Contingent Processing in Strategic Reasoning: An Experimental Illustration

by

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and

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Abstract

Contingent processing occurs when agents select computational procedures for making decisions contingent on specific features of the decision environment they face. We illustrate contingent processing in a simple strategic situation—the well-known takeover game where the naive model of bidding provides an explanation of the winner's curse. We interpret the naïve model as a simplification procedure used when the complexity of the takeover game is sufficiently great in a well-defined way we call type complexity. Experimental variation of type complexity across takeover games changes the frequency of naïve simplification, consistent with contingent processing theories of decision cognition.

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Contingent processing theory assumes that humans can and in fact do process the information in decision environments in many different ways, even changing processing in the midst of a single decision problem. According to this view, we have many algorithms (that is, computational procedures for comparing alternatives or strategies, along with various screening and simplification procedures) that are used singly or in sequence to make decisions efficiently and quickly. Theories of contingent processing classify features of decision environments and seek to predict how those features determine algorithm use and, ultimately, choice (Payne 1982). As with most psychological views of decision making, contingent processing is viewed as adaptive, generally cutting decision time and cognitive effort with little serious decision error (Payne, Bettman and Johnson 1988). In fact, the contingencies of contingent processing are frequently modeled as resulting from an error/effort tradeoff, though other factors (such as justifiability, if decisions must be explained to others) are theorized to have effects too. However—and again as with most psychological views on decision making—contingent processing theories do sometimes predict consequential errors in decision making.[1]

Although it is conceptually close to economic theories of, and experiments on, bounded rationality (Conlisk 1980; Pingle 1992; Wilcox 1993), contingent processing has not had the same impact on economics that other behavioral ideas did (such as prospect theory, loss aversion and, in the particular case of game theory, other-regarding preferences). Yet in game theory, it may be an opportune time to reintroduce contingent processing theory as a useful framework and illustrate its empirical promise. Experiments reveal marked heterogeneity in the depth of strategic reasoning of game players (Stahl and Wilson 1995; Nagel 1995; Costa-Gomes, Crawford and Broseta 2001; Camerer, Ho and Chong 2004). Contingent processing suggests that

[1] For an excellent book-length treatment of contingent processing theory and experiments by a leading research team, and containing extensive bibliography and historical background, see Payne, Bettman and Johnson (1993).

depth of reasoning may not be a fixed characteristic of a game player, but rather a processing metachoice that depends on features of the strategic environment such as complexity. In that event, contingent processing theory provides a framework for understanding how depth of strategic reasoning both varies across cognitively heterogeneous players, and for the same player across strategic situations of varying complexity.

We illustrate a contingent processing effect brought about by a variation in what we call type complexity in a game of asymmetric information—the well-known takeover game (Samuelson and Bazerman). In this game, the "naïve model" of bidding behavior explains the phenomenon known as the winner's curse, and even correctly predicts a "loser's curse" in suitably reformulated takeover games (Holt and Sherman 1994). We do not question the usefulness of the naïve model. Rather, we reinterpret the central processing error in the naïve model as a simplification algorithm that most players begin with when processing the typical version of the game. We then modify the game to decrease the cognitive demand for that particular simplification, and find that this decreases the frequency of naïve choices relative to rational ones, as one would expect from the viewpoint of contingent processing theory.

Two Takeover Games

In the takeover game (Samuelson and Bazerman 1985; Ball, Bazerman and Carroll 1990) and other bidding games with asymmetric information (Kagel and Levin 1986), many winning bids yield a worse *ex ante* expected outcome than would have been received with no bid at all (even with substantial experience)—an outcome usually called the winner's curse. One explanation for this is the "naïve" bidding model (Samuelson and Bazerman 1985; Kagel and Levin 1986)—a good example of progressive theory in the sense that it successfully predicted new and unexpected behavior in suitably modified takeover games (Holt and Sherman 1994). The naive model posits that agents fail to condition the expected consequences of their bids on information that will be revealed by other agents' acceptance of those bids.

Consider two versions of the takeover game.[2] We call the first one the "typical game." In the typical game, a risk-neutral[3] first mover or "buyer" submits a single bid b for an object she values at λv and, at the time of her bidding, she only knows that the second mover or "seller's" reservation value v is uniformly distributed on $[x,x+r] \in \mathbb{R}^+$. She receives the payoff $\lambda v–b$ only if her bid is accepted (only one bid is permitted); otherwise her payoff is zero. In a perfect equilibrium, the seller accepts if $b \geq v$. Most takeover game experiments use a "robot seller" (a fixed decision rule) that always does this (and buyer are so informed) to focus on the cognitive processes of the first mover. This removes interpretive difficulties introduced by potential effects of strategic uncertainty and/or other-regarding preferences, and we continue in this tradition.

Letting $\pi(b)$ be the probability that the buyer's bid b is accepted, the expected payoff from the bid b, given any particular realization of v, is $\pi(b)[\lambda v–b]$. Rational maximization of this involves two insights. First, the buyer must correctly deduce $\pi(b)$ which, in a perfect equilibrium, is $\pi(b) = (b\text{-}x)/r$, and both rational and naïve models assume that buyers do this. But the rational model also requires that the buyer substitute for v the expected value of v conditional on the event that b is accepted, which is $(x+b)/2$. By contrast, the naive model posits that buyers instead substitute the unconditional expected value $x+r/2$ for v. Holt and Sherman (1994) show that, depending on λ, x and r, naïve subjects may either underbid or overbid relative to the rational bid. When overbidding is so pronounced that expected losses occur, the naïve model

[2] Our exposition closely follows Holt and Sherman (1994) though using slightly different notation.
[3] Our experimental design relaxes this assumption. We maintain it in this section for ease of exposition.

predicts the winner's curse.[4] Holt and Sherman show that the naïve model performs well regardless of whether it predicts under- or overbidding in this Typical Game.

From a contingent processing perspective, the central feature of naïve models of bidding (replacing a distribution by its unconditional expected value) may be viewed as a simplification algorithm applied at an early processing stage. Apparently, few human subjects see that the adverse selection problem inherent in the asymmetric information of takeover games requires reasoning in terms of conditional expectations. This theoretical insight is, of course, not strictly necessary. Subjects could always use a brute force approach, actually enumerating every lottery associated with every possible bid b, and then selecting among these. But this is computationally quixotic if both seller types v and possible bids b are numerous (as in the typical game). Enumeration is a very lengthy process and leaves behind a huge decision set to be dealt with afterwards. Put differently, the typical game generates a high cognitive demand for an early problem simplification (in the absence of appropriate theoretical insight), and what we will call "naïve simplification" meets this demand by replacing a distribution with its unconditional mean.

Now consider a second takeover game—a "minimal game" where v is binomially distributed, so that there are only two possible seller types indexed by v (x or x+r with equal likelihood). Suppose also that buyers may bid only one of these two values. The naïve simplification could still be applied; the unconditional expected value of v is still x+r/2, and the subject could, in principle, use this to evaluate the consequences of her two possible bids x and x+r. But we rather doubt this will happen very often. In this instance, direct enumeration is simple and quick because there are only two possible seller types, and this leaves behind a simple choice between two binary lotteries associated with the two possible bids. Generalizing this idea to a class of takeover games with k distinct, equally spaced seller types (the typical and

[4] Classic parameters for such a case, used in many experiments, are $\lambda = 1.5$, $x = 0$ and $r > 0$.

minimal games being the boundary games in this class)—what we will call *k*-value games—we expect that as *k* grows, more subjects will employ naïve simplification. We call this the type multiplicity conjecture. Its basis closely resembles contingent processing models that posit the use of early screening and simplification procedures when decision sets are sufficiently complex (many alternatives and/or many attributes in each alternative), but not when they are very simple.

Experimental Design

The two treatments for this experiment are two *k*-value games. In both games, $x = 0$, $r = 16$ and $\lambda = 2$. In both games, v is distributed with uniform probability over a discrete set of equally spaced values—the set {0,8,16} yielding a 3-value game, and the set {0,2,4,…,14,16} yielding a 9-value game.[5] In both games, buyers' bids are constrained to the set of possible seller values, and buyers know that after they submit their bid a ten-sided die roll determines v. Tables 1-R and 2-R show the state-action mapping from the die rolls (columns) and possible bids (rows) to payoffs (cells). This is how the mapping is perceived under rational processing, so rows of these tables constitute the rational decision sets of the *k*-value games. By contrast, Tables 1-N and 2-N show how states and actions appear to map into payoffs under naïve simplification of the games. In both games, the naïve simplification substitutes the unconditional expected value of the seller's value (which is 8 dollars) for the appropriate conditional one regardless of the bid. In the 9-value game for instance, the bid of 8 is accepted for any seller value less than or equal to 8. Naïve simplification then results in a misperceived gain of $2 \cdot 8 - 8$ or 8 dollars in these

[5] Plausibility (or "justifiability to the self") may be a feature of an algorithm enhancing its appeal (Curley, Yates and Abrams 1986). In the minimal game, the support of v does not actually contain the unconditional expectation of v, while it does in the typical game. For this reason, naïve simplification may appear less plausible to subjects in minimal games than in typical games. This particular reason for contingent processing differs from the complexity-based reason we wish to examine here. This is why we compare the 3-value and 9-value games: In both games, the unconditional expectation of v is in the support of the v distribution, reducing plausibility differences as a source of

instances, as shown in Table 1-N. We call the rows of these tables the naïve decision sets of the *k*-value games. Note that the true decision set of a game is always the rational decision set, while the game's naïve decision set is what the decision set appears to be under naïve simplification.

The experimental design simply elicits preference orderings over the common actions in the rational and naïve decision sets associated with a game by direct presentation of lottery ranking problems resembling these tables. We then ask which of these elicited preference orderings best predicts direct bidding behavior in the conventionally presented *k*-value takeover game that generates those sets. As Tables 1-R and 2-R show, $\lambda = 2$ results in a situation where all bids are symmetric, zero return lotteries, with increasing risk down the table. In theory (e.g. Rothschild and Stiglitz 1970), it is well-known that risk averters will prefer lotteries toward the top of these tables, and in fact the overwhelming majority of subjects prefer less risk to more risk for such symmetric, zero expectation lotteries (Kahneman and Tversky 1979). That is, under rational perception of the games, we should expect low or zero bids for most subjects. By contrast, maximum (perceived!) expected values occur in the middle row(s) of Tables 1-N and 2-N; many mild risk averters would thus be drawn toward those central rows and away from the highest rows. That is, under naïve perception of the games, one might expect more central bids, such as bids of 8 in both games or perhaps bids of 4 or 6 in the 9-value game.

This discussion merely illustrates how bidding may differ under rational and naïve perception, given typical observations of, and theories about, choice under risk. But in fact the design requires no specific risk attitude for any particular subject, much less that any particular theory of choice under risk guides those preferences. Instead, success of the design simply requires that subjects have empirically distinct preference orderings over the actions in the

contingent processing differences. This is a chief difference between our experiment and that of Charness and Levin (2004).

rational and naïve decision sets, in which case it is sensible to ask which preferences (empirically measured in the option ranking problems) best predict subjects' bids.[6]

We use between-subjects treatment variation in this experiment. Both treatments consist of nine decision tasks, divided into three parts. Part I consists of two option ranking problems in which subjects rank options identical to the rational and naïve decision sets generated by their bidding game. In part II, subjects bid in five consecutive rounds of their bidding game, with a die roll following each bid and immediate feedback on prospective payoffs resulting from those bids to allow for learning and/or removal of misunderstandings. In this part, bidding in the takeover game is described in conventional terms (see e.g. Holt and Sherman 1994) and, importantly, without any tables such as those discussed above. Part III is an exact replication of Part I undertaken as a reliability check on subject rankings (the purposes of this are described below). Subjects are told that exactly one of these nine decisions will be selected at the experiment's conclusion to determine their experimental earnings—a procedure called random task selection.[7]

The option ranking problems of parts I and III are derived directly from the rational and naïve decision sets shown in Tables 1-R, 1-N, 2-R and 2-N. These tables are simply changed in

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[6] It is unusual to examine games where risk neutrality yields rational indifference between bids. We are certainly counting on subjects having distinct preferences over the options in the rational and naïve decision sets; normally, designs inducing different expected values of bids would be a part of this. Yet in discrete takeover games like ours, risk differences between bids become quite pronounced (see Tables 1-R and 2-R). In pilot experiments with our intended subject population, we found it impossible to create discrete games where perceived expected value differences dominate risk differences for the majority of subjects in the option ranking problems. Observing this, we abandoned that effort, and found in further pretests that it was relatively simple to get distinctive preferences over naïve and rational options with $\lambda = 2$ (which generates equal rationally perceived expected values of all bids, but a strict ordering of risk across them). It seems sensible to us not to fight against subjects' observed preferences, but rather to work with these instead; and to measure and use subject preferences directly rather than make assumptions about them (for another example, see Archibald and Wilcox 2002). Our method of eliciting and using actual preferences is another main difference between this work and that of Charness and Levin (2004).

[7] This is a standard way of inducing subjects to treat a collection of risky choice problems in isolation from one another—rather than as a portfolio of risks. If subjects obey the "independence axiom" in its unreduced compounds form, the mechanism should ensure this. Holt (1986) has noted that violations of that assumption can undermine the mechanism, but evidence suggests that subjects do obey the independence axiom in its unreduced compounds form. This is Kahneman and Tversky (1979) "isolation effect;" also see Conlisk (1989). Additionally, direct examinations of the mechanism suggest that random task selection does not create preference artifacts (Starmer and Sugden 1991).

these ways to eliminate overt associations with the bidding games: (a) Bid labels of rows are replaced with letter labels of options; (b) seller values are eliminated from column headers, leaving only the die roll column labels; and (c) a column is added at the far right of each table for subjects to write in their rankings. Tables 3-R and 3-N show this for the 3-value treatment.

To motivate subjects' option rankings in Parts I and III, we use a version of random task selection in which subjects never play an option they rank as least preferred, and play one of the remaining options with probabilities that increase for more preferred options. If an option ranking problem is selected to determine subject payoffs, the option selected by this scheme is played with a die roll, as shown in the option ranking problem. For example, in the Three Value Game treatment subjects were informed that if an option ranking task was selected, the experimenter would begin by rolling a ten-sided die (numbered 0 to 9). If the number rolled was less than or equal to 6 (a seventy percent chance), the subject would play the option they ranked as best; otherwise (a thirty percent chance) the subject would play the option they ranked as second-best. The procedure in the 9-value game was similar, but two ten-sided dice were used, and positive probabilities of selection was assigned to the options ranked 1^{st} through 8^{th} (with these selection probabilities declining for less preferred alternatives).

The experiment was performed in four sessions (two of each treatment) at the University of Houston, using summer term students in introductory economics classes in the summer of 2000. These subjects received course credit for participation. Additionally, three subjects in each session were randomly selected at the end to "play" for cash (of course known to all subjects at the outset of their session). Because some decisions involved potential losses, subjects were informed that subjects selected to play would immediately have a $16.00 cash balance and that any loss would be deducted from this (the maximum possible loss in any decision is $16.00).

Dependent Measures

The measure we use to analyze subject bids is a rank difference. Let b_{stk} be the observed Part 2 bid of subject s in trial $t \in \{1,2,\ldots,5\}$ of treatment $k \in \{3,9\}$. Note that each b_{stk} corresponds to a unique option in both rational and naïve option ranking problems in Parts I and III. Let R_{jstk} denote the mean rank (across the two trials in parts I and III) that subject s assigned to the option corresponding to her observed b_{stk}, where $j = r$ or n denotes rational and naïve option ranking problems, respectively. Then $\Delta_{stk} = R_{rstk} - R_{nstk}$ is our basic dependent measure on each subject—the difference between the average ranks the subject assigned to the rational and naïve options that correspond to her observed bids b_{stk}. We interpret $\Delta_{stk} > 0$ as evidence of naïve simplification (and $\Delta_{stk} < 0$ as evidence against it) since $\Delta_{stk} > 0$ means that subject s's mean naïve option ranking explains her observed bids better than her mean rational option ranking.[8]

There are three matters that may argue for the exclusion of certain subjects from data analysis. As it turns out, this is inconsequential: Our results are largely identical whether we exclude subjects according to the three criteria discussed immediately below or not. Yet we think attention to these matters is necessary in a careful study. First, note that Δ_{stk} near zero can occur for two quite different reasons. As mentioned earlier, subjects with distinct rankings of the rational and naïve option ranking problems are what we need for our design to work (if we had pretested subjects for suitable option preferences, these are the kinds we would have invited back). It can still occur that $\Delta_{stk} \approx 0$ for such subjects for particular bids they make, and this is useful information for our purposes. But if subjects do not have very distinct rankings across the rational and naïve option ranking problems, $\Delta_{stk} \approx 0$ definitionally occurs for them almost

[8] Low numerical ranks are "better ranks," that is, subjects gave the rank of "1" to their most preferred option, with successively higher numbers corresponding to successively less preferred option. Thus when the difference between the numerical rank of the rational and naïve option is positive, this is evidence in favor of the naïve ranking of bids.

regardless of their observed bid b_{stk}. Put differently, the experimental design is arguably poorly suited to the preferences of these subjects since, for them, $\Delta_{stk} \approx 0$ is not really evidence that the naïve and rational model are equally explanatory of their bidding behavior—our design simply cannot detect which model best explains their bidding behavior. Because of this, one may wish to exclude subjects whose mean rankings in the two trials of the rational and naïve ranking problems are not "different enough," and we want to apply the same criterion in both treatments. To do this, we compute Kendall's tau τ_{rn} (a nonparametric correlation coefficient) between each subjects' average naïve and average rational ranks, using the ranking data from both Parts I and III of the experiment to get these averages. If $\tau_{rn} > 0.5$, we may regard the rankings as similar and may wish to exclude such subjects from analysis.

The second matter is reliability. If a subject's ranking of either rational or naïve options is not very reliable, then the subject's Δ_{stk} are not informative and noisy as well. This is the main reason our design includes two trials of both ranking problems. Let τ_r and τ_n denote Kendall's tau between a subject's Part I and Part III rational and naïve rankings, respectively. If $\tau_r < .33$ or $\tau_n < .33$ for a subject, we may consider that subject as not reliable enough in expressing her rankings to pay much attention to, and may wish to exclude her from data analysis as well.[9]

Finally, the option ranking problems include a natural flag for obvious subject confusion. Tables 1-N and 2-N show that the options corresponding to $b = 16$ in both treatments is stochastically dominated by all other options in naïve option ranking problems. As a result of this, any subject who minimally understands the option ranking problems should rank this option

[9] The value of 0.33 is chosen for combinatorial reasons. In the Three Value Game treatment—since only three options are ranked—the rank correlation coefficient can only take four possible values: 1, 0.33, –0.33 or –1. We simply select all subjects who show a positive rank correlation in the 3-value treatment ranking problems, and this is necessarily everyone with rank correlations of 0.33 or higher. For consistency, the same numerical criterion is used for subjects in 9-value treatments.

as least preferred in naïve option ranking problems.[10] We may also choose to exclude the (very) small number of subjects who failed to do this. In summary, we will analyze our data for two different subject groups: All subjects, and a restricted group of subjects who are not excluded by any of the three potential "exclusion criteria" we have just discussed.

Results

Table 4 shows tests against the hypothesis that the location of the distribution of Δ_{stk} is zero, by treatment *k* and bidding trial *t*. The left pair of columns show tests for all subjects, while the right pair of columns show them for the restricted group of subjects. Five rows of this table show a t-statistic, a sign test statistic and a signed-rank test statistic, along with their two-tailed p-values, by bidding trial. Although statistics appear for the first bidding trial, we discount these results since we use the first trial as an opportunity to clear up subjects' misunderstands on the basis of their mistakes in calculating gains and/or losses at the end of that trial. Accordingly the last row of Table 4 shows averaged results $\frac{1}{4}\Sigma_{t=2 \text{ to } 5}\ \Delta_{stk}$ based only on results of trials 2 to 5.

Table 4 shows that, in the 9-value treatment, naïve simplification is in strong evidence (both for all subjects and the restricted subset of subjects). Consider for example the sign test statistics S and their p-values, which are in certain respects the best-justified test statistics.[11] In trials two through five, six of the eight sign test statistics are individually significant at five percent or better, and all eight of them are positive—indicating that the naïve option rankings

[10] Although subjects sometimes violate dominance through a sequence of choices, violations are extremely rare in a single two-choice decision problem where dominance relations are transparent (Loomes and Sugden 1998). Thus, these events appear to signal a "trembling hand" or outright confusion, rather than something systematic and behaviorally relevant.

[11] From a very formal viewpoint, the sign test statistic is best justified since the ranks R_{rstk} and R_{nstk} on which the rank differences Δ_{stk} are based have no cardinal meaning (as the t-statistic requires), nor are the magnitudes of differences in ranks Δ_{stk} necessarily comparable across subjects (as the signed-rank test requires). Nevertheless, we have included the t-statistics and signed-rank statistics for purposes of comparison.

explain observed bids best. The overall test results given in the last row also show this (at one percent or better); and all results using the t-statistics or signed-rank statistics are quite similar. This is a good place to emphasize that though our discrete versions the takeover game are quite different from the typical game, and though our method of identifying subjects' lottery preferences is very different from what is usually done in experiments on the typical game (in most, risk neutrality is simply assumed), our results in the 9-value game fully and strongly replicate known experimental findings for the typical game.

By contrast, there is no consistent evidence favoring naïve simplification in the 3-value game. Again consider the sign tests in Table 4: Five of the eight statistics for trials 2 to 5 are positive, but only one significantly so (and only in the restricted group of subjects); the other three are negative, with one significantly so. The averaged sign test results in the last row are slightly negative, but tiny and insignificant; and all results using the t-statistics or signed rank statistics are similar. In the 3-value game, systematic evidence of naïve simplification vanishes.

Table 5 addresses the question of whether there is a significant treatment difference between the 9-value and 3-value treatments, showing results of tests against the hypothesis that the distributions of Δ_{stk} are equivalent across treatments. Almost necessarily, the variance of Δ_{stk} must differ between treatments: Ranks only range from 1 and 3 for subjects in the 3-value game while they range from 1 to 9 in the 9-value game. Figure 1 illustrates this higher variability with graphs of the cumulative empirical distribution functions of the average rank difference measures $\frac{1}{4}\Sigma_{t=2 \text{ to } 5}\ \Delta_{stk}$ in the two treatments. Because of this, 2-sample test statistics for detecting a treatment difference must finesse this problem of unequal variances. A 2-sample t-test with unequal variances (with Satterthwaite-corrected degrees of freedom) and the robust-rank-order test (Siegel and Castellan 1988), both of which satisfy this requirement. We also count the number of observations in each treatment for which $\Delta_{stk} > 0$ (that is, observations that favor the naïve model) and use Fisher's exact test to test the hypothesis that the two treatments are equivalent with respect to these counts.[12]

The results of all three tests are given in Table 5, and these results indicate that as predicted, naïve simplification is more common in the 9-value game. The t-statistics and robust-rank-order statistics are formed for the difference between the locations (means and medians, respectively) of the distributions of Δ_{stk} in the 9-value and 3-value game, and the consistently positive signs indicate that this location is consistently greater in the 9-value game, supporting more frequent use of the naïve simplification when type multiplicity is greater. Six of the eight p-values of Fisher's exact test indicate that this difference is significant (at five percent or better) in bidding trials two through five, and the t-tests and robust-rank-order tests confirm this. All of the aggregate test p-values in the last row of Table 5 also show that naïve behavior is relatively more common in the 9-value game. Our data strongly support the type multiplicity conjecture.

Conclusions

Given the large psychological literature on contingent processing—the notion that simplification procedures are many, and that their use is contingent upon features of the decision task at hand—support for the type multiplicity conjecture is unsurprising. In particular, that literature suggests that an increase in either the number of alternatives in a decision set, or the number of dimensions in a fixed number of alternatives in a decision set, increases the use of screening and simplification procedures early in processing (Payne, Bettman and Johnson 1993).

[12] For the same kinds of reasons mentioned in the previous footnote, Fisher's exact test is probably the best justified test; but we give the t-test and the robust-rank order test for purposes of comparison. Robust-rank-order tests rarely appear in the experimental literature though they are theoretically better than the Wilcoxon two-sample test when

In passing from the 3-value to the 9-value game, both the number of alternatives (possible bids) and the number of dimensions of each alternative (possible realizations of seller values and hence payoffs) increase; so it is no surprise that the 9-value game is associated with a greater cognitive demand for an early simplification algorithm like the naïve simplification.

Results on the typical takeover game show that learning is agonizingly slow in these games—if it takes place at all (Ball, Bazerman and Carroll 1990; Holt and Sherman 1994). It is possible that learning would be more rapid when type multiplicity is low (as in the 3-value game) for simple "experimental" reasons: Less sampling is required to learn from experience when there are both fewer bids to try and fewer possible outcomes of each bid. Perhaps one reason that naïve simplification is more evident in the 9-value game is that learning is simply slower there. We allow that this may contribute to what we observe in Table 4, and results from a similar experimental design (Charness and Levin 2004) support this argument too. Generally, the relative value of early simplification may be reduced by informative feedback. Since feedback is more quickly informative in the 3-value than the 9-value game, we do not believe there is any fundamental conflict between our contingent processing interpretation of the type multiplicity effect and this learning explanation of the effect. But we point out that evidence of the type multiplicity effect is present in our data from the second trial forward (see Tables 4 and 5), strongly suggesting that learning speed is an incomplete explanation of what we observe—which, we argue, is contingent processing at work. We believe contingent processing theories hold some promise as explanatory frameworks for variations in the depth of strategic reasoning across people and games, and believe this experiment illustrates that promise.

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variability differs between samples. For an interesting discussion of the relative power, and relative rate of false positives, of the Wilcoxon and robust-rank-order test under various assumptions, see Feltovich (2003).

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Tables 1-R and 1-N

Tables 1-R and 1-N: Rational and Naïve Mappings from Actions (bids) and States (seller values) to Payoff Consequences in the 9-value Game.

Table 1-R: The Rational Mapping

Die Roll→ Seller v→ Bids↓	1 0	2 2	3 4	4 6	5 8	6 10	7 12	8 14	9 16	Expected Gain
0	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**
2	**–2**	**+2**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**
4	**–4**	**0**	**+4**	**0**	**0**	**0**	**0**	**0**	**0**	**0**
6	**–6**	**–2**	**+2**	**+6**	**0**	**0**	**0**	**0**	**0**	**0**
8	**–8**	**–4**	**0**	**+4**	**+8**	**0**	**0**	**0**	**0**	**0**
10	**–10**	**–6**	**–2**	**+2**	**+6**	**+10**	**0**	**0**	**0**	**0**
12	**–12**	**–8**	**–4**	**0**	**+4**	**+8**	**+12**	**0**	**0**	**0**
14	**–14**	**–10**	**–6**	**–2**	**+2**	**+6**	**+ 10**	**+ 14**	**0**	**0**
16	**–16**	**–12**	**–8**	**–4**	**0**	**+4**	**+8**	**+12**	**+16**	**0**

Table 1-N: The Naïve Mapping

Die Roll→ Seller v→ Bids↓	1 0	2 2	3 4	4 6	5 8	6 10	7 12	8 14	9 16	Expected Gain
0	**+16**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**+1.77**
2	**+14**	**+14**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**+3.11**
4	**+12**	**+12**	**+12**	**0**	**0**	**0**	**0**	**0**	**0**	**+4.00**
6	**+10**	**+10**	**+10**	**+10**	**0**	**0**	**0**	**0**	**0**	**+4.44**
8	**+8**	**+8**	**+8**	**+8**	**+8**	**0**	**0**	**0**	**0**	**+4.44**
10	**+6**	**+6**	**+6**	**+6**	**+6**	**+6**	**0**	**0**	**0**	**+4.00**
12	**+4**	**+4**	**+4**	**+4**	**+4**	**+4**	**+4**	**0**	**0**	**+3.11**
14	**+2**	**+2**	**+2**	**+2**	**+2**	**+2**	**+2**	**+2**	**0**	**+1.77**
16	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**	**0**

Tables 2-R, 2-N, 3-R and 3-N

Tables 2-R and 2-N: Rational and Naïve Mappings from Actions (bids) and States (seller values) to Payoff Consequences in the 3-value Game.

Table 2-R: The Rational Mapping

Die Roll→ Seller v→ Bids↓	1, 2 or 3 0	4, 5 or 6 8	7, 8 or 9 16	Expected Gain
0	**0**	**0**	**0**	**0**
8	**-8**	**+8**	**0**	**0**
16	**-16**	**0**	**+16**	**0**

Table 2-N: The Naïve Mapping

Die Roll→ Seller v→ Bids↓	1, 2 or 3 0	4, 5 or 6 8	7, 8 or 9 16	Expected Gain
0	**+16**	**0**	**0**	**+5.33**
8	**+8**	**+8**	**0**	**+10.67**
16	**0**	**0**	**0**	**0**

Tables 3-R and 3-N: Option Ranking Problems used for the 3-value Game

Table 3-R. The Rational Option Ranking Problem

Die Roll→ Options↓	1, 2 or 3	4, 5 or 6	7, 8 or 9	My Ranking of Options
A	**+16**	**0**	**0**	
B	**-8**	**+8**	**0**	
C	**-16**	**0**	**+16**	

Table 3-N. The Naïve Option Ranking Problem

Die Roll→ Options↓	1, 2 or 3	4, 5 or 6	7, 8 or 9	My Ranking of Options
A	**+16**	**0**	**0**	
B	**+8**	**+8**	**0**	
C	**0**	**0**	**0**	

Table 4

Table 4: One-Sample Tests Against the Hypothesis that the Location of the Distribution of Δ_{stk} is Zero, By Treatment

	All Subjects		Restricted Samples of Subjects[a]	
Bidding Trial	3-value Game (N=57)	9-value Game (N=41)	3-value Game (N=33)	9-value Game (N=24)
1st	t = 1.80, p = .07 S = 7.5, p = .02 SR = 118.5, p = .09	t = 1.89, p = .07 S = 7.5, p = .02 SR = 169, p = .02	t = 2.10, p = .04 S = 7, p = .09 SR = 74, p = .05	t = 2.04, p = .05 S = 7.5, p < .01 SR = 68.5, p = .03
2nd	t = 0.60, p = .55 S = 4, p = .27 SR = 44, p = .55	t = 1.55, p = .13 S = 6.5, p = .05 SR = 168.5, p = .02	t = 1.16, p = .25 S = 5.5, p = .05 SR = 48, p = .25	t = 4.62, p < 0.01 S = 8.5, p < 0.01 SR = 110, p < .01
3rd	t = –1.42, p = .16 S = –2.5, p = .53 SR = –109, p = .15	t = 1.86, p = .07 S = 9, p < .01 SR = 162.5, p = .02	t = –0.87, p = .39 S = 0.5. p = 1.00 SR = –42, p = .31	t = 1.59, p = .12 S = 6, p = .02 SR = 57.5, p = .06
4th	t = 0.82, p = .41 S = 5.5, p = .12 SR = 65, p = .40	t = 1.73, p = .09 S = 6.5, p = .05 SR = 139, p = .03	t = 0.51, p = .61 S = 4.5, p = .12 SR = 20.5, p = .62	t = 2.23, p = .06 S = 6, p = .02 SR = 68.5, p = .02
5th	t = –2.60, p = .01 S = –6.5, p = .06 SR = –193.5, p = .01	t = 1.42, p = .16 S = 4.5, p = .19 SR = 109.5, p = .10	t = –2.52, p = .02 S = –4.5, p = .14 SR = –114, p < .01	t = 1.17, p = .25 S = 3.5, p = .21 SR = 42.5, p = .20
Average of 2nd to 5th Trial	t = –0.88, p = .38 S = –2.5, p = .54 SR = –89.5, p = .28	t = 2.11, p = .04 S = 9, p < .01 SR = 177, p = .02	t = –0.62, p = .54 S = –1, p = .85 SR = –32.5, p = .51	t = 2.55, p=0.02 S = 6.5, p = .01 SR = 73, p = .02

Notes: All tests are one-sample tests; all p-values are two-tailed. t is a t-statistic against the hypothesis that the mean is zero. SR is a signed-rank test statistic against the hypothesis that the median is zero. S is a sign test statistic against the hypothesis that the median is zero.

[a] Subjects are excluded as a robustness check. Data for the excluded subjects suffers from one or more of these potential problems: (i) Mean rankings in the rational and naïve ranking problems are not very different; (ii) retest reliability of either rational or naïve rankings (or both) is low; and/or (iii) rankings in the naïve ranking problem violate stochastic dominance. See the text for details.

<u>**Table 5**</u>

Table 5: Two-Sample Tests Against the Hypothesis that the Distributions of Δ_{stk} are Equivalent across the 9-value and 3-value Game Treatments

Bidding Trial	All Subjects N = 57, 3-value Game N = 41, 9-value Game	Restricted Samples of Subjects[a] N = 33, 3-value Game N = 24, 9-value Game
1st	t = 1.45, p = .15 FE, p = .10 Ù = 2.23, p = 0.03	t = 1.47, p = .15 FE, p = .16 Ù = 2.77. p < .01
2nd	t = 1.34, p = .19 FE, p = .04 Ù = 2.36, p = .02	t = 3.77, p < .01 FE, p = .05 Ù = 4.59, p < .01
3rd	t = 2.15, p = .04 FE, p < .01 Ù = 3.07, p < .01	t = 1.78, p = .09 FE, p = .03 Ù = 2.39, p = .02
4th	t = 1.49, p = .09 FE, p = .16 Ù = 2.25, p = .02	t = 2.00, p = .06 FE, p = .28 Ù = 2.89, p < .01
5th	t = 2.03, p = .05 FE, p < .01 Ù = 2.61, p < .01	t = 1.88, p = .07 FE, p = .03 Ù = 2.60, p < .01
Average of 2nd to 5th Trials	t = 2.26, p = .03 FE, p < .01 Ù = 2.96, p < .01	t = 2.62, p = .01 FE, p = .03 Ù = 2.93, p < .01

Notes: All tests are 2-sample tests; all p-values are two-tailed. t is a Student-t test statistic, with unequal variances and Satterthwaite-corrected degrees of freedom, against the hypothesis that sample means are equivalent. Ù is a robust-rank-order test statistic against the hypothesis that sample medians are equivalent. FE is Fisher's exact test, which has no test statistic, just a p-value: For each individual trial, it is a test for a treatment difference in the proportion of subjects showing evidence of the naïve simplification, that is, the proportion of observed bids for which $\Delta_{stk} > 0$. For the final row labeled "Average of 2nd to 5th Trials," it is actually a test for a treatment difference in the proportion of subjects for whom more than half of the 2nd to 5th trials show evidence of naïve behavior, that is, a test against the hypothesis that the proportion of subjects for whom $\Delta_{stk} > 0$ in three or more of the four trials $t \in \{2,3,4,5\}$ is equivalent across the two treatments.

[a] Subjects are excluded as a robustness check. Data for the excluded subjects suffers from one or more of these potential problems: (i) Mean rankings in the rational and naïve ranking problems are not very different; (ii) retest reliability of either rational or naïve rankings (or both) is low; and/or (iii) rankings in the naïve ranking problem violate stochastic dominance. See the text for details.

Figure 1

Cumulative Distributions of Average Rank Difference across Bidding Trials 2 To 5, By Game