.036	0.034	0.035	0.032	0.031	0.029	0.042
Brier score (difficult)	0.114	0.158	0.117	0.175	0.112	0.167	0.119	0.192
Spinning bias	0.002	-0.006	0.003	-0.003	0.002	-0.001	0.003	0.019
Spinning bias (difficult)	-0.016	-0.021	-0.014	-0.013	-0.005	-0.006	-0.007	0.106

Notes: The table shows the estimated parameters and the goodness-of-fit of the structural model with limited foresight for four different time periods. The first period covers seasons 1979-80 to 1992-93, the second period 1993-94 to 2007-08, the third period 2008-09 to 2014-15, and the fourth period 2015-16 to 2020-21. Other definitions are as in Table 2.

structural models account for such changes and show a similar pattern across the four time periods. Table 5 gives the period-by-period estimation results for the limited foresight model. The fraction of spinning decisions that are made in accordance with limited foresight more than halves over time: β decreases monotonically from 55.4 to 23.8 percent. In addition, the rationality parameter λ increases monotonically from 1.469 to 1.857.

The improved decision making over time is in line with learning. The results for the last period, however, show that even after more than forty years of *The Price Is Right*, a sizable proportion of contestants remain unable to follow the optimal strategies deriving from backward induction.

5 Conclusion and Discussion

The present paper examines high-stakes strategic decision making in the *Showcase Showdown* (SCSD), a sequential game of perfect information that is part of the long-running American TV game show *The Price Is Right*. The optimal strategies for this game can be found through backward induction. Most tests of the descriptive validity of backward induction as a solution concept rely on controlled laboratory experiments.[21] The SCSD provides an appealing alternative test bed, allowing for assessing the descriptive validity under conditions that are markedly different. The high stakes and ample learning opportunities provide a particularly benign setting for game-theoretic predictions to hold.

In spite of this, we find that contestants systematically deviate from the unique

[21] One exception is Spenkuch et al. (2018), who find that voting behavior of US Senators during roll-call votes is largely consistent with the equilibrium predictions of a model in which the senators rely on backward induction.

subgame perfect Nash equilibrium. Their behavior is well explained by an agent quantal response equilibrium model that not only allows for random evaluation errors but also for limited foresight.[22] The results suggest that contestants are likely to simplify the decision problem by adopting a myopic representation and optimize their chances of beating the next contestant only. Omission bias, risk aversion, and overconfidence cannot explain the deviations from the equilibrium strategies. In line with learning, we find that the degree of limited foresight decreases over the course of our sample period, but both systematic and non-systematic deviations remain commonplace, even after several decades.

Various published papers have derived the equilibrium strategies for the SCSD. Apparently, many contestants do not take heed of this information before coming on the show. This is consistent with research that demonstrates that people frequently do not use important and readily available information when they make important decisions (for an overview, see Handel and Schwartzstein, 2018). Such ignorance is rational if the search costs outweigh the expected benefits (Stigler, 1961). For the SCSD, the expected benefits of thorough preparation are low: only six out of the several hundred audience members who travel to the recording studio actually play the SCSD, and only a fraction of those six end up in a relatively difficult choice situation where knowing the optimal strategy may truly be helpful. For many laypeople, the low expected benefits probably do not outweigh the costs of looking up and reading a rather complicated academic paper.

The Price Is Right can be seen as an atypical setting to test the descriptive validity of backward induction, and critics may therefore view it as a negative distraction. However, novel settings should not be too easily dismissed as they can provide rare opportunities for relevant tests of economic theory (List, 2023). The SCSD uniquely allows for a large-scale analysis of strategic decision making at stakes that are impossible to replicate in the lab.

Nevertheless, following List (2023), it is important to explicitly consider how selection procedures and the naturalness of our setting may affect the generalizability of our results. Before contestants play the SCSD, they self-selected into the audience, were selected from the audience by the producers, and won a *One Bid* game. Unfortunately, it is unclear whether these elements of selection have led to any under- or overrepresentation of strategically sophisticated contestants. Selection effects, however, are inevitable in any lab or field setting. Moreover, SCSD contestants are quite diverse in terms of demographic characteristics, such as age, gender, ethnicity,

[22] Chakraborty and Kendall (2023) analyze a single-player decision problem that requires subjects to reason contingently about their own decisions at hypothetical future events, and similarly find that behavior is best described by a model that combines QRE-like noise and limited foresight.

and education, and as a group they seem to resemble a cross-section of the general population more closely than the subject pools of most laboratory experiments.

The setting in which contestants make their decisions is rather unusual. The presence of a studio audience and camera's likely induces stress. Psychological research indicates that the mere presence of others can facilitate performance in simple tasks but impair it in more complex ones (Zajonc, 1965; Bond and Titus, 1983). We cannot fully dismiss the impact the setting may have had on contestants, but prior research suggests that our findings are unlikely to be an artifact of the setting. Tenorio and Cason (2002) compare the behavior of laboratory subjects who play the SCSD to that of real contestants, and Antonovics et al. (2009), Healy and Noussair (2004), and Baltussen et al. (2016) make such a comparison for other games or game shows. None of these studies find that the patterns of behavior are different between the two settings. Moreover, every setting—including the experimental laboratory—is in some way special. It is impossible to study behavior under each and every possible set of conditions, and hence the optimal approach is to investigate if similar patterns are found in settings that are markedly different.

The finding that contestants in the SCSD often deviate from the optimal strategy and instead behave as if they adopt a simplified representation of the game adds to an ongoing debate about whether cognitive biases disappear in high-stake situations (Levitt and List, 2007*a*,*b*). Experimental research by Smith and Walker (1993), Cooper et al. (1999), Rapoport et al. (2003), and Parravano and Poulsen (2015) finds that the decisions of subjects tend to be closer to equilibrium play when the monetary incentives are higher. At the same time, Camerer and Hogarth (1999) and Enke et al. (2021) find that cognitive errors in experiments are largely impervious to the size of the stakes. Our results align with the findings of the latter two studies, and show that random and systematic violations of game-theoretic predictions abound in a high-stakes game that subjects can be expected to be highly familiar with.

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Appendix

Table A1: Data coverage per season

Season	Episodes	SCSDs	Showcases
1979-1980	32	61	60
1980-1981	30	54	58
1981-1982	31	59	60
1982-1983	192	368	378
1983-1984	93	174	179
1984-1985	16	28	31
1985-1986	15	27	29
1986-1987	65	122	118
1987-1988	22	41	39
1988-1989	25	49	49
1989-1990	25	49	49
1990-1991	15	27	28
1991-1992	83	163	163
1992-1993	80	151	160
1993-1994	75	134	92
1994-1995	111	207	175
1995-1996	112	201	223
1996-1997	140	227	268
1997-1998	116	189	219
1998-1999	130	240	256
1999-2000	134	264	265
2000-2001	171	309	341
2001-2002	182	363	364
2002-2003	173	345	346
2003-2004	170	340	294
2004-2005	159	316	255
2005-2006	168	336	267
2006-2007	150	249	125
2007-2008	179	354	353
2008-2009	190	372	375
2009-2010	188	375	374
2010-2011	189	370	372
2011-2012	192	376	382
2012-2013	186	361	370
2013-2014	193	378	386
2014-2015	187	369	373
2015-2016	193	383	385
2016-2017	178	355	356
2017-2018	175	350	350
2018-2019	176	350	351
2019-2020	158	313	316
2020-2021	136	272	272

Notes: The table displays the coverage of our sample per season. *Episodes* is the number of episodes for which we have the data for at least one of the two SCSDs. *SCSDs* is the number of SCSDs for which we have all spinning decisions and outcomes. *Showcases* is the number of showcases for which we know the stated retail price.

Table A2: Optimal choices and prior winnings

	All choices		Difficult choices	
	Model 1	Model 2	Model 3	Model 4
Prior winnings	0.00003		0.0003	
	(0.00005)		(0.0002)	
ln(Prior winnings)		0.001		0.001
		(0.002)		(0.007)
Fixed effects	Yes	Yes	Yes	Yes
Observations	12,665	12,665	2,491	2,491

Notes: The table shows regression results for the relationship between departures from optimality and prior winnings. The dependent variable is a dummy variable that takes the value of 1 if the contestant follows the optimal strategy according to the SPNE, and 0 otherwise. *Prior winnings* is the inflation-corrected monetary value of the prizes won by the contestant prior to the SCSD, in thousands of dollars. *ln(Prior winnings)* is the natural logarithm of *Prior winnings.* Fixed effects allow for differences in the average likelihood of a departure from optimality across first-spin outcomes, separately for both Contestant 1 and 2, and, in the case of Contestant 2, for whether their first spin beats or ties the previous contestant's score. Models 1 and 2 are estimated on all observations for which prior winnings are available in our data; Models 3 and 4 are estimated on relatively difficult choice situations only. Difficult choices are choices where the first-spin outcome is no more than two steps below the stopping threshold and no more than one step above it. Standard errors are in parentheses.